Sentiment, Financial Agents and Decision-Making

by

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Declaration

I declare that the material included in this thesis has not been submitted for a degree to any other University. Further, I declare that part of the material included in this thesis have also been used in co-authored papers with Alok Kumar and Constantinos Antoniou.

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Abstract

This thesis examines whether strong exogenous shocks to the sentiment of sophisticated financial agents can influence their decision-making. To capture any sentiment changes, we use extreme negative events such as terrorist attacks and mass shootings. Specifically, we conjecture that financial agents that are local to these events during the period of the attacks should experience strong negative feelings related to fear and anxiety, which in turn would affect their decisions.

In the first chapter, we examine whether terrorist attacks and mass shootings tend to affect the earnings forecasts of sell-side equity analysts. Our findings suggest that analysts located near these events are more likely to issue pessimistic forecasts. This effect becomes stronger when the distance between the analyst and the event decreases, when fewer days separate the event and the forecast, and when the analyst resides in a region with low murder rate. Interestingly, pessimistic analyst forecasts are more accurate since the negative sentiment induced by terrorist events partially mitigates the well-documented optimism bias among equity analysts.

In the second chapter, we focus on corporate managers and examine whether they apply different firm policies when they are exposed to such negative events. Our results show that local firms around attack periods increase cash holdings, and reduce R&D expenditure and long-term leverage. These effects are temporary, and become weaker as the firm-event distance increases. Further, we show that these effects are mainly concentrated in firms managed by younger CEOs, and tend to be larger for events with greater media coverage.

In the third chapter, we show that institutional investors located near these terrorist events tend to increase their selling propensity around that time period. Similar to previous chapters, we find that this effect becomes stronger as the geographical proximity of investors to the location of the attacks increases, and when investors trade near the date of the attacks. However, these effects are less pronounced for firms which entail higher transaction costs such as small-sized firms, illiquid firms, and firms with volatile and skewed stock returns. Such trading

behavior has a negative impact on the quarterly trading performance of institutional investors and on stock returns.

Overall, our findings are consistent with the view that strong negative shocks to sentiment, induced by extreme negative events, can significantly affect the decision-making of sophisticated financial agents such as sell-side analysts, corporate managers and institutional investors.

Overview

A number of studies in psychology suggest that individuals who experience a strong negative shock to their sentiment alter their risk assessment and in turn their decision-making (e.g., Johnson and Tversky, 1983; Slovic, Finucane, Peters and MacGregor, 2002). Despite the common belief about the sophistication of financial agents, several studies document that these agents are also susceptible to cognitive biases that affect their beliefs, and influence their financial decisions (Barberis and Thaler, 2002; Baker and Wurgler, 2012). Motivated by this evidence, this thesis examines whether sophisticated financial agents, such as sell-side analysts, corporate managers and institutional investors, tend to change their financial decisions as a result of strong exogenous negative shocks to their sentiment.

Since sentiment is unobservable, we focus on the decisions of financial agents located near terrorist attacks and mass shootings in the U.S. Such events can cultivate strong negative sentiment in population, because their random nature highlights that anyone is potentially vulnerable. Further, this effect will be particularly intense for local agents, because they are more likely to interact with (or hear about) people directly affected by the event (Galea, Ahern, Resnick, Kilpatrick, Bucuvalas, Gold and Vlahov, 2002; Hughes, Brymer, Chiu, Fairbank, Jones, Pynoos, Rothwell, Steinberg and Kessler, 2011). Following this evidence, we conjecture that financial agents located near terrorist attacks and mass shootings will experience stronger negative sentiment around the time period of the attacks, which in turn will affect their financial decisions.

In the first chapter of this thesis, we focus on the earnings forecasts of sell-side equity analysts. Sell-side analysts tend to have an important monitoring role on firms (Jensen and Meckling, 1976), while their forecasts can significantly influence the investment decisions of both retail and institutional investors (Malmendier and Shanthikumar, 2007).

Specifically, a number of studies show that sell-side analysts incorporate past released public information and new information in their forecasts, acting as information intermediaries (Asquith, Mikhail. and Au, 2005; Frankel, Kothari, and Weber, 2006; Lui, Markov, and Tamayo, 2012). Analysts with high forecast accuracy tend to have better career prospects (Hong and Kubik, 2003), make bolder forecasts (Clement and Tse, 2005), increase their customer base and enhance their reputation (Stickel, 1992; Hilary and Hsu, 2013). However, their accuracy is subject to several personal characteristics related to experience (Clement, 1999; Clement, Koonce, and Lopez, 2007), expertise (Boni and Womack, 2006; Kadan, Madureira, Wang, and Zach, 2012), locality (Malloy, 2005; Bae, Stulz, and Tan, 2008), gender (Kumar, 2010) and interpersonal relations (Cohen, Frazzini, and Malloy, 2010).

Further, several studies support that sell-side analysts often provide biased forecasts, which are driven from psychological traits and cognitive factors. More specifically, Hilary and Menzly (2006) examine whether analysts become overconfident when they predict past earnings more accurately than the median analyst. Their findings suggest that analysts with short-lived success often overestimate their ability to forecast future earnings. Easterwood and Nutt (1999) find that analysts underreact to negative information and overreact to positive, while Cen, Hilary, and Wei (2013) support that analyst forecasts suffer from the anchoring bias since they are driven by the industry norm. This chapter contributes to this literature by providing new evidence which support that sell-side analysts who are exposed to terrorist events and experience strong negative sentiment, tend to issue more pessimistic earnings forecasts.

In the second chapter, we examine the impact of terrorist attacks and mass shootings on the decision-making of corporate managers. We focus on corporate managers due to their direct association with the prospects of the firms, and in turn with shareholder value (Bertrand and Schoar, 2003; Adams, Almeida, and Ferreira, 2005; Kaplan, Klebanov, and Sorensen, 2012).

As a consequence, several studies in finance literature examine how behavioral biases can cause variation in corporate policies across firms. In particular, Malmendier and Tate (2005) show that overconfident managers tend to overinvest when they have abundant internal funds, while Malmendier and Tate (2008) argue that overconfident managers engage in value-destroying mergers and acquisitions. Landier and Thesmar (2009) and Hirshleifer, Low, and Teoh (2012) show that overconfidence affects decisions related to capital structure and R&D expenditure, respectively. Further, Dessaint and Matray (2016) and Hutton, Jiang, and Kumar (2014) show that corporate policies are affected by the availability heuristic and managerial conservatism, respectively. Other related research shows that CEOs' personality traits affect choices related to capital structure and acquisition activity (Malmendier, Tate, and Yan, 2011; Cain and McKeon, 2016).

The second chapter of this thesis contributes to this literature by showing that strong negative sentiment cultivated from proximity to terrorist events can also affect corporate policies. Further, the main advantage of our framework reduces concerns for potential endogeneity since terrorist attacks and mass shootings occur at random locations and time periods (Meyer, 1995; Roberts and Whited, 2012), while at the same time they do not illustrate any direct linkage to the economic fundamentals of firms.

In the third chapter of this thesis, we examine whether sophisticated market participants such as institutional investors, who are exposed to terrorist attacks and mass shootings tend to alter their trading decisions. Institutional investors play a key role in the price formation process, since they are thought as rational arbitragers that tend to eliminate any price distortions. Therefore, if institutional investors are prone to extreme sentiment, we will observe changes in their trading decisions which can affect daily stock returns, while price distortions will continue to exist (Nagel, 2005).

A number of recent studies use several proxies in order to capture fluctuations in the sentiment of investors. More specifically, Saunders (1993) and Hirshleifer and Shumway (2003) use weather as proxy for investors' sentiment and show that stock returns increase more often in sunny days. A recent study by Goetzmann, Kim, Kumar, and Wang (2015) finds that weather can also affect the trading decisions of more sophisticated market participants such as institutional investors. Further evidence in favor of the existence of mood-related biases is given by Edmans, García, and Norli (2007), who employ international soccer results as mood variable and argue that there is a significant decline on stock prices after soccer losses. Accordingly, Kaplanski and Levy (2010) focus on aviation disasters in order to capture bad mood, anxiety and fear, and find evidence which support that these events can cause a decrease on stock prices.

In comparison to previous studies, the third chapter of this thesis provides a new proxy to capture negative shocks in the sentiment of institutional investors. Since terrorist attacks and mass shootings occur at random times and locations, this proxy allows us to minimize any potential self-selection bias that would be related with the location preferences of institutional investors, and potentially with their trading activity. Further, in this chapter we contribute to the literature by providing additional evidence which suggest that institutional investors are not immune to cognitive biases.

The rest of the thesis is organized as follows. Chapter 1 presents the paper regarding the impact of terrorist events on the earnings forecasts of sell-side equity analysts. Chapter 2 focuses on corporate managers and provides a detailed analysis regarding the effect of terrorist attacks and mass shootings on their corporate decision-making. In Chapter 3, we examine whether institutional investors change their trading decisions when they experience strong negative sentiment induced from terrorist events. Chapter 4 concludes.

Chapter 1

Terrorist Attacks, Analyst Sentiment, and Earnings Forecasts

1.1. Introduction

Evidence from psychology suggests that terrorist attacks and mass shootings (henceforth, “terrorist attacks”) generate feelings of fear, anxiety and depression among the affected people (e.g., Lerner and Keltner, 2001; Lerner, Gonzalez, Small and Fischhoff, 2003; Galea, Ahern, Resnick, Kilpatrick, Bucuvalas, Gold and Vlahov, 2002; Hughes, Brymer, Chiu, Fairbank, Jones, Pynoos, Rothwell, Steinberg and Kessler, 2011).[1] Consequently, the “sentiment” and the decisions of individuals exposed to such extreme shocks are affected. In particular, people directly exposed to extreme negative events are likely to become more pessimistic in their risk assessments in *unrelated* domains (e.g., Lerner and Keltner, 2001; Lerner, et al. 2003).[2]

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[1] For more information on the effects of terrorist attacks and mass shootings, see http://www.apa.org/helpcenter/terrorism.aspx and http://www.apa.org/helpcenter/mass-shooting.aspx.

[2] The key finding from this literature is that people who experience a negative shock to their sentiment become more pessimistic in their assessments of risk, and vice-versa (e.g., Johnson and Tversky, 1983; Finucane, Alhakami, Slovic, and Johnson, 2000; Slovic, Finucane, Peters and MacGregor, 2002; Kuhnen and Knutson, 2011).

Motivated by this literature in psychology, we investigate whether equity analysts exposed to extreme negative events such as mass shootings and terrorist attacks issue relatively more pessimistic earnings forecasts. We focus on the behavior of sell-side analysts as they are sophisticated information intermediaries and their earnings forecasts significantly influence the investment decisions of both retail and institutional investors. Analyst forecasts also affect the speed with which security prices incorporate new information.

Previous studies have examined whether analyst forecasts are affected by various behavioral biases and whether those potentially biased analyst forecasts have any effect on asset prices (e.g., DeBondt and Thaler 1990; Hilary and Menzly, 2006; DeHaan, Madsen and Piotroski, 2015). Our study adds to this literature by examining whether extreme negative events that are *exogenous* to corporate earnings influence analyst sentiment and, consequently, their earnings forecasts. This unique economic setup allows us to examine whether psychological factors can affect the behavior of sophisticated market participants who are more likely to influence asset prices. If equity analysts are unbiased aggregators of information, exogenous shocks generated by terrorist attacks that are unlikely to affect firm performance should not influence their forecasts of corporate earnings.

We identify analysts who are more likely to be affected by the terrorist attacks by measuring the distance between the locations of all analysts and the locations of extreme negative events. Our conjecture is that analysts who are located closer to terrorist attacks are likely to perceive these events as more salient as they are more likely to interact with (or hear about) people who are more directly affected. This can create a more "personal" connection between "local" analysts and the event and may generate a stronger negative shock to their sentiment. This conjecture is motivated by previous studies in psychology, which show that terrorism events generate a larger shock to the sentiment of the local community (Vlahov, Galea, Resnick, Ahern, Boscarino, Bucuvalas, Gold and Kilpatrick, 2002; Galea et al., 2002; Hughes et al., 2011).[3]

To test the hypothesis that proximity to terrorist attacks affects analysts' earnings forecasts more strongly, our econometric models compare the forecasts of local analysts and analysts who are located farther away. Specifically, we compare the local and non-local analyst forecasts *for the same firm* within a window around the terrorist attacks. We expect the forecasts of local analysts to be relatively more pessimistic. This empirical framework allows for a relatively accurate test of our main hypothesis since we can capture differences in the forecasts of the affected group (analysts who are local to terrorist attacks) and the unaffected group (analysts who are non-local to the attacks). The two groups are exposed to the *same* fundamental information about firm earnings but *only* differ in their exposure to the terrorist attacks, and, therefore, the shock to their sentiment.[4]

In our empirical analysis, we use Thomson Reuters' Institutional Brokers Estimate System (I/B/E/S) and analyze a large sample of quarterly earnings forecasts for the 1994-2013 period. We collect information on the dates and locations of terrorist attacks from the Global Terrorism Database (GTD), and for mass shootings from The Washington Post list (WP), which provides details on the deadliest shootings in U.S. history. After applying several filters to ensure that we focus on major events that are likely to affect analyst sentiment, we end up with a sample of 28 extreme events.[5] To identify local analysts, we use hand-collected data to measure the distance between the location of the attacks and the location of the brokerage house where analysts are employed. In our baseline models, analysts who are employed by

[3] An article published by *Daily Mail* in the U.K. (http://www.dailymail.co.uk/news/article-2870512/In-Newtown-mental-health-problems-emerging.html) discussed the mental health issues faced by residents in Newtown, Connecticut two years after the terrorist attacks in Sandy Hook elementary school. Such anecdotal evidence further supports the notion that terrorist attacks and mass shootings are likely to exert a stronger impact on the sentiment of the local community.

[4] Kothari (2001) suggests that "apparent" biases in analyst forecasts may arise artificially due to different data definitions and treatments across databases, a general survivorship bias, or the effect of firm characteristics on the proclivity of analysts to revise their forecasts. We can rule out such concerns because our tests compare the forecasts of affected and unaffected analysts for the same company at the same time.

[5] In our robustness tests, we also consider a larger sample of negative events identified using less stringent criteria for inclusion in the event sample. As expected, our results are similar but weaker.

brokerage firms located within a 100-mile radius of the attack are identified as local or affected.

The empirical results are consistent with our broad conjecture. Using logit models we find that affected analysts who are local to terrorist attacks are 2.93% more likely to issue forecasts that are below the consensus during the 90-day period after the attacks. This effect is large compared to other attributes that affect the likelihood of a pessimistic forecast. Specifically, among the eight variables in our regression model that significantly affect the likelihood of a pessimistic forecast, the economic impact of terrorism on affected analysts ranks third, only behind forecast horizon and lagged accuracy. We obtain similar findings when we use continuous measures of pessimism in ordinary least squares regressions. These results are highly statistically significant, supporting our conjecture that proximity to terrorist attacks increase analyst pessimism.

Further, we find that affected analysts are more likely to issue bold pessimistic forecasts (i.e., forecasts that are below their last forecast and the consensus) and less likely to issue bold optimistic forecasts (i.e., forecasts that are above their last forecast and the consensus), as compared to their propensity to issue herding forecasts. Since herding forecasts tend to reflect corrections to analysts' previous opinions, whereas bold forecasts reflect analysts' efforts to bring new information into the market, this finding shows that exposure to terrorist attacks and mass shootings has a negative impact on the earnings expectations of affected analysts.

In additional tests that examine the role of geography and timing of forecasts, we find that the distance between analysts and attacks is negatively related to the likelihood of a pessimistic forecast. The marginal effect associated with the issuance of a pessimistic forecast for analysts located within a radius of 0-50 miles from the attacks is 2.97%, and the effect decreases to 2.46% (0.84%) for analysts within a 51-100 mile (101-150 miles) radius. The timing of forecasts has a similar effect, as we find that the forecasts by local analysts issued between 0-30 days after the attacks have a 4.83% chance of being more pessimistic, which decreases to 2.07% (-0.01%) for forecasts issued between 31-90 (91-180) days after the attacks. These effects are consistent with our hypothesis, since attacks that are geographically and temporally closer to analysts are more salient, and they are likely to induce a larger adverse shock to analyst sentiment.

Our next set of geography-based tests are motivated by the evidence in psychology, which finds that individuals exhibit a stronger emotional reaction to violence if they have previously been less exposed to such stimuli (e.g., Anderson and Dill, 2000; Krahé, Möller, Huesmann, Kirwil, Felber, and Berger, 2011). Specifically, we conjecture that affected analysts who are located in states with lower murder rates are likely to issue more pessimistic forecasts than affected analysts who reside in states with higher murder rates. The shock element generated by the extreme negative events is likely to be higher among analysts in states with low murder rates. Our results support this conjecture, as we find that affected analysts in low murder-rate states are 3.54% more likely to issue pessimistic forecasts. In contrast, affected analysts in high murder-rate states are not significantly affected by terrorist attacks.

Next, we examine whether terrorist attacks influence the forecast accuracy of affected analysts in absolute terms, while controlling for various analyst-related characteristics and additional fixed effects. Our results show that affected analysts issue *more* accurate forecasts, which is perhaps not surprising since exposure to terrorist attacks encourages pessimistic forecasts and therefore counterbalances the known tendency of analysts to issue inaccurate forecasts due to optimism related to their career concerns (Lin and McNichols, 1998; Michaely and Womack, 1999; Hong and Kubik, 2003).

To further ensure that our results reflect the impact of terrorist attacks on analyst sentiment, we examine the behavior of analysts around anniversaries of terrorist attacks. Ceremonies are typically held at the location of terrorist attacks to commemorate the victims, and those events that remind local individuals of past negative experiences are likely to trigger further emotional reactions. To test this possibility, we examine whether affected analysts become more pessimistic relative to non-affected analysts around the anniversaries of terrorist attacks.

Our results show that there is a significant one-year anniversary effect among analysts who are geographically as well as temporally closer to an attack and reside in states with low murder rates. The anniversary effect is roughly one-half of the effect associated with the occurrence of the terrorist attack. This finding suggests that the impact of extreme negative events on analyst sentiment is not entirely transitory. In addition, these results indicate that our results are unlikely to reflect the effect of other factors correlated with the occurrence of terrorist attacks.

Even though our models control for several variables that are known to affect the propensity of analysts to be pessimistic, we conduct several checks to ensure that our findings are robust. To control for potential fundamental effects, we test whether the pessimism we document only exists around attack periods, or whether it reflects pre-existing (and confounding) trends. We do not find any evidence that affected and non-affected analysts differ in their forecasts *prior* to the attacks. We also repeat our analysis using a state-level macroeconomic index as an additional control variable, which should capture any potential impact of the local macro-economy on the forecasting behavior of affected analysts. The inclusion of this index does not change any of our findings. Further, we find that our results are robust when we consider alternative sample specifications or estimate models that account for unobserved heterogeneity through additional fixed effects.

These results contribute to the accounting and finance literature that examines whether analyst forecasts are affected by various behavioral biases. DeBondt and Thaler (1990) demonstrate that analysts are influenced by the representativeness heuristic and overreact to past earnings information, while Easterwood and Nutt (1999) find that analysts underreact to negative information and overreact to positive information. We contribute to this literature by showing that analysts overreact/underreact not only to corporate news but also to events, such as terrorism attacks, which are exogenous to the economic fundamentals of firms.

More recently, Hilary and Menzly (2006) show that analyst forecasts are affected by overconfidence generated by biased self-attribution, and Cen, Hilary and Wei (2013) show that analyst forecasts exhibit an industry-related anchoring bias (see also Jiang, Kumar and Law, 2016). In addition, DeHaan, Madsen and Piotroski (2015) show that variations in sunlight, which can affect attitude toward risk, affect the speed with which analysts incorporate earnings-related information in their forecasts. We contribute to this literature by using unpredictable events that tend to occur at random locations and time periods provides, such as terrorist attacks, to test directly whether an exogenous stimuli to the sentiment of sell-side analysts can affect their earnings forecasts. This framework allows us to minimize any self-selection bias related to the location preferences of sell-side analysts.

We also contribute to the broader literature that analyzes the economic impact of terrorist attacks and mass shootings. Ahern (2012) shows that terrorist attacks influence various psychological indicators of well-being and macroeconomic activity. Di Tella and Schargrodsky (2004) and Gould and Stecklov (2009) show that terrorist attacks alter government policies, while Gould and Klor (2010) and Montalvo (2011) demonstrate that terrorism-related events influence political views and election outcomes, respectively. Most recently, Antoniou, Kumar and Maligkris (2016a) show that terrorist attacks and mass shootings, through their adverse effect on managerial sentiment, influence the riskiness of publicly-traded firms. Our study contributes to this literature by showing that terrorist attacks and mass shootings influence analyst forecasts and the information dissemination process in financial markets.

The rest of the paper is organized as follows. We define our data sources and the main measures in Section 1.2. The main empirical results are presented in Section 1.3. In Section 1.4, we present results from various robustness tests and examine alternative explanations for our findings. We conclude in Section 1.5 with a brief summary.

1.2. Data and Methods

1.2.1. Terrorist Attacks and Mass Shootings Data

We obtain data on terrorist attacks and mass shootings from the Global Terrorism Database (GTD)[6] and The Washington Post list (WP),[7] respectively. GTD is an open-source database that contains systematic data on terrorist attacks (START, 2013),[8] while WP illustrates information regarding the deadliest shootings in U.S. history. We obtain data regarding the location and the date of each event, covering the period 1994-2013. Since GTD includes information on terrorist attacks around the world, we eliminate any events that have occurred outside the U.S. Further, we consider only events that caused human casualties and were covered in newspaper articles,[9] since these events are more likely to affect sentiment. From the resulting list, we eliminate 3 duplicate events, 2 events that involved robberies,[10] 12 events for which we could not validate an exact location or the motive for the attack, and 10 events for which there are no local analysts around the period of the attacks.[11]

[6] The data are available at http://www.start.umd.edu/gtd/.

[7] The data are available at http://www.washingtonpost.com/wp-srv/special/nation/deadliest-us-shootings/.

[8] To consider an event as terrorist attack and distinguish it from common criminal activities, we apply the following filters as they appear in GTD: Firstly, "The act must be aimed at attaining a political, economic, religious, or social goal"; Secondly, "There must be evidence of an intention to coerce, intimidate, or convey some other message to a larger audience (or audiences) than the immediate victims"; And thirdly, "The action must be outside the context of legitimate warfare activities, i.e. the act must be outside the parameters permitted by international humanitarian law (particularly the admonition against deliberately targeting civilians or non-combatants)".

[9] We consider all events covered in at least one major U.S. outlet (*The Los Angeles Daily News, The NY Daily News, The NY Post, The NY Times, The Wall Street Journal-US edition, The Washington Post and USA Today*) during the next 7 days after the event.

[10] Since we aim to examine the impact of unpredictable and salient events, we exclude robberies, which reflect common criminal activity.

[11] In Chapter 3 of this Thesis, I use Google Trends to validate whether terrorist attacks and mass shootings are able to capture the attention of local population during the sample period 2004-2010. Since in both Chapters, I used the same filters for the events (i.e. occurred in the US, had at least one human casualty, and were covered in major national outlets), we can infer that the events included in the sample of this Chapter during the 2004-2010 period are also able to draw the attention of individuals around that period.

Table 1.1 lists the 28 events during the 1994-2013 period that are included in our final sample. Figure 1.1 shows their geographical dispersion. These attacks are spread all across the country, and do not exhibit any obvious regional clustering.

1.2.2. Analyst Forecasts

We obtain information on quarterly analyst forecasts for U.S. firms traded on the NYSE, Amex or NASDAQ from Thomson Reuters' Institutional Brokers Estimate System (I/B/E/S). We delete from our sample forecasts for firms where the corresponding stock price information in the Center for Research in Security Prices (CRSP) database is missing. We also exclude forecasts made by unidentified analysts (i.e., analyst identifier equal to 0) and forecasts for stocks with reported earnings measured in a currency other than U.S. dollars. Similar to Easton and Sommers (2007), Malmendier and Shanthikumar (2014) and Jiang et al. (2016), our sample period starts in 1994 where I/B/E/S data accuracy improves, and extends until 2013.

As is common in the analyst literature, we retain only the last forecast made by each analyst for each company and each quarter (Hong and Kubik, 2003; Jegadeesh, Kim, Krische and Lee, 2004; Clement and Tse, 2005). Similar to Lim (2001) and Bernhardt, Campello and Kutsoati (2006), we filter for potential entry errors by deleting any forecast with an absolute forecast error (forecast minus actual earnings, scaled by the previous month-end stock price) greater than 10. To mitigate the influence of outliers, we keep only forecasts for firms with average share price higher than 5 dollars (Chen and Jiang, 2006; Cen et al., 2013; Malmendier and Shanthikumar, 2014). Similar to Hilary and Hsu (2013), we eliminate all forecasts for firms that are covered by fewer than five analysts. Finally, we retain forecasts with maximum (minimum) horizon of 100 (2) days from the earnings announcement to minimize the effect of stale forecasts and information leakage (Jegadeesh et al., 2004; Jackson, 2005).

Our final sample consists of 486,186 forecasts issued by 4,674 analysts for 3,299 firms during the 1994-2013 period. Figure 1.2 illustrates the distribution of these forecasts across different states. Consistent with the findings in Malloy (2005) 51.50% of analysts are located in the state of New York and their forecasts make up 56.32% of the total number of forecasts.

1.2.3. Variable Definitions and Econometric Models

To capture the likelihood of affected analysts issuing pessimistic earnings forecasts following a terrorist attack, we use the following logit estimator:

$$\mathrm{P}\left(\mathrm{Pessimism}_{\mathrm{i,j,t}} \mid \mathrm{Z}_{\mathrm{i,j,t}}\right) = \mathrm{F}(\mathrm{Z}_{\mathrm{i,j,t}}) \tag{1}$$

where *i* indexes analyst, *j* indexes firm, and *t* indexes time (quarter). $Pessimism_{i,j,t}$ is a dummy variable equal to one if the forecast of analyst *i* is less than the consensus forecast of analysts who cover the same firm *j* at the same quarter. As in Hong, Kubik, and Solomon (2000), the consensus forecast is equal to $\bar{\mathrm{F}}_{-\mathrm{i,j,t}} = \frac{1}{\mathrm{N}_{-\mathrm{i,j,t}}} \sum \mathrm{F}_{-\mathrm{i,j,t}}$ where $N_{-i,j,t}$ is the set of all analysts excluding analyst *i*, $\sum F_{-i,j,t}$ is the summation of earnings forecast values of all analysts except analyst *i* who cover firm *j* in time *t*, and $\bar{F}_{-i,j,t}$ is the average forecast value of all analysts except analyst *i*. Additionally, this model uses the cumulative standard logistic distribution *F*, and $Z_{i,j,t}$ takes the following form:

$$\mathrm{Z}_{\mathrm{i,j,t}} = \mathrm{c} + \alpha_{\mathrm{state}} + \delta_{\mathrm{time}} + \beta \times \mathrm{Impact}_{\mathrm{i,t}} + \gamma \times \mathrm{X}_{\mathrm{i,j,t}} + \varepsilon_{\mathrm{i,j,t}} \tag{2}$$

Our main variable of interest is $Impact_{i,t}$ which is a dummy variable that equals one if the distance between the location of the analyst and the location of the attack is less than 100 miles, and the forecast is issued during the 90-day period following the terrorist attack. To calculate the distance between the analyst and the event locations, we use hand-collected data on the coordinates of these locations and follow the procedure in Vincenty (1975).[12] We obtain the coordinates of terrorist

[12] Malloy (2005) uses a similar procedure to identify analysts that are local to a firm.

attacks using their address and the service called "GPS Geoplaner".[13] To find the coordinates of each analyst's location (measured at the city center where the branch office is located),[14] we use Gazetteer Files from the U.S. Census Bureau.

Our models control for a number of forecast, analyst and broker characteristics that may be associated with analyst forecasts, indicated in equation (2) as $X_{i,j,t}$. Specifically, we control for the *Horizon* (the days that separate the forecast from analyst *i* for company *j* at time *t* with the corresponding earnings announcement date for company *j*), *Brokerage Size* (the number of analysts employed in the brokerage of analyst *i* at time *t*), *Lagged Accuracy* (measured as the lagged value of *Accuracy* for analyst *i*'s forecast for company *j* at time *t*-1), analyst's general experience (i.e., $Experience_{General}$, measured as the number of years since analyst *i'*s forecast for company *j* at time *t* and the first forecast by analyst *i* for any company in the IBES database), firm-specific experience (i.e., $Experience_{Firm}$, measured as the number of years analyst *i* covers firm *j*), and the number of industries that an analyst follows (i.e., *Industries*, defined as the number of two-digit SIC codes analyst *i* covers at time *t*) (Clement and Tse, 2005; Cohen, Frazzini and Malloy, 2010; Walther and Willis, 2013).

Malloy (2005) finds that analysts who work near the firms they cover are more accurate, which may affect their propensity to issue pessimistic forecasts. To control for such effects, we include the variable *Local* in our regression specification, which is the distance between analyst and firm locations.[15] Following the findings in Kumar (2010) that analysts' gender affects their forecasts, we include the dummy variable *Female*. Finally, to further control for analysts' ability and reputation, we include in the regression specification the *All-Star* dummy variable, which equals to one if the

[13] "GPS Geoplaner" is available at http://www.geoplaner.com/.

[14] To retrieve the city of location for each branch, we expand an initial dataset used in Jiang et al. (2016).

[15] We obtain the coordinates of firms by matching their ZIP codes with the Gazetteer Files from the U.S. Census Bureau. We drop from our sample firms with missing ZIP codes. To calculate the distance between the firms and the analysts, we follow the same procedure as described before in relation to analysts and attacks.

analyst is ranked as first, second, third, or runner-up in the Institutional Investor Magazine in the previous year.[16]

In addition to these control variables, our specifications include analyst location (state) and time (year-quarter) fixed effects, indicated in equation (2) with α_{state} and δ_{time}, respectively. State fixed effects capture systematic variation in analyst behavior across states,[17] whereas time fixed effects capture systematic variation related to macroeconomic shocks. Furthermore, we cluster the error term at the analyst location level (state) to further account for potential dependencies in analyst behavior that is related to their location.

We additionally conduct analysis with continuous measures of pessimism, using the following model estimated with ordinary least squares:

$$\mathrm{Y_{i,j,t}} = \mathrm{c} + \alpha_{\mathrm{state}} + \delta_{\mathrm{time}} + \beta \times \mathrm{Impact_{i,t}} + \gamma \times \mathrm{X_{i,j,t}} + \varepsilon_{\mathrm{i,j,t}}. \qquad (3)$$

In equation (3) $Y_{i,j,t}$ is a continuous measure of pessimism for analyst *i*'s forecast for firm *j* at time *t*. We construct two such measures, namely *Relative Pessimism* and *Rank Pessimism*. *Relative Pessimism* is equal to the difference between the consensus forecast for firm *j* at time *t* minus the forecast of analyst *i*. Increases in *Relative Pessimism* indicate that analyst *i* is more pessimistic relative to his peers in his forecast for firm *j* at time *t*.

Following Clement and Tse (2005), we scale each of the continuous variables in our models to a range from 0 to 1 to make their coefficients comparable. This transformation preserves the relative distances in each forecast characteristic and takes the following form:

$$\mathrm{X_{i,j,t}} = \frac{\mathrm{X_{i,j,t}} - \mathrm{Xmin_{j,t}}}{\mathrm{Xmax_{j,t}} - \mathrm{Xmin_{j,t}}}, \qquad (4)$$

[16] The data on analysts' gender and all-star status are from Kumar (2010) and Jiang et al. (2016). We update these databases for our sample period by hand-collecting data following their method.
[17] The macroeconomic environment in analysts' home state is likely to be correlated with their income, and thus can affect their propensity to take risk through their forecasts.

where $Xmax_{j,t}$ and $Xmin_{j,t}$ are the maximum and the minimum values of X for each firm *j* at time *t*. As in Clement and Tse (2005), we define *Accuracy* as:

$$\mathrm{Accuracy_{i,j,t}} = \frac{Max(AFE)_{j,t} - AFE_{i,j,t}}{Max(AFE)_{j,t} - Min(AFE)_{j,t}}, \tag{5}$$

where $AFE_{i,j,t}$ is equal to the absolute value of FE, and $AFEmax_{j,t}$ and $AFEmin_{j,t}$ are the maximum and minimum *FE*'s for firm *j* at time *t*, respectively.

The third measure we construct, *Rank Pessimism*, is defined similarly to Hong and Kubik (2003), and is constructed on the basis of how analyst *i*'s pessimism for firm *j* at time *t* ranks in the distribution of all forecasts for this particular firm and quarter. The advantage of this measure is that it is less sensitive to outliers, since it is based on rankings. To calculate *Rank Pessimism* we first compute the forecast error (*FE*) for analyst *i* for firm *j* at time *t*,[18] and then sort all forecasts for firm *j* at time *t* based on this value. *Rank Pessimism* is equal to the ranking minus one, divided by the number of analysts covering firm *j* at time *t* minus one. A higher ranking value reflects that analyst *i* is relatively more pessimistic.[19] The specification in equation (3) contains the same set of control variables as the regression specification in equation (2), which are standardized according to the scheme shown in (4) and (5).

1.3. Main Empirical Results

In this section, we present our main empirical results. We begin our analysis by examining whether affected analysts issue more pessimistic earnings forecasts around terrorist attack periods. In addition, we test whether more salient events have a stronger impact on the forecasting behavior of affected analysts, and finally whether

[18] The forecast error FE is defined as $\frac{Forecasted\ Earnings_{i,j,t} - Actual\ Earnings_{j,t}}{Stock\ Price_{j,t}}$. The stock price is measured at the end of the previous month in which the forecast is issued.

[19] If two or more analysts were equally pessimistic, we assign the midpoint value of the ranks to all those analysts.

any variation in the forecasting behavior of affected analysts has a significant effect on their forecast accuracy.

1.3.1. Descriptive Statistics

To begin, Table 1.2 presents the descriptive statistics for our sample. Panel A contains information on the number of forecasts, analysts, brokers and stocks that we include in our sample each year. We observe an increase in the number of analysts and the number of covered firms over time. Also, the results in Panel A indicate that the proportion of affected forecasts is significantly lower during the 2004-2013 period.

This table also shows the descriptive statistics for the variables we consider in the main specifications. More specifically, in Panel B of Table 1.2, we observe that the average forecast error (*FE)* across the whole sample is positive, which suggests that analysts are on average optimistic. The average number of analysts following a firm at a specific quarter is approximately 11. Affected analysts provide 2% of the total forecasts in our sample. Finally, *Female* and *All-Star* analysts correspond to 12% and 19% of our sample, respectively.

Figure 1.3 shows preliminary evidence that affected stock analysts tend to provide on average more pessimistic forecasts. The increase in the level of pessimism is statistically significant over the entire 1994 to 2013 period, and robust to all three proxies we use to capture pessimism. These results are consistent with our hypothesis, which we formally test in the next section.

1.3.2. Terrorist Attacks and Analyst Pessimism

We present our main empirical results in Table 1.3. We report the marginal effects from logit models in columns (1) to (3), and results from OLS regressions using the continuous measures of pessimism (i.e., *Relative Pessimism* and *Rank Pessimism*) in columns (4) to (9). In each specification, we add the control variables sequentially. Throughout the paper, we discuss the results from models that include all the control variables, which are shown in columns (3), (6) and (9) of Table 1.3.

Consistent with our hypothesis, we find that affected analysts (i.e., analysts who are local to terrorist attacks and issue forecasts during the 90-day period following the event) are more likely to be pessimistic. Specifically, as shown in column (3), affected analysts are 2.93% more likely to issue a forecast below the consensus, relative to non-affected analysts. This effect is statistically significant at the 1% level and ranks third in terms of economic impact among all the control variables in the model. Only *Horizon* and *Lagged accuracy* variables, with marginal effects of -3.60% and -8.18% respectively, have stronger economic impact.

Our hypothesis is also supported by the results in columns (6) and (9), where we consider the two continuous measures of pessimism: *Relative Pessimism* and *Rank Pessimism. Impact* has the expected positive sign and is statistically significant at 1% level. Moreover, in these specifications, *Impact* ranks third among all variables in terms of the magnitude of the coefficient estimate, behind *Horizon*, and *Lagged Accuracy*.

Examining the estimates of control variables, we find that in all three specifications, our results are comparable to those reported in previous studies. Specifically, in line with the results in Malloy (2005), Cowen, Groysberg and Healy (2006), and Walther and Willis (2013), *Horizon* is negatively related to pessimism. Further, *Lagged Accuracy* and $Experience_{Firm}$ are negatively related to pessimism, while $Experience_{General}$ is positively related to pessimism. Consistent with Lim (2001) and Walther and Willis (2013), we find that analysts who work in larger brokerage houses tend to be more pessimistic. Finally, our results show that female analysts tend to be more pessimistic, while *All-Star* analysts are somewhat less pessimistic.

1.3.3. Analyst Pessimism and Bold Forecasts

In the next set of tests, we examine whether affected analysts exhibit a greater propensity to issue bold forecasts, as opposed to herding forecasts. A bold pessimistic (optimistic) forecast is below (above) the analyst's previous forecast for the same period and the same company, *and* below (above) the consensus analyst forecast. Given our key hypothesis, we expect that affected analysts will be more pessimistic than the consensus. Therefore, those analysts are more likely to issue pessimistic bold forecasts and less likely to issue optimistic bold forecasts.

To test this conjecture, we use a multinomial logit model, where the dependent variable takes the following three values: one, zero and minus one for bold pessimistic, herding, and bold optimistic forecasts, respectively. In our estimation, we treat herding forecasts as the reference category. We report the regression estimates in columns (1) and (2) of Table 1.4 where *Forecast Type* takes the value of one in column (1) and minus one in column (2). Our findings show that affected analysts are 2.76% more likely to issue bold pessimistic forecasts and 1.70% less likely to issue bold optimistic forecasts, compared to their propensity to issue herding forecasts.[20]

In column (3) of Table 1.4, we estimate a binary logit model where the dependent variable, *Bold Pes*, is a dummy variable that equals one for bold pessimistic forecasts. In this specification, the reference category is all the non-bold pessimistic forecasts, which includes herding and bold optimistic forecasts. The results in column (3) show that affected analysts are 2.77% more likely to issue bold pessimistic forecasts.

Overall, using various measures of pessimism, we show that affected analysts who reside near terrorist attacks issue more pessimistic forecasts around attack periods, relative to analysts who are situated farther away.

[20] Our results are robust when we loose the IIA assumption and use a multinomial probit model.

1.3.4. Event Salience and Analyst Pessimism

Our key conjecture is that analysts who are affected by terrorist attacks will perceive these events as more salient, which will negatively impact their sentiment and induce them to issue more pessimistic forecasts. In this section, we characterize the saliency of terrorist attacks using different proxies, and test whether forecasts associated with more salient events are more pessimistic.

First, the geographical distance between an analyst and the location of the terrorist attacks should be related to perceived saliency, since analysts who are closer to an event are likely to have a more “personal” experience of the event (e.g., see Galea et al., 2002). Temporal proximity to the attack is likely to have a similar effect, as recent events are likely to be perceived as more salient than events farther back in time. In Table 1.5, we test these conjectures by allowing for different coefficient estimates of *Impact* for various distance (Table 1.5, Panel A) and time (Table 1.5, Panel B) specifications.

In Panel A, we examine whether the coefficient on *Impact* changes for analysts who reside within a 0-50 mile radius (and 51-100 or 101-200 mile radius) around the event. In line with our hypothesis, the results from the logit model in column (1) show that analysts who reside within 0-50 mile radius from an event have a 2.97% probability of issuing a more pessimistic forecast. This probability decreases to 2.46% (0.84%) for analysts who reside in a radius of 51-100 (101-200) miles. Similar conclusions hold in columns (2) and (3), where the dependent variable is a continuous measure of pessimism (i.e., *Relative Pessimism* and *Rank Pessimism*). Although the test statistics shown in the last row of Panel A indicate that the difference in the coefficients for the farthest and closest analysts is not statistically significant, the monotonic pattern obtained for the different distance specifications is in the direction predicted by our hypothesis.

Panel B of Table 1.5 shows the results related to temporal proximity, which are also in line with our hypothesis. The logit model in column (1) shows that affected analysts (100-mile radius) are 4.83% more likely to issue a pessimistic forecast in the 30-day period after the attack. This effect decreases to 2.07% in the 31-90 day period after the attack, decreasing further to an insignificant -0.01% in the 91-180 day period after the attack. Similar conclusions hold in columns (2) and (3), where the dependent variable is a continuous measure of pessimism (*Relative Pessimism* and *Rank Pessimism*, respectively). The test statistics shown in the last row of Panel B indicate a significant difference in the coefficients of *Impact* between recent and older forecasts.

People are known to exhibit a stronger emotional reaction when they experience a highly unexpected event (Mellers, Schwartz and Ritov, 1999; Wilson, Centerbar, Kermer and Gilbert, 2005). In line with this view, several studies have shown that people tend to be desensitized to repeated violent stimuli (Averill, Malstrom, Koriat and Lazarus, 1972; Anderson and Dill, 2000; Krahé et al., 2011). Based on this evidence, we conjecture that affected analysts located in states with low murder rates will perceive terrorist attacks and mass shootings as more salient and issue more pessimistic forecasts. In comparison, affected analysts who resided in states with high murder rates will exhibit a relatively lower degree of pessimism.

To test our hypothesis, we obtain information on the murder rates of each state from FBI's Uniform Crime Reporting (UCR).[21] We divide the number of murders with the population of the state to measure the level of murder activity in each state, and to compare the murder activity between states, we compute the average murder rate for each year. We define a dummy variable $Murder_h$, which is equal to one if the state of analyst's location has a higher murder rate than the average rate across U.S. states in a given year. Accordingly, $Murder_l$ is a dummy variable equal to one if $Murder_h$ is equal to zero.

Table 1.6 reports the results. Consistent with our hypothesis, the logit model in column (1), shows that affected analysts who are located in states with low murder rates are 3.54% more likely to issue pessimistic forecasts, whereas affected analysts who reside in states with high murder states are 0.62% more likely to issue pessimistic forecasts. Similar conclusions hold in columns (2) and (3), where the

[21] UCR defines as murder "the willful (non-negligent) killing of one human being by another". For further information, refer to https://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2013/crime-in-the-u.s.-2013/violent-crime/murder-topic-page/murdermain_final.

dependent variable is a continuous measure of pessimism (*Relative Pessimism* and *Rank Pessimism*, respectively). The test statistics shown at the last row of Table 1.6 indicate that affected analysts who reside in states with low murder rates are significantly more pessimistic than affected analysts who reside in states with high murder rates.

Overall, the results in this section provide additional support for our main hypothesis and show that events that are likely to be perceived as more salient generate more pessimistic earnings forecasts.

1.3.5. Terrorist Attacks and Forecast Accuracy

In this section, we examine the accuracy of forecasts issued by affected analysts in absolute value. If affected analysts become too pessimistic, their forecasts may become less accurate. However, if terrorist attacks only generate a moderate degree of pessimism among affected analysts, their forecasts may be more accurate since analysts are known to be optimistic due to issues related to career-concerns (Hong and Kubik, 2003). The results from forecast accuracy regressions in Table 1.7 indicate that affected analysts are *more* accurate than non-affected analysts, thus support the latter view. This finding is robust to different model specifications with analyst and forecast related control variables and various fixed-effect controls.

Examining the estimates of the control variables, we find that analyst forecasts with longer horizon, and forecasts from analysts who cover more industries are less accurate. On the contrary, *Brokerage size*, *Lagged accuracy*, and *All-Star* exhibit a positive correlation with the level of *Accuracy*. These results are in line with the findings in the previous literature (e.g., Malloy, 2005; Clement and Tse, 2005; Kumar, 2010; Jiang et al., 2016).

1.3.6. Anniversaries of Terrorist Attacks and Analyst Pessimism

During the anniversaries of terrorist attacks various memorial services are held at the attack locations to commemorate the victims.[22] Such events will to some extent evoke recollection of the negative experience associated with the attack, and may exert a negative shock on the sentiment of the local community.[23] In this section, we examine whether local analysts issue more pessimistic forecasts around the anniversaries of terrorist attacks.

Table 1.8 Panel A reports the results related to the first anniversary. In Panel A1, we define affected forecasts as those issued by analysts those who reside within a 100-mile radius of the attack and issued in the 90-day window after the first anniversary of the attack. In Panel A2, we define affected forecasts as those that are issued by analysts who are both geographically and temporally closer to the anniversary of the attacks (50-mile radius and 30-days after the anniversary). Our results in Table 1.5 already show that events that are closer to analysts are more salient and thus associated with more pessimistic earnings forecasts.

The results in Panel A1 indicate that anniversaries of negative events typically do not influence the pessimism of local analysts. However, the results in Panel A2 for the sample of more salient events show a significant anniversary effect. In this set, affected analysts are 2.51% more likely to issue a pessimistic forecast around the anniversary of the attack. This effect is roughly half of the effect associated with the actual terrorist attack, which is expected since the shock to the sentiment of the analyst around the anniversary of the event is likely to be weaker.[24] The analysis with *Relative Pessimism* as the dependent variable also reveals a significant effect in the

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[22] See, for example, http://edition.cnn.com/2002/US/09/11/ar911.memorial.newyork/, http://edition.cnn.com/2010/US/11/05/texas.fort.hood.anniversary/, http://www.usatoday.com/story/news/nation/2013/12/14/newtown-sandy-hook-shooting-anniversary/4022649/, http://edition.cnn.com/2014/04/15/us/boston-marathon-bombing-anniversary/.

[23] For a review of the psychological literature on the retrieval of emotional memories see Buchanan (2007).

[24] For all the tests we conduct in this section, we perform a Wald test to examine whether the coefficients on *Impact* and $Impact_{(anniversary)}$ are statistically different. In all cases, we find that they are, with *Impact* having a coefficient roughly twice as large.

expected direction. In Panel B, we conduct the same analysis for the second anniversary, and find that it has no effect on analyst pessimism.

In Panel C, we conduct the same analysis, but further account for event salience using the murder rate in the state of the analyst, similar to the analysis in Table 1.6. For this subsample, we find even stronger results for anniversaries for analysts who reside in states with low murder rates. In all models, affected analysts are more pessimistic than non-affected analysts around anniversaries, and this effect is about half of the effect associated with the actual terrorist attacks. Consistent with our findings in Table 1.6, terrorist attacks do not influence the forecasts by affected analysts who reside in states with high murder rates where the sensitivity to terrorist events are likely to be lower.

Overall, the results in this section shows that anniversary effects are significant for terrorist attacks that are more salient (i.e., Panels A1, C1 and C2), and suggests that such extreme sentiment events can exert an impact on analyst behavior that is not entirely transitory.

1.4. Robustness Checks and Alternative Explanations

Our empirical results could neglect systematic differences between affected and non-affected analysts other than exposure to terrorist attacks and mass shootings. In this section, we present results from a series of tests to ensure that our findings are robust, and also rule out alternative explanations for our findings.

1.4.1. Pre-Existing Effects

In the first test, we examine whether the differences between affected and non-affected analysts exist only around terrorist attack periods, or reflect some pre-existing trends. Ruling out the existence of such trends is important since our hypothesis states that differences in analyst forecasts are due to the exposure of affected analysts to terrorist attacks which affect their sentiment. To examine this possibility, we include additional control variables in the model that capture the lagged values for *Impact*. The results in Panel A of Table 1.9 show that the lagged variables are *all* insignificant, while $Impact_{(0\ to\ 90\ days)}$ remains similar in magnitude as in Table 1.3 and significant at the 1% level. These results suggest that the differences in forecasts *only* exist following the terrorist attacks, and are relatively short-lived (as indicated in Table 1.5).

1.4.2. Analyst Pessimism or Macroeconomic Factors?

Another potential concern may be that terrorist events are related to the macroeconomic environment of the state in which they occur, which could subsequently affect the compensation of affected analysts, and thus their risk attitudes. Such shifts in risk attitudes could influence the pessimism in their forecasts. To control for the macroeconomic conditions in the home state, we repeat our baseline analysis after including the quarterly, state-level macroeconomic index at time *t*, proposed by Korniotis and Kumar (2013), as an additional control variable.[25] Panel B of Table 1.9 presents the results. We find that the estimate of *Impact* remains statistically significant at the 1% level across all model specifications. In contrast, the *Macro-state index* variable is statistically insignificant.[26]

1.4.3. Analyst Pessimism or Unobservable Factors?

To control for any systematic variation in pessimism due to factors related to analyst, brokerage houses, firms, or industry characteristics that our models omit, we repeat

[25] *Macro-state index* is available for a sample period extending from 1994 to 2012.
[26] For further robustness, in unreported analysis, we also control for the *Macro-state index* at time *t-1*. Similar to the reported results, we find that the coefficient estimate of *Impact* is highly significant, while the lagged *Macro-state index* remains insignificant.

our baseline analysis using various fixed-effect controls at these levels. As shown in Table 1.10, the inclusion of these fixed effects does not greatly influence the coefficient on *Impact*. For example, the results from the logit model, columns (1) to (4), show that the marginal effect associated with *Impact* ranges from 2.92% to 3.02% depending on the combination of fixed effects used, and is always statistically significant at the 1% level. Similar findings are shown in columns (5)-(12) where the dependent variable is *Relative Pessimism* and *Rank Pessimism*.

1.4.4. Other Empirical Specifications

Our next set of robustness checks examines whether our results are robust to different model specifications. In our main analysis, we follow prior studies and keep the last forecast of each analyst (Hong and Kubik, 2003; Jegadeesh et al., 2004; Clement and Tse, 2005). This allows us to compare the forecasts between affected and unaffected analysts for the same company and around the same time period. However, our hypothesis should also hold for other forecasts as well. To examine whether this is the case we perform an additional robustness test using the first forecast of each analyst for each company and quarter. The results are shown in Panel A of Table 1.11, and remain consistent with our hypothesis in these alternative specifications.

1.4.5. Sensitivity to 9/11 Attacks

To ensure that our findings are not driven only by the 9/11 attacks (by far the most significant events in our sample), we re-estimate our main specifications after excluding these events from our sample. The results in Panel B of Table 1.11 show that *Impact,* although somewhat reduced, remains highly statistically significant in all regression specifications.

1.4.6. Excluding New York Analysts

Analysts located in the state of New York provide the majority of our forecast sample (56.32%). To ensure that our results are not driven by the forecasts of analysts located in the state of New York or from attacks that occur in this area, we exclude from our sample all these forecasts and repeat our analysis. The results in Panel C of Table 1.11 show that *Impact* remains highly statistically significant even in the sample that excludes NY analysts.

1.4.7. Alternative Mass Shooting Sample

Our initial event sample includes mass shootings from the WP list, which contains the deadliest events in U.S. history (average of 12 casualties). We choose to focus on this sample because these events are the most likely to be perceived as salient by analysts. In this section, we examine the sensitivity of our results to increasing our sample to include less salient events with a smaller number of casualties.

To increase our mass shooting sample, we use data from Stanford's Mass Shootings in America database (MSA) and consider events with a minimum of four casualties during the 1994-2013 period. From the resulting list, we eliminate 21 events for which there are no local analysts around the period of the events.

Panel A of Table 1.12 lists the 38 additional events that are included in our sample. In Panel B, we examine whether our baseline results change when we include these less salient terrorist attacks. The results in Panel B show that our coefficient estimates are comparable to our baseline results when we include in our sample mass shootings with equal or more than eight human casualties. When we additionally include events with less human casualties, the magnitude of the coefficients and their statistical significance decreases, which is expected since these events are less salient.

Overall, the results from this analysis show that our baseline findings continue to hold when we expand our mass shootings sample, but get progressively weaker as we add events that are likely to be perceived as less salient.

1.5. Summary and Conclusions

This paper examines the effect of terrorist attacks and mass shootings on the earnings forecasts of sell-side equity analysts. We conjecture that analysts who are located near such events will experience a negative shock to their sentiment, which will induce a pessimistic bias in their earnings expectations. Our models test this hypothesis comparing the forecasts of affected analysts and non-affected analysts for the same company and time period. This method allows us to account for the fundamental information about earnings that is common to all analysts.

Consistent with our hypothesis, we find that sell-side equity analysts who are located near major terrorist attacks and mass shootings in the U.S. tend to issue more pessimistic forecasts than analysts who are located farther away during the period following the attacks. Specifically, we show that affected analysts are significantly more likely to issue a forecast that is below the consensus in comparison to non-affected analysts. Further, we find that these effects are stronger when the forecasts are associated with more salient events. We also find that the one-year anniversary of salient events is associated with increased pessimism among local analysts. The increase in analyst pessimism is associated with higher accuracy, since it partially mitigates the well-documented optimism bias among analysts.

Collectively, these findings complement the evidence from previous literature on analysts' behavioral biases. Our key contribution is to demonstrate that increased pessimism bias among local analysts following terrorist attacks and mass shootings influence their forecasts and the information dissemination process in financial markets. Future research could examine whether the decisions of other market participants, including buy-side analysts, are affected by exposure to these types of extreme negative events.

Table 1.1. Sample of Terrorist Events

This table shows our event sample during 1994-2013. We consider only events that took place in the U.S., resulted to at least one human casualty and were displayed in newspapers. Furthermore, we restrict our sample to only those attacks that appear to have analysts located in a 100 miles radius.

No	Event	Date	Location
1	Brooklyn Bridge	01 Mar 1994	New York City, NY
2	Unabomber - Thomas Mosser	10 Dec 1994	North Caldwell, NJ
3	Planned Parenthood Clinic	30 Dec 1994	Brookline, MA
4	Unabomber - Gilbert Murray	24 Apr 1995	Sacramento, CA
5	Olympic Park Bombing	27 Jul 1996	Atlanta, GA
6	Empire State Building	23 Feb 1997	New York City, NY
7	U.S. Capitol	24 Jul 1998	Washington, DC
8	Columbine High School	20 Apr 1999	Littleton, CO
9	Korean Methodist Church	04 Jul 1999	Bloomington, IN
10	9/11 Attacks: World Trade Center	11 Sep 2001	New York City, NY
11	9/11 Attacks: Hijacked Plane Crashed	11 Sep 2001	Alexandria, VA
12	9/11 Attacks: Hijacked Plane Crashed	11 Sep 2001	Somerset County,
13	Bank of America	05 Jan 2002	Tampa, FL
14	LA International Airport	04 Jul 2002	Los Angeles, CA
15	Seattle Jewish Federation	28 Jul 2006	Seattle, WA
16	Little Rock	01 Jun 2009	Little Rock, AR
17	Holocaust Museum	10 Jun 2009	Washington, DC
18	Fort Hood	05 Nov 2009	Killeen, TX
19	IRS Building	18 Feb 2010	Austin, TX
20	Pentagon	04 Mar 2010	Arlington, VA
21	Discovery Communications	01 Sep 2010	Silver Springs, MD
22	Aurora	20 Jul 2012	Aurora, CO
23	Sikh Temple	05 Aug 2012	Oak Creek, WI
24	Sandy Hook School	14 Dec 2012	Sandy Hook, CT
25	Boston Marathon Bombing	15 Apr 2013	Boston, MA
26	MIT Shooting	18 Apr 2013	Cambridge, MA
27	Navy Yard	16 Sep 2013	Washington, DC
28	Los Angeles Airport	01 Nov 2013	Los Angeles, CA

Table 1.2. Descriptive Statistics

This table presents statistics for the main variables used in the empirical analysis. The sample includes forecasts for stocks included in CRSP from the 1st quarter of 1994 to the 4th quarter of 2013. We consider only forecasts for firms located in the U.S.. Panel A provides sample information for each year of our sample. Panel B presents statistics of all the variables we consider in our specifications. To improve readability, we multiply *Forecast Error* by 100 and report the statistics.

Panel A: Distribution of Sample Across Years

Year	Forecasts	Analysts	Brokerage Houses	Stocks	Affected Forecasts	Affected Analysts
	(1)	(2)	(3)	(4)	(5)	(6)
1994	5,428	672	83	374	1,093	324
1995	10,636	960	97	584	1,246	388
1996	12,658	1,193	111	683	40	11
1997	16,010	1,564	142	834	2,714	634
1998	19,953	1,900	162	1,036	165	67
1999	22,483	2,058	160	1,136	26	12
2000	20,375	2,079	153	1,037	0	0
2001	23,431	2,104	137	1,060	4,261	967
2002	27,893	2,085	147	1,187	141	45
2003	32,060	2,045	182	1,278	0	0
2004	37,483	2,138	201	1,402	0	0
2005	39,549	2,153	205	1,505	0	0
2006	39,057	2,018	187	1,551	30	8
2007	36,730	1,828	180	1,544	0	0
2008	35,229	1,598	175	1,443	0	0
2009	31,382	1,272	149	1,260	583	87
2010	26,740	1,086	137	1,161	611	67
2011	21,005	917	119	980	0	0
2012	16,490	750	101	812	310	117
2013	11,594	610	89	680	2,065	337

Table 1.2—*Continued*

Panel B: Summary Statistics						
Variable	Obs.	Mean	Std. Dev.	10^{th} Pctl.	Median	90^{th} Pctl.
Pessimism	358,885	0.50	0.50	0.00	1.00	1.00
Relative Pessimism	358,885	0.50	0.36	0.00	0.50	1.00
Rank Pessimism	358,885	0.50	0.31	0.07	0.50	0.93
Forecast Error	358,885	0.01	10.36	-0.39	-0.03	0.19
No. of Analysts	358,885	10.64	4.55	6.00	10.00	17.00
Impact	358,885	0.02	0.15	0.00	0.00	0.00
Horizon	358,885	0.61	0.42	0.00	0.86	1.00
Brokerage size	358,885	0.46	0.34	0.00	0.44	1.00
Lagged accuracy	358,885	0.58	0.36	0.00	0.67	1.00
Experience $_{General}$	358,885	0.52	0.36	0.00	0.50	1.00
Experience $_{Firm}$	358,885	0.52	0.38	0.00	0.50	1.00
Industries	358,885	0.38	0.39	0.00	0.33	1.00
Local	358,885	0.52	0.43	0.00	0.55	1.00
Female	358,885	0.12	0.32	0.00	0.00	1.00
All-Star	358,885	0.19	0.40	0.00	0.00	1.00

Table 1.3. Terrorist Events and Analyst Pessimism: Baseline Estimation

This table presents the results from regressions examining the impact of attacks on forecast pessimism. The sample includes forecasts for U.S. stocks included in CRSP from 1994 to 2013. *Impact* is a dummy variable equal to one if the analyst who provided the forecast is inside a 100 miles radius from an attack, and the forecast took place during the next 90 days after the attack. To examine the effect of attacks on analysts' sentiment we construct three measures. *Pessimism* is equal to one if the forecast is less than the consensus forecast for firm *j* at time *t*, where the consensus forecast is equal to the average value of forecasts for firm *j* at time *t* without considering the forecasts of the current analyst *i*. *Relative Pessimism* is equal to the difference of consensus forecast for firm *j* at time *t* minus the actual forecast of analyst *i*. To construct *Rank Pessimism*, we first compute the forecast error (*FE*) for each analyst *i* for firm *j* at time *t*, where the *FE* is equal to the difference of the value of the forecast minus the actual earnings, scaled by the lagged monthly price of the firm. Then, we sort them such as a higher ranking value reflects a more pessimistic forecast. *Rank Pessimism* is equal to the ranking minus one, divided by the number of analysts covering firm *j* at time *t* minus one. *Horizon* is the number of days until the actual earnings announcement. *Brokerage size* is equal to the number of analysts who work in a brokerage house each quarter. We define *Accuracy* as the difference between the maximum absolute forecast error for each firm-quarter and the absolute forecast error of analyst divided by maximum absolute forecast error minus the minimum absolute forecast error, where the absolute forecast error is equal to the absolute difference of earnings forecast and earnings announcement scaled by the lagged monthly stock price. *Lagged accuracy* is the lag values of this measurement. $Experience_{General}$ is the number of years since an analyst first appeared in I/B/E/S. $Experience_{Firm}$ is the number of years an analyst covers a specific firm. *Industries* illustrate the number of two-digit SIC codes an analyst covers each quarter. *Local* is equal to the distance between the analysts and the firms they cover. *Female* is a dummy variable equal to one when the analyst is female. *All-Star* is equal to one if the analyst is ranked as first, second, third, or runner-up in the Institutional Investor Magazine in the previous year. To allow comparisons we scale all continuous variables to range from 0 to 1. All regressions include year-quarter and state fixed effects. The coefficient estimates in columns (1), (2) and (3) illustrate the marginal probabilities. Standard errors, shown in parentheses, are clustered at the level of analyst's home state. All coefficients and standard errors are multiplied by 100. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

	Pessimism			Relative Pessimism			Rank Pessimism		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Impact	2.49***	2.94***	2.93***	1.63***	2.16***	2.15***	1.51***	1.94***	1.94***
	(0.60)	(0.62)	(0.62)	(0.32)	(0.30)	(0.30)	(0.29)	(0.29)	(0.29)
Horizon		-3.63***	-3.60***		-3.30***	-3.28***		-1.59***	-1.57***
		(0.25)	(0.26)		(0.22)	(0.22)		(0.14)	(0.14)
Brokerage size		2.42***	2.53***		2.08***	2.13***		1.84***	1.95***
		(0.74)	(0.77)		(0.56)	(0.60)		(0.46)	(0.50)
Lagged accuracy		-8.18***	-8.18***		-6.66***	-6.66***		-5.25***	-5.25***
		(0.28)	(0.28)		(0.18)	(0.19)		(0.12)	(0.12)

Table 1.3—*Continued*

	Pessimism			Relative Pessimism			Rank Pessimism		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Experience $_{General}$		1.22**	1.28**		0.60	0.63		0.86**	0.91**
		(0.53)	(0.56)		(0.46)	(0.48)		(0.40)	(0.43)
Experience $_{Firm}$		-0.53***	-0.50***		-0.26**	-0.23*		-0.54***	-0.51***
		(0.18)	(0.18)		(0.12)	(0.12)		(0.14)	(0.13)
Industries		-0.36	-0.32		-0.09	-0.06		-0.33	-0.29
		(0.29)	(0.29)		(0.20)	(0.20)		(0.24)	(0.25)
Local		-0.24	-0.23		-0.21	-0.20		-0.03	-0.02
		(0.28)	(0.28)		(0.22)	(0.22)		(0.20)	(0.21)
Female			1.33***			1.16***			1.38***
			(0.44)			(0.34)			(0.31)
All-Star			-0.34*			-0.19			-0.35**
			(0.18)			(0.15)			(0.16)
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	480,209	358,883	358,883	480,209	358,885	358,885	480,209	358,885	358,885

Table 1.4. Terrorist Events and Bold Forecasts

This table examines whether affected analysts provide bold forecasts after being exposed to a terrorist attack. In columns (1) and (2), we consider the variable *Forecast Type* to examine whether analysts tend to upgrade or downgrade their forecasts after a terrorist attack. *Forecast Type* is equal to one if the analyst provides a forecast below the consensus forecast and his/her prior forecast for firm *j* at time *t*; *Forecast Type* is equal to minus one if the analyst issues a forecast above the consensus and above his/her prior forecast; *Forecast Type* is equal to zero for the rest of the forecasts. In column (1), *Forecast Type* takes the value of one and in column (2) *Forecast Type* is equal to minus one. The category of *Forecast Type* equal to zero is considered as the base outcome to estimate the specifications in columns (1) and (2). In column (3), we examine whether the affected forecasts are more likely to issue bold pessimistic forecasts. *Bold Pes* is a dummy variable equal to one if the analyst issues a forecast for firm *j* at time *t* below the consensus forecast and his/her prior forecast. All regressions include year-quarter fixed effects, state fixed effects and similar control variables as in Table 1.3. All coefficient estimates illustrate the marginal probabilities. Standard errors, shown in parentheses, are clustered at the level of analyst's home state. All regression coefficients and standard errors are multiplied by 100. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

	Forecast Type		Bold Pes
	(1)	(2)	(3)
Impact	2.76***	-1.70*	2.77***
	(0.71)	(0.94)	(0.70)
Horizon	-8.61***	-7.27***	-8.98***
	(0.76)	(0.19)	(0.74)
Brokerage size	2.74***	-0.43	2.73***
	(0.50)	(0.54)	(0.50)
Lagged accuracy	-1.06***	3.29***	-1.03***
	(0.14)	(0.16)	(0.15)
Experience $_{General}$	-0.14	-1.06***	-0.13
	(0.84)	(0.30)	(0.85)
Experience $_{Firm}$	0.91***	-0.00	0.91***
	(0.32)	(0.18)	(0.32)
Industries	-0.53*	0.19	-0.53*
	(0.27)	(0.35)	(0.27)
Local	-1.08*	0.78	-1.07*
	(0.56)	(0.96)	(0.55)
Female	-1.59***	1.08**	-1.62***
	(0.45)	(0.49)	(0.46)
All-Star	-0.43	0.56***	-0.43
	(0.47)	(0.20)	(0.48)
Constant	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
N	142,115	142,115	142,109

Table 1.5. Geographical and Temporal Proximity to Terrorist Events

This table examines how different geographical and temporal proximity to the terrorist attacks affects the forecast pessimism of affected analysts. In Panel A, we examine how the distance between the attacks and the analysts affect the pessimism in their forecasts during the next 90 days after the attacks. In our baseline model, we define as local analysts those located in a 100 miles radius from the attacks. In this Panel, we allow the distance to vary. To examine the association of distance with the magnitude of the effect, we include in our baseline model the variables $Impact_{(0\ to\ 50\ miles)}$, $Impact_{(51\ to\ 100\ miles)}$ and $Impact_{(101\ to\ 200\ miles)}$ which are dummy variables equal to one if a forecast is issued from an analyst located less than 50 miles, 51-100 miles, and 101-200 miles from an attack, respectively. In Panel B, we examine how the time after the attacks can affect the forecasting behavior of affected analysts. In our baseline model, we consider as affected analysts those who are located in a 100 miles radius from the attacks and issue a forecast 90 days after the attacks. In this Panel, we allow the time gap between the attacks and the issued forecasts from local analysts to vary. In particular, we include in our baseline model the variables $Impact_{(0\ to\ 30\ days)}$, $Impact_{(31\ to\ 90\ days)}$ and $Impact_{(91\ to\ 180\ days)}$ which are dummy variables equal to one if a forecast is issued from a local analyst 0-30 days, 31-90 days, and 91-180 days from the date of an attack, respectively. All regressions include year-quarter fixed effects, state fixed effects and similar control variables as in Table 1.3. All coefficient estimates in the first column illustrate the marginal probabilities. In both Panels, we perform a Wald test in each specification to examine the difference between the highest and lowest category. Standard errors, shown in parentheses, are clustered at the level of analyst's home state and χ^2/F-statistics are reported in square brackets. All regression coefficients and standard errors are multiplied by 100. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

Panel A: Geographical Proximity

	Pessimism (1)	Relative Pessimism (2)	Rank Pessimism (3)
$Impact_{(0\ to\ 50\ miles)}$	2.97**	2.20**	2.20**
	(1.45)	(1.06)	(0.85)
$Impact_{(51\ to\ 100\ miles)}$	2.46**	1.44	1.29
	(1.07)	(1.07)	(1.20)
$Impact_{(101\ to\ 200\ miles)}$	0.84	0.47	0.50
	(1.11)	(0.71)	(0.73)
Control Variables	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
$Impact_{(0\ to\ 50\ miles)}$ - $Impact_{(101\ to\ 200\ miles)}$	2.13	1.73	1.70
χ^2/F-statistic	[0.96]	[1.19]	[1.63]

Table 1.5—*Continued*

Panel B: Temporal Proximity			
	Pessimism	Relative Pessimism	Rank Pessimism
	(1)	(2)	(3)
$Impact_{(0\ to\ 30\ days)}$	4.83***	3.20***	2.97***
	(1.38)	(0.92)	(0.81)
$Impact_{(31\ to\ 90\ days)}$	2.07***	1.58***	1.45***
	(0.52)	(0.23)	(0.31)
$Impact_{(91\ to\ 180\ days)}$	-0.01	-0.25	-0.03
	(0.60)	(0.27)	(0.49)
Control Variables	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
$Impact_{(0\ to\ 30\ days)}$ - $Impact_{(91\ to\ 180\ days)}$	4.84***	3.45***	3.00***
χ^2/F-statistic	[27.28]	[20.85]	[30.36]

Table 1.6. Analyst Pessimism and State's Murder Activity

This table examines whether the general murder activity in affected analysts home state affects their pessimism around attack periods. To measure the level of murder activity, we divide the number of murders (available from the FBI and reported in the Uniform Crime Reporting Program) with the population of the state, and we compute the average murder rate across states for each year. $Murder_h$ is a dummy variable equal to one if the state of analyst's location has a higher murder rate than the average murder rate of states in a given year. $Murder_l$ is a dummy variable equal to one if $Murder_h$ is equal to zero. All regressions include year-quarter fixed effects, state fixed effects and similar control variables as in Table 1.3 We perform a Wald test in each specification to examine the difference of forecasts between the analysts who live in a state with low and high murder rates. The coefficient estimates in column (1) illustrate the marginal probabilities. Standard errors, shown in parentheses, are clustered at the level of analyst's home state and χ^2/F-statistics are reported in square brackets. All regression coefficients and standard errors are multiplied by 100. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

	Pessimism	Relative Pessimism	Rank Pessimism
	(1)	(2)	(3)
Impact × $Murder_l$	3.54***	2.42***	2.23***
	(0.43)	(0.25)	(0.29)
Impact × $Murder_h$	0.62	1.15**	0.83
	(0.99)	(0.52)	(0.59)
$Murder_l$	-0.23	-0.14	-0.31
	(0.65)	(0.48)	(0.44)
Control Variables	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Impact × $Murder_l$ - Impact × $Murder_h$	2.92***	1.27**	1.40*
χ^2/F-statistic	[8.63]	[5.08]	[3.96]

Table 1.7. Terrorist Events and Forecast Accuracy

This table examines whether exposure to terrorist attacks and mass shootings affect accuracy of analysts' forecasts. We define *Accuracy* as the absolute value of the difference between the maximum forecast error for firm *j* at time *t* and the forecast error of analyst *i* for firm *j* at time *t* divided by maximum forecast error for firm *j* at time *t* minus the minimum forecast error for firm *j* at time *t*, where the forecast error is equal to the difference of earnings forecast and actual earnings, scaled by the lagged monthly stock price. All regressions include year-quarter fixed effects and state fixed effects. We also control for firm and industry (2-digit SIC classification) fixed effects. All regressions include year-quarter fixed effects, state fixed effects and similar control variables as in Table 1.3. Standard errors, shown in parentheses, are clustered at the level of analyst's home state. All regression coefficients and standard errors are multiplied by 100. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

	Accuracy			
	(1)	(2)	(3)	(4)
Impact	0.63*	0.62*	0.67*	0.65**
	(0.31)	(0.31)	(0.33)	(0.32)
Horizon	-10.94***	-10.94***	-11.01***	-10.96***
	(0.22)	(0.22)	(0.27)	(0.26)
Brokerage size	0.93**	0.81*	1.15***	0.89**
	(0.43)	(0.41)	(0.37)	(0.40)
Lagged accuracy	7.99***	7.98***	6.67***	7.81***
	(0.17)	(0.17)	(0.21)	(0.16)
Experience $_{General}$	-0.38	-0.43	-0.75***	-0.46*
	(0.26)	(0.27)	(0.24)	(0.27)
Experience $_{Firm}$	0.06	0.05	0.66***	0.10
	(0.19)	(0.18)	(0.20)	(0.21)
Industries	-0.67***	-0.68***	-0.19	-0.48**
	(0.17)	(0.18)	(0.18)	(0.18)
Local	0.15	0.15	-0.15	0.11
	(0.28)	(0.28)	(0.18)	(0.22)
Female		-0.33	-0.59	-0.57*
		(0.31)	(0.38)	(0.33)
All-Star		0.31*	0.47***	0.47***
		(0.16)	(0.11)	(0.13)
Constant	Yes	Yes	Yes	Yes
Firm Fixed Effects	No	No	Yes	No
Industry Fixed Effects	No	No	No	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes
N	358,630	358,630	358,630	358,630

Table 1.8. Terrorist Attacks, Anniversaries and Analyst Pessimism

This table examines whether the first and second anniversary of terrorist attacks can affect the forecast pessimism of affected analysts. Panel A reports the results from regressions examining the effect of the first anniversary of terrorist attacks on the forecast pessimism of affected analysts. $Impact_{(First\ Anniversary)}$ is equal to one if an analyst is local to an attack and issues a forecast around the period of the first anniversary. To have a valid comparison between the effect of the attacks and their anniversaries, we consider all the events occurred the 1994-2012 period since we lack of analyst location data for the 2014 year. In Panel B, we examine the effect of the second anniversary of terrorist attacks. $Impact_{(Second\ Anniversary)}$ is equal to one if an analyst is local to an attack and issues a forecast around the period of the second anniversary. Similar to Panel A, we consider a reduced event sample for the 1994-2011 period, since we lack of analyst location data for the 2014-2015 period. In Panel C, we examine whether the general murder activity in affected analysts home state affects their pessimism around first anniversary periods. Similar to Table 1.6, $Murder_h$ is a dummy variable equal to one if the state of analyst's location has a higher murder rate than the average murder rate of states in a given year. $Murder_l$ is a dummy variable equal to one if $Murder_h$ is equal to zero. In each panel, we allow for distance and time to vary and we perform a Wald test to compare coefficient estimates. All regressions include year-quarter fixed effects, state fixed effects and similar control variables as in Table 1.3. The coefficient estimates in columns (1) and (4) show the marginal probabilities. Standard errors, shown in parentheses, are clustered at the level of analyst's home state and χ^2/F-statistics are reported in square brackets. All regression coefficients and standard errors are multiplied by 100. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

Panel A: First Anniversary of Terrorist Attacks

	Pessimism	Relative Pessimism	Rank Pessimism	Pessimism	Relative Pessimism	Rank Pessimism
	A1: (<100 miles, <=90 days)			A2: (<50 miles, <=30 days)		
	(1)	(2)	(3)	(4)	(5)	(6)
$Impact_{(First\ Anniversary)}$	0.47	0.24	0.48	2.51*	1.98**	0.85
	(0.76)	(0.38)	(0.35)	(1.44)	(0.96)	(0.79)
Impact	3.10***	2.27***	2.05***	5.52***	4.03***	3.65***
	(0.58)	(0.24)	(0.27)	(1.48)	(1.00)	(0.94)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
$Impact_{(First\ Anniversary)}$- Impact	-2.63***	-2.03***	-1.57***	-3.01***	-2.05***	2.8***
χ^2/F-statistic	[30.01]	[41.17]	[28.17]	[11.74]	[12.28]	[23.30]

Table 1.8—*Continued*

Panel B: Second Anniversary of Terrorist Events						
	Pessimism	Relative Pessimism	Rank Pessimism	Pessimism	Relative Pessimism	Rank Pessimism
	B1: (<100 miles, <=90 days)			B2: (<50 miles, <=30 days)		
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Impact}_{(\text{Second Anniversary})}$	0.16	-0.35	0.05	-0.76	-0.18	-0.43
	(0.42)	(0.35)	(0.34)	(0.46)	(0.42)	(0.39)
Impact	3.53***	2.76***	2.61***	5.51***	4.08***	3.68***
	(0.79)	(0.42)	(0.42)	(1.45)	(0.94)	(0.90)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
$\text{Impact}_{(\text{Second Anniversary})}$- Impact	-3.37***	-3.11***	-2.56***	-6.27***	-4.26***	-4.11***
χ^2/F-statistic	[23.39]	[93.34]	[48.39]	[16.57]	[20.80]	[26.18]

Table 1.8—*Continued*

Panel C: First Anniversary of Terrorist Events and State's Murder Activity						
	Pessimism	Relative Pessimism	Rank Pessimism	Pessimism	Relative Pessimism	Rank Pessimism
	C1: (<100 miles, <=90 days)			C2: (<50 miles, <=30 days)		
	(1)	(2)	(3)	(4)	(5)	(6)
$Impact_{(First\ Anniversary)} \times Murder_l$	1.12**	0.58*	0.69***	3.22***	2.46***	1.16**
	(0.50)	(0.33)	(0.22)	(1.07)	(0.78)	(0.55)
$Impact_{(First\ Anniversary)} \times Murder_h$	-1.86	-0.99	-0.29	0.15	0.34	-0.25
	(1.31)	(0.73)	(0.98)	(2.44)	(1.35)	(1.71)
$Impact \times Murder_l$	3.74***	2.52***	2.34***	6.38***	4.79***	4.29***
	(0.43)	(0.24)	(0.30)	(0.98)	(0.71)	(0.74)
$Impact \times Murder_h$	0.58	1.23**	0.92	1.18	0.05	0.27
	(0.96)	(0.56)	(0.62)	(3.20)	(1.42)	(1.39)
$Murder_l$	-0.33	-0.19	-0.34	-0.21	-0.14	-0.30
	(0.64)	(0.47)	(0.44)	(0.66)	(0.49)	(0.45)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
$Impact_{(First\ Anniversary)} \times Murder_l$ - $Impact_{(First\ Anniversary)} \times Murder_h$	2.98***	1.57**	0.98	3.07*	2.12**	1.41
χ^2/F-statistic	[6.77]	[4.27]	[1.02]	[3.18]	[6.68]	[0.99]
$Impact_{(First\ Anniversary)} \times Murder_l$ - $Impact \times Murder_l$	-2.62***	-1.94***	-1.65***	-3.16***	-2.33***	3.13***
χ^2/F-statistic	[32.20]	[31.88]	[22.99]	[17.02]	[21.02]	[25.50]

Table 1.9. Robustness Tests: Pre-Existing Trends and Macroeconomic Conditions

In this table, we perform various robustness tests to examine whether alternative hypotheses could justify our main results. In Panel A, we include lag values of *Impact* to test whether there are any potential pre-existing shocks that could affect our estimations. In Panel B, we examine whether our results are robust when we control for the economic climate of the state where the analyst is located. To control for economic conditions we create the index *Macro-state index*. To define *Macro-state index* we sum the collateral ratio and the income growth rate, subtract the relative state unemployment rate and divide them by three. The state-level housing collateral ratio is the log ratio of state-level housing equity to state labor income. The relative state unemployment rate depicts the fraction of the current rate to the moving 16 quarter-average of past rates. The growth rate of labor income captures the state-level changes in labor income. In Panel B, our sample period extends from 1994-2012 due to unavailability of *Macro-state index* for 2013. All regressions include year-quarter fixed effects, state fixed effects and similar control variables as in Table 1.3. All coefficient estimates in the first column illustrate the marginal probabilities. Standard errors, shown in parentheses, are clustered at the level of analyst's home state. All regression coefficients and standard errors are multiplied by 100. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

Panel A: Pre-Existing Effects

	Pessimism	Relative Pessimism	Rank Pessimism
	(1)	(2)	(3)
$\text{Impact}_{(0\ to\ 90\ days)}$	2.98***	2.28***	1.92***
	(0.56)	(0.33)	(0.28)
$\text{Impact}_{(-90\ to\ -1\ days)}$	0.17	0.66	-0.20
	(0.85)	(0.40)	(0.51)
$\text{Impact}_{(-180\ to\ -91\ days)}$	0.44	0.46	0.54
	(0.97)	(0.61)	(0.39)
$\text{Impact}_{(-270\ to\ -181\ days)}$	-0.34	0.18	-0.33
	(0.90)	(0.94)	(0.74)
$\text{Impact}_{(-365\ to\ -271\ days)}$	0.04	0.09	0.03
	(0.45)	(0.48)	(0.27)
Control Variables	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes

Table 1.9—*Continued*

Panel B: Controlling for State-level Macroeconomic Conditions			
	Pessimism	Relative Pessimism	Rank Pessimism
	(1)	(2)	(3)
Impact	3.53***	2.77***	2.44***
	(0.78)	(0.37)	(0.43)
Macro-state index	0.25	0.16	-0.04
	(0.23)	(0.14)	(0.13)
Control Variables	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes

Table 1.10. Terrorist Events, Pessimism and Unobservable Characteristics

This table presents the results from regressions examining the impact of attacks on forecast pessimism while including various fixed effects to control for unobservable characteristics. We consider analyst, brokerage firm, firm and industry (2-digit SIC classification) fixed effects additionally to state and year-quarter fixed effects. The sample includes forecasts for U.S. stocks included in CRSP from the 1st quarter of 1994 to the 4th quarter of 2013. *Impact* is a dummy variable equal to one if the analyst who provided the forecast is inside a 100 miles from an attack, and the forecast took place during the next 90 days after the attack. To allow comparisons we scale all continuous variables to range from 0 to 1. All regressions include year-quarter fixed effects, state fixed effects and similar control variables as in Table 1.3. In columns (1), (2), (3), and (4) we use *Pessimism* as dependent variable, in columns (5), (6), (7), and (8) we examine the effect of attacks on *Relative Pessimism*, and in the rest of the columns we examine the changes on *Rank Pessimism*. Standard errors, shown in parentheses, are clustered at the level of analyst's home state. All regression coefficients and standard errors are multiplied by 100. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

	Pessimism				Relative Pessimism				Rank Pessimism			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Impact	2.97***	3.02***	3.02***	2.92***	2.10***	2.22***	2.27***	2.15***	1.95***	2.02***	1.94***	1.94***
	(0.60)	(0.59)	(0.54)	(0.61)	(0.28)	(0.28)	(0.25)	(0.28)	(0.27)	(0.26)	(0.27)	(0.28)
Horizon	-4.39***	-3.71***	-3.63***	-3.63***	-3.89***	-3.37***	-3.37***	-3.32***	-2.08***	-1.65***	-1.64***	-1.62***
	(0.26)	(0.25)	(0.25)	(0.25)	(0.25)	(0.22)	(0.20)	(0.21)	(0.12)	(0.13)	(0.15)	(0.15)
Brok. size	0.71	-0.11	2.64***	2.52***	0.67	0.09	2.23***	2.10***	0.63	0.33	2.03***	1.96***
	(0.61)	(0.64)	(0.74)	(0.78)	(0.45)	(0.44)	(0.59)	(0.61)	(0.40)	(0.40)	(0.50)	(0.49)
Lag. accuracy	-6.17***	-7.96***	-7.86***	-8.11***	-5.05***	-6.49***	-6.18***	-6.58***	-3.85***	-5.08***	-5.34***	-5.25***
	(0.22)	(0.20)	(0.25)	(0.29)	(0.13)	(0.14)	(0.18)	(0.19)	(0.10)	(0.12)	(0.13)	(0.12)
$\text{Experence}_{\text{Gen.}}$	1.07*	1.09***	1.49**	1.28**	0.70	0.47	0.90*	0.68	0.56	0.80***	0.97**	0.94**
	(0.54)	(0.38)	(0.57)	(0.55)	(0.49)	(0.35)	(0.47)	(0.48)	(0.38)	(0.28)	(0.43)	(0.41)
$\text{Experience}_{\text{Firm}}$	-0.54***	-0.59**	-0.69***	-0.51**	-0.31**	-0.31**	-0.48***	-0.26*	-0.49***	-0.58***	-0.54***	-0.51***
	(0.18)	(0.23)	(0.20)	(0.20)	(0.14)	(0.15)	(0.15)	(0.13)	(0.12)	(0.17)	(0.14)	(0.14)
Industries	-0.60***	-0.34	-0.54*	-0.47	-0.29	-0.08	-0.30	-0.21	-0.22	-0.29	-0.29	-0.28
	(0.21)	(0.27)	(0.28)	(0.29)	(0.19)	(0.17)	(0.20)	(0.20)	(0.19)	(0.24)	(0.22)	(0.24)

Table 1.10—*Continued*

	Pessimism				Relative Pessimism				Rank Pessimism			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Local	-0.35**	-0.22	-0.19	-0.27	-0.35**	-0.18	-0.12	-0.29	-0.09	-0.01	-0.04	-0.02
	(0.16)	(0.26)	(0.32)	(0.28)	(0.17)	(0.19)	(0.22)	(0.20)	(0.13)	(0.20)	(0.22)	(0.22)
Female		1.36***	1.78***	1.58***		1.16***	1.54***	1.47***		1.42***	1.55***	1.46***
		(0.42)	(0.46)	(0.48)		(0.32)	(0.36)	(0.36)		(0.29)	(0.34)	(0.34)
All-Star	0.45**	0.13	-0.51*	-0.45*	0.35***	0.14	-0.36	-0.26	0.48***	0.10	-0.38*	-0.36*
	(0.18)	(0.31)	(0.26)	(0.23)	(0.11)	(0.30)	(0.22)	(0.19)	(0.05)	(0.26)	(0.22)	(0.18)
Constant	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Analyst F.E.	Yes	No	No	No	Yes	No	No	No	Yes	No	No	No
Brokerage F.E.	No	Yes	No	No	No	Yes	No	No	No	Yes	No	No
Firm F.E.	No	No	Yes	No	No	No	Yes	No	No	No	Yes	No
Industry F.E.	No	No	No	Yes	No	No	No	Yes	No	No	No	Yes
State F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	358,885	358,885	358,885	358,885	358,885	358,885	358,885	358,885	358,885	358,885	358,885	358,885

Table 1.11. Robustness Tests: Different Sample Specifications

In this table, we perform additional robustness tests. In Panel A, we examine the robustness of our results, by focusing on different forecast samples. In particular, in our main analysis we keep the last forecast of each analyst *i* for each firm *j* at time *t*. In this Panel, we examine whether our results are robust when we keep the first forecast of each analyst *i* for firm *j* at time *t*. In Panel B, we examine whether our results are robust to the exclusion of 9/11 attacks. In Panel C, we exclude from our sample all the analysts that live in the NY state and we repeat our main analysis. All regressions include year-quarter fixed effects, state fixed effects and similar control variables as in Table 1.3. All coefficient estimates in the first column illustrate the marginal probabilities. Standard errors, shown in parentheses, are clustered at the level of analyst's home state. All regression coefficients and standard errors are multiplied by 100. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

Panel A: First Forecast Sample

	Pessimism	Relative Pessimism	Rank Pessimism
	(1)	(2)	(3)
Impact	2.33**	1.54**	1.55**
	(0.94)	(0.73)	(0.65)
Control Variables	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes

Panel B: Sensitivity to 9/11 Attacks

	Pessimism	Relative Pessimism	Rank Pessimism
	(1)	(2)	(3)
$\text{Impact}_{\text{(Excluding 9/11 Attacks)}}$	1.56***	0.69**	0.86**
	(0.37)	(0.32)	(0.40)
Control Variables	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes

Panel C: Excluding New York Analysts

	Pessimism	Relative Pessimism	Rank Pessimism
	(1)	(2)	(3)
$\text{Impact}_{\text{(Excluding NY analysts)}}$	2.58***	2.72***	2.22***
	(0.93)	(0.61)	(0.56)
Control Variables	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes

Table 1.12. Robustness Tests: Alternative Mass Shooting Sample

In this table, we increase the sample of mass shootings by including events with at least four human casualties. We get data from MSA Stanford database for the 1994-2013 period. In Panel A, we show the additional event sample. In Panel B, we examine the robustness of our results when we include mass shootings with less human casualties in comparison to our initial event sample. All regressions include year-quarter fixed effects, state fixed effects and similar control variables as in Table 1.3. All coefficient estimates in column (1) of Panel B illustrate the marginal probabilities. Standard errors, shown in parentheses, are clustered at the level of analyst's home state. All regression coefficients and standard errors are multiplied by 100. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

Panel A: Description of Additional Mass Shooting Events

Event	Date	Location
Mass shootings with equal or more than 8 casualties		
All-Tech I.G. / Momentum Securities	29 Jul 1999	Atlanta, GA
Wedgwood Baptist Church	15 Sep 1999	Fort Worth, TX
Living Church of God	12 Mar 2005	Brookfield, WI
Goleta Post Office	30 Jan 2006	Goleta, CA
Covina Shootings	24 Dec 2008	Covina, CA
Hartford Beer Distributors	03 Aug 2010	Manchester, CT
Salon Meritage	12 Oct 2011	Seal Beach, CA
Mass shootings with 6 or 7 casualties		
Fort Lauderdale City Parks Office	09 Feb 1996	Fort Lauderdale, FL
Edgewater Technology	26 Dec 2000	Wakefield, MA
Navistar International	05 Feb 2001	Melrose Park, IL
Windy City Core Supply Warehouse	27 Aug 2003	Chicago, IL
Birchwood Shootings	21 Nov 2004	Birchwood, WI
Party in Capitol Hill	25 Mar 2006	Seattle, WA
West Nickel Mines Amish School	02 Oct 2006	Nickel Mines, PA
Carnation Shootings	24 Dec 2007	Carnation, WA
Kirkwood City Hall	07 Feb 2008	Kirkwood, MO
Northern Illinois University	14 Feb 2008	DeKalb, IL
Rivermark	29 Mar 2009	Santa Clara, CA
Oikos University	02 Apr 2012	Oakland, CA
Cafe Shootings	30 May 2012	Seattle, WA
Accent Signage Systems	27 Sep 2012	Minneapolis, MN
Village of Manchester	24 Apr 2013	Manchester, IL
Santa Monica College	07 Jun 2013	Santa Monica, CA
Mass shootings with 4 or 5 casualties		
Residence in Union	26 May 1994	Union, KY
Montclair Post Office	21 Mar 1995	Montclair, NJ

Table 1.12—*Continued*

Event	Date	Location
Caltrans Maintenance Yard	18 Dec 1997	Orange, CA
Connecticut State Lottery Headquarters	06 Mar 1998	Newington, CT
Westside Middle School	24 Mar 1998	Jonesboro, AR
Radisson Bay Harbor Inn	30 Dec 1999	Tampa, FL
Youth With A Mission / New Life Church	09 Dec 2007	Arvada, CO
Parkland Coffee Shop	29 Nov 2009	Lakewood, WA
Ensley Shootings	29 Jan 2012	Birmingham, AL
Su Jung Health Sauna	22 Feb 2012	Norcross, GA
Azana Spa	21 Oct 2012	Brookfield, WI
Los Angeles Police Department	03 Feb 2013	Irvine, CA
Ladera Ranch Shootings	19 Feb 2013	Ladera Ranch, CA
Mohawk Shootings	13 Mar 2013	Mohawk, NY
Pinewood Village Apartments	21 Apr 2013	Federal Way, WA

Panel B: Model Estimates with Alternative Mass Shooting Sample

	Pessimism			Relative Pessimism			Rank Pessimism		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Impact_{(Initial\ Sam.\ +\ MSA\ Sam.>=8\ Casualties)}$	2.84***			2.23***			1.84***		
	(0.71)			(0.39)			(0.42)		
$Impact_{(Initial\ Sam.\ +\ MSA\ Sam.>=6\ Casualties)}$		1.75*			1.33**			1.19**	
		(1.00)			(0.64)			(0.55)	
$Impact_{(Initial\ Sam.+\ MSA\ Sam.>=4\ Casualties)}$			1.04			0.79			0.75*
			(0.78)			(0.48)			(0.42)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

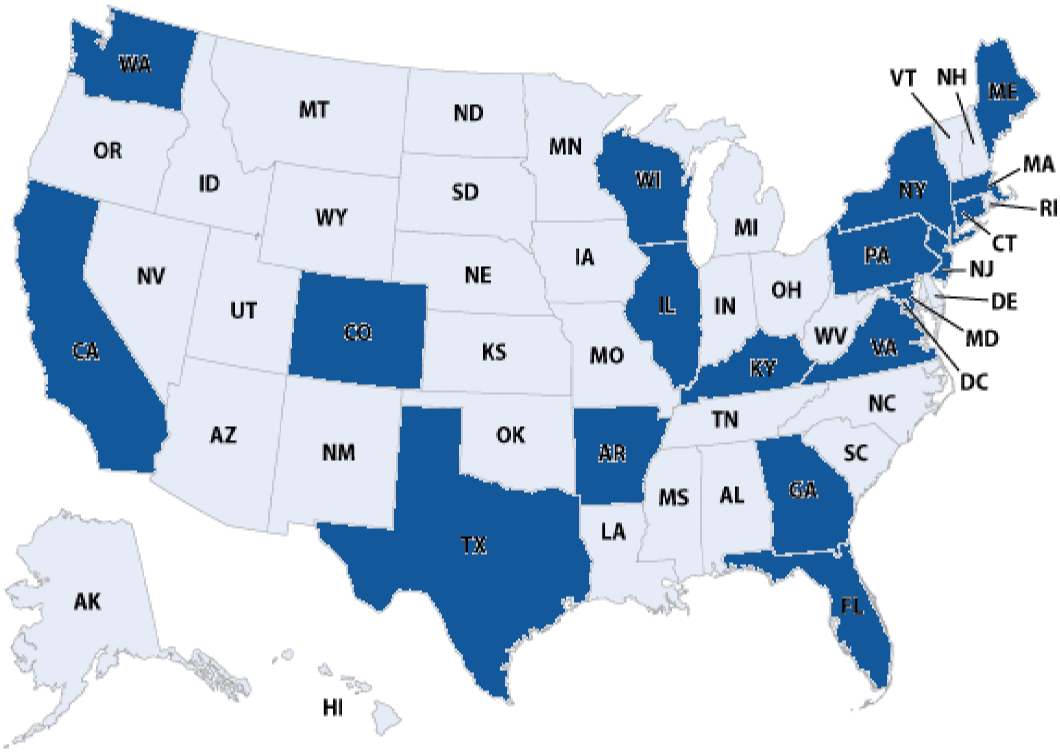

Figure 1.1. Terrorist Attacks and Locations. This figure shows the states where the terrorist attacks and mass shootings took place.

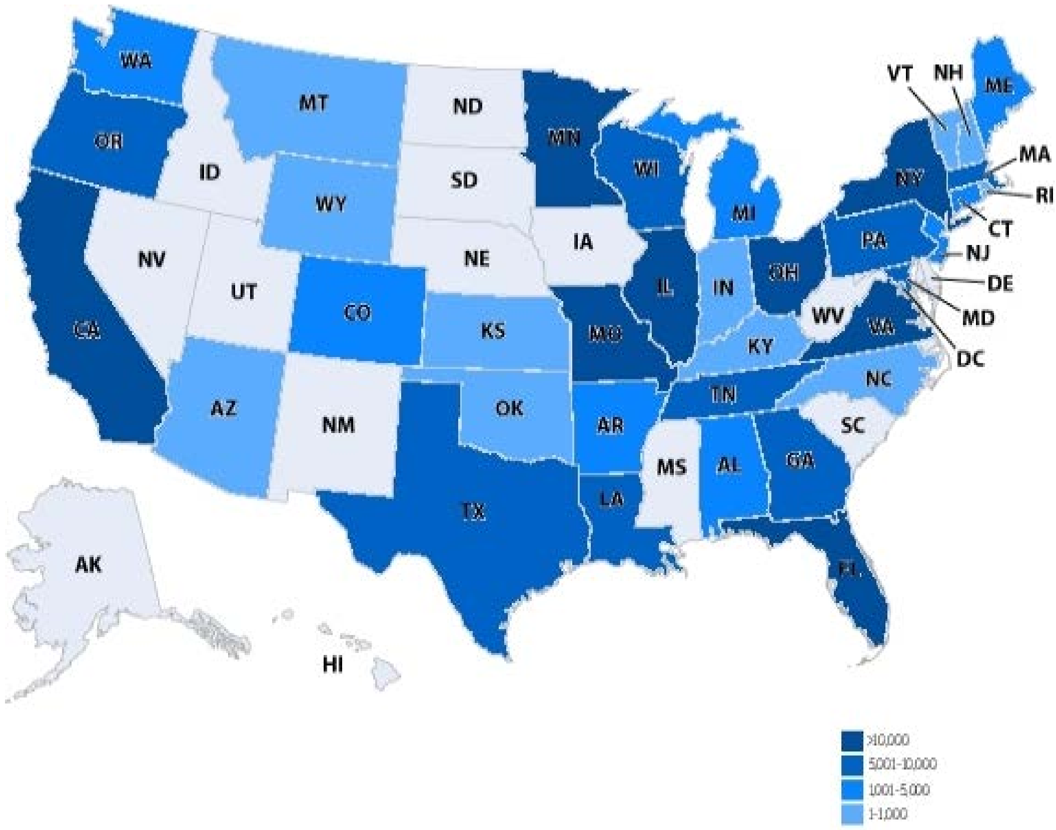

Figure 1.2. Forecasts and Locations. This figure shows the distribution of forecasts across different states. States with no forecasts are highlighted with white.

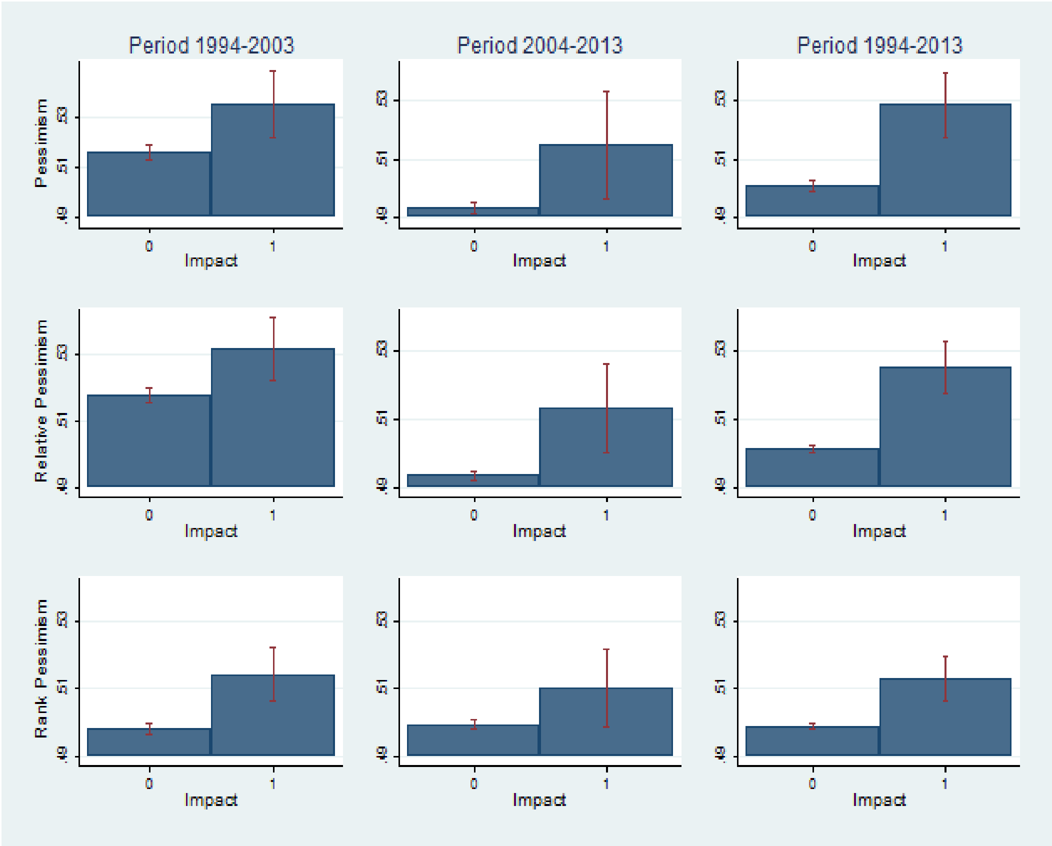

Figure 1.3. Pessimism, Attacks and Different Sample Periods. This figure shows the average pessimism of analysts during different sample periods between the affected and non-affected analysts. Each column represents a different period and each row illustrates different variables. The red spikes show the upper and lower bound of each variable in a 95% confidence interval.

Chapter 2

Terrorist Attacks, Managerial Sentiment, and Corporate Policies

2.1 Introduction

An established literature in psychology demonstrates that extremely negative events adversely affect people's sentiment. Consequently, they become more pessimistic in their assessments of risk in other *unrelated* domains.[1] Recent studies in finance have shown that such sentiment-related biases influence portfolio choice and stock returns.[2] We extend this literature by examining whether such events also influence the sentiment of corporate managers, and, consequently, their corporate policies.

We focus on two types of extreme negative events: terrorist attacks and mass shootings (henceforth, "terrorist attacks"). We focus on these types of events because research in psychology shows that such extreme events generate strong feelings of fear, anxiety and depression among affected people, and induce a

[1] See, for example, Johnson and Tversky (1983), Finucane, Alhakami, Slovic, and Johnson (2000), Slovic, Finucane, Peters and MacGregor (2002), Kuhnen and Knutson (2011).

[2] These studies show that economically irrelevant events generate strong emotional reactions influence stock returns. See, for example, Hirshleifer and Shumway (2003), Kamstra, Kramer and Levi (2003), Edmans, García and Norli (2007), Kaplanski and Levy (2010).

degree of pessimism in their risk assessments (e.g., Lerner and Keltner, 2001; Lerner, Gonzalez, Small and Fischhoff, 2003).[3] In addition, such events are unlikely to be tied to economic fundamentals since they occur at random times and locations, and they are usually not induced by economic factors.

We assume that the adverse shock to sentiment would be more pronounced among "local" managers, i.e., managers who are located close to these events. This assumption is based on the premise that local managers are more likely to have a more "personal experience" with the event, as they are more likely to interact with or hear about people who are more directly affected. Such direct exposure would make the event significantly more salient to managers, and affect their sentiment more strongly.

Our view that terrorist attacks influence the sentiment of the local community more strongly is motivated by studies in psychology (e.g., Vlahov, Galea, Resnick, Ahern, Boscarino, Bucuvalas, Gold and Kilpatrick, 2002; Galea et al., 2002; Hughes et al., 2011).[4] Specifically, we conjecture that managers of local firms located near terrorist attacks will become more pessimistic and adopt more prudent corporate policies than managers of firms who are located far away from the location of these events.

We test our hypothesis using data on terrorist attacks and mass shootings obtained from the Global Terrorism Database (GTD) and the Washington Post (WP). Due to various filters and data requirements that are aimed to identify the more important and salient events, which are more likely to affect managerial sentiment, our final sample contains 25 major events during the 1997 to 2012 period.[5]

Our econometric models compare financial decisions of local firms and non-local firms around the period of the attacks in a difference-in-difference

[3] For additional evidence, see Galea, Ahern, Resnick, Kilpatrick, Bucuvalas, Gold and Vlahov (2002) and Hughes, Brymer, Chiu, Fairbank, Jones, Pynoos, Rothwell, Steinberg and Kessler (2011). For more information on the effects of terrorist attacks and mass shootings, see http://www.apa.org/helpcenter/terrorism.aspx and http://www.apa.org/helpcenter/mass-shooting.

[4] Anecdotal evidence also support this view. For example, an article published by *Daily Mail* in the U.K. discussed the mental health issues faced by residents in Newtown, Connecticut two years after the terrorist attacks in Sandy Hook elementary school (http://www.dailymail.co.uk/news/article-2870512/In-Newtownmental-health-problems-emerging.html).

[5] In our robustness tests, we also consider a larger sample of negative events identified using less stringent criteria for inclusion in the event sample. As expected, when we consider events that are likely to be perceived as less salient, our results are similar but weaker.

setting. Specifically, we test for systematic differences in corporate cash holdings, research and development (R&D), and long-term leverage. In all our tests, we control for several variables that capture the potential impact of economic variables on corporate policies.[6]

Previous research indicates that higher levels of cash help firms cope with potential liquidity shocks and mitigate any refinancing problems when capital markets become too costly (Opler, Pinkowitz, Stulz and Williamson, 1999; Almeida, Campello and Weisbach, 2004; Bates, Kahle and Stulz, 2009; Harford, Klasa and Maxwell, 2014). R&D expenditure is related to firm risk, since innovative projects are considered to be riskier (Hilary and Hui, 2009; Hutton, Jiang and Kumar, 2014). Finally, corporate leverage levels can significantly affect firm's risk exposure, since higher corporate leverage can increase firm's stock volatility and financial distress (Lewellen, 2006; Hackbarth, 2008).[7]

Motivated by these findings, we posit that, adverse shocks to managerial sentiment generated by negative local events would induce local firms to adopt more prudent corporate policies. In particular, they would increase their corporate cash holdings and decrease their R&D expenditure as well as their corporate leverage.

Our empirical results support this key hypothesis. We find that around the period of the attack, relative to non-local firms, local firms on average increase their cash holdings by 1.67%, and decrease their R&D expenditure and long-term leverage by 0.17% and 0.87%, respectively. These results are statistically significant and robust. We also find that these policy adjustments are temporary and last for only one quarter. This finding is reasonable as the negative shock to sentiment is likely to be short-lived. Collectively, these findings are consistent with our view that transient, emotion-related biases induced by proximity to terrorist attacks affect managerial sentiment and corporate policies.

Next, we investigate whether events that are likely to be perceived as more salient cause even larger adjustments to corporate policies. To define our first

[6] In our baseline analysis, firms that have their headquarters within a radius of 50 miles from an attack are identified as local. In our robustness section, we conduct sensitivity analysis using different distance cut-offs to identify local firms.
[7] Higher firm leverage is also associated with a higher level of risk-taking (Coles, Daniel and Naveen, 2006).

salience proxy, we consider the geographical distance of a firm to an attack, since proximity is likely to increase an event's saliency. In line with our hypothesis, we find that the adjustments to corporate policies become larger as the event-firm distance decreases.

We also examine whether events that are covered in the media more prominently, and are thus more salient, are associated with larger changes to corporate policies.[8] To conduct this test, we use hand-collected newspaper articles from important national outlets that are related to the attacks and form three salience proxies. First, we measure article length, since events that are covered with longer articles are likely to be more prominent. Second, we identify whether the event is covered in a leading story on the first page of a newspaper. Third, we measure whether the event is a leading newspaper story for multiple days. Our results across all three salience proxies show that changes in cash holdings, R&D expenditure, and leverage are larger when the attack is featured more prominently in the media, and is likely to be more salient.

Next, we examine whether adjustments to corporate policies are larger for local managers who are more likely to exhibit stronger emotional reactions to terrorist attacks. This test is motivated by studies in psychology, which document that younger people exhibit stronger emotional reaction (Carstensen, Pasupathi, Mayr and Nesselroade, 2000; Blanchard-Fields, Mienaltowski and Seay, 2007; Scheibe and Blanchard-Fields, 2009). Younger people are also likely to be less experienced, and lack of experience may generate stronger behavioral biases (e.g., List, 2003; Dhar and Zhu, 2006). In line with the findings in this literature, we observe that changes to corporate policies around terrorist attacks are mainly concentrated among firms that are managed by younger CEOs.

We conduct several additional tests to ensure that our results reflect the effects of managerial sentiment and not adjustments to economic shocks. To begin, we use a direct measure of managerial sentiment to examine the impact of negative events on sentiment. Specifically, we examine whether exposure to terrorist attacks influences the linguistic tone used by managers in the

[8] Several studies in finance show that the media is a major propagator and amplifier of sentiment, i.e., Tetlock, (2007), Tetlock, Saar-Tsechansky and Macskassy (2008), Barber and Odean (2008), Da, Engelberg and Gao (2011), and García (2013).

Management Discussion and Analysis (MD&A) section of quarterly financial statements (Feldman, Govindaraj, Livnat and Segal, 2010; Li, 2010; Loughran and McDonald, 2011; Bochkay and Dimitrov, 2014). We find that managers who are local to terrorist attacks adopt a more pessimistic tone in their MD&A corporate disclosure. This result provides direct evidence that exposure to terrorist attacks adversely influence managerial sentiment, which consequently influence corporate policies.

The remaining tests examine whether attack periods coincide with periods of known economic shocks. First, in an aggregate setting, we show that terrorist attacks are not related to state-level macroeconomic variables, which provides support to the claim that shocks generated by terrorist attacks are exogenous. Next, we examine whether local firms are exposed to economic conditions that may merit adjustments to corporate policies. Specifically, we check whether the differences we document between the corporate policies of local and non-local firms subsequent to attack periods are also found in the quarters prior to these events. We find that, prior to the attacks, there is no difference in the corporate policies of local and non-local firms. This evidence suggests that differences in economic environment across local and non-local firms are unlikely to explain our findings.

Although this evidence is re-assuring since it precludes the existence of pre-existing and confounding "parallel trends", it is still possible that attack periods experience economic shocks that are not captured appropriately by our control variables. To examine this possibility, we investigate whether local firms around event periods experience noticeable shifts to corporate credit ratings, analyst recommendations, stock return volatility, and firm sales. These firm-level indicators would affect the underlying company fundamentals, and firms that experience economic distress should experience changes in at least some of these measures. Our results show that there is no significant change in any of these indicators among local firms around negative events, which suggests that our results are unlikely to reflect the impact of economic shocks during the attack periods. To account for the potential misspecification of the control group, we also use a propensity score matching method to identify a control group of non-local firms that share similar characteristics as the local firms. We find that our results remain very similar in this setting where we use several different specifications for defining the control group.

We conduct several additional tests to ensure that our results our robust. First, we conduct placebo tests where we randomize the time or the location of the negative events and find no significant effects. Our results are robust to eliminating the 9/11 attacks from the sample (the most economically significant event in our sample), and to eliminating firms with missing R&D values (as opposed to setting their R&D's to zero). Finally, our results remain similar when we consider a larger terrorist attack sample, constructed using less stringent criteria for selecting negative events.

Several studies in this literature demonstrate that corporate decisions are affected by managerial overconfidence (e.g., Malmendier and Tate, 2005, 2008; Landier and Thesmar, 2009; Hirshleifer, Low and Teoh, 2012), context-related heuristics (Baker, Pan, and Wurgler, 2012; Dougal, Engelberg, Parsons and Wesep, 2015, Dessaint and Matray, 2016), and personality traits of CEOs (Malmendier, Tate and Yan, 2011; Cain and McKeon, 2016; Bernile, Bhagwat, and Rau, 2016).[9] Our findings contribute to the behavioral corporate finance literature that examines the impact of managerial biases on corporate decisions, since this is the first study that examines the effect of managerial sentiment on corporate policies. Additionally, our study contributes to this literature by showing that managerial emotions, induced by exogenous and economically irrelevant events, can be a significant source of variation in corporate policies across firms.

Additionally, several studies show that investors' trading behavior is correlated with local weather (Hirshleifer and Shumway, 2003; Goetzmann et al. 2015). In related research, Kaplanski and Levy (2010) show that stock prices decrease after aviation disasters, while Edmans, García, and Norli (2007) find that variation in stock returns is also associated with the outcomes of international soccer tournaments. In this Chapter, we contribute to the growing literature that associates sentiment with the decision-making of sophisticated financial agents.

[9] For a review of the behavioral corporate finance literature, see Baker and Wurgler (2012).

Further, we provide a new proxy to capture negative sentiment shocks to sophisticated financial agents such as corporate managers.

We also contribute to the broader finance and economics literature that analyzes the implications of terrorism. Ahern (2012) presents causal evidence that terrorist attacks adversely influence various psychological indicators. Di Tella and Schargrodsky (2004) and Gould and Stecklov (2009) show that terrorist attacks have an indirect economic effect as they could alter government policies. Other studies show that terrorist attacks influence political views (Gould and Klor, 2010), and election outcomes (Montalvo, 2011). More recently, Antoniou, Kumar and Maligkris (2016b) show that terrorist attacks influence the earnings forecasts of sell-side analysts and the process of information dissemination in financial markets. Our study complements this line of research by presenting new evidence that terrorist attacks and mass shootings affect the behavior of local corporate managers and induce them to adopt more prudent corporate policies.

The rest of the paper is organized as follows. In the next section, we describe the data and the empirical model. The main empirical results are reported in Section 2.3. We examine the robustness of our results in Section 2.4. We conclude in Section 2.5 with a brief summary.

2.2 Data and Methods

2.2.1 Econometric Model

Our empirical models test for changes in corporate policies in the quarter after the occurrence of an attack. We make this methodological choice because corporate policies are fairly "slow-moving", and, therefore require time to reflect managerial sentiment. Further, in a behavioral sense, temporary emotions can have a relatively long-lasting impact on decision making, since they can influence the core decisions that become the basis for subsequent decisions (i.e., Andrade and Ariely, 2009).

Specifically, following Bertrand and Mullainathan (2003), we use a difference-in-difference model (DiD) to capture the impact of attacks on the corporate policies of local firms. This methodology controls for fixed differences between the control and treatment groups via firm and time fixed effects. Similar to Bertrand and Mullainathan (2003), our treatment group includes all firms that are local to terrorist attacks that occur at time *t*. The control group includes all the remaining firms. In our baseline analysis, firms that are that are headquartered within 50 miles radius from the location of events are identified as local firms.[10] The empirical model has the following structure:

$$\mathrm{Y}_{\mathrm{i,s,t+1}} = \mathrm{c} + \alpha_{\mathrm{i}} + \delta_{\mathrm{t}} + \beta \times \mathrm{Impact}_{\mathrm{s,t}} + \gamma \times \mathrm{X}_{\mathrm{i,s,t}} + \varepsilon_{\mathrm{i,s,t+1}} \qquad (1)$$

In equation (1), *i* indexes firms, *t* indexes time (quarter) and *s* indexes location. $Y_{i,s,t+1}$ is the corporate policy (i.e., cash holdings, R&D expenditure or long-term-leverage) of firm *i* at time *t+1*, α_i is firm fixed effects, and δ_t is time fixed effects. Our main variable of interest is $Impact_{s,t}$, which is a dummy variable that equals one for firms that are local to an attack at time *t*.

Our empirical models include several control variables that have been shown in the previous literature to affect corporate policies. These variables are indexed in equation (1) with $X_{i,s,t}$, and include firm size (i.e., log(assets)), return on assets (ROA), market-to-book ratio (MB), growth of sales, and firm age (Hilary and Hui, 2009; Hirshleifer et al., 2012; Gao, Harford and Li, 2013; Hutton et al., 2014).[11] To control for the possibility that terrorist attacks and mass shootings are influenced by the local macroeconomic environment, we include the state-level macroeconomic index defined in Korniotis and Kumar (2013) as an additional control variable. An increase in the value of this index indicates improvements in the local macroeconomic conditions. The appendix provides a description of all variables.

[10] To determine the coordinates of the firms' headquarters and the location of the attacks, we use the services of Google Geocoding API V3 and GPS Geoplaner, respectively, which use Google maps and GPS data to produce the latitude and longitude of any given address or ZIP code. We follow the procedure in Vincenty (1975) to calculate the distance between these coordinates.

[11] Firm size and MB can relate to risks associated with distress (Fama and French, 1993); Firm age with risks associated to information uncertainty (Zhang, 2006); ROA and growth of sales with risks associated with expected growth rates (Johnson, 2002).

2.2.2 Data and Summary Statistics

The sample period for our baseline tests is from 1997 to 2012. The data on terrorist attacks and mass shootings come from the Global Terrorism Database (GTD)[12] and The Washington Post list (WP),[13] respectively. GTD is an open-source database that contains systematic data on terrorist attacks (START, 2012), while WP captures the deadliest shootings in U.S. history. Based on these databases, we collect information regarding the date, location, and the type of each event. We use the following filters to identify our list of extreme negative events: first, we retain events that occur in the U.S. Second, to ensure that our sample includes high-impact and salient events that are likely to generate negative sentiment, we only retain events that involve human casualties, and are covered in newspaper articles.[14] From the resulting sample, we eliminate 7 events for which we could not validate an exact location, and 2 events that involve robberies.[15] Table 2.1 lists the 25 events during the 1997-2012 period that are included in our final sample, and Figure 2.1 shows their geographical locations.

We obtain quarterly firm-level financial variables from Compustat. We exclude from our sample all firms that are not headquartered in the U.S. We also exclude utility and financial firms with SIC codes between 4910 to 4939 and 6000 to 6999, respectively. All firm-level variables are winsorized at the 1st and 99th percentile levels. Our sample includes only firms with non-missing zip codes from the first quarter of 1997 until the fourth quarter of 2012.

Table 2.2 presents the descriptive statistics for the variables included in our models, for the whole sample and when the sample is split between firms that were affected by an event ($Impact_{s,t}$=1) and those they were not ($Impact_{s,t}$=0). From Table 2.2, we observe some early evidence in support of our hypothesis as

[12] For more information, please see http://www.start.umd.edu/gtd/.

[13] This list contains the deadliest shootings in the U.S. See http://www.washingtonpost.com/wp-srv/special/nation/deadliest-us-shootings/.

[14] To find whether an event appeared in the media, we use Factiva to search all articles published in major U.S. newspapers (*The Los Angeles Daily News, The NY Daily News, The NY Post, The NY Times, The Wall Street Journal-US edition, The Washington Post* and *USA Today*) for a period of 7 days after the event. The keywords in this search are the name and type of the event, or the name of the place that the attack took place. In the robustness section, we conduct additional tests with an alternative sample of terrorist attacks, created using less stringent selection criteria.

[15] Since our aim is to examine the impact of unpredictable and salient events, we exclude robberies, which reflect common criminal activity.

affected firms exhibit higher levels of cash holdings, lower R&D expenditure, and long-term leverage in period *t+1*.

2.3 Empirical Results

2.3.1 Baseline Estimates

Our baseline results are presented in Table 2.3. We find that terrorist attacks and mass shootings affect the corporate policies of local firms. More specifically, local firms increase their cash holdings by 1.67% (*t*-statistic= 4.06) relative to non-local firms. In addition, local firms decrease their R&D expenditure by -0.17% (*t*-statistic= -2.99), and decrease their long-term leverage by -0.87% (*t*-statistic= -2.93). These results are economically meaningful and consistent with our conjecture that local firms would adopt more prudent policies around terrorist attacks, relative to firms that are headquartered away from the location of the events.

Examining the estimates of the control variables, we find that smaller and growth oriented firms have higher cash holdings, in line with the findings in Bates et al. (2009) and Dessaint and Matray (2016). Further, similar to Hilary and Hui (2009) and Hirshleifer et al. (2012), we find that larger firms and firms with higher ROA have lower R&D expenditure. Our results also show that firms with higher profits exhibit lower levels of long-term leverage, in line with the findings in Hutton et al. (2014). Last, we find that increase in the local macroeconomic index is associated with higher cash holdings, but there is no significant impact on R&D expenditure or long-term leverage.

2.3.2 Effect of Distance

We next examine the sensitivity of our findings to event-firm distance. According to our hypothesis, the impact of terrorist attacks on firm decisions should be more intense among firms that are closer to the event location. Therefore, we expect that, as we progressively expand our definition of “local” to include firms located farther away from the location of the negative event, the magnitude and statistical significance of $Impact_{s,t}$ would decrease.

To test this conjecture, we re-estimate equation (1) using different definition of "local". We define local firms as those firms with headquarters within 30, 50, 70, 90, 110, 130, and 150 miles from the location of negative events. We summarize the key results in Figure 2.2, where we report the coefficient estimates of $Impact_{s,t}$ for cash holdings, R&D expenditure, and long-term leverage for these different subsamples.

In line with our hypothesis, the results show that as we expand the definition of "local" to include more geographically distant firms, the coefficient estimates on $Impact_{s,t}$ decrease and become statistically insignificant as we move beyond a radius of 70-90 miles. This finding is consistent with our hypothesis, and suggests that the impact of terrorist attacks on corporate policies is stronger when a firm is headquartered closer to the location of the event.

2.3.3 Effect of Time

The impact of terrorist events on managerial sentiment is likely to be relatively short-lived, which implies that the observed changes to corporate policies would be temporary. To test this conjecture, we examine whether the observed changes to corporate policies of local firms last for more than one quarter after the events.

Table 2.4 shows the results. Consistent with a temporary sentiment effect, we find that the observed changes to corporate policies last only for one quarter after the events. Specifically, while local firms increase their one quarter ahead cash holdings by 1.67%, they do not significantly increase their two and three quarter ahead cash holdings. Similarly, local firms decrease their one quarter ahead R&D expenditure, and long-term leverage by 0.17% and 0.87%, respectively, but these changes become smaller and insignificant during the following two quarters. Even though this effect is significant only for period t+1, we observe a reversal in the effect during the periods t+2 and t+3 for the R&D expenditure and long-term leverage of local firms. This finding is consistent with Kaplanski and Levy (2010) who support that if the decision-making of decision-making agents is indeed driven by sentiment shocks instead of rational expectation, impact of sentiment shocks is not likely to be “permanent” and subsequent reversals may be observed. Overall, these findings suggest that the sentiment effects following terrorist events are economically meaningful, but they are short-lived.

2.3.4 Impact of Salience

In this section, we examine whether more salient events, which are likely to generate stronger negative sentiment among managers, lead to larger changes in corporate policies. To quantify the saliency of the negative events, we construct proxies based on news coverage, which has been shown to have a strong influence on behavior in different domains (Shiller, 2000; Tetlock, 2007; Tetlock et al., 2008; Barber and Odean, 2008; Da et al., 2011; García, 2013; Liu and McConnell, 2013). Our conjecture is that events that are featured more prominently in the media will affect managerial sentiment more strongly, and thus would have a larger impact on corporate policies. To test this conjecture, we use newspaper articles to identify whether an event is high or low in saliency, and then we re-estimate the model in equation (1) by interacting $Impact_{s,t}$ with dummy variables that correspond to these different cases.

To construct the media-based saliency proxies, we use Factiva to search for articles published in major media outlets in the seven-day period after each attack in our sample. The keywords for the search are the name and type of the event, or the name of the place that the event occurred. We examine articles from the following major outlets: *The Los Angeles Daily News, The NY Daily News, The NY Post, The NY Times, The Wall Street Journal-US edition, The Washington Post* and *USA Today*. We read all the articles to ensure that their main focus is the event in question. Using this procedure, we obtain 372 articles, which amounts to an average of 14.88 articles per attack.

Our first saliency proxy measures the length of articles by counting the number of words. To construct this proxy, we first gather all the articles corresponding to a specific attack, count the number of words in each article, noting the median of this distribution. We do this for every event, which results in an overall distribution of medians. If any event specific median is higher or equal to the median of this overall distribution, the dummy variable *Article-Size*$_h$ is equal to one, or else, it is set equal to zero. Similarly, *Article-Size*$_l$ is equal to one if the attack-specific median is less than the median of the overall distribution.

The second news coverage proxy is a dummy variable, which indicates whether an article is a leading story presented in the first page of a newspaper. Since leading stories are more salient, we expect that such events will exert a stronger impact on managerial sentiment and corporate policies. From our total sample of 372 articles, 76 of them are displayed on the first page of the newspaper outlets we consider. *First Page*$_{Dummy}$ is a dummy variable, which is equal to one if an event is presented on the first page of at least one newspaper.

We also consider a variation of this dummy variable by examining whether an attack featured as leading story on multiple days. Our conjecture is that attacks that are covered as leading stories on multiple days should exert a stronger influence on managerial sentiment and corporate policies. To construct this proxy, we gather all articles related to a specific event and count the number of days that this event was displayed as a cover story of a newspaper.[16] Among the 25 events in our sample, we find that the median number of days that they were presented on the first page of newspapers is two days. We compare the event-specific duration with the median duration of all the events in our sample. If the event-specific duration is greater or equal to the median duration of all events, the *First Page*$_{(Long\ Duration)}$ dummy variable is equal to one, or else, it is set to zero. Similarly, *First Page*$_{(Short\ Duration)}$ dummy variable is set to one if the event-specific duration is less than the overall median duration.

Table 2.5 presents the results, where the effect of *Impact*$_{s,t}$ on corporate policies is estimated separately for high and low salience events. In Panel A, salience is captured by the length of the article. Consistent with our hypothesis, we find that, across all three measures of corporate policies, the effect of terrorist attack is stronger for longer articles. Specifically, in relation to low salience events, high salience events are associated with an additional increase in cash

[16] If an event was never displayed on the first page, we consider it as zero duration.

holdings of 1.38%, and an additional decrease in R&D expenditure and long-term leverage by 0.22% and 2.01%, respectively.

In Panel B, salience is captured by coverage of terrorist attacks in a leading story. The results show that, relative to low salience events, local firms near high salience events increase their cash holdings by an additional 0.88%, and decrease their R&D expenditure and long-term leverage by an additional 0.26% and 1.57%, respectively. Last, in Panel C, event salience is captured by coverage as a leading story for multiple days. The results show that, relative to low salience events, local firms near high salience events increase their cash holdings by an additional 1.39%, and decrease their R&D expenditure and long-term leverage by an additional 0.17% and 1.50%, respectively.

In all Panels of Table 2.5, the row *difference* presents the difference between the coefficients on $Impact_{s,t}$ for the two groups (high vs. low saliency). We also report whether the difference is significant using a Wald test. We find that across all different corporate policy measures and salience proxies, the differences are statistically significant.

Overall, the results in Table 2.5 support our key hypothesis, and demonstrate that corporate policies are affected more strongly by more salient events that are more likely to adversely influence managerial sentiment.

2.3.5 Role of Demographics

Several studies in psychology show that younger people are less able to control their emotions, and are thus more prone to make emotionally-driven decisions (Carstensen et al., 2000; Blanchard-Fields et al., 2007; Scheibe and Blanchard-Fields, 2009). In addition, younger people are likely to be less experienced, and several studies have shown that lack of experience can lead to stronger behavioral biases (List, 2003; Dhar and Zhu, 2006). Motivated by this evidence, we examine whether the changes in corporate policies among firms local to terrorist attacks are particularly pronounced among firms with younger CEOs.

To test this conjecture, we obtain the age of CEOs from Execucomp, and define the $Age_{(Low)}$ ($Age_{(High)}$) dummy. The variable equals one if the CEO's age falls in the bottom (top) third of the age distribution for that particular industry, where we use the Fama and French (1997) 48 industry classification (correspondingly *Age*$_{(Mid)}$ dummy variable equals one if the CEO's age falls in the middle third of the age distribution).[17] The classifications of CEOs used here capture material differences in their characteristics. The average age for CEOs in the low age group is 46 years, whereas the average age for CEOs in the high age group is 63 years. In addition, CEOs in the low age group have an average of 17 quarters of company-specific CEO experience, while the corresponding number for CEOs in the high age group is 23 quarters (p-value of difference$\approx$ 0.000).

We estimate the model in equation (1) by interacting these age-related dummies with $Impact_{s,t}$. In this model, in addition to the controls used in the baseline model in Table 2.3, we also control for the CEO's gender since experimental evidence show that males are less risk averse than females (Antoniou, Harrison, Lau and Read, 2016). Due to limited data availability in Execucomp, the sample used for this test is significantly smaller, containing roughly 30% of the observations used in the baseline analysis (Table 2.3).[18]

The results are presented in Table 2.6. In columns 1, 3 and 5 we estimate our baseline model for this smaller sample without accounting for CEO age. We find that our baseline results continue to hold in this smaller sample. The coefficient on $Impact_{s,t}$ has the expected sign across all three corporate policy measures and is statistically significant in all cases.

In columns 2, 4 and 6 of Table 2.6, we consider CEO age and estimate the regression model for cash holdings, R&D expenditure, and long-term leverage variable, respectively. The results show that younger CEOs increase cash holdings by 4.08%, and decrease R&D and leverage by 0.26% and 2.52%, respectively. These changes to corporate policies by younger CEOs are statistically significant. The corresponding changes for older CEOs are 1.98%, -0.04%, and -2.00%, and are statistically insignificant. The difference in the coefficient of $Impact_{s,t}$ for low and high age CEOs is statistically significant only for R&D expenditure. For the

[17] We consider industry-adjusted benchmarks, since CEO age varies systematically between certain types of firms (e.g., Acemoglu, Akcigit, and Celik, 2014).
[18] Further, the sample period of cash holdings starts from 2002 to 2012 and is based on 15 negative events instead of 25.

remaining two variables, the difference is in the predicted direction but it is statistically insignificant, perhaps due to reduced sample size. Overall, the results in this section show that changes to corporate policies in response to terrorist attacks are mainly concentrated among firms managed by younger CEOs.

2.4 Robustness Tests and Alternative Explanations

Our main hypothesis posits that changes to corporate policies among firms close to terrorist attacks reflect the impact of emotions. These changes are unlikely to reflect adjustments to economic shocks. In this section, we conduct several tests to examine whether attacks indeed influence managerial sentiment, and whether local firms experience economic shocks during event periods.

2.4.1 Terrorist Attacks and Managerial Sentiment: A Direct Test

We first analyze the linguistic tone in the MD&A section of financial reports to investigate whether terrorist attacks adversely influence managerial sentiment.[19] Recent studies show that linguistic tone in the MD&A section reflects managerial sentiment (Feldman, Govindaraj, Livnat and Segal, 2010; Li, 2010; Loughran and McDonald, 2011; Bochkay and Dimitrov, 2014).[20] In our economic setting, we expect that managers who are local to terrorist attacks will be relatively more pessimistic in their communication.

We use the dictionary for extreme positive and negative words developed in Bochkay, Chava and Hales (2016) to analyze the tone of the MD&A section using the Bochkay and Dimitrov (2014) procedure.[21] Specifically, we analyze three

[19] According to the SEC, the objective of the MD&A according is to provide a narrative explanation of a company's financial statements, as well as to communicate information related to potential variability of earnings and cash flows. Thus, through linguistic analysis of tone, the sentiment of managers at the time of writing can be identified.

[20] For example, Bochkay and Dimitrov (2014) show that an aggregate index of managerial sentiment constructed using linguistic analysis of tone in MD&A's is strongly correlated to the Baker and Wurgler (2006) investor sentiment index. They also find that optimistic (pessimistic) tone predicts a deterioration in firm performance, which is consistent with the view that tone in MD&A captures managerial sentiment that is unrelated to firm fundamentals.

[21] This dictionary captures tone more precisely as it rates words according to their tone. It is more general than the standard binary dictionary developed in Loughran and McDonald (2011), which

variables: *Extreme Pos*, *Extreme Neg* and *Pessimistic Tone*. The *Extreme Pos* (*Neg*) variable is equal to the number of extreme positive (negative) words (i.e., N_{Pos} and N_{Neg}, respectively) scaled by the total number of words in the MD&A section. Since MD&A section may include both extreme positive and extreme negative words, we set the *Pessimistic Tone* variable equal to the logarithm of $(1+N_{Neg})/(1+N_{Pos})$ to capture the negative to positive linguistic tone imbalance.[22]

Panel A of Table 2.7 presents the summary statistics for our linguistic measures, and Panel B presents the results from OLS regressions.[23] We find a negative and significant relation between *Extreme Pos* and $Impact_{s,t}$, and a positive and significant relation (at the 10% level) between *Pessimistic Tone* and $Impact_{s,t}$. These estimates provide direct evidence of an impact of local negative events on managerial sentiment.

One caveat with this analysis is that the MD&A section provides a low frequency snapshot of managerial sentiment, and it is also partly contaminated by the influence of other agents such as legal teams and auditors. Clearly, it would be preferable to analyze more direct proxies of managerial sentiment at higher frequencies. Nevertheless, the results in this section suggest that exposure to terrorist attacks affect managerial mood and sentiment.

2.4.2 Emotional or Economic Impact?

We continue our analysis with tests that examine whether terrorist attacks relate to firm fundamentals. The first test examines whether terrorist attacks tend to occur when the state-level macro-economic conditions are worse. We regress state level GDP growth at time *t* on dummy that equals to 1 if an attack has occurred in that state at time *t*, and also include time and state fixed effects.[24] In untabulated results, we find that the terrorist attack dummy is *insignificant* in this regression,

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only classifies words into positive or negative. For more details on the dictionary, see Bochkay et al. (2016).

[22] *Pessimistic Tone* captures negative to positive word imbalance, and is used in a similar manner in Tetlock et al. (2008).

[23] We continue to use the difference-in-difference specification with controls, as outlined in the previous section. However, we examine whether tone is affected in the same quarter as the attack. We choose this specification because the MD&A section is considerably more “fast-moving” than corporate policies, and is therefore more likely to respond to attacks.

[24] The state-level GDP data are obtained from the Bureau of Economic Analysis.

which suggests that attacks are not related to the local macro-economic environment.[25]

Next, we conduct firm-level tests to examine whether local companies experience economic shocks that may justify changes to corporate policies. First, we test the parallel trends assumption, where we estimate our model in equation (1) after including lagged values of the treatment variable, $Impact_{s,t-1}$ and $Impact_{s,t-2}$. Our goal is to examine whether changes to the corporate policies we document are related to any pre-existing shocks that are unrelated to the attacks. Panel A in Table 2.8 reports the results, and shows that the coefficient estimates on $Impact_{s,t-1}$ and $Impact_{s,t-2}$ are insignificant, whereas the estimate on $Impact_{s,t}$ remains highly significant. This evidence suggests that changes to corporate policies only occur around the period of the events, and are temporary (as shown in Table 2.4).

In our next test, we examine whether firm-level indicators of fundamentals, which are set *externally* to the firm by other agents or the market, change around the period of the events. Specifically, we estimate four different versions of our models, using one of the following four variables as the dependent variable: the firms' credit ratings from Standard and Poor's (S&P), their average analyst recommendation from the Institutional Brokers' Estimate System (I/B/E/S), their stock price volatility calculated from daily return data from the Center of Research in Security Prices (CRSP), and their sales obtained from Compustat.[26] These variables are important indicators of the state of firms' fundamentals, as it is likely that companies that experience economic shocks will experience changes in at least some of these measures. In addition, because the agents responsible for producing these indicators are largely external to the firm (credit risk experts, sell-side analysts, investors and consumers, respectively), any changes in them among local firms during attack periods would suggest that agents, whose sentiment is

[25] This regression is conducted using annual data. In similar analysis, we use as the state-level macroeconomic index developed in Korniotis and Kumar (2013) as the dependent variable and re-run a similar model on quarterly frequency. Again, the coefficient on the terrorist attack dummy is insignificant.
[26] Table 2A.2 and Table 2A.3 in the Appendix provide descriptive statistics on credit rating and recommendation data used in our analysis.

unlikely to be affected by the attacks, are adjusting their behavior toward these firms due to an economic shock.[27]

The results are shown in Table 2.8, Panel B. In each case, for robustness, we present results from two separate models, where the dependent variable is measured at time *t* or *t*+1. Across all four indicators, we find that the coefficient on $Impact_{s,t}$ is indistinguishable from zero. This evidence suggests that local firms around attack periods do not experience any changes to their credit ratings, analyst recommendations, stock price volatility, or sales. Collectively, these results suggest that local firms around attack periods do not experience significant economic shocks.

One potential concern in our analysis is that the control group (i.e., the entire set of non-local firms) is too coarse and may fail to capture fundamental shifts in corporate policies among treated firms. To address these potential concerns, we create alternative control groups with firms that have similar characteristics as the firms in our treatment group along firm-specific (i.e., *Log(assets)*, *MB ratio*, *Sales growth*) and aggregate dimensions (*Industry, Macro-state index*). To construct the matched sample, we use the nearest neighbor matching estimator, which allows us to match firms with similar propensity scores and thus similar characteristics. We then drop all firms from the sample that are not matched, and re-estimate the baseline model.

Panel C in Table 2.8 presents our results and shows that our main findings are robust when we use different matched control groups, with coefficient estimates similar in magnitude with those presented in Table 2.3.

2.4.3 Additional Robustness Tests

We perform additional tests to examine the robustness of our findings. First, we conduct placebo tests, which examine whether our findings arise mechanically, perhaps due to some methodological flaw. In the first placebo test, we randomly assign a new date to each of the terrorist events in our sample during the period 1997-2012, and create the dummy variable $Impact_{(Random\ time)}$. This dummy

[27] If these agents are local, they may adjust their behavior due to attack-induced sentiment. However, these agents are not likely to be local to treated firms, at least not on average.

variable takes the value of one if firms are local to the attacks at these random times, and zero otherwise. Then, we estimate our baseline model in equation (1), recording the coefficient, the standard error, and the *p*-value of this variable. We repeat this procedure 1000 times. In Panel A of Table 2.9, we report the average coefficient, the standard error, and the *p*-value of $Impact_{(Random\ time)}$. The results show that, across all three corporate policy proxies, the average coefficient of $Impact_{(Random\ time)}$ is indistinguishable from zero, with an average *p*-value of at least 0.37.

In the second placebo test, we randomize the location of each event in our sample,[28] forming the variable $Impact_{(Random\ location)}$. This dummy variable takes the value of one if firms are local to the random locations at the time of the attack, and zero otherwise. We estimate the baseline model in equation (1), and record the coefficient, the standard error, and the *p*-value of this variable. We repeat this procedure 1000 times. In Panel B of Table 2.9, we report the average coefficient, the standard error, and the *p*-value of $Impact_{(Random\ location)}$. Again, across all three corporate policy proxies, we find that the average coefficient of $Impact_{(Random\ location)}$ is indistinguishable from zero. Overall, the evidence from the two placebo tests suggests that the changes to the corporate policies we document only occur at the time of the attacks among local firms.

In the next test, we examine the sensitivity of our results to our assumption about missing R&D data. In the analysis in Table 2.3, we treat missing values for R&D expenditure as zero expenses. However, missing values of these expenses do not necessarily mean that firms have zero R&D costs (Hilary and Hui, 2009). In the next test, we drop observations with missing R&D values. Table 2.9, Panel C reports our findings. The first column of Panel C shows results when we estimate the model shown in equation (1) for the reduced sample. The second column presents the baseline results from Table 2.3 to facilitate comparisons. Our baseline results remain robust when we consider the reduced sample.

Next, we establish that our results are not driven by the economic effects associated with the 9/11 attacks. The 9/11 terrorist attacks are by far the most

[28] Since terrorist attacks and mass shootings do not occur in uninhabitable locations such as deserts and lakes, we use U.S. Census Bureau's files to collect the coordinates of all habitable locations in the U.S.

economically impactful events in our sample, as evidenced by the sharp decline in the global stock market during that period. We repeat the analysis in equation (1) after excluding the three 9/11-related events from the sample. Panel D of Table 2.9 shows the results. Our results are robust and remain consistent with the baseline findings in Table 2.3.

The next robustness test uses a larger sample of events. In our initial event sample, we obtain mass shootings data from the WP list, which contains only the important events with the large number of human casualties.[29] Further, we only considered events that are featured in important media outlets. We focus on these events because they are high-profile negative events, which are more likely to have a significant effect on the sentiment of corporate managers. In the robustness test, we use an alternative mass shooting sample, in which we include events with fewer human casualties. This sample is obtained from Stanford's Mass Shootings in America database (MSA). We complement our sample with events from this database that caused at least 6 human casualties. Stanford's MSA provides the date and the location of each event. We obtain data on the location and the date of each event for the 1997-2012 period. From the resulting list, we eliminate 5 events as there are no local firms around the period of those events. For this sample, we also do not require that the events be featured in the national media outlets.

Panel A of Table 2.10 lists the 25 events that we additionally include in our initial event sample, broken down into sub-categories based on the casualties associated with each event. In Panel B, we re-estimate equation (1) using this expanded sample. We estimate several models, gradually expanding the sample to include mass shootings with less human casualties. We find that our results continue to hold in this alternative sample, but as expected, they get progressively weaker as we expand the sample to include events with fewer casualties. This finding is reasonable as we consider events that are less salient (fewer casualties and potentially lower media coverage) and, therefore, they are likely to be associated with weaker shocks to the sentiment of local managers.

[29] WP list contains all the mass shooting events with 12 or more casualties.

2.5 Summary and Conclusion

This paper shows that terrorist attacks and mass shootings, events that are known to induce negative emotions, affect the corporate decisions of managers who are located near these events. Using textual analysis of the MD&A section of financial reports, we first show directly that exposure to terrorist attacks adversely influences managerial sentiment. Further, we demonstrate that, in comparison to non-local firms, local firms around attack periods increase the level of their cash holdings and decrease their R&D expenditure as well as long-term leverage. These effects are mainly concentrated among firms managed by younger CEOs, and are stronger when the negative event is salient. We use media coverage of an event and its distance from firm headquarters to quantify salience. Local firms do not experience changes to their credit ratings, analyst recommendations, stock price volatility, or firm sales during the attack periods. This evidence suggests that shifts in corporate policies are unlikely to reflect impact of local economic shocks.

Collectively, these results suggest that exogenous and economically irrelevant negative events influence managerial sentiment. Consequently, corporate managers adopt more prudent and conservative corporate policies. In future work, it would be interesting to examine whether corporate managers exhibit weaker or stronger emotional reaction to extreme negative events compared to other market participants such as investors and equity analysts. It would also be useful to examine whether managers learn to control their emotional reaction to exogenous events as they are exposed to multiple negative events. In particular, corporate managers who are exposed to similar events early in their lives may exhibit weaker emotional reaction and may also learn faster.

Table 2.1. Sample of Terrorist Events

This table shows the event sample during the 1997-2012 period. All events took place in the U.S., resulted in at least one human casualty, and were covered in newspapers.

No	Events	Date	Location
1	Empire State Building	23 Feb 1997	New York City, NY
2	Abortion Clinic Bombing	29 Jan 1998	Birmingham, AL
3	U.S. Capitol	24 Jul 1998	Washington, DC
4	Barnett Slepian Murder	23 Oct 1998	Amherst, NY
5	Columbine High School	20 Apr 1999	Littleton, CO
6	Korean Methodist Church	04 Jul 1999	Bloomington, IN
7	9/11 Attacks: World Trade Center	11 Sep 2001	New York City, NY
8	9/11 Attacks: Hijacked Plane Crashed	11 Sep 2001	Alexandria, VA
9	9/11 Attacks: Hijacked Plane Crashed	11 Sep 2001	Somerset County, PA
10	Bank of America	05 Jan 2002	Tampa, FL
11	LA International Airport	04 Jul 2002	Los Angeles, CA
12	Seattle Jewish Federation	28 Jul 2006	Seattle, WA
13	Virginia Tech	16 Apr 2007	Blacksburg, VA
14	Knoxville Church	27 Jul 2008	Knoxville, TN
15	Immigration Centre	03 Apr 2009	Binghamton, NY
16	George Tiller Murder	31 May 2009	Wichita, KS
17	Little Rock	01 Jun 2009	Little Rock, AR
18	Holocaust Museum	10 Jun 2009	Washington, DC
19	Fort Hood	05 Nov 2009	Killeen, TX
20	IRS Building	18 Feb 2010	Austin, TX
21	Pentagon	04 Mar 2010	Arlington, VA
22	Discovery Communications	01 Sep 2010	Silver Springs, MD
23	Aurora	20 Jul 2012	Aurora, CO
24	Sikh Temple	05 Aug 2012	Oak Creek, WI
25	Sandy Hook School	14 Dec 2012	Sandy Hook, CT

Table 2.2. Summary Statistics

This table presents the summary statistics for all the variables. The sample includes all the nonutility and nonfinancial firms from the 1st quarter of 1997 to the 4th quarter of 2012. All sample firms are located in the U.S. The samples of the dependent variables are unbalanced due to the limited availability of certain variables in Compustat. We define as *affected firms* those with headquarters within a 50 miles radius from an attack at time *t*. Accordingly, *unaffected firms* represent the rest of the firms in the sample.

Panel A: Descriptive Statistics

Variable	Obs.	Mean	Std. Dev.	25th Pctl.	Median	75th Pctl.
Cash holdings$_{t+1}$	39,032	0.17	0.18	0.04	0.11	0.24
R&D expenditure$_{t+1}$	143,311	0.02	0.05	0.00	0.00	0.03
Long-term leverage$_{t+1}$	135,002	0.24	0.35	0.00	0.08	0.38
Impact$_{s,t}$	143,311	0.01	0.10	0.00	0.00	0.00
Log(assets)	143,311	4.86	2.00	3.55	4.90	6.24
ROA	143,311	-0.04	0.18	-0.04	0.00	0.02
MB ratio	143,311	2.94	6.84	0.97	1.89	3.61
Sales growth	143,311	0.27	1.27	-0.30	0.11	0.77
Firm age	143,311	8.17	5.50	4.00	7.00	12.00
Macro-state index	143,311	-0.01	0.56	-0.35	-0.04	0.34

Panel B: Affected and Unaffected Firms

Variable	Affected Firms			Unaffected Firms		
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
Cash holdings$_{t+1}$	314	0.20	0.19	38,718	0.17	0.18
R&D expenditure$_{t+1}$	1,483	0.02	0.04	141,828	0.02	0.05
Long-term leverage$_{t+1}$	1,368	0.23	0.34	133,634	0.24	0.35
Log(assets)	1,483	4.59	2.13	141,828	4.87	2.00
ROA	1,483	-0.04	0.15	141,828	-0.04	0.18
MB ratio	1,483	2.78	6.66	141,828	2.94	6.84
Sales growth	1,483	0.14	1.30	141,828	0.27	1.27
Firm age	1,483	7.07	5.28	141,828	8.18	5.50
Macro-state index	1,483	-0.24	0.56	141,828	-0.01	0.56

Table 2.3. Terrorist Events and Corporate Policies: Baseline Estimates

This table presents the results from regressions that examine the impact of terrorist attacks on firm policies. The sample includes all the nonutility and nonfinancial firms from the 1st quarter of 1997 to the 4th quarter of 2012, which are located in the U.S. We define as local firms those that have their headquarters inside a 50 miles radius from an event. The dependent variables are one quarter ahead cash holdings, R&D expenditure, and long-term leverage. All regressions include year-quarter fixed effects and firm fixed effects. Standard errors, shown in parentheses, are clustered at the local level. All regression coefficients and standard errors are multiplied by 100. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

	Cash Holdings$_{t+1}$			R&D Expenditure$_{t+1}$			Long-term Leverage$_{t+1}$		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
Impact$_{s,t}$	1.54***	1.66***	1.67***	-0.21***	-0.17***	-0.17***	-0.97***	-0.87***	-0.87***
	(0.41)	(0.41)	(0.41)	(0.05)	(0.06)	(0.06)	(0.29)	(0.30)	(0.30)
Log(assets)		-3.25***	-3.26***		-0.79***	-0.79***		-0.23	-0.23
		(0.21)	(0.21)		(0.03)	(0.03)		(0.25)	(0.25)
ROA		2.92***	2.92***		-3.11***	-3.11***		-7.54***	-7.54***
		(0.61)	(0.61)		(0.24)	(0.24)		(1.10)	(1.10)
MB ratio		0.06***	0.05***		0.01***	0.01***		-0.28***	-0.28***
		(0.01)	(0.01)		(0.00)	(0.00)		(0.02)	(0.02)
Sales growth		-0.28***	-0.27***		0.02*	0.02*		0.44***	0.44***
		(0.03)	(0.03)		(0.01)	(0.01)		(0.06)	(0.06)
Firm age		-0.25	-0.26		-0.03*	-0.03*		1.37***	1.38***
		(0.46)	(0.47)		(0.02)	(0.02)		(0.46)	(0.46)
Macro-state index			0.25*			0.02			0.03
			(0.13)			(0.02)			(0.25)
Constant	27.40***	29.90***	30.10***	2.31***	5.69***	5.69***	18.90***	18.60***	18.60***
	(0.39)	(0.96)	(0.98)	(0.09)	(0.14)	(0.15)	(0.33)	(1.32)	(1.31)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	39,032	39,032	39,032	143,311	143,311	143,311	135,002	135,002	135,002
Adjusted R^2	0.73	0.73	0.73	0.59	0.61	0.61	0.59	0.59	0.59

Table 2.4. Duration of Changes in Corporate Policies

This table presents the results from regressions that examine the impact of attacks on firm policies during the following quarters. The sample includes all the nonutility and nonfinancial U.S. firms from the 1st quarter of 1997 to the 4th quarter of 2012. We define as local firms those that have their headquarters within a 50 miles radius from an attack. To examine the impact of attacks on the corporate policies of local firms, we use the following model: $\mathrm{Y}_{\mathrm{i,s,t+i}} = \mathrm{c} + \alpha_{\mathrm{i}} + \delta_{\mathrm{t}} + \beta\,\mathrm{Impact}_{\mathrm{s,t0}} + \gamma\,\mathrm{X}_{\mathrm{i,s,t+i-1}} + \varepsilon_{\mathrm{i,s,t+i}}$. The dependent variables are defined as the one, two and three quarters ahead cash holdings, R&D expenditure, and long-term leverage. $Impact_{s,t0}$ is a dummy equal to 1 if firm's headquarters, at quarter t_0, is local to attacks occurred at time t_0. We also include all control variables as in Table 2.3. We run one regression for each i=1, 2 and 3, and report the coefficient and standard error on $Impact_{s,t0}$. All regressions include year-quarter fixed effects and firm fixed effects. Standard errors, shown in parentheses, are clustered at the local level. All regression coefficients and standard errors are multiplied by 100. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

Dependent Variable	Independent Variable	Control Variables	Firm F.E.	Time F.E.
	$\mathrm{Impact}_{s,t0}$			
Cash Holdings$_{t+1}$	1.67*** (0.41)	Yes	Yes	Yes
Cash Holdings$_{t+2}$	0.59 (1.12)	Yes	Yes	Yes
Cash Holdings$_{t+3}$	1.00 (0.86)	Yes	Yes	Yes
R&D Expenditure$_{t+1}$	-0.17*** (0.06)	Yes	Yes	Yes
R&D Expenditure$_{t+2}$	0.10 (0.06)	Yes	Yes	Yes
R&D Expenditure$_{t+3}$	0.08 (0.06)	Yes	Yes	Yes
Long-term Leverage$_{t+1}$	-0.87*** (0.30)	Yes	Yes	Yes
Long-term Leverage$_{t+2}$	-0.29 (0.25)	Yes	Yes	Yes
Long-term Leverage$_{t+3}$	0.39 (0.42)	Yes	Yes	Yes

Table 2.5. Terrorist Events, Saliency and Corporate Policies

This table presents the results from regressions that examine the impact of attack saliency on the corporate policies of local firms. In Panel A, we test whether events that are presented in longer articles are more salient. If the median size of articles for an attack is greater or equal to the median length of articles from all the attacks in our sample, the dummy variable $\textit{Article-Size}_h$ is set equal to one, or else $\textit{Article-Size}_l$ is equal to one. In Panel B, we examine whether first page articles exert a stronger impact on corporate policies. $\textit{First Page}_{(Dummy=1)}$ is a dummy variable equal to one if an event is presented on the first page of at least one newspaper. In Panel C, we test whether attacks that are covered on the first page for multiple days have a stronger influence on corporate policies. $\textit{First Page}_{(Long\ Duration)}$ is equal to one if the attack-specific duration of articles placed in the first page is higher or equal to 2 days, while $\textit{First Page}_{(Short\ Duration)}$ is equal to one if the duration is less than 2 days. In each specification, the row *difference* measures the difference between the coefficients on $\textit{Impact}_{s,t}$ for the two groups (high vs. low saliency). We use a Wald test to examine if this difference is statistically significant. We include all control variables, year-quarter fixed effects and firm fixed effects as in Table 2.3. Standard errors, shown in parentheses, are clustered at the local level. All regression coefficients and standard errors are multiplied by 100. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

Panel A: Events and Article Size

	Cash Holdings$_{t+1}$	R&D Expenditure$_{t+1}$	Long-term Leverage$_{t+1}$
$\text{Impact}_{s,t}$ × Article-Size_h	2.07***	-0.21***	-1.26***
	(0.42)	(0.05)	(0.43)
$\text{Impact}_{s,t}$ × Article-Size_l	0.69	0.01	0.75
	(0.61)	(0.09)	(0.51)
Control Variables	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes
Difference	1.38**	-0.22**	-2.01***

Panel B: Events and First Page

	Cash Holdings$_{t+1}$	R&D Expenditure$_{t+1}$	Long-term Leverage$_{t+1}$
$\text{Impact}_{s,t}$ × $\text{First Page}_{(Dummy=1)}$	1.68***	-0.21***	-1.12**
	(0.41)	(0.05)	(0.40)
$\text{Impact}_{s,t}$ × $\text{First Page}_{(Dummy=0)}$	0.80***	0.05	0.45
	(0.28)	(0.12)	(0.56)
Control Variables	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes
Difference	0.88*	-0.26*	-1.57**

Table 2.5—*Continued*

Panel C: Events and First Page Duration			
	Cash Holdings$_{t+1}$	R&D Expenditure$_{t+1}$	Long-term Leverage$_{t+1}$
Impact$_{s,t}$ × First Page$_{(Long\ Duration)}$	2.06***	-0.21***	-1.21**
	(0.41)	(0.05)	(0.45)
Impact$_{s,t}$ × First Page$_{(Short\ Duration)}$	0.67	-0.04	0.29
	(0.65)	(0.07)	(0.48)
Control Variables	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes
Difference	1.39**	-0.17*	-1.50**

Table 2.6. Terrorist Events, CEO Age and Corporate Policies

This table presents results from regressions that examine how terrorist attacks affect the policies of local firms, conditioned upon whether these firms are managed by young, middle-aged, or older CEOs. To capture the effect on different age groups, we create three dummy variables: $Age_{(Low)}$, $Age_{(Middle)}$ and $Age_{(High)}$, which correspond to young, middle-aged and older CEOs, respectively. $Age_{(Low)}$ is equal to one if CEOs' age is equal to or below the 33th percentile in their industry using the Fama-French 48 industry classification, and zero otherwise. Accordingly, $Age_{(High)}$ is equal to one if the age is equal to or above the 67th percentile. $Age_{(Middle)}$ is equal to one if both $Age_{(Low)}$ and $Age_{(High)}$ are equal to zero. The sample for the specification of cash holdings includes all nonutility and nonfinancial U.S. firms from the 2nd quarter of 2002 to the 4th quarter of 2012. The sample for R&D expenditure and long-term leverage specifications is from the 1st quarter of 1997 to the 4th quarter of 2012. In each specification, we measure the difference of the interaction terms between the two groups (low vs. high age), and we perform a Wald test to test whether the coefficients are statistically different. All regressions include similar control variables as in Table 2.3, plus year-quarter fixed effects and firm fixed effects. Standard errors, shown in parentheses, are clustered at the local level. All regression coefficients and standard errors are multiplied by 100. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

	Cash Holdings$_{t+1}$		R&D Expenditure$_{t+1}$		Long-term Leverage$_{t+1}$	
	[1]	[2]	[3]	[4]	[5]	[6]
Impact$_{s,t}$	2.24***		-0.14*		-2.50***	
	(0.41)		(0.07)		(0.83)	
Impact$_{s,t}$ × Age$_{(Low)}$		4.08***		-0.26***		-2.52***
		(1.06)		(0.07)		(0.82)
Impact$_{s,t}$ × Age$_{(Middle)}$		1.19		0.03		-4.97**
		(0.69)		(0.18)		(2.26)
Impact$_{s,t}$ × Age$_{(High)}$		1.98		0.04		-2.00
		(1.90)		(0.12)		(1.18)
Age$_{(Low)}$		-0.12		0.05***		0.54
		(0.48)		(0.02)		(0.84)
Age$_{(High)}$		-0.98**		-0.01		1.45**
		(0.35)		(0.02)		(0.59)
CEO gender$_{(Male)}$		-0.18		0.15***		-1.39
		(0.59)		(0.04)		(1.66)
Constant	42.40***	42.00***	4.94***	4.67***	20.10***	24.10***
	(1.58)	(2.04)	(0.35)	(0.39)	(5.91)	(8.02)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes
Difference		2.1		-0.3***		-0.52
N	15,051	14,854	39,433	35,306	37,896	33,942
Adjusted R^2	0.75	0.75	0.65	0.67	0.65	0.67

Table 2.7. Terrorist Events and Managerial Tone in MD&A Section

In this table, we examine the effect of terrorist events on the linguistic tone of the Managerial Discussion and Analysis (MD&A) section of companies' quarterly financial statements. Following Bockay et al. (2016), we define *Extreme Pos (Neg)* as the number of extreme positive (negative) words divided by the total number of words in each MD&A section. Further, we consider *Pessimistic Tone*, which is equal to $\log((1+N_{Neg})/(1+N_{pos}))$, where N_{Neg} and N_{Pos} are the number of extreme negative and positive words, respectively. To minimize the measurement error of these variables, we drop observations in the upper 1% of their distribution. In Panel A, we present the summary statistics of these variables. In Panel B, using an OLS, we examine whether local managers to terrorist events increase the number of extreme positive and negative words included in the MD&A section. All regressions include year-quarter fixed effects, firm fixed effects, and similar control variables as in Table 2.3. Standard errors, shown in parentheses, are clustered at the local level. Summary statistics presented in the Panel A are multiplied by 100. To improve the readability of the results in Panel B, we multiply the coefficient estimates and the standard errors with 1000. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

Panel A: Descriptive Statistics

	Obs.	Mean	Std. Dev.
Extreme Positive	75,210	0.27	0.16
Extreme Negative	75,210	0.11	0.10
Pessimistic Tone	75,013	-89.68	84.11

Panel B: Extreme Positive and Negative Tone

	Extreme Pos	Extreme Neg	Pessimistic Tone
$Impact_{s,t}$	-0.05**	0.03	46.66*
	(0.02)	(0.02)	(25.59)
Control Variables	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes
N	75,210	75,210	75,013
Adjusted R^2	0.57	0.42	0.47

Table 2.8. Regression Estimates with Controls for Local Economic Shocks

In this table, we report results that examine whether attacks are related to economic shocks. In Panel A, we include lag values of *Impact*$_{s,t}$ in our model to test the parallel trends assumption and re-estimate the model in equation (1). In Panel B, we examine whether terrorist events had an impact on the following economic indicators: credit ratings, analysts' recommendations, stock return volatility, and sales of local firms. To measure the impact of terrorist events on the credit worthiness of local firms, we use as dependent variable the *Credit Rating* variable, which ranges from 22 for the highest rating (AAA) to 1 if the rating is equal to selective default. To examine potential effects of terrorist events on stock recommendations, we create the *Average Recommendation* variable, which is equal to the mean recommendations of all stock analysts for each firm during each quarter. Analysts' recommendations can be equal to 5 (Strong Buy), 4 (Buy), 3 (Hold), 2 (Underperform) and 1 (Sell). We also focus on the effects of terrorist events on stock return volatility. We measure *Stock Return Volatility* as the standard deviation of daily stock returns during each quarter. Finally, we examine potential changes in the sales of local firms. We measure firm sales as the fraction of quarterly sales divided by quarterly assets. In Panel C, we present the results from estimating our baseline specification for a sample of matched firms. To construct the matched sample, we use the nearest neighbor matching estimator, which allows us to match firms with similar propensity scores. Firms with comparable propensity scores correspond to firms with similar characteristics. To estimate the propensity scores, we use major attributes such as *Log(assets)*, *MB ratio*, *Sales growth*, *Macro-state index* and firm's *Industry*. We define *Industry* using the Fama-French 48 industry classification. We then re-estimate our baseline model using as control group only the matched firms. All regressions in this table include year-quarter fixed effects and firm fixed effects. Standard errors, shown in parentheses, are clustered at the local level. All regression coefficients and standard errors are multiplied by 100. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

Panel A: Pre-Existing Shocks

	Cash Holdings$_{t+1}$	R&D Expenditure$_{t+1}$	Long-term Leverage$_{t+1}$
Impact$_{s,t}$	1.65***	-0.14***	-1.06***
	(0.40)	(0.05)	(0.28)
Impact$_{s,t-1}$	0.87	0.09	-0.15
	(1.27)	(0.06)	(0.29)
Impact$_{s,t-2}$	0.80	0.10	0.53
	(0.86)	(0.06)	(0.49)
Control Variables	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes
N	38,845	141,863	133,619
Adjusted R^2	0.73	0.62	0.59

Table 2.8—*Continued*

Panel B: Attacks and Economic Indicators

	Credit Rating		Average Recommendation		Stock Return Volatility		Firm Sales	
	[t]	[t+1]	[t]	[t+1]	[t]	[t+1]	[t]	[t+1]
$Impact_{s,t}$	5.75	0.15	4.92	-3.51	0.64	0.13	0.10	0.26
	(5.41)	(3.26)	(4.42)	(5.00)	(0.52)	(0.76)	(0.31)	(0.34)
Firm F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	13,650	13,014	30,742	27,770	5,226	4,948	143,311	139,063
Adjusted R^2	0.86	0.85	0.20	0.18	0.16	0.16	0.78	0.77

Panel C: Propensity Score Matching

Matching Attributes	Cash $Holdings_{t+1}$	R&D $Expenditure_{t+1}$	Long-term $Leverage_{t+1}$	Control Variables	Firm F.E.	Time F.E.
MB ratio, Industry	2.69***	-0.18*	-1.10*	Yes	Yes	Yes
	(0.57)	(0.09)	(0.58)			
MB ratio, Macro-state index	1.19**	-0.28**	-2.29***	Yes	Yes	Yes
	(0.47)	(0.12)	(0.52)			
Log(assets), Macro-state index	1.76*	-0.26***	-1.87**	Yes	Yes	Yes
	(0.90)	(0.07)	(0.70)			
Log(assets), Industry	0.82*	-0.21***	-1.25	Yes	Yes	Yes
	(0.44)	(0.07)	(0.77)			
Log(assets), MB ratio, Industry	1.59**	-0.19**	-0.88**	Yes	Yes	Yes
	(0.63)	(0.09)	(0.42)			
Macro-state index, MB ratio, Log(assets), Sales growth	1.11**	-0.22*	-1.33**	Yes	Yes	Yes
	(0.48)	(0.11)	(0.61)			
Macro-state index, MB ratio, Log(assets), Sales growth, Industry	3.68***	-0.14**	-1.43**	Yes	Yes	Yes
	(0.92)	(0.06)	(0.55)			

Table 2.9. Robustness Test Results

This table presents results from several robustness checks. In Panel A, we create random dates during 1997-2012, and we randomly assign them in each event of our sample creating the dummy variable $Impact_{(Random\ time)}$. Then, we estimate the model in equation (1), recording the coefficient, standard error and *p*-value of this variable. We repeat this procedure 1000 times, and in Panel A we report the average of these values, average standard errors in parentheses, and average *p*-values in square brackets. In Panel B, we repeat the same procedure, randomizing however the locations while keeping unchanged the real dates of the events. Since terrorist attacks and mass shootings are exogenous events that can happen in any habitable location in the U.S., we use the files of U.S. Census Bureau to find all the coordinates of habitable locations. Afterwards, we assign to each attack a random location (specified by the exact coordinates) and measure the distances between random attacks and firms. In the first column of Panel C, we drop observations with missing R&D values and re-estimate the R&D model in equation (1). The second column of Panel C shows again the R&D result from Table 2.3 (column 6). In Panel D, we examine the sensitivity of our findings to the 9/11 attacks by excluding them from the event sample. In all Panels, we include similar control variables, year-quarter fixed effects and firm fixed effects as in Table 2.3. Coefficients and standard errors are multiplied by 100 and standard errors are clustered at the local level. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

Panel A: Randomize Time

	Cash Holdings$_{t+1}$	R&D Expenditure$_{t+1}$	Long-term Leverage$_{t+1}$
Impact$_{(Random\ time)}$	-0.10	0.00	0.14
	(0.49)	(0.08)	(0.55)
	[0.37]	[0.43]	[0.42]
Control Variables	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes

Panel B: Randomize Location

	Cash Holdings$_{t+1}$	R&D Expenditure$_{t+1}$	Long-term Leverage$_{t+1}$
Impact$_{(Random\ location)}$	0.04	-0.02	0.32
	(0.69)	(0.13)	(1.10)
	[0.39]	[0.37]	[0.42]
Control Variables	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes

Table 2.9—*Continued*

Panel C: Alternative Specification for R&D Expenditure

	R&D Expenditure$_{t+1}$	
	[1]	[2]
Impact$_{s,t}$	-0.21**	-0.17***
	(0.10)	(0.06)
Control Variables	Yes	Yes
Firm F.E.	Yes	Yes
Time F.E.	Yes	Yes
N	80,300	143,311
Adjusted R^2	0.63	0.61

Panel D: Sensitivity to 9/11 Attacks

	Cash Holdings$_{t+1}$	R&D Expenditure$_{t+1}$	Long-term Leverage$_{t+1}$
Impact$_{s,t}$	1.68***	-0.09**	-1.16***
	(0.41)	(0.04)	(0.40)
Control Variables	Yes	Yes	Yes
Firm F.E.	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes
N	39,032	143,311	135,002
Adjusted R^2	0.73	0.61	0.59

Table 2.10. Estimating Using an Alternative Mass Shooting Sample

In this table, we increase the sample of mass shootings by including events with less human casualties, in comparison to those in the Washington Post list. We get data from the MSA Stanford database for the 1997-2012 period. In Panel A, we show the additional event sample. In Panel B, we re-estimate our baseline model when we consider mass shootings with different number of human casualties. We include similar control variables, year-quarter fixed effects and firm fixed effects as in Table 2.3. The coefficients and standard errors are multiplied by 100 and standard errors are clustered at the local level. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

Panel A: Description of MSA Sample

Event	Date	Location
Mass shootings with equal or more than 9 casualties		
All-Tech I.G. / Momentum Securities	29 Jul 1999	Atlanta, GA
Covina Shootings	24 Dec 2008	Covina, CA
Westroads Mall	05 Dec 2007	Omaha, NE
Hartford Beer Distributors	03 Aug 2010	Manchester, CT
Mass shootings with 8 casualties		
Wedgwood Baptist Church	15 Sep 1999	Fort Worth, TX
Living Church of God	12 Mar 2005	Brookfield, WI
Goleta Post Office	30 Jan 2006	Goleta, CA
Pinelake Health and Rehab	29 Mar 2009	Carthage, NC
Residences Shootings	07 Jul 2011	Grand Rapids, MI
Salon Meritage	12 Oct 2011	Seal Beach, CA
Mass shootings with 7 casualties		
Edgewater Technology	26 Dec 2000	Wakefield, MA
Windy City Core Supply Warehouse	27 Aug 2003	Chicago, IL
Party in Capitol Hill	25 Mar 2006	Seattle, WA
Atlantis Plastics	25 Jun 2008	Henderson, KY
Oikos University	02 Apr 2012	Oakland, CA
Accent Signage Systems	27 Sep 2012	Minneapolis, MN
Mass shootings with 6 casualties		
Navistar International	05 Feb 2001	Melrose Park, IL
West Nickel Mines Amish School	02 Oct 2006	Nickel Mines, PA
Trolley Square	12 Feb 2007	Salt Lake City, UT
Carnation Shootings	24 Dec 2007	Carnation, WA
Kirkwood City Hall	07 Feb 2008	Kirkwood, MO
Northern Illinois University	14 Feb 2008	DeKalb, IL
Rivermark	29 Mar 2009	Santa Clara, CA
Tucson Shootings	08 Jan 2011	Tucson, AZ
Cafe Shootings	30 May 2012	Seattle, WA

Table 2.10—*Continued*

Panel B: Estimation with MSA Sample												
	Cash Holdings$_{t+1}$				R&D Expenditure$_{t+1}$				Long-term Leverage$_{t+1}$			
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Impact$_{\text{(Initial + MSA Sample >=9 Casualties}}$	1.15**				-0.15**				-0.77*			
	(0.54)				(0.06)				(0.41)			
Impact$_{\text{(Initial Sample + MSA >=8 Casualties)}}$		0.98*				-0.11				-0.68*		
		(0.55)				(0.07)				(0.37)		
Impact$_{\text{(Initial Sample + MSA >=7 Casualties)}}$			0.26				-0.10*				-0.44	
			(0.64)				(0.06)				(0.40)	
Impact$_{\text{(Initial Sample + MSA >=6 Casualties)}}$				0.03				-0.07				-0.21
				(0.48)				(0.05)				(0.46)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

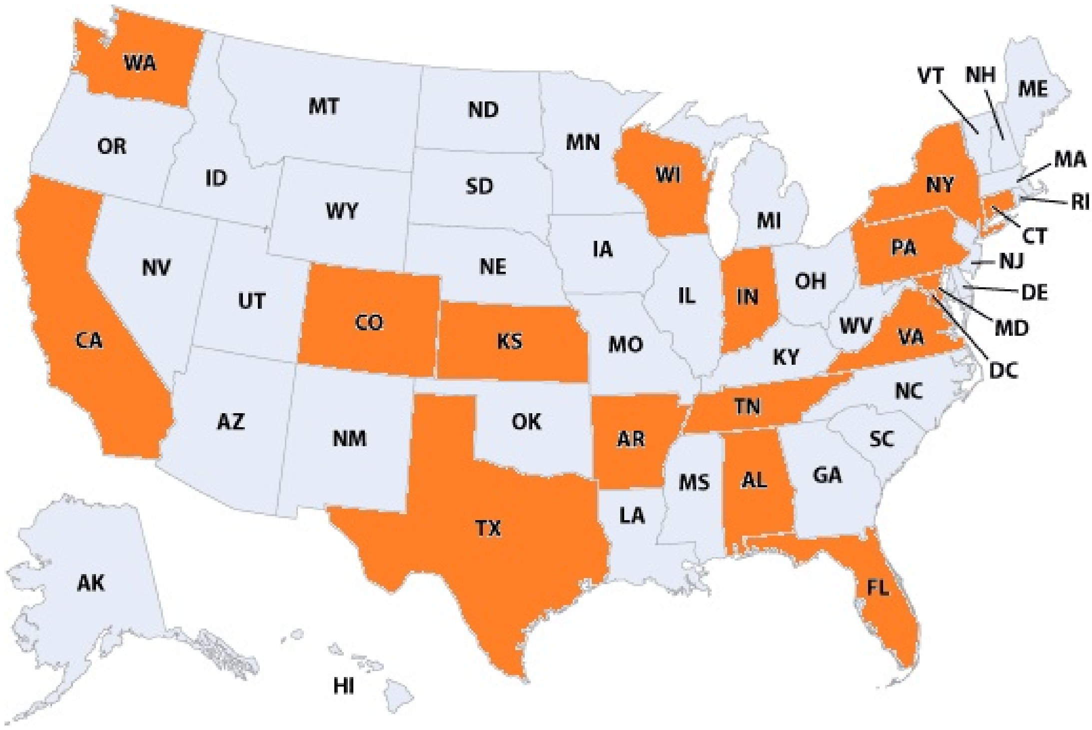
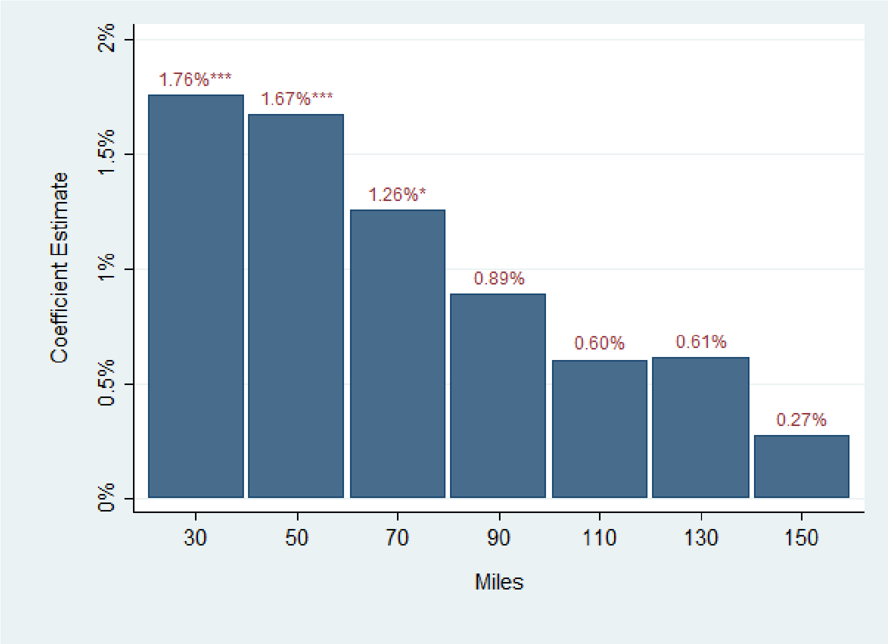
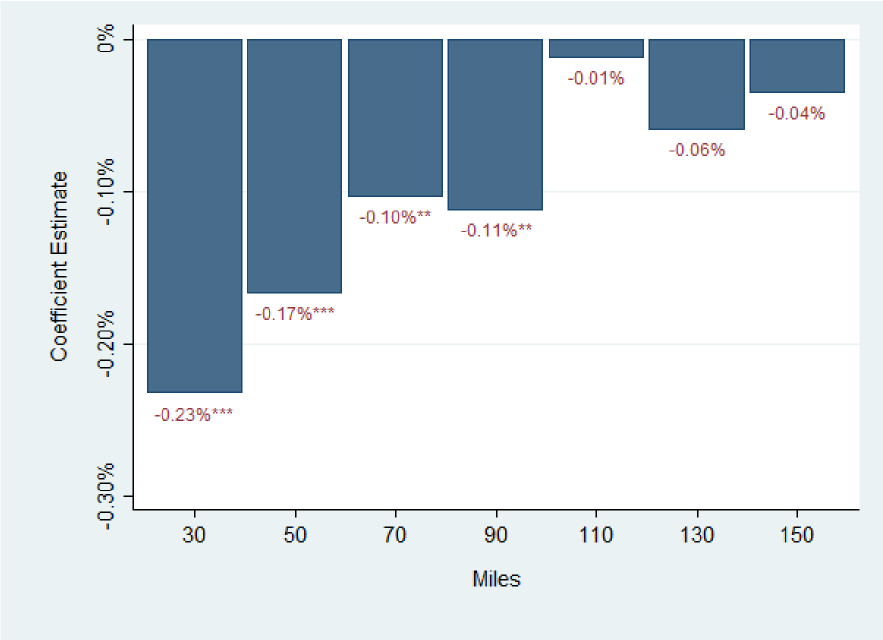


Figure 2.1. Terrorist Events and Locations. This figure shows the states where the terrorist attacks and mass shootings took place.

Panel A: Distance and Change in Cash Holdings



Panel B: Distance and Change in R&D Expenditure



Panel C: Distance and Change in Long-term Leverage

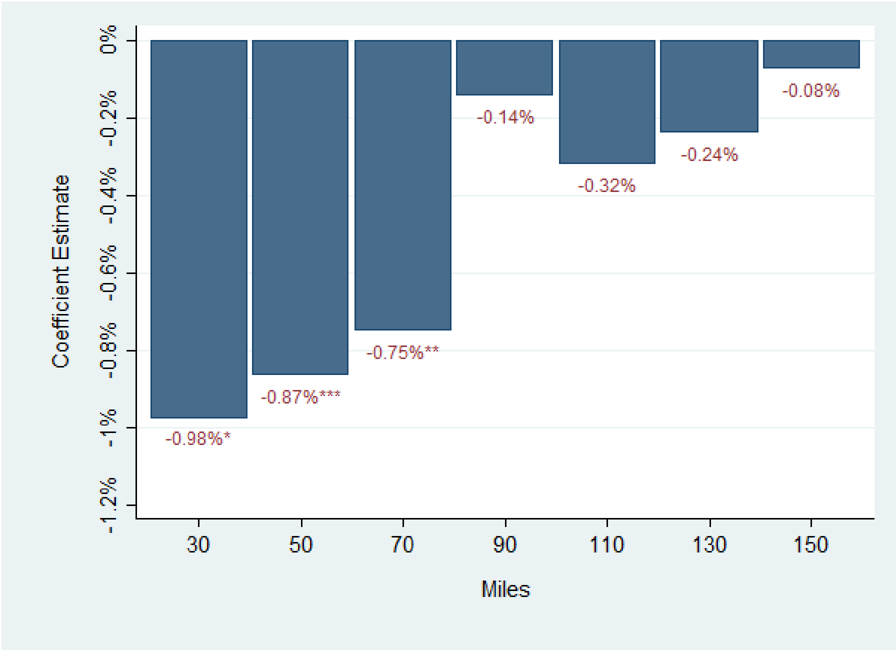


Figure 2.2. Terrorist Events and Geographical Proximity. This figure shows how the distance between the location of the attacks and the headquarters of the firms affects the change in corporate policies. In our baseline model, we define as local firms those with headquarters within 50 miles from the area of the attacks. In this figure, we follow the regression specification from Table 2.3 and define as local firms those with headquarters closer than 30, 50, 70, 90, 110, 130 and 150 miles from the attacks, respectively. We include all control variables, year-quarter fixed effects and firm fixed effects, as in Table 2.3. Panel A shows the coefficient estimates for the Cash Holdings$_{t+1}$. Panel B shows the coefficient estimates for the R&D Expenditure$_{t+1}$. Panel C shows the coefficient estimates for the Long-term Leverage$_{t+1}$. All regression coefficients are multiplied by 100. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

Chapter 3

Terrorist Events, Sentiment, and Institutional Investors

3.1 Introduction

A number of studies from psychology document that extreme violent events which occur at random times and locations such as terrorist attacks and mass shootings (henceforth, "terrorist events"), tend to adversely influence the *sentiment*[1] of local community.[2] Consequently, such intense sentiment shifts can affect the decisions of individuals and influence their risk assessment (Lerner and Keltner, 2001; Lerner, Gonzalez, Small and Fischhoff, 2003). These findings can be strongly associated with economic and market outcomes if market participants are also prone to such behavior.[3]

[1] We use the term "sentiment" throughout the paper to refer to feelings, mood and emotional states.

[2] For example, Vlahov, Galea, Resnick, Ahern, Boscarino, Bucuvalas, Gold and Kilpatrick (2002) find a 17.5% increase in alcohol use in the aftermath of 9/11 terrorist attacks. Further, Galea, Ahern, Resnick, Kilpatrick, Bucuvalas, Gold and Vlahov (2002) show that 9.7% of people who lived in Manhattan suffered from severe depression. Additionally, Hughes, Brymer, Chiu, Fairbank, Jones, Pynoos, Rothwell, Steinberg and Kessler (2011) show that 15.4% of students and faculty involved in Virginia Tech shootings in 2007 experienced high levels of posttraumatic stress disorder.

[3] Previous studies suggest that market participants are susceptible to several biases which affect their trading decisions, and in turn affect asset prices and market outcomes (Odean, 1998; Coval and Shumway, 2005; Bailey, Kumar and Ng, 2011; Goetzmann, Kim, Kumar and Wang, 2015).

In this paper, we investigate whether extreme negative sentiment induced from terrorist attacks and mass shootings, would affect the trading decisions of institutional investors. We focus on institutional investors as they are sophisticated financial agents who play a significant role in the price formation process and liquidity of the market. Despite their sophistication, we expect that institutional investors would still be affected, since even sophisticated investors are susceptible to cognitive biases (Coval and Shumway, 2005; Frazzini, 2006).

Motivated by this evidence, we conjecture that institutional investors who are *affected* by terrorist events would become more pessimistic, and in turn increase their selling propensity during the following days of the events. To measure the selling propensity of institutional investors, we match disaggregated daily trading data with hand-collected data about the location of each investor, which allows us to capture any direct effects on the trading activity of the affected investors after the dates of the attacks. Following previous studies in psychology which show that terrorist attacks and mass shootings have a significant impact on the sentiment of local population (Vlahov et al., 2002; Galea et al., 2002; Hughes et al., 2011), we identify as affected investors those who are geographical proximate to the location of the attacks around that time period. Specifically, institutional investors who are located closer to these attacks are more likely to be exposed to these events, hear about them, or interact with people who were involved. As a result, we expect these investors to experience stronger negative sentiment and in turn increase their selling propensity.

In our empirical analysis, we obtain institutional daily trading data from ANcerno Ltd. for the 1999-2010 period. The main advantage of this dataset is that we are able to observe each trade direction (i.e., buy/sell), and consequently, construct a measure to capture the selling propensity of institutional investors. To obtain information about the dates and locations of terrorist attacks and mass shootings, we use the Global Terrorism Database (GTD) and the Stanford Mass Shootings in America database (MSA), respectively, which gather systematic data about these events. We apply several filters in our event sample in order to reduce the noise and keep only the most salient events.[4] To identify local investors, we match the trading dataset to hand-collected data about the distance between the locations of investors and the locations of the attacks. Initially, we define as local investors those who are located in a radius of 100 miles from the attacks, which we allow to vary in later sections of the analysis.

To test our conjecture, first we use a specification which allows comparing the trading activity of local and non-local investors, while controlling for several time-varying factors that could be associated with the selling propensity of investors. Additionally, we estimate a fixed effects model to limit the variation to within stock-date pairs. This specification allows us to capture any unobservable factors for each particular stock at each point in time, and reduce potential bias in our estimates due to regression misspecification. The main advantage of this framework is that both groups of investors are exposed to the same public information about the stocks at each date and only differ in their exposure to the attacks.

Our findings are consistent with our conjecture and can be described as follows. First, we find that local investors increase their selling propensity by 8% in comparison to non-local investors the following 7 days after the events. This result holds even after we control for several stock characteristics, seasonal effects and local macroeconomic conditions. Similarly, our results remain robust when we control for the well-known home-bias proposed by Coval and Moskowitz (1999). We observe a similar effect when we estimate a fixed effects model with stock-date pairs as an additional covariate. The magnitude of this effect increases when investors are located closer to these attacks, while this effect diminishes when investors are located farther away. Further, we find that the attacks are perceived as more salient when they occur closer in time, since investors tend to increase even more their selling propensity. These results are in accordance with Antoniou, Kumar and Maligkris (2016a) who show that geographical and temporal proximity to these attacks is strongly associated with the magnitude of the effect. Additionally, we observe that the selling propensity of local investors is less pronounced among types of firms

[4] We describe these filters in detail in Section 3.2.

which entail higher transaction costs such as smaller sized firms, illiquid firms and firms with more volatile and skewed stock returns.

Second, we examine whether investors who are exposed to these events tend to substitute the sold stocks with other stocks in order to rebalance their portfolio holdings. Our results reject this hypothesis since we do not observe any statistically significant fluctuations in their buying propensity around the period of the attacks.

Third, we assess how the increase in the selling propensity of the affected investors relates to their performance. Following Puckett and Yan (2011), we construct an interim trading skill measure which captures the abnormal performance for all stocks that investors trade from the execution date until the end of the quarter. The main advantage of this performance measure is that it is not affected from potential disposition effects or rebalancing requirements. Using this measure, we find that investors underperform the market return by 2%. These findings are robust and remain highly significant when we use alternative specifications.

Fourth, we assess how the selling propensity of the affected investors relates to individual stock returns during the days of the attacks. Our results imply that the trading behavior of institutional investors negatively impacts stock returns during these days. Additionally, we find evidence that this effect is more pronounced among stocks with smaller market capitalization.

We perform several robustness checks to ensure the validity of our findings. First, we examine whether the events of our sample are able to draw the attention of local individuals since only those events would be likely to affect their sentiment. Following Da, Engelberg, and Gao (2011), we use the Search Volume Index (SVI) obtained from Google Trends, to capture the attention of local individuals. Consistent with our main conjecture, we find that during the first days following the attacks, there is a statistically significant increase in the SVI for words associated with the attacks. In further robustness checks, we examine whether the increased selling propensity is the result of a liquidity fluctuation, however we do not find any supportive evidence. In addition, we find that both terrorist attacks and mass shootings have similar effect on the trading activity of local investors. However, when we include less salient mass shootings in our event sample, there is not a statistically significant impact on the selling propensity of local investors. Finally, we do not find any evidence that would suggest the existence of pre-existing effects that could drive our findings or that this effect is driven only from the 9/11 attacks or from investors who are located in the state of New York.

These empirical findings contribute to the growing literature that associates sentiment with investors' financial decisions and market outcomes. Specifically, several studies show that fluctuations in stock returns are associated with local weather (Saunders, 1993; Hirshleifer and Shumway, 2003; Goetzmann et al. 2015). Kamstra, Kramer, and Levi (2003) and Kamstra, Kramer, Levi, and Wermers (2016) provide additional evidence and find that stock returns and flows of capital into equity funds are affected by changes in risk attitude due to seasonal affective disorder. In related research, Kaplanski and Levy (2010) show that stock prices fall after aviation disasters, and that these losses are fully recouped within two weeks. Further, Edmans, García, and Norli (2007) show that the stock markets of countries which experience a loss in international soccer tournaments experience a decline. In comparison to previous studies, we provide a new proxy to capture strong adversely shock in the sentiment of investors. Due to the nature of terrorist attacks and mass shootings, these events can provide an unexpected negative shock to the sentiment of institutional investors since they occur at random location and time. As a consequence, this proxy allows us to minimize any potential self-selection location bias that might be associated with investors' trading activity.

Further, we contribute to the literature by showing that not only retail investors but sophisticated investors are also susceptible to cognitive biases.[5] Coval and Shumway (2005) show that the traders of Chicago Board of Trade tend to exhibit highly loss aversion which causes fluctuations in the afternoon stock prices. Accordingly, Frazzini (2006) finds that mutual fund managers are susceptible to the disposition effect which can cause an adversely effect on stock returns. Additionally,

[5] Several studies demonstrate that individual investors are susceptible to cognitive biases that affect their trading decisions (Barber and Odean, 2000; Benartzi and Thaler, 2001; Agnew, 2006; Kumar, 2009).

Goetzmann et al. (2015) show that institutional investors' trading activity is associated with the local weather, while it can also have a negative impact on returns of stocks with higher arbitrage costs. Our study contributes to this literature by showing that negative exogenous events such as terrorist attacks and mass shootings tend to affect the sentiment of institutional investors, and in turn their selling propensity and performance.

This paper also contributes to the broader literature that associates terrorist attacks and mass shootings with the general economic activity. Specifically, several studies show that terrorist attacks can affect political views and election outcomes (Gould and Klor, 2010; Montalvo, 2011). Also, these events can alter the priority of governmental policies (Di Tella and Schargrodsky, 2004; Gould and Stecklov, 2009), and firm policies that are applied (Antoniou et al., 2016a). Motivated by these studies, we examine potential fluctuations of investors' trading activity following these events.

Taken as a whole, our findings provide strong evidence on how unexpected negative shocks in sentiment influence the trading decisions and performance of institutional investors. To our knowledge, our study is the first to directly test the effect of terrorist attacks and mass shootings on the trading activity of market participants and identify the channel through which these attacks are associated with financial and market outcomes.

This paper is organized as follows: In Section 3.2, we describe our data sources, the event sample and the trading data we use in this paper. In Section 3.3, we present the methodology and the variable construction included in our analysis. In Section 3.4, we present our main empirical results. In Section 3.5, we examine alternative explanations for our findings and we present results from various robustness checks. Section 3.6 concludes.

3.2 Data

3.2.1 Terrorist Attacks and Mass Shootings Data

We obtain data on terrorist attacks and mass shootings for the 1999-2010 period from the Global Terrorism Database (GTD)[6] and the Stanford Mass Shootings in America database (MSA),[7] respectively. GTD is an open-source database that includes systematic data on terrorist events around the world (START, 2013),[8] while MSA is a data project[9] that contains information about mass shootings in America, collected via online media sources. To reduce noise in our event sample, we consider only terrorist attacks that have caused human casualties and were covered in newspaper articles.[10] Following the findings of Antoniou et al. (2016a), we consider only mass shootings which were displayed in newspapers and have caused at least 8 human casualties, since these events are more likely to influence sentiment. Further, we eliminate events that occurred outside the U.S. We obtain data about the date and the location of each of the remaining events.

Table 3.1 shows the final event sample which includes 20 events for the 1999-2010 period. Figure 3.1 illustrates the geographical dispersion of these events and shows that these attacks do not exhibit any obvious regional clustering.

[6] The data are available at http://www.start.umd.edu/gtd/.
[7] The data are available at https://library.stanford.edu/projects/mass-shootings-america.
[8] To consider an event as terrorist attack, we apply the following filters as they appear in GTD: First, “The act must be aimed at attaining a political, economic, religious, or social goal”; Second, “There must be evidence of an intention to coerce, intimidate, or convey some other message to a larger audience (or audiences) than the immediate victims”; And third, “The action must be outside the context of legitimate warfare activities, i.e. the act must be outside the parameters permitted by international humanitarian law (particularly the admonition against deliberately targeting civilians or non-combatants)”.
[9] “Stanford Mass Shootings in America, courtesy of the Stanford Geospatial Center and Stanford Libraries”.
[10] We consider all events covered in at least one major U.S. outlet (*The Los Angeles Daily News, The NY Daily News, The NY Post, The NY Times, The Wall Street Journal-US edition, The Washington Post and USA Today*) during the next 7 days after the event.

3.2.2 Institutional Trading Data

We obtain the institutional daily trading from ANcerno Ltd. for the 1999-2010 sample period. ANcerno is a widely recognized firm that works with institutional investor trade data and monitors their equity trading costs. ANcerno's clients include pension plan sponsors as well as money managers.[11] ANcerno's dataset shows the transaction history for institutions in the sample and includes among others the stock historical CUSIP number, the quantity and execution price of shares traded, the trade date and its trade direction (i.e., buy/sell).

The main advantage of this dataset is that observing the trade direction for all executed trades allows us to construct measures that capture the selling and buying propensity of institutional investors. Further, since ANcerno provides information about the identities of institutional investors, we are able to find the ZIP codes of their headquarters using the Nelson's Directory of Investment Managers, and calculate their distance from the terrorist attacks and mass shootings of our sample.

Figure 3.2 depicts the geographical distribution of institutional investors across the United States. Consistent with the findings in Coval and Moskowitz (1999), while there is a fair degree of dispersion of investors across country, most of them are located in states with greater population and a large concentration firms.

3.2.3 Other Data Sources

Other datasets that we use in the analysis are from the Center for Research in Security Prices (CRSP), CRSP/Compustat Merged Database, U.S. Census Bureau, Bureau of Economic Analysis (BEA), Bureau of Labor Statistics (BLS) and Google Trends. Specifically, we collect stock data from CRSP to calculate the returns, the market capitalization and the inverse of price measure for each stock. To calculate the distance between the investors and the traded firms, we obtain firms' ZIP codes from CRSP/Compustat Merged Database, and we use the Gazetteer Files from the U.S.

[11] For example, some of ANcerno's clients are California Public Employees' Retirement System (CalPERS), Commonwealth of Virginia, Putman Investments, and Lazard Asset Management.

Census Bureau to match the ZIP codes with their coordinates. To capture local macroeconomic conditions, we obtain the quarterly state-level labor income from BEA and the unemployment data from the BLS. Finally, we use Google Trends[12] to collect the Search Volume Index (SVI) for terms that are associated with terrorist attacks and mass shootings.

3.3 Methodology

3.3.1 Investor Trade Measures

To examine the selling and buying propensity of institutional investors, we distinguish all trades into sells and buys, and compare them separately. Specifically, we construct two measures based on institutional investors' trade data. First, we construct a measure of selling propensity, *Investor sell ratio*, that aggregates all daily sell trades in each ZIP code for each stock and is equal to the daily total dollar sell volume for a particular stock within the same ZIP code divided by the daily total dollar sell volume for the same stock of investors located across all ZIP codes. This measure allows us to compare the differences in the selling propensity among institutional investors located in different ZIP codes, who are exposed to the same public information and trade the same stocks. Second, we construct an analogous measure using buy trades, *Investor buy ratio*, to examine the buying propensity of institutional investors.

Since, idiosyncratic trading behavior may increase noise in the measures of *Investor sell (buy) ratio*, we restrict our sample to ZIP codes with at least two investors at each point in time. To minimize potential measurement error in the database and mitigate the influence of outliers, we aggregate daily trades at the stock level and we delete the top and bottom 1% of the total dollar sell (buy) volume.

[12] The data are available at https://www.google.com/trends/.

Because we cannot observe daily institutional holdings in the ANcerno trading data, we only use dates where investors trade in the stock.

3.3.2 Variable Construction and Regression Specification

To examine whether terrorist attacks influence the selling (buying) propensity of institutional investors, we aggregate investor trades to the ZIP-stock-date code level. Using this three-level panel allows us to capture differences between the trading activity of local and non-local investors to terrorist events. These two groups are exposed to the same public information about stocks but only differ in their exposure to the terrorist events. Also, this setup allows us to account for stock level factors that may be systematically correlated with a subsample of institutional investors trading behavior. To account for potential dependencies in investor trading activity that is related to their locations, we cluster the error term at the ZIP code level in our specification. The model described above is specified as follows:

$$\text{Investor sell (buy) ratio}_{z,j,t} = c + \beta \times \text{Impact}_{z,t} + \Phi \times X_{z,j,t} + \varepsilon_{z,j,t} \qquad (1)$$

where *z* indexes ZIP code, *j* indexes stock, and *t* indexes time (daily). To capture the effect of terrorist events, we construct the variable $Impact_{z,t}$ which is a dummy variable that equals one if the distance between the ZIP code of institutional investors and the location of the attack is less than 100 miles, and the trade took place during the following 7 days after the terrorist attack. Narrowing down the number of days allows us to capture any short-term effects that may vanish in subsequent days as investors rebalance their portfolios. Therefore, this variable allows us to capture short-term effects when they are still present. We collect the coordinates of terrorist events using their address and the service called “GPS Geoplaner”.[13] To obtain the coordinates of investors, we match their ZIP codes with the Gazetteer Files from the U.S. Census Bureau. We calculate the distance between the investor and the locations of the attacks, following the procedure in Vincenty (1975).

[13] “GPS Geoplaner” is available at http://www.geoplaner.com/.

$X_{z,j,t}$ represents a set of other explanatory variables that may be associated with investors' selling (buying) propensity. We proxy for stock related risks that could affect the trading activity of investors by controlling for individual stock characteristics such as market capitalization, inverse of price and lag value of stock return (Brennan and Subrahmanyam, 1996; Goetzmann et al. 2015). *Market capitalization* is equal to the natural logarithm of the stock's market capitalization, *Inverse of price* is measured as the inverse of stock's daily price, and *Stock return*$_{t-1}$ is the daily return of firm's share at the end of the previous day. Further, we control for well-known seasonal effects such as the Monday and January effects. Specifically, we include in the model the dummy variable *Monday* which is equal to one if the investor traded on Monday, and zero otherwise. Accordingly, *January* is a dummy equal to one if the investor traded on January, and zero otherwise. Coval and Moskowitz (1999) document a strong preference in the investment choices of institutional investors for locally headquartered firms. To capture such effects, we include *Ln(Distance* $_{Investor\text{-}Stock}$*)* in the model, measured as the natural logarithm of the distance between the ZIP code of the institutional investor and the ZIP code of the traded firm.[14] Finally, local economic conditions may affect investors' current investment opportunities and future prospects, which in turn can influence their selling (buying) propensity for stocks. Therefore, we proxy for local economic conditions following Korniotis and Kumar (2013), and include in the model explanatory variables such as *Housing collateral*, measured as the log ratio of state-level housing equity divided by state's labor income; *Relative unemployment* which is equal to the fraction of the current rate to the moving 16 quarter-average of past rates; and *Income growth* rate measured as the state-level changes in labor income.[15]

To minimize any potential misspecification of our model, we also examine an additional specification in which we include fixed effects of stock-date pairs. This specification can capture any unobservable factors for each stock at each particular

[14] We drop from our sample firms with missing ZIP codes. To calculate the distance between the investors and the firms, we follow the same procedure as described before.
[15] See Korniotis and Kumar (2013) for further details on the construction of these variables.

date, and thus minimize the estimation bias related to the regression misspecification. This fixed effect model is defined as follows:

$$\text{Investor sell (buy) ratio}_{z,j,t} = c + \beta \times \text{Impact}_{z,t} + \gamma \times X_{z,j,t} + \delta_{j,t} + \varepsilon_{z,j,t} \qquad (2)$$

where $\delta_{j,t}$ represents the fixed effects of stock-date pairs. In this model, all the stock characteristics and time variables are subsumed by the fixed effects term.

3.3.3 Descriptive Statistics

Table 3.2 presents the summary statistics for our sample. Panel A contains information about the main variables of our regression specification when we include only sells in our sample. Specifically, we observe that the average market capitalization of our firm sample is 17.60$billions, while the average value of the inverse of price ratio is equal to 0.05. Also, we find that the average distance between the investors and the stock which they trade is 1215 miles, while the standard deviation is 995 miles. These statistics imply that investors tend to trade firms located near and farther away. During our sample period, we also observe that investors' headquarters are concentrated in states with an increased relative unemployment rate and income growth rate.

Panel B presents the statistics when we consider only buys. Even though, the average market capitalization of firms that investors buy is still quite high (i.e. 16.60$billions), it is reduced in comparison to the market capitalization of firms that are sold from investors. We observe similar statistics as in Panel A for the rest of the control variables.

3.4 Main Empirical Results

In this section, we present our main empirical results. Our key findings suggest that institutional investors who are local to terrorist events increase their selling propensity the days following the events. This effect is stronger among investors located even closer to the events, and investors who trade near the date of the attacks. However, the selling propensity of local investors is less pronounced for smaller sized firms, illiquid firms and for firms with more volatile and skewed stock returns. Finally, we demonstrate that the trading behavior of local investors is negatively associated with their performance, and it has an adversely impact on stock returns.

3.4.1 Terrorist Events and Institutional Investor Trading

3.4.1.1 Investors' Selling Propensity and Terrorist Events

We begin by examining whether terrorist events increase the selling propensity of local institutional investors during the days following the events. We present the results in columns (1) to (4) in Table 3.3.

Consistent with our hypothesis, we find that local investors tend to increase their sells in comparison to non-local investors during the following 7 days after the events. Specifically, as shown in column (2), investors increase approximately 8% their sells in comparison to non-local investors around the date of the events. This result is statistically significant at 1% level. Further, this finding is robust when we control for a systematic variation across stocks in column (3).

We also examine another specification in which we limit the variation of *Impact* to within stock-date pairs by including stock-date fixed effects. This specification allows us to compare the selling propensity of investors for the same stock at a particular date. Also, this specification can capture any unobservable factors associated with a particular stock at each point in time. Column (4) of Table 3.3 presents the results which show that local investors increase their sells by 9% in comparison to non-local investors who sell the same stock during the same day. Since we limit the variation within stock-date pairs, stock characteristics and time variables are subsumed by the fixed effects term.

3.4.1.2 Investors' buying propensity and terrorist events

To examine whether investors substitute the stocks they sold with other stocks in order to rebalance their portfolio holdings, we focus on their buying propensity around the date of the attacks. Columns (5) to (8) of Table 3.3 show the results.

In these columns, we show that investors do not change their buying propensity the days following the attacks. These results are highly insignificant and robust across different specifications. Also, in column (8) we observe similar results when we compare local and non-local investors who buy the same stock the days following the event. These findings suggest that investors who are located within 100 miles radius from terrorist events tend to sell stocks from their daily holdings which are not replaced in their portfolio from other stocks in the short-term.

Examining the estimates of the control variables, we find that the market capitalization and the inverse of price ratio of the stock are negatively correlated with the selling propensity of investors, while the stock returns are positively correlated. On the contrary, we find a weak association between stock returns and the buying propensity of investors. Interestingly, we find that institutional investors show an increase in their selling and buying propensity on Monday, while on January they tend to trade fewer stocks. Because this dataset does not include the investor's daily holdings, all the estimates are conditional on whether the investor trades a stock for a particular date. Therefore, we interpret the estimates of these variables as a fluctuation in the trading activity. Similarly, we find some mild evidence which show a negative correlation between the trading activity and the state's macroeconomic environment related to the relative unemployment rate and the income growth rate. On the contrary, we do not find any strong relation between the investor-stock distance and the selling or buying propensity of investors.

3.4.2 Geographical and Temporal Proximity to Terrorist Events

According to our main hypothesis, investors who are located closer to terrorist events and trade during the following days should perceive these events as more salient. In this section, we examine further this conjecture by allowing the geographical and temporal proximity to the attacks to vary. Specifically, we expect to find a strong association between the changes in the selling propensity of investors and the geographical and temporal proximity to the events, since as the distance and time between the investors and the attacks decrease, investors are more likely to suffer a negative shock to their sentiment (e.g., see Galea et al., 2002).

First, we test whether investors who are located farther away from these events change their selling propensity around the date of the events. Specifically, we construct the dummy variables $Impact_{(0\ to\ 50\ miles)}$, $Impact_{(0\ to\ 150\ miles)}$ and $Impact_{(0\ to\ 250\ miles)}$, which are dummy variables that are equal to one if an investor sold a stock within the following 7 days after an attack and is located in less than 50 miles, 150 miles, and 250 miles, respectively.

Panel A in Table 3.4 shows the results. In line with our hypothesis, we find an 11% increase in the selling propensity of investors who are located in a radius of 50 miles from the location of a terrorist attack. This coefficient estimate is higher than the estimate of column (4) in Table 3.3 (i.e. 9%) in which we define as local investors those who are located in a 100 miles radius from an attack. We also observe that when we increase the distance between the location of the institutional investors and the location of the attacks to 150 miles, the magnitude of the effect declines to 7% while the coefficient estimate is statistically significant only to 10% level. Finally, when we increase the distance further to 250 miles, the coefficient estimate becomes statistically insignificant.

Second, we examine whether local investors who trade during the days following the events, increase more their selling propensity in comparison to those investors who trade several days after the events. Specifically, we construct the dummy variables $Impact_{(0\ to\ 5\ days)}$, $Impact_{(0\ to\ 10\ days)}$, and $Impact_{(0\ to\ 15\ days)}$ which are dummy variables equal to one if an investor is located in a radius of 100 miles from the attack and traded within the following 5 days, 10 days, and 15 days, respectively.

We present the results in Panel B of Table 3.4. Our findings show that investors who trade during the next 15 days after the events do not illustrate any statistically significant changes in their selling propensity. On the contrary, investors sell a bigger fraction of stocks during the following 5 and 10 days after the events. These results are in accordance with our hypothesis that recent events are likely to be perceived as more salient.

In summary, consistent with our predictions, we find that the geographical and temporal proximity to terrorist events are strongly associated with the selling propensity of institutional investors, since only investors located near these events and investors who trade the following days after the events exhibit an increase in their selling propensity.

3.4.3 Firm Characteristics, Selling Propensity, and Terrorist Events

Next, we examine whether the increase in the selling propensity of institutional investors vary among stocks with different characteristics. Specifically, we examine whether the selling propensity of affected investors is less pronounced among smaller firms, illiquid stocks, volatile stock returns and skewed stock returns, since any trading activity on these types of stocks would entail higher transaction costs (Amihud, 2002; Chordia, Sarkar, and Subrahmanyam, 2005; Marshall, Nguyen, Visaltanachoti, 2012). To examine this conjecture, we estimate the following augmented models where we include an interaction of *Impact* with each of these firm characteristics:

$$\text{Sell ratio}_{\mathrm{z,j,t}} = \mathrm{c} + \beta \times \text{Impact}_{z,\mathrm{t}} \times FC_{\mathrm{z,j,t}} + \gamma \times \text{Impact}_{z,\mathrm{t}} + \delta \times FC_{\mathrm{z,j,t}} + \Phi \times \mathrm{X}_{\mathrm{z,j,t}} + \varepsilon_{\mathrm{z,j,t}} \quad (3)$$

where *z* indexes ZIP code, *j* indexes stock, and *t* indexes time, while *FC* represents the firm characteristic considered in each regression specification.

Table 3.5 reports the regression estimates. Consistent with our hypothesis, our results show that even though affected institutional investors increase their selling propensity for all types of firms, the magnitude of this increase is reduced for smaller firms, illiquid firms, and for firms with volatile and skewed stock returns.

Specifically, column (1) shows that local investors decrease their selling propensity for smaller firms by 7% in comparison to larger firms, during the following 7 days after the events. Accordingly, the results in columns (4), (7) and (10) imply that local investors decrease their propensity to sell illiquid stocks by 4%, volatile stocks by 6% and stocks with skewed returns by 6%.

To examine the robustness of our findings, we additionally include several fixed effects in our specifications. Specifically, to minimize the possibility that our results are driven from economic shocks at particular dates which could affect the stock characteristics we consider, we include date fixed effects in columns (2), (5), (8) and (11). Our results remain consistent with our baseline estimates. Further, we include stock fixed effects in columns (3), (6), (9) and (12) to capture time-invariant stock characteristics that could be associated with the selling propensity of investors. Once again, our results remain robust.

3.4.4 Terrorist Events and Interim Trading Skills

In this section, we investigate whether the increased propensity of local investors to sell stocks around terrorist events can affect their performance. An increase in their selling propensity may have either a positive or negative impact on performance. Specifically, selling more stocks during these dates may offset other types of biases such as overconfidence, and thus have a positive effect on investors' performance. On the other hand, if investors increase disproportionally their selling propensity, they will have a negative effect on their performance since they may sell stocks which are going to have a higher future value.

To examine whether such trading behavior is positively or negatively associated with their performance, we follow Puckett and Yan (2011) and create an interim trading skill measure. This measure allows us to examine the within-quarter performance of local investors' trading activity around the dates of the events. The main advantage of this measure is that it measures the abnormal performance for all stocks that investors trade from the execution date until the end of the quarter.

Therefore, this measure minimizes any biases that might exist in the short-term due to potential presence of a disposition effect in the trading activity of investors or any rebalancing requirements for investors' portfolios.

To construct this measure and examine whether the increased selling propensity is associated with changes in the interim performance of the affected investors, first we calculate the raw cumulative stock return from the current price until the end of the quarter. Then, we subtract the equally-weighted market index return over the same holding period to compute the *Equal-weighted market adjusted return* measure.

Table 3.6 shows our findings. Panel A reports the results when investors sell stocks and shows that affected investors have a negative performance when they sell stocks around the dates of the events. Specifically, we find that these investors underperform the equal-weighted market return by approximately 2%. These results are highly significant at the 1% level. Further, they remain robust when we control for stock, date and ZIP code fixed effects to capture any systematic differences across these dimensions.

In contrast to the findings of Antoniou, Kumar, and Maligkris (2016b) who show that such extreme negative events can have a positive impact on the performance of particular financial agents such as sell-side analysts, our findings suggest that institutional investors are negatively affected from such events.

3.4.5 Stock Returns, Market Capitalization and Terrorist Events

In previous sections, we demonstrate that extreme negative events such as terrorist attacks and mass shootings can affect the beliefs of institutional investors, and in turn their trading decisions. In this section, we examine whether this trading behavior is associated with stock market outcomes. Following Hirshleifer and Shumway (2003) and Goetzmann et al. (2015), we posit that any fluctuations in the sentiment of investors are more likely to impact the sign rather than the magnitude of stock returns. As a consequence, we conjecture that the increase in the selling propensity of local institutional investors can negatively affect the sign of stock prices for the traded firms. Further, we conjecture that this effect should be more pronounced among stocks with smaller market capitalization, since even if local institutional investors sell a low absolute volume of a small-cap stock, of a low trading volume can have a large effect on stock prices.

To examine this conjecture, we use as regressand the dummy variable *NegRet* which is equal to one if the daily return of the stock is negative, and zero otherwise. To reduce potential noise in our estimates due to rebalancing requirements, spillover effects and stock-specific economic shocks that may be present during that period, we examine the effect of investors' trading activity during the actual day of the attacks.

Panel A of Table 3.7 presents the results. Consistent with our hypothesis, column (1) shows that it is 18% more likely to obtain negative returns, when local investors sell stocks with small capitalization during the days of the events. Columns (3) and (5) show that this effect decreases as the market capitalization increases and becomes insignificant for firms with larger market capitalization. To capture any date-specific economic shocks that could have influenced our results, we additionally estimate fixed effect models in which we include time fixed effects. We present these results in columns (2), (4), (6) and (8), which are consistent with our baseline findings. Further, the results are statistically significant when we consider the whole sample. Overall, these results suggest that when institutional investors located near terrorist attacks trade at the day of the event, there is a higher probability that the traded stocks will have a negative return.

To ensure that any fluctuations in stock prices are due to the selling propensity of local institutional investors rather than a random effect which occurred across stocks during these days, we estimate similar tests using as dependent variables the market-adjusted stock returns. Specifically, we construct the variable *NegAdjRet*, which is equal to one if the market-adjusted stock return is negative, and zero otherwise. According to our conjecture, we expect to find a statistically significant association between the market-adjusted stock returns and the trading activity of local institutional investors.

We present our results in Panel B of Table 3.7. The coefficient estimates in column (1) suggest that it is 13% more likely to obtain negative market-adjusted returns, when local investors sell stocks with small market capitalization during the days of the attacks. Similar to Panel A, the coefficient estimates decrease in magnitude, and finally become insignificant when we consider stocks with larger market capitalization. Once again, these results are robust when we estimate fixed effect models to capture any date-specific economic shocks.

Overall, these findings suggest that institutional investors located near terrorist attacks increase their selling propensity around that time period and as a result the probability of obtaining a negative stock return is increased.

3.5 Robustness Checks

3.5.1 Salience, Attention and Terrorist Events

3.5.1.1 Terrorist Events and National Search Volume Index

In this paper, we conjecture that exogenous negative events such as terrorist attacks and mass shootings are salient enough to draw the attention of local investors and influence their sentiment. Following Da et al. (2011), we use as a measure of attention the Search Volume Index (SVI)[16] to examine whether these events can capture the attention of individuals, and similarly induce an effect on financial agents such as institutional investors.[17]

[16] Google Trends adjusts search data and scale them to range between 0 and 100 to make comparisons easier. Specifically, according to Google “each data point is divided by the total searches of the geography and time range it represents, to compare relative popularity”. As a result, an increase in the SVI of a search term indicates that people in a specific location and time period, searched this term more than they normally do. The data are available at https://www.google.com/trends/.

[17] Several papers have used proxies to capture investors’ attention such as news (Yuan, 2015), firm advertising expense (Lou, 2014), trading volume (Gervais, Kaniel, and Mingelgrin, 2001), company's Wikipedia page views (Focke, Ruenzi, and Ungeheuer, 2016), and extreme returns (Barber and Odean, 2008).

We obtain SVI data for the following search terms: “shooting”, “shootings”, “mass shooting”, “terrorism” and “terrorist attack”. To confirm that these search terms are related to these events, we associate our event sample with the weekly changes of their SVI in the United States over the sample period of January 2004 to December 2010.[18] We restrict the SVI measure to the United States, since our event sample includes only events from this region.

Figure 3.3 shows that there is a positive weekly change in the SVI around the period of the majority of the events in our sample when we use these search terms. Specifically, we find that when we use as search terms words that are related to the mass shootings such as “shooting”, “shooting” and “mass shooting”, there is a positive weekly change in the SVI around the time periods of all the mass shooting events obtained from the MSA database. Further, we find that the Virginia Tech shootings draw the most attention in comparison to the other mass shooting events. When we focus on search terms related to terrorist events such as “terrorism” and “terrorist attack”, we find that there is a mild weekly change in the national SVI around the majority of terrorist events obtained from the GTD database. Overall, these results suggest that there is a national awareness for these events around their time period.

3.5.1.2 Terrorist Events and State Search Volume Index

According to our hypothesis, terrorist events will draw more the attention of local individuals instead of those individuals who are located farther away. Further, we expect that more attention will be paid to these events during the first days following the events.

To examine these conjectures, we download the daily SVI for each state for the same list of search terms as before. Because SVI is not available at a ZIP level, and the data are sparse on a regional level, we obtain data on a state-level and examine the state-level awareness of these events. Further, since SVI does not represent the

[18] Since SVI is available only from 2004, we restrict our sample period to start from this year.

absolute search volume, we scale each series by dividing it with its daily national SVI. Following Da, Engelberg, and Gao (2015), we create the variable *ΔASVI* which is a winsorized, deseasonalized, and standardized measure for each of the search terms. Specifically, first we construct the daily ΔSVI measured as the daily difference of the natural logarithm plus one for each of the scaled series.[19] Second, we winsorize each of the series at the 1% level to mitigate potential outliers. Third, we regress the daily ΔSVI on weekdays and month dummies and keep the residuals to eliminate seasonality. Fourth, aiming to address heteroscedasticity, we standardize each of the time series by dividing each by its standard deviation.

Next, we estimate the following model for each of the adjusted search series.

$$\Delta\mathrm{ASVI}_{\mathrm{s,t}} = \mathrm{c} + \beta \times \mathrm{Impact}_{s,\mathrm{t}} + \alpha_{\mathrm{s}} + \delta_{\mathrm{t}} + \varepsilon_{\mathrm{s,t}} \qquad (4)$$

where *s* indexes the state and *t* indexes the date. To capture the effect of terrorist events on the state's adjusted daily change in search volume at the date of the attacks, we construct the variable $Impact_{s,t}$. $Impact_{s,t}$ is a dummy variable equal to one for the state of each attack around the time period when the attack occurred. Further, we include state and time fixed effects, represented as α_s and δ_t, respectively, to capture any systematic variation across states and time. We run this regression specification for each of the search terms.

Figure 3.4 shows the results. In the first bar of each graph, $Impact_{s,t}$ is a dummy equal to one for each state where the attack occurred at the day of the attack (t_0). In the rest of the bars, $Impact_{s,t}$ is equal to one if an attack occurred at the same state and took place during the next one day, two, three, four or five days after the attack, respectively. The results show that there is a steep and statistically significant increase in the search volume of the terms "shooting", "shootings" and "mass shooting" when a terrorist event takes place in the same state during the day of the event. These events continue to draw the attention of local population also during the next few days, however this effect decreases until it becomes insignificant. When we

[19] We add one to each of the scaled series to be able to take the natural logarithm of these numbers in case they are equal to zero.

use as search terms the words “terrorism” and “terrorist attack”, we observe much smaller effects on their search volume in comparison to the words associated with mass shootings. However, we still find that the effect of the terrorist events continues to decrease as the days following the events increase.

Overall, these findings show that the events in our sample tend to be highly associated with the state’s Google search volume during the days following the events. Similar to Antoniou et al. (2016a), these results confirm that except of media attention, terrorist events can also capture the attention of local individuals, and therefore they are also likely to draw the attention of local financial agents such as institutional investors.

3.5.2 Liquidity Shocks and Pension Plan Sponsors

Until now, we interpret the changes in the selling propensity of the affected investors as the consequence of terrorist events. However, any change in the trading activity of institutional investors may also be associated with the trading activity of other market participants such as retail investors. For example, retail investors may actually trade systematically differently around the dates of terrorist events, which as a result can affect the liquidity of the stock market and institutions’ fund flows (e.g., see Frazzini and Lamont, 2008). As a response, institutional investors may have adjusted their trading strategy around these dates due to these conditions and not due to any changes in their sentiment.

To examine this conjecture, we use additional information regarding the type of the institutional investors that made each trade. Our sample includes the trades from both pension plan sponsors and money manager. We distinguish these trades and keep only the trades from pension plan sponsors, since they are more likely to experience fewer short-term changes in the inflows and outflows of their capital (Puckett and Yan, 2008).

Table 3.8 presents the results. Consistent with our main conjecture, we find that affected pension plan sponsors tend to increase their selling propensity around the dates of the events. The magnitudes of the coefficient estimates are approximately similar to those in Table 3.3 and range between 4% and 9%. Further, these findings are highly statistically significant and remain robust when we include stock, date and ZIP code fixed effects in our specification.[20]

3.5.3 Distinct Effects of Terrorist Attacks and Mass Shootings

In this study, our event sample includes both terrorist attacks and mass shootings which we treat as exogenous events that can influence the sentiment of local institutional investors. To examine whether our findings are driven only from one event type, we distinguish our event sample to terrorist attacks and mass shootings and re-estimate our baseline specification as in Table 3.3.

Specifically, to capture the distinct effect of terrorist attacks and mass shootings, we create the variables $Impact_{(Terrorist\ attacks)}$ and $Impact_{(Mass\ shootings)}$, respectively. $Impact_{(Terrorist\ attacks)}$ is a dummy variable that equals one if an investor is local to a terrorist attack and sells stocks during the following 7 days after the events. Accordingly, $Impact_{(Mass\ shootings)}$ is equal to one when an investor is local to a mass shooting and sells stocks during the following 7 days after the events.

We present our findings in Panel A of Table 3.9. In columns (1) and (2), the results show that there are still statistically significant effects when we consider only terrorist attacks and mass shootings, respectively. In column (3), we include both $Impact_{(Terrorist\ attacks)}$ and $Impact_{(Mass\ shootings)}$ variables to capture the distinct effects of both event types. Our results show that the coefficient estimate for each variable remains similar to the estimate presented in columns (1) and (2), respectively. Further, we find that both event types produce a similar effect since their coefficient estimates are statistically similar.[21]

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[20] Table 3.7 does not report results after including stock-date fixed effects, since it is infeasible to estimate such a specification due to the large decrease of the sample size and the large number of these fixed effect factors.

[21] We perform a test between the two coefficients to examine whether their difference is statistically significant. The results show that their difference is statistically insignificant since we obtain an *F*-statistic equal to 0.04.

3.5.4 Mass Shootings and Alternative Event Sample

Our initial event sample includes mass shootings which contain at least 8 human casualties since these events are more likely to have a stronger impact on the sentiment of investors. In this section, we include mass shootings with a smaller number of human casualties in our event sample and examine whether such events can also have a similar effect on the selling propensity of the affected investors.

Panel B of Table 3.9 lists the 18 additional events that we include in our event sample. To examine whether such events can similarly affect the trading activity of institutional investors, we construct the variables $Impact_{(6\text{-}7\ casualties)}$ and $Impact_{(3\text{-}7\ casualties)}$. $Impact_{(6\text{-}7\ casualties)}$ is a dummy variable equal to one when an investor is local to a mass shooting which involved 6 to 7 human casualties. Similarly, $Impact_{(3\text{-}7\ casualties)}$ equals one when the mass shooting involved 3 to 7 human casualties.

Panel C of Table 3.9 presents the results. Our findings show that $Impact_{(Mass\ shootings)}$ remains highly statistically significant and similar in coefficient magnitude as in Panel A. When we include the variables $Impact_{(6\text{-}7\ casualties)}$ and $Impact_{(3\text{-}7\ casualties)}$, their coefficient estimates are highly statistically insignificant. Also, their coefficient estimates are lower than the estimate of $Impact_{(Mass\ shootings)}$. Consistent with our conjecture, these results suggest that only salient events can have a significant impact on the sentiment of the affected investors, which in turn can influence their trading activity.

3.5.5 Pre-Existing Effects

One concern with the estimates of Table 3.3 is that they may be driven from pre-existing effects. In this case, our estimates would capture the spillover effects of some pre-existing events that are not related to terrorist events. To rule out this possibility, we examine whether there are any significant effects for the local investors a few days before the events take place. Particularly, we create the variables $Impact_{(-2\ to\ -1}$

$_{days)}$, *Impact*$_{(-5\ to\ -1\ days)}$ and *Impact*$_{(-7\ to\ -1\ days)}$ to capture potential pre-existing shocks that took place 2, 5 or 7 days before the actual dates of the events.

Panel A of Table 3.10 presents our findings. These results show that all the lag variables of *Impact* are highly statistically insignificant. Also, they suggest that institutional investors change their selling propensity only as a result of the terrorist events and not due to potential pre-existing shocks.

3.5.6 Sensitivity to 9/11 Attacks

Since 9/11 attacks are the most important events in our sample and could potentially drive our main results, we re-estimate our main specification after excluding these events from our sample. We present our findings in column (1) of Panel B in Table 3.10. Even though, we exclude the 9/11 attacks from our event sample, we still obtain statistically significant results. Further, the coefficient estimate is close to those displayed in Table 3.3.

3.5.7 Excluding New York Investors

Investors located in the state of New York provide the majority of trades in our sample (i.e. 28.59%). Following Coval and Moskowitz (1999), we exclude such investors from our sample and we re-estimate our main specification to ensure that our results are not driven only by the trading activity of these investors. Column (2) of Panel B in Table 3.10 shows our findings which remain robust to the exclusion of New York institutional investors.

3.6 Conclusion

Previous studies demonstrate that investors are susceptible to biases associated with their mood and sentiment, while other studies show that institutional investors are not immune to cognitive biases. This paper contributed to this literature by using terrorist attacks and mass shootings as a new proxy to capture strong adversely shocks in the sentiment of institutional investors. These events occur at random location and time, and as a result they tend to minimize any potential self-selection bias associated with the location preference of investors. To our knowledge, this study is the first to identify the channel through which these attacks are associated with financial and market outcomes.

In our analysis, we conjecture that institutional investors who are geographical proximate to the attacks and trade the following days should receive a strong negative shock in their sentiment, which in turn will affect their trading activity, and increase their selling propensity. We test this hypothesis by comparing the selling propensity of local and non-local investors around the period of the attacks, since both groups should be exposed to the same public information. Our results show that local institutional investors tend to increase their selling propensity during the following days after the attacks. The magnitude of this effect increases when investors are located closer to these attacks, and when the time proximity of the trades and the attacks increases. However, this effect is less pronounced when affected investors trade smaller firms, illiquid stocks, and stocks with volatile and skewed returns. Additionally, we find evidence which suggests that the trading activity of local investors affects their quarterly performance, since local investors tend to underperform the market return. The trading behavior of local institutional investors is also associated with stock market outcomes since the increase in their selling propensity has a negative impact on stock returns during the day of the attacks.

Collectively, these findings highlight the impact of terrorist attacks and mass shootings on financial trading activity and stock market outcomes. Future research could examine whether less sophisticated market participants are also prone to extreme shifts in their sentiment which can affect their trading decisions.

Table 3.1. Sample of Terrorist Events

This table shows our event sample during the period 1999-2010. All the events took place in the U.S. and were local to institutional investors. Also, they resulted to at least one human casualty and were displayed in major news outlets.

N	Events	Date	Location	Database
1	Columbine High School	20 Apr 1999	Littleton, CO	MSA
2	Korean Methodist Church	04 Jul 1999	Bloomington, IN	GTD
3	All-Tech Group/Momentum Securities	29 Jul 1999	Atlanta, GA	MSA
4	Wedgwood Baptist Church	15 Sep 1999	Fort Worth, TX	MSA
5	9/11 Attacks: World Trade Center	11 Sep 2001	New York City, NY	GTD
6	9/11 Attacks: Hijacked Plane Crashed	11 Sep 2001	Alexandria, VA	GTD
7	9/11 Attacks: Hijacked Plane Crashed	11 Sep 2001	Somerset County, PA	GTD
8	Bank of America	05 Jan 2002	Tampa, FL	GTD
9	LA International Airport	04 Jul 2002	Los Angeles, CA	GTD
10	Living Church of God	12 Mar 2005	Brookfield, WI	MSA
11	Goleta Post Office	30 Jan 2006	Goleta, CA	MSA
12	Seattle Jewish Federation	28 Jul 2006	Seattle, WA	GTD
13	Virginia Tech	16 Apr 2007	Blacksburg, VA	MSA
14	Covina Shooting	24 Dec 2008	Covina, CA	MSA
15	Pinelake Health and Rehab	29 Mar 2009	Carthage, NC	MSA
16	Immigration Centre	03 Apr 2009	Binghamton, NY	MSA
17	Holocaust Museum	10 Jun 2009	Washington, DC	GTD
18	Pentagon	04 Mar 2010	Arlington, VA	GTD
19	Hartford Beer Distributors	03 Aug 2010	Manchester, CT	MSA
20	Discovery Communications	01 Sep 2010	Silver Springs, MD	GTD

Table 3.2. Descriptive Statistics

This table presents the descriptive statistics for the ANcerno trade data and the variables of our analysis. The sample includes daily trades of U.S. institutional investors on common U.S. stocks (share code of 10 or 11) from January 1999 to December 2010. Panel A shows the descriptive statistics for the variables when we restrict our sample to sell trades. Panel B presents the descriptive statistics when we include in our sample only buy trades. *Investor sell (buy) ratio* is a variable equal to the daily total dollar sell (buy) volume for a particular stock within the same ZIP code divided by the daily total dollar sell (buy) volume across all ZIP codes for the same stock. *Impact* is a dummy variable equal to one if the investor who traded in the stock market is inside a 100 miles radius from an attack, and the trade took place during the next 7 days after the attack. We also obtain data from CRSP to control for stock characteristics. Particularly, *Market cap.* is equal to the natural logarithm of the stock's market capitalization, where market capitalization is the number of shares outstanding multiplied with the price. *Inverse of price* is measured as the inverse of stock's share price. *Stock return$_{t-1}$* is the lag value of the daily stock return. *Monday* is a dummy variable equal to one if the investor traded on Monday and zero otherwise. *January* is a dummy equal to one if the investor traded on January and zero otherwise. *Ln(Distance $_{Investor\text{-}Stock}$)* is equal to the natural logarithm of the distance, measured in miles, between the ZIP code of the institutional investor and the ZIP code of the traded firm. *Housing collateral* is the state-level housing collateral ratio which is equal to the log ratio of state-level housing equity divided by state's labor income. *Relative unemployment* depicts the fraction of the current rate to the moving 16 quarter-average of past rates. *Income growth rate* captures the state-level changes in labor income.

Panel A: Descriptive Statistics – Sell

Variable	Obs.	Mean	Std. Dev.	25th Pctl.	Median	75th Pctl.
Investor sell ratio	5,810,704	0.49	0.43	0.03	0.41	1.00
Impact	5,810,704	0.00	0.03	0.00	0.00	0.00
Market cap. ($billions)	5,810,704	17.60	42.60	0.95	3.12	13.70
Inverse of price	5,810,704	0.05	0.09	0.02	0.03	0.05
Stock return$_{t-1}$	5,810,704	-0.00	0.07	-0.01	-0.00	0.01
Monday	5,810,704	0.18	0.39	0.00	0.00	0.00
January	5,810,704	0.09	0.28	0.00	0.00	0.00
Distance $_{Investor\text{-}Stock\ (miles)}$	5,810,704	1215.19	995.25	403.54	944.05	1962.13
Housing collateral	5,810,704	-0.07	0.56	-0.12	0.06	0.17
Relative unemployment	5,810,704	1.14	0.33	0.88	1.02	1.38
Income growth rate	5,810,704	0.03	0.22	-0.01	0.00	0.02

Panel B: Descriptive Statistics – Buy

Variable	Obs.	Mean	Std. Dev.	25th Pctl.	Median	75th Pctl.
Investor buy ratio	5,603,457	0.52	0.43	0.05	0.50	1.00
Impact	5,603,457	0.00	0.04	0.00	0.00	0.00
Market cap. ($billion)	5,603,457	16.60	41.30	0.91	2.87	12.40
Inverse of price	5,603,457	0.05	0.06	0.02	0.03	0.05
Stock return$_{t-1}$	5,603,457	-0.00	0.07	-0.01	0.00	0.01
Monday	5,603,457	0.18	0.38	0.00	0.00	0.00
January	5,603,457	0.09	0.28	0.00	0.00	0.00
Distance $_{Investor\text{-}Stock\ (miles)}$	5,603,457	1250.96	993.90	433.84	1016.02	2026.91
Housing collateral	5,603,457	-0.06	0.55	-0.12	0.07	0.17
Relative unemployment	5,603,457	1.13	0.33	0.87	1.01	1.36
Income growth rate	5,603,457	0.03	0.21	-0.01	0.00	0.02

Table 3.3. Terrorist Events and Institutional Investors: Baseline Estimation

This table presents the results from regressions examining the impact of attacks on institutional investors' sell and buy ratio. In columns (1) to (4), we use *Investor sell ratio* as dependent variable while in columns (5) to (8) we use as dependent variable the *Investor buy ratio*. In columns (3) and (7), we include stock fixed effects, and in columns (4) and (8) we include fixed effects based on date-stock pairs. Standard errors, shown in parentheses, are clustered at the level of investors' ZIP code. *, ** and *** measure significance at the 10%, 5%, and 1% levels, respectively.

	Investor sell ratio				Investor buy ratio			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Impact	0.07***	0.08***	0.08***	0.09***	0.02	0.02	0.03	0.00
	(0.03)	(0.02)	(0.02)	(0.04)	(0.03)	(0.03)	(0.03)	(0.06)
Ln(Market cap.)	-0.11***	-0.11***	-0.17***		-0.11***	-0.11***	-0.18***	
	(0.00)	(0.00)	(0.01)		(0.00)	(0.00)	(0.01)	
Inverse of price	-0.07***	-0.04*	-0.27***		0.03	0.06*	-0.32***	
	(0.02)	(0.02)	(0.04)		(0.03)	(0.03)	(0.07)	
Stock return$_{t-1}$	0.07***	0.07***	0.08***		0.01*	0.01	0.01	
	(0.03)	(0.03)	(0.03)		(0.01)	(0.01)	(0.01)	
Monday		0.01***	0.01***			0.01***	0.01***	
		(0.00)	(0.00)			(0.00)	(0.00)	
January		-0.03***	-0.03***			-0.03***	-0.03***	
		(0.01)	(0.01)			(0.01)	(0.01)	
Ln(Distance $_{\text{Investor-Stock}}$)		-0.00	-0.00	0.00		-0.00	-0.01	-0.01
		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)	(0.01)
Housing collateral		-0.00	0.00	0.01		-0.01	-0.00	0.00
		(0.02)	(0.01)	(0.02)		(0.01)	(0.01)	(0.02)
Relative unemployment		-0.05**	-0.04*	0.11		-0.04*	-0.02	0.12
		(0.02)	(0.02)	(0.20)		(0.02)	(0.02)	(0.23)
Income growth rate		-0.07***	-0.07***	0.00		-0.08***	-0.08***	-0.01
		(0.02)	(0.01)	(0.01)		(0.02)	(0.02)	(0.01)
Constant	2.17***	2.23***	3.10***	0.36	2.15***	2.22***	3.25***	0.43
	(0.05)	(0.05)	(0.19)	(0.23)	(0.04)	(0.05)	(0.18)	(0.26)
Stock F.E.			Yes				Yes	
Date and Stock F.E.				Yes				Yes
N	5,810,704	5,810,704	5,810,704	5,810,704	5,603,457	5,603,457	5,603,457	5,603,457
Adj-R^2	22.28%	22.64%	26.84%	23.98%	21.24%	21.56%	25.52%	18.94%

Table 3.4. Temporal and Geographical Proximity to Terrorist Events

This table examines how different temporal and geographical proximity to the terrorist attacks affects investors' sell ratio. In Panel A, we examine how the distance between the attacks and the investors affect *Investor sell ratio*. To examine the association of distance with the magnitude of the effect, we allow distance to vary. Specifically, we consider $Impact_{(0\ to\ 50\ miles)}$, $Impact_{(0\ to\ 150\ miles)}$ and $Impact_{(0\ to\ 250\ miles)}$, which are dummy variables equal to one if an investor traded within the following 7 days from an attack and is located in less than 50 miles, 150 miles, and 250 miles, respectively. In Panel B, we examine how the time after the attacks is associated with the *Investor sell ratio* of the affected investors. Particularly, we include in our models the variables $Impact_{(0\ to\ 5\ days)}$, $Impact_{(0\ to\ 10\ days)}$, and $Impact_{(0\ to\ 15\ days)}$ which are dummy variables equal to one if an investor is located in a radius of 100 miles from the attack and traded within the following 5 days, 10 days, and 15 days, respectively. All regressions include date-stock pair fixed effects, and similar control variables as in Table 3.3. Standard errors, shown in parentheses, are clustered at the level of investors' ZIP code. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

Panel A: Geographical Proximity

	(1)	(2)	(3)
$Impact_{(0\ to\ 50\ miles)}$	0.11**		
	(0.05)		
$Impact_{(0\ to\ 150\ miles)}$		0.07*	
		(0.04)	
$Impact_{(0\ to\ 250\ miles)}$			0.04
			(0.05)
Control Variables	Yes	Yes	Yes
Date and Stock F.E.	Yes	Yes	Yes
N	5,810,704	5,810,704	5,810,704
Adj-R^2	23.98%	23.99%	23.98%

Panel B: Temporal Proximity

	(1)	(2)	(3)
$Impact_{(0\ to\ 5\ days)}$	0.09**		
	(0.04)		
$Impact_{(0\ to\ 10\ days)}$		0.07*	
		(0.04)	
$Impact_{(0\ to\ 15\ days)}$			0.06
			(0.04)
Control Variables	Yes	Yes	Yes
Date and Stock F.E.	Yes	Yes	Yes
N	5,810,704	5,810,704	5,810,704
Adj-R^2	23.98%	23.98%	23.98%

Table 3.5. Terrorist Events, Institutional Investors, and Firm Characteristics

This table examines whether local institutional investors around the period of the attacks increase more or less their selling propensity for hard-to-value stocks. In all columns, we consider *Investor sell ratio* as dependent variable. In columns (1) to (3), we define *Firm char.* as an indicator variable equal to one for firms with market capitalization on the bottom quarter of our sample, and zero otherwise. In columns (4) to (6), we capture the illiquidity of firms using the Amihud (2002) illiquidity measure which is equal to the ratio of absolute return to the daily dollar trading volume, and we define *Firm char.* as an indicator variable equal to one for firms with an illiquidity measure which falls at the top quarter of the sample distribution, and zero otherwise. In columns (7) to (9), we define *Firm char.* as an indicator variable equal to one for firms with yearly volatility which falls at the top quarter of the sample distribution, and zero otherwise. Similarly, in columns (10) to (12) we define *Firm char.* as an indicator variable equal to one for firms with yearly skewness which falls at the top quarter of the sample distribution, and zero otherwise. In columns (2), (5), (8) and (11), we include date fixed effects and in columns (3), (6), (9) and (12) we include stock fixed effects in our specifications. Standard errors, shown in parentheses, are clustered at the level of investors' ZIP code. *, ** and *** measure significance at the 10%, 5%, and 1% levels, respectively.

	Small Firms			Illiquid Firms			Volatile Firms			Skewed Firms		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Impact×Firm char.	-0.07***	-0.05**	-0.07***	-0.04**	-0.02	-0.04**	-0.06**	-0.02	-0.08***	-0.06***	-0.05***	-0.06***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Impact	0.09***	0.05**	0.10***	0.09***	0.04*	0.10***	0.10***	0.04*	0.11***	0.09***	0.05**	0.10***
	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
Firm char.	0.05***	0.04***	0.01**	0.09***	0.06***	0.05***	0.05***	-0.01**	0.03***	0.02***	0.01***	0.01**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Date F.E.		Yes			Yes			Yes			Yes	
Stock F.E.			Yes			Yes			Yes			Yes
N	5,810,704	5,810,704	5,810,704	5,810,704	5,810,704	5,810,704	5,810,704	5,810,704	5,810,704	5,810,704	5,810,704	5,810,704
Adj-R^2	22.78%	31.26%	26.85%	23.10%	31.41%	26.94%	22.84%	31.18%	26.92%	22.67%	31.18%	26.85%

Table 3.6. Terrorist Events and Interim Trading Skill

This table examines the impact of attacks on the interim trading skill of institutional investors when they sell stocks. For each ZIP code, we calculate the raw cumulative stock return from the current price until the end of the quarter. Then, we subtract the equally-weighted market index return over the same holding period to compute the *Equal-weighted market adjusted return* measure. To construct the raw cumulative stock return, we subtract the price at the end of the quarter from the current price and divide by the current price. Columns (2), (3) and (4) include stock, date and ZIP code fixed effects, respectively. Standard errors, shown in parentheses, are clustered at the level of investor's ZIP code. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

	Equal-weighted market adjusted return			
	(1)	(2)	(3)	(4)
Impact	-0.02***	-0.02***	-0.01***	-0.02***
	(0.00)	(0.00)	(0.00)	(0.00)
Ln(Market cap.)	0.00	0.03***	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Inverse of price	-0.15***	-0.20***	-0.16***	-0.16***
	(0.02)	(0.02)	(0.02)	(0.02)
Stock return$_{t-1}$	-0.08***	-0.00	-0.10***	-0.08***
	(0.03)	(0.02)	(0.03)	(0.03)
Monday	-0.00	-0.00		0.00
	(0.00)	(0.00)		(0.00)
January	0.01***	0.01***		0.01***
	(0.00)	(0.00)		(0.00)
Ln(Distance $_{\text{Investor-Stock}}$)	0.00	-0.00**	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Housing collateral	-0.01***	-0.00***	-0.00	-0.01***
	(0.00)	(0.00)	(0.00)	(0.00)
Relative unemployment	0.02***	0.04***	0.01	0.02***
	(0.00)	(0.00)	(0.01)	(0.00)
Income growth rate	0.01***	0.02***	0.00	0.02***
	(0.00)	(0.00)	(0.00)	(0.00)
Constant	-0.00	-0.50***	0.01	-0.01
	(0.01)	(0.03)	(0.01)	(0.01)
Stock F.E.		Yes		
Date F.E.			Yes	
ZIP code F.E.				Yes
N	5,810,704	5,810,704	5,810,704	5,810,704
Adj-R^2	1.08%	7.34%	2.85%	1.19%

Table 3.7. Terrorist Events, Stock Capitalization and Stock Returns

This table examines whether the selling propensity of local institutional investors at the day of the attacks influence the stock returns. $\text{Impact}_{\text{Day0}}$ is a dummy variable equal to one if an investor is located in a 100 miles radius from an attack at the day of the event. The columns of this table present subsample estimates based on the *market capitalization* of stocks. Specifically, columns (1) and (2) present estimates when the *market capitalization* of the stock falls on the bottom quarter of the distribution. Accordingly, columns (3) and (4) present estimates the *market capitalization* of the stock is under the 50^{th} percentile of the distribution, columns (5) and (6) show estimates when the *market capitalization* of the stock is equal or above the 50^{th} percentile of the distribution, and columns (7) and (8) show estimates for the whole sample. Panel A examines whether at the day of the attacks it is more likely to obtain negative stock returns. In this panel, we use as regressand the indicator variable *NegRet* which is equal to one if the daily return of the stock is negative, and zero otherwise. In Panel B, we examine whether at the day of the attacks it is more likely to obtain negative market-adjusted stock returns. Therefore, we use as regressand the dummy variable *NegAdjRet* which is equal to one if the market-adjusted stock return is negative, and zero otherwise. In columns (1), (3), (5) and (7), we estimate logit regression models and their coefficient estimates illustrate marginal probabilities. In columns (2), (4), (6) and (8), we estimate fixed effect regression models to exploit the variation within each date. Standard errors, shown in parentheses, are clustered at the level of investors' ZIP code. *, ** and *** measure significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Stock Returns, Stock Capitalization and Terrorist Events

	[0-25%)		[0%-50%)		[50%-100%]		All	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{Impact}_{\text{Day0}}$	0.18***	0.07*	0.17***	0.06**	0.10	0.01	0.14**	0.04*
	(0.06)	(0.04)	(0.06)	(0.03)	(0.07)	(0.02)	(0.05)	(0.02)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Date F.E.		Yes		Yes		Yes		Yes
N	1,452,676	1,452,676	2,905,351	2,905,351	2,905,353	2,905,353	5,810,704	5,810,704

Panel B: Market-Adjusted Stock Returns, Stock Capitalization and Terrorist Events

	[0-25%)		[0%-50%)		[50%-100%]		All	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\text{Impact}_{\text{Day0}}$	0.13***	0.09**	0.10**	0.06*	0.01	0.03	0.06*	0.04
	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Date F.E.		Yes		Yes		Yes		Yes
N	1,452,676	1,452,676	2,905,351	2,905,351	2,905,353	2,905,353	5,810,704	5,810,704

Table 3.8. Robustness Tests: Pension Plan Sponsors and Terrorist Events

This table examines the impact of attacks on pension plan sponsors trading activity. Columns (3), (4) and (5) include stock, date and ZIP code fixed effects, respectively. Standard errors, shown in parentheses, are clustered at the level of investor's ZIP code. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

	Investor sell ratio				
	(1)	(2)	(3)	(4)	(5)
Impact	0.08***	0.09***	0.09***	0.04**	0.06***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
Ln(Market cap.)	-0.08***	-0.08***	-0.12***	-0.08***	-0.07***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Inverse of price	-0.10***	-0.07***	-0.16***	-0.05***	-0.07***
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
Stock return$_{t-1}$	0.16***	0.16***	0.16***	0.13***	0.16***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Monday		0.01***	0.01***		0.01***
		(0.00)	(0.00)		(0.00)
January		-0.00	-0.00		-0.00
		(0.01)	(0.01)		(0.01)
Ln(Distance $_{\text{Investor-Stock}}$)		-0.01***	-0.01***	-0.00***	-0.00
		(0.00)	(0.00)	(0.00)	(0.00)
Housing collateral		0.01	0.01	0.02	-0.00
		(0.01)	(0.01)	(0.01)	(0.00)
Relative unemployment		-0.10***	-0.10***	-0.24**	-0.07***
		(0.02)	(0.02)	(0.11)	(0.01)
Income growth rate		-0.04**	-0.04***	0.00	-0.04***
		(0.02)	(0.01)	(0.01)	(0.01)
Constant	1.91***	2.08***	2.66***	2.21***	1.89***
	(0.09)	(0.11)	(0.16)	(0.20)	(0.06)
Stock F.E.			Yes		
Date F.E.				Yes	
ZIP code F.E.					Yes
N	1,777,352	1,777,352	1,777,352	1,777,352	1,777,352
Adj-R^2	12.77%	14.04%	17.08%	20.67%	18.91%

Table 3.9. Robustness Tests: Terrorist Attacks and Mass Shootings

This table examines the distinct effects of terrorist attacks and mass shootings, and explores the effect of mass shooting with less human casualties. Panel A presents the results from regressions examining the distinct effects of terrorist attacks and mass shootings on *Investor sell ratio*. $Impact_{(Terrorist\ attacks)}$ is a dummy variable equal to one if an investor is local to a terrorist attack and sold stocks the following 7 days. Accordingly, $Impact_{(Mass\ shootings)}$ is a dummy variable equal to one if an investor is local to a mass shooting attack and sold stocks the following 7 days. In Panel B, we obtain additional data from MSA Stanford database for the 1999-2010 period and we increase the sample of mass shootings events. In this Panel we show information on the additional event sample. In Panel C, we examine the robustness of our results when we include mass shootings with less human casualties in comparison to our initial event sample. Particularly, $Impact_{(6\text{-}7\ casualties)}$ is a dummy equal to one when an investor is local to an attack which involved 6 or 7 human casualties and sold stocks the following 7 days. Accordingly, $Impact_{(3\text{-}7\ casualties)}$ is a dummy equal to one when an investor is local to an attack which involved between 3 and 7 human casualties and sold stocks the following 7 days. All regressions include date-stock pair fixed effects, and similar control variables as in Table 3.3. Standard errors, shown in parentheses, are clustered at the level of investor's ZIP code. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

Panel A: Distinct Effects of Terrorist Attacks and Mass Shootings

	(1)	(2)	(3)
$\text{Impact}_{(\text{Terrorist attacks})}$	0.09**		0.09**
	(0.04)		(0.04)
$\text{Impact}_{(\text{Mass shootings})}$		0.10**	0.10**
		(0.05)	(0.05)
Control Variables	Yes	Yes	Yes
Date and Stock F.E.	Yes	Yes	Yes
N	5,810,704	5,810,704	5,810,704
Adj-R^2	23.98%	23.98%	23.98%

Table 3.9—*Continued*

Panel B: Description of Additional MSA Sample

Events	Date	Location
Mass shootings with 7 casualties		
Edgewater Technology	26 Dec 2000	Wakefield, MA
Windy City Core Supply Warehouse	27 Aug 2003	Chicago, IL
Party in Capitol Hill	25 Mar 2006	Seattle, WA
Mass shootings with 6 casualties		
Navistar International	05 Feb 2001	Melrose Park, IL
Birchwood Shootings	21 Nov 2004	Birchwood, WI
West Nickel Mines Amish School	02 Oct 2006	Nickel Mines, PA
Trolley Square	12 Feb 2007	Salt Lake City, UT
Carnation Shootings	24 Dec 2007	Carnation, WA
Kirkwood City Hall	07 Feb 2008	Kirkwood, MO
Northern Illinois University	14 Feb 2008	DeKalb, IL
Rivermark Shootings	29 Mar 2009	Santa Clara, CA
Mass shootings with 5 casualties		
Radisson Bay Harbor Inn	30 Dec 1999	Tampa, FL
Alrosa Villa Nightclub	08 Dec 2004	Columbus, OH
Youth With A Mission / New Life Church	09 Dec 2007	Arvada, CO
Parkland Coffee Shop	29 Nov 2009	Lakewood, WA
Mass shootings with 4 casualties		
University of Arizona Shooting	28 Oct 2002	Tucson, AZ
Mass shootings with 3 casualties		
Appalachian School of Law	16 Jan 2002	Grundy, VA
Tyler Courthouse	24 Feb 2005	Tyler, TX

Panel C: Model Estimates with Additional Mass Shooting Sample

	(1)	(2)
$\text{Impact}_{(\text{Mass shootings})}$	0.10**	0.10**
	(0.05)	(0.05)
$\text{Impact}_{(\text{6-7 casualties})}$	0.05	
	(0.05)	
$\text{Impact}_{(\text{3-7 casualties})}$		0.05
		(0.04)
Control Variables	Yes	Yes
Date and Stock F.E.	Yes	Yes
N	5,810,704	5,810,704
Adj-R^2	23.98%	23.98%

Table 3.10. Robustness Tests: Pre-Existing Effects and Different Sample Specifications

In this table, we perform additional robustness tests. In Panel A, we include lag values of *Impact* to examine whether there are any potential pre-existing shocks that could affect our estimations. In Panel B, we examine whether our results are robust to the exclusion of 9/11 attacks and NY institutional investors. Particularly, in column (1) we exclude 9/11 attacks from our event sample and re-estimate our main model similar to Table 3.3. In column (2), we examine whether our estimations are robust to the exclusion of NY investors. All regressions include date-stock pair fixed effects, and similar control variables as in Table 3.3. Standard errors, shown in parentheses, are clustered at the level of investor's ZIP code. *, ** and *** measure significance at the 10%, 5%, and 1% level, respectively.

Panel A: Pre-Existing Effects

	(1)	(2)	(3)
$\text{Impact}_{(-2 \text{ to } -1 \text{ days})}$	0.02		
	(0.07)		
$\text{Impact}_{(-5 \text{ to } -1 \text{ days})}$		-0.01	
		(0.04)	
$\text{Impact}_{(-7 \text{ to } -1 \text{ days})}$			0.01
			(0.04)
Control Variables	Yes	Yes	Yes
Date and Stock F.E.	Yes	Yes	Yes
N	5,810,704	5,810,704	5,810,704
Adj-R^2	23.98%	23.98%	23.98%

Panel B: 9/11 Attacks and New York Investors

	Excluding 9/11 attacks	Excluding NY investors
	(1)	(2)
Impact	0.10**	0.10**
	(0.04)	(0.04)
Control Variables	Yes	Yes
Date and Stock F.E.	Yes	Yes
N	5,810,704	4,149,193
Adj-R^2	23.98%	29.36%

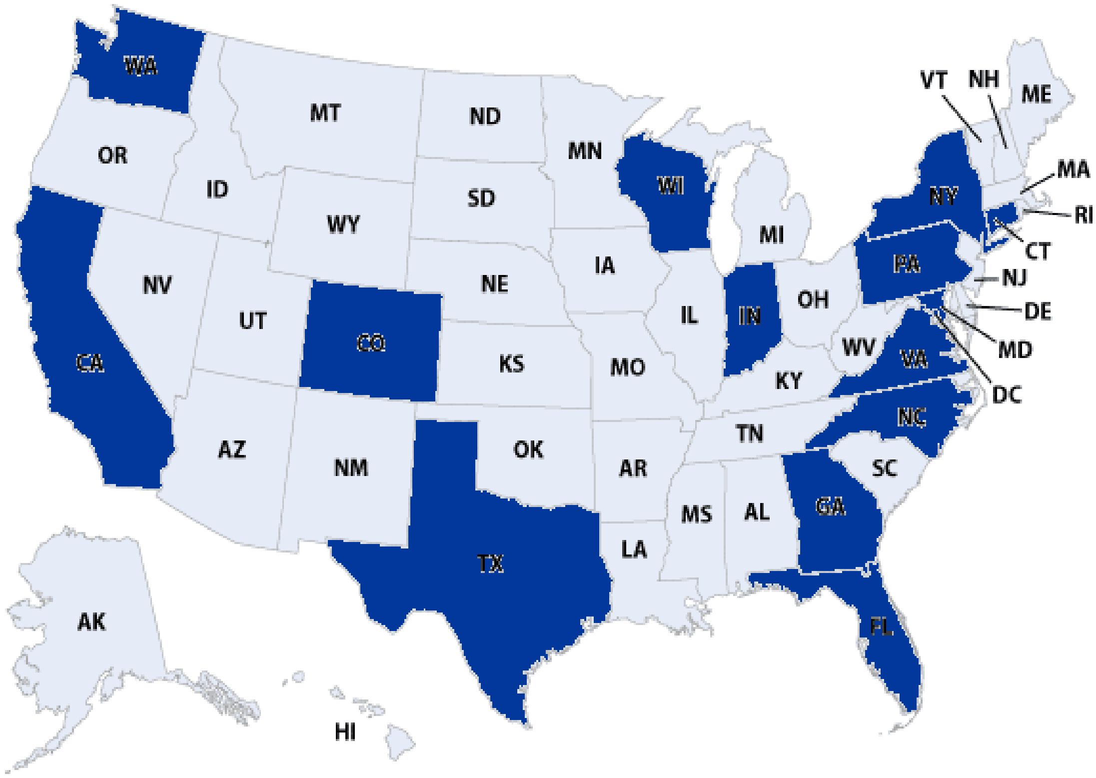

Figure 3.1. Geographical Distribution of Attacks. This figure shows the states where the terrorist attacks and mass shootings occurred.

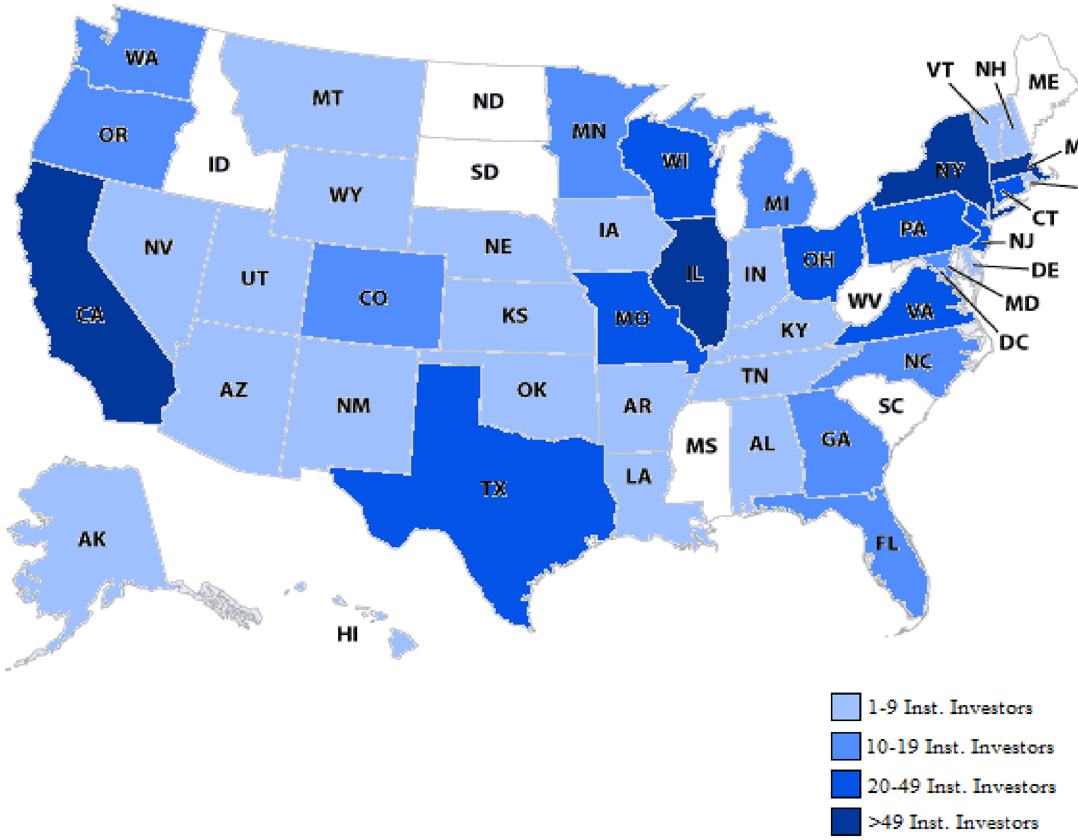

Figure 3.2. Geographical Distribution of Institutional Investors. This figure shows the distribution of institutional investors across U.S. states. States with no institutional investors located in are highlighted with white.

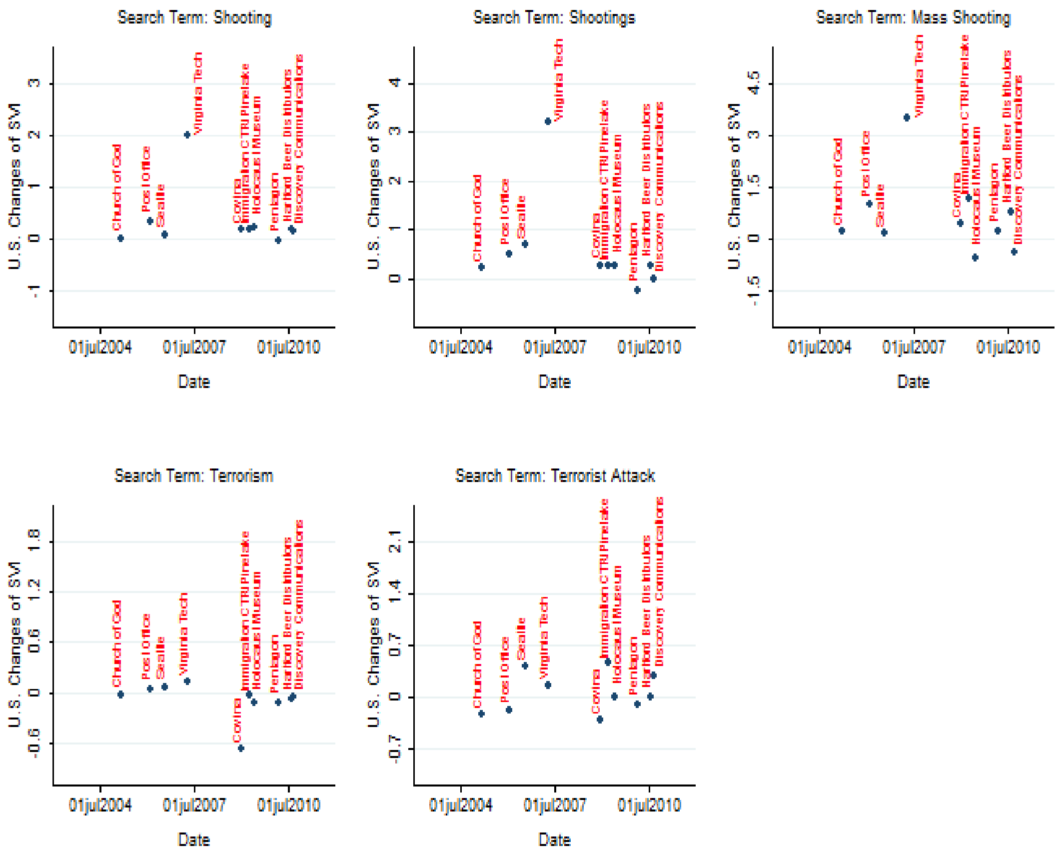

Figure 3.3. Weekly Changes of SVI in U.S. and Terrorist Events. In this figure, we associate our event sample with the weekly changes of the U.S. SVI for the following search terms: “shooting”, “shootings”, “mass shooting”, “terrorism” and “terrorist attack”. The changes of the U.S. SVI are measured as the weekly difference of the natural logarithm plus one for each of these search terms. Our SVI sample spans from January 2004 until December 2010.

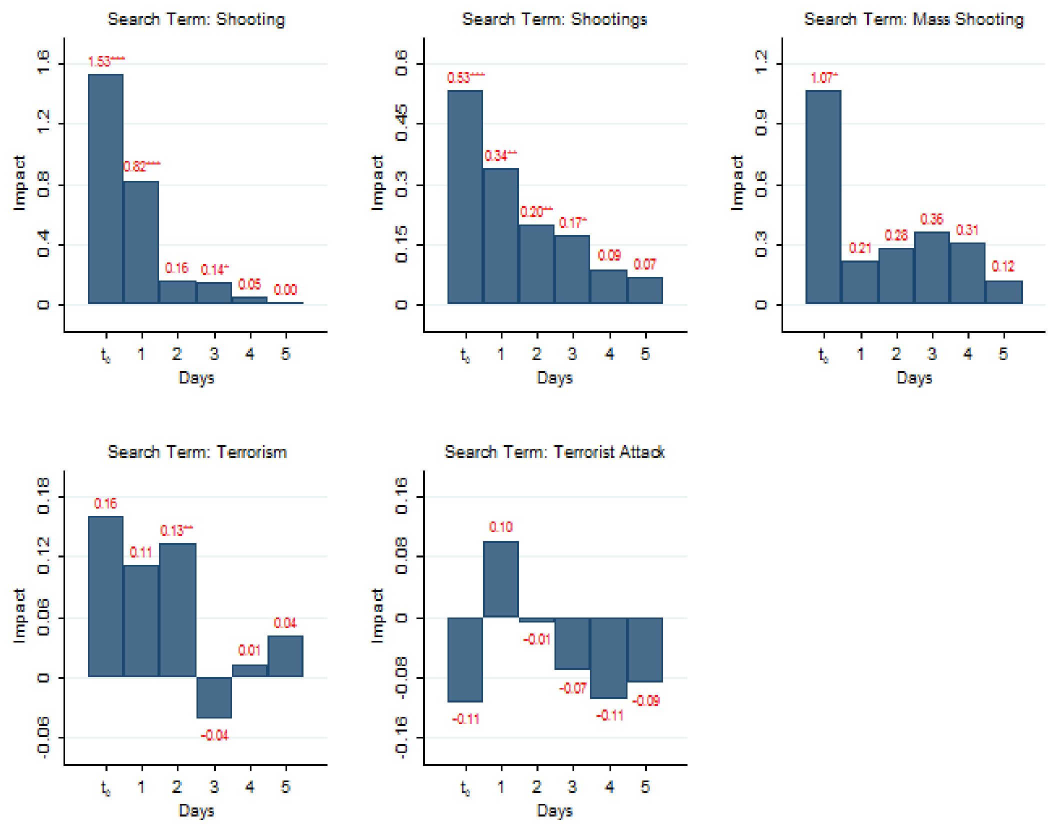

Figure 3.4. State's Daily Changes in SVI and Terrorist Events. This figure shows how state daily adjusted changes of SVI (*ΔASVI*) variate when terrorist events take place in these states. Particularly, we measure the daily SVI for each state for the following search terms: "shooting", "shootings", "mass shooting", "terrorism" and "terrorist attack". To be comparable across states, we scale each series by dividing it with the daily national SVI for each of the terms. Then, we measure the daily *ΔSVI* as the daily difference of the natural logarithm plus one for each of these scaled series. We winsorize each of the series at the 1% level. To eliminate seasonality, we regress the daily *ΔSVI* on weekdays and month dummies and keep the residuals. To address heteroscedasticity, we standardize each of the time series by dividing each by its standard deviation, which leaves us with the *ΔASVI*. This figure shows the coefficient of *Impact*, when we regress *ΔASVI* on *Impact* and include time and state fixed effects. In the first bar of each graph, *Impact* is a dummy equal to one for each state where the attack occurred at the day of the attack t_0. In the rest of the bars, *Impact* is equal to one if an attack occurred at a specific state and took place during the next one day, two, three, four or five days after the attack, respectively. Our sample spans from January 2004 until December 2010.

Concluding Remarks

This thesis examines the effect of terrorist attacks and mass shootings on the decision-making of sophisticated financial agents such as sell-side analysts, corporate managers and institutional investors. Motivated from psychology literature, we conjecture that financial agents who are located near the location of the attacks would experience a strong negative sentiment which in turn would affect their financial decisions. Our models test this conjecture by comparing the financial decisions of local agents and non-local agents around the time period of the attacks.

In the first chapter, we find strong evidence which support that sell-side analysts who are exposed to terrorist events tend to issue more pessimistic earnings forecasts. Consistent with our hypothesis, this effect is stronger for analysts that are located closer to the events and among analysts who issue forecasts during the following days after the attacks. Further, analysts who are located in states with low murder rates tend to issue more pessimistic forecasts around that period, since they perceive terrorist attacks and mass shootings as more salient. In this chapter, we also present evidence which show that affected analysts are more likely to issue bold pessimistic forecasts and less likely to issue bold optimistic forecasts. To minimize the possibility that our estimates capture some random effects, we examine whether local analysts issue more pessimistic forecasts around the anniversaries of terrorist events. According to our conjecture, such events exert a negative shock on the sentiment of the local analysts since they tend to evoke recollection of the negative experience associated with the attack. Consistent with this conjecture, our results show that around the time of the anniversaries the affected analysts tend to issue more pessimistic forecasts, however the magnitude of this effect is smaller in comparison to the effect of the original event. In this chapter, we also examine whether the forecasts of the affected analysts are more or less accurate in comparison to other forecasts which are issued for the same firm at the same quarter. Our findings suggest that the forecasts of the analysts who are exposed to the terrorist events tend to be more accurate since they can mitigate the well-documented optimism bias among analysts.

In the second chapter, we examine whether corporate managers tend to apply more risk averse firm policies when they are located near the location of terrorist events. Our findings show that managers who are located within a 50 miles radius from the attacks increase the corporate cash holdings and decrease the R&D expenditure and long-term leverage one quarter after the attacks have occurred. These effects become weaker as the distance between the firm and the attack increases. Further, these effects are present only for one quarter, suggesting that the shock to the sentiment of local managers can only cause temporary changes in the corporate policies of their firms. In this chapter, we also construct a newspaper measure to capture the saliency of the events. Our results show that the attacks that were covered by newspapers in longer articles, and the attacks that were displayed in the first page of newspapers exert a stronger effect on the local managers, who apply more prudent corporate policies in comparison to managers who are located farther away. Finally, we find evidence which suggest that the effects of terrorist events are mainly concentrated in firms managed by younger CEOs.

In the last chapter of the thesis, we examine whether sophisticated market participants such as institutional investors tend to be susceptible to the effect induced by terrorist events. Using disaggregated daily institutional trade data and hand-collected data about the location of investors, we find that institutional investors who are located near terrorist events tend to increase their selling propensity during the following 7 days after the events. Similar to previous chapters, we find that this effect increases as the geographical and temporal proximity to the attacks increase. Further, we find that this increase in the selling propensity of local institutional investors have a negative impact on stock returns and on the quarterly performance of investors. These findings are opposite to the findings presented in the first chapter where sell-side equity analysts increase their forecast accuracy when they are exposed to terrorist events.

Overall, this thesis provides evidence which supports that terrorist events can cultivate strong negative sentiment among sophisticated financial agents, distort their beliefs and in turn affect their financial decisions.

Appendix

Supporting Documentation: Chapter 2

In this section, we provide additional information regarding the second chapter of this thesis. Specifically, Table 2A.1 describes the construction of the variables included in the main empirical analysis. Table2A.2 presents information about the average corporate credit ratings of firms from the 1st quarter of 1997 to the 4th quarter of 2012. Table 2A.3 shows the analyst stock recommendations for firms included in our sample from the 1st quarter of 1997 to the 4th quarter of 2012.

Table 2A.1. Description of Variables

This table describes the accounting and macroeconomic variables used in this study. All variables are in quarterly frequency and the firm data are retrieved from Compustat.

Variables	Description
Dependent Variables	
Cash holdings	Cash divided by total assets.
R&D expenditure	Research and development expenses divided by total assets. If R&D expenses are missing, we place zero instead, unless it is stated otherwise.
Long-term leverage	Long-term financial debt scaled by short-term financial debt plus long-term financial debt plus total common equity.
Independent Variables	
Log (assets)	Logarithm of assets.
ROA	ROA is defined as net income scaled by total assets.
MB ratio	Market value divided by stockholders' equity plus deferred taxes and investment tax credit minus preferred stocks.
Sales growth	Sales growth is the logarithm of current net sales divided by last quarter's net sales.
Firm age	Fiscal year minus the year of the Initial Public Offering.
Macro-state index	To construct the index we sum the collateral ratio and the income growth rate, subtract the relative state unemployment rate and divide them by three. The state-level housing collateral ratio is the log ratio of state-level housing equity to state labor income. The relative state unemployment rate depicts the fraction of the current rate to the moving 16 quarter-average of past rates. The growth rate of labor income captures the state-level changes in labor income.

Table 2A.2. Corporate Credit Ratings

This table presents the corporate credit ratings of firms from the 1st quarter of 1997 to the 4th quarter of 2012. To measure corporate credit ratings we use the Standard & Poor's Issuer Credit Rating scale which shows the overall creditworthiness of each firm. S&P's ratings include 22 different scales, and range from AAA (very strong capacity to meet financial obligations) to SD (selective default). We do not consider credit ratings classifies as not meaningful (N.M.) and firms with no ratings. Panel A presents the definitions of ratings and the number of firms in each category. Panel B shows the distribution of corporate credit worthiness across years in our sample. The data are obtained from Compustat.

Panel A: Definition of Credit Ratings

Rating	S&P Definition	No. Firms	Rating	S&P Definition	No. Firms
AAA, AA(+,none,-)	Very strong capacity to meet financial commitments	61	*B(+,none,-)*	Adverse financial conditions will likely deteriorate the obligor's capacity to meet its financial commitments	4699
A(+,none,-)	Strong capacity to meet financial commitments	610	*CCC(+,none,-)*	Currently vulnerable and dependent upon favorable financial conditions to meet financial commitments	243
BBB(+,none,-)	Adequate capacity to meet financial commitments	2800	*CC*	Currently highly vulnerable	24
BB(+,none,-)	Major uncertainties could lead to an inadequate capacity to meet financial commitments	5162	*D, SD*	Default, Selective Default	51

Table 2A.2—*Continued*

Panel B: Average Credit Rating per Year					
Year	Aver. Rating	No. Firms	Year	Aver. Rating	No. Firms
1997	11.26	528	2005	10.95	877
1998	11.15	619	2006	10.72	935
1999	11.11	683	2007	10.64	932
2000	10.86	762	2008	10.52	946
2001	10.68	841	2009	10.22	933
2002	10.62	880	2010	10.42	942
2003	10.65	863	2011	10.50	981
2004	10.80	906	2012	10.53	1022

Table 2A.3. Stock Recommendations

This table presents the average analysts' stock recommendations from the 1st quarter of 1997 to the 4th quarter of 2012. Analysts' recommendations can be equal to 5 (Strong Buy), 4 (Buy), 3 (Hold), 2 (Underperform) and 1 (Sell). The data are obtained from I/B/E/S.

Year	Aver. Recommendation	No. Firms	Year	Aver. Recommendation	No. Firms
1997	3.93	1569	2005	3.54	1958
1998	3.90	1770	2006	3.49	2050
1999	3.96	1838	2007	3.52	2040
2000	3.99	1790	2008	3.46	2154
2001	3.84	1756	2009	3.47	1945
2002	3.58	2335	2010	3.61	1900
2003	3.46	1955	2011	3.61	1890
2004	3.51	2044	2012	3.52	1748

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