Terrorism, Emotions, and Corporate Policies

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Abstract

This paper examines whether emotion-related biases induced by extraneous negative events affect corporate decision-making. Specifically, we conjecture that corporate managers located near major terrorist attacks will experience negative emotions, which would induce them to adopt more conservative corporate policies. Consistent with our conjecture, we demonstrate that firms located near terrorist events increase their cash holdings, and reduce their R&D expenditure and their long-term leverage around the events. These effects are temporary, become weaker as the distance between the firm and the event location increases, and are mainly concentrated among firms managed by younger CEOs. Using multiple media proxies to capture the saliency of negative events, we also find that events with greater media exposure are associated with larger changes in corporate policies.

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"The truth is that the fear of Chernobyl has done more damage than Chernobyl itself." (Specter, 1996)

1. Introduction

Recent literature has demonstrated that corporate decisions are affected by managers' behavioral traits such as overconfidence, which are relatively stable over time.[1] However, little is known whether managers' *transient* emotional states also leave a mark on corporate policies. This is an important question since emotions are frequently experienced and can significantly affect decision-making.[2] In this study we examine whether terrorist attacks and mass shootings, events that have been shown to cultivate strong feelings of anxiety and fear, systematically affect corporate policies.

Our hypothesis is based on a robust finding from psychology that negative-valence events that cultivate the emotion of fear can bias the expectations for *unrelated* future outcomes.[3] For example, Johnson and Tversky (1983) found that people primed to feel negative emotions viewed negative future outcomes as *more likely* than positive future outcomes, a phenomenon broadly known as the *affect heuristic*. In a recent experimental study Kuhnen and Knutson (2011) found that the affect heuristic influences financial decisions, where people exposed to negative stimuli chose relatively safer investment options.[4]

To test whether the affect heuristic influences corporate decisions we study changes in corporate policies around terrorist attacks and mass shootings. Such events adversely affect

[1] See for example, Malmendier and Tate (2005, 2008), Landier and Thesmar (2009), Hirshleifer, Low and Teoh (2012), Malmendier, Tate and Yan (2011) and Cain and McKeon (2014).
[2] For a general discussion on the effects of emotions see Lowenstein and Lerner (2003).
[3] See for example Johnson and Tversky (1983), Bower (1991), Wright and Bower (1992), Finucane, Alhakami, Slovic and Johnson (2000), Lerner and Keltner (2001) and Slovic, Finucane, Peters and MacGregor (2002).
[4] Several studies in asset pricing have shown that external stimuli that affects emotional states, such as the weather (Saunders, 1993; Hirshleifer and Shumway, 2003; Kamstra, Kramer and Levi, 2003; Kamstra, Kramer, Levi and Wermers, 2014; Goetzmann, Kim, Kumar and Wang, 2015), sporting events (Edmans, García and Norli, 2007), and aviation disasters (Kaplanski and Levy, 2010), affect investors decisions, and asset prices.

"sentiment" because their violent and random nature highlights that anyone is potentially vulnerable. Moreover, their extensive and vivid media coverage makes them significantly more salient, which further amplifies this perception. In line with this view, Slovic (1987) suggests that terrorist attacks and mass shootings entail high "dread risk", which he identifies as the most important determinant of risk perception, and Ahern (2012) presents causal evidence that terrorism attacks adversely affect several psychological indicators.[5]

The negative shock to sentiment will be particularly intense for managers who are local to these events because such managers are more likely to interact with (or hear about) people who are more directly affected. Such "personal" experience makes the event significantly more salient, thus affecting sentiment more strongly. For example, several studies have shown that in the months after a terrorist attack a significant proportion of the local community experiences symptoms of post-traumatic stress disorder (Vlahov, Galea, Resnick, Ahern, Boscarino, Bucuvalas, Gold and Kilpatrick, 2002; Galea, Ahern, Resnick, Kilpatrick, Bucuvalas, Gold and Vlahov, 2002; Hughes, Brymer, Chiu, Fairbank, Jones, Pynoos, Rothwell, Steinberg and Kessler, 2011).[6]

Motivated from this evidence our hypothesis is that these attacks will create negative sentiment among managers of local firms, which in turn will affect their corporate decisions. To test this hypothesis, we obtain data on attacks from the Global Terrorism Database (GTD) and The Washington Post (WP). Due to various filters and data requirements, which we detail in the next section, our final sample includes 25 events during the 1997 to 2012 period.

[5] For a study on the effects of terrorism on emotions see Fischhoff, Gonzalez, Lerner and Small (2005).
[6] Further anecdotal evidence supports this notion. For example, an article published by the *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.

Our econometric models compare financial decisions of local firms that are related to firms' risk profile to the decisions of non-local firms around the period of the attacks.[7] Specifically we test for systematic differences in corporate cash holdings, research and development (R&D) and long term leverage. 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 also related to firm's level of risk, since innovative projects are considered to be riskier in comparison to other projects (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).[8] We expect that due to the adverse shock to managerial sentiment from the attacks,[9] local firms will adopt more prudent policies around attack periods, thus increasing their corporate cash holdings and decreasing their R&D expenditure and levels of corporate leverage.

Our results support this hypothesis. Around the period of the attack, 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, relative to non-local firms. These effects are robust and highly statistically significant. These policy adjustments are temporary, which is expected since emotions are relatively short-lived. Moreover, the magnitude of these adjustments is negatively related to the distance of a firm from the attack, which is also expected since proximity to an attack increases its saliency. Overall

[7] In our baseline analysis, we define as local firms those that have their headquarters in a radius of 50 miles from an attack. In our robustness section we conduct sensitivity analysis using different distances to identify local firms.

[8] Higher firm leverage is also associated with a higher level of CEO's risk-taking (Coles, Daniel and Naveen, 2006).

[9] Recent experimental work by Kuhnen and Knutson (2011) shows that exposure to negative stimuli can have an impact on both preferences and beliefs.

these results are consistent with the view that managerial emotions can be a significant source of variation in corporate policies across firms.[10]

For our next test we examine whether events that were covered in the media more prominently, and were thus more salient, caused a larger change to corporate policies. Specifically we hand-collect newspaper articles from important national outlets[11] that were related to the attacks and form three proxies. Firstly, we measure article length, since events that were covered with longer articles are more prominent. Secondly, we identify whether the attack was covered by a leading story shown in the first page of a newspaper. Thirdly, we measure whether the attack was covered in a newspaper by a leading story for multiple days. Our results across all three proxies suggest that more salient events lead to larger changes in corporate policies, supporting our hypothesis.

According to several studies in psychology, younger people cannot effectively regulate their emotions and are thus more susceptible to emotional decisions (Carstensen, Pasupathi, Mayr and Nesselroade, 2000; Blanchard-Fields, Mienaltowski and Seay, 2007; Scheibe and Blanchard-Fields, 2009). Moreover, younger people are likely to be less experienced, and several papers have shown that lack of experience can lead to stronger behavioural biases (List, 2003; Dhar and Zhu, 2006). Motivated from this evidence we examine whether younger CEOs are more responsive to these attacks, and find that the changes to corporate policies around terrorist attacks are mainly concentrated in firms that are managed by younger CEOs.

Under our affect-related hypothesis the changes to corporate policies we document are behaviourally driven, and do not reflect adjustments to genuine economic shocks. Although

[10] Several studies have shown that such mood-related biases affect investors decisions, and asset prices: see for example Saunders (1993), Hirshleifer and Shumway (2003), Kamstra et al. (2003), Edmans et al. (2007), Kaplanski and Levy (2010), Kamstra et al. (2014) and Goetzmann et al. (2015).

[11] Several studies in finance show that the media exerts a powerful influence on investors' sentiment, i.e. Tetlock (2007), Tetlock, Saar-Tsechansky and Macskassy (2008), Barber and Odean (2008), Da, Engelberg and Gao (2011), García (2013).

our models include several control variables that aim to capture the effects of economic forces on corporate policies, we conduct additional tests to analyse whether local firms around the periods of attacks are undergoing economic turmoil. Firstly, we examine whether the effects we document occur in the quarters prior to the attacks, and find no supportive evidence. However, even though this test precludes the existence of pre-existing "parallel trends", it is still possible that attack periods coincide with economic shocks that merit adjustments to corporate policies. To examine whether this is the case we test whether local firms around attack periods are experiencing changes to corporate credit ratings, analyst recommendations, stock return volatility and firm sales. Since these economic indicators are produced by agents that are *external* to the firm and thus not affected by the attacks, any changes in them would signal the occurrence of an economic shock. Our results show that there is *no* significant change in *any* of these indicators, which suggests that local firms are not undergoing significant economic shocks during attack periods.

We use various tests to ensure that our results our robust. Firstly, we conduct placebo tests randomizing the time or the location of the attacks and, as expected, find no significant effects. Moreover, our results are robust to eliminating the 9/11 attacks from our sample (the most economically significant event), and to eliminating firms with missing R&D values (as opposed to setting their R&D's to zero). Finally, to control for misspecification of the control group, we use propensity scores matching to create control groups of non-treated firms that share similar characteristics as treated firms, and find that our results continue to hold. We discuss our robustness checks in more detail in Section 4.

Our study contributes to the behavioral corporate finance literature that examines whether CEOs behavioural biases affect corporate decisions. Malmendier and Tate (2005) show that overconfident managers tend to overinvest when they have abundant internal funds, while Malmendier and Tate (2008) argue that these managers engage in value-

destroying mergers and acquisitions. Landier and Thesmar (2009) and Hirshleifer et al. (2012) show that overconfidence affects decisions related to capital structure and R&D expenditure, respectively. Baker, Pan, and Wurgler (2012) show that psychological anchors related to the target's past price patterns affect premium decisions in mergers and acquisitions, whereas Dougal, Engelberg, Parsons and Wesep (2015) show that anchoring on the behaviour of credit spreads affects firms' cost of borrowing. Dessaint and Matray (2015) and Hutton et al. (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 et al., 2011; Cain and McKeon, 2014).[12] Our study contributes to the literature by showing that transient emotion-related biases cultivated from proximity to terrorist attacks also affect corporate policies.

We also contribute to the literature that analyses the implications of terrorist attacks. Ahern (2012) shows that terrorist attacks, through their adverse effect on sentiment, impact macroeconomic activity. Di Tella and Schargrodsky (2004) and Gould and Stecklov (2009) show that terrorist attacks entail an indirect economic effect as they alter government policies. Other studies show that terrorist attacks influence political views (Gould and Klor, 2010), and election outcomes (Montalvo, 2011). Our study contributes by presenting new evidence that terrorist attacks influence the risk profiles of publicly traded firms.

2. Data and Methodology

2.1 The Model

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

[12] For a review of the behavioural corporate finance literature see Baker and Wurgler (2012).

methodology controls for fixed differences between the control and treatment groups via firm and time fixed effects. Similarly to Bertrand and Mullainathan (2003), our treatment group includes all the firms which are local to a terrorist attacks at time *t*, when the attack took place. The control group includes all the remaining firms. In our baseline analysis, we define as local firms those that have their headquarters in a distance less than 50 miles from an attack.[13] The model has the following structure:

$$Y_{i,s,t+1} = c + \alpha_i + \delta_t + \beta \times Impact_{s,t} + \gamma \times X_{i,s,t} + \varepsilon_{i,s,t+1} \qquad (1)$$

In (1) *i* indexes firms, *t* indexes time (quarter) and *s* indexes location. $Y_{i,s,t+1}$ is the examined corporate policies for firm *i* at time *t+1* (cash holdings, R&D expenditure or long-term-leverage), α_i is firm fixed effects, 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 local to an attack at time *t*.[14]

Our models include several control variables that have been shown by previous literature to affect corporate policies, indexed in equation (1) with $X_{i,s,t}$. These are 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).[15] To control for the possibility that terrorist attacks are driven by the local macroeconomic environment we include in our models the state-level macroeconomic index proposed by Korniotis and Kumar (2013), where increases in this index flag improvements

[13] 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. To calculate the distance between these coordinates we follow the procedure in Vincenty (1975).

[14] Even though emotions tend to be short-lived, Andrade and Ariely (2009) show that they can have a relatively longer-lasting effect on decision making because they can influence decisions which become the basis for future decisions. Thus, they influence decision making even after the emotion subsides.

[15] 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 in sales with risks associated with expected growth rates (Johnson, 2002).

in local macroeconomic conditions. The appendix provides a detailed description for all variables. Due to data availability the sample period for our baseline tests is 1997-2012.

The data on terrorist attacks and mass shootings come from the Global Terrorism Database (GTD)[16] and The Washington Post list (WP),[17] respectively. GTD is an open-source database that contains systematic data on terrorist attacks (START, 2012), while WP shows the deadliest shootings in U.S. history. Based on these databases, we collect information regarding the date, location, and the type of each event. To include an event in our sample we apply the following filters: firstly, we retain events that have taken place in the U.S. Secondly, to ensure that our sample includes high-impact events that are likely to cultivate negative sentiment, we only retain events that involved human casualties, and were covered in newspaper articles.[18] From the resulting sample we eliminate 7 events for which we could not validate an exact location, and 2 events that involved robberies.[19] Table 1 lists the 25 events for the period 1997-2012 that are included in our final sample, and Figure 1 shows their geographical dispersion.

[Insert Table 1 and Figure 1 here]

We retrieve quarterly firm-level financial variables from Compustat. We exclude from our sample all firms not located in the U.S., and 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.

[16] For more information, please see http://www.start.umd.edu/gtd/.
[17] This list contains the deadliest shootings occurred in the US, http://www.washingtonpost.com/wp-srv/special/nation/deadliest-us-shootings/.
[18] To find whether an event appeared in the media, we search using Factiva all articles published in major U.S. 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*) for a period of 7 days after the event, using as keywords the name and type of the event, or the name of the place that the attack took place.
[19] Since our aim is to examine the impact of unpredictable and salient events, we exclude robberies, which reflect common criminal activity.

Table 2 displays 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 attack ($Impact_{s,t}$=1) and those they were not ($Impact_{s,t}$=0). From Table 2, we observe some early evidence in support of our hypothesis as affected firms exhibit higher levels of cash holdings, lower R&D expenditure and long-term leverage in period *t+1*.

[Insert Table 2 here]

3. Empirical Analysis

3.1 The Impact of Attacks on Local Firms

The results are shown in Table 3. In terms of our hypothesis, we find that terrorist attacks affect the risk exposure of local firms. More specifically, local firms increase their cash holdings by 1.67% (*t-statistic*: 4.06) relative to non-local firms. Moreover, as seen in the sixth column of Table 3, local firms decrease their R&D expenditures by 0.17% (*t-statistic*: -2.99). Finally, the ninth column of Table 3 shows that local firms decrease their long-term leverage by 0.87% (*t-statistic*: -2.93). Since these variables capture the risk profile chosen by managers, the changes we document are consistent with the notion that local firms adopt more prudent policies around the period of an attack relative to firms that are situated further away.

In terms of 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 (2015). Furthermore, similarly 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 of Hutton et al. (2014). Finally, as shown by columns 3, 6

and 9, increases to the macroeconomic index lead to higher cash holdings, but do not affect significantly the R&D expenditure, and long-term leverage.

[Insert Table 3 here]

We next examine the sensitivity of our findings to the specification of local firms. According to our hypothesis, the sentiment effects related to the attack will become more intense the closer firms are to the attack. Therefore we expect that, as we expand our definition of local to include firms situated further away from the attacks, the magnitude of $Impact_{s,t}$ should decrease. To test this notion, we estimate the model shown by equation (1), by specifying as local firms those with headquarters 30, 50, 70, 90, 110, 130, and 150 miles away from the attacks. In Table 4, we list the coefficient of $Impact_{s,t}$ for cash holdings, R&D expenditure and long-term leverage for these different specifications. As shown by Table 4, when we define as local the firms that are situated at a radius of 30 miles from the attacks, the changes to corporate policies *are larger* compared to those in the baseline analysis of Table 3 (shown in the second row of Table 4 to ease comparison), with local firms increasing their cash holdings by 1.76%, and reducing their R&D and leverage by -0.23% and -0.98%, respectively. These effects are statistically significant. Conversely, when we define as local the firms situated at a radius of 90 miles or more, we observe that the changes to corporate policies are smaller and generally statistically insignificant. These findings suggest that sentiment effects are stronger when the firm is situated closer to the location of the attack, in line with our hypothesis.

[Insert Table 4 here]

The negative sentiment that will be cultivated by terrorist attacks will be relatively short-lived, which implies that the observed changes to corporate policies should be temporary. We continue to examine this notion, by testing a version of the model in

equation (1), where we specify lead values for the dependent and control variables ($t+i$, with t being the quarter of the attack). We estimate two versions of this model, for $i=2$ and 3, listing in Table 5 the coefficients of $Impact_{s,t}$.

The results related to $t+1$ are identical to those in Table 3 (columns 3, 6 and 9), and we re-list them here to ease comparisons. As it can be seen from Table 5, the coefficient on $Impact_{s,t}$ becomes smaller and insignificant during the following two quarters, which suggests that the sentiment effects we document in Table 3 are short-lived.

[Insert Table 5 here]

3.2 News Coverage, Emotions and Firm Policies

In this section, we conduct tests that identify whether more salient events, which are likely to cultivate stronger sentiment effects, led to larger changes in corporate policies. To quantify the saliency of the attacks, we construct proxies based on news coverage, which has been shown to have a strong influence on behaviour across 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 attacks which featured more prominently in the media will affect managerial sentiment more strongly, and thus have a larger impact on corporate policies. To test this notion, we use media analysis to identify whether an attack was high or low in saliency, and then re-estimate the model in (1) by interacting $Impact_{s,t}$ with dummies that flag these different cases.

To construct the media proxies, we search using Factiva for articles published in major media outlets in the seven day period after each attack in our sample, using as keywords the name and type of the event, or the name of the place that the attack 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 proxy measures articles' length by counting the number of words in each attack-related article. Our conjecture is that, since events which were presented in longer articles are more salient, such articles will exert a larger impact on corporate policies. To construct this proxy, we firstly gather all the articles referred to a specific attack, count the number of words in each article, noting the median of this distribution. We do this for every attack, which results to an overall distribution of medians. If any attack specific median is higher or equal to the median of this overall distribution, the dummy variable $Article\text{-}Size_h$ is equal to one, or else it is equal to zero. Similarly, $Article\text{-}Size_l$ 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 illustrates whether an article is a leading story, thus 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 corporate policies. From our total sample of 372 articles 76 of them are displayed in 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 in the first page of at least one newspaper.

We also consider a variation of the aforementioned dummy, by measuring whether an attack featured as leading story on multiple days. Our conjecture is that attacks that were covered as leading stories for multiple days should exert a stronger influence on managerial sentiment and corporate policies. To construct this proxy, we gather all the articles referred to a specific event and count the number of days that this event was displayed as a cover story of a newspaper.[20] Among the 25 events of our sample, we find that the median number

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

of days that they were presented in the first page of newspapers is two days. We compare the attack-specific duration with the median duration of all the events of our sample, and if the attack-specific duration is higher or equal to the median duration of all the attacks the dummy variable $First\ Page_{(Long\ Duration)}$ is equal to one or else it is equal to zero. Similarly, $First\ Page_{(Short\ Duration)}$ is equal to one if the attack-specific duration is less than the overall median duration.

Table 6 presents the results, whereby the effect of $Impact_{s,t}$ on corporate policies is estimated separately for high and low salience events. In Panel A salience is captured with articles' length. Consistent with our hypothesis we find that, across all three measures of firms' risk profile, the effect is stronger for longer articles. Specifically, high salience attacks were associated with an additional increase in cash holdings of 1.38%, and an additional decrease in R&D expenditure and long-term leverage by 0.22% and 2.01%, respectively, in relation to low salience attacks.

In Panel B salience is captured by coverage as a leading story. The results show that local firms after high salience attacks increase their cash holdings by a further 0.88%, and decrease their R&D expenditure and long-term leverage by 0.26% and 1.57%, respectively, relative to low salience attacks.

Finally in Panel C, salience is captured with coverage as a leading story for multiple days. The results show that local firms after high salience attacks increase their cash holdings by an additional 1.39%, and decrease their R&D expenditure and long-term leverage by 0.17% and 1.50%, respectively, relative to low salience attacks.

In every Panel of Table 6 the row *difference* presents the difference between the coefficients on $Impact_{s,t}$ for the two groups (high vs. low saliency), and indicates whether it is significant using a Wald test. We find that across all the different corporate policy measures and salience proxies, the difference is statistically significant.

Overall, the results in Table 6 support our hypothesis, showing that corporate policies are affected more strongly by more salient attacks, which are more likely to adversely influence managerial sentiment.

[Insert Table 6 here]

3.3 CEO Age, Emotions and Corporate Policies

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). Motivated from this evidence, we examine whether the changes in corporate policies we document are particularly pronounced amongst younger CEOs.

To test this conjecture, we use data from Execucomp regarding the age of CEOs, and define the dummy $Age_{(Low)}$ ($Age_{(High)}$), which equals 1 if the CEO's age falls in the bottom (top) third of the age distribution for that particular industry, using the Fama-French 48 industry classification (correspondingly $Age_{(Mid)}$ equals one if the CEO's age falls in the middle third of the age distribution).[21] 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. Moreover, CEOs in the low age group have an average of 17 quarters of company-specific CEO experience, whilst the corresponding figure for CEOs in the high age group is 23 quarter (*p*-value of difference: 0.000).

We estimate the model in (1) by interacting these age-related dummies with $Impact_{s,t}$. To this model, in addition to the controls we use in our baseline model in Table 3, we also control for the CEO's gender, since experimental evidence show males are less risk averse

[21] We consider industry-adjusted benchmarks, since it has been shown that CEO age varies systematically between certain types of firms (i.e. Acemoglu, Akcigit, and Celik, 2014).

than females (Antoniou, Harrison, Lau and Read, 2014). 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 3).[22]

The results are presented in Table 7. In columns 1, 3 and 5 of Table 7, we estimate our baseline model in this smaller sample without accounting for CEO age. We find that our baseline results are robust, with $Impact_{s,t}$ having the expected sign across all three corporate policy measures and being statistically significant. In columns 2, 4 and 6 of Table 7 we estimate the models for cash holdings, R&D expenditure and long-term leverage, respectively. The results show 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. Even though the difference in the coefficient of $Impact_{s,t}$ for low and high age CEOs is statistically significant only for R&D expenditure, the finding that in all cases the point estimate of $Impact_{s,t}$ indicates a smaller, and statistically insignificant effect for older CEOs, suggests that the changes to the corporate policies are mainly concentrated among firms managed by younger CEOs.

[Insert Table 7 here]

4. Additional Tests

Under our affect-related hypothesis the changes to corporate policies we document are behaviourally driven, and do not reflect adjustments to economic shocks. Such shocks may be related to either an economic impact of the attacks themselves, or to other external

[22] Furthermore, the sample period of cash holdings starts from 2002 to 2012 and is based on 15 terrorist attacks (instead of 25).

factors that just happen to coincide with attack periods. In this section we conduct tests that examine in more detail whether local firms during attack periods are experiencing any economic turmoil.

Firstly we test the parallel trends assumption, whereby we estimate our model in equation (1) whilst including lagged values of the treatment variable, $Impact_{t-1}$ and $Impact_{t-2}$, to examine whether the changes to the corporate policies we document are related to any pre-existing shocks, unrelated to the attacks. Panel A in Table 8 reports the results, and shows that $Impact_{t-1}$ and $Impact_{t-2}$ are insignificant, whereas $Impact_t$ remains highly significant, similarly to Table 3. This suggests that the changes to corporate policies only occur around the period of the attack, and are temporary (as shown in Table 5).

The 9/11 terrorist attacks are by far the most economically impactful events in our sample, as evidenced by the sharp declines in global stock markets during that period.[23] To establish that our results are not just driven by the economic effects associated with the 9/11 attacks we repeat the analysis in equation (1) excluding the three 9/11-related events from our sample. Panel B in Table 8 shows the results, which are robust and remain consistent with our findings in Table 3.

For 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 attacks. Specifically we estimate four different versions of our models, using as 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

[23] It is estimated that the 9/11 attacks resulted in approximately $40 billion in insurance losses. Furthermore, these attacks resulted to losses from reductions in tourism, and contributed to increased political instability, with potentially even greater costs. For a review on the economic consequences of the 9/11 attacks see Makinen (2002).

(CRSP), and their sales obtained from Compustat.[24] These variables are important indicators of the state of firms' fundamentals, as it is likely that companies that are undergoing economic turmoil will experience changes, in at least some of them. 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 not so affected by the attacks, are adjusting their behaviour toward these firms due to an economic shock. [25]

The results are shown in Table 8 Panel C. In each case, for robustness, we present results from two separate models, where the dependent variable is measured at *t* or *t*+1. Across all four indicators, we find that the coefficient on $Impact_{s,t}$ is indistinguishable from zero, showing that local firms around attack periods are not experiencing any changes to their credit ratings, analyst recommendations, stock price volatility or sales. This suggests that local firms around attack periods are not experiencing significant economic shocks.

[Insert Table 8 here]

We continue various robustness checks. Firstly, we conduct two placebo tests to examine whether our effects are spurious. In the first test we randomly assign a new date to each of the terrorist attacks in our sample during the period 1997-2012, and create the dummy variable $Impact_{(Random\ time)}$. This dummy variable takes the value of 1 if firms are local to the attacks at these random times and 0 otherwise. Then we estimate our baseline model shown in equation (1), recording the coefficient, standard error and *p*-value of this variable. We repeat this procedure 1000 times, and in Panel A of Table 9 we report the

[24] Tables A2 and A3 in the Appendix provide descriptive statistics on credit rating and recommendation data we use in our analysis.
[25] If these agents are local, they may adjust their behaviour due to affect-related biases. However, these agents are not likely to be local to treated firms, at least not on average.

average coefficient, standard error and *p*-value of $Impact_{(Random\ time)}$. Across all three corporate policy proxies we find that 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,[26] forming the variable $Impact_{(Random\ location)}$. This dummy variable takes the value of 1 if firms are local to the random locations at the time of the attack and 0 otherwise. Then we estimate our baseline model shown in equation (1), recording the coefficient, standard error and *p*-value of this variable. We repeat this procedure 1000 times, and in Panel B of Table 9 we report the average coefficient, standard error and *p*-value of $Impact_{(Random\ location)}$. Again, across all three corporate policy proxies we again find that the average coefficient of $Impact_{(Random\ location)}$ is indistinguishable from zero. Overall, the evidence from these placebo tests suggests that the changes to the corporate policies we document only occur at the time of the attacks among local firms.

In our analysis in Table 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). For our next test we examine whether our results related to R&D expenditure are robust when we drop observations with missing R&D values. Panel C in Table 9 illustrates 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 again the results from Table 3 to ease comparisons. Our findings remain robust in this reduced sample.

One potential concern in our estimations may be that our findings are induced by the misspecification of the control group. To mitigate these concerns, we create alternative control groups with similar characteristics as our treatment group, along firm-specific (i.e.,

[26] Since terrorist attacks 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.

company size, MB ratio, Sales growth) and aggregate dimensions (Industry, Macro-Index). To construct the matched sample, we use the nearest neighbour matching estimator, which allows us to match firms with similar propensity scores and thus similar characteristics. We then drop all the firms that were not matched, and re-estimate our baseline model. Panel D in Table 9 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 3.

[Insert Table 9 here]

5. Conclusion

In this study we examine whether managerial sentiment affects CEOs' corporate decisions. Our hypothesis is that the sentiment of managers of firms that are local to terrorist attacks will be adversely affected, inducing them to adopt more prudent corporate policies.

Supporting this hypothesis, we find that local firms around attack periods increase the level of their cash holdings and decrease their R&D expenditure and long-term leverage. This finding is consistent with the behavioural literature, which shows that extraneous events that cultivate negative sentiment can lead to increased risk aversion and/or pessimistic beliefs. Moreover, we find that these effects are mainly concentrated among firms managed by younger CEOs, and are stronger when the attack can be classified as more salient. We do not find any evidence that local firms during attack periods experience changes to their credit ratings, analyst recommendations, stock price volatility or firm sales, which suggests the absence of economic shocks. Overall, our results suggest that proximity to terrorist attacks cultivates negative sentiment, which induces managers to adopt more prudent corporate policies.

Appendix

Table A1
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-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 A2
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

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

Panel B

1997-2004			*2005-2012*		
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 A3
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.

1997-2004			*2005-2012*		
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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Table 1
Sample of Terrorist Attacks

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

No	Description	Date	Location
1	Empire State Building	23/02/1997	New York City, NY
2	Abortion Clinic Bombing	29/01/1998	Birmingham, AL
3	U.S. Capitol	24/07/1998	Washington, DC
4	Barnett Slepian Murder	23/10/1998	Amherst, NY
5	Columbine High School	20/04/1999	Littleton, CO
6	Korean Methodist Church	04/07/1999	Bloomington, IN
7	9/11 Attacks: World Trade Center	11/09/2001	New York City, NY
8	9/11 Attacks: Hijacked Plane Crashed	11/09/2001	Alexandria, VA
9	9/11 Attacks: Hijacked Plane Crashed	11/09/2001	Somerset County, PA
10	Bank of America	05/01/2002	Tampa, FL
11	LA International Airport	04/07/2002	Los Angeles, CA
12	Seattle Jewish Federation	28/07/2006	Seattle, WA
13	Virginia Tech	16/04/2007	Blacksburg, VA
14	Knoxville Church	27/07/2008	Knoxville, TN
15	Immigration Centre	03/04/2009	Binghamton, NY
16	George Tiller Murder	31/05/2009	Wichita, KS
17	Little Rock	01/06/2009	Little Rock, AR
18	Holocaust Museum	10/06/2009	Washington, DC
19	Fort Hood	05/11/2009	Killeen, TX
20	IRS Building	18/02/2010	Austin, TX
21	Pentagon	04/03/2010	Arlington, VA
22	Discovery Communications	01/09/2010	Silver Springs, MD
23	Aurora	20/07/2012	Aurora, CO
24	Sikh Temple	05/08/2012	Oak Creek, WI
25	Sandy Hook School	14/12/2012	Sandy Hook, CT

Table 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 the firms of the sample 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 at a 50 miles radius from an attack at time *t*. Accordingly, *unaffected firms* represent the rest of the firms in the sample.

Panel A

Variable	Obs.	Mean	Std. dev.	25^{th} Pctl.	Median	75^{th} Pctl.
Cash holdings$_{t+1}$	39032	0.170	0.182	0.035	0.110	0.237
R&D expenditure$_{t+1}$	143311	0.021	0.046	0.000	0.000	0.025
Long-term leverage$_{t+1}$	135002	0.239	0.348	0.000	0.078	0.379
Log(assets)	143311	4.864	2.004	3.545	4.904	6.241
ROA	143311	-0.043	0.179	-0.040	0.003	0.018
MB ratio	143311	2.941	6.835	0.971	1.891	3.607
Sales growth	143311	0.268	1.266	-0.302	0.113	0.772
Firm age	143311	8.172	5.496	4.000	7.000	12.000
Macro-index	143311	-0.013	0.561	-0.352	-0.036	0.342

Panel B

	Affected Firms			*Unaffected Firms*		
Variable	Obs.	Mean	Std. dev.	Obs.	Mean	Std. dev.
Cash holdings$_{t+1}$	314	0.202	0.191	38718	0.169	0.182
R&D expenditure$_{t+1}$	1483	0.018	0.041	141828	0.021	0.046
Long-term leverage$_{t+1}$	1368	0.230	0.340	133634	0.239	0.348
Log(assets)	1483	4.585	2.129	141828	4.867	2.002
ROA	1483	-0.044	0.154	141828	-0.043	0.179
MB ratio	1483	2.782	6.655	141828	2.942	6.837
Sales growth	1483	0.137	1.303	141828	0.269	1.265
Firm age	1483	7.067	5.276	141828	8.184	5.497
Macro-index	1483	-0.240	0.559	141828	-0.010	0.560

Table 3
Terrorist Attacks and Corporate Policies

This table presents the results from regressions examining the impact of 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 which have their headquarters inside a 50 miles radius from an attack. The dependent variables are defined as the lead values by one quarter of cash holdings, R&D expenditure and long-term leverage. The samples of the dependent variables are unbalanced due to the limited availability of certain variables in Compustat. 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	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-index			0.25*			0.02			0.03
			(0.13)			(0.02)			(0.25)
Constant	27.4***	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 Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	39032	39032	39032	143311	143311	143311	135002	135002	135002
Adjusted R^2	0.73	0.73	0.73	0.59	0.61	0.61	0.59	0.59	0.59

Table 4
Distance to Terrorist Attacks and Corporate Policies

This table examines 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 closer than 50 miles from the area of the attacks. In this table, we follow the regression specification of Table 3 and we 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 the control variables, year-quarter fixed effects and firm fixed effects as in Table 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.

Independent Variable	**Dependent Variable**			**Control Variables**	**Firm F.E.**	**Time F.E.**
	Cash Holdings$_{t+1}$	*R&D Expenditure*$_{t+1}$	*Long-term Leverage*$_{t+1}$			
Impact$_{(30miles)}$	1.76***	-0.23***	-0.98*	Yes	Yes	Yes
	(0.40)	(0.03)	(0.49)			
Impact$_{(50miles)}$	1.67***	-0.17***	-0.87***	Yes	Yes	Yes
	(0.41)	(0.06)	(0.30)			
Impact$_{(70miles)}$	1.26*	-0.10**	-0.75**	Yes	Yes	Yes
	(0.69)	(0.04)	(0.29)			
Impact$_{(90miles)}$	0.89	-0.11**	-0.14	Yes	Yes	Yes
	(0.65)	(0.04)	(0.52)			
Impact$_{(110miles)}$	0.60	-0.01	-0.32	Yes	Yes	Yes
	(0.65)	(0.06)	(0.42)			
Impact$_{(130miles)}$	0.61	-0.06	-0.24	Yes	Yes	Yes
	(0.45)	(0.07)	(0.48)			
Impact$_{(150miles)}$	0.27	-0.04	-0.08	Yes	Yes	Yes
	(0.41)	(0.08)	(0.40)			

Table 5
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 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 which have their headquarters inside 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: $Y_{i,s,t+i} = c + \alpha_i + \delta_t + \beta\ Impact_{s,t0} + \gamma\ X_{i,s,t+i-1} + \varepsilon_{i,s,t+i}$. The dependent variables are defined as the lead values by one, two and three quarters of cash holdings, R&D expenditure and long-term leverage. $Impact_{t0}$ 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 the control variables as in Table 3.We run one regression for each i=1,2 and 3, and report the coefficient and standard error on $Impact_{t0.}$ 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.
	$Impact_{t0}$			
Cash Holdings$_{t+1}$	1.67***	Yes	Yes	Yes
	(0.41)			
Cash Holdings$_{t+2}$	0.59	Yes	Yes	Yes
	(1.12)			
Cash Holdings$_{t+3}$	1.00	Yes	Yes	Yes
	(0.86)			
R&D Expenditure$_{t+1}$	-0.17***	Yes	Yes	Yes
	(0.06)			
R&D Expenditure$_{t+2}$	0.10	Yes	Yes	Yes
	(0.06)			
R&D Expenditure$_{t+3}$	0.08	Yes	Yes	Yes
	(0.06)			
Long-term Leverage$_{t+1}$	-0.87***	Yes	Yes	Yes
	(0.30)			
Long-term Leverage$_{t+2}$	-0.29	Yes	Yes	Yes
	(0.25)			
Long-term Leverage$_{t+3}$	0.39	Yes	Yes	Yes
	(0.42)			

Table 6
Terrorist Attacks, 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 which were presented in longer articles are more salient. If the median size of articles for an attack is higher or equal to the median length of articles from all the attacks in our sample, the dummy variable *Article-Size*$_h$ is equal to one, or else *Article-Size*$_l$ is equal to one. In Panel B, we examine whether first page articles exert a stronger impact on corporate policies. *First Page*$_{(Dummy=1)}$ is a dummy equal to one if an event is presented in the first page of at least one newspaper. In Panel C, we test whether attacks that were covered in the first page for multiple days have a stronger influence on corporate policies. *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 *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 *Impact* for the two groups (high vs. low saliency), using a Wald test to examine if this difference is statistically significant. We include all the control variables, year-quarter fixed effects and firm fixed effects as in Table 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

Independent Variables	**Dependent Variables**		
	Cash Holdings$_{t+1}$	*R&D Expenditure*$_{t+1}$	*Long-term Leverage*$_{t+1}$
Impact × Article-Size$_h$	2.07***	-0.21***	-1.26***
	(0.42)	(0.05)	(0.43)
Impact × Article-Size$_l$	0.69	0.01	0.75
	(0.61)	(0.09)	(0.51)
Control Variables	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Difference	1.38**	-0.22**	-2.01***

Panel B

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

Panel C

Independent Variables	**Dependent Variables**		
	Cash Holdings$_{t+1}$	*R&D Expenditure*$_{t+1}$	*Long-term Leverage*$_{t+1}$
Impact × First Page$_{(Long\ Duration)}$	2.06***	-0.21***	-1.21**
	(0.41)	(0.05)	(0.45)
Impact × First Page$_{(Short\ Duration)}$	0.67	-0.04	0.29
	(0.65)	(0.07)	(0.48)
Control Variables	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
Difference	1.39**	-0.17*	-1.50**

Table 7
Terrorist Attacks, CEO Age and Corporate Policies

This table presents regressions which examine how terrorist attacks affected the policies of local firms depending on whether these firms were managed by young, middle-aged and 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. $Age_{(Low)}$ is equal to one if CEOs' age is lower or equal to 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 or greater 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 the nonutility and nonfinancial firms from the 2nd quarter of 2002 to the 4th quarter of 2012 which are located in the U.S. 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 3, 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	2.24***		-0.14*		-2.50***	
	(0.41)		(0.07)		(0.83)	
Impact × $Age_{(Low)}$		4.08***		-0.26***		-2.52***
		(1.06)		(0.07)		(0.82)
Impact × $Age_{(Middle)}$		1.19		0.03		-4.97**
		(0.69)		(0.18)		(2.26)
Impact × $Age_{(High)}$		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 Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Difference		2.1		-0.3***		-0.52
N	15051	14854	39433	35306	37896	33942
Adjusted R^2	0.75	0.75	0.65	0.67	0.65	0.67

Table 8
Economic Impact

In this table we test the parallel trends hypothesis and examine the potential economic effect of terrorism attacks. In Panel A, we include lag values of *Impact* to test whether the parallel trends assumption holds and we re-estimate the model shown in equation (1). In Panel B, we examine whether potential economic effects driven by the 9/11 attacks affect the corporate policies of local firms. Therefore, we exclude 9/11 terrorist attacks from the event sample and repeat the analysis of Table 3. In Panel C, we examine whether terrorist attacks had an impact on the credit ratings, analysts' recommendations, stock return volatility, and sales of local firms. To measure the impact of terrorist attacks on the credit worthiness of local firms, we use as dependent variable the variable *Credit Rating* 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 attacks on stock recommendations, we create the variable *Average Recommendation* 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 attacks on stock return volatility. We measure *Stock Return Volatility* as the standard deviation of all 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. 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.

Panel A

	Cash Holdings$_{t+1}$	*R&D Expenditure*$_{t+1}$	*Long-term Leverage*$_{t+1}$
Impact$_t$	1.65***	-0.14***	-1.06***
	(0.40)	(0.05)	(0.28)
Impact$_{t-1}$	0.87	0.09	-0.15
	(1.27)	(0.06)	(0.29)
Impact$_{t-2}$	0.80	0.10	0.53
	(0.86)	(0.06)	(0.49)
Control Variables	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
N	38845	141863	133619
Adjusted R^2	0.73	0.62	0.59

Panel B

	Cash Holdings$_{t+1}$	*R&D Expenditure*$_{t+1}$	*Long-term Leverage*$_{t+1}$
Impact	1.68***	-0.09**	-1.16***
	(0.41)	(0.04)	(0.40)
Control Variables	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes
N	39032	143311	135002
Adjusted R^2	0.73	0.61	0.59

Table 8 (Continued)

Panel C

	Credit Rating		Average Recommendation		Stock Return Volatility		Firm Sales	
	[t]	*[t+1]*	*[t]*	*[t+1]*	*[t]*	*[t+1]*	*[t]*	*[t+1]*
Impact	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 Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	13650	13014	30742	27770	5226	4948	143311	139063
Adjusted R^2	0.86	0.85	0.20	0.18	0.16	0.16	0.78	0.77

Table 9
Robustness Tests

This table presents 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 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 from our sample observations with missing R&D values and re-estimate the R&D model from (1). The second column of Panel C shows again the R&D result from Table 3 (column 6). In panel D, we present the results from estimating our baseline specification on a sample of matched firms. To construct the matched sample, we use the nearest neighbour 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-index* and firm's *Industry*. We define *Industry* as in Table 7, using the Fama-French 48 industry classification. We then re-estimate our baseline model using as control group only the matched firms. In all Panels, we include similar control variables, year-quarter fixed effects and firm fixed effects as in Table 3. Also, 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

	$Cash\ Holdings_{t+1}$	$R\&D\ Expenditure_{t+1}$	$Long\text{-}term\ Leverage_{t+1}$
$Impact_{(Random\ time)}$	-0.10	0.00	0.14
	(0.49)	(0.08)	(0.55)
	[0.37]	[0.43]	[0.42]
Control Variables	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes

Panel B

	$Cash\ Holdings_{t+1}$	$R\&D\ Expenditure_{t+1}$	$Long\text{-}term\ Leverage_{t+1}$
$Impact_{(Random\ location)}$	0.04	-0.02	0.32
	(0.69)	(0.13)	(1.10)
	[0.39]	[0.37]	[0.42]
Control Variables	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes

Table 9 (Continued)

Panel C

	R&D Expenditure$_{t+1}$	
	[1]	[2]
Impact	-0.21**	-0.17***
	(0.10)	(0.06)
Control Variables	Yes	Yes
Firm Fixed Effects	Yes	Yes
Time Fixed Effects	Yes	Yes

Panel D

Matching Attributes	**Dependent Variable**			**Control Variables**	**Firm F.E.**	**Time F.E.**
	Cash Holdings$_{t+1}$	*R&D Expenditure*$_{t+1}$	*Long-term Leverage*$_{t+1}$			
MB ratio, Industry	2.69***	-0.18*	-1.10*	Yes	Yes	Yes
	(0.57)	(0.09)	(0.58)			
MB ratio, Macro-index	1.19**	-0.28**	-2.29***	Yes	Yes	Yes
	(0.47)	(0.12)	(0.52)			
Log(assets), Macro-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-index, MB ratio, Log(assets), Sales growth	1.11**	-0.22*	-1.33**	Yes	Yes	Yes
	(0.48)	(0.11)	(0.61)			
Macro-index, MB ratio, Log(assets), Sales growth, Industry	3.68***	-0.14**	-1.43**	Yes	Yes	Yes
	(0.92)	(0.06)	(0.55)			

Figure 1
Terrorist Attacks and Locations

This figure shows the states where the terrorist attacks and mass shootings took place.