Economic Costs of Terror: An Asset Market Approach

By Edward Jee*

I show that large terror attacks do not have a significant negative effect on UK asset markets. Furthermore, when I expand my analysis to include all terror attacks from 1970 to present day I find no evidence of a negative asset market response. Finally, I find no evidence of event heterogeneity. That is, target type and location; success of an attack; number of wounded or killed; weapons used; attack type and media coverage cannot be used as predictors for asset market responses. Results are consistent across a range of UK indices and specifications.

Terror attacks in developed countries have long been studied by economists as sources of exogenous shocks and events of interest in their own right. However, comparatively little effort has been expended exploring responses to the terror distribution outside of extreme tail events such as 9/11 or London 7/7 [1]. This paper aims to quantify the responses to all attempted terror attacks occurring in the UK from 1970-2016 and estimate the determinants of these responses.

The Global Terrorism Database (GTD) defines terror attacks as the threatened or actual use of illegal force and violence by a non-state actor to attain a political, economic, religious, or social goal through fear, coercion, or intimidation. This definition admits two clear economic channels through which the consequences of an attack propagate: the direct effect is comprised of destruction of physical or human capital, whilst the indirect effect deters investment and consumption through increased uncertainty or heightened perception of terror risk, rational or otherwise. Second-order effects such as greater costs incurred due to increased border security or terror's role as a catalyst for overseas armed intervention, whilst sizeable, are not considered.

The UK is a natural candidate for terror analysis as a victim of both extreme events such as the London 7/7 Bombings as well as a long history of attacks due to the Troubles. Furthermore, the diverse nature of terrorism in the UK, with attacks inspired by both Islamic extremism and secessionist movements, suggests my results are likely to be externally valid and can be applied across a number of western economies where social and political discourse concerning terrorism is increasingly prevalent. Finally, the GTD reports over 3000 terror attacks on UK soil since 1970 as well as more than 100 attack variables providing ample data with which to estimate terror responses. The high number of attacks

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[1] To name a few studies concerned with large attacks, see Abadie and Dermisi (2008); Sandler and Enders (2008) or Draca, Machin and Witt (2011). The full terror spectrum is studied in Abadie and Gardeazabal (2003); Brodeur (Forthcoming) and Enders and Sandler (1991).

observed, however, is both a blessing and a curse, particularly when estimating event heterogeneity, as many attacks have to be discarded for fear of overlap and contamination of results. I employ a range of methods to address this overlap problem however none are entirely satisfactory.

Throughout the paper as a proxy for terror responses I use daily equity index returns. This approach has a number of advantages over measuring a more direct economic variable of interest such as GDP or unemployment. Firstly, major UK indices are characterised by high liquidity and volumes and hence offer a clear, almost instantaneous identification of agent reactions to terror attacks. Secondly, index prices, and therefore returns, are a function of agents' beliefs about current and future cash flows and therefore capture any changes in both direct and indirect effects unlike a stock variable. Third, index returns encompass a broad range of firms within an economy and can be interpreted as a proxy for the rate of return of capital in an economy. Finally, index returns are increasingly being used as a saving technology by households with the rise of index tracking funds.

I use two methods to estimate index responses to attacks. First, I apply an event study methodology, which is commonly used in the finance literature[2], to identify the effect of an event on asset prices or returns. Event studies are particularly useful in this situation due to their ability to handle small N, large T situations common to financial time series and extreme event modelling. I estimate cumulative abnormal returns (CARs) under the constant mean return model to quantify the effects of the five largest terror attacks, measured by a weighted sum of injuries and fatalities, and find mixed evidence of a significant fall in index returns in four of the five cases but no significant fall on aggregate. Then, I adapt Chesney, Reshetar and Karaman (2011)'s conditional probability approach within a Bayesian hierarchical setting using MCMC sampling from the software programme stan and fail to conclude that any of the five attacks cause an extreme or abnormal movement in returns. Next, I apply the same methodologies to twenty of the UK's largest attacks, stratified by decade, and again fail to reject the null of significantly negative responses. Finally, I aggregate cumulative abnormal returns for every attack recorded in the dataset to find a terror attack cumulative average abnormal return (CAAR) which, again, is not significantly different from zero.

Estimating event heterogeneity produces broadly similar results. I regress event CARs and event day returns on covariates such as weapons used; event location; event target; number of killed or wounded; attack success and a range of other variables using a Laplace prior to perform Bayesian LASSO in order to overcome concerns of overfitting. Projection predictive variable selection (Piironen and Vehtari, 2017) suggests a number of variables of interest such as media intensity and number of injured provide the most predictive power when estimating event returns but my tests lack the power to conclude that these variables are differ-

[2] A 2007 estimate puts the number of published papers in finance using an event study methodology at over 565 (Kothari and Warner, 2007).

ent from zero using 90% credibility intervals. Finally, these variables of interest perform little better than a simple, intercept-only specification when I compare models using expected log predictive densities and leave-one-out cross-validation.

Overall, it seems that index returns are resilient to terror attacks and the effects of terror are negligible. Furthermore, there's little evidence to suggest that lack of significant negative responses on aggregate are driven by heterogeneity amongst terror attack characteristics.

I. Literature Review and Data

A. The Literature

The current terror literature is broadly split along two lines, a finance based approach that uses stock market returns as a dependent variable of interest and an approach more grounded in economic theory, using GDP, unemployment or tourism figures as a dependent variable. Both approaches find broadly similar results. On the whole, terrorism is bad but not all attacks are bad equally. For instance, the literature is almost unanimous in finding that 9/11 had a significant, negative effect on a number of variables of interest but the London 7/7 Bombings are followed by no statistically significant fall in equity returns after six days (Chesney, Reshetar and Karaman, 2011). Abadie and Dermisi (2008) find that in the aftermath of 9/11, commercial real estate vacancy rates in the 'shadow' of likely terror targets in Chicago rose significantly and conclude that agglomeration economies of scale and business activity in central business districts is extremely susceptible to perception of terror threat. Naturally, much of the literature is focused on the effect of 9/11, it was a shocking and tragic event with widespread repercussions, however I believe there is a real risk of confounding the effects of terrorism with a paradigm shifting, once in a lifetime event that undeniably altered the outlook and mentality of the world's largest economy and de facto superpower. In other words, if 9/11 was included in my dataset it would be an outlier and not a normal characterisation of terror, this in turn motivates my exploration of the less-explored, wider terror attack spectrum.

The most well-known paper exploring the consequences of terrorism in a developed economy is probably Abadie and Dermisi (2008)'s use of synthetic controls in the Basque county to quantify ETA's effect on local GDP. Abadie and Dermisi find that the onset of terror activity leads to a ten percentage point fall in local GDP relative to the synthetic control county. There are many similarities both in timing, objectives and modus operandi of ETA and the IRA campaigns of the 70s so it's plausible that we should observe a similar effect, albeit with stock returns rather than GDP, in my dataset. A potential flaw in the synthetic control approach employed by Abadie and Dermisi is contamination between treatment and (synthetic) control. For instance, the heighest weighted counties composing the synthetic control are Catalonia and Madrid, both of which were victims of

ETA attacks throughout the estimated period[3]. This makes the synthetic control method particularly problematic for my research question since there are very few countries that never experienced a terror attack between 1970 and present day and these few countries are unlikely to provide a good counterfactual for UK GDP/index returns in the absence of terrorism. Finally, whilst it's difficult to compare across contexts and using differing specifications, I find no evidence of a negative effect like Abadie and Dermisi do.

Another paper worth mentioning is Brodeur's forthcoming work in the American Economic Journal that uses the same dataset from the GTD to estimate the effects of terror attacks on employment, earnings and house prices using panel data on US states. Interestingly, Brodeur's results differ from my own, he finds evidence of a negative response even accounting for the entire terror distribution and not just catastrophic attacks. Furthermore, in Brodeur's sample the mean property damage from an attack is $750,000 which, whilst sizeable, suggests that the outsized effects on employment and house prices is a result of increased perception of terror risk rather than mechanical destruction of capital.

There is little previous work exploring heterogeneous aspects of terrorism apart from the loosely related work of Zussman and Zussman. In Zussman and Zussman (2006) the authors find evidence that markets respond differently to certain types of attacks. For example, counter terrorism assassinations of senior political figures lead to market falls whilst assassinations of senior military figures are associated with market gains in the context of Israeli counter terrorism operations against Palestinian terror organisations[4]. Unfortunately, I find little evidence of such an effect in the UK data although I don't distinguish between attacks at quite so granular a level as Zussman and Zussman.

Overall, this paper is set against a context which has often found evidence of negative consequences of terror attacks although there are also results that support this paper's findings of no negative effects in the UK, notably by Chesney, Reshetar and Karaman (2011) which is reassuring considering aspects of this paper build heavily on their approach. It is plausible that this result may be idiosyncratic to the UK due to the large number of attacks and long history of terror on UK soil. Perhaps British households accurately calculate terror risk thanks to long exposure to militant Irish Fenianism, starting as far back as the 1880s (Kelly, 2006). Alternatively, British counter-terror strategy and counter-terror forces are often considered world leading[5] and therefore the actual effect of terror such as capital destruction may be severely inhibited by the UK's counter-terror policies.

[3]Madrid in December 20, 1973 and Catalonia in June 19, 1987.

[4]Whether these assassinations meet the criteria of the GTD are left to the reader.

[5]For instance, the UK's success in counter-terrorism strategy employed in the Malayan Emergency (running from 1948 to 1960) is often cited as a textbook example of successful counter-terrorism strategy Jackson (2008).

B. The Data

The GTD records over 3000 attacks occurring in the UK since 1970 and includes both successful and unsuccessful attempts at terrorism. In accordance with the idea of indirect terror costs I include all events, successful and otherwise, because it's plausible that thwarted attacks still raise perceived terror risk amongst households and should lead to stock market adjustments if these unsuccessful attacks aren't already incorporated in agent information.Table 1 shows summary statistics of a few terror attack variables recorded in the GTD. Attacks are extremely diverse and attack magnitude measured along a number of metrics are characterised by very skewed distributions with fat right tails. For instance, Figure 1 shows the distribution of wounded in an attack given at least one victim is injured and omits attacks with more than 100 wounded, in reality the empirical distribution has an even fatter tail.

Table 1—Some Terror Summary Statistics

Statistic	Fatalities	Wounded	Ransom Amount	No. Perpetrators
Minimum	0	0	0	1
1st Quartile	0.00	0.00	0	1.00
Median	1.00	0.00	0	1.00
Mean	0.97	1.66	1366	1.89
3rd Quartile	1.00	0.00	0	2.00
Maximum	270	784	2200000	700

Note: Ransom Amount only recorded in hostage/kidnapping situations. Data from 1970-2016 in the UK only.

I make a few adjustments to the data. The first is moving the timing of events that occur on the weekends to the following Monday. This transformation is necessary because market data is only available for weekdays, in these cases instead of interpreting responses as an event day return, responses are interpreted as market returns on the first trading day where the terror attack is included in agents' information set. It's also necessary to assume that terrorists don't select into weekend attacks and pre-market trading doesn't materially affect returns. Secondly, I aggregate attacks at the day level, if multiple attacks occur on the same day I sum injuries, fatalities and use data from both events to record event characteristics. For instance, if two attacks occur on the same day where each attack leads to three injuries, the first attack involves a bomb and the second a knife, the data for that day will show six injuries, two incidents and a knife and bomb attack present. Whilst aggregating attacks leads to loss of information it would be impossible to undertake this analysis without doing so since attack time is only recorded at the day level within the dataset for the vast majority of attacks.

Throughout the paper I use a simple heuristic to calculate how large an attack is:

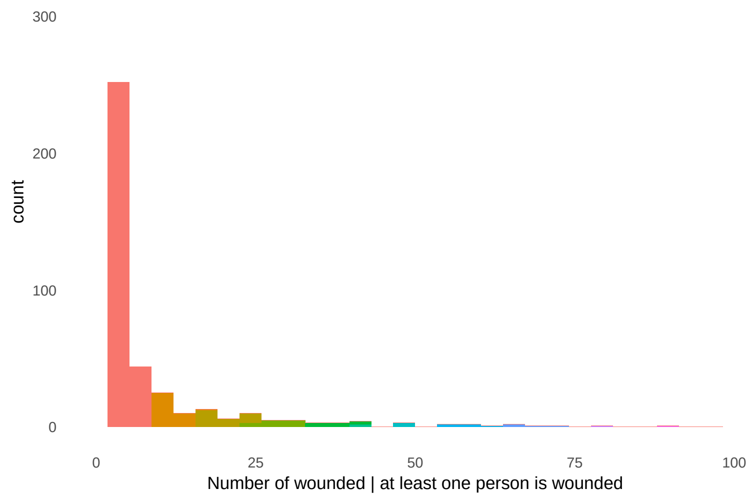

FIGURE 1. HISTOGRAM OF ATTACK WOUNDED GIVEN AT LEAST ONE INJURY IS RECORDED. ATTACKS WITH GREATER THAN 100 WOUNDED ARE OMITTED TO IMPROVE VISUAL CLARITY.

'terror intensity' is a weighted sum of injuries and fatalities[6]. Clearly if households judge the intensity of attacks using some other metric then the results from the largest five and twenty attacks will be misleading. However, overall I think this is a reasonable proxy to use for event magnitude and this distinction is only made for the initial analysis involving the largest five and largest twenty attacks.

Finally, the GTD lost all terror records for the year 1993. Since this data is essentially missing at random (the index cards holding the data went missing prior to digitisation of the GTD) any results shouldn't be biased by this. I don't manually add events as this would introduce sample selection - there are far more records of the Bishopsgate Bombing than the many small incidents that this paper focuses on.

II. Model

Equity index returns are a broad proxy for the returns on a risky asset in an economy and therefore can be considered of interest to economists for a variety of reasons. In this section I explicitly motivate the use of index returns as a variable of interest using a simple asset pricing model. Consider a world where household expected utility is:

$$U = u(C_t) + \beta E[u(C_{t+1})]$$

[6]Fatalities have a weight of 3, each distinct incident a weight of 1 and injuries a weight of 0.5. This broadly follows the methodology of the Global Terrorism Index 2017

where C_t is consumption satisfying the standard conditions, $u'(C) > 0, u''(C) < 0$, and β is the discount factor. Households receive real income Y_t and pay lump-sum taxes T_t to fund government spending G_t. Households can purchase two types of asset, a risk free real bond with payoff one next period and a risky asset (the equity index analogue) with uncertain nominal payoff next period $Z_{t+1} = X_{t+1} - \eta_{t+1}$, where X_{t+1} is some random variable determining pay-off associated with exogenous economic fundamentals and η_{t+1} a terror shock. The risky asset has current period price V_t and the household budget identities are:

$$C_t + \frac{V_t}{P_t} s_t + q_t b_{h,t} = Y_t - T_t, \quad C_{t+1} = Y_{t+1} - T_{t+1} + \frac{Z_t}{P_{t+1}} s_t + b_{h,t}$$

where q_t is the price of the risk-free real bond, $b_{h,t}$ the quantity of bonds held by households at period t and s_t the quantity of the risky asset the household chooses to buy.[7]

Substituting into the utility function gives:

$$U = u(Y_t - T_t - \frac{V_t}{P_t} s_t - q_t b_{h,t}) + \beta E_t[u(Y_{t+1} - T_{t+1} + \frac{Z_t}{P_{t+1}} s_t + b_{h,t})]$$

First order conditions with respect to optimal risky asset holdings, s_t:

$$-\frac{V_t}{P_t} u'(C_t) + \beta E_t \left[\frac{Z_t}{P_{t+1}} u'(C_{t+1}) \right] = 0$$

Therefore, equilibrium price of risky asset, V_t:

$$V_t = \beta E_t \left[\frac{u'(Y_{t+1} - G_{t+1})}{u'(Y_t - G_t)} \frac{Z_{t+1} + V_{t+1}}{1 + \pi_{t+1}} \right] \tag{1}$$

If we define the real return of holding the risky asset ex-post as[8]:

$$1 + r_{t+1} = \frac{Z_{t+1} + V_{t+1}}{(1 + \pi_{t+1}) V_t}$$

and substitute into equation (1):

$$\frac{1}{\beta} = E_t \left[\frac{u'(Y_{t+1} - G_{t+1})}{u'(Y_t - G_t)} (1 + r_{t+1}) \right] \tag{2}$$

[7]The government has a budget constraint:

$$G_t + q_t b_{g,t} = T_t, \quad G_{t+1} = T_{t+1} + b_{g,t}$$

where $b_{g,t}$ corresponds to government holdings of bonds or issuance of debt.

[8]i.e. the dividend plus the change in the asset price in real terms.

Equation (2) shows that the discount factor β is determined by households' expectations of equity index real returns. If households are acting rationally they will incorporate terror risk into their assessments of expected real returns and we wouldn't expect on aggregate terror attacks to significantly move markets, attacks are already 'priced in'. If, however, agents don't successfully incorporate terror attacks into their expectations of future firm dividends and capital stock, or agents systematically underestimate the likelihood of terror threats due to heuristic biases and difficulties in estimating tail risk, we'd expect terror attacks, on average, to be followed by a fall in returns as households update their expectations or re-assess the terror threat.

Therefore, significant evidence of negative real returns following attacks will suggest that households aren't rationally incorporating all terror knowledge into their decision making process and the discount factor β should be higher than it actually is. This incorrect β implies that households are not optimally inter-temporally smoothing consumption and there would be welfare gains from improving understanding of terror risk or eliminating terror risk entirely.

In this set-up we abstract away from capital destruction of V_{t+1} and only consider terror destruction of future dividends Z_{t+1}, however it makes little material difference as both would enter the expectation function linearly. We also abstract away from the idea of direct and indirect costs of terror but I believe this is no great loss as these costs cannot be separately estimated in the data. The advantage of such a simple model is that it presents a clear channel through which negative index returns following an attack transmit to welfare losses in an economy both through mechanical destruction of dividends $Z_{t+1} = X_{t+1} - \eta_{t+1}$ and household uncertainty surrounding terror expectations leading to sub-optimal resource allocation.

III. Methods and Results

A. *Event Study*

I use a standard event study approach, outlined by MacKinlay (1997) to ascertain whether attacks are followed by periods of significant negative returns. Rather than use returns following a terror attack directly, it's common to estimate cumulative abnormal returns following an event. Cumulative abnormal returns are considered a better estimator of an event's effect on returns as they effectively de-mean returns to account for prevailing market sentiment in the lead up to an attack. The event study involves first calculating abnormal returns at time τ, where τ is defined in relation to event time so that $\tau = 0$ indicates the day of the attack, for event j to give cumulative abnormal returns:

$$AR_{j,\tau} = R_{j,\tau} - E[R_{j,\tau}|\Omega_{j,\tau}]$$

$$CAR_{j(\tau_1,\tau_2)} = \sum_{t=\tau_1}^{\tau_2} AR_{j,t}$$

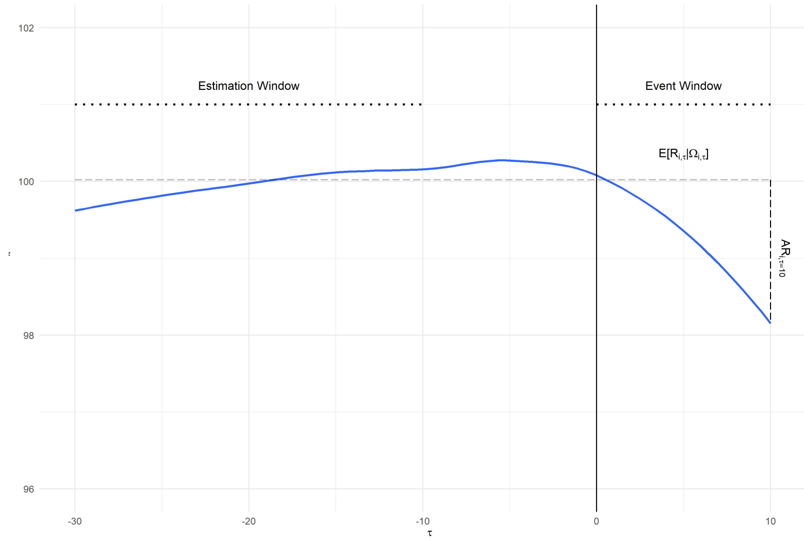

FIGURE 2. DEMONSTRATION OF AN EVENT STUDY TO CALCULATE ABNORMAL RETURNS.

where $\tau_1 = 0$ and $\tau_2 = 10$, indicating an event window of eleven days[9], and $E[R_{j,\tau}|\Omega_{j,\tau}]$ is the expected index return derived from the constant mean return model conditioning on information, $\Omega_{j,\tau}$, common to all agents. The constant mean return model can be written as:

$$R_{jt} = \mu_j + \psi_{jt}$$
$$E[\psi_{jt}] = 0, \ \mathrm{var}[\psi_{jt}] = \sigma^2_{\psi_{jt}}$$

where μ_j is some constant and ψ_{jt} white noise i.e. index prices follow a random walk with drift[10]. Whilst the constant mean return model is the most simple [11], it has been shown by Brown and Warner (1980) to perform similar to more advanced methods and has the advantage of being well defined for indices in addition to individual securities which would be problematic under a CAPM approach. To estimate constant mean returns I use a twenty day estimation window that ends ten days before the terror attack. Unfortunately, there is a trade-off unique to event studies between T and N; a longer estimation window, i.e. greater T, results in less events being included in the experiment as there is greater risk of event overlap i.e. another event ocurring during the estimation (or event) window. This problem becomes particularly acute when studying many thousands of small

[9]There is a little friction here between the model and reality since it's unlikely that households access equity markets to help smooth consumption over 10 day periods.
[10]From log differencing prices P_{jt} to get returns R_{jt}.
[11]Compared to more advanced methods such as the market model which incorporates CAPM or multi-factor models building on Fama and French (1993)'s three factor model.

terror attacks rather than one catastrophic event [12]. To overcome this problem I report both screened and overlapping results where appropriate and perform robustness checks using a constant median return model which should be less sensitive to potential overlap issues. After calculating CARs for each event I

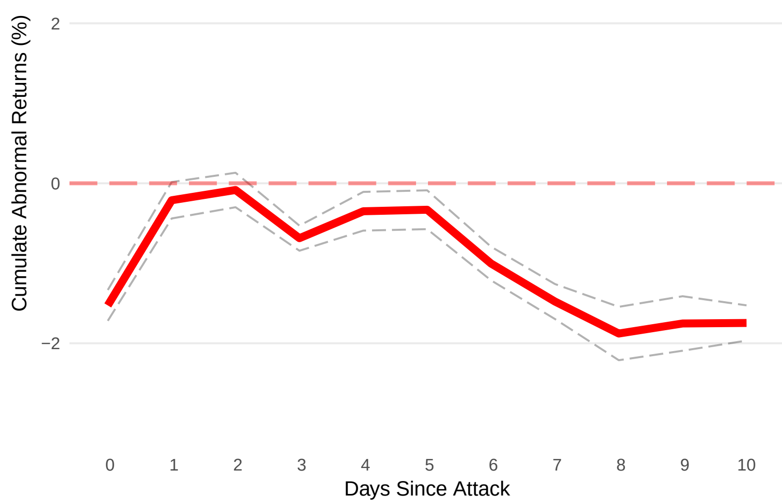

FIGURE 3. LONDON 7/7 BOMBINGS - CARS: FTSE ALLSHARE PRICE INDEX

aggregate estimates into cumulative average abnormal returns (CAARs):

$$CAAR_{(\tau_1,\tau_2)} = \frac{1}{N}\sum_{j=1}^{N} CAR_{j(\tau_1,\tau_2)}$$

it is these CAARs that form our estimators of interest since they rely on fewer identifying assumptions than the CARs they are derived from and CAARs relate directly to agents' terror expectations as described in section II.

For CAARs and CARs to admit a causal interpretation I rely on the following assumption: $E[attack_j|\Omega_{j\tau}] = 0$. That is, terror attacks are realised orthogonal to any current and past information that influences equity indices. Intuitively, if terror groups plan attacks based off economic observables likely to move markets we cannot disentangle the effect of terror attacks from these confounders. This seems like a plausible assumption, it is unlikely that terror groups would plan attacks based off macroeconomic news likely to influence equity indices and if this

[12]Particularly if attacks are planned as part of a terror campaign over the course of multiple days.

were the case presumably governments would incorporate the same information to inform counter-terror strategy, something of which there is no evidence[13]. However, this condition also means that terror attacks must occur independently[14]. If attacks do not occur independently then timing and magnitude of attacks will not be exogenous and there will be confounders present, since I don't use any control variables such as heightened police presence or greater counter-terror spending after an attack this could be particularly problematic. However, if an attack occurs and households surmise that this attack influences other, related attacks e.g. terrorists changing attack methods due to a crackdown on air travel, then this information will enter $\Omega_{j\tau}$ when $E[R_{j,\tau}|\Omega_{j,\tau}]$ is next estimated since each event re-estimates $E[R_{j,\tau}]$ using a new estimation window and events will be independent, conditioning on this new information set. Finally, non-independent attacks also create a problem when I screen for overlapping attacks. For example, any events that are planned to occur in close proximity as part of a terror campaign will be systematically removed from the dataset - events are not dropped at random.

Finally, for CARs to be interpreted causally I also need to make use of a tweaked parallel trends assumption where the estimated constant mean return $E[R_{j,\tau}|\Omega_{j,\tau}] = \bar{\mu}$ is identical for event j's estimation window (which occurs by construction) *and* event window. Essentially $E[R_{j,\tau}|\Omega_{j,\tau}]$ needs to be a good counterfactual for stock market returns in the absence of a terror attack. However, our estimator of interest, CAARs, can relax this assumption by exploiting the law of large numbers[15]and rely instead on any deviations from the parallel trends assumption to be random and hence wash out during aggregation.

Event Study Results - Five Largest Events

Table 2 shows the cumulative average abnormal returns using the five largest attacks witnessed in the UK from 1980 till present day. Day 0 refers to the day of the attack and therefore although day 10 is the eleventh CAAR observation it indicates 10 days have passed since the attack. My preferred CAARs of interest are the 4-day and 10-day CAARs. I believe the 4-day CAAR is a short enough time span to fully reflect market movements after information about an attack has disseminated, since 0- or 1-day CAARs may suffer from information lags or simply incorrect information being spread shortly in the immediate aftermath of an attack and 10-day CAARs indicate reactions after a longer timeframe whilst still being relatively close to an attack. Due to the nature of market trading days, a 10-day CAAR effectively reflects two weeks of trading after the attack. It seems plausible that the 4-day CAAR will be better at identifying indirect effects

[13]For example, Staniforth (2013)'s counter-terrorism handbook makes no mention of such a relationship.

[14]I attempted to relax this assumption in the conditional probability approach by stacking observations and estimating an event covariance matrix using seemingly unrelated regression and a linear probability model but the MCMC chains suffered from lack of convergence and divergent transitions indicating there was too little information to accurately estimate the model.

[15]Whether five or twenty events are enough for this is debatable.

following the immediate hysteria surrounding an attack and the 10-day CAAR a more measured response that reflects beliefs about loss of capital etc. however I propose no formal model or structure to assess this. Overall, estimated CAARs are negative indicating large terror attacks on average cause investors to lower expectations of current and future cash flow, or at the very least, current and future dividends or share buybacks. The 10-day CAAR of -0.97 indicates that after an attack similar to the five used in our event study, we'd expect abnormal returns of -0.97, that is, returns 0.97 lower than what we'd otherwise expect on average[16]. However, neither the 4- nor 10-day CAAR results are significantly different from 0 at any reasonable significance level using traditional p-values and 0 lies within the bootstrapped 95% confidence intervals for every CAAR estimate.

[16]This definition comes from using the constant mean return model. If, for instance, I'd used the market model and incorporated CAPM we'd conclude that returns will be 0.97 lower than their expected return using the asset's β and the risk-free rate.

Table 2—Cumulative Average Abnormal Returns for the five largest attacks since 1980. FTSE Allshare.

Day	$\widehat{CAAR}$	T	p-value
0	-0.45	-1.68	0.17
	(-1.53, 0.14)		
1	0.22	0.36	0.74
	(-1.63, 2.84)		
2	0.15	0.16	0.88
	(-3.31, 3.64)		
3	-0.24	-0.23	0.83
	(-4.13, 3.37)		
4	-0.50	-0.82	0.46
	(-0.71, 1.05)		
5	-0.20	-0.23	0.83
	(-0.98, 2.04)		
6	0.42	0.45	0.68
	(-1.85, 3.30)		
7	-0.22	-0.26	0.81
	(-1.48, 2.95)		
8	-0.90	-1.13	0.32
	(-1.88, 3.18)		
9	-1.20	-1.45	0.22
	(-1.75, 2.83)		
10	-0.97	-1.04	0.36
	(-1.75, 3.64)		

Note: 95% Confidence intervals calculated using bias-corrected and accelerated bootstrap interval with 10,000 replications. Results from robustness checks using other indices can be found in Appendix Table A6. Events are screened for overlap within the five largest attacks. For the five largest attacks I use 1980 as the earliest time period as this allows me to use a greater number of robustness checks as many modern indices didn't exist in the 70s. Individual CARs for each event can be found in the Appendix and in Figure III.A.

Event Study Results - Largest Twenty Events

Table 3 shows the same results but now including the twenty largest attacks since 1970. More observations should lead to more precise estimates, however there's now a trade-off where the results don't identify the effects of particularly extreme attacks. For example, the mean number of fatalities from the attacks drops from 74.2 to 26.6 when moving from the largest five to largest twenty events. Again, we find no evidence of significant, negative responses to terror attacks and in fact the confidence intervals are wider than in the five event case. I believe this loss of precision comes from including attacks with very different characteristics as we move from five to twenty events. Although 20 observations make up a very small fraction of the dataset there are clearly large differences between the twentieth and first, fifth or even tenth largest attack, more details of which can be seen in the appendix.

Although individual CARs such as those shown in Figure 2 suggest there may be some evidence of a negative terror response. On aggregate these terror responses do not seem to be statistically significant, at least for the twenty events considered so far. Furthermore, there's no evidence of households learning from terror attacks over time, there are as many attacks insignificant in the 1980s as there are in the 2010s. This suggests that either households have already learnt about terror attacks and accurately set their expectations accordingly before the sampled data begins or the effect of terror attacks are negligible on aggregate in an economy. One of the drawbacks of the event study is that it's highly parametric, results can be influenced by a number of hyperparameters such as the size of the event window; the size and location of the estimation window and the model used to calculate abnormal returns as well as relying on a number of assumptions that may not necessarily hold in the dataset such as event independence. The following sections try to overcome these concerns with an alternative approach that is less susceptible to arbitrary parametrisation and fully reflects our uncertainty concerning the best hyperparameter choice.

Table 3—Cumulative Average Abnormal Returns for the twenty largest attacks since 1970. FTSE Allshare.

Day	$\widehat{\text{CAAR}}$	T	p-value
0	0.02	0.06	0.96
	(-2.51, 5.88)		
1	0.11	0.23	0.82
	(-4.01, 6.74)		
2	0.40	0.61	0.55
	(-7.11, 7.42)		
3	1.00	1.29	0.21
	(-4.13, 9.17)		
4	1.14	1.48	0.16
	(-3.51, 9.51)		
5	1.09	1.26	0.22
	(-3.34, 11.28)		
6	1.13	1.26	0.22
	(-1.85, 11.16)		
7	1.07	1.14	0.27
	(-2.48, 12.50)		
8	0.88	0.92	0.37
	(-2.50, 12.31)		
9	0.83	0.83	0.42
	(-3.20, 12.85)		
10	1.10	0.96	0.35
	(-2.21, 15.49)		

Note: 95% Confidence intervals calculated using bias-corrected and accelerated bootstrap interval with 10,000 replications. Results from robustness checks using other indices can be found in Appendix Table A7. Events are screened for overlap within the twenty five largest attacks. For the twenty largest attacks I use fewer robustness checks and hence measure from 1970 to include as many large attacks as possible.

B. *Conditional Probability*

An alternative specification to the event study identification strategy is outlined by Chesney, Reshetar and Karaman (2011)[17]. It involves specifying a conditional probability distribution, $\pi(z|x) \equiv P(Z_i \leq z|X_i = x)$ where Z_i are index returns and X_i represents a lagged vector of Z_i's. If $y_i = I(Z_i \leq z)$, where I denotes the indicator function, then $E[y_i|X_i = x] = \pi(z|x)$ and a regression of y_i on X_i will give the probability, conditional on the value of lagged returns, of observing $Z_i \leq z$. Applying this to terror attacks gives:

$$y_{jt} = I(R_{jt} \leq r_{j,terror})$$

[17]Occasionally referred to as CRK henceforth.

where R_{jt} are index returns for attack j at time t and $r_{j,terror}$ is the observed event day terror return. Chesney, Reshetar and Karaman set X_{jt} equal to $R_{j,t-1} - r_{j,pre\_terror}$[18] where r_{j,pre_terror} is the return observed the day *before* the attack. Then, regression equation (3) is estimated using a local polynomial regression and observations from two hundred days before the attack.[19]

$$y_{jt} = \alpha + \beta(R_{j,t-1} - r_{j,pre\_terror}) + u_{jt} \tag{3}$$

Using the (out-of-sample) return actually observed on the day of the attack gives our estimator of interest, a fitted value that can be interpreted as the conditional probability of observing a return as bad or worse than the return empirically observed. Chesney, Reshetar and Karaman define an event as an abnormal terror attack if this fitted value is less than 10% and an extreme attack at less than 5%.

I modify their approach in two main ways. Firstly, instead of using local polynomial regression I use logistic regression which I believe is more sensible in a binary dependent variable setting. Secondly, I apply this within a Bayesian hierarchical model. The hierarchical model has two main advantages over CRK's approach. First, it overcomes a major flaw in CRK's methodology where by construction $y_{jt} = I(R_{jt} \leq r_{j,terror})$ is almost guaranteed to exhibit separation - y_{jt} is set to 0 for every observation - due to the extreme nature of some terror attacks, $r_{j,terror}$ is often negative and very large in magnitude[20]. A simple Bayesian logistic regression would solve this separation problem (Gelman et al., 2008) which would otherwise be intractable under a frequentist approach without pooling multiple attacks. However, another advantage of hierarchical models is that I can exploit the observation of multiple terror attacks and share information across attacks to better inform estimates of α and β.

C. Hierarchical Logistic Model

If each terror attack j is drawn from some underlying population parameter, Θ, then we observe attacks with corresponding likelihood function $p(attack_j|\theta_j)$ where $attack_j$ is a 200×1 vector of y_{jt} as described in (3). Therefore, equation (3) can be written as likelihood function:

$$y_{jt} \sim \text{Bernoulli}(G(\alpha_j + \beta_j(R_{j,t-1} - r_{j,pre\_terror}))) \tag{4}$$

Here, $G(\cdot)$ is the logistic link function and the Bernoulli distribution is used as the binary dependent variable can be regarded as the outcome from a Bernoulli trial. Both α and β are indexed by event j indicating a varying-slope, varying-intercept

[18] i.e. lagged index values minus the return the day before the attack.

[19] It's unclear from both the paper and online appendix whether CRK perform this separately for each attack or pool attacks. Separate estimation makes more sense in terms of interpretation however pooling overcomes the problem of separation discussed shortly.

[20] Both 9/11 and 7/7 suffer from this problem for instance.

model. For weakly informative priors and logistic regression, Gelman et al. (2008) recommend a student's t distribution, $\alpha_j, \beta_j \sim t(1, 0, 2.5)$[21], however Ghosh, Li and Mitra (2015) find that in the case of logistic separation $\nu = 3$ degrees of freedom performs better. Incorporating this advice leads to priors:

$$\alpha_j \sim t(\nu = 3, \gamma, \sigma_\alpha) \tag{5}$$

$$\beta_j \sim t(\nu = 3, \mu, \sigma_\beta) \tag{6}$$

where $\gamma, \mu = 0$ and $\sigma = 2.5$. However, we can encode our uncertainty about what priors to set in (5) and (6) by specifying hyperpriors which involves setting prior distributions for γ, μ and σ:

$$\gamma \sim N(0, 1), \quad \sigma_\alpha \sim \text{Cauchy}^+(0, 5) \tag{7}$$

$$\mu \sim N(0, 1), \quad \sigma_\beta \sim \text{Cauchy}^+(0, 5) \tag{8}$$

The location and scale chosen in (7) and (8) are intended to be weakly informative[22] and also encompass the advised prior recommendations of Gelman et al.; Ghosh, Li and Mitra. A half-Cauchy distribution with scale five reflects a reasonably diffuse prior with a fat tail and is a common hyperprior for scale paramaters in hierarchical models (Gelman, 2006)[23]. A standard normal distribution is a generic weak prior, although substantially stronger than the half-Cauchy prior, and reflects the fact that both Gelman et al.; Ghosh, Li and Mitra recommend a location parameter of zero for γ, μ.

Whilst the model outlined above appears more complex than the standard frequentist approach seen in equation (3), it has a number of advantages. If I were to estimate regression (3) separately, each terror attack would produce different estimates of α and β either due to underlying event heterogeneity or noise in the data. Furthermore, the estimates would be less precise as we're losing information by only using 200 observations for each estimate. Rather than completely pool the data and destroy this heterogeneity, the hierarchical model partially pools the data to estimate α and β depending on the hierarchical variance, σ_α and σ_β respectively. If events produce identical estimates there's no evidence of heterogeneity across attacks, $\sigma_{\alpha,\beta} = 0$, and the model is identical to a completely pooled model - we 'shrink' estimates towards an average effect common to all events. However, if there is event heterogeneity present estimates aren't shrunk

[21]i.e. a Cauchy distribution

[22]It is common to choose weakly informative priors in settings where the researcher has no previous knowledge to draw upon or wishes to increase the burden of proof the experiment faces. I choose not to use informative priors based off results from previous literature since there is relatively little previous research characterising terror responses outside of maybe one or two events in my dataset such as London 7/7. Whilst incorporating CRK's results in an informative prior distribution makes sense given my approach is adapted from theirs originally, I have some misgivings about their methodology vis-a-vis separation and local polynomial regression. Overall, I believe it's better to err on the side of caution and set weak priors.

[23]In fact Gelman recommends a significantly weaker scale parameter for the half-Cauchy of 25 but I fear this is too weak for my purposes (Stan Development Team, 2017).

as much and allowed to vary with attack. In short, the hierarchical model has the advantage of sharing information across attacks to improve estimate precision. This occurs by shrinking estimates towards a population average and this shrinkage varies inversely with event heterogeneity, $\sigma_{\alpha,\beta}$.

Conditional Probability Results - Five Largest Events

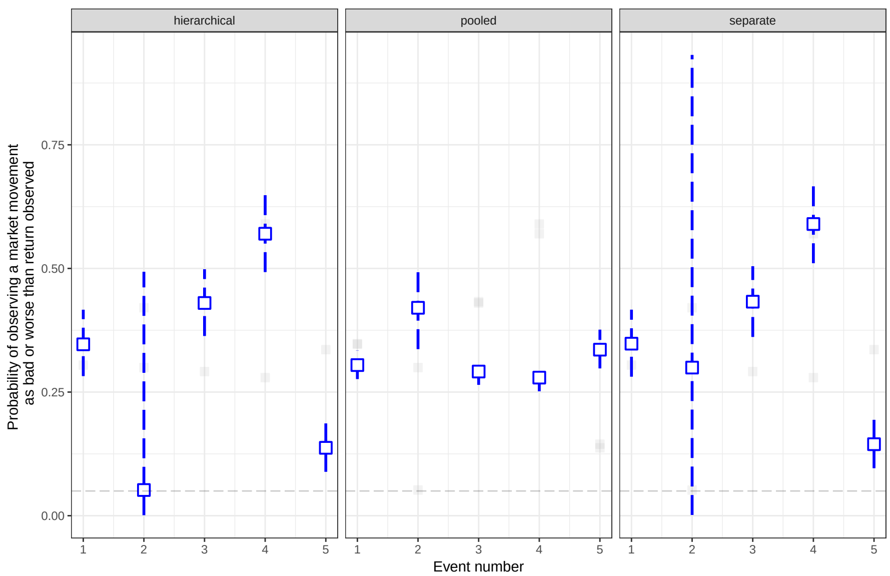

Figure 4. Conditional Probability Results for five largest terror attacks in the UK from 1980 to today. Horizontal dashed line indicates $P(X < x) = 0.05$.

Figure 4 shows the posterior distributions of the fitted values for the five largest[24], measured in terms of injuries and fatalities, terror attacks on UK soil from 1980 to present day using three different specifications: the full Bayesian hierarchical model discussed above; a pooled Bayesian OLS with the default priors described in (3) and (4) and separate OLS, again with default priors. Rather than display the fully characterised posterior distribution I show the posterior mean and 90% credibility intervals for the fitted values.

The advantages of the hierarchical model are clearly apparent when comparing plots. The pooled model confines the fitted values between 0.25 and 0.5 which suggests the probability of observing a more extreme return on the day of the attack is broadly similar across events. Whilst the fitted values are less than 50% they are still far from the definition of an abnormal or extreme attack as laid out

[24]The Lockerbie Bombing in 1988; London 7/7 in 2005; the Omagh Car bombing in 1998; the 1996 Manchester Bombing and the 1982 Droppin Well Disco Bombing respectively.

by CRK. The pooled results are so similar since the only source of variation in the fitted values comes from observing different event day returns for each attack. The separate model clearly displays more dispersed results at the cost of greater posterior uncertainty. Event two is the London 7/7 Bombings which suffered from separation and so should be interpreted with caution - the large posterior uncertainty probably reflects prior choice rather than any information imparted by the data - again, no events can be deemed abnormal or extreme by CRK's criteria.

Finally the hierarchical results, these aren't as tightly constrained as the separate model and posterior uncertainty is reduced. Greater posterior certainty is particularly apparent in the London 7/7 estimate, whereas the estimate under the separate specification could lie in almost the entire probability interval it's now restricted to below 50%. Unfortunately, although the London 7/7 posterior mean estimate of 0.0519 or 5.2% passes the CRK criteria for an abnormal event there is insufficient posterior certainty to definitively conclude that the parameter lies beneath the bound of 10%.[25]

Table 4—Hierarchical Variance of Event Estimates

Parameter	mean	sd	2.5%	50%	97.5%
$\hat{\sigma}_\alpha$	3.4	1.4	1.6	3.1	6.9
$\hat{\sigma}_\beta$	0.2	0.3	0.0	0.1	1.1

Note: I use $\hat{\sigma}_\alpha$ instead of σ_α to differentiate between a variable that has been estimated from the model and a prior distribution.

Table 4 displays the estimates of event hierarchical variance as well as posterior distribution quantiles. The intercept term, $\hat{\sigma}_\alpha$, has posterior mean 3.4 whilst the hierarchical variance of the slope, $\hat{\sigma}_\beta$ has mean 0.2. This suggests that the effect of each attack is broadly similar but the context in which each attack is placed, determined by the constant term, varies considerably as market conditions change[26]. Overall, the parameters suggests that shrinkage has occurred for each slope estimate but not as much for each individual intercept. We see this in the plots, the hierarchical model estimates are closer to the separate than pooled specification. In all further analysis using the conditional probability approach I only present results from the hierarchical model, theoretically it offers large advantages over the alternative specifications and as we have seen these are borne out in results of the parameter estimates.

[25]For the full regression table see appendix Table A3.

[26]For instance, consider the case where the index return on the day of the attack is zero. In this case a large constant term indicates particularly bearish conditions for the two hundred days preceding the attack as the probability of observing a return as bad or worse than zero is considered quite likely. A large hierarchical variance for the constant estimates suggests market conditions vary between bullish and bearish frequently across events.

Conditional Probability Results - Twenty Largest Events

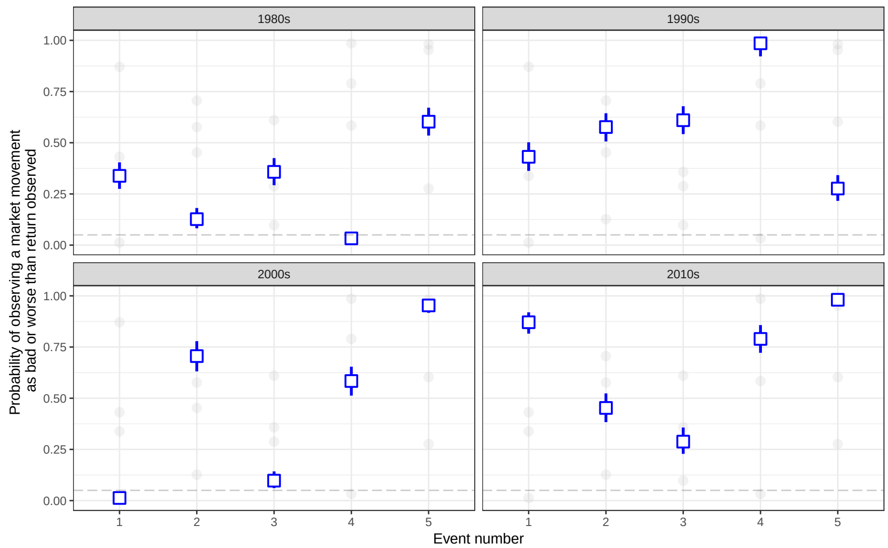

Figure 5. Conditional Probability of observing more extreme market return on day of attack for 20 largest attacks, stratified by decade. Horizontal dashed line indicates $P(X < x) = 0.05$.

Figure 5 and Table 5 display the fitted value posterior means and 90%/95% credibility intervals, respectively, for the five largest attacks to occur in each decade. The increase in estimate precision, moving from five events to twenty, is stark. The greater power of the model means we can now conclude with 95% certainty that the London 7/7 Bombings ($\hat{p}_{2000s,1}$) was a terror attack that led to an extreme return response. We also find that the fourth largest attack in the 1980s qualifies according to the CRK criteria as an abnormal event. However, overall evidence for a negative terror response from the conditional probability approach seems unconvincing. Of the twenty largest terror attacks in the UK in the past 40 years only two seem to move markets significantly. It seems either the heuristic used to determine whether an event is 'large' fails to capture the dimension along which terror attacks affect agents or it seems there is no evidence of a negative effect on aggregate. Whilst inspecting credibility intervals and applying CRK's criteria to each terror attack's fitted value tests whether an individual event can be considered abnormal or extreme, I don't formally jointly test all twenty events as a cursory inspection of Figure 5 shows that $Pr(P)$ where $P = Pr((\hat{p}_{1980s,1} \leq 0.1) \cap (\hat{p}_{1980s,2} \leq 0.1) \cap (\hat{p}_{1980s,3} \leq 0.1) \cap \dots)$ will be trivially close to zero.

Table 5—Fitted value Posterior Means and 95% credibility intervals for twenty largest attacks.

Parameter	mean	sd	2.5%	50%	97.5%
$\hat{p}_{1980s,1}$	0.338	0.034	0.275	0.337	0.404
$\hat{p}_{1980s,2}$	0.127	0.025	0.082	0.126	0.181
$\hat{p}_{1980s,3}$	0.358	0.034	0.293	0.357	0.425
$\hat{p}_{1980s,4}$	0.033	0.013	0.012	0.031	0.064
$\hat{p}_{1980s,5}$	0.603	0.035	0.535	0.603	0.671
$\hat{p}_{1990s,1}$	0.431	0.035	0.363	0.430	0.502
$\hat{p}_{1990s,2}$	0.577	0.036	0.506	0.578	0.644
$\hat{p}_{1990s,3}$	0.610	0.035	0.542	0.610	0.678
$\hat{p}_{1990s,4}$	0.986	0.057	0.922	0.996	1.000
$\hat{p}_{1990s,5}$	0.276	0.032	0.216	0.276	0.342
$\hat{p}_{2000s,1}$	0.013	0.018	0.001	0.009	0.042
$\hat{p}_{2000s,2}$	0.706	0.038	0.631	0.706	0.779
$\hat{p}_{2000s,3}$	0.098	0.021	0.061	0.096	0.143
$\hat{p}_{2000s,4}$	0.584	0.036	0.513	0.584	0.654
$\hat{p}_{2000s,5}$	0.953	0.016	0.917	0.955	0.979
$\hat{p}_{2010s,1}$	0.871	0.026	0.815	0.872	0.919
$\hat{p}_{2010s,2}$	0.453	0.035	0.383	0.452	0.523
$\hat{p}_{2010s,3}$	0.288	0.033	0.228	0.287	0.357
$\hat{p}_{2010s,4}$	0.790	0.034	0.722	0.789	0.857
$\hat{p}_{2010s,5}$	0.981	0.010	0.955	0.983	0.995
$\hat{\sigma}_\alpha$	2.666	0.524	1.871	2.587	3.931
$\hat{\sigma}_\beta$	0.076	0.041	0.014	0.069	0.172

Note: $\hat{p}_{1980s,1}$ corresponds to $P(r \leq r_{terror}|r_{pre\_terror})$ for the largest attack observed in the 1980s.

D. *Terror Cumulative Average Abnormal Returns*

In this section I turn my attention to the effects of the entire terror spectrum. I present three results, the first is the four-day and ten-day cumulative average abnormal return from every event in the dataset. The second result shows the effect of every attack that *doesn't* overlap and the final result shows *only* overlapping attacks. Table 6 shows that in the non-overlapping case, on average cumulative abnormal returns after a terror attack are positive with a cumulative abnormal return after four days of 0.36, falling to 0.26 after ten days. The only statistically significant results, the ten-day estimates using the overlapping attacks and every attack, are statistically significantly different from 0 at the 10% level however finance data is often characterised by fat tails and it's likely that this is exacerbated by extreme events, such as terror attacks, which makes using traditional t-tests problematic. Therefore, rather than a t-test, my preferred test is the 95%

bootstrapped confidence intervals which fail to find any of the CAAR parameters statistically significant. The bootstrapped confidence intervals are very wide indicating a great deal of uncertainty about the estimates. Whilst it's plausible that this uncertainty is due to the large heterogeneity present in the data, I find little evidence of heterogeneous effects driving CAAR responses later in the paper[27]. Indeed, in Figure 6 we can see that the initial largest attacks recorded have negative CAARs, followed by the next ten leading to broadly positive CAARs after which CAARs grow closer to 0 suggesting no significant effect.

Table 6—Terror Attack Cumulative Average Abnormal Returns from 1970-2016 in the UK.

Parameter	$\widehat{CAAR}$	Lower 95%	Upper 95%	T	p-value	N
10-day all	-0.15	-1.30	33.23	-1.76	0.08	3448
10-day no overlap	0.26	-17.12	2.71	0.59	0.56	105
10-day overlap	-0.17	-1.95	31.80	-1.87	0.06	3343
4-day all	-0.03	-2.68	11.77	-0.58	0.56	3448
4-day no overlap	0.36	-0.29	6.96	1.48	0.14	105
4-day overlap	-0.04	0.13	28.69	-0.81	0.42	3343

Note: Confidence intervals calculated in columns (3) and (4) use bias-corrected and accelerated bootstrap interval with 10,000 replications. T statistics and p-values computed under the null of zero effect.

Moving from non-overlapping to overlapping estimates the sign of the estimated CAARs becomes negative. It's ambiguous what is going on here, it could be that events occurring in close succession are deemed more harmful by agents or attacks aren't independent and agents systematically under predict the magnitude of the next attack, given their information about the first attack, and this attack therefore causes markets to react more negatively compared to one off events in the non-overlapping case. Another explanation is the constant mean return calculated in the estimation window could be artificially high, if for instance, markets are rebounding following an attack and hence CARs are lower. However, overall any biases present in the unfiltered case are likely to be small, especially when compared in magnitude to the size of our uncertainty surrounding CAAR estimates indicated by the bootstrapped confidence intervals, and doesn't change the conclusion of our analysis that terror attacks, on aggregate, have no effect on UK asset markets. I test for sensitivity to index choice in the appendix, using the FTSE Smallcap and various sector specific indices such as FTSE 350

[27]It seems counter-intuitive that moving from $N = 105$ to $N = 3448$ doesn't increase the precision of my estimates and in fact the confidence intervals grow wider. However, it's worth noting that the attacks recorded in the GTD are *extremely* diverse. The modal number of killed and wounded from an attack is zero whilst some attacks suffer from upwards of 700 injuries. Therefore, whilst N is increasing, we're not learning much more about CAARs from these extra datapoints - the modal injury or fatality is 0 and a handful of extremely large attacks are added with which to estimate a negative response from. For more information on the terror dataset see the summary statistics in the appendix or data section of the paper.

aerospace/defence, banking and industrial and find no evidence of index or sector sensitivity although the uncertainty surrounding the estimates is, again, very large.

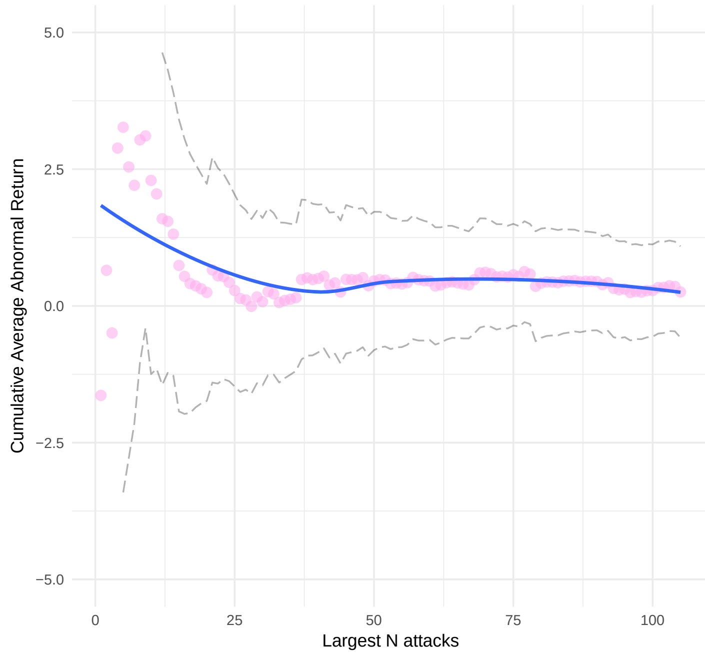

FIGURE 6. 10-DAY CUMULATIVE AVERAGE ABNORMAL RETURNS AS A FUNCTION OF N LARGEST ATTACKS. BLUE LINE INDICATES A LOCAL POLYNOMIAL LINE OF FIT. CONFIDENCE INTERVALS SHOWN ARE TRADITIONAL 95% CONFIDENCE INTERVALS DRAWN FROM A T-DISTRIBUTION SINCE BCA CONFIDENCE INTERVALS ARE TOO WIDE TO DISPLAY.

Finally, it's possible to test whether overlapping and non-overlapping events are significantly different by testing for balance on observables. If attacks are in fact i.i.d. as assumed under the event study methodology I should find that attack characteristics do not differ significantly between treatment and control i.e. overlapping and non-overlapping events should have similar characteristics. However, as displayed in Figure 1, many event variables recorded in the dataset are characterised by extremely fat-tailed distributions. This again creates issues when using a traditional t-test, not only are data non-normally distributed but with a sample of $N = 105$ it's possible that the central limit theorem won't hold in this case either. Therefore, instead of using a t-test I use a non-parametric alternative, the Kruskal-Wallis test for differences in medians. Unfortunately, this test for balance fails even after a Bonferroni correction[28], more details of which are contained

[28]A Bonferroni correction is one of the most conservative corrections for multiple testing which works

in the Appendix Table A8. An alternative testing strategy would be to impart known information into the testing model by specifying a fat-tailed distribution, such as the Cauchy, as the likelihood function for event characteristics and a weak prior centred at zero for the estimated difference in scale parameters between the overlapping and non-overlapping samples. However, there's a risk that this would introduce too much bias into the model and it's not immediately obvious what motivates a 'good' distribution to pick as a likelihood function. Therefore, I cannot provide evidence that overlapping and non-overlapping events are identical, which is a necessary condition for i.i.d. events. I believe it's plausible that I've been unlucky with the non-overlapping sample. It's feasible that a small sample drawn from a very fat-tailed distribution will not look similar to the far larger, overlapping, sample but it could equally well be due to the fact that overlapping and non-overlapping events are fundamentally different and therefore all event study estimates should be interpreted cautiously.

IV. Estimating Event Heterogeneity

Attacks in the dataset range from catastrophic events involving hundreds of injured to unsuccessful plots where no injuries or fatalities were recorded. It seems sensible that the effect of the former will differ from the latter and that the perhaps surprising absence of a terror response reported above is driven by this event heterogeneity. Fortunately, the Global Terror Database records over a hundred variables of interest for each attack[29]. In this section I leverage the rich nature of the GTD dataset and construct a media intensity index[30]to establish whether an event's cumulative abnormal returns are a function of event characteristics. Since LexisNexis' historical newspaper archives only run from 1984 (LexisNexis, 2018), I restrict the sample to attacks from 1984 onwards.

Whilst there are over three thousand events in the dataset many of the CARs calculated from these events cannot be used in the following analysis since any overlap violates the assumptions under which the event study is undertaken. This means any estimation of heterogeneity will involve approximately fifty parameters, P, and only eighty non-overlapping datapoints, N. Whilst this isn't as extreme as situations often faced in machine learning where often $P > N$, there is still a significant risk of overfitting. I attempt to solve this problem by performing variable selection using shrinkage estimators, effectively reducing P. As a robustness check I also estimate event heterogeneity using event day returns and the full, overlapping and non-overlapping, dataset in the appendix. This requires the (strong) assumption[31] that a terror attack only influences the return on the day of the attack - there cannot be any lagged effects. However, this also means our

in my favour in this case, so represents a best possible testing scenario for balance tests. I 'want' to be unable to reject the null hypothesis in as many cases as possible.

[29]For a full list see the appendix

[30]For details of media intensity index construction see the appendix.

[31]Although evidence from this paper alleviates these worries somewhat.

dependent variable is potentially measured with more noise - the effect of using the constant mean return model to find abnormal returns in an event study makes the CAR less noisey in the sense that only extreme events should cause significant movements in CARs.

I perform variable selection by setting Laplace priors which is known as the Bayesian LASSO (Park and Casella, 2008), and projection predictive variable selection (Piironen and Vehtari, 2017) to try and ensure estimates don't overfit the sample at the expense of the population. Laplace priors are commonly used to perform shrinkage in a Bayesian setting because their marginal distribution is similar to the $\ell 1$-norm penalty term used in frequentist LASSO. Laplace priors effectively have a lot of density at their mode whilst maintaining relatively fat tails. This means if a signal is strong enough it can 'escape' any noise but otherwise parameter estimates are set to zero where much of the Laplace's density is found[32].

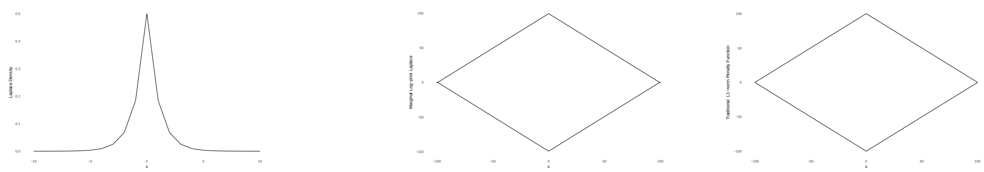

FIGURE 7. LAPLACE PRIOR VS $\ell 1$-NORM. FROM LEFT TO RIGHT: THE LAPLACE PDF, THE MARGINAL LOG LAPLACE DISTRIBUTION[34]AND THE TRADITIONAL $\ell 1$-NORM PENALTY TERM.

First, I estimate a standard OLS regression to serve as a baseline with which to compare the LASSO results. Again I decide to use weakly informative priors and the standard Gaussian likelihood function. The weak priors help indicate a reasonable scale for the parameters but aren't designed to impart any particular knowledge or information into the model:

$$\alpha \sim N(0, 10)$$
$$\beta \sim N(0, 2.5)$$
$$\sigma \sim \exp(1)$$

$$Y_i \sim N(\alpha + \beta X_i, \sigma^2)$$

where Y_i is the dependent variable of interest, 4-day CARs, X_i a vector of event characteristics and exp indicates the exponential distribution. It's worth noting that whilst I refer to the above specification as a standard OLS model it

[32]Or very, very, very close to 0 due to the arcanities of Stan and the fact that the posterior mean $P(\beta = 0|y) = 0$ when β is continuous (Stan Development Team, 2017).

[34]Transformed here to better demonstrate similarities with the $\ell 1$-norm by adding 100 and displaying both the positive and negative of the function - also, it seems we've lost a footnote somewhere in the LaTeX abyss.

is in fact the Bayesian interpretation of ridge regression[35] because the intercept and coefficient distributions are centred at zero and the scale parameters act as regularisation penalties (the inverse of precision in the machine learning sense). Ridge regression is another technique often used to prevent overfitting however it's known to perform poorly compared to the LASSO at dealing with sparsity (Tibshirani, 1996), i.e. we expect many of the event characteristics in the vector X_i to have coefficients of zero in the true population model. I think it's reasonable to assume that all 81 variables do not influence event CARs which motivates the use of LASSO later.

Heterogeneous OLS - Results

The OLS results displayed in Figures 8-11 show the posterior medians, 50% credibility intervals and 90% credibility intervals for the coefficients of all event covariates using 4-day cumulative abnormal returns as the dependent variable. The vast majority of event covariates have little impact on 4-day cumulative abnormal returns, even when posterior medians are large in magnitude the 90% credibility intervals often include zero. Figure 8 offers surprising evidence, whilst the success of an attack leads to more negative cumulative abnormal returns, as we'd expect, number of wounded and an indicator variable for property damage both have positive posterior medians and have 90% credibility intervals that exclude zero. Interestingly, there's no evidence that time trends (T) influence CARs and the posterior distribution of media intensity (MA4)'s coefficient displays a large amount of uncertainty making it hard to definitively conclude there's a negative relationship between CARs and terror media intensity. Finally, Figure 9 shows that the only attack type that influences CARs with any certainty at the 90% level is assassination. However, there's too much uncertainty amongst both target type (Figure 10) and weapon type (Figure 11), excluding unknown explosives, to conclude with any reasonable degree of certainty that any of the variables influence 4-day CARs.

The results from the OLS specification are surprising but if the population model is in fact sparse, as I suspect, should be interpreted with caution. As is, the results suggest that only a few metrics determine the size of cumulative abnormal returns such as whether an event was an assassination; the number of wounded and whether there was any property damage - a useful proxy for capital destruction[36]. However, the results suggest even events with wildly different characteristics on the whole produce similar cumulative abnormal returns and we cannot reasonably conclude that the absence of significant negative returns in the previous section is driven by event heterogeneity.[37]

[35] Also known as Tikhonov regularisation.

[36] Although the sign of the coefficient on this proxy is the opposite to what we'd expect from the asset pricing model described in section II.

[37] For robustness checks using both overlapping and non-overlapping CARs as well as event day returns as the dependent variable and a more detailed list of event characteristics see Table A9.

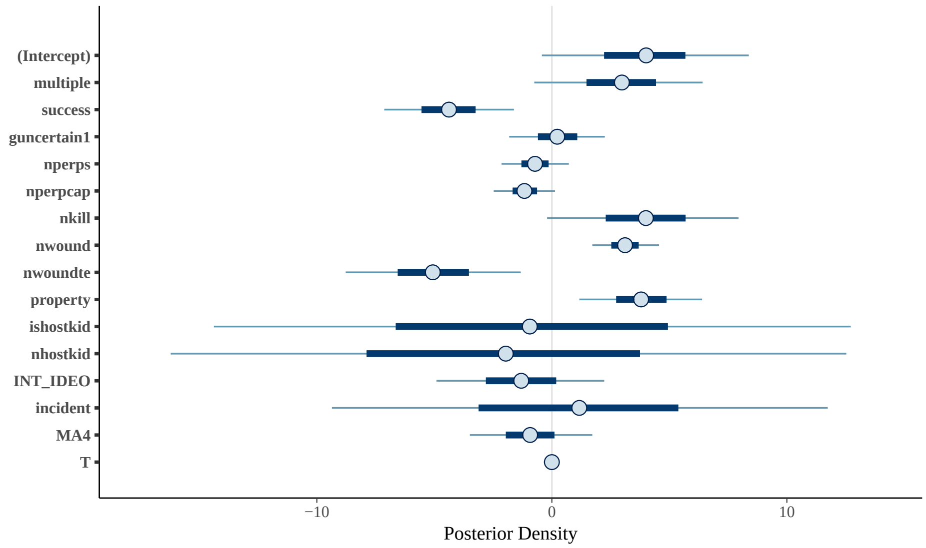

Figure 8. OLS of 4-day Cumulative Abnormal Returns on event covariates. Displayed here are posterior medians, 50% credibility intervals and 90% credibility intervals.

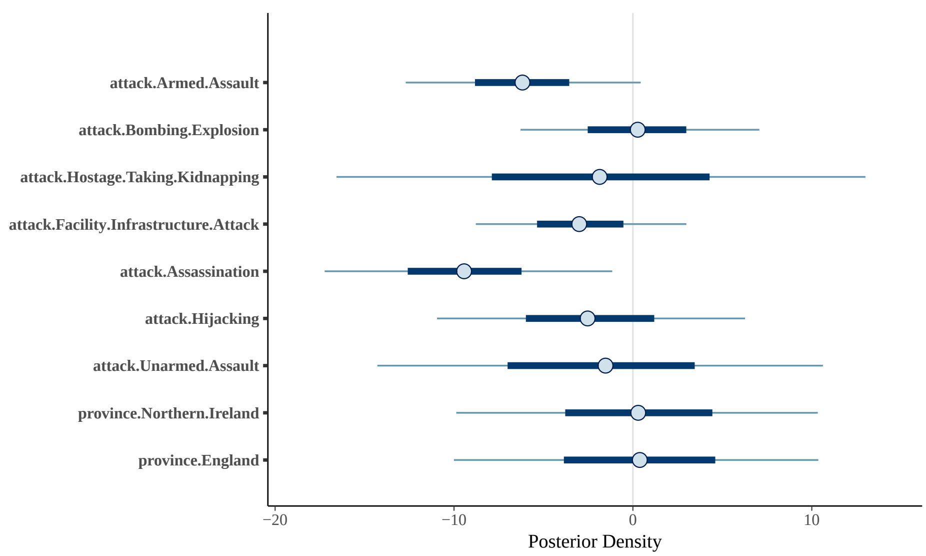

Figure 9. OLS of 4-day Cumulative Abnormal Returns on event covariates. Displayed here are posterior medians, 50% credibility intervals and 90% credibility intervals.

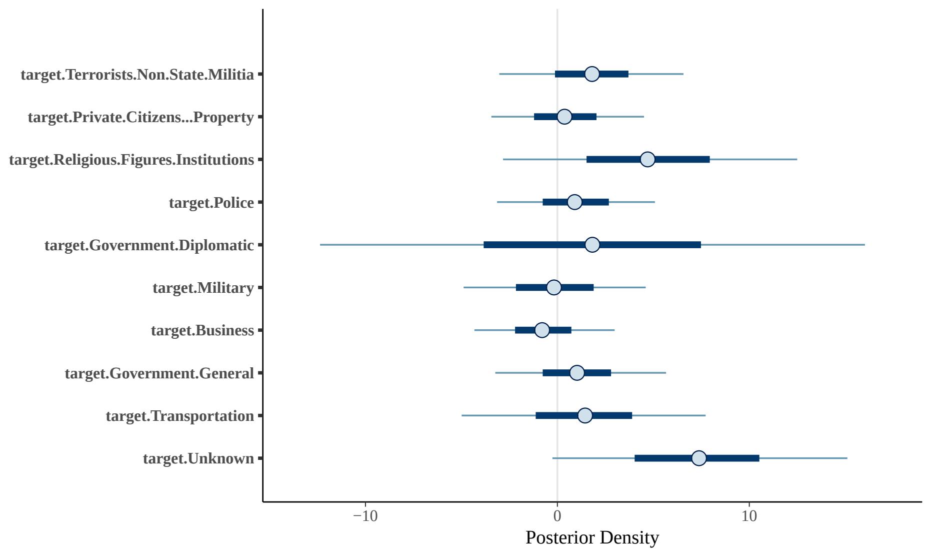

FIGURE 10. OLS of 4-day Cumulative Abnormal Returns on event covariates. Displayed here are posterior medians, 50% credibility intervals and 90% credibility intervals.

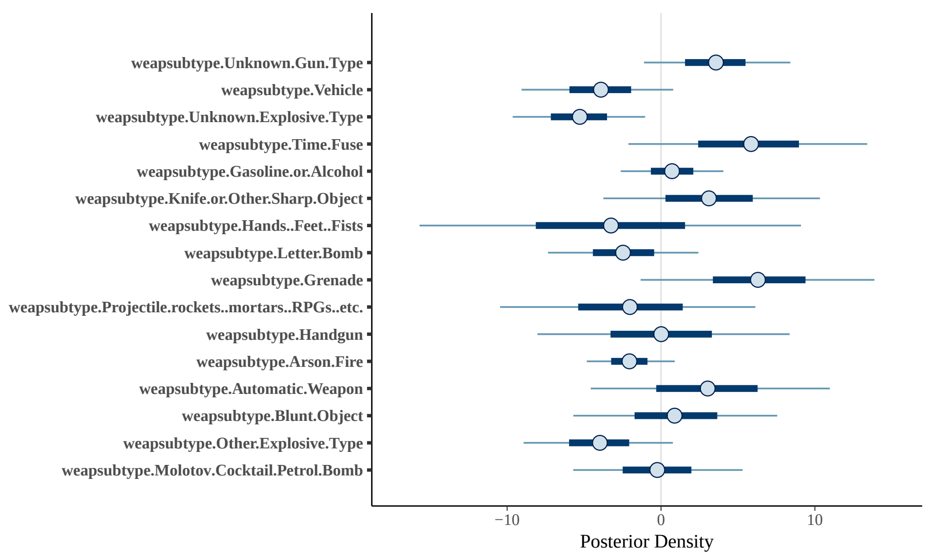

FIGURE 11. OLS of 4-day Cumulative Abnormal Returns on event covariates. Displayed here are posterior medians, 50% credibility intervals and 90% credibility intervals.

Heterogeneous LASSO - Results

The LASSO set-up is similar to the ridge but with independent Laplace (double exponential) distributions on the β coefficients (Park and Casella, 2008):

$$\alpha \sim N(0, 10)$$
$$\beta \sim \text{Laplace}(0, 2.5)$$
$$\sigma \sim \exp(1)$$

$$Y_i \sim N(\alpha + \beta X_i, \sigma^2)$$

Typically in machine learning settings researchers use cross-validation to choose an appropriate tuning parameter[38], t, but a more Bayesian approach is to specify a prior distribution on t itself. Accordingly, I set $t \sim \chi^2_1$ using rstanarm's (default) weakly informative prior suggestion (Stan Development Team, 2016).

The LASSO model can be considered a sub-model of the full model with certain coefficients set to zero thanks to the regularisation properties of the Laplace priors. Furthermore, the LASSO may have more desirable properties than the full model due to exploitation of the bias-variance trade-off. However, there's no reason to stop there, we can go a step further and ask which sub-model of the LASSO provides the best predictive performance. That is, the LASSO performs variable subset selection to produce the set of 'important'[39] variables, denoted S. Now I explore which number of variables in S produces the best predictive accuracy, this could range from all of S, the full model, or a sub-model with just the intercept term and no predictor variables.[40]

One way to do this would be to perform Leave-One-Out cross-validation (LOO) or use Watanabe-Akaike information criteria (WAIC) for every subset in S and compare expected log predictive densities (elpd) to choose a model and hence the variables with the best predictive power of cumulative abnormal returns after an attack. However, Piironen and Vehtari (2017) show that when there is little data to estimate the model with and a large number of models are considered, as is the case when performing variable selection, overfitting becomes a concern. Instead of LOO or WAIC they suggest projecting information from the full Bayesian model into the sub-models and comparing expected log predictive densities to choose number of predictors and *then* performing LOO on this variable selection process.[41]

[38] i.e. $\arg\min_{\alpha,\beta}\{\frac{1}{N}\sum_{i=1}^{N}(y_i - \alpha - \beta X_i)^2\}$ subject to $\sum_{j=1}^{K}|\beta_j| \leq t$

[39] i.e. variables with coefficients not set to 0.

[40] It'd be interesting to compare the powerset of S to find which combination of variables perform best however as mentioned in Piironen and Vehtari (2017) cross-validation performs much better at choosing model size by comparing $p + 1$ models, with given variable order, than 2^p combinations of sub-models.

[41] For more information on variable selection in a Bayesian setting using projection see Dupuis and Robert (2003).

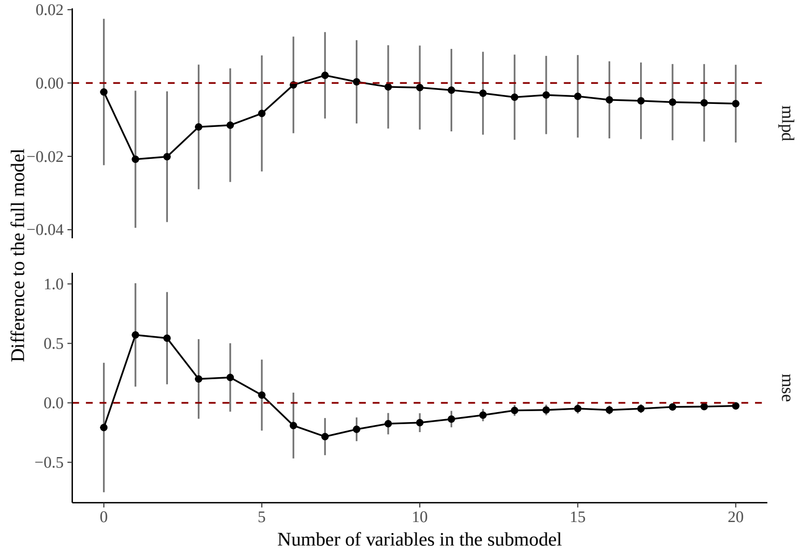

FIGURE 12. LASSO variable subset selection

Figure 12 displays the mean squared error and mean log predictive densities of each sub-model. Mean log predictive density is my preferred metric since it incorporates posterior uncertainty unlike mean squared error which only uses point-estimates from each sub-model. The results provide conclusive evidence, I believe, that event characteristics do not determine cumulative abnormal returns after a terror attack. A model with no variables and just a constant performs almost as well as a model incorporating the seven best predictor variables. The uncertainty intervals using mean log predictive density are practically identical and although the mean squared error measure shows a reduction in uncertainty, the point estimates indicate a negligible improvement moving from the zero variable to seven variable case.

Interestingly, the best predictors as shown in Figure 13 make economic sense. The media intensity four-day moving average could perhaps suggest that panic and hysteria are more influential than capital destruction's effect on stock market returns and therefore is tentative evidence of indirect effects trumping direct effects of terror. Number of wounded seems like a good proxy for human capital destruction and event magnitude. Furthermore, the inclusion of the wounded variable validates the use of wounded in the large event heuristic used earlier. Finally, assassination and attack success seem like reasonable determinants of cumulative abnormal returns. However, only very limited inferences should be made using these results. We know that they perform little better than just guessing mean CAR (i.e. CAAR) which is the intercept-only model. Also, the posterior medians shown in Figure 13 are all relatively small in magnitude and

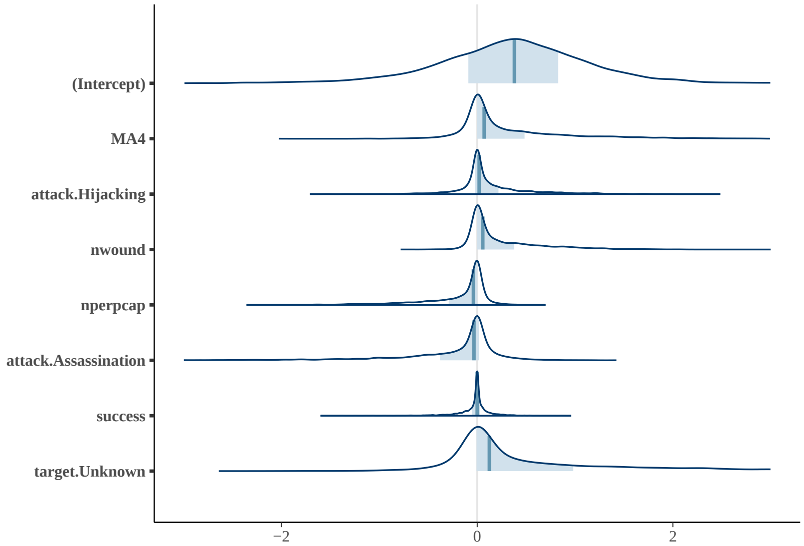

FIGURE 13. THE SEVEN BEST PREDICTOR VARIABLES SHOWN IN THE FULL LASSO MODEL. SHADED DENSITIES INDICATE 50% CREDIBILITY INTERVALS. VARIABLE SUBSET SELECTION PERFORMED USING THE PROJECTION METHOD, COMPARING PREDICTIVE DENSITIES AND LOO. THE PROJECTED MODEL CAN BE FOUND IN THE APPENDIX.

the credibility intervals indicate a great deal of uncertainty.

MODEL COMPARISON

Considering the dismal conclusions about predictive power we draw from Figure 12 I now check whether the LASSO really does outperform the OLS specification. We know that theoretically the LASSO should perform better than ridge/OLS when fitting small N, large P, sparse models but I can also check whether this holds empirically in the data. To test this I perform LOO for the OLS and LASSO full model and compare expected log predictive densities. This method is commonly used to compare Bayesian models as it has a number of advantages over the more traditional Deviance Information Criteria (DIC) and Akaike Information Criteria (AIC). Namely, LOO uses all the posterior information from a model instead of point estimates (Vehtari, Gelman and Gabry, 2015)[42]. Table 7 shows that the expected log predictive density difference between the two models is negative, indicating that the first model, the LASSO has superior predictive performance. Finally, it's worth noting that I only compare model goodness-of-fit in relative and not absolute terms, a more Bayesian approach would involve posterior predictive

[42]Watanabe-Akaike information criteria (WAIC) is identical to LOO in the asymptotic case (Vehtari, Gelman and Gabry, 2015).

checking and examination of various test statistics which I omit from this paper in the interests of brevity.

Table 7—Expected Log Predictive Density Difference LASSO vs OLS

Parameters	Estimate	Standard Error
$\text{elpd}_{LASSO} - \text{elpd}_{OLS}$	-18.3	7.5

Note: A negative difference in expected log predictive density indicates that the first model, the LASSO, perfoms best.

V. Conclusion

I have shown that on aggregate terror attacks do not cause statistically significant falls in returns. We cannot reject the null hypothesis that the terror attack cumulative average abnormal return in the UK from 1970-2016 is statistically significantly different from zero at the 5% significance level. Performing sub-sample analysis at the five and twenty largest attack level produces similar results. Using an alternative conditional probability approach and exploiting the multi-level nature of the data in a logistic Bayesian hierarchical framework leads to the same conclusions. Furthermore, there's no evidence of event heterogeneity. Whilst ridge regression of cumulative abnormal returns on event characteristics suggests some evidence of heterogeneity, these effects disappear using a LASSO model. Not only does this LASSO model set the majority of variable coefficients to 0, variable subset selection on the LASSO model finds that the optimal number of variables to include to maximise expected log predictive density is practically indifferent between using no variables or seven variables.

Whilst surprising, this result qualitatively fits the asset pricing model specified in section II and reasonable expectations from the data, where modal attack injury is 0 for instance. Either attacks have no effect, i.e. human and physical capital destruction is negligible, or households form accurate and robust expectations of the terror threat and asset market prices reflect this. It seems reasonable to discard the former, although many attacks inflict no or few injuries, there are verifiable estimates of large property damage from attacks and there have been many attempts to quantify the social costs of injury and premature death.

Alternatively, my identification strategy has gone awry. There are number of limitations with my approach. Firstly, the event study relies on the assumption of independent attacks. This independence is by no means guaranteed, there is evidence in my sample that attacks are not in fact independent and overlapping attacks are fundamentally different to non-overlapping attacks. Secondly, I assume that attacks only cause stock market movements in a ten-day window and that twenty observations in the estimation window are adequate to produce a terror counterfactual, my approach could be augmented with a volume-based or varying sized window as outlined in Krivin et al. (2003). Finally, my approach attempts to link equity index returns to economic costs using an asset pricing model. This approach has its drawbacks. Although I use a range of indices to combat this in my robustness checks, it's plausible that equity indices don't respond to UK attacks because of the large amount of firms with primarily foreign earnings listed on UK stock exchanges. Furthermore, it's unlikely that households use equity indices as a saving technology over the length of my event windows, ten days.

Throughout the paper there is a trade-off between increasing N, and therefore considering more events, and reducing the size and hence probable effect of each attack. For instance, the largest attack recorded in the dataset from 1980 till today led to 270 fatalities, the second largest was a fraction of that with only 56 fatalities. Therefore, when performing sub-sample analysis for the five largest attacks the gain in statistical power relative to the reduction in event magnitude is tiny. This imbalanced tradeoff is a natural product of the extremely skewed distribution and relatively fat tails that characterises terror attack size. Further work could try and combat this by using a hierarchical model and incorporating multiple countries. For example, a hierarchical model that included the largest attacks in Europe and the US would include events such as 9/11, the Madrid train bombings and the more recent Paris attacks in November 2015. This dataset wouldn't suffer from such a drastic fall-off in event magnitude as the researcher increased N.

Terror attacks are tragic and disturbing events. However, overall it seems that terror's prominence in public discourse, at least in the UK, is oversized relative to terror attacks' costs. This is not entirely surprising, terror attacks are designed to instil fear in a populace rather than pose any existential threat to the performance and running of an economy. I show that the large, negative responses to terror attacks well-documented in the current literature are the exception and not the norm and shouldn't be regarded as an average terror response.

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Appendix

A1. Media Intensity Construction

I use LexisNexis' historical newspaper archive running from 1984 to present day to search all UK newspaper journals containing the stem words 'terror' and 'attack' in either the headline or first paragraph of the article. If a match is found I then search within the article for keywords related to the UK such as: 'Great Britain', 'Britain', 'England', 'Ireland' etc. and if at least two hits are recorded, add the article to the dataset. I remove all articles that contain references to either 9/11 or the November 2015 Paris attacks due to the large number of articles published in the British press in the aftermath of these attacks. Finally, I construct a four-day moving average of the number of terror articles recorded for use in the four-day cumulative average abnormal return specification.

A2. Additional Results and Robustness Checks

Table A1—Five Largest Events used in event study analysis

Date	Fatalities	Wounded	Incident	Intensity	Event
1988-12-21	270	0	3	813	Lockerbie
2005-07-07	56	784	4	564	London 7/7
1998-08-15	29	220	1	198	Omagh
1996-06-15	0	200	1	101	1996 Manchester
1982-12-06	16	66	1	82	Droppin Well

Note: The GTD has lost all data from 1993 so the Bishopgate Bombing isn't shown here.

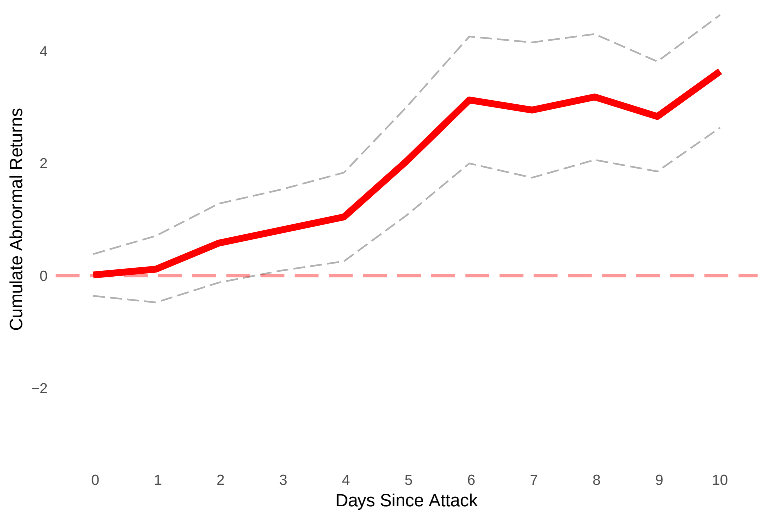

Figure A1. Lockerbie Cumulative Abnormal Returns - FTSE Allshare, 21/12/1998

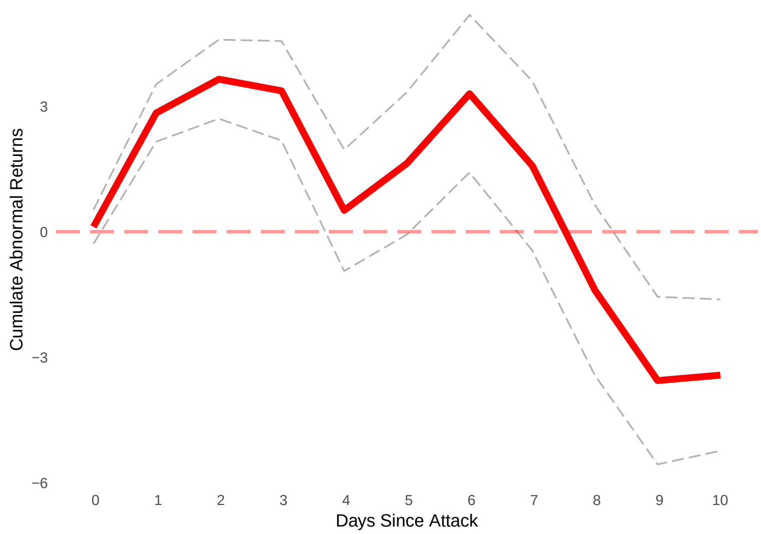

Figure A2. Omagh Car Bombing Cumulative Abnormal Returns - FTSE Allshare, 15/08/1998

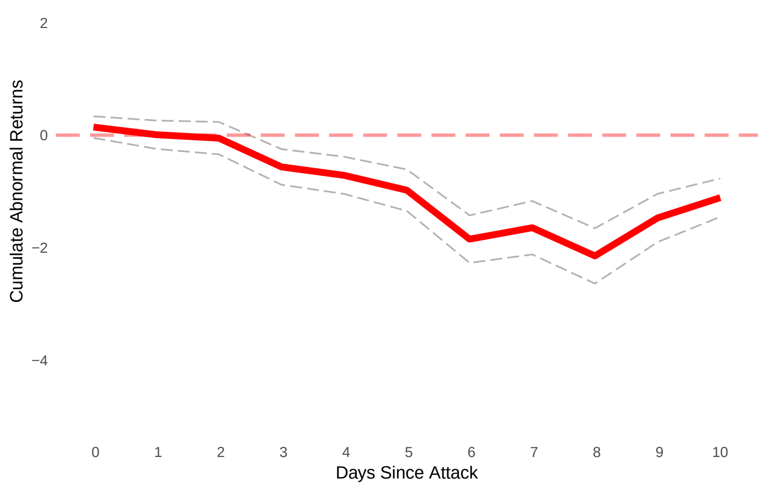

Figure A3. 1996 Manchester Bombing - FTSE Allshare, 15/06/1996

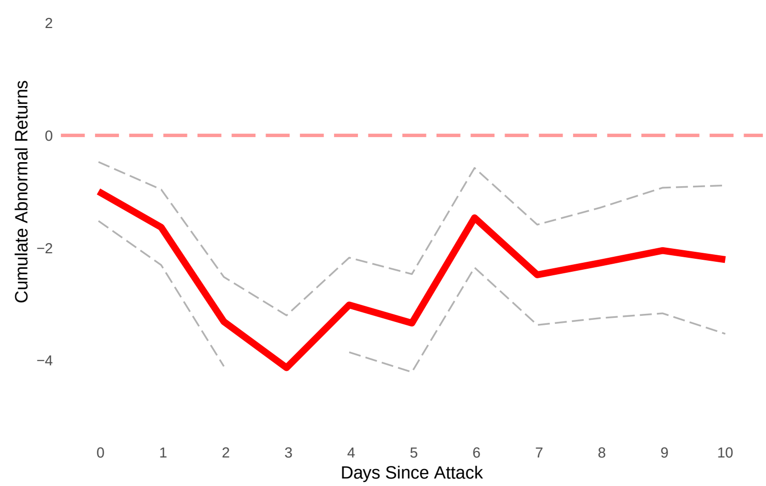

Figure A4. Droppin Well - FTSE Allshare, 06/12/1982

Table A2—Twenty largest events used in event study analysis

	Date	Fatalities	Wounded	Incidents	Intensity	Overlap
1	1988-12-21	270	0	3	813	FALSE
2	2005-07-07	56	784	4	564	FALSE
3	1998-08-15	29	220	1	198	FALSE
4	1974-11-21	22	162	3	150	FALSE
5	1973-03-08	3	238	3	131	FALSE
6	1996-06-15	0	200	1	101	FALSE
7	1982-12-06	16	66	1	82	FALSE
8	1985-02-28	19	30	3	75	FALSE
9	1987-11-07	11	61	1	64	FALSE
10	1983-12-17	6	80	2	60	FALSE
11	1996-02-09	2	100	1	57	FALSE
12	1979-08-27	18	2	2	57	FALSE
13	1992-04-10	3	90	2	56	FALSE
14	1978-02-17	14	25	1	56	FALSE
15	1982-07-20	9	49	2	54	FALSE
16	1972-02-22	14	19	2	54	FALSE
17	1974-02-04	13	17	5	52	FALSE
18	1971-12-04	16	0	2	50	N/A
19	1989-09-22	11	30	1	49	FALSE
20	1979-03-23	0	0	43	43	FALSE

Note: Overlap checks whether events overlap within the subset of events considered, not whether events overlap with other, non-included events. The first event in 1971 is set to N/A as by virtue of being first in set of events considered.

Table A3—Conditional Probability Posterior Distribution Results - Five Largest Attacks

Term	Estimate	std.error	conf.low	conf.high	event	model
$\hat{p}_1$	0.347	0.035	0.280	0.417	1	hierarchical
$\hat{p}_2$	0.054	0.148	0.001	0.572	2	hierarchical
$\hat{p}_3$	0.431	0.036	0.360	0.504	3	hierarchical
$\hat{p}_4$	0.568	0.038	0.493	0.643	4	hierarchical
$\hat{p}_5$	0.138	0.028	0.088	0.197	5	hierarchical
$\hat{p}_1$	0.304	0.015	0.275	0.335	1	pooled
$\hat{p}_2$	0.419	0.046	0.332	0.514	2	pooled
$\hat{p}_3$	0.291	0.015	0.263	0.321	3	pooled
$\hat{p}_4$	0.279	0.015	0.250	0.309	4	pooled
$\hat{p}_5$	0.335	0.021	0.295	0.377	5	pooled
$\hat{p}_1$	0.346	0.036	0.278	0.418	1	separate
$\hat{p}_2$	0.576	0.389	0.001	1.000	2	separate
$\hat{p}_3$	0.431	0.037	0.358	0.503	3	separate
$\hat{p}_4$	0.592	0.041	0.512	0.673	4	separate
$\hat{p}_5$	0.137	0.028	0.087	0.193	5	separate

Note: Out-of-sample fitted value estimates from the conditional probability approach using three different models. A lower estimate indicates a more extreme negative event return.

Table A4—CAARs calculated using constant median return model

Parameter	CAAR	Lower 95%	Upper 95%	T	p-value	n
10-day CAAR all	-0.21	-6.22	17.86	-2.38	0.02	3448
10-day CAAR no overlap	0.40	-5.87	14.39	0.89	0.38	105
10-day CAAR overlap	-0.23	-3.28	29.11	-2.55	0.01	3343
4-day CAAR all	-0.06	-6.18	5.32	-1.12	0.26	3448
4-day CAAR no overlap	0.43	0.50	7.27	1.70	0.09	105
4-day CAAR overlap	-0.07	-0.61	26.38	-1.40	0.16	3343

Note: CAAR estimates are robust to using a constant median return model which suggests findings in the body of the paper aren't driven by event overlap or sensitivity to extreme outliers.

Table A5—Non-Overlapping 10-day CAARs using sector specific industries and FTSE small-cap.

Parameter	CAAR	boot.ci.lower	boot.ci.upper	T.statistic	p	n
FTSE smallcap	0.38	-5.35	15.51	0.68	0.50	85
Aerospace/Defence	0.02	-17.40	2.53	0.04	0.97	85
Industrial	0.62	0.00	16.02	1.07	0.29	85
Retail	-0.15	-3.55	19.63	-0.24	0.81	85

Note: Only non-overlapping results are shown due to computational constraints (bootstrapping the overlapping and entire set of events takes significantly longer). The lack of a negative return amongst smallcap stocks suggest results aren't driven by large corporate foreign earnings in the FTSE 250. There seems to be no evidence of sector specific responses although the uncertainty surrounding these estimates is very large.

Table A6—Cumulative Average Abnormal Returns for the five largest attacks since 1980. Multiple Indices

Index	Day	CAAR	Lower 95%	Upper 95%	T stat	p-value
FTSE Allshare	0	-0.45	-1.53	0.14	-1.68	0.17
FTSE Allshare	1	0.22	-1.63	2.84	0.36	0.74
FTSE Allshare	2	0.15	-3.31	3.64	0.16	0.88
FTSE Allshare	3	-0.24	-4.13	3.37	-0.23	0.83
FTSE Allshare	4	-0.50	-0.71	1.05	-0.82	0.46
FTSE Allshare	5	-0.20	-0.98	2.04	-0.23	0.83
FTSE Allshare	6	0.42	-1.85	3.30	0.45	0.68
FTSE Allshare	7	-0.22	-1.48	2.95	-0.26	0.81
FTSE Allshare	8	-0.90	-1.88	3.18	-1.13	0.32
FTSE Allshare	9	-1.20	-1.75	2.83	-1.45	0.22
FTSE Allshare	10	-0.97	-1.75	3.64	-1.04	0.36
MSCI	0	-0.44	-1.51	0.52	-1.30	0.26
MSCI	1	0.31	-2.03	3.84	0.38	0.72
MSCI	2	0.28	-3.57	4.68	0.25	0.81
MSCI	3	-0.15	-4.34	4.39	-0.12	0.91
MSCI	4	-0.41	-2.74	0.93	-0.71	0.52
MSCI	5	-0.05	-3.34	2.51	-0.05	0.96
MSCI	6	0.71	-1.87	4.48	0.68	0.53
MSCI	7	0.01	-2.53	2.78	0.01	0.99
MSCI	8	-0.66	-1.92	2.97	-0.85	0.45
MSCI	9	-0.84	-1.82	2.72	-1.22	0.29
MSCI	10	-0.64	-1.84	3.42	-0.79	0.47
FT30	0	-0.55	-1.80	0.30	-1.68	0.17
FT30	1	0.11	-2.06	3.27	0.15	0.89
FT30	2	0.13	-3.96	4.72	0.11	0.92
FT30	3	-0.23	-5.11	4.77	-0.17	0.88
FT30	4	-0.52	-3.80	1.78	-0.61	0.57
FT30	5	-0.31	-4.45	2.98	-0.28	0.79
FT30	6	0.55	-2.35	4.93	0.46	0.67
FT30	7	-0.28	-3.78	3.29	-0.25	0.82
FT30	8	-0.92	-1.41	2.59	-1.13	0.32
FT30	9	-1.09	-1.71	2.23	-1.51	0.21
FT30	10	-0.91	-1.43	3.21	-1.04	0.36

Note: Confidence intervals calculated using bias-corrected and accelerated bootstrap interval with 10,000 replications.

Table A7—Cumulative Average Abnormal Returns for the twenty largest attacks since 1970. Multiple Indices

Index	CAAR	Lower 95%	Upper 95%	T stat	p-value	Day
FTSE Allshare	0.02	-2.51	5.88	0.06	0.96	0
FTSE Allshare	0.11	-4.01	6.74	0.23	0.82	1
FTSE Allshare	0.40	-7.11	7.42	0.61	0.55	2
FTSE Allshare	1.00	-4.13	9.17	1.29	0.21	3
FTSE Allshare	1.14	-3.51	9.51	1.48	0.16	4
FTSE Allshare	1.09	-3.34	11.28	1.26	0.22	5
FTSE Allshare	1.13	-1.85	11.16	1.26	0.22	6
FTSE Allshare	1.07	-2.48	12.50	1.14	0.27	7
FTSE Allshare	0.88	-2.50	12.31	0.92	0.37	8
FTSE Allshare	0.83	-3.20	12.85	0.83	0.42	9
FTSE Allshare	1.10	-2.21	15.49	0.96	0.35	10
MSCI	0.11	-2.41	2.58	0.28	0.78	0
MSCI	0.26	-4.15	6.92	0.52	0.61	1
MSCI	0.62	-6.62	7.70	0.92	0.37	2
MSCI	1.27	-4.62	9.44	1.56	0.13	3
MSCI	1.38	-4.12	9.91	1.78	0.09	4
MSCI	1.16	-3.88	11.72	1.34	0.20	5
MSCI	1.20	-1.87	11.61	1.34	0.20	6
MSCI	1.07	-2.53	13.03	1.16	0.26	7
MSCI	0.86	-1.92	12.83	0.93	0.36	8
MSCI	1.05	-1.98	13.69	1.07	0.30	9
MSCI	1.36	-2.29	16.54	1.19	0.25	10
FT30	0.10	-3.45	1.21	0.22	0.83	0
FT30	0.19	-6.33	3.27	0.33	0.74	1
FT30	0.57	-6.80	7.00	0.81	0.43	2
FT30	1.25	-5.11	9.63	1.53	0.14	3
FT30	1.15	-4.99	10.10	1.35	0.19	4
FT30	0.91	-4.58	12.10	0.96	0.35	5
FT30	1.08	-2.35	12.32	1.08	0.29	6
FT30	1.09	-4.06	13.57	1.06	0.30	7
FT30	0.86	-1.96	13.44	0.83	0.42	8
FT30	0.90	-1.96	14.46	0.82	0.42	9
FT30	1.18	-3.07	17.65	0.95	0.35	10

Note: Confidence intervals calculated using bias-corrected and accelerated bootstrap interval with 10,000 replications.

Table A8—Balance on observables test for overlapping and non-overlapping events using a Kruskal-Wallis test.

Variable	p-value	sig different 5%	sig different 5% adj
extended	0.5065	FALSE	FALSE
multiple	0.0752	FALSE	FALSE
success	0.0017	TRUE	FALSE
suicide	0.8021	FALSE	FALSE
guncertain1	0.0003	TRUE	TRUE
individual	0.4510	FALSE	FALSE
nperps	0.0609	FALSE	FALSE
nperpcap	0.0000	TRUE	TRUE
nkill.x	0.0000	TRUE	TRUE
nkillter	0.4401	FALSE	FALSE
nwound.x	0.7508	FALSE	FALSE
nwoundte	0.1187	FALSE	FALSE
property	0.2475	FALSE	FALSE
ishostkid	0.2910	FALSE	FALSE
nhostkid	0.7001	FALSE	FALSE
ransom	0.5747	FALSE	FALSE
ransomamt	0.6917	FALSE	FALSE
ransompaid	0.8593	FALSE	FALSE
INT_IDEO	0.0072	TRUE	FALSE
INT_MISC	0.0004	TRUE	TRUE
incident.x	0.0004	TRUE	TRUE
attack.Armed.Assault	0.6352	FALSE	FALSE
attack.Bombing.Explosion	0.9191	FALSE	FALSE
attack.Hostage.Taking.Kidnapping	0.7558	FALSE	FALSE
attack.Facility.Infrastructure.Attack	0.0243	TRUE	FALSE
attack.Assassination	0.0000	TRUE	TRUE
attack.Hostage.Taking.Barricade.Incident	0.5065	FALSE	FALSE
attack.Hijacking	0.0001	TRUE	TRUE
attack.Unarmed.Assault	0.0991	FALSE	FALSE
attack.Unknown	0.3217	FALSE	FALSE
target.Terrorists.Non.State.Militia	0.3028	FALSE	FALSE
target.Private.Citizens...Property	0.7921	FALSE	FALSE
target.Religious.Figures.Institutions	0.4651	FALSE	FALSE
target.Police	0.0542	FALSE	FALSE
target.Government.Diplomatic	0.8416	FALSE	FALSE
target.Airports...Aircraft	0.2052	FALSE	FALSE
target.Military	0.0035	TRUE	FALSE
target.Business	0.8163	FALSE	FALSE
target.Utilities	0.6158	FALSE	FALSE
target.Maritime	0.6917	FALSE	FALSE
target.NGO	0.5747	FALSE	FALSE
target.Government.General	0.0862	FALSE	FALSE
target.Journalists...Media	0.4916	FALSE	FALSE
target.Transportation	0.5545	FALSE	FALSE
target.Telecommunication	0.6640	FALSE	FALSE
target.Unknown	0.8279	FALSE	FALSE
target.Tourists	0.6389	FALSE	FALSE
target.Food.or.Water.Supply	0.8021	FALSE	FALSE
target.Educational.Institution	0.3297	FALSE	FALSE
target.Violent.Political.Party	0.7229	FALSE	FALSE
target.Abortion.Related	0.8593	FALSE	FALSE
target.Other	0.8021	FALSE	FALSE
weapsubtype.Unknown.Gun.Type	0.0637	FALSE	FALSE
weapsubtype.Vehicle	0.1068	FALSE	FALSE
weapsubtype.Unknown.Explosive.Type	0.0241	TRUE	FALSE
weapsubtype.Rifle.Shotgun.non.automatic	0.0841	FALSE	FALSE
weapsubtype.Land.Mine	0.1812	FALSE	FALSE
weapsubtype.Time.Fuse	0.5346	FALSE	FALSE
weapsubtype.Gasoline.or.Alcohol	0.0063	TRUE	FALSE
weapsubtype.Knife.or.Other.Sharp.Object	0.0320	TRUE	FALSE
weapsubtype.Hands..Feet..Fists	0.2676	FALSE	FALSE
weapsubtype.Letter.Bomb	0.0296	TRUE	FALSE
weapsubtype.Grenade	0.7639	FALSE	FALSE
weapsubtype.Projectile.rockets..mortars..RPGs..etc.	0.1393	FALSE	FALSE
weapsubtype.Handgun	0.0040	TRUE	FALSE
weapsubtype.Poisoning	0.8593	FALSE	FALSE
weapsubtype.Remote.Trigger	0.2785	FALSE	FALSE
weapsubtype.Arson.Fire	0.0096	TRUE	FALSE
weapsubtype.Rope.or.Other.Strangling.Device	0.8593	FALSE	FALSE
weapsubtype.Automatic.Weapon	0.1478	FALSE	FALSE
weapsubtype.Unknown.Weapon.Type	0.6640	FALSE	FALSE
weapsubtype.Sticky.Bomb	0.5561	FALSE	FALSE
weapsubtype.Blunt.Object	0.0637	FALSE	FALSE
weapsubtype.Other.Explosive.Type	0.4927	FALSE	FALSE
weapsubtype.Molotov.Cocktail.Petrol.Bomb	0.7960	FALSE	FALSE
weapsubtype.Suicide.carried.bodily.by.human.being	0.8593	FALSE	FALSE
weapsubtype.Pressure.Trigger	0.8593	FALSE	FALSE
province.Northern.Ireland	0.0001	TRUE	TRUE
province.England	0.2047	FALSE	FALSE
province.Scotland	0.4267	FALSE	FALSE
province.Wales	0.4639	FALSE	FALSE

Note: sig different 5% indicates sample estimates are significantly different at a 5% significance level. sig different 5% adj indicates sample estimates are significantly different at a 5% significance level after a Bonferroni correction.

Table A9—OLS using event covariates

Dependent Variable:	Returns		4-day CAR filtered		4-day CAR unfiltered	
term	estimate	std.error	estimate	std.error	estimate	std.error
(Intercept)	0.04	0.07	4.01	2.56	0.02	0.29
number of terror news articles	-0.01	0.02				
attack Armed Assault	0.37	0.24	-6.17	3.91	0.29	1.05
attack Assassination	0.31	0.24	-9.44	4.71	0.15	1.07
attack Bombing Explosion	0.27	0.26	0.27	4.08	0.65	1.09
attack Facility Infrastructure Attack	0.34	0.25	-3.00	3.59	0.06	1.06
attack Hijacking	0.47	0.32	-2.53	5.28	2.83	1.37
attack Hostage Taking Barricade Incident	-0.02	0.58			2.32	2.47
attack Hostage Taking Kidnapping	0.49	0.48	-1.86	9.02	2.59	2.09
attack Unarmed Assault	0.27	0.31	-1.53	7.72	-0.73	1.34
attack Unknown	0.27	0.31			1.16	1.42
extended	-0.21	0.34			-0.35	1.58
group uncertain	0.03	0.06	0.23	1.24	-0.25	0.27
incident	-0.33	1.07	1.17	6.30	-0.05	4.64
individual	-0.09	0.30			1.27	1.28
international	0.01	0.09	-1.30	2.22	0.77	0.39
hostage/kidnapping	-0.26	0.41	-0.94	8.62	-3.74	1.77
media intensity MA(4)			-0.93	1.53	-0.04	0.10
multiple	-0.05	0.04	2.98	2.19	-0.06	0.18
no. hostages/kidnapped	0.02	0.05	-1.96	8.66	0.13	0.23
no. killed	0.00	0.00	4.00	2.52	0.00	0.02
no. killed terrorist	0.14	0.10			0.58	0.44
no. perpetrators captured	0.04	0.06	-1.17	0.78	-0.37	0.26
no. perpetrators	0.00	0.00	-0.72	0.86	0.00	0.01
no. wounded	0.00	0.00	3.11	0.86	-0.00	0.01
no. wounded terrorists	-0.21	0.23	-5.07	2.24	-0.10	0.98
property	0.04	0.05	3.80	1.59	0.02	0.22
province England	-0.11	1.04	0.39	6.28	-0.21	4.69
province Northern Ireland	-0.12	1.05	0.30	6.09	-0.44	4.69
province Scotland	-0.46	1.11			0.32	4.75
province Wales	-0.18	1.09			-1.10	4.82
ransom	-0.17	0.93			-2.14	4.05
ransom amount	0.00	0.00			0.00	0.00
ransom paid	0.00	0.00			0.00	0.00
success	0.00	0.03	-4.38	1.71	-0.03	0.15
suicide	-0.05	0.69			-1.27	3.07
time	-0.00	0.00	-0.00	0.00	-0.00	0.00
target Abortion Related	0.30	0.96			-3.13	4.10
target Airports/Aircraft	-0.24	0.28			1.03	1.23
target Business	0.11	0.18	-0.80	2.17	0.35	0.76
target Educational Institution	0.14	0.22			0.02	0.99
target Food or Water Supply	-0.36	0.96			1.02	4.25
target Government(Diplomatic)	0.14	0.38	1.82	8.40	2.12	1.59
target Government(General)	0.12	0.19	1.03	2.64	1.24	0.77
target Journalists/Media	-0.09	0.42			0.69	1.89
target Maritime	0.17	0.67			1.84	2.93
target Military	0.12	0.18	-0.18	3.00	0.57	0.76
target NGO	-0.12	0.41			0.56	1.78
target Other	-0.02	0.50			0.72	2.11
target Police	0.12	0.18	0.90	2.55	0.57	0.74
target Private Citizens(Property)	0.13	0.17	0.37	2.42	0.62	0.72
target Religious Figures/Institutions	-0.01	0.21	4.70	4.75	0.77	0.87
target Telecommunication	0.43	0.59			0.93	2.51
target Terrorists/Non-State Militia	0.09	0.20	1.81	2.85	0.22	0.86
target Tourists	0.26	0.35			-2.89	1.50
target Transportation	0.03	0.20	1.44	3.74	0.34	0.85
target Unknown	0.20	0.20	7.38	4.82	1.01	0.87
target Utilities	0.66	0.69			3.23	3.05
target Violent Political Party	0.03	0.63			1.94	2.62
weapsubtype Arson Fire	0.12	0.10	-2.05	1.74	0.02	0.42
weapsubtype Automatic Weapon	0.01	0.11	3.04	4.90	-0.04	0.50
weapsubtype Blunt Object	-0.14	0.17	0.88	4.00	-0.11	0.68
weapsubtype Gasoline/Alcohol	0.13	0.12	0.71	2.04	-0.94	0.50
weapsubtype Grenade	-0.03	0.19	6.30	4.45	0.15	0.85
weapsubtype Handgun	0.01	0.10	0.01	4.88	-0.19	0.42
weapsubtype Hands/Feet/Fists	0.05	0.31	-3.25	7.20	1.54	1.36
weapsubtype Knife/Other Sharp Object	0.19	0.24	3.11	4.21	-0.60	1.05
weapsubtype Land Mine	0.17	0.30			-0.23	1.25
weapsubtype Letter Bomb	0.05	0.13	-2.47	2.96	-0.51	0.57
weapsubtype Molotov Cocktail/Petrol Bomb	-0.11	0.13	-0.24	3.31	-0.01	0.59
weapsubtype Other Explosive Type	0.02	0.13	-3.97	2.88	-0.62	0.53
weapsubtype Pressure Trigger	0.62	0.87			2.15	3.93
weapsubtype Projectile rockets/mortars/RPGs/etc.	-0.03	0.13	-2.02	5.04	-0.63	0.54
weapsubtype Remote Trigger	0.54	0.28			-0.72	1.31
weapsubtype Rifle/Shotgun non automatic	-0.14	0.22			-0.74	0.95
weapsubtype Sticky Bomb	0.11	0.35			-1.93	1.45
weapsubtype Suicide carried bodily by human being	-0.08	0.55			-0.46	2.51
weapsubtype Time Fuse	-0.43	0.24	5.85	4.86	-0.91	1.02
weapsubtype Unknown Explosive Type	-0.03	0.10	-5.28	2.70	-1.07	0.43
weapsubtype Unknown Gun Type	-0.00	0.10	3.57	2.90	-0.49	0.44
weapsubtype Unknown Weapon Type	0.71	0.74			-1.15	3.18
weapsubtype Vehicle	0.10	0.12	-3.91	2.96	-0.42	0.53
N	1791		88		88	

LASSO Results

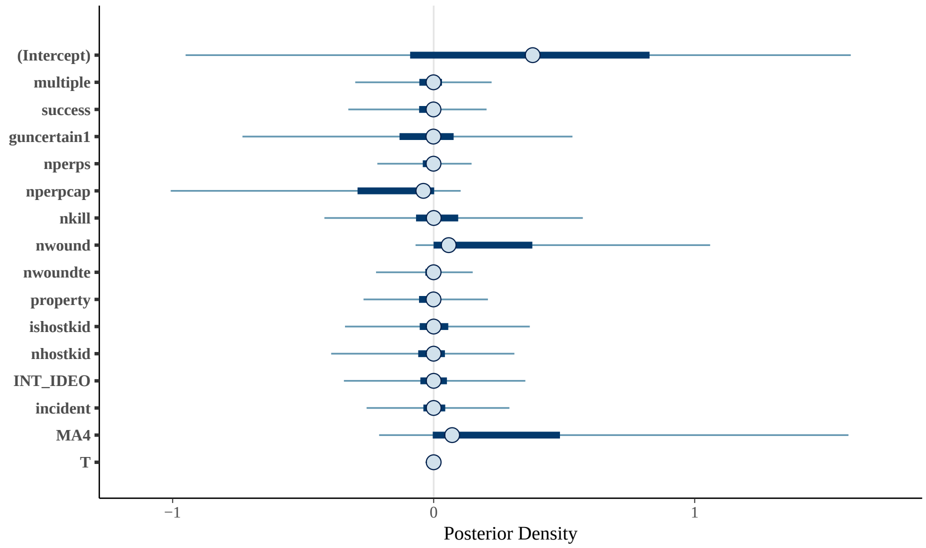

Figure A5. LASSO of 4-day Cumulative Abnormal Returns on event covariates.

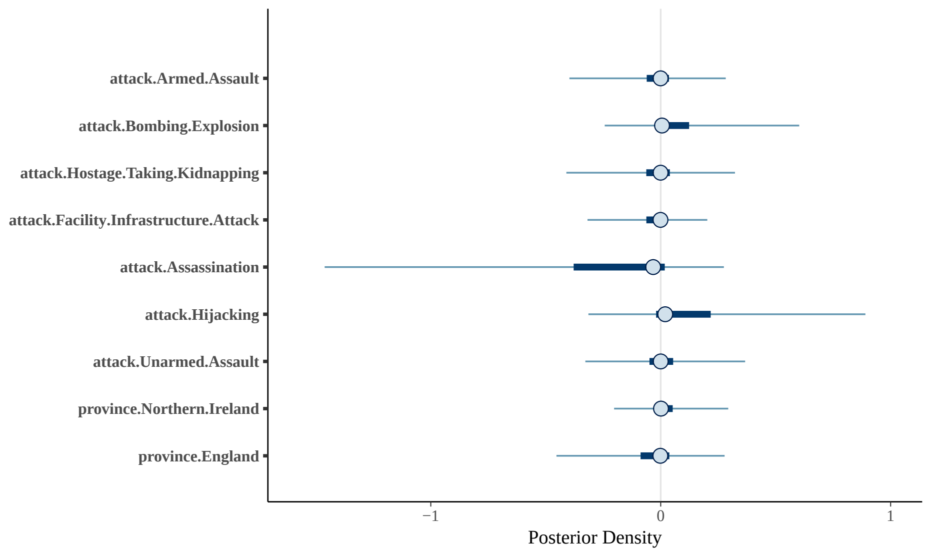

FIGURE A6. LASSO OF 4-DAY CUMULATIVE ABNORMAL RETURNS ON EVENT COVARIATES.

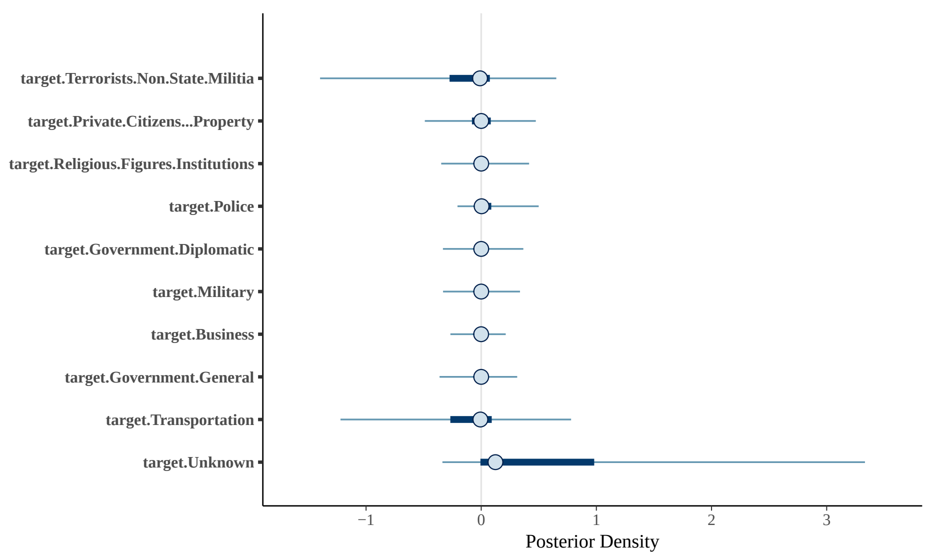

FIGURE A7. LASSO OF 4-DAY CUMULATIVE ABNORMAL RETURNS ON EVENT COVARIATES.

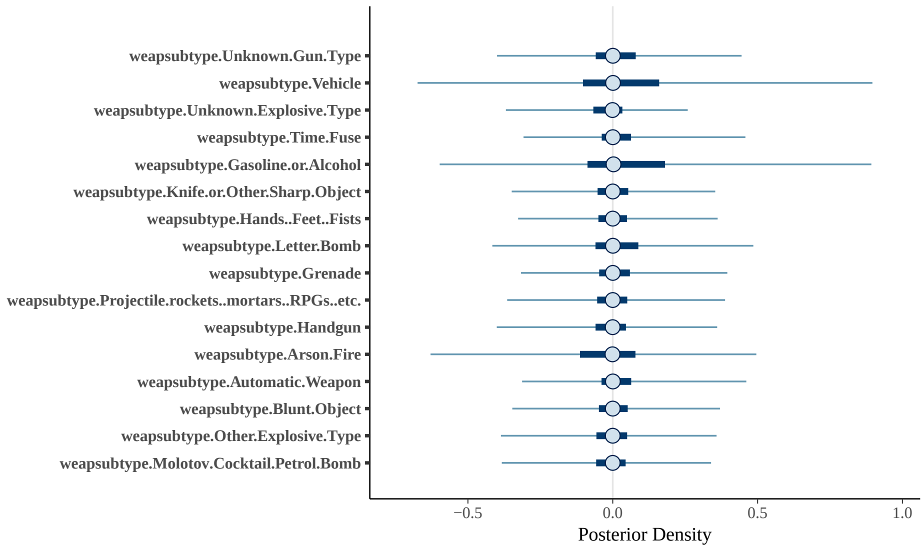

Figure A8. LASSO of 4-day Cumulative Abnormal Returns on event covariates.

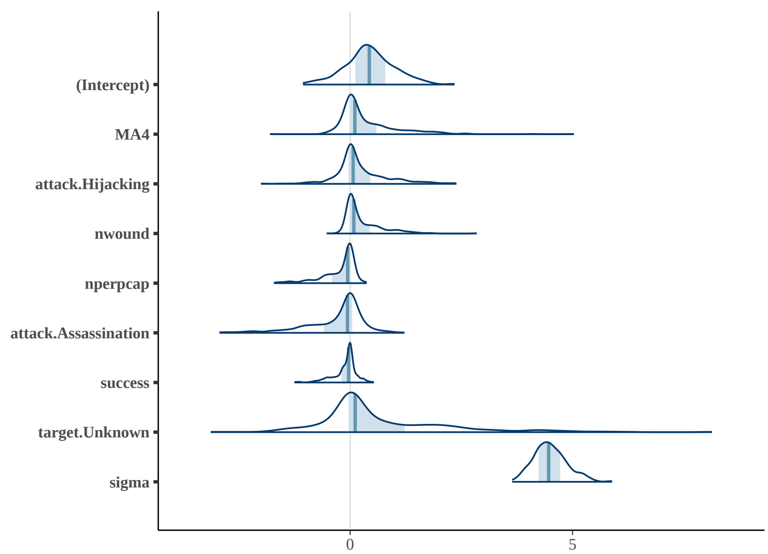

Figure A9. Projected LASSO sub-model using seven best predictors.