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The effects of terrorist attacks on inventor productivity and mobility☆

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A B S T R A C T

We investigate the impact of deadly terrorist attacks on inventor productivity and mobility in the U.S. During the five-year window after such events, nearby firms generate fewer and less impactful inventions. Moreover, their inventors typically exhibit a post-attack decline in their patent production, unless they move to a distant company (which some tend to do after an attack). Firms' financial constraints and inventor talent appear to provide channels underlying our productivity and mobility findings, respectively. These results provide novel insights about the impact of shocks that distort the invention process and promote the mobility and reallocation of inventors among firms.

1. Introduction

This paper empirically examines the effects of terrorism on inventor productivity and mobility for US firms. To evaluate these research questions, we propose two hypotheses. The first, *invention disruption*, follows from Abadie and Gardeazabal's (2008) theory showing that, in an open economy, terrorism accounts for much of the mobility of productive capital.[1] The empirical predictions from this hypothesis are that some inventors in the affected firms should be more likely to move to faraway companies. Moreover, while inventor productivity in firms near terrorist strikes should suffer a non-trivial decline, the productivity of inventors that relocate might not. The competing hypothesis is rooted on research in psychology showing resilient behavior that leads people to thrive after traumatic events (e.g., Bonnano, 2004). Under the *resilience* view, inventors in firms affected by a terrorism event will become more productive as they will work harder in order to increase job security at their current firms, to heed calls for regional unity, or both. Under these circumstances, inventor mobility after a terrorist attack should be limited.

To study our hypotheses, we construct a dataset consisting of innovating firms located near terrorist attacks and matching firms located at least 400 miles away from these events. Using an inventor-level dataset consisting of over 2.1 million inventor-year observations during our sample period, we examine the effects of terrorism on the generation of inventions. For this purpose, we use outcome variables like those in previous studies: the number of patents granted, and the number of patent citations (see, for example, Atanassov, 2013; Seru, 2014). We also assess the quality (value) of the inventions with the method outlined by Kogan et al. (2017), and the invention's novelty with the Trajtenberg et al. (1997) measures. Trajtenberg et al. (1997) identify patents that start a citation stream (Originality) and those that impact a wide range of succeeding patent classes (Generality).

Given the lifecycle of the invention process, we evaluate changes in inventor productivity and mobility during the five-year window

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[1] They use a stochastic variant of the AK theoretical model of economic growth anchored on one of the effects of terrorist attacks: the reduction of a country's capital stock (human and physical). In line with their theory, other authors find that worker productivity drops after terrorist attacks (see, for example, Bram et al., 2002).

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subsequent to a terrorist attack. We construct two proxies of terrorism based on a firm's proximity to an attack. The first flags cases in which the distance between a firm's headquarters and the strike's site is within 100 miles. The second proxy indicates whether corporate headquarters and the terrorist strike are in the same metropolitan statistical area (MSA). The rationale for these measures rests on the idea that the effect of terrorism increases as the distance from the attack's scene decreases (Ahern, 2018). Importantly, similar to the setting in Brav et al. (2018), the five-year window following the strike lessens the concern that inventions completed *before* an attack might distort our findings.

Brodeur (2018) argues that successful terrorist strikes (e.g., those that generate human losses) are more salient as they generate media attention. In our sample, all of the 122 terrorist attacks that exhibit deaths are covered by at least one of the six major US newspapers we consider.[2]

Using stacked difference-in-differences estimation, we find that within five years from a strike with at least one confirmed person killed, patents per employee and per inventor decline by 5.82 % and 1.69 %, respectively, for the average firm located within 100 miles from the terrorism scene. Confirming the results on inventor productivity at the firm level, we find a robust negative association between terrorism activity and invention activities at the inventor level. According to our tests, within a five-year period after a lethal terrorist attack, inventors in firms afflicted by the strike are associated with a decline in patents and citations of 2.18 % and 5.35 %, respectively.

Other analyses indicate that the value of the patents generated by inventors in attacked locations declines by 4.4 %. Moreover, after deadly attacks, inventors produce patents that integrate existing knowledge from *fewer* dissimilar areas and, as a result, have a lower originality value. In addition, we show that, on average, firms located within 100 miles of a fatal strike are associated with a 3.92 % reduction in the pool of inventors, a 1.98 % decline in new inventors hired, and a 3.05 % increase in inventors leaving the area.

Overall, our empirical evidence indicates that terrorism: (a) disrupts the invention process (as evidenced by a post-strike drop in several invention metrics at firms located near the attacks); and (b) promotes the mobility and reallocation of inventors among firms (as shown by the relocation of some inventors to companies far away from the stricken scenes).

We study potential channels underlying the effect of fatal terrorist attacks on inventor productivity and mobility. The results show that the post-terrorism decline in inventor productivity is particularly severe in financially constrained firms. Notably, constrained firms generate patents from *fewer* locations after an attack. This finding suggests that constrained firms might be less able to either outsource invention activities or conduct them far from the attacked sites and that either possibility is likely to uniquely (and adversely) affect the invention process. In addition, other tests show that among all inventors affected by a deadly terrorist attack, those classified as 'star' inventors are more likely to subsequently relocate to firms in distant locations. As such, we identify "inventor talent" as a channel underlying our mobility findings.

A question that arises is whether specific attributes of inventors (and the invention process) make inventors distinctly vulnerable to the effects of terrorist incidents or whether *all* high-skill individuals are equally affected by these events. One ideal experimental setting to address this question entails an out-of-sample test that examines the effect of terrorist attacks on a different creative activity. If, after applying our methodology on this activity, we find that terrorist attacks have either no effect or an opposite effect on the productivity of the individuals who perform that activity, then such evidence would make the empirical regularity we find more compelling.

While such an ideal test is not within our reach, there is academic work considering the effects of terrorism-like shocks (i.e., wars) on the creative process involving other activities. For example, Simonton (1975) rejects the hypothesis that the creativity of about 5000 diverse artistic individuals is a negative function of wars. More recently, Murray (2003) presents regression results suggesting that the impact of war on the accomplishments of notable visual artists, writers, and composers is *positive* and statistically significant. According to Murray, chaos triggered by war appears to boost the creativity and productivity of famous painters, writers, and composers as these individuals find inspiration in this environment. By contrast, in the presence of a comparable shock, we find that inventors become less productive, and their inventions turn less impactful (i.e., lower citations). We conjecture that this happens because, unlike other high-skill individuals engaged in different creative activities, chaotic environments do not enhance the invention process. Instead, the creative work of inventors is distinct as it often requires, among other things, sound knowledge of the state of the art, methodical research, sophisticated research equipment, and collaborative work. All these activities need a stable—non-chaotic—environment to thrive.

While productivity of prominent artistic creators improves during wars, the opposite is true for Scientists. To this end, Simonton (1980) provides quantitative time-series evidence of an inverse association between scientists' productivity and the occurrence of wars. Despite Simonton's (1980) findings, war and terrorism are undeniably different. Yet, the war-related findings discussed above together with our own results suggest that unique attributes of inventors (and the invention process) make inventors distinctly susceptible to the effects of chaotic events such as wars or terrorist attacks. Consistent with this view, Simonton (2014) discusses the attributes that make eminent artistic creators (e.g., writers, composers, and visual artists) different from each other as well as from other eminent scientific creators.

The main contribution of this paper is to provide new evidence on how terrorism uniquely affects inventor productivity and mobility.[3] In this vein, our findings deliver empirical support for the theoretical prediction by Abadie and Gardeazabal (2008) that terrorism triggers the reallocation of productive capital stock in an open economy. Importantly, while their predictions are formulated in the context of reallocation of capital stock across countries, our evidence indicates that such a reallocation can occur within a single country. In addition, our results on the adverse effect of terrorism on the generation of inventions in stricken areas provide a potential channel underlying the results in studies showing that terrorist events are associated with significant GDP declines (Blomberg et al., 2004; Bloom, 2009).

Our findings on financial constraints and on inventor talent as channels underlying our baseline findings, suggest rational decision-making by firms and inventors affected by a terrorism event. In this regard, our evidence complements the work by Ahern (2018) in which he conjectures that terrorism's key channel of influence must be psychological and by Becker and Rubinstein (2011) in which they posit that exposure to terror triggers intense personal fear about future attacks leading to reduced job satisfaction, participation, effort, learning, and creativity.[4]

[2] The NY Daily News, The NY Post, The NY Times, The Wall Street Journal, The Washington Post, and USA Today.

[3] Other characteristics known to affect invention activity include: competition (Aghion et al., 2005); bankruptcy laws (Acharya and Subramanian, 2009); private equity involvement (Lerner et al., 2011); analyst coverage (He and Tian, 2013); institutional ownership (Aghion et al., 2013); anti-takeover provisions (Atanassov, 2013); labor laws (Acharya et al., 2013, 2014); venture capital (Chemmanur et al., 2014); investors' attitudes toward failure (Tian and Wang, 2014); stock liquidity (Fang et al., 2014); firm boundaries (Seru, 2014); public offering decisions (Bernstein, 2015); employee stock options (Chang et al., 2015); banking competition (Cornaggia et al., 2015); lending relationships (Hombert and Matray, 2017); corporate taxes (Mukherjee et al., 2017); independent directors (Balsmeier et al., 2017), employment non-discrimination acts (Gao and Zhang, 2017); and hedge fund activism (Brav et al., 2018).

[4] This is also consistent with medical research which shows that, after terrorist attacks, emotional disorders weaken the productivity and economic stability of a firm's workforce (North and Pfefferbaum, 2002).

Table 1
Sample description.
This table presents the annual distribution by attack type (columns (1) through (9)), total number of attacks (column (10)), and the number of attacks with at least one death (column (11)).

	Assassination	Armed assault	Bombing/ explosion	Hijacking	Barricade incident	Kidnapping	Facility/ infrastructure	Unarmed assault	Unknown	Total attacks	Attacks with at least one death
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
1985	3	0	25	0	0	0	11	1	0	40	3
1986	2	1	32	0	0	0	13	1	0	49	1
1987	0	0	18	0	0	0	16	0	0	34	1
1988	0	0	15	0	1	0	11	0	0	27	1
1989	6	0	16	0	0	0	20	0	0	42	3
1990	4	0	15	0	0	0	13	0	0	32	4
1991	2	1	7	0	0	0	20	0	0	30	2
1992	5	1	3	0	1	0	22	0	0	32	2
1993	0	0	0	0	0	0	0	0	0	0	0
1994	8	7	8	0	10	0	21	0	1	55	8
1995	2	6	15	0	8	0	28	1	0	60	4
1996	2	3	14	0	0	0	15	1	0	35	2
1997	2	1	19	0	0	1	19	0	0	42	2
1998	0	2	9	0	0	0	19	1	0	31	3
1999	0	9	7	0	0	1	23	14	0	54	3
2000	0	9	5	0	1	0	25	2	0	42	2
2001	0	6	4	4	0	0	21	12	0	47	11
2002	0	2	20	1	0	0	10	0	0	33	1
2003	0	0	7	0	0	0	24	2	0	33	0
2004	0	0	0	0	0	0	9	0	0	9	0
2005	0	0	9	0	0	0	11	1	0	21	0
2006	0	1	2	0	0	0	2	1	0	6	1
2007	0	0	2	0	0	0	6	0	0	8	0
2008	0	2	2	0	0	0	12	2	0	18	1
2009	0	8	2	0	0	0	2	0	0	12	6
2010	0	1	3	0	1	0	11	1	0	17	1
2011	0	3	3	0	0	1	3	0	0	10	0
2012	0	3	2	0	0	0	15	0	0	20	1
2013	0	3	6	1	0	0	7	3	0	20	6
2014	0	18	5	0	0	0	5	1	0	29	9
2015	0	12	6	0	2	0	16	3	0	39	6
2016	2	18	8	0	1	0	35	4	0	68	6
2017	2	32	6	0	1	1	21	3	0	66	18
2018	0	16	25	0	0	0	27	7	0	75	5
2019	1	20	1	1	2	1	31	11	0	68	9
Total	41	185	321	7	28	5	544	72	1	1204	122

This paper complements recent work using the underlying exogeneity of terrorist attacks as the source of identification to study their effect on investors' risk preferences (Wang and Young, 2020), on the sentiment and forecasts of sell-side equity analysts (Cuculiza et al., 2021), and on CEO pay (Dai et al., 2020).[5] The latter paper, for instance, shows that after a terrorist strike, CEOs of firms located near the attack's scene get a sizable salary raise. The fact that some CEOs who get a pay raise stay with their firms amidst the chaos, while some inventors move faraway to remain productive, suggests that the terrorism effect is different for different types of high-skill individuals and is particularly detrimental for inventors.

The paper continues as follows. Section 2 describes our data and the measures of terrorist attacks, inventor productivity and mobility, and invention activity. Section 3 presents our main empirical analyses at both the firm and inventor level. Section 4 concludes. Appendix A provides the definition for all the variables we use in this study and the internet Appendix details our robustness tests.

2. Data and quasi-experimental design

2.1. Terrorism

We collect data on the date, location, and number of victims of each terrorist attack in the US between 1985 and 2019. This information is drawn from the Global Terrorism Database (GTD) maintained by the National Consortium for the Study of Terrorism and Responses to Terrorism (START). The GTD contains systematic records on domestic and international terrorism events. The attacks most often target businesses, private citizens, and property.[6]

[5] Other papers studying the economic effects of terrorism include the work by Abadie and Gardeazabal (2003) studying the stock price performance of firms with a significant part of their business in the Basque Country, by Burch et al. (2003) considering closed-end fund discounts and investor sentiment after the 9/11 attacks; by Di Telia and Schargrodsky (2004) and Draca et al. (2011) examining whether the deployment of law enforcement during terrorist attacks affects local crime rates, by Poteshman (2006) looking at option trading after the 9/11; by Krueger and Maleckova (2003) considering the association between terrorism, education, and poverty, by Arin et al. (2008) examining the effect of terrorism on stock market volatility; by Karolyi and Martell (2010) examining the stock price impact of terrorism during 1995–2002 with a list of 75 attacks compiled by the US Department of State; by Procasky and Ujah (2016) studying whether terrorism affects the cost of debt; by Kim and Kung (2017) determining how terrorism-induced uncertainty affects corporate investment under varying degrees of asset redeployability; and by Brodeur (2018) showing that terrorist strikes boost consumer pessimism.

[6] According to START, to be included in the GTD, events must involve a deliberate act of violence or threat of violence and, additionally, meet two of the following three criteria: (1) the attack aimed at attaining a political, economic, religious, or social goal; (2) the attack intended to coerce, intimidate, or deliver some other message to a larger audience (other than the immediate victims); and (3) the act was outside the precepts of International Humanitarian Law (particularly the warning against deliberately targeting civilians or noncombatants).

We verify all the attacks and the accuracy of the data pertaining to each event (e.g., location, casualties, perpetrators, etc.) by performing a manual search in US newspapers through Lexis-Nexis. Table 1 presents the temporal distribution and attack classification for the 1204 terrorism events in our sample. These incidents produced 3663 deaths and 24,788 injuries in total. Attacks are categorized by either the type of incident (e.g., explosion) or by the characteristics of the target (e.g., airplane hijacking). At 544, attacks of facility/infrastructure (e.g., the October 9, 1995, Palo Verde, AZ train derailment with a toll of 1 killed and 78 injured individuals) is the most common type of terrorism event. There are five kidnapping events in the sample. The year 1993 shows no incidents. In contrast, 2018 is the year with most terrorist attacks (at 75) which includes the Austin (Texas) serial bombings in which several package bombs exploded, killing two people and injuring another five. Notably, in our sample, 122 attacks are independently responsible for at least one confirmed person killed.

Existing work notes that the impact of terrorist attacks is stronger for individuals closer to the incident's location (e.g., Ahern, 2018). With this logic in mind, we create an indicator variable, labeled "*attack vicinity within 100 miles*," that is set to one if firm *i* is headquartered within 100 miles of terrorism scene *j*.[7] We match company location data with information from the US Census Bureau's Gazetteers and Zip Code Database to obtain details on the latitude and longitude of the firms and terrorist incident sites. Following the procedure in Vincenty (1975), we use this information to calculate the distance between a firm's headquarters and a terrorist attack location. Recent studies note that headquarter sites provide a good approximation of a firm's most important economic activities (see, for example, Barrot and Sauvagnat, 2016; Dougal et al., 2015; Grieser et al., 2022; Pirinsky and Wang, 2006). Accordingly, we assume that these are the places where invention activity occurs. Moreover, to increase the likelihood that invention activity occurs at the headquarters site, we follow Almazan et al. (2010) and retain firms for which a high percentage of their assets and employees are at the headquarter location. Notably, robustness tests suggest that any error-in-measurement related to our invention activity (headquarter) location assumption does not materially alter our findings (see Internet Appendix, section B.5).

We also define local firms as those headquartered in the same Metropolitan Statistical Area (MSA) as the attack. We identify MSAs with information provided by the US Census Bureau. According to the Office of Management and Budget (OMB), an MSA consists of a "core area that contains a substantial population nucleus, together with adjacent communities that have a high degree of social and economic integration with that core."

2.2. *Inventor productivity and mobility*

We evaluate the effects of terrorism on both inventor productivity and mobility at the firm and inventor level.

2.2.1. *Productivity*

To measure inventor productivity at the firm level, we collect patent characteristics from the datasets created by Kogan et al. (2017) and Stoffman et al. (2022), respectively. Combined, these datasets contain information for all patent applications filed with (and eventually granted by) the US Patent and Trademark Office (USPTO) from 1926 to 2021. Both datasets provide identifiers for each filing firm which we use to merge the patent data with CRSP and Compustat. We focus on the patent filing year because, as Griliches et al. (1987) note, the filing (rather than the grant) year better captures the actual time when an invention is generated. Focusing on the filing date also addresses the concern of potential anomalies that may arise due to lags between the application and granting dates (two years, on average).

We track patent output (the number of patents granted) as it is a widely accepted measure of inventor productivity (Hall et al., 2001). We follow Hall et al. (2001, 2005) and adjust patent counts by weighting each patent by the mean number of patents granted in the same year and technology class. Hence, patents granted in fields with more patent activity receive less weight. We also exclude 2021 because the truncation bias is probably the most severe in that year. As a result, our patent-based sample spans the period 1986 to 2020. There is a one-year lag between the terrorism sample and the patent-based sample because we examine the response of the invention variables in year $t + 1$ to a terrorist attack that occurs in year t.

We use two measures to track invention productivity within the firm: i) the natural logarithm of the number of patents per 1000 firm employees, plus one; and ii) the natural logarithm of the number of patents scaled by the number of inventors, plus one. The later variable intends to capture the people more likely to be involved in the invention process. Our measures of invention productivity within the firm are based on inventors who are awarded a patent on behalf of their firm in a given year and are not awarded another patent on behalf of a different firm during the same year. This criterion ensures that our inventor-firm match is correct.

To measure invention productivity at the inventor level, we create a time-series for inventors, using data from the USPTO covering approximately 7.7 million patent records and 4.3 million inventors from 1975 until 2020.[8] As in Baghai et al. (2019), we start by identifying the first and last year an inventor appears in the patent database. We then assign a value of zero to the inventor's output variables for the years in between and without any patent record. Every inventor is also matched to a patent's assignee (the firm listed in the patent's application). This procedure generates over 2.1 million inventor-year observations during our sample period.

Some of our analyses use five invention productivity outcome variables at the inventor level. To define them, we collect patent and citation characteristics from the datasets created by Kogan et al. (2017) and Stoffman et al. (2022), respectively. We track patent output (the number of patents granted) as it is a widely accepted measure of inventor productivity (Hall et al., 2001). We also assess the novelty of a patent with the number of citations it receives after the grant date and follow the Hall et al. (2001 & 2005) process of adjustment and truncation described above by also excluding the last year of the dataset when the truncation bias is most severe.

We measure the quality of an invention (or patent's dollar value) with the firm's market-adjusted stock return running from the day of the patent approval announcement date until two days after (t, $t + 2$), multiplied by the firm's market capitalization on the day prior to the announcement (t-1). We assess the importance of an invention with the Trajtenberg et al. (1997) measures of Originality and Generality. The first measure identifies patents that start a citation stream. The second captures patents that influence an extensive range of succeeding patent classes.

2.2.2. *Mobility*

With data from Kogan et al. (2017), Stoffman et al. (2022) and the USPTO, we identify inventors that move to a different company. These data include the names of the inventors for every patent as well as the identity of their employer but not consistent listings of inventor names or unique inventor identifiers. Fortunately, through a disambiguation

[7] Using distances of 50 or 200 miles from the attack's site does not alter our results.

[8] Please see Li et al. (2014). These data along with accompanying programs are available from the UC-Berkeley Fung Institute for Engineering Leadership.

algorithm, Lai et al. (2009) and Li et al. (2014) generate unique inventor identifiers,[9] which we use to follow individual inventors over time.[10] Next, we isolate one observation per inventor-employer-year (hence, we drop $n-1$ observations whenever the same inventor files n patents during the same year with the same employer). The unit of analysis is each pair of subsequent patents filed by every unique inventor. The year associated with each observation is the midpoint between the year in which the first patent is filed and the year in which the subsequent patent is filed (inventors who appear only once are excluded). This process enables us to detect an employer switch (and the timing of such a switch) when the inventor's employer changes identity from one patent to another. We find that 15.21 % of observations are associated with a move.

We create three variables to measure mobility at the inventor level under the premise that an inventor has moved if she filed a patent for company *A* and later files a patent for company *B*. The first variable, "*Ln (Distance of the Move + 1)*", is the natural logarithm of inventor's distance of the move in miles to a new employer from her previous employer. The second variable is an indicator variable, "*Over-100-Miles Move*", which equals one if companies *A* and *B* are located more than 100 miles from one another. The indicator is set to zero whenever the distance between companies *A* and *B* is less than 100 miles. The third variable, "*Out-of-MSA Move,*" is set to one if companies *A* and *B* are in different MSAs and set to zero if they are in the same MSA.

To measure inventor mobility at the firm level, we use three different proxies: i) the natural logarithm of the number of inventors plus one, adding inventors in a firm's staff in a given year; ii) the natural logarithm of the number of new hires plus one, tracking inventors hired from another firm in a given year; and iii) the natural logarithm of the number of leavers plus one, counting inventors who move to another firm in a given year. We track new hires and leavers by verifying subsequent patents filed by the same inventor within one year.

The procedure we use to detect inventor moves is subject to some caveats. First, our method is unable to detect whether inventors move to other locations within the same company. This issue will prevent us from correctly flagging an inventor's move to a location far away from an attacked scene. Second, we drop inventors that appear only once in the inventor-employer-year dataset. Yet, it is possible that a dropped inventor goes to another firm but has no subsequent patent filings. Third, our inventor mobility detection process relies on the accuracy of the inventor-employer association recorded in granted patents. However, even if this information is generally correct, it is still possible that an inventor of record in a granted patent leaves her firm during the patent review period. This issue will lead us to miss inventors' moves. Fourth, as noted earlier, to increase the likelihood that inventors work at the headquarter location, we follow Almazan et al. (2010) and analyze firms in which a high percentage of their assets and employees are in the firm's headquarters. Yet, it is possible that some firms conduct invention activities elsewhere within the firm. Notably, for our purposes, all these error-in-measurement issues are likely to prevent us from uncovering any effects stemming from inventors that switch employers as they create a downward bias that understates the magnitude of our mobility measures.

Panel A of Fig. 1 provides a map of the United States where we identify the location of the 122 terrorist attacks with confirmed deaths. In Panel B of Fig. 1, we use a similar map to identify the inventor relocation sites. Some inventors relocate close to locations attacked at some point during our sample period. We see this pattern in the NY-NJ-CT 'tri-state' area around New York City, in the vicinity of San Francisco-Silicon Valley, in the suburbs adjacent to Dallas, TX, and in small cities facing the southwest area of Lake Michigan (from Southern Wisconsin to Chicago). Nonetheless, most inventors end up in locations without a death-producing terror attack during our sample period. These places include small cities in Vermont and New Hampshire near the Canadian border, in Montana, Idaho, and Western Washington State, in Oregon, and in South Carolina and Tennessee. While these locales are not as densely populated as the attacked sites, they are close to major cities. Notably, the map does not suggest that inventors are willing to return to some attacked areas (e.g., Western Texas, Southern Mississippi, and Southern Oregon).

2.3. Sample overview

We merge the databases described in the previous sections to form our main sample. The sample includes potentially innovative firms, defined as those that: i) file at least one patent that is eventually granted prior to the year of a terrorist attack; and ii) experience at least one positive R&D expenditure within the five-year window before the attack.

Our main analyses rely on the pooled sample of innovative (treated) firms that experience a terrorist attack associated with at least one death and innovative (control) firms matched by propensity scores. Aside from being innovative, firms become candidates to enter the control group if, in the year of the terrorist attack, they are sited in a region with similar population density as the region that suffered a deadly strike, they are at least 400 miles away from the treated firm, and they operate in the same 2-digit SIC industry as the treated firm. To create the control group, we first estimate propensity scores through probit regressions, $p(Y = 1/X = x)$, based on the probability of receiving the treatment, Y, conditional on a vector of firm characteristics, x. These characteristics include size, Tobin's Q, cash holdings, leverage, return on assets (ROA), tangible assets, capital expenditures, the natural logarithm of firm age, H-index, and H-index^2.

For each terrorism-affected firm-year, we then use the propensity score to find a control firm-year based on the nearest-neighbor method (i.e., one-to-one matching) without replacement.[11] The event year for a terrorism-afflicted firm also serves as the "pseudo-event" year for its matched firm. To ensure the adequacy of the matching estimation, the absolute difference in propensity scores among pairs cannot exceed 0.05. If there are multiple control firms-years that meet this criterion, we retain the firm-year with the *smallest* propensity score difference.

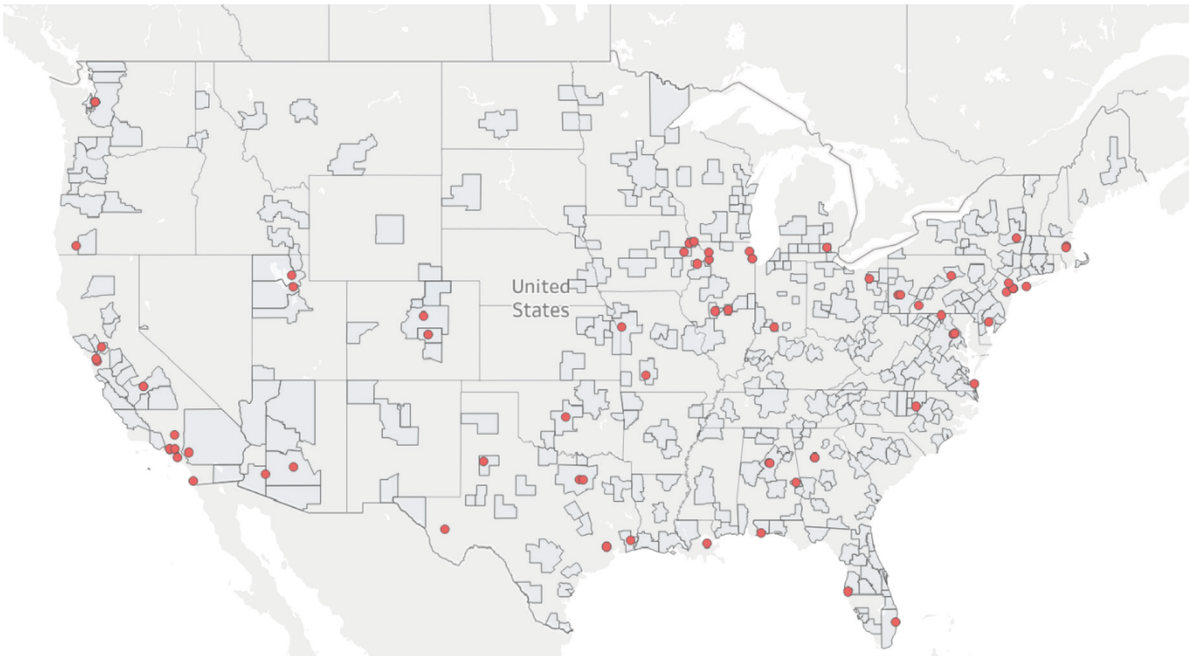
Our focus on attacks that generate deaths and the propensity score matching procedure we use combine to identify intrinsically similar control and matching firms. A concern that threatens this similarity—and, in turn, the identification assumption—is that the geographic scene of a terrorist attack is unlikely to be random. In our setting, both treated and control firms are similar, except for the fact that, unlike treated firms, control firms are not near a lethal attack. Yet, because treated and matching firms exhibit similar attributes in terms of their average number of employees, their industrial sector, and their location's population density, matched firms are just as likely to be near a deadly attack as their treated counterparts. This issue matters because, conditional on being in a location attractive for terrorist activity, the

[9] See: https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/YJUNUN.

[10] Specifically, we use the dataset from Li et al. (2014), which contains disambiguated inventor names and unique inventor IDs that enable us to track inventors across firms. For example, an inventor that files a patent with firm A in 1999 and one with firm B in 2000 is designated as an employee of firm A in 1999 and as an employee of firm B in 2000. If more than one year elapse between two patent filings, we assume that the employment transition between the two firms occurs at the midpoint between the patent application years. Accordingly, if an inventor employed by firm A is granted a patent in 1995 and a different patent while employed by firm B in 2000, and has no other patents granted in the interim, we assume that the inventor is employed by firm A until 1997 and by firm B from 1998 onwards. In the analysis, we identify an inventor's employer with the Compustat GVKEY recorded in the patent.

[11] To be thorough, we also use 3- and 5-nearest-neighbors matching estimators and obtain similar results.

Panel A: The geographical distribution of terrorist attacks



Panel B: Areas where inventors relocate

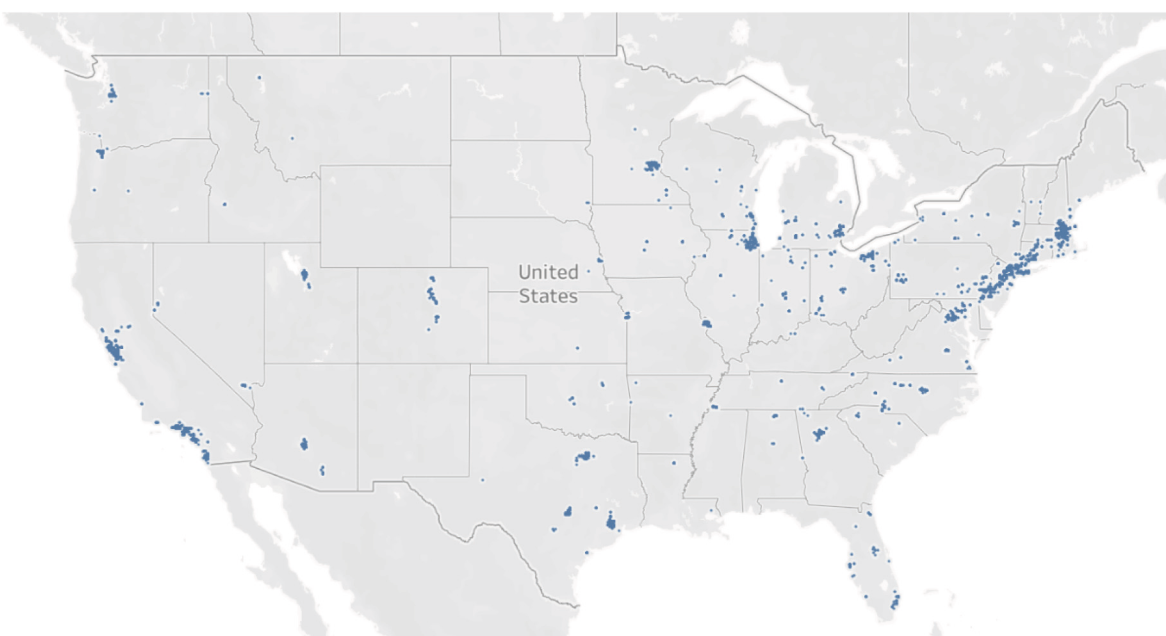


Fig. 1. Terrorist attack sites and inventor mobility.
Panel A identifies the sites of 122 deadly terrorist attacks and Panel B identifies the areas where inventors relocate after those attacks.

extent to which an attack ultimately kills people is random and therefore plausibly exogenous. Therefore, since we allow matched firms to be near the scene of non-fatal attacks, our setting most likely satisfies the identification assumption. Moreover, contrasting firms near strike sites with deaths with those in places just as vulnerable to a similar attack avoids the need to control for other unobservable location characteristics. Overall, the criteria we use to identify our sample alleviate the concern that other intrinsic differences between treated and control firms might drive our findings.

Table 2, Panel A reports summary statistics for observations related to attacks within 100 miles away from the firm's headquarters (for 1658 treated firms and 1658 matched firms), whereas Panel B presents the statistics related to attacks in the same MSA as the firm's headquarters (for 1545 treated firms and 1545 matched firms). As is the case for all

Table 2

Summary statistics for innovating firms and the matched control sample.

This table reports firm characteristics at the firm-year level for the subsample of innovating firms defined as firms that filed for at least one patent that was eventually granted prior to the year of the terrorist attack with at least one positive R&D expenditure within the five-year window prior to the attack and for the control sample. A firm enters the treatment group if it is headquartered within 100 miles (MSA) of the location of the terrorism event that occurs at time *t* and has not experienced other terrorist attacks during the previous five years. The matching control group consists of innovative firms which, during the year of the attack, are sited in a region with similar population density as the region that suffered a lethal strike, are located at least 400 miles away from the strike and operate in the same 2-digit SIC industry as its corresponding treated firm. The event year for a terrorism-afflicted firm also serves as the "pseudo-event" year for its matched firm. We match firms using one-to-one nearest neighbor propensity score matching without replacement, where the propensity score is estimated using size, Tobin's Q, cash holdings, leverage, return on assets (ROA), tangible assets, capital expenditures, ln (firm age), H-index, and H-index^2. Panel A reports statistics for the cohort in which treated firms are within 100 miles from an attack and Panel B for the cohort in which treated firms are located in the same MSA of an attack. The detailed definitions of all variables are provided in the Appendix A. The variable values are measured as of the year prior to the terrorist attack. For each variable, we report the mean, standard deviation, 25th, 50th, and 75th percentiles. We also report the *t*-statistics for the differences in mean values between the treated and matched control firms.

	Panel A: attack vicinity within 100 miles											
	Treatment (N = 1658)					Control (N = 1658)					Difference	
	Mean	Std. dev.	p25	p50	p75	Mean	Std. dev.	p25	p50	p75	Mean	*t*-Statistic
Firm and industry variables												
Size	4.792	1.956	3.425	4.618	6.061	5.248	2.073	3.743	5.079	6.651	−0.455	−1.531
Tobin's Q	2.297	1.812	1.143	1.602	2.653	2.187	1.662	1.148	1.592	2.518	0.110	1.110
Cash holdings	0.235	0.249	0.030	0.133	0.380	0.212	0.230	0.032	0.122	0.323	0.023	1.161
Leverage	0.184	0.187	0.014	0.140	0.292	0.191	0.188	0.017	0.155	0.305	−0.008	−0.875
ROA	0.032	0.245	−0.013	0.109	0.175	0.043	0.218	0.023	0.112	0.172	−0.011	−1.138
Tangible assets	0.448	0.295	0.220	0.393	0.618	0.482	0.309	0.247	0.423	0.657	−0.034	−0.984
Capital expenditures	0.057	0.049	0.024	0.044	0.074	0.049	0.043	0.021	0.038	0.065	0.007	1.097
Ln (firm age)	1.995	1.121	1.099	1.946	2.890	2.150	0.947	1.946	2.639	3.219	−0.155	−1.106
H-index	0.194	0.163	0.084	0.130	0.270	0.200	0.167	0.084	0.134	0.275	−0.005	−0.612
H-index^2	0.064	0.127	0.007	0.017	0.073	0.068	0.133	0.007	0.018	0.076	−0.004	−1.257
Number of employees	6.227	26.011	0.171	0.660	3.102	7.294	24.659	0.224	0.997	4.325	−1.067	−1.361
Location variables												
Population	1,279,925	1,575,332	296,232	788,500	1,454,868	1,221,559	1,412,685	446,276	859,718	1,510,515	58,366	0.743
Population density	2868.619	6050.993	693.395	1870.431	2752.020	2758.495	9405.525	416.873	892.026	1332.507	110.124	0.266
Firm-level invention												
Ln (#patents/employees+1)	0.823	1.108	0.000	0.200	1.406	0.845	1.194	0.000	0.066	1.451	−0.022	−1.064
Ln (#patents/inventors+1)	0.238	0.266	0.000	0.199	0.432	0.252	0.294	0.000	0.091	0.467	−0.014	−0.805
Ln (#inventors+1)	1.030	1.262	0.000	0.603	1.786	1.193	1.554	0.000	0.620	2.061	−0.164	−1.376
Ln (#leavers+1)	0.231	0.563	0.000	0.000	0.000	0.313	0.723	0.000	0.000	0.000	−0.082	−1.215
Ln (#new hires+1)	0.326	0.787	0.000	0.000	0.000	0.359	0.822	0.000	0.000	0.000	−0.033	−1.324
Inventor-level invention												
Ln (#patents+1)	0.824	1.107	0.000	0.202	1.410	0.839	1.190	0.000	0.061	1.434	−0.015	−1.056
Ln (#citations+1)	0.238	0.266	0.000	0.205	0.431	0.251	0.294	0.000	0.000	0.465	−0.013	−1.341
Ln (invention value+1)	1.031	1.261	0.000	0.603	1.786	1.188	1.548	0.000	0.000	2.061	−0.157	−1.534
Generality	0.231	0.561	0.000	0.000	0.000	0.310	0.716	0.000	0.000	0.000	−0.079	−1.516
Originality	0.327	0.787	0.000	0.000	0.000	0.357	0.817	0.000	0.000	0.000	−0.030	−1.463

	Panel B: attack vicinity within MSA											
	Treatment (N = 1545)					Control (N = 1545)					Difference	
	Mean	Std. dev.	p25	p50	p75	Mean	Std. dev.	p25	p50	p75	Mean	*t*-Statistic
Firm and industry variables												
Size	5.147	1.911	3.458	4.549	5.912	5.215	1.844	3.863	5.013	6.438	−0.068	−1.574
Tobin's Q	2.321	2.468	1.158	1.616	2.575	2.443	3.035	1.204	1.650	2.648	−0.121	−0.797
Cash holdings	0.259	0.258	0.043	0.175	0.415	0.246	0.262	0.033	0.128	0.404	0.012	0.865
Leverage	0.175	0.211	0.005	0.120	0.284	0.189	0.205	0.011	0.146	0.300	−0.015	−1.275
ROA	0.018	0.284	−0.012	0.097	0.164	0.036	0.253	0.002	0.107	0.167	−0.018	−1.214
Tangible assets	0.436	0.353	0.202	0.369	0.594	0.434	0.291	0.212	0.367	0.596	0.001	0.071
Capital expenditures	0.050	0.051	0.019	0.036	0.063	0.047	0.050	0.019	0.036	0.060	0.003	1.069
Ln (firm age)	2.477	0.948	2.079	2.565	3.135	2.446	0.985	1.792	2.565	3.178	0.031	0.577
H-index	0.192	0.177	0.083	0.117	0.272	0.184	0.157	0.083	0.123	0.256	0.008	0.889
H-index^2	0.068	0.141	0.007	0.014	0.074	0.059	0.124	0.007	0.015	0.066	0.010	1.304

(*continued on next page*)

Table 2 (*continued*)

	Panel B: attack vicinity within MSA											
	Treatment ($N = 1545$)					Control (N = 1545)					Difference	
	Mean	Std. dev.	p25	p50	p75	Mean	Std. dev.	p25	p50	p75	Mean	t-Statistic
Number of employees	5.729	15.457	0.206	0.810	4.300	4.766	15.575	0.152	0.508	2.293	0.963	1.123
Location variables												
Population	1,443,075	1,601,700	378,547	1,020,286	1,891,328	1,321,495	1,664,644	403,164	891,356	1,561,366	121,580	1.354
Population density	2487.773	4851.008	419.147	3014.673	3557.917	2317.008	4510.491	588.572	1357.130	2104.428	170.765	0.660
Firm-level invention												
Ln (#patents/employees+1)	0.727	1.154	0.000	0.000	1.247	0.909	1.100	0.000	0.451	1.590	−0.183	−2.947
Ln (#patents/inventors+1)	0.193	0.271	0.000	0.000	0.405	0.216	0.231	0.000	0.208	0.356	−0.023	−1.677
Ln (#inventors+1)	0.900	1.433	0.000	0.000	1.386	1.241	1.355	0.000	0.947	2.067	−0.340	−4.440
Ln (#leavers+1)	0.244	0.664	0.000	0.000	0.000	0.313	0.646	0.000	0.000	0.693	−0.068	−1.900
Ln (#new hires+1)	0.220	0.600	0.000	0.000	0.000	0.261	0.550	0.000	0.000	0.000	−0.040	−1.268
Inventor-level invention												
Ln (#patents+1)	0.565	0.500	0.000	0.693	0.693	0.580	0.509	0.000	0.693	0.693	−0.014	−0.769
Ln (#citations+1)	1.588	1.637	0.000	1.435	2.904	1.493	1.622	0.000	1.182	2.779	0.095	1.096
Ln (invention value+1)	1.616	1.511	0.000	1.574	2.735	1.769	1.621	0.000	1.772	2.959	−0.153	−1.442
Generality	0.443	2.464	0.000	0.127	0.722	0.424	0.886	0.000	0.000	0.679	0.019	0.839
Originality	0.514	0.976	0.000	0.268	0.778	0.517	0.844	0.000	0.312	0.766	−0.003	−0.323

other firm characteristics, including the average number of employees and the location's population, none of the differences in the firm-level inventor or inventor productivity variables are either economically or statistically significant even though these variables are not part of the matching criteria.[12] The pre-attack similarity in the summary statistics for the inventor-level invention variables suggests that our data satisfy the "parallel trends" condition which is necessary to ensure internal validity of difference-in-differences estimates. Section 3.6 describes other tests related to parallel trends. In addition, the similarity in the number of employees and in the population statistics between treated and control samples lessens the concern that terrorists deliberately perpetrate attacks in places in which a strike has a better chance to kill many people.

2.4. Quasi-experimental design

The sample consists of firm-year level observations of innovative firms located in the vicinity of a terrorist attack that generates at least one confirmed death and their (untreated) matched firms. In this setting, treatment effects from two-group/two-period fixed effect difference-in-differences (DiD) ordinary least squares (OLS) regressions are often biased. This issue arises because the two-way fixed effect (TWFE) design uses units treated at two different points in time, with later-treated groups acting as a control before treatment occurs and the earlier-treated groups potentially serving as a control after their treatment happens. Moreover, depending on their location, some units might be treated more than once. Consequently, when the treatment varies across time within treated units, some of the TWFE estimates enter the average with negative weights (Barrios, 2021). As a result, in the absence of homogeneous treatment effects, the traditional TWFE DiD estimator is not a reliable source of unbiased average treatment effects. Fortunately, according to Goodman-Bacon (2021), using a *stacked* DiD approach in these situations may be more appropriate. Baker et al. (2021) note that by stacking and aligning events in event-time, this approach is akin to a situation where all events occur at once. Therefore, we follow Cengiz et al. (2019) and Deshpande et al. (2021) and estimate stacked event-by-event DiD models.

To implement the stacked DiD method, we use the following strategy. We first create 122 separate event-specific datasets of lethal terrorist attacks. Each event h dataset includes firms treated by attack h and 'clean' control matching firms for a 6-year panel by event time (t—1 *to* $t+5$) around the respective attack. Clean control firms are matching firms not in the vicinity of any attack with confirmed deaths during our sample period.[13] Treated firms are those impacted only by the attack of interest during our sample period. These strict restrictions for acceptable control and treatment groups prevent heterogenous treatment problems as they ensure that previously treated firms never serve as good controls for firms impacted by future attacks. We then stack all of the event-specific datasets in relative time to calculate an average treatment effect across the 122 attacks in which persons die.

Our primary interest is to evaluate how terrorist attacks with confirmed deaths affect inventor productivity and mobility. To do so, Eq. (1) presents our stacked DiD regression framework.

[12] Patent-data-based samples like ours have been extensively used in previous studies, so we refrain from discussing descriptive statistics but verify that they are in line with prior studies (see, for example, Balsmeier et al., 2017; Chang et al., 2015; Cornaggia et al., 2015; Mukherjee et al., 2017).

[13] Our results are similar when the control group consists exclusively of (a) firms in the vicinity of non-death producing attacks, or (b) firms located in vicinities without any attack.

$$\text{Inventor Productivity (Mobility)}_{i,l,t+1} = \alpha_t + \alpha_i + \beta_1\, Attack\ Vicinity_{i,l,t} {}^{*} Post_{i,l,t} + \beta_2\, Post_{i,l,t} + \gamma\, X_{i,l,t} + \in_{i,l,t+1} \tag{1}$$

where i indexes firms, l indexes the location of the firm, and t indexes time. *Attack Vicinity* $_{i,\ l,\ t}$ is an indicator variable that equals one if a firm is located within 100 miles (or within the MSA) of the attack. *Post* $_{i,\ l,\ t}$ is a dummy equal to one if the firm-year (i, t) observation is within [$t+1$, $t+5$] years of a terrorist attack (for treated firms) or a pseudo-event year (for matched firms).[14] Notably, as in Brav et al. (2018), allowing for a five-year window subsequent to a terrorism event mitigates the concern that invention activities completed prior to the treatment might alter our estimates.

Our main variable of interest is the interaction term *Attack Vicinity* $_{i,\ l,\ t}$ * *Post* $_{i,\ l,\ t}$, which captures the differential change in inventor productivity and mobility for firms subject to deadly terrorist attacks, compared to that for matched firms. We use the vector X $_{i,\ l,\ t}$ to control for several variables such as firm size, Tobin's Q, cash holdings, leverage, ROA, tangible assets, capital expenditures, ln (firm age), H-index, and H-index^2. We winsorize these controls at the 1st and 99th percentiles.

Since we analyze firm-year observations, the question is whether inventor productivity and mobility change after a local terrorist attack. Therefore, our test must identify the change in inventor productivity and mobility for the same firm before and after a terrorist attack, compared to other firms that are not located near a lethal attack. For this purpose, Eq. (1) controls for time-invariant unobservable firm characteristics with firm fixed effects, α_i. Eq. (1) also includes year indicator variables α_t to control for economy-wide shocks.

We also estimate Eq. (1) with higher order fixed effects to control for unobserved firm heterogeneity, time-varying differences across regions, and time-varying differences across industries by including firm (α_i) and year (t), region-by-year ($\omega_{p,t}$), and 2-digit SIC industry-by-year ($\lambda_{z,t}$) fixed effects for a firm i, located in region p, operating in industry z, at time t. As in Acharya et al. (2014, p. 322), we distinguish 4 US regions (Northeast, South, Midwest, and West) following the classification of the US Census Bureau. Angrist and Pischke (2009) and Gormley and Matsa (2014) argue that including control variables in the presence of high order fixed effects may lead to biased parameter estimates. Therefore, in the estimations that use the high order fixed effects, we suppress all control variables. In all tests, we follow Petersen's (2009) advice to control for serial correlation with robust Rogers (1993) standard errors clustered at the MSA level.[15]

3. Empirical results

3.1. Employee and inventor productivity (firm-level analysis)

Table 3 reports sixteen difference-in-differences regressions based on Eq. (1) to evaluate the effect of terrorism on employee and inventor productivity. The key independent variable, *Attack Vicinity * Post,* follows from Eq. (1) and we estimate it for firms located within 100 miles of the attacked site and for those within a stricken MSA. In Panel A of Table 3, the dependent variable in models (1)–(4) is the natural logarithm of the number of patents per 1000 firm employees, plus one. In Panel A, the dependent variable in models (5)–(8) is the natural logarithm of the number of patents scaled by the number of inventors, plus one. The odd-numbered tests omit the controls and use standard and multiplicative fixed effects while the even-numbered models include control variables and standard fixed effects. The dependent variables in Panel B are like those in Panel A, except that they are not subject to the natural logarithm transformation.

According to both panels in Table 3, both patents/employees and patents/inventors decrease significantly during the five-year period that follows a lethal terrorist attack. Looking at Panel A, an average firm headquartered within 100 miles from a terrorist attack location is related to a 5.82 % decline in patents/employee and a 1.69 % drop in patents/inventor. These effects rely on the *Attack Vicinity within 100 miles * Post* coefficients in columns (2) and (6), respectively. The estimates for terrorist attacks within an MSA lead to analogous inferences.

At first glance, it is a bit puzzling that the effect is much larger on a per employee basis than on a per inventor basis. Hence, a question that arises is whether the terrorism effect is concentrated in firms that have a high inventor/employee ratio or driven by a decline in that ratio for impacted firms. To shed light on this question, we examine the average inventor/employee ratio before and after the attack for both treated and matched groups. The table below presents the results.

Average inventor/ employee ratio	After attack (t + 1)	Before attack (t-1)	After - Before	t-statistic
Treated	1.127 %	1.495 %	−0.368 %	−7.07
Matched	1.398 %	1.434 %	−0.036 %	−1.24
Treated - matched	−0.271 %	0.061 %	−0.332 %	
t-Statistic	−5.29	1.51		

According to this test, firms near attacked locations experience a significant drop in their inventor/employee ratio. At just over 24 % (i.e., (1.127/1.495) – 1), the decline in the ratio is also economically meaningful and consistent with the overall decrease in invention productivity at the firm level. Importantly, the post-attack decline in the inventor/employee ratio for treated firms is consistent with our conjecture that inventors (and the invention process) are particularly vulnerable to terrorist attacks.

3.2. Inventor productivity (inventor-level analysis)

We now study the effect of terrorist attacks associated with confirmed deaths on inventor productivity with data at the inventor level. This alternative specification allows examining the effect of terrorist attacks on conventional outcome variables such as patents, citations, invention value, generality and originality. The inventor-level analyses appear in Table 4. The dependent variable is the natural logarithm of the number of patents plus one in models (1) and (2), the natural logarithm of the number of patent citations plus one in models (3) and (4), the natural logarithm of the invention's value plus one in models (5) and (6), the patent's generality in models (7) and (8), and the patent's originality in models (9) and (10).

In Table 4, the odd-numbered columns omit the control variables and contain both standard and multiplicative fixed effects (i.e., inventor and year fixed effects, region-by-year fixed effects, and industry-by-year fixed effects), while the even-numbered columns include control variables and standard fixed effects (i.e., inventor and year fixed effects).

[14] The results are robust if we instead use the three-year period following the event.

[15] Our baseline results are unaltered when we repeat all our empirical tests in regressions that simultaneously use all control variables and high-order fixed effects. These analyses are available upon request. Moreover, the results are similar if we use the suggestions by Bertrand et al. (2004) and cluster the standard errors at the state of location level or allow for correlated error terms at the state of incorporation level.

Table 3
Terrorist attacks and inventor productivity.
This table presents the effects of terrorist attacks on invention productivity of employees and inventors. The main variables of interest are the Attack vicinity within 100 miles * Post and the Attack vicinity within MSA * Post, respectively. In columns (1) through (4) of Panel A, the dependent variable is the natural logarithm of the number of patents per 1000 firm employees (EMP) plus one and measures invention productivity of firm employees in a given year. In columns (5) through (8) of Panel A, the dependent variable is the natural logarithm of the number of patents/inventors plus one and measures invention productivity of firm inventors in a given year. All control variables are lagged by one year. The detailed definitions of all variables are provided in the Appendix A. The odd-numbered columns omit the control variables and contain both standard and multiplicative fixed effects (i.e., firm and year fixed effects, region*year fixed effects, and industry*year fixed effects whose coefficients are suppressed), while the even-numbered columns include control variables and standard fixed effects (i.e., firm and year fixed effects, whose coefficients are suppressed). Standard errors, which are adjusted for heteroscedasticity and are clustered at MSA level, are reported in parentheses below the coefficient estimates. Panel B presents abbreviated regressions like those in Panel A in which the dependent variables are not log-transformed. The symbols ***, ** and * indicate statistical significance at the 1 %, 5 % and 10 % levels, respectively.

Panel A	Ln (#patents/employees+1)				Ln (#patents/inventors+1)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Attack vicinity within 100 miles*post	−0.049***	−0.060***			−0.017***	−0.017***		
	(0.016)	(0.015)			(0.004)	(0.004)		
Attack vicinity within MSA*post			−0.051***	−0.052***			−0.016***	−0.016***
			(0.016)	(0.016)			(0.004)	(0.004)
Post	0.012	0.004	0.013	−0.024	0.006	−0.003	−0.003	−0.011
	(0.021)	(0.019)	(0.021)	(0.020)	(0.005)	(0.005)	(0.005)	(0.015)
Size		0.054***		0.053***		0.043***		0.044***
		(0.019)		(0.020)		(0.004)		(0.004)
Tobin's Q		0.040***		0.042***		0.008***		0.008***
		(0.008)		(0.008)		(0.001)		(0.002)
Cash holdings		0.559***		0.551***		0.056***		0.052***
		(0.078)		(0.080)		(0.016)		(0.016)
Leverage		−0.395***		−0.396***		−0.069***		−0.074***
		(0.064)		(0.066)		(0.015)		(0.016)
ROA		−0.090		−0.088		0.004		0.002
		(0.076)		(0.080)		(0.014)		(0.015)
Tangible assets		−0.125*		−0.143**		0.011		0.009
		(0.067)		(0.069)		(0.016)		(0.016)
Capital expenditures		0.037		0.068		0.107**		0.119**
		(0.184)		(0.193)		(0.048)		(0.050)
Ln (firm age)		−0.108***		−0.106***		−0.011**		−0.011**
		(0.018)		(0.019)		(0.005)		(0.005)
H-index		−0.696***		−0.660***		−0.200***		−0.208***
		(0.252)		(0.255)		(0.077)		(0.078)
H-index^2		0.617**		0.571**		0.113		0.116
		(0.243)		(0.247)		(0.076)		(0.077)
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region*year fixed effects	Yes	No	Yes	No	Yes	No	Yes	No
Industry*year fixed effects	Yes	No	Yes	No	Yes	No	Yes	No
No. of obs.	28,180	28,180	26,302	26,302	28,180	28,180	26,302	26,302
Adjusted R^2	0.566	0.562	0.555	0.562	0.434	0.427	0.424	0.431

Panel B	#Patents/employees				#Patents/inventors			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Attack vicinity within 100 miles*post	−1.362***	−1.592***			−0.035***	−0.037***		
	(0.257)	(0.251)			(0.007)	(0.007)		
Attack vicinity within MSA*post			−1.091***	−1.222***			−0.030***	−0.030***
			(0.263)	(0.268)			(0.007)	(0.006)
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region*year fixed effects	Yes	No	Yes	No	Yes	No	Yes	No
Industry*year fixed effects	Yes	No	Yes	No	Yes	No	Yes	No
Control variables	No	Yes	No	Yes	No	Yes	No	Yes
No. of obs.	28,180	28,180	26,302	26,302	28,180	28,180	26,302	26,302
Adjusted R^2	0.441	0.437	0.425	0.428	0.391	0.383	0.383	0.387

Panel A reports results for inventors working within 100 miles of the terrorist attack whereas Panel B analyzes inventors located within a stricken MSA. The results with the inventor-level data show that deadly terrorist attacks reduce patenting, citations, invention value, and patent originality. Using the estimates in columns (1) and (3) of Panel A, during the five-year window following such terrorist attacks, inventors in local firms afflicted by the strike are associated with a decline in patents and citations of 2.18 % and 5.35 %, respectively. According to the estimates in column (5), the value of the patents generated by the same inventors drops by 4.4 %, which provides further evidence of reduction in inventor productivity after terrorist attacks. Results for the remaining tests in Table 4 show that the Originality (but not the Generality) of patents is

Table 4
The effect of terrorist attacks on corporate invention at the inventor level.
This table presents the effects of terrorist attacks on corporate invention using inventor-level data. Panel A reports the results for the effect of terrorist attacks on treated firms that are located within 100 miles from an attack and Panel B for the effect of terrorist attacks on treated firms that are located in the same MSA of an attack. The dependent variable in columns (1) and (2) is the natural logarithm of the number of patents plus one. The dependent variable in columns (3) and (4) is the natural logarithm of citation counts plus one. The dependent variable in columns (5) and (6) is the natural logarithm of the cumulative dollar value (in millions of 2005 nominal US dollars) of patents that a firm applies for in a given year plus one. The dependent variable in specifications (7) and (8) is the patent generality score. The dependent variable in specifications (9) and (10) is the patent originality score. We use the same control variables as in Table 3. All control variables are lagged by one year. The detailed definitions of all variables are provided in the Appendix A. The odd-numbered columns omit the control variables and contain both standard and multiplicative fixed effects (i.e., inventor and year fixed effects, region*year fixed effects, and industry*year fixed effects whose coefficients are suppressed), while the even-numbered columns include control variables and standard fixed effects (i.e., inventor and year fixed effects, whose coefficients are suppressed). Standard errors, which are adjusted for heteroscedasticity and are clustered at MSA level, are reported in parentheses below the coefficient estimates. The symbols ***, ** and * indicate statistical significance at the 1 %, 5 % and 10 % levels, respectively.

	Ln (#patents+1)		Ln (#citations+1)		Ln (invention value+1)		Generality		Originality	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: attack vicinity within 100 miles										
Attack vicinity within 100 miles*post	−0.022***	−0.025***	−0.055***	−0.061***	−0.045***	−0.085***	−0.009	−0.008	−0.010***	−0.013***
	(0.003)	(0.003)	(0.011)	(0.010)	(0.010)	(0.009)	(0.010)	(0.005)	(0.002)	(0.002)
Post	0.002	−0.003	−0.004	−0.006	0.014	0.012	0.001	0.019	0.002	−0.000
	(0.004)	(0.003)	(0.010)	(0.009)	(0.010)	(0.009)	(0.010)	(0.015)	(0.002)	(0.002)
Control variables of Table 3	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region*year fixed effects	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Industry*year fixed effects	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
No. of obs.	558,160	558,160	558,160	558,160	558,160	558,160	513,168	513,168	450,544	450,544
Adjusted R^2	0.404	0.402	0.387	0.385	0.396	0.392	0.160	0.159	0.340	0.338
Panel B: attack vicinity within MSA										
Attack vicinity within MSA*post	−0.021***	−0.023***	−0.068***	−0.065***	−0.044***	−0.032**	−0.005	−0.004	−0.009***	−0.014***
	(0.005)	(0.004)	(0.015)	(0.013)	(0.015)	(0.013)	(0.004)	(0.003)	(0.004)	(0.003)
Post	0.007	0.004	0.006	0.003	0.004	0.003	0.004	0.003	0.004	0.003
	(0.005)	(0.004)	(0.014)	(0.012)	(0.015)	(0.013)	(0.004)	(0.004)	(0.003)	(0.003)
Control variables of Table 3	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region*year fixed effects	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Industry*year fixed effects	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
No. of obs.	405,924	405,924	405,924	405,924	405,924	405,924	372,924	372,924	324,094	324,094
Adjusted R^2	0.425	0.422	0.406	0.404	0.412	0.409	0.204	0.203	0.354	0.351

curtailed after a terrorist attack.

Overall, the results in Tables 3 and 4 provide support for the *invention disruption* hypothesis but not for the *resilience* view.

3.3. Inventor mobility (firm-level analysis)

We examine whether terrorism promotes inventor mobility. To consider this issue, the dependent variables in Panel A of Table 5 are: the natural logarithm of the number of inventors plus one in models (1)–(4), adding inventors in a firm's staff in a given year; the natural logarithm of the number of new hires plus one in models (5)–(8), tracking inventors hired from another firm in a given year; and the natural logarithm of the number of leavers plus one in models (9)–(12), counting inventors who move to another firm in a given year.

Looking at Panel A, both the number of inventors in a firm's staff and the number of new inventor hires decline during the five years subsequent to a terrorist attack that generates deaths. Conversely, after the same event and during the same period, the number of inventors who move to other firms increases. On average, a firm located within 100 miles from an attack is associated with a 3.92 % decline in staff inventors, a 1.98 % decrease in the number of new inventor hires, and a 3.05 % increase in the number of inventors who leave to work elsewhere in the five-year period after the attack. These effects are based on the *Terrorist Attack within 100 miles * Post* coefficients in columns (1), (5), and (9), respectively. Defining local firms with MSAs yields comparable results.

A potential concern is that the analyses in Panel A of Table 5 are affected by the natural log +1 specification we use for the dependent variables and thus by the skewness of the patent data. Cohn et al. (2022) describe the issues related to the log +1 transformation when using count data and also offer several ways to address them. One of the solutions involves scaling the dependent variable (to estimate a rate) because doing so "substantially deskews an outcome variable, as skewness in the distribution of the outcome is often partly a product of skewness in the distribution of scale" (Cohn et al., 2022, p. 2–3). Based on this advice, in Panel B of Table 5, we re-estimate the tests reported in Panel A by scaling inventor level variables by the number of employees. For example, in Panel B, we replace Ln(#Inventors+1) with #Inventors/Employees. The new scaled results yield inferences that are qualitatively similar to those from Panel A. Indeed, according to Panel B, during the five years after the strike, firms within 100 miles of an attack exhibit (i) a 1.096 % decrease in their inventor/employee ratio (model (1)), (ii) a 0.236 % decrease in their new hires/employee ratio (model (5)), and (iii) a 0.841 % increase in their leavers/employee ratio (model (9)).

Table 5
Terrorist attacks and inventor mobility.
This table presents the effects of terrorist attacks on inventor mobility. In columns (1) through (4) of Panel A the dependent variable is the natural logarithm of the number of firm's inventors in a given year plus one. In columns (5) through (8) of Panel A the dependent variable is the natural logarithm of the number of firm's newly hired inventors in a given year plus one. In columns (9) through (12) of Panel A the dependent variable is the natural logarithm of the number of firm's inventors who leave for other firms in a given year plus one. All control variables are lagged by one year. The detailed definitions of all variables are provided in the Appendix A. The odd-numbered columns omit the control variables and contain both standard and multiplicative fixed effects (i.e., firm and year fixed effects, region*year fixed effects, and industry*year fixed effects whose coefficients are suppressed), while the even-numbered columns include control variables and standard fixed effects (i.e., firm and year fixed effects, whose coefficients are suppressed). Standard errors, which are adjusted for heteroscedasticity and are clustered at MSA level, are reported in parentheses below the coefficient estimates. Panel B presents abbreviated regressions like those in Panel A in which the dependent variables are not log-transformed and are scaled by the number of firm's employees. The symbols ***, ** and * indicate statistical significance at the 1 %, 5 % and 10 % levels, respectively.

Panel A	Ln (#Inventors+1)				Ln (#New Hires+1)				Ln (#Leavers+1)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Attack vicinity within 100 miles*post	−0.040***	−0.045***			−0.020**	−0.029***			0.030***	0.025**		
	(0.015)	(0.014)			(0.008)	(0.008)			(0.011)	(0.010)		
Attack vicinity within MSA*post			−0.054***	−0.046***			−0.020**	−0.025***			0.024**	0.023**
			(0.016)	(0.015)			(0.008)	(0.008)			(0.011)	(0.011)
Post	0.004	−0.014	−0.019	−0.019	0.005	−0.005	0.007	−0.011	−0.018	−0.017	−0.016	−0.009
	(0.017)	(0.016)	(0.017)	(0.017)	(0.009)	(0.008)	(0.009)	(0.009)	(0.011)	(0.011)	(0.012)	(0.011)
Size		0.316***		0.320***		0.111***		0.113***		0.139***		0.142***
		(0.022)		(0.023)		(0.013)		(0.013)		(0.014)		(0.015)
Tobin's Q		0.041***		0.042***		0.027***		0.026***		0.019***		0.019***
		(0.005)		(0.006)		(0.003)		(0.003)		(0.004)		(0.004)
Cash holdings		0.231***		0.236***		0.069**		0.078**		0.071*		0.074*
		(0.058)		(0.060)		(0.032)		(0.033)		(0.037)		(0.038)
Leverage		−0.281***		−0.284***		−0.070**		−0.066**		−0.028		−0.029
		(0.058)		(0.059)		(0.029)		(0.029)		(0.036)		(0.037)
ROA		−0.155***		−0.149***		−0.082***		−0.077***		−0.132***		−0.140***
		(0.051)		(0.053)		(0.025)		(0.025)		(0.031)		(0.032)
Tangible assets		0.171***		0.171***		0.090***		0.089***		0.149***		0.157***
		(0.060)		(0.061)		(0.031)		(0.032)		(0.036)		(0.037)
Capital expenditures		−0.120		−0.105		−0.012		0.002		−0.247**		−0.242**
		(0.154)		(0.158)		(0.090)		(0.092)		(0.111)		(0.114)
Ln (firm age)		−0.037*		−0.038*		−0.060***		−0.061***		0.012		0.010
		(0.019)		(0.020)		(0.009)		(0.010)		(0.011)		(0.011)
H-index		−0.945***		−0.933***		−0.257		−0.260		−0.182		−0.195
		(0.355)		(0.359)		(0.174)		(0.175)		(0.194)		(0.198)
H-index^2		0.905***		0.867**		0.320**		0.332**		0.305*		0.330*
		(0.336)		(0.343)		(0.156)		(0.163)		(0.185)		(0.196)
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region*year fixed effects	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Industry*year fixed effects	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
No. of obs.	28,180	28,180	26,302	26,302	28,180	28,180	26,302	26,302	28,180	28,180	26,302	26,302
Adjusted R^2	0.762	0.765	0.756	0.766	0.676	0.671	0.664	0.669	0.672	0.667	0.660	0.664

Panel B	#Inventors/Employees				#New hires/employees				#Leavers/employees			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Attack vicinity within 100 miles*post	−1.096*	−1.098*			−0.236**	−0.338***			0.841***	0.592**		
	(0.609)	(0.605)			(0.110)	(0.109)			(0.245)	(0.241)		
Attack vicinity within MSA*post			−1.471**	−1.585**			−0.343***	−0.400***			0.679**	0.526*
			(0.632)	(0.637)			(0.108)	(0.108)			(0.310)	(0.313)
Post	1.238	−0.246	1.511	−0.345	0.294*	0.024	0.219	−0.104	−0.511	−0.428	−0.243	−0.264
	(0.944)	(0.853)	(0.987)	(0.923)	(0.171)	(0.162)	(0.152)	(0.150)	(0.327)	(0.321)	(0.291)	(0.287)
Size		−2.188***		−2.449***		−0.560***		−0.562***		−1.179***		−1.224***
		(0.846)		(0.904)		(0.192)		(0.201)		(0.412)		(0.430)

(*continued on next page*)

Table 5 (*continued*)

Panel B	#Inventors/Employees				#New hires/employees				#Leavers/employees			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Tobin's Q		0.449		0.548		−0.026		−0.034		−0.069		−0.031
		(0.331)		(0.335)		(0.093)		(0.098)		(0.175)		(0.189)
Cash holdings		13.465***		14.003***		2.179***		2.098***		1.119		1.105
		(3.311)		(3.377)		(0.737)		(0.724)		(1.953)		(2.032)
Leverage		−11.216***		−11.014***		−1.083**		−1.307***		−1.962*		−2.065
		(2.443)		(2.478)		(0.475)		(0.406)		(1.172)		(1.276)
ROA		−9.896**		−9.822**		−0.714		−0.545		−2.252*		−2.636*
		(4.082)		(4.329)		(1.003)		(1.017)		(1.321)		(1.378)
Tangible assets		−8.341**		−8.866**		−1.139**		−1.131**		−2.049*		−2.178*
		(3.353)		(3.535)		(0.550)		(0.562)		(1.096)		(1.154)
Capital expenditures		−18.445***		−18.306***		−0.161		0.005		−8.260***		−8.991***
		(6.298)		(6.275)		(1.093)		(1.066)		(2.278)		(2.419)
Ln (firm age)		−2.127***		−1.818***		−0.385***		−0.346***		0.382		0.373
		(0.633)		(0.635)		(0.129)		(0.130)		(0.293)		(0.315)
H-index		−2.601		−0.104		−2.609		−2.426		−3.270		−3.701
		(5.799)		(5.969)		(1.852)		(2.003)		(4.935)		(5.420)
H-index^2		2.166		−0.391		2.487		2.235		3.811		4.181
		(5.612)		(5.841)		(1.666)		(1.793)		(4.200)		(4.630)
Firm and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region*year fixed effects	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Industry*year fixed effects	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
No. of obs.	28,180	28,180	26,302	26,302	28,180	28,180	26,302	26,302	28,180	28,180	26,302	26,302
Adjusted R^2	0.762	0.765	0.756	0.766	0.676	0.671	0.664	0.669	0.672	0.667	0.660	0.664

3.4. Labor market relocation (inventor-level analysis)

To provide further insights on the effects of terrorism on inventor mobility, we track inventors over time and across the firms for which they file patents. In the regressions reported in Table 6, ***Ln (Distance of the Move + 1)*** is the dependent variable in columns (1) and (2), ***Over-100-Miles Move*** is the dependent variable in columns (3)–(4), and ***Out-of-MSA Move*** is the dependent variable in columns (5)–(6). The estimates in column (1) indicate that terrorist attacks lead to an 18.89 % increase in the distance of the move. Similarly, the estimates from a probit regression in column (3) suggest that inventors in firms within 100 miles from the strike are 3.62 percentage points more likely to move to a firm located more than 100 miles from their previous firm. Additionally, according to the results in column (5), inventors in a stricken MSA are 3.7 percentage points more likely to relocate to a firm in a different MSA. These findings suggest that inventors in firms near terrorist strikes get new jobs farther away relative to other inventors who leave firms not located near an attack.

The results in Tables 5 and 6 are consistent with another key prediction of the *invention disruption* hypothesis that terrorist attacks promote the reallocation of inventors among firms.

3.5. Productivity of inventors that relocate from the attacked areas

We estimate several ancillary tests that evaluate the productivity of inventors who relocate from stricken areas and file patents in firms domiciled outside the 100 miles radius from the terrorist attack, and those who relocate away from a stricken MSA. These analyses contrast the productivity of the relocated inventors against two benchmark groups. The first group consists of the relocated inventors themselves *before* the deadly terrorist attack that led to their move. The second benchmark group is the cohort of inventors who were already employed at the relocation firms. The results indicate that, relative to both benchmarks, inventors who relocate away from the stricken sites do not exhibit a statistically significant drop in their productivity.[16] According to our tests, the relocated inventors are as productive (a) as they were in their previous firms (*before* the terrorist attack), and (b) as their new co-workers in the relocation firms (*after* the attack). This evidence contrasts with our earlier results showing that, after terrorist attacks, inventor productivity in firms near the attacks exhibits a material decline. To better understand why this occurs, in Section 3.7, we explore potential channels underlying our findings.

3.6. Anticipation and persistence of the terrorism effect

Three potential concerns with our analyses are (i) whether events other than the terrorist attack might be driving our results, (ii) whether some terrorist attacks are anticipated, and (iii) whether our experiment is vulnerable to reverse causality. Notably, these issues could prevent our setting from satisfying parallel trends, a condition needed to ensure internal validity of difference-in-differences estimates. While this condition is not testable (Callaway and Sant'Anna, 2021), we produce plots that provide visual evidence suggesting that our setting satisfies it. The process we use to generate these plots is as follows. We first assign every terrorism event to a placebo date one year (t - 1), two years (t - 2), and three years (t - 3) *before* the year of the real attack (i.e., year t). We also assign each terrorism event to a placebo date one year ($t + 1$), two years ($t + 2$), three years ($t + 3$), four years ($t + 4$) and five years ($t + 5$) *after* the actual attack. Next, for each of the 8 placebo dates, we respectively estimate separate regressions like those in Table 3 (inventor productivity) and Table 5 (inventor mobility). For every outcome variable, we then trace the effect corresponding to the regression coefficients for

[16] To conserve space and because of the absence of statistical significance, these ancillary analyses are not tabulated.

Table 6
Terrorist attacks and inventor relocation.
This table examines the effects of terrorist attacks on labor market relocation using inventor-level data. In columns (1) and (2) the dependent variable "Ln (Distance of the Move+1)" is the natural logarithm of inventor's distance of the move to a new employer from her previous employer. In columns (3) and (4) the dependent variable "Over-100-mile Move" is an indicator variable that takes the value of one if an inventor moves to another employer located over 100 miles away from her previous employer, and zero otherwise. In columns (5) through (6) the dependent variable, "Out-of-MSA Move" is an indicator variable that takes the value of one if an inventor moves to another employer located in a different MSA than her previous employer, and zero otherwise. All control variables are lagged by one year. Detailed definitions of all variables appear in the Appendix A. All specifications include inventor and year fixed effects, whose coefficients are suppressed. Standard errors, which are adjusted for heteroscedasticity and are clustered at MSA level, are reported in parentheses below the coefficient estimates. The symbols ***, ** and * indicate statistical significance at the 1 %, 5 % and 10 % levels, respectively.

	Ln (distance of the move+1)		Over-100-mile move		Out-of-MSA move	
	(1)	(2)	(3)	(4)	(5)	(6)
Attack vicinity within 100 miles*Post	0.173***		0.182**		0.184**	
	(0.067)		(0.083)		(0.080)	
Attack vicinity within MSA*Post		0.203***		0.190**		0.197**
		(0.071)		(0.074)		(0.078)
Post	−0.051	−0.077	−0.019	−0.058	−0.016	−0.057
	(0.052)	(0.056)	(0.067)	(0.065)	(0.064)	(0.064)
Size	−0.015	−0.023	−0.033	−0.037*	−0.039*	−0.045**
	(0.023)	(0.023)	(0.023)	(0.022)	(0.024)	(0.023)
Tobin's Q	0.040*	0.040*	0.027	0.027	0.028*	0.028*
	(0.021)	(0.021)	(0.018)	(0.018)	(0.016)	(0.016)
Cash holdings	−0.285	−0.319	−0.231	−0.244	−0.226	−0.260
	(0.279)	(0.288)	(0.226)	(0.228)	(0.221)	(0.226)
Leverage	−0.289	−0.352	−0.085	−0.087	−0.109	−0.109
	(0.205)	(0.218)	(0.205)	(0.213)	(0.193)	(0.202)
ROA	−0.021	0.022	0.141	0.190	0.121	0.170
	(0.240)	(0.258)	(0.194)	(0.207)	(0.191)	(0.201)
Tangible assets	−0.251**	−0.361***	−0.244**	−0.346***	−0.257**	−0.362***
	(0.114)	(0.120)	(0.110)	(0.116)	(0.111)	(0.118)
Capital expenditures	1.182	1.110	0.980	0.842	0.919	0.807
	(0.762)	(0.787)	(0.786)	(0.865)	(0.752)	(0.831)
Ln (firm age)	−0.205***	−0.193***	−0.160***	−0.151***	−0.182***	−0.175***
	(0.047)	(0.047)	(0.035)	(0.036)	(0.036)	(0.036)
H-Index	0.733	0.834	0.799	0.979	0.981	1.168
	(0.697)	(0.682)	(0.747)	(0.756)	(0.744)	(0.749)
H-Index^2	−0.484	−0.593	−0.532	−0.724	−0.803	−0.999
	(0.765)	(0.748)	(0.866)	(0.868)	(0.870)	(0.875)
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Inventor fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
No. of Obs.	24,207	21,161	24,207	21,161	24,207	21,161
Adjusted R^2	0.060	0.064	(0.097)	(0.096)	(0.097)	(0.096)

Attack Vicinity within 100 miles (dashed lines) and within MSA (solid lines). Fig. 2 presents five separate plots describing the temporal relation between terrorist attacks that generate deaths and invention-related variables during the pre- and post-attack periods for each of the five outcome variables we study at the firm level, respectively.

To facilitate visual inspection of the trends in our variables, we overlay a vertical line in each plot to denote the year of the terrorist attack and report the actual regression coefficient estimates along with their respective *p*-values. According to Fig. 2, the significant downward (upward) trend for inventor productivity, inventor mobility, number of employees, and number of new hires (leavers) starts on the year of the attacks but not earlier. These patterns remain when local firms are defined within 100 miles or within the MSA of the attack. These findings lessen concerns of reverse causality or possible anticipation of the attacks which would have affected inventor productivity and mobility ex-ante. Moreover, the absence of pre-trends suggests that our data plausibly satisfy the parallel trends condition which is essential to ensure the internal validity of difference-in-differences models.

The plots in Fig. 2 also show that both the per-employee and per-inventor measures reveal that the drop in patenting lasts for 3 years after a terrorist attack that produces human losses. The plots also illustrate that, during the three years after the terrorist strike, firms near an attack keep losing inventors and are less likely to hiring new ones. Altogether, the post-trend evidence in Fig. 2 suggests that terrorism generates effects that linger for some time after an attack.

3.7. Channels

Existing work shows that after the departure of a "superstar" inventor, collaborators suffer a productivity decline (Zacchia, 2018). As a result, we need to be cautious in interpreting any differences across the productivity of inventors who move and those who do not because any distinction could be due to inventors' individual characteristics or due to economic issues confronting the firms in which they work. Indeed, existing studies argue that a channel through which the effects of terrorism manifest is psychological (e.g., Ahern, 2018; Becker and Rubinstein, 2011). We complement this work by considering other non-mutually exclusive channels that could potentially influence the effects of terrorism.

3.7.1. Financial constraints

To investigate whether financial constraints is a channel that makes some firms more vulnerable to the terrorism shock, we begin by defining firms as constrained whenever their Kaplan and Zingales (1997) (KZ) index is in the top tercile of all firms in the previous year.[17] Otherwise, firms are classified as unconstrained.

For all firms in the immediate vicinity of a terrorist attack, we

[17] As in Lamont et al. (2001), the KZ index equals [−1.001909[(ib + dp)/lagged ppent] + 0.2826389[(at + prcc_f × csho - ceq - txdb)/at] + 3.139193 [(dltt + dlc)/(dltt + dlc + seq)] − 39.3678[(dvc + dvp)/lagged ppent] − 1.314759[che/lagged ppent], where all variables in italics are Compustat data items.

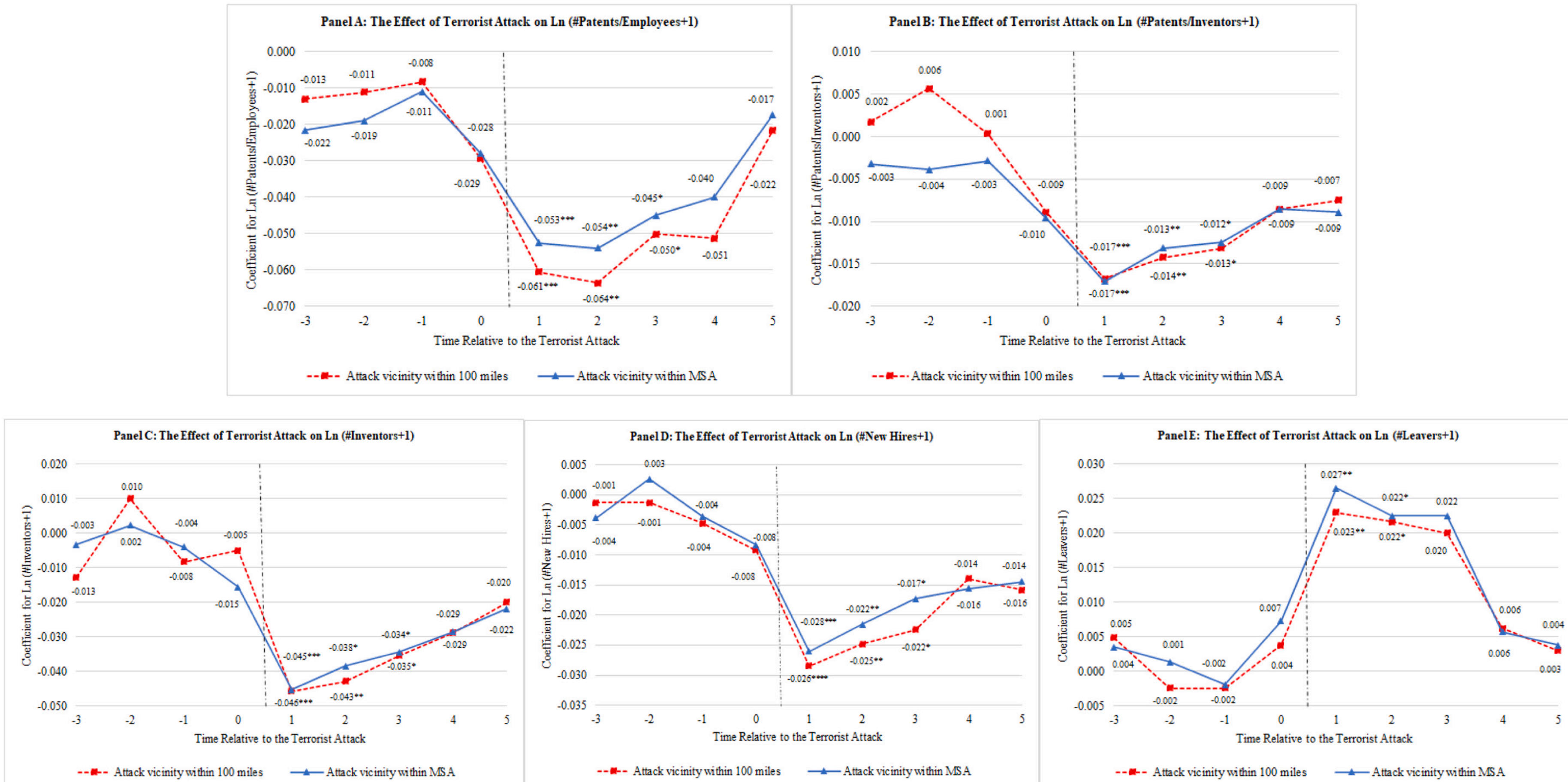


Fig. 2. Trends in the relation between terrorist attacks and inventor productivity, and inventor mobility.
This figure shows the trends in the relation between terrorist attacks and inventor productivity, and inventor mobility over the 3-year period before and 5-year period after the terrorist attacks. The event window is the same as in Brav et al. (2018). The *y*-axis plots the estimated coefficients after regressing on inventor productivity variables (Panel A for ln (#Patents/Employees +1) and Panel B for ln (#Patents/Inventors+1)), and inventor mobility variables (Panel C for ln (#Inventors+1), Panel D for ln (#New Hires+1), and Panel E for ln (#Leavers+1)), as well as on the control variables and firm and year fixed effects used in Table 3 (for Panels A and B), and in Table 5 (for Panels C through E). The *x*-axis shows the time relative to terrorist attacks for the 3-year period before and 5-year period after the terrorist attacks. The red dotted line corresponds to the attack vicinity within 100 miles and the blue solid line corresponds to the attack vicinity within MSA. The symbols ***, ** and * denote statistical significance at the 1 %, 5 % and 10 % levels, respectively. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

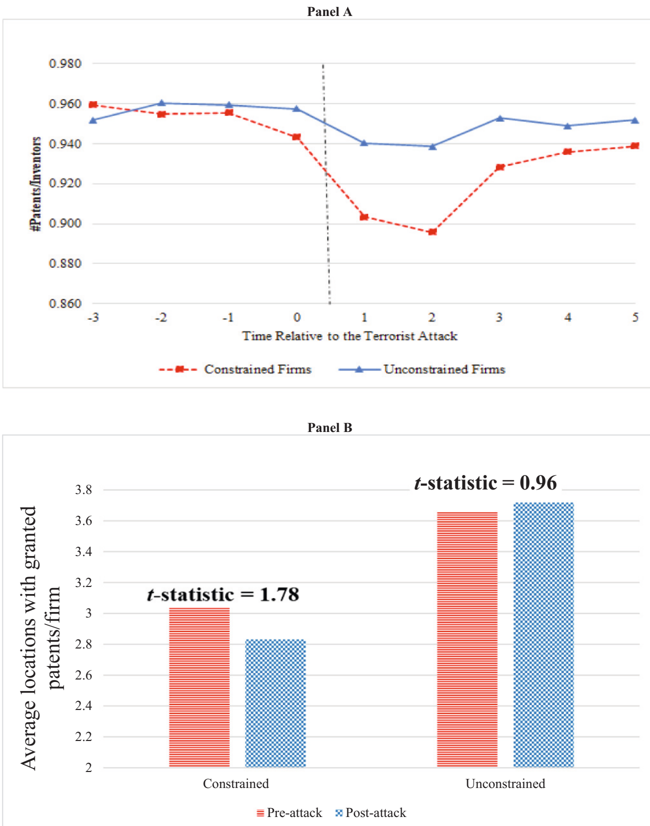


Fig. 3. Inventor productivity and granted patent locations for constrained and unconstrained firms.
This figure displays inventor productivity (Panel A) and the average inventor/employee ratio for both constrained and unconstrained firms with inventors that are located within 100 miles from the location of the attack. In Panel A, the *y*-axis plots the annual average number of patents per inventors. The *x*-axis shows the time relative to terrorist attacks for the 3-year period before and 5-year period after the terrorist attacks. The red dotted line corresponds to constrained firms and the blue solid line corresponds to unconstrained firms. In Panel B, the *x*-axis shows the average number of locations with granted patents per firm whereas the *y*-axis splits firms into (financially) constrained and unconstrained. Panel B provides *t*-statistics for the difference in the average number of locations with granted patents from 1 year before an attack until 1 year after the attack. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

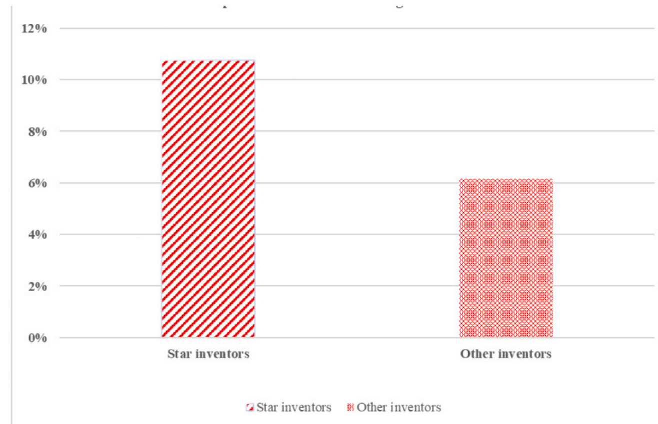


Fig. 4. The proportion of "star" and other inventors moving after the attack.
This figure charts the proportion of "star" and other inventors in treated and matching firms that move to another firm during the year immediately after a terrorist strike. "Star" inventors are those that belong to the top 25 % decile of inventors in terms of the number of granted patents as in Baghai et al. (2019).

estimate the annual average number of patents per inventor during the three years before and the five years after the strike. In Panel A of Fig. 3, we plot the annual average number of patents per inventors for both constrained and unconstrained firms. These plots suggest that firms' financial constraints provide a major channel through which inventor productivity declines after a terrorist attack. According to the plots, inventor productivity exhibits a similar trend for both constrained and unconstrained firms *before* an attack. However, *after* a terrorist attack, the productivity of inventors in constrained firms is significantly lower that the productivity of inventors in unconstrained firms. In the first year after the attack, the mean difference in productivity between constrained and unconstrained firms exhibits a *t*-statistic equal to 2.93. Two years after the attack, the *t*-statistic jumps to 3.56. The difference in inventor productivity between the two groups remains statistically significant during the three years following the attack.[18] This evidence is consistent with the view that financial constraints provide a channel through which a terrorist attack more acutely affects inventor productivity.

The bar charts in Panel B of Fig. 3 show that constrained firms experience a statistically significant post-attack decrease in their average number of locations with granted patents (from 3.04 before the attack to 2.83 after the attack). By contrast, unconstrained firms exhibit a slight increase in the same metric (albeit not significant at conventional levels). For this test, we recorded data from the address field in the patent data to determine the invention activity location. The evidence in Panel B suggests that constrained firms are probably less able to either outsource invention activities or conduct them away from the stricken sites and that either possibility is consistent with the view that terrorism uniquely affects the invention process.

3.7.2. *Inventor talent*

It might be easier for the most talented inventors to relocate after a terrorist attack. To assess this conjecture, we classify "star" inventors as those in the top 25 % decile of inventors in terms of the number of granted patents. This taxonomy is similar to that in Baghai et al. (2019).

Using the universe of inventors working for firms in attacked areas, Fig. 4 charts the proportion of star and other inventors that move to another firm during the year immediately after a lethal terrorist strike. To provide a benchmark, the figure also plots the proportion of star and other inventors that move to another firm for the set of matching firms we identify in Section 2.4. According to the figure, 10.75 % of inventors classified as stars and 6.15 % of other inventors move after the attack. A chi-square test statistic of 8.06 confirms that the difference in proportions between the two groups is statistically significant. Moreover, the proportion of star inventors that leave treated firms is also statistically higher than the proportion of stars leaving the cohort of matched firms (chi-square = 10.71). This evidence suggests that exceptional ability or talent is a channel that allows some inventors to relocate to other firms after a deadly terrorist attack.

3.7.3. *Invention disruption vs. resilience*

The preceding analyses indicate that superstar inventors are better

[18] The results are similar if we use the Hadlock and Pierce (2010) measure of financial constraints.

able to move to firms distant from the attacked areas. While this finding highlights a potential channel underlying our baseline results, it also raises the question of whether our invention disruption and resilience hypotheses can coexist. Indeed, the resilience of inventors who do not move after the attack might be more than offset by the departure of superstars. Such a situation, which suggests that our hypotheses are not necessarily mutually exclusive, would be hard to capture through the reduced form econometric techniques we use. Motivated by the above discussion, we perform univariate comparisons of the mean change in patents per inventor at firms with and without superstars sorted by whether firms lose inventors after a terrorist strike as follows.

% Change in patents/inventor after the attack for treated firms			*t*-Statistic for mean differences (3)–(4)
Lose inventors?	No (3)	Yes (4)	
Firms with superstar inventors (1)	−1.73 %	−3.24 %	3.42
Firms without superstar inventors (2)	−1.62 %	−2.13 %	1.91
t-statistic for mean differences (1)–(2)	−1.02	−2.48	1.52 (1)(3)–(2)(4)

In this test, all firms are treated as they are located within 100 miles of an attack. In line with the disruption hypothesis, firms are significantly disadvantaged when they lose any inventors and more so when they lose superstars. Notably, the difference in the average drop in patent/inventor between firms without superstars that lose other inventors (−2.13 %) and firms that retain all their superstars and other inventors (−1.73 %) is not statistically significant (*t*-statistic = 1.52). Moreover, the difference between the average decline in patent/inventor between firms with and without superstars that do not lose any inventors is also insignificant (*t*-statistic = −1.02). These results provide support for the resilience hypothesis. Thus, while our baseline tests suggest that the disruption/mobility effects dominate (and likely suppress the impact of resilience), the resilience hypothesis cannot be rejected.

3.8. Robustness tests

Using additional data and different samples, further tests probe the robustness of our baseline findings. We describe and report the results of all robustness tests in the internet Appendix.

4. Conclusions

We study the effects of terrorism on inventor productivity and mobility for US firms. Using stacked difference-in-differences estimation, we find robust evidence that during the five-year period after a terrorism event with confirmed human losses, inventors in firms located near an attack's site exhibit material declines in various measures of inventor productivity. In addition, firms geographically close to terrorism-afflicted areas are less likely to hire new inventors and more likely to have inventors move to firms located far away from the stricken scenes. Other tests show that: (a) firms' financial constraints are related to a more serious post-terrorism decrease in inventor productivity; and (b) inventors' intellectual talent or ability is associated with a higher incidence of inventor mobility after a terrorist attack. Overall, these results are not congruent with the view that inventors will exhibit *resilient* behavior following a terrorist attack. Instead, the results support the hypothesis that terrorism *disrupts* the innovation process.

The findings we present help reconcile the evidence in Bloom (2009) that even large terrorism events such as the 9/11 attacks are not associated with long-lasting economic effects. According to our evidence, the detrimental effects of terrorism on the invention process persist for three years after the attacks, particularly in firms located near the strikes. However, because the same attacks promote inventor mobility to firms in distant locations, invention activity at those remote firms probably mitigates the broader economic impact of terrorism.

Our evidence on the effects of terrorism on inventor productivity and mobility should be of interest given the centrality of invention activity for sustaining long-run economic growth and the growing incidence of terrorist strikes. Therefore, our results have implications related to the measures companies should take to lessen the effects of terrorist attacks on their employees, and to the economic benefits of implementing public policies that improve the security of regions in which invention activity takes place. Because of this, we hope that our study motivates future work in which terrorist attacks, wars, and similar events provide the basis for an identification strategy useful (a) to improve our understanding of more nuanced aspects of the invention process, (b) to explore the consequences of inventor mobility on other important outcome variables, and (c) to help further distinguish the invention disruption hypothesis from the resilience alternative.

CRediT authorship contribution statement

Eliezer M. Fich: Conceptualization, Methodology, Validation, Writing – original draft, Writing – review & editing, Supervision. **Tung Nguyen:** Conceptualization, Methodology, Software, Formal analysis, Investigation, Data curation, Writing – original draft, Visualization. **Dimitris Petmezas:** Conceptualization, Methodology, Validation, Writing – original draft, Writing – review & editing, Supervision, Project administration.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

Appendix A. Variable definitions

Dependent variables

- Ln (#Patents/Employees+1): The natural logarithm of the number of patents per 1000 firm employees (EMP) plus one. Patent data is from Kogan et al. (2017) and Stoffman et al. (2022).
- Ln (#Patents/Inventors+1): The natural logarithm of the ratio of the number of patents scaled by the number of inventors who applied for a patent at the firm in a given year and have not yet filed any patent for a different firm plus one. Patent data is from Kogan et al. (2017) and Stoffman et al. (2022). Inventor data are from Li et al. (2014).
- Ln (#Patents+1): The natural logarithm of the total number of patents that a firm applies for (and are subsequently granted) in a given year plus one. This variable is created using data from Kogan et al. (2017) and Stoffman et al. (2022).
- Ln (#Citations+1): The natural logarithm of the total number of citations obtained on all patents that a firm applies for (and are subsequently granted) in a given year plus one. This variable is created using data from Kogan et al. (2017) and Stoffman et al. (2022).
- Ln (Invention Value+1): The natural logarithm of the cumulative dollar value of patents (in millions of 2005 nominal US dollars) that a firm applies for in a given year plus one. A patent's value is measured as the firm stock return in excess of the market over the three-day window around the date of patent approval (t, $t + 2$), multiplied by the firm's market capitalization on the day prior to the announcement of the patent issuance. The dollar value of each patent is obtained from Kogan et al. (2017).

- Generality: One minus the Herfindahl concentration index of the number of patents citing across technological classes. We use the bias correction of the Herfindahl measures, described in Jaffe and Trajtenberg (2002), to account for cases with a small number of patents within technological categories. This variable is created using data from the NBER patent database *(https://www.nber.org/patents/)* and Bhaven Sampat's United States Patent and Trademark Office (USPTO) patent and citation database.

  (See, *http://thedata.harvard.edu/dvn/dv/boffindata*.)

- Originality: One minus the Herfindahl concentration index of the number of cited patents across technological classes. We use the bias correction of the Herfindahl measures, described in Jaffe and Trajtenberg (2002), to account for cases with a small number of patents within technological categories. This variable is created using data from the NBER patent database *(https://www.nber.org/patents/)* and Bhaven Sampat's United States Patent and Trademark Office (USPTO) patent and citation database.

  (See, *http://thedata.harvard.edu/dvn/dv/boffindata*.)

- Ln (#Inventors+1): The natural logarithm of the number of firm inventors in a given year plus one. We define "Inventors" as those who produce at least one patent in a firm in our sample period. This variable is created using data from Li et al. (2014).
- Ln (#New Hires+1): The natural logarithm of the number of newly hired inventors in a given year plus one. We define "New Hires" as those inventors who produce at least one patent at a new assignee firm in our sample within one year after producing a patent at a different assignee. This variable is created using data from Li et al. (2014).
- Ln (#Leavers+1): The natural logarithm of the number of inventors who leave for other firms in a given year plus one. We define "Leavers" as those inventors who stop filing patents at a sample firm where they had previously produced a patent and file at least one patent in a new firm in our sample within one year after producing a patent at the firm they were previously producing patents. This variable is created using data from Li et al. (2014).
- Ln (Distance of the Move+1): The natural logarithm of inventor's distance moves to a new employer from her previous employer.
- Over 100 Miles Move: An indicator set to one if an inventor files a new patent at a new company located more than 100 miles from the location of another company he had filed patents for, and zero otherwise. We create this variable with data from Li et al. (2014).
- Out-of-MSA Move: An indicator which takes the value of one, if an inventor who had filed a patent for a firm located in an MSA, files a new patent for another firm in a different MSA, and zero otherwise. This variable is created using data from Li et al. (2014) and Compustat for identifying firm's MSA.

  Terrorism variables

- Attack vicinity within 100 miles: An indicator variable that equals one if a firm is located within 100 miles of the attack. We use data from the U.S. Census Bureau's Gazetteers and Zip Code Database to identify the latitude and longitude of the firms and the places where the terrorism incidents took place.
- Attack vicinity within MSA: An indicator variable that equals one if a firm is located within the MSA of the attack.
- Post: An indicator variable equal to one if a firm-year observation is within $[t+1, t+5]$ years of a terrorism event associated with at least one confirmed death (for treated firms) or a pseudo-event year (for matched firms).

  Firm variables

- Size: The natural logarithm of total assets (AT). This variable is created using data from Compustat.
- Tobin's Q: The market value of equity (CSHO*PRCC_F) plus book value of assets (AT) minus book value of equity (CEQ) minus balance sheet deferred taxes (TXDB), scaled by total assets (AT). This variable is created using data from Compustat.
- Cash Holdings: Cash and short–term investments (CHE) scaled by total assets (AT). This variable is created using data from Compustat.
- Leverage: The sum of long-term debt (DLTT) and debt in current liabilities (DLC) scaled by total assets (AT). This variable is created using data from Compustat.
- ROA: Income before extraordinary items (IB) plus interest expense (item XINT) plus income taxes (item XINT), divided by total assets (item AT). This variable is created using data from Compustat.
- Tangible Assets: Property, plant, and equipment (PPEGT) scaled by total assets (AT). This variable is created using data from Compustat.
- Capital Expenditures: Capital expenditures (CAPX) scaled by total assets (AT). This variable is created using data from Compustat.
- Ln (Firm Age): The natural logarithm of one plus the number of years since the firm's first appearance in the Center for Research in Security Prices (CRSP). This variable is created using data from CRSP.
- Number of Employees: The number of people employed by the company (in thousands). This variable is created using data from Compustat.

  County variables

- Population: The number of residents for counties. This variable is created using data from The US Census Bureau.
- Population density: The number of people per square mile for counties. This variable is created using data from The US Census Bureau.

  State variables

- Crime Rate: The natural logarithm of the crime rate of the state. (Source: The Federal Bureau of Investigation (FBI)).
- Unemployment Insurance Benefit: The natural logarithm of the maximum unemployment insurance benefits that an unemployment insurance claimant can receive in a year. (Source: The US Department of Labor's "Significant Provisions of State UI Laws").

  Market variables

- VIX: The Chicago Board Options Exchange (CBOE) Volatility Index. (Source: http://www.cboe.com/products/vix-index-volatility/vix-options-and-futures/vix-index/vix-historical-data).

  Industry variables

- H–Index: This is the Herfindahl index which represents the sum of squares of the market shares of all firms in a given year and three–digit SIC industry, where market share is defined as sales of the firm divided by the sum of the sales in the industry. This variable is created using data from Compustat.
- H–Index^2: The squared root of the H-index.

Appendix B. Supplementary data

Further robustness checks can be found online at https://doi.org/10.1016/j.respol.2022.104655.

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