Insecurity and Firm Displacement: Evidence from Afghan Corporate Phone Records*

Sylvan Herskowitz[†] Joshua E. Blumenstock[‡] Tarek Ghani[§]
Ethan B. Kapstein[¶] Thomas L. Scherer[‖] Ott Toomet[**]

August 19, 2024

Abstract

We provide empirical evidence on how insecurity affects firm behavior by linking data on deadly terrorist attacks in Afghanistan to geolocated data on corporate mobile phone activity. We first develop an approach to estimate the geographic footprint of firms from employee locations. Using these measures, our main analysis finds that violent shocks reduce local firm presence by both increasing firm exit and decreasing entry. Firms react most to violence in their 'headquarters' district. We further find suggestive evidence of persistence, stronger impacts in more secure districts, and spillovers, whereby attacks in provincial capitals reduce firm presence in surrounding rural districts.

Keywords: Insecurity, Firms, Mobile Phones
JEL classification: O12, L22, F50

*We are grateful to Shahim Kabuli, Sami Safiullah and Shafique Shalizi for providing excellent research assistance, and to Arvind Jain, Hasibullah Mowahed, Kirk Schmidt and Bernd Struck for help with data. We thank Ajmal Ahmady, Naseem Akbar, Jenny Aker, Mohammad Azizi, Eli Berman, Nick Bloom, Michael Callen, Lorenzo Casaburi, Alex Chernoff, Ernesto Dal Bo, Kyle Emerick, Gladys Lopez-Acevedo, Claudia Nassif, Martin Rama, Jacob Shapiro, Chris Woodruff for providing insightful feedback. We acknowledge funding from the World Bank Multi-donor Trust Fund (#TF0A2657), the Office of Naval Research (Minerva N00014-17-1-2313), and the National Science Foundation (#CCF-1637360) with project management assistance from the United States Institute of Peace. It builds on work supported by the Minerva Research Initiative through the Air Force Office of Scientific Research (FA9550-09-1-000001).The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the World Bank and its affiliated organizations, or those of the Executive Directors of the World Bank or the governments they represent.

† *Corresponding Author*, World Bank, Development Impact Evaluation, sherskowitz@worldbank.org.
‡ University of California at Berkeley, jblumenstock@berkeley.edu.
§ Washington University in St. Louis, Olin Business School, tghani@wustl.edu.
¶ Princeton University and Empirical Studies of Conflict Project, kapstein@princeton.edu.
‖ University of California San Diego, tlscherer@ucsd.edu.
** University of Washington, Information School, otoomet@uw.edu.

1 Introduction

A vibrant private sector is essential for long-run growth. Prior work has thus sought to understand the institutional barriers to private sector development (North, 1990; Svensson, 1998) and documents key impediments to firm growth such as regulatory quality, capital constraints, and rule of law (Hallward-Driemeier and Pritchett, 2015). But while one-fifth of the world's population lives in insecure countries, much less is known about how the private sector responds to insecurity (Baranyi et al., 2011). This gap stems in part from a scarcity of data on firms during and after violent conflict (Besley and Mueller, 2018).

This paper makes both methodological and substantive contributions to understanding how insecurity affects the private sector. Methodologically, we develop and validate a new approach for measuring the presence, entry, and exit of private firms at high frequency and spatial granularity. We derive these measures using administrative records of corporate mobile phone activity from one of Afghanistan's largest mobile phone operators. The data contain records from over 217 million corporate phone calls in 173 districts across four years. Our first set of results validate these novel measurements with multiple independent data sources, including administrative data from the Afghan government, World Bank survey micro-data, satellite nightlights data, and an original survey with 406 Afghan companies.

Our main substantive analysis uses these new measures to study how thousands of Afghan firms respond to violent shocks. Specifically, we quantify how major outbreaks of violence from Taliban-linked attacks, as recorded in the Global Terrorism Database, affect the geographic footprint of 2,292 Afghan firms between 2013 and 2016. Since violence is not randomly assigned, and is undoubtedly influenced by the local environment, we attempt to isolate changes in firm behavior that follow 'unexpected' outbreaks of local violence. Our econometric specification includes restrictive fixed effects (for each firm-district, each month, and each district-season) and both linear and quadratic district-specific time trends. We show robustness to a variety of alternative specifications.

Firms respond to violent shocks by immediately reducing their presence in the affected district by 3-5%. This is driven both by an increase in firm exit and a decrease in firm entry: firms are 5-23% more likely to leave a district in the month after violence, and are 7-16% less likely to enter. The effect is most pronounced in the first month, with suggestive evidence of persistence in subsequent months. Firms are particularly sensitive to violent shocks that occur in their primary 'headquarters' district, a location that we infer from the phone records based on where the plurality of each firm's employees are based (a definition that we validate with a subsample of firms for which we have administrative and survey records).

We also find evidence of heterogeneity and spillovers. In particular, using a measure of the overall state of insecurity in a district, first used by Blair and Wright (2021), we find that shocks cause greater displacement when they occur in secure provinces. We also find that violent shocks affect surrounding districts within the same province, particularly when the violence occurs in provincial capitals.

Taken together, these results paint a nuanced picture of how firms respond to insecurity. Insecure environments are marked by uncertainty and risk, as violent conflict can disrupt economic activity and supply chains, damage business assets, and expose personnel to possible injury or death. The profit maximization problem faced by firms in Afghanistan is more complex than in other, more stable contexts, as they must account for these costs and consequences of insecurity.[1] Firms in these contexts must make difficult choices about where to operate based on their perceptions of the current security environment and expectations of future insecurity. We show that firm activity is substantially impacted by terrorist attacks. While we find only modest evidence of persistence beyond the first month, short-lived impacts on firm location choice are likely to disrupt productive activity, impeding deliveries, delaying meetings, and distorting investments (Botzen et al., 2019).

Our work engages a burgeoning literature on the economic consequences of insecurity.[2] Important examples have highlighted the consequences of violent conflict on GDP in Spain (Abadie and Gardeazabal, 2003), on long-run growth in Vietnam (Miguel and Roland, 2011), on investment in Israel (Fielding, 2004), on housing prices in Ireland (Besley and Mueller, 2012), and on employment in the U.S. (Brodeur, 2018). Our work also contributes to a literature on agglomeration by highlighting the importance of security as an amenity in cities (Glaeser, 2010; Puga, 2010).

However, studying firm response to insecurity in developing countries *during* an active conflict presents major challenges, especially where insecurity directly inhibits traditional approaches to gathering data on firm operations. Thus, only a handful of studies have explored the impact of violence on the private sector: Besley and Mueller (2018) quantifies the costs of protection for firms in predatory environments; Guidolin and La Ferrara (2007) show how conflict impacts the public valuation of Angolan diamond companies; Ksoll et al. (2016) study the effect of electoral violence on labor supply in Kenya; and Amodio and Di Maio (2017) show how conflict affects firms' access to inputs in Palestine.

[1] This relates to a recent literature highlighting how firms in developing countries often often face a more complex optimization problem beyond profit-maximization (Chandrasekhar et al., 2020; Macchi and Stalder, 2023).

[2] See Collier et al. (2003) and Blattman and Miguel (2010) for overviews of research linking aggregate economic activity and insecurity.

More broadly, our approach connects to recent work using non-traditional 'big' data to measure productivity and wealth (Henderson et al., 2012; Blumenstock et al., 2015; Jean et al., 2016; Aiken et al., 2022), unemployment (Toole et al., 2015), urban mobility (Hanna et al., 2017), and migration (Blumenstock, 2012). Blumenstock et al. (2021) use phone data to examine how violent conflict affects individual financial decisions. We do not, however, know of any studies using satellite or phone data to assess firm-level behavior. Our study complements these literatures by illustrating how new sources of passively-collected digital data can be used to measure and provide insights on the behavior of private firms in developing and insecure countries, even in the midst of an active conflict. While our call detail record data are unique in measuring firm location choices over time, we do not observe traditional measures of business activity in the phone data - such as trade, sales, or hiring decisions, limiting our ability to assess these other important dimensions of firm response to insecurity. Still, our analysis shows that the responses that we do observe are economically meaningful and offer considerable nuance beyond our previous understanding of firm activity in insecure environments.

2 Setting

While Afghanistan has experienced conflict for decades, we study a period of rising violence and economic downturn from 2013-2016 following relative stability and high growth. In 2009, the U.S. and NATO launched a surge of troops, pushing the Taliban insurgency to the most remote parts of the country and across the border into Pakistan. Starting in 2012, U.S. forces began to draw down their presence, leading to a sharp escalation in the intensity and geographic scope of insecurity across the country. As Figure 1a shows, the five years from 2012-2016, which cover the period of this study, marked a steady increase in the number of confirmed fatalities from terrorist attacks and in the number of Afghan districts perceived as insecure. This destabilizing trend culminated several years later when the Taliban reclaimed control over the country in August 2021.

In spite of the ongoing conflict, Afghanistan maintained a significant formal sector (Ghiasy et al., 2015), which is a key contributor to long-run economic growth and job creation (Klapper and Richmond, 2011). There is, however, very little data on investment and the private sector (World Bank, 2015). The most recent and comprehensive source, the 2009 Integrated Business Environment Survey (IBES), estimated that approximately 400,000 firms were operating in Afghanistan. However, this survey is outdated relative to the time period of our study, and contains only basic characteristics of each firm (e.g., size and sector).

To learn more about the importance of insecurity for Afghan firms, we conducted a survey of 406 business owners in 2017.[3] 81% of firms reported that they viewed security as a primary obstacle to their businesses. In listing all challenges facing their businesses, 91% included security, while power cuts (86%), labor problems (82%), and infrastructure (76%) were the next three highest responses, all plausibly linked to issues of security.[4] Insecurity from insurgents remains the primary concern of firms: 78% said they were very affected by insecurity from anti-government groups generally, and even more said they were specifically concerned about land mines or IEDs (85%), small arms fire (84%), kidnappings (83%), and suicide bombers (93%) specifically. While the impact of shifting insecurity cannot be inferred from these descriptive statistics, the responses make clear that insecurity related threats were at the forefront of many business owners' conscience at the time of the study. Focus group discussions gave further insights into the broad range of issues encapsulated in insecurity, with different respondents emphasizing road security while transporting goods, corruption of customs officials, and simple street harassment and gender-linked violence issues.

3 Measuring Firm Location Choice with Phone Data

Our first set of results develop and validate a new approach to measuring the geographic footprint of private firms using corporate mobile phone metadata. The resulting measurements provide a fine-grained, quantitative perspective on the behavior of a large number of firms that would be difficult, if not impossible, to obtain using traditional data sources or survey methods – particularly in areas of active conflict. In this section, we provide background on the data source, then describe how the data can be used to measure firm presence, and finally validate these new measurements with more traditional data.

3.1 Corporate Mobile Phone Metadata

Call Detail Records (CDR) are the transaction logs generated when people communicate over mobile phone networks. CDR do not contain the contents of the phone call, but rather basic metadata about each communication event: the unique identifiers for the calling and called parties, the date and time of the call, and the geo-coordinates of the cell phone tower

[3] All of the statistics cited in this paragraph appear in Appendix Table A1.

[4] In the 2014 World Bank Enterprise Survey in Afghanistan, the most commonly cited obstacle to business was "political instability"; other top answers included "corruption and crime" and "theft and disorder" (World Bank, n.d.).

used by the calling party, which makes it possible to roughly locate that individual at the time the call was initiated.

Our analysis is based on anonymized CDR of corporate accounts from one of Afghanistan's largest mobile network operators. These accounts are used by businesses to link multiple phones to a single corporate account, which allows for consolidated billing services and discounts for within-organization calls. We observe the names of these organizational customers as well as the mobile operator's classification of each account's business type (e.g., "construction", "government", "transport"). After removing public and non-profit organizations, we remain with a sample of 2,292 private firms with over 125,000 associated phone numbers (which we will refer to as 'employees') that were active during the 45 months of data between April 2013 and December 2016. We focus on a set of 173 districts (out of 398) that had uninterrupted mobile tower coverage during the period of analysis, an area which covers 65% of Afghanistan's population.[5] We remain with over 217 million phone calls, placed through 1,056 active cell phone towers distributed across the country.

We do not expect that the set of firms with corporate phone accounts are representative of all Afghan firms. We would like to be able to characterize these differences, however a reliable, representative firm census does not exist. One benchmark is the World Bank's Enterprise Survey conducted May-July 2013 that aimed at creating a representative sample. Firms in our CDR data have twice as many linked numbers as the number of employees reported among firms in the Enterprise Survey sample (See Appendix Table A2). The firms in our sample are also relatively less likely to appear in trade or manufacturing categories, and are more likely to have their headquarters in Kabul. Our sample is thus comprised of relatively large formal firms, a group that accounts for a major portion of formal employment, and which is of particular interest as a driver of economic growth (Klapper and Richmond, 2011). Section 3.4 provides further discussion on some of the nuances and limitations of these data.

3.2 Measuring Firm Presence and Movement

We use the corporate CDR to measure the geographic footprint of Afghan firms over time. We focus our analysis on firm presence in each district in each month, using two distinct measures. First, we define a firm as having 'any presence' in a district-month if any of the firm's employees place any calls from that district in that month (using a phone number

[5]This district selection criteria was used to avoid confounding absence of cell phone coverage with reduction in firm activity. We later show that our results are qualitatively unchanged if we relax this restriction.

associated with the corporate account). Second, we define a firm as having 'intense presence' in a district-month if that district was the primary district of one or more employees in that month, where each employee's 'primary' district for a given month is defined as the district from which they made the most calls. These measures have different strengths: any presence is more sensitive and picks up on short-term visits, while intense presence requires a higher threshold of employee and firm presence in the district.

We also use the CDR data to identify the primary location of each firm's operations as a whole, our best prediction of a firm's headquarters. We use the first six months of CDR for each firm to identify the primary district of each of its employees, and identify the firm's primary district as the district in which the majority of its employees are based.

Among the 2,292 firms in our data, the average (median) firm is active in 32 (41) months out of 45 total months of data. While 60% of firms are based in Kabul, most operate in multiple locations, with the average firm observed in 34 (22) districts. The average firm has 33 employees (with associated mobile accounts), but the median firm has only 4. In an average month, the average firm has any employee presence in 5% of the 173 districts in the estimation sample and intense presence in 1.7% of those districts. Appendix Table A3 provides additional summary statistics for each firm, district-month, and firm-district-month.[6]

3.3 Validation of CDR-Based Measures of Firm Location

Figure 1b provides a map of firm presence, as inferred from the mobile phone data. Districts are colored according to the log number of active firms in April 2013. As expected, major urban centers such as Kabul, Kandahar, Hirat, Mazar, Kunduz, and Jalalabad show high levels of activity. The red dots mark locations of violent fatalities in the year preceding our study (May 2012-April 2013), highlighting the wide spatial variation we use in later analysis.

In Table 1, we validate the CDR-based measures of firm presence using three different and independent data sources: (i) the 2016 Central Business Registry (CBR), in which formal firms must register to receive a tax identification number; (ii) the 2016 Afghanistan Investment Support Agency (AISA), a database of firms seeking foreign investment, and (iii) our own survey of 406 firms from the CDR conducted in 2017.[7] We match across datasets using the name of each firm, successfully linking 934 firms in our CDR data to the CBR

[6]Appendix A2 describes how the firm-district-month panel was constructed from the original CDR.
[7]We attempted to contact all firms in the CDR sample in spring of 2017.

dataset and 110 to the AISA dataset. For each of these matched sets of firms, we compare the headquarters inferred from the CDR (see Section 3.2) to the headquarters reported in the other traditional data source. We find that our method has a high success rate: 72% match to the CBR reports; 82% match to the AISA, and 78% match to our own survey.[8]

In Table 2, we find a strong correlation in district-level changes of aggregate CDR-based measures of private sector presence and nightlights emissions from NOAA's VIIRS luminosity data, which are frequently used as a proxy for aggregate economic activity (Henderson et al., 2012; Bilicka and Seidel, 2022).[9] Panel A compares the first-difference of the logarithm of values of total active firms and sum of nightlight pixel values between adjacent months in the same district, while panel B replaces total active firms with total active subscribers. As shown in column 1, the elasticities between CDR-based measures of firm presence and nightlights are large (magnitudes of 0.34-0.35) and significant ($p < 0.01$). The correlation persists even when including time fixed effects (to isolate differences in changes between districts in each month) and district fixed effects (to compare within-district differences in changes in CDR-based activity to within-district differences in changes in nightlights). In other words, changes in firm activity, as captured in the CDR data, appear to reflect meaningful changes in local economic activity.

3.4 Data Limitations

Ultimately, CDR provide objective data on where and when subscribers use mobile phones. The analysis that follows uses CDR from corporate accounts in Afghanistan to infer where those firms' employees are, as a way to understand the geographic footprint of the firms. The analysis above suggests that (i) the location of firms inferred from CDR closely match the administrative headquarters reported in administrative data; and (ii) changes in firm activity over time correlate with satellite-based measures of growth.

However, some caveats are needed when interpreting the data. First, as noted earlier, the sample of firms using corporate lines is not nationally representative of all firms and is more likely to reflect relatively large formal firms (See Appendix Table A2). Second, firms

[8] These match rates are on par with other studies that have compared locations inferred from mobile phone data to other independent sources of ground truth location data (Warren et al., 2022; Vanhoof et al., 2018; Frias-Martinez et al., 2010). In our context, mismatches between our prediction of a firm's primary location and the administrative records of firms' headquarters could result from either outdated or incorrect information in the government databases. It could also reflect that our CDR-based measure captures a firm's primary operating location that, for some firms, may be distinct from where their administrative headquarters is registered.

[9] See Cao et al. (2012) for NOAA data.

may not provide all employees with corporate phone lines, resulting in an incomplete picture of firm presence. In our survey, firms reported they were most likely to give phones to senior management, followed by staff working in sales, distribution or production.[10] Firm fixed effects can address time invariant-differences in mobile phone usage and allocation, but the variation in our data represents a select sample of employees.

Third, presence is measured only when phones are used. We partially address this by aggregating data to the district-month level – making it less likely that we under-measure presence relative to a more granular unit of analysis. We also will use two different main measures of firm presence ('any' presence and 'intense' presence) to understand the sensitivity of our findings to different thresholds of observed activity (in further robustness checks we show our main findings for a wide range of additional measures and codings). A related concern might arise if employees used their corporate phones during non-work ("leisure") hours, and those locations differed substantially from working locations. While we do not believe this is a major concern in Afghanistan, where most employees in Afghanistan live and work in the same district, we later show that our results are substantively unchanged if we restrict analysis to locations inferred exclusively from calls placed during the work week.

Finally, insecurity may influence phone usage itself. In our survey, firms reported that they were more likely to make calls and to check in more frequently with others when entering into dangerous areas. To the extent that employees use their phones more often when feeling insecure, presence measured after major violent events is, if anything, likely to be overestimated relative to normal times; this, in turn, would lead us to *under*-estimate the extent to which violence causes firms to reduce presence in affected areas.

4 How Do Firms Respond to Insecurity?

4.1 Estimation

Our main results examine the impact of violent shocks on firm location choice. This analysis is based on a firm-district-month panel that links information on firm presence, derived from corporate phone records, to information on violent events. As discussed in Section 3.2, we focus on how violence affects whether a firm has 'Any Presence' or 'Intense Presence' in a given district in a given month.

Our primary econometric specification estimates the relationship between violent shocks

[10] Corporate lines were typically rationed and we did not receive reports of firms issuing them to non-employees, such as family members.

and firm presence:

$$Y_{idt} = \beta_1 \mathbb{1}(VS)_{dt-1} + \theta_{id} + \sigma_{dm} + \delta_t + g_d(t) + f(towers_{dt}) + \epsilon_{idt} \tag{1}$$

where Y_{idt} is an indicator variable that equals 1 if firm i is present in district d in month t, and $\mathbb{1}(VS)_{dt-1}$ indicates whether district d experienced a Violent Shock in month $t-1$ (defined below in Section 4.2). To try and isolate quasi-random variation in extreme violence, we include a rich set of fixed effects to control for other factors that are correlated with both violence and firm activity: θ_{id} are a set of firm-district fixed effects (to control for time-invariant, firm-specific geographic factors, such as a firm's preference for operating in district d); σ_{dm} are a set of district-calendar month fixed effects (to control for seasonal factors shared across firms in a given region and time of year, such as seasonal variation in violence and firm activity); δ_t are year-month fixed effects (to control for factors that affect all firms equally in a given month, such as the timing of national elections); and $g_d(t)$ are linear and quadratic district-specific time trends (to account for different local trends in violence and economic activity). We also flexibly control for a polynomial (linear and quadratic) function of the number of active mobile phone towers in a district-month with $f(towers_{dt})$, to account for the potential of violence to affect mobile network availability. Throughout, we cluster our standard errors, ϵ_{idt}, at the district level.

The primary coefficient of interest in Equation (1) is β_1, which we interpret as the average effect of a violent shock in district d in month $t-1$ on firm presence in d in the subsequent month t. We also use an event study framework to examine the persistence of shocks by including leads and lags of the violent shock variable:

$$Y_{idt} = \sum_{k=-6}^{6} \beta_k \mathbb{1}(VS)_{dt-k} + \theta_{id} + \sigma_{dm} + \delta_t + g_d(t) + f(towers_{dt}) + \epsilon_{idt} \tag{2}$$

The identifying assumption behind Equations (1) and (2) is that the timing of shocks is 'as good as random' after conditioning on θ_{id}, σ_{dm}, δ_t, $g_d(t)$ and $f(towers_{dt})$. Although violence itself is not random – it is likely correlated with local economic and security conditions – we expect that these specifications isolate discrete changes in firm behavior that occur immediately after 'unexpected' violence, where our model defines what a reasonable expectation of violence would be.[11] We revisit this assumption, and what can still be learned

[11]If firms anticipate major violent shocks in a district better than our econometric specification and decrease presence in advance of the shock, this would likely lead us to underestimate β_k. As we discuss below, our event study results do not contain evidence of such anticipatory behavior.

if this assumption is violated, after presenting our main results.

4.2 Measuring Insecurity

Our violence data come from the Global Terrorism Database (GTD), which contains records of over 24,000 confirmed fatalities from terrorism in Afghanistan from 2012 to 2016.[12] As a media-based dataset, GTD likely understates the true incidence of terrorist events due to potential coverage gaps, particularly events in remote areas or without fatalities. While this may lead to under-measuring incidents, it increases our confidence that we capture meaningful shocks to civilian security that might impact private sector behavior.[13]

Our primary measure of violence is an indicator variable, violent shock (VS_{dt}), that indicates whether a district d in month t is in the top 1% of confirmed fatalities in insurgent-linked attacks, relative to all district-months in the 45-month panel — in practice, this implies a threshold of 23 or more killings.[14] After dropping districts without complete CDR coverage, we observe 68 violent shocks distributed across 34 unique districts and 34 unique months.[15] Appendix Figure A1 plots fatalities against the count of violent shocks, and indicates that the highest number of fatalities occur in months when violence simultaneously affects multiple districts. Appendix Figure A2 maps the spatial variation in our violent shock measure. Reassuringly, violent shocks are distributed across the country rather than concentrated in a single region, and cover both urban and rural districts.

While violence was quite common in Afghanistan during our study period, outbreaks of violence were often sudden and not obviously predictable. In Figure 2, we test whether violence that occurs in a district in a reference period ($t = 0$) is correlated with violence in that same district in the 6 months before or after. The figure plots the coefficients from a

[12] Maintained by National Consortium for the Study of Terrorism and Responses to Terrorism (START) at the University of Maryland, the GTD database is constructed from keyword filtering of high-quality media sources and hand coded by teams of researchers, including providing geo-coordinates for the city or district an event takes place. Confirmed fatalities include either victims or attackers and must meet GTD's definition of terrorism: "the threatened or actual use of illegal force and violence by a non-state actor to attain a political, economic, religious, or social goal through fear, coercion, or intimidation."

[13] Alternative geocoded violence datasets have limitations. ACLED only begins covering Afghanistan in 2017. UCDP's emphasis on state-based conflict may undercount violence directed at civilians; from 2012-2016, UCDP records less than 5,000 civilian fatalities in Afghanistan, or 20% GTD's total count of fatalities related to terrorism. The publicly available SIGACTs data do not include a measure of fatalities.

[14] As shown in Appendix Table A3, the mean (median) district-month records 1.3 (0) GTD fatalities.

[15] In aggregate, the 68 shocks used in the analysis account for 3050 confirmed fatalities that meet GTD's criteria. The most common types of attacks were armed assaults (53%) and bombings (47%) with less common categories including kidnappings (7%) and infrastructure attacks (5%) – percentages reflect the share of fatalities and are non-exclusive by type. Private citizens were the primary target in over half (51%) of fatalities, followed by police (30%), military (27%), government (11%), and businesses (4%).

regression of the log (+1) of fatalities (Panel A) and terrorist linked events (Panel B) on a set of distributed leads and lags of the violent shock indicator, controlling for district fixed effects, time fixed effects, and district linear and quadratic trends. There is a clear spike in violence in the reference period, where we have identified a violent shock ($t = 0$), but there is neither a clear anticipatory increase in violence leading up to these shocks nor is there a persistent change in levels of violence following them.

4.3 Results: Firm response to violent shocks

Table 3 presents our main results on the impact of violent shocks on firm presence. Panel A shows the effect on any firm presence in the affected district (any employees placing calls) and Panel B shows the effect on intense firm presence (any employees based primarily in that district). The columns of the table include progressively more restrictive controls: column (1) includes district-by-firm fixed effects; column (2) adds month and calendar-month fixed effects; column (3) adds polynomial controls for cellphone tower coverage in each district-month; and column (4) adds district-specific linear and quadratic time trends. The binary outcome indicator of firm presence is scaled by a factor of 100 to improve readability. Effects can therefore be interpreted as percentage point changes in the likelihood of firm presence in a given district.

Interpreting the magnitude of the coefficients in Table 3, the results in columns (3) and (4) of Panel A imply that violent shocks in a district are associated with a 0.14-0.54 percentage point decrease in the likelihood of any firm presence in the following month (Panel A), and a 0.09-0.19 percentage point decrease in the likelihood of intense firm presence (Panel B). The row labeled "β / Mean" provides an indication of the relative importance of these effects, by dividing the coefficient by the average firm presence in a district. These estimates indicate that violent shocks are followed by a 3-11 percent reduction in any firm presence and a 5-11 percent reduction in intense presence in a district. Between the two columns, we expect that the estimates in column (3) capture broader shifts in firm presence and local security, whereas the more conservative estimates in column (4) better capture the discrete changes in firm behavior immediately following unexpected violent shocks.[16]

Several tests of the robustness of these results are presented in the appendix. In particular, Appendix Table A4 shows widespread persistence of our main effects when varying our approach to coding violence. Appendix Figure A3 shows that our results are qualitatively

[16]While violent shocks might displace employees into non-violent districts, such spillovers are unlikely to significantly change our estimates of β_1, since there are roughly 172 non-violent districts for each district impacted by a violent shock.

unchanged when using alternative thresholds of fatalities for our definition of a violent shock. Appendix Table A5 demonstrates robustness to different methods of measuring our outcome variable, firm presence, from CDR, in addition to the 'any' and 'intense' measures of presence used in our main tables. Appendix Table A6 shows that results are not sensitive to calls placed outside of working hours (column 1), whether or not we drop observations from regions with interrupted phone coverage (column 2), or the clustering of our standard errors (column 3). In Appendix Table A7, we split our sample by business type (industry) categories. We find that "any presence" effects are concentrated in transport firms and "intense presence" effects are concentrated in manufacturing firms.

Persistence

Next, we examine the persistence of our main effects. Figure 3 shows the β_k coefficients and 95% confidence intervals obtained from estimating equation (2) (see Appendix Table A8 for a tabular version of these results). Responses to major attacks are biggest and concentrated in the first month following major attacks. Point estimates suggest that negative effects may persist for at least an additional five months, but they lack statistical significance and shrink in magnitude.

The event study also affords us an opportunity to examine pre-trends using a variety of approaches suggested by Roth (2022). Looking at the significance of six leads across each of our two primary outcome measures, we find a single coefficient that shows marginal significance ($p<0.1$ for the third lead of the specification using the 'any' firm presence measure). This is in line with what we would expect as the result of random chance. We additionally conduct a joint F-test of whether the sum of the leads is equal to zero for each of our two main outcomes. We do not detect joint significance in either model, with p-values of 0.49 and 0.39 using our any and intense firm presence measures respectively.[17]

Firm entry and exit

We find evidence that the overall decrease in firm presence after violence is driven both by an increase in firm exit and a decrease in firm entry. These results are shown in Table 4,

[17]We additionally tried a more agnostic F-test of joint significance that is indifferent as to whether the leads indicate a consistent heightened (or reduced) level of firm presence in the periods leading up to violent shocks. The p-value for the intense measure leads of firm presence is 0.28, however the p-value for any firm presence is <0.01. This is because many of the coefficients in this specification have t-statistics of 1 or greater despite landing on both sides of zero. While, computationally this will lead to a restricted R-squared that is significantly smaller than the unrestricted R-squared as suggested by the p-value of the F-test, it does not suggest an interpretable or rationalizable anticipatory period of violence.

using specification (1) to estimate the impact of violent shocks on three different measures of firm activity: columns (1) and (4) reproduce the effects on any/intense firm presence; columns (2) and (5) show the impact on 'Firm Entry', which is an indicator equal to one if a firm is not present in the previous month ($t-1$) and then is present in the current month (t); columns (3) and (6) show the impact on 'Firm Exit', an indicator equal to one if a firm was present in the previous month and then absent in the current month. Outcomes are scaled by a factor of 100. In Panel A of Table 4, we find that firm entry decreases by 4.1-10.2 percentage points (7.2-16.7%), and that firm exit increases by 6.1-8.0 percentage points (5.4-23.6%).

In Panel B of Table 4, we explore heterogeneity based on whether the shock occurs in a firm's primary district, identified using the methods described in Section 3.3. The second row of Panel B shows that firms are particularly sensitive to shocks in their primary district. Columns (1) and (4) show that the decrease in firm presence is driven by primary districts. In columns (2) and (5), we find that while firms stop entering non-primary districts affected by violence (row 1), they are actually *more* likely to enter primary districts after a violent shock – though these effects are only statistically significant using the any presence outcome measure in column (2). This may indicate that while some firms are more likely to return to areas around their headquarters after a violent shock, others are more likely to leave, as shown in column (3).

The effects on entry and exit in Table 4 show how firms present in a district react in the month immediately following a violent shock. In Appendix Table A9, we generalize this analysis over longer time horizons, to better understand whether these effects are transient or impact longer-run decisions around entry and exit. Specifically, we vary the period of time over which a firm must be observed in order to be included in the entry and exit analysis. When comparing Appendix Table A9 to Table 4, we observe in Panel A that entry effects are concentrated in firms that have been away longer (e.g. not present in any of the last three months) – particularly in column (2) using any presence, and also (with marginal significance) in column (4) using intense presence. We also note in column (6) using any presence that firms with three consecutive months of presence can be dislodged by violent shocks (column 8 with intense presence is consistent in sign and magnitude but lacks precision). In Panel B, we observe that exit effects in primary districts are precisely estimated across specifications, underlining that shocks can dislodge firms that have been present for three months and that this effect can persist for at least three months in many firms.

Violent shocks and insecure states

Our next set of results explores the relationship between unexpected violent shocks and the general security environment of the location in which they occur. Specifically, we use a new indicator for whether district, d, was considered to be insecure in that month and explore its interaction with our main treatment. This variable is based on the internal assessment of an Afghan survey firm, reflecting their determination of whether each district was safe to visit in each month based on the presence of Taliban insurgents.[18] Relative to public assessments of district security by the Afghan government, U.S. forces, and independent analysts, this private rating is unlikely to be distorted by strategic considerations and reflects a high-stakes decision on employee deployment and safety.

These results, presented in Table 5, paint a more nuanced picture of how firm location decisions are influenced by violence. The first specification, in columns (1) and (4) adds the district-level measure of insecurity to the violent shock of our main specification. The magnitude of the effect of shocks on firm presence increases while controlling for district insecurity. The coefficient on district insecurity itself is negative with marginal significance ($p < .1$) for the specification in column (4) using intense firm presence.

The imprecision of the estimates of the relationship between security status and firm presence, observed in columns (1) and (4), masks important heterogeneity. In particular, in columns (2) and (5), we find that firms substantially and significantly decrease presence when the security status of their primary district changes, but do not significantly change presence when the status of a non-primary district changes. While these changes in security status are not as cleanly identified as the violent shock variable, the results echo the heterogeneity observed in Panel B of Table 4, which showed that firms responded most to violent shocks in their primary district.

Finally, columns (3) and (6) of Table 5 test the interaction between violent shocks and district insecurity to explore whether the broader security environment influences how firms respond to shocks. These results indicate that the effect of a shock in a secure district is large and statistically significant – the coefficients on shocks in secure provinces in columns (3) and (6) both increase by just under 20% relative to the pooled estimates while the effect of shocks in insecure districts is smaller and not statistically significant. The proportional effects (relative to their relevant means) are both approximately twice as large in secure districts as in insecure districts. However, the estimates are too noisy to detect statistical significance of

[18] This is the same measure used to construct Figure 1a. Blair and Wright (2021) provide an in-depth discussion and application of this measure of insecurity.

differential effects. While not definitive, this broad pattern of results is consistent with firms updating their assessment of the likelihood of future violence in secure districts in response to shocks, while shocks in insecure districts carry less new information.[19]

Violent shocks and provincial spillovers

While this paper has thus far focused on localized effects of violent shocks and, in the previous section, broader shifts in the state of provincial insecurity, the effects of violent shocks could extend beyond the district in which the violence occurs. Thus, Table 6 exploits the spatial nature of our firm and violence data to consider spatial spillovers. In this analysis, we continue to control for whether a shock occurred locally, and add treatments for shocks that occured elsewhere in the province. Columns (1) and (2) show the impact on any firm presence while columns (3) and (4) show the impact on intense firm presence. Column (1) shows that shocks occurring elsewhere in a province reduce firm presence by 0.05 percentage points or approximately 1% (p=0.08). This effect is one third the size of a locally experienced shock (shown in the first row). With intense presence (column (3), the sign of coefficient is the same, but the magnitude is smaller and not statistically significant.

Columns (2) and (4) split this effect by whether the shock occurred in the provincial capital or in other rural (non-capital) districts. In this specification, we do not see evidence of significant effects when other rural districts experience shocks. However, in column (2), we observe a significant (p=0.06) reduction in firm presence when the provincial capital experiences a shock; the effect size is similar in size to that of a locally experienced shock. There is no evidence of spatial spillovers on intense firm presence. Broadly, these results are consistent with the Afghan government's efforts to secure provincial capitals given concerns that disruptions there could lead to broader economic and political disruptions.

5 Discussion

The previous section analyzes the impact of violent shocks on firm location choices over time. Before concluding, we provide some discussion and interpretation of these empirical results.

As a point of departure, we consider the broader economic significance of the observed effects. Our central finding, from column (4) of Table 3, suggests a reduction in local presence

[19] Appendix Table A10 shows the fully saturated interactions between district security, primary districts, and violent shocks; the results, while harder to interpret, further highlight the importance of shocks in primary, previously secure, districts.

by 3-5% in the month immediately following major violence. We interpret this as prima facie evidence that firms believe the costs of being physically present in a violent district outweigh the benefits. These costs and benefits include both standard factors, such as access to local markets, business interactions, operational expenditures, as well as non-standard factors, including the risk of physical violence against staff and damage or destruction of firm assets and essential infrastructure. The existence of these non-standard risks, in particular, requires firms to solve a more complex optimization problem than simple profit-maximization.

How consequential are such effects to the broader Afghan economy? While it is challenging to convert such effects into more interpretable units, we conduct a back-of-the-envelope calculation to convert these coefficients into changes in GDP. For this exercise, we leverage the growing literature using nightlights to predict local GDP; specifically, Hu and Yao (2022) use cross-country data to estimate an elasticity of nightlights to GDP of 1.3. Using the estimates from Table 2, which relate firm activity to nightlights, we infer that, on average, the immediate local response to each violent shock reduces monthly district GDP by 1.2-4.3%. Aggregating this impact across the 68 violent shocks in our study, this would amount to 80-300% of local monthly GDP. However, this back of the envelope calculation on the immediate response to shocks in the targeted district is likely a considerable under-estimate. In particular, this 1-month effect does not account for the persistent effects of violence (observed in Figure 3, which could scale these estimates by a factor of 3X) or the potential for spillovers (shown in Table 6, which could also increase these estimates by a factor of 4X).[20]

The need to conduct such back-of-the-envelope calculations highlights a central limitation of using corporate mobile phone records to study firm behavior, which is that we observe very little other than the locations of employees over time. We do not observe sales or other economic transactions; we do not know whether employees are making calls for personal or business purposes; and we do not observe the motives behind *why* firms reduce activity in response to violence, or what other adaptive measures they may or may not have taken.[21] However, our analysis of the different types and margins of firm presence adds some nuance to the main empirical result. For instance, in exploring the distinction between "any presence" (which better captures short-term visits) and "intense presence" (which better reflects more

[20]While they are noisily estimated, Figure 3 shows that after the initial drop in firm presence in the month immediately following a shock, point estimates remain negative and about 50% the magnitude of the initial response for another five months. Additionally, Table 6 suggests that violent shocks affect other districts within the same province with a magnitude between 10-33% of the local, main effects.

[21]Our qualitative survey data suggest employees who receive phones are likely to be more senior within the firm, but the phone data do not indicate the identity or role of that individual - and we thus cannot analyze the internal organization of the firm's response. See Section 3.4 for further discussion of these and other limitations.

prolonged presence), we can see that intense presence effects are consistently larger than any presence effects. This underscores the interpretation that firms are more fundamentally changing their operations after violent shocks. The analysis of entry and exit decisions reinforces this finding, as we observe that firms are both less likely to enter a district after violence, and that previously present firms are more likely to leave.

We also take an orthogonal, more qualitative approach to contextualizing these results. In particular, we conducted a firm survey in 2017, through which we directly asked firms in our sample about how, why and when they adjust their activities in response to conflict. The responses of 406 firms are tabulated in Appendix Table A1. These firms made clear that they perceive substantial physical risk to their employees, with 22.6% of respondents reporting that employees had been threatened by insurgents (anti-government groups), 8.2% reported employee injuries from insurgents, and 5.2% reported employees had actually been killed. Correspondingly, 24.5% indicated insurgents had threatened or destroyed firm assets and 58% indicated insurgents had destroyed public infrastructure crucial to business operations. Firms confirmed a range of economic responses to violence, with 45.7% investing in private security and 33.3% spending money on protection payments. 35.7% said that insecurity reduced demand for their products or services and 28.3% said that local violence had caused them to delay investments in a district. When asked about responses in their firms' movement and location choices, 28.3% said that violence had caused them to move staff away from affected cities or districts, 28.6% said that they decreased deliveries, while 39.8% changed transportation routes. 18.8% were forced to change suppliers and 15.1% changed buyers. 30.8% said that they had been forced to temporarily halt local operations due to insecurity while 7.5% said they had permanently stopped local operations. Given the diversity of firms we study, it is perhaps unsurprising that they experience and respond to insecurity in many different ways. Still, it is noteworthy that all of these dimensions of adaptation and firm response are economically distortionary (Besley and Mueller, 2018), and therefore deserving of future research.

6 Conclusion

We show how novel data on corporate phone activity can be used to study how firms change their geographic footprint in response to insecurity. We find evidence that firms reduce local presence by 3-5% in the month immediately following major violence. The effect is composed of both an increase in exit by firms that were present in that district during the month of the event, and a decrease in entry of firms that were not. The negative impact on firm presence persists for several months, but decays in magnitude and significance. Our analysis further suggests that the impact of shocks may be concentrated in districts that had been considered more secure. Finally, we find suggestive evidence of spillovers within provinces, whereby shocks, particularly those occurring in provincial capitals, reduce firm presence in nearby rural districts within the province as well.

Methodologically, these results – and the validation exercises we conduct – highlight the potential for using novel data to study firm activity, particularly in fragile and conflict-affected settings where it may be difficult or dangerous to conduct surveys. Such data can complement the traditional data used by researchers, businesses, and policymakers, and add a new perspective on private sector dynamics. However, CDR data have several limitations as a measure of firm activity. As discussed earlier, they do not capture the activity of all firms or employees, and it is difficult to separate personal calling activity from corporate calling activity. And, as noted previously, the phone data alone do not reveal the motives behind *why* firms reduce activity in response to violence, or the nuances of the manner in which they respond. These issues emphasize the need for careful validation and benchmarking when using such data to measure firm activity.

Substantively, the disruptions on firm operations and location choices that we observe likely have meaningful consequences. While violent conflict in Afghanistan carries an immense human toll, it also damaged the ability of firms to function in times of rising insecurity. By limiting the private sector's growth, the Taliban insurgency sought to hasten the fall of the U.S.-backed Afghan government - a goal they ultimately achieved in August 2021. This setting is thus a mirror image to that of Besley and Mueller (2012), who estimate the economic dividends from peace using increases in housing prices in Northern Ireland. That setting illustrates a potential virtuous cycle, where decreased killings led to increased asset values. Tragically, like many other conflicts in developing economies, Afghanistan instead has suffered from a vicious cycle: rising insecurity has harmed economic activity which, in turn, undermines state capacity and public confidence that the situation would improve. In both settings, the implication is that the provision of public security is of paramount importance for private economic activity and the ultimate viability of the state.

References

Abadie, Alberto and Javier Gardeazabal, "The Economic Costs of Conflict: A Case Study of the Basque Country," *American Economic Review*, 2003, *93* (1), 113–132.

Aiken, Emily, Suzanne Bellue, Dean Karlan, Chris Udry, and Joshua E. Blumenstock, "Machine learning and phone data can improve targeting of humanitarian aid," *Nature*, March 2022, pp. 1–7. Publisher: Nature Publishing Group.

Amodio, Francesco and Michele Di Maio, "Making Do with What You Have: Conflict, Input Misallocation, and Firm Performance," *The Economic Journal*, 2017.

Baranyi, Stephen, Pierre Beaudet, and Uli Locher, "World Development Report 2011: Conflict, Security, and Development," 2011.

Besley, Timothy and Hannes Mueller, "Estimating the Peace Dividend: The Impact of Violence on House Prices in Northern Ireland," *American Economic Review*, 2012, *102* (2), 810–833.

_ and _ , "Predation, protection, and productivity: a firm-level perspective," *American Economic Journal: Macroeconomics*, 2018, *10* (2), 184–221.

Bilicka, Katarzyna A and André Seidel, "Measuring Firm Activity from Outer Space," Working Paper 29945, National Bureau of Economic Research April 2022.

Blair, Christopher and Austin L Wright, "Refugee Return and Conflict: Evidence from a Natural Experiment," *University of Chicago, Becker Friedman Institute for Economics Working Paper*, 2021, (2021-82).

Blattman, Christopher and Edward Miguel, "Civil War," *Journal of Economic Literature*, 2010, *48* (1), 3–57.

Blumenstock, Joshua E., "Inferring patterns of internal migration from mobile phone call records: evidence from Rwanda," *Information Technology for Development*, April 2012, *18* (2), 107–125.

Blumenstock, Joshua, Gabriel Cadamuro, and Robert On, "Predicting poverty and wealth from mobile phone metadata," *Science*, 2015, *350* (6264), 1073–1076.

_ , Michael Callen, Tarek Ghani, and Robert Gonzalez, "Violence and Financial Decisions: Evidence from Mobile Money in Afghanistan," *The Review of Economics and Statistics*, 12 2021, pp. 1–45.

Botzen, WJ, Olivier Deschenes, and Mark Sanders, "The Economic Impacts of Natural Disasters: A Review of Models and Empirical Studies," *Review of Environmental Economics and Policy*, 2019.

Brodeur, Abel, "The Effect of Terrorism on Employment and Consumer Sentiment: Evidence from Successful and Failed Terror Attacks," *American Economic Journal: Applied Economics*, October 2018, *10* (4), 246–82.

Cao, Changyong, Frank Deluccia, Shuang Qiu, and NOAA JPSS Program Office, "NOAA JPSS Visible Infrared Imaging Radiometer Suite (VIIRS) Sensor Data Record (SDR) from IDPS," 2012. Accessed: 2018-09-30.

Chandrasekhar, Arun G, Melanie Morten, and Alessandra Peter, "Network-based hiring: Local benefits; global costs," Technical Report, National Bureau of Economic Research 2020.

Collier, Paul et al., *Breaking the conflict trap: Civil war and development policy*, World Bank Publications, 2003.

Fielding, David, "How does violent conflict affect investment location decisions? Evidence from Israel during the Intifada," *Journal of Peace Research*, 2004, *41* (4), 465–484.

Frias-Martinez, Vanessa, Jesus Virseda, Alberto Rubio, and Enrique Frias-Martinez, "Towards large scale technology impact analyses: Automatic residential localization from mobile phone-call data," in "Proceedings of the 4th ACM/IEEE international conference on information and communication technologies and development" 2010, pp. 1–10.

Ghiasy, Richard, Jiayi Zhou, and Henrik Hallgren, *Afghanistan's Private Sector: Status and Ways Forward*, SIPRI, 2015.

Glaeser, Edward L, *Agglomeration economics*, University of Chicago Press, 2010.

Guidolin, Massimo and Eliana La Ferrara, "Diamonds Are Forever, Wars Are Not: Is Conflict Bad for Private Firms?," *American Economic Review*, 2007, *97* (5), 1978–1993.

Hallward-Driemeier, Mary and Lant Pritchett, "How Business is Done in the Developing World: Deals Versus Rules," *Journal of Economic Perspectives*, 2015, *29* (3), 121–40.

Hanna, Rema, Gabriel Kreindler, and Benjamin A. Olken, "Citywide effects of high-occupancy vehicle restrictions: Evidence from "three-in-one" in Jakarta," *Science*, July 2017, *357* (6346), 89–93.

Henderson, J Vernon, Adam Storeygard, and David N Weil, "Measuring Economic Growth from Outer Space," *American Economic Review*, 2012, *102* (2), 994–1028.

Hu, Yingyao and Jiaxiong Yao, "Illuminating economic growth," *Journal of Econometrics*, 2022, *228* (2), 359–378.

Jean, Neal, Marshall Burke, Michael Xie, W Matthew Davis, David B Lobell, and Stefano Ermon, "Combining satellite imagery and machine learning to predict poverty," *Science*, 2016, *353* (6301), 790–794.

Klapper, Leora and Christine Richmond, "Patterns of business creation, survival and growth: Evidence from Africa," *Labour Economics*, 2011, *18*, S32–S44.

Ksoll, Christopher, Rocco Macchiavello, and Ameet Morjaria, "Guns and Roses: Flower exports and electoral violence in Kenya," *Buffett Institute Global Poverty Research Lab Working Paper*, 2016, (17-102).

Macchi, Elisa and Jeremia Stalder, "Work Over Just Cash: Informal Redistribution," 2023.

Miguel, Edward and Gerard Roland, "The long-run impact of bombing Vietnam," *Journal of development Economics*, 2011, *96* (1), 1–15.

North, Douglass C, *Institutions, Institutional Change and Economic Performance*, Cambridge University Press, 1990.

Puga, Diego, "The Magnitude and Causes of Agglomeration Economies," *Journal of Regional Science*, 2010, *50* (1), 203–219.

Roth, Jonathan, "Pretest with Caution: Event-Study Estimates after Testing for Parallel Trends," *American Economic Review: Insights*, September 2022, *4* (3), 305–22.

Svensson, Jakob, "Investment, Property Rights and Political Instability: Theory and Evidence," *European Economic Review*, 1998, *42* (7), 1317–1341.

Toole, Jameson L., Yu-Ru Lin, Erich Muehlegger, Daniel Shoag, Marta C. González, and David Lazer, "Tracking employment shocks using mobile phone data," *Journal of The Royal Society Interface*, June 2015, *12* (107), 20150185.

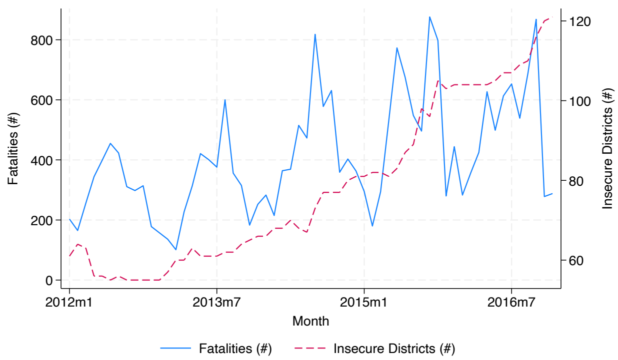
Vanhoof, Maarten, Fernando Reis, Thomas Ploetz, and Zbigniew Smoreda, "Assessing the quality of home detection from mobile phone data for official statistics," *Journal of Official Statistics*, 2018, *34* (4), 935–960.

Warren, Rachel, Emily Aiken, and Joshua Blumenstock, "Note: Home Location Detection from Mobile Phone Data: Evidence from Togo," in "ACM SIGCAS/SIGCHI Conference on Computing and Sustainable Societies (COMPASS)" 2022, pp. 685–692.

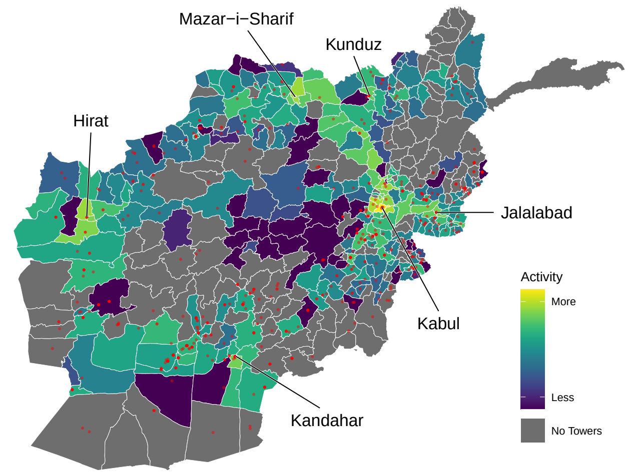
World Bank, "Afghanistan Development Update.," Technical Report, World Bank 2015.

_ , "Enterprise Surveys," Technical Report, World Bank. https://www.enterprisesurveys.org.

7 Tables and Figures



(a) Fatalities and Insecure Districts (2012-2016)

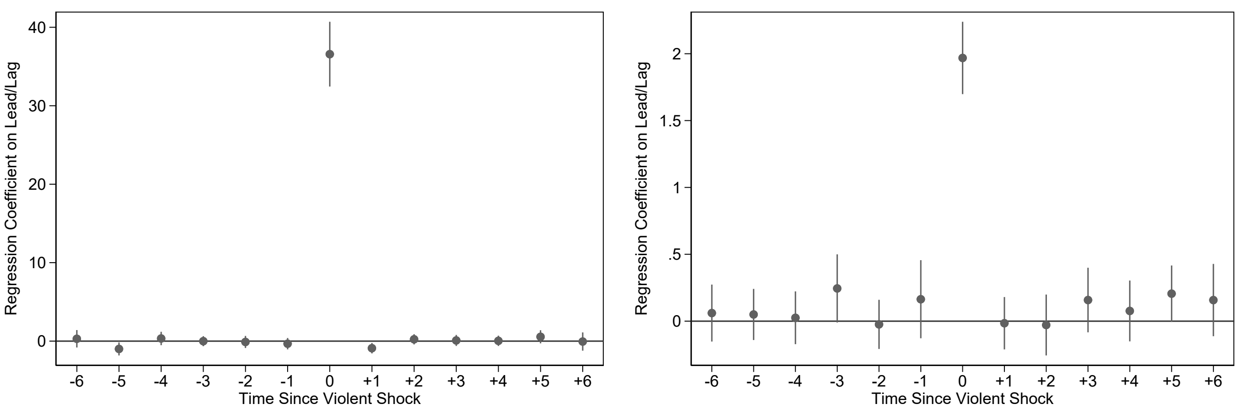


(b) Corporate Line Activity and Fatalities

Figure 1: Rising Insecurity and Corporate Mobile Phone Activity

Notes: (a) Confirmed fatalities per month, according to the GTD (solid line, left axis), and the number of insecure districts, based on internal security tracking data from a national survey firm (dashed line, right axis). (b) Map of Afghanistan, with districts colored based on the log number of active firms per district in April 2013; districts without any mobile coverage are shown in grey. Red dots mark locations of confirmed fatalities recorded in Global Terrorism Database (GTD) for May 2012-April 2013.

Figure 2: Violent Shocks and Surrounding Violence

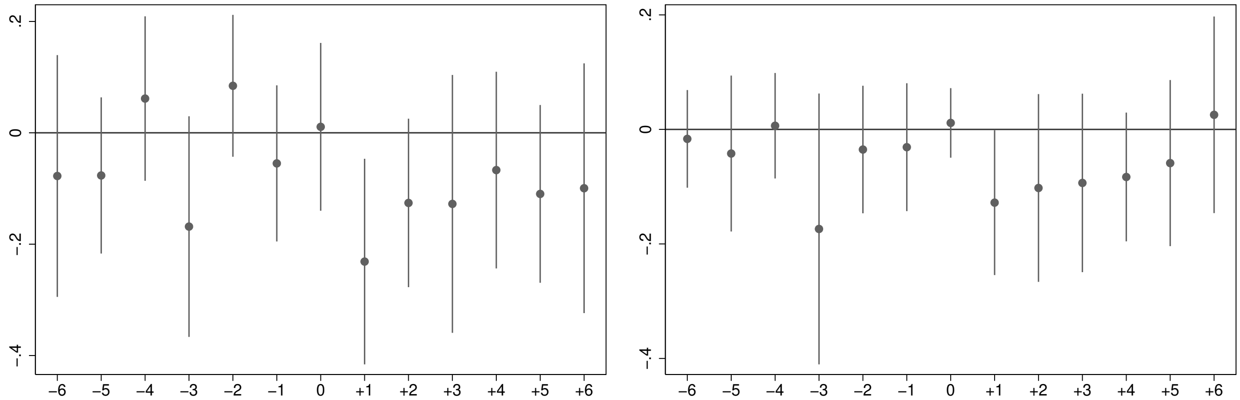


(a) Fatalities

(b) Events

Notes: Figure shows estimates of lower levels of violence occurring prior to and following violent shocks. This figure shows estimates, for districts with a violent shock in t_0, the amount of violence by other measures in the same district in the six months before and after t_0. The dependent variable in Panel A is the number of fatalities; Panel B uses the number of terrorist-linked events. Time = 0 indicates the period in which a violent shock was observed. Negative indices on the x-axis correspond to months prior to a violent shock while positive indices reflect months after a shock. Each regression includes six leads and lags, district fixed effects, time fixed effects, and linear and quadratic district time trends. Dots indicate coefficients; vertical bars indicate 95% confidence intervals.

Figure 3: Event Studies of Firm Response to Violent Shocks



(a) Any Presence

(b) Intense Presence

Notes: Figure shows event studies of violent shocks in relative time (indicated on the x-axis), t, on current firm presence using the "any" presence measure in panel (a) and the "intense" measure of firm presence in panel (b). These event studies are estimated using equation (2), see paper text for details. Dots represent coefficients and bars indicate 95% confidence intervals.

Table 1: Location Validation

Panel A: Headquarters

		% HQ Match	
	Obs	Top 1 Modal "Primary"	Top 5 Modal
AISA	110	81.82	91.82
CBR	934	72.06	82.98
Survey	406	78.08	87.93
All Combined	1119	73.28	84.45

Panel B: All Offices

	Obs	Num of Offices	% Office Match Top 5 Modal
Survey 2017 Response	406	2.71	62.06
Survey 2014 Response	395	2.39	64.54
Survey All	801	2.55	61.52

Notes: This table shows validation and comparison between measures of high firm presence detected from the first six months of the CDR data and those recorded in other administrative sources and reported in our own survey. Top X Modal indicates that it was among the top X most observed districts for that firm during the first six months of the data. Observation is a firm in Panel A and a firm-year in Panel B.

Table 2: Aggregate Economic Validation with Nightlights Data

	Nightlight Growth (%)		
Panel A: Total Active Firms	(1)	(2)	(3)
Firms Growth (%)	0.35***	0.29***	0.29***
	(0.06)	(0.05)	(0.05)
Constant	0.02***	0.02***	0.02***
	(0.00)	(0.00)	(0.00)
# Districts	173	173	173
# Observations	6569	6569	6569
R-Squared	0.007	0.235	0.236
Month FE	NO	YES	YES
District FE	NO	NO	YES

	Nightlight Growth (%)		
Panel B: Total Active Subscribers	(1)	(2)	(3)
Subscribers Growth (%)	0.34***	0.28***	0.29***
	(0.06)	(0.06)	(0.06)
Constant	0.02***	0.02***	0.02***
	(0.00)	(0.00)	(0.00)
# Districts	173	173	173
# Observations	6569	6569	6569
R-Squared	0.007	0.235	0.236
Month FE	NO	YES	YES
District FE	NO	NO	YES

Notes: Observation is a district-month. Standard errors clustered at district level. Columns include fixed effects for month of year and district as indicated. Nightlight Growth (%), Firms Growth (%) and Subscribers Growth (%) are calculated as the first difference of the logarithm of values between adjacent months in the same district, where Nightlights is sum of nightlight pixel values, Firms is the count of total active firms, and Subscribers is the count of active subscribers. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Firm Response to Violent Shocks

	(1)	(2)	(3)	(4)
Panel A: Any Presence	Firm employee ever present in district (=100)			
Violent Shock (=1)	-0.995**	-1.003**	-0.535**	-0.143***
	(0.477)	(0.496)	(0.218)	(0.054)
Mean Outcome	4.996	4.996	4.996	4.996
β / Mean	-0.199	-0.201	-0.107	-0.029
Observations	15809432	15809432	15809432	15809432
Adjusted R2	0.580	0.582	0.582	0.584
Panel B: Intense Presence	Firm employee with primary location in district (=100)			
Violent Shock (=1)	-0.719	-0.801	-0.193*	-0.087**
	(0.447)	(0.509)	(0.113)	(0.040)
Mean Outcome	1.713	1.713	1.713	1.713
β / Mean	-0.420	-0.468	-0.113	-0.051
Observations	15809432	15809432	15809432	15809432
Adjusted R2	0.686	0.686	0.687	0.688
Firm-district FEs	Yes	Yes	Yes	Yes
Year-month FEs	No	Yes	Yes	Yes
District-calendar month FEs	No	Yes	Yes	Yes
Active towers	No	No	Yes	Yes
District time trends	No	No	No	Yes

Notes: Each observation is a firm-district-month. Dependent variable in Panel A equals 100 if any call was made by any employee of that firm in that district-month, and 0 otherwise. Dependent variable in Panel B equals 100 if the primary calling tower for at least one of the firm's employees was in that district during that month, and 0 otherwise. Violent Shock equals 1 if previous month's killings in that district were in top 1% of killings distribution, and 0 otherwise. Controls listed on the bottom of the table apply to both Panel A and B, which are estimated separately. District time trends include both district linear and quadratic trends. Tower controls include linear and quadratic controls for number of tower-days of coverage in that month. Standard errors are clustered at district level. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Firm Entry and Exit after Violent Shocks

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Base specification	Any Presence			Intense Presence		
	Active	Entry	Exit	Active	Entry	Exit
Violent Shock (=1)	-0.143***	-0.102***	0.080*	-0.087**	-0.041*	0.061*
	(0.054)	(0.034)	(0.042)	(0.040)	(0.021)	(0.035)
Mean Outcome	4.996	1.428	1.474	1.713	0.248	0.258
β / Mean	-0.029	-0.072	0.054	-0.051	-0.167	0.236
Observations	15809432	15411013	15411013	15809432	15411013	15411013
Adjusted R2	0.584	0.091	0.092	0.688	0.069	0.069
Panel B: Heterogeneity	Any Presence			Intense Presence		
	Active	Entry	Exit	Active	Entry	Exit
Violent Shock (=1)	-0.021	-0.137**	0.035	0.122	-0.043	0.067
	(0.095)	(0.065)	(0.049)	(0.137)	(0.029)	(0.043)
Violent Shock x Primary	-1.131***	0.314***	0.473***	-1.774***	0.053	0.180**
	(0.095)	(0.097)	(0.092)	(0.110)	(0.055)	(0.091)
Mean Non-Prim	4.449	1.371	1.420	1.275	0.225	0.234
Mean Prim	76.080	2.185	2.922	72.888	2.729	3.512
β_1 / Mean Non-Primary	-0.005	-0.100	0.024	0.096	-0.193	0.286
$(\beta_1 + \beta_2)$ / Mean Primary	-0.015	0.081	0.174	-0.023	0.003	0.070
P-value: $\beta_1 + \beta_2 = 0$	0.000	0.000	0.000	0.000	0.837	0.000
Observations	13445387	13445387	13445387	13445387	13445387	13445387
Adjusted R2	0.603	0.093	0.095	0.713	0.072	0.073

Notes: Each observation is a firm-district-month. Binary dependent variables are all scaled by 100 for readability. 'Active' is a measure of firm presence, as in Table 3. 'Entry' indicates an observation where the firm is present in that district in t but was not in the previous month $t-1$. 'Exit' indicates an observation where firm is not present in t but was present in $t-1$. Columns 2, 3, 5, and 6 have fewer observations in Panel A because a 1-month lag is needed to code the outcome variable. 'Primary' indicates a firm's primary district, as defined over the first 6 months the firm appears in the data. Panel B has fewer observations than Panel A because those 6 months are required to identify each firm's primary district. In Panel B, relevant means of the dependent variable are shown for non-primary and primary districts and then scaled effect sizes are shown for shocks in non-primary and primary districts. All regressions include firm-district, time, and district-calendar month fixed effects, as well as district linear and quadratic time trends. Standard errors are clustered at district level. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Shocks, States, and Primary Locations

	Outcome: Any Presence			Outcome: Intense Presence		
	(1)	(2)	(3)	(4)	(5)	(6)
Violent Shock (=1)	-0.158**			-0.094*		
	(0.065)			(0.054)		
Insecure District (=1)	-0.030		-0.030	-0.067*		-0.067*
	(0.072)		(0.072)	(0.040)		(0.039)
Insecure District x Primary		-8.542***			-10.425***	
		(0.428)			(0.451)	
Insecure District x Non-Primary		-0.011			-0.049	
		(0.080)			(0.036)	
Violent Shock x Insecure District			-0.046			-0.027
			(0.106)			(0.046)
Violent Shock x Secure District			-0.186**			-0.110*
			(0.073)			(0.059)
Mean Outcome	4.858	4.858	4.858	1.684	1.684	1.684
Mean Group 1	4.858	76.080	2.425	1.684	72.888	0.864
Mean Group 2		4.449	5.076		1.275	1.757
P-value: Test of equality	.	0.000	0.281	.	0.000	0.165
Observations	13445387	13445387	13445387	13445387	13445387	13445387
Adjusted R2	0.603	0.603	0.603	0.713	0.713	0.713

Notes: Each observation is a firm-district-month. Binary dependent variables are scaled by 100 for readability. 'Insecure District' indicates that the observation is located in a district that was considered insecure by an independent firm's internal assessment. 'Primary' indicates a firm's primary district, as defined over the first 6 months the firm appears in the data. In columns 3 and 7, 'Mean group 1' indicates the mean of thd dependent variable for primary districts, while Mean Group 2 is the mean for non-primary districts. In columns 4 and 8, Mean Group 1 is the mean of insecure districts whereas Mean Group 2 is the mean for secure districts. The P-value tests for equality of the two interacted Violent Shock (VS) coefficients. In columns 3 and 5 this is a test of equivalency in the effects of violent shocks across primary and non-primary districts. All regressions include firm-district, time, and district-calendar month fixed effects as well as district linear and quadratic time trends. Standard errors are clustered at district level. *** p<0.01, ** p<0.05, * p<0.1.

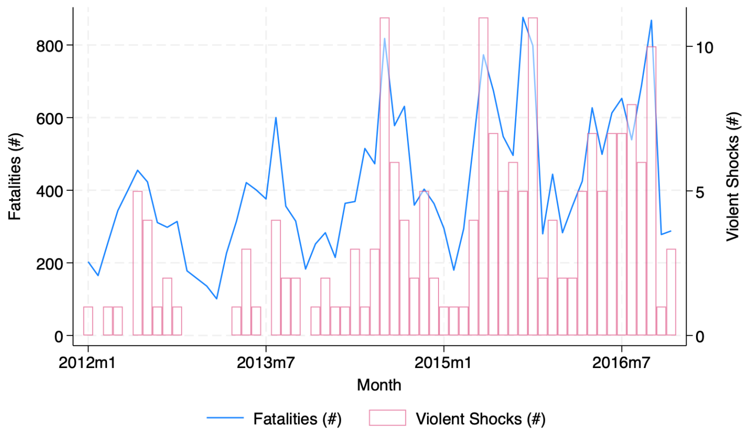
Table 6: Provincial and Neighbor Effects, District Trends

	Any Presence		Intense Presence	
	(1)	(2)	(3)	(4)
Violent Shock (=1)	-0.1477***	-0.0651	-0.0881**	-0.0033
	(0.0542)	(0.0750)	(0.0402)	(0.0215)
Violent Shock x Capital		-0.1517		-0.1572***
		(0.0973)		(0.0541)
Violent Shock Elsewhere in Province	-0.0466*		-0.0082	
	(0.0268)		(0.0104)	
Violent Shock Elsewhere in Province - Capital		-0.0708*		-0.0167
		(0.0369)		(0.0145)
Violent Shock Elsewhere in Province - Rural		-0.0122		0.0046
		(0.0327)		(0.0122)
Mean All	4.996	4.996	1.713	1.713
Mean Capital	8.989	8.989	5.303	5.303
Mean Rural	4.158	4.158	0.960	0.960
P-value: $\beta_1 + \beta_2 = 0$	.	0.001	.	0.002
Observations	15809432	15809432	15809432	15809432
Adjusted R2	0.584	0.584	0.688	0.688

Notes: Each observation is a firm-district-month. All violent shocks are from the previous period (first lag). Binary dependent variables are scaled by 100 for readability. Capital is an indicator for whether the district is a provincial capital. Row (2) is an interaction of the (lagged) violent shock with an indicator for capital district. The next three rows signify shocks occurring within the same province as a given district, excluding any shocks occurring in the district itself. Rows (4) and (5) restrict whether these shocks occurred in the provincial capital or another rural district. Rural districts are defined as all districts that are not provincial capitals. All other controls are the same as those in equation (1) and used in estimating column (4) of Table 3. *** p<0.01, ** p<0.05, * p<0.1.

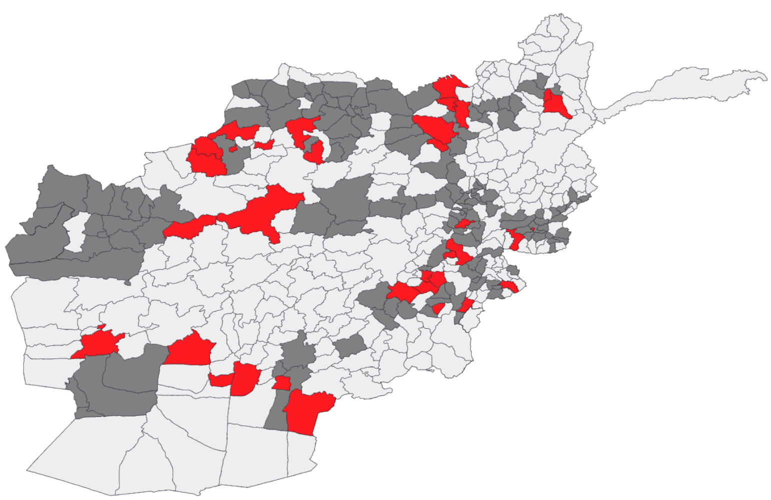
A1 Online Appendix Figures and Tables

Figure A1: Fatalities and Violent Shocks



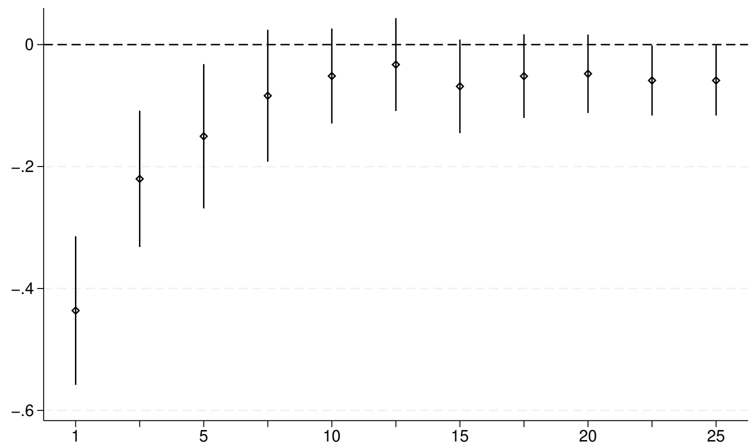
Notes: Figure shows confirmed fatalities per month, according to the GTD (solid line, left axis), and the total number of Violent Shocks per month, using the threshold of 23 or more GTD fatalities in a district (bars, right axis).

Figure A2: Spatial Distribution of Violent Shocks



Notes: Figure shows map of Afghanistan. Red-colored districts experienced violent shocks during the study period, while dark grey districts did not. Districts without continuous tower coverage are show in light grey, following the definition in the main estimation sample. (Note that this sample restriction implies more districts are coded light grey here than those without any coverage in the main Figure 1b).

Figure A3: Alternative Violent Shock Thresholds - Any Presence



Notes: Figure shows results from a set of estimates varying the threshold of violent shocks used for the lagged measure of the shock. This is defined as being the top x percentile of number of people killed among district-months with positive fatalities. The shock used in the main paper is 6, indicating a district-month in the top 6% of people killed in terrorist attacks which is approximately the top 1% of all observations independent of having any fatalities in terrorist linked attacks in that district-month. The Y-axis shows the estimate of the effect of this shock with the dependent variable of any presence scaled by 100 for readability. 90% confidence intervals are illustrated around the point estimates. The X-axis indicates the percentile threshold of fatalities for a violent shock.

Table A1: Firm Survey Responses

Survey Question	**Mean**	**Std. Dev.**	**N**
Panel A: Business obstacles			
Most important business obstacle - answer includes insecurity (=1)	0.813	0.391	406
Security was an important business obstacle last year (=1)	0.911	0.285	406
Power cuts were an important business obstacle last year (=1)	0.858	0.349	402
Labor problems were an important business obstacle last year (=1)	0.816	0.388	402
Lack of infrastructure an important business obstacle last year (=1)	0.755	0.431	396
Panel B: Concerns about anti-government groups			
Very or extremely affected by insecurity from anti-government groups (=1)	0.784	0.412	403
Very or extremely concerned about land mines and IEDs on roads (=1)	0.851	0.356	403
Very or extremely concerned about attacks with small arms fire (=1)	0.836	0.371	403
Very or extremely concerned about kidnappings (=1)	0.831	0.375	403
Very or extremely concerned about attacks with suicide bombs (=1)	0.93	0.255	402
Panel C: Effects of anti-government groups			
Local employees ever threatened by anti-government groups (=1)	0.226	0.419	403
Local employees ever injured by anti-government groups (=1)	0.082	0.275	402
Local employees ever killed by anti-government groups (=1)	0.052	0.222	405
Firm assets ever threatened or destroyed by anti-government groups (=1)	0.245	0.431	404
Infrastructure ever threatened or destroyed by anti-government groups (=1)	0.58	0.494	402
Panel D: Has your business ever done the following in response to anti-government groups?			
–Spent additional money on private security (=1)	0.457	0.499	403
–Spent money for protection payments (=1)	0.333	0.472	400
–Experienced fall in demand (=1)	0.357	0.48	398
–Delayed an investment in that city or district? (=1)	0.283	0.451	403
–Moved staff away from that city or district (=1)	0.283	0.451	399
–Decreased deliveries to or movement in that city or district? (=1)	0.286	0.453	398
–Changed transportation route? (=1)	0.398	0.49	402
–Changed suppliers to that city or district? (=1)	0.188	0.391	394
–Changed your buyers in that city or district? (=1)	0.151	0.359	397
–Stopped operating in that city or district temporarily? (=1)	0.308	0.462	402
–Stopped operating in that city or district permanently? (=1)	0.075	0.263	402

Notes: Data from original survey of 406 Afghan business owners conducted in 2017, see text for details.

Table A2: Survey Instrument Representativeness Table

	Enterprise Survey (Survey Vars)	CDR Sample (CDR Vars)	CDR Surveyed Sample (CDR Vars)	Survey Sample (Survey Vars)
Num Employees At Present	21.375	52.261	54.788	33.970
Sector Trade (=1)	0.397	0.112	0.103	0.073
Sector Manufacturing (=1)	0.355	0.134	0.379	0.271
Sector Construction (=1)	0.104	0.190	0.185	0.268
Sector Transport (=1)	0.144	0.119	0.106	0.148
Sector Security (=1)	0.000	0.015	0.012	0.010
Sector Finance (=1)	N/A	0.012	0.017	0.033
Sector Information Technology (=1)	N/A	0.005	0.010	N/A
Sector Other (=1)	0.000	0.408	0.187	0.178
HQ in Kabul (=1)	0.404	0.615	0.603	0.700
HQ in Hirat (=1)	0.192	0.167	0.200	0.200
HQ in Balkh (=1)	0.137	0.079	0.103	0.079
HQ in Nangahar (=1)	0.146	0.029	0.025	0.020
HQ in Kandahar (=1)	0.122	0.024	0.012	0.000
HQ in Kunduz (=1)	N/A	0.020	0.012	0.002
N	416	2292	406	406

Notes: Mean values reported for each variable. Enterprise survey means reweighted to reflect nationally representative population. Columns 2 and 3 utilize CDR variables. CDR "Num Employees At Present" calculated based on total MSISDNS for each firm in 2016. CDR sector code was calculated based on a category provided by the phone company, matched to the corresponding two-digit ISIC code (Rev. 4). CDR headquarters are calculated using the firm's first modal district as a proxy. CDR Surveyed refers to the firms in CDR who were surveyed. Columns 1 and 4 utilize survey variables. 'Sectors' and 'Number of Employees at Present' are self-reported, as provided by each survey. World Bank (Enterprise) sector code was calculated based on the four-digit ISIC code (Rev. 3) reported for the primary good or service produced by each firm. Survey headquarters are self-reported, as provided by each survey.

Table A3: Summary Statistics

	Mean	SD	Min	Med	Max
Panel A: Firm Level (N=2,292)					
Total Months Active	32.42	15.05	1.00	41.00	45.00
Total Districts Active	33.75	33.25	1.00	22.00	172.00
Mean Active Districts Per Month	8.53	13.76	0.02	3.60	140.98
Total Employees / Subscribers	33.19	205.84	1.00	4.00	8341.00
Total Calls	94714	813687	1	12279	36102988
Primary Location = Kabul (=1)	0.60	0.49	0.00	1.00	1.00
Primary Location = Provincial Capital (=1)	0.31	0.46	0.00	0.00	1.00
Primary Location = Rural (=1)	0.08	0.27	0.00	0.00	1.00
Active in Primary District (=1)	0.79	0.31	0.02	0.98	1.00
Panel B: District-Month Level (N=7,785)					
Total Firms (Any)	80.90	282.59	0.16	19.15	3934.13
Total Firms (Intense)	36.81	245.10	0.04	3.94	3659.75
Total Employees / Subscribers (Any)	154.81	707.76	0.18	27.22	10351.73
Total Employees / Subscribers (Intense)	82.35	621.23	0.04	5.61	9373.72
Total Calls	11053	84134	3	442	1233656
Total Killed	1.28	5.80	0.00	0.00	244.00
Violent Shock (=1)	0.01	0.09	0.00	0.00	1.00
Insecure Province	0.36	0.48	0.00	0.00	1.00
Panel C: Firm-District-Month Level (N=15,809,432)					
Firm Any Activity in District (=1)	0.050	0.22	0.00	0.00	1.00
Firm Any Entry to District (=1)	0.014	0.12	0.00	0.00	1.00
Firm Any Exit from District (=1)	0.015	0.12	0.00	0.00	1.00
Firm Intense Activity In District (=1)	0.017	0.13	0.00	0.00	1.00
Firm Intense Entry to District (=1)	0.002	0.05	0.00	0.00	1.00
Firm Intense Exit from District (=1)	0.003	0.05	0.00	0.00	1.00

Notes: This table shows summary statistics for different levels of aggregation of our dataset of employee mobile phone records. Panel A shows firm-level characteristics, where an 'employee' indicates a mobile subscriber linked to a specific firm's corporate account. Panel B shows district-month level variable,s which include aggregate measures of firm presence and violence (including the two main 'treatment' variables: Violent Shocks and Insecure Province). Panel C provides summary statistics at the level of the firm-district-month, which is the primary unit of observation in our empirical analysis. This is an unbalanced panel of firms (since different firms appear at different points in the panel).

Table A4: Firm District Activity - Alternative Violence Definitions

	(1)	(2)	(3)	(4)	(5)	(6)
	Firm has employee who is active in district (=100)					
Number of Deaths (1 lag)	-0.004***					
	(0.001)					
1-3 Deaths (0-50%)		-0.051***				
		(0.016)				
4-7 Deaths (50-75%)		-0.015				
		(0.028)				
8-22 Deaths (75-95%)		-0.055*				
		(0.031)				
23+ Deaths (>95%)		-0.166***				
		(0.058)				
Kills/100K people			-0.002***			
			(0.001)			
0-3.5 Deaths/100K Pop (0-50%, > 0)				-0.034*		
				(0.018)		
3.5-8.75 Deaths/100K Pop (50-75%, > 0)				-0.057***		
				(0.021)		
8.75-30 Deaths/100K Pop (75-95%, > 0)				-0.058*		
				(0.032)		
>30 Deaths/100K Pop (>95%, > 0)				-0.095		
				(0.061)		
Biggest Event					-0.074	
					(0.048)	
Biggest Two Events						-0.068
						(0.042)
Mean Outcome	4.996	4.996	4.998	4.996	4.996	4.996
Observations	15809432	15809432	15352512	15809432	15809432	15809432
Adjusted R2	0.584	0.584	0.586	0.584	0.584	0.584

Notes: Observation is a firm-district-month. Dependent variable is indicator for any presence, whether any employee linked to the firm made any calls in a given district and month, scaled by 100. All independent variables represent one month lagged measures of violence. Column 1 shows the effects of a continuous number of terrorist attack-linked deaths. Column 2 splits this continuous variable into 4 bins. Column 3 scales the number of people killed by local population size and column 4 divides this per capitized version into bins. Percentage ranges indicate where this level of deaths falls in the distribution of district-months with positive number of deaths. Biggest Event indicates the district-month with the highest number of terrorist-linked deaths for that district whereas Biggest Two Events indicates the two months with the highest number of casualties. Standard errors clustered at district level. *** p<0.01, ** p<0.05, * p<0.1.

Table A5: Effects of Major Events on Alternative Measures of Firm and Employee Presence

Panel A	(1) Emps-Any	(2) Emps-Intense	(3) Calls	(4) log(Emps-Any)	(5) log(Emps-Intense)	(6) log(Calls)	(7) 2 Calls
Violent Shock (=1)	-0.003	-0.005*	-2.511**	-0.002***	-0.001**	-0.005**	-0.092**
	(0.006)	(0.003)	(1.233)	(0.001)	(0.000)	(0.002)	(0.036)
Mean Outcome	0.250	0.106	13.731	0.058	0.021	0.143	3.256
β / Mean	-0.011	-0.044	-0.183	-0.026	-0.041	-0.038	-0.028
Observations	15809432	15809432	15809432	15809432	15809432	15809432	15809432
Adjusted R2	0.831	0.844	0.821	0.780	0.832	0.778	0.620

Panel B	Emps-Any	Emps-Intense	Calls	log(Emps-Any)	log(Emps-Intense)	log(Calls)	2 Calls
Violent Shock (=1)	0.027	0.033	2.277	0.002	0.003	0.006	0.056
	(0.025)	(0.030)	(2.560)	(0.002)	(0.003)	(0.007)	(0.103)
Violent Shock x Primary	-0.269***	-0.337***	-51.567***	-0.030***	-0.037***	-0.104***	-1.265***
	(0.024)	(0.020)	(3.160)	(0.001)	(0.002)	(0.005)	(0.085)
Mean Non-Primary	0.182	0.049	6.114	0.049	0.014	0.116	2.773
Mean Primary	11.875	9.949	1355.175	1.227	1.137	4.251	74.325
β_1 / Mean Non-Primary	0.150	0.675	0.372	0.036	0.243	0.049	0.020
β_2 / Mean Primary	-0.020	-0.031	-0.036	-0.023	-0.030	-0.023	-0.016
P-value: $\beta_1 + \beta_2 = 0$	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Observations	13445387	13445387	13445387	13445387	13445387	13445387	13445387
Adjusted R2	0.927	0.936	0.922	0.809	0.861	0.802	0.643

Notes: This table shows the main effects of violent shocks on firm presence using alternative, intensive measures of firm activity in a given district-month from the CDR. The number of associated employees (emps, subscribers linked to a corporate account) are counted in terms of any or intense presence. Columns 1-3 use the levels of each variable whereas columns 4-6 convert these measures to logs using a transformation of the form Log(1+x) to adjust for skewness of the underlying distribution without dropping zero-valued observations. Emps is a continuous count of the number of unique associated employees who are present in a given district-month for a given firm. The definition of presence is indicated in the column headers following the definitions used throughout the paper. Calls are simply the aggregate number of calls placed from a given district in that month among all affiliated employees for that firm. Column 7 again uses a binary indicator of firm presence but sets the threshold at a minimum of two calls placed from that district by a mobile number linked to that firm in that month. In Panel B, relevant means of the dependent variable are shown for non-primary and primary districts and then scaled effect sizes are shown for shocks in non-primary and primary districts. The reported P-value is a test of shocks in primary districts against zero. All estimates use the full specification from equation 1 while Panel B adds the interaction term of violent shocks and primary location. Standard errors clustered at district level. *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Robustness - Work Week, Towers, Provincial Clustering

	(1)	(2)	(3)
Panel A:	Any Presence (=100)		
Violent Shock	-0.109**	-0.116**	-0.143***
	(0.048)	(0.188)	(0.041)
Mean Outcome	4.441	4.499	4.996
β / Mean	-0.025	-0.026	-0.029
Adjusted R2	0.580	0.581	0.584
Panel B:	Intense Presence (=100)		
Violent Shock	-0.063**	-0.072**	-0.087*
	(0.230)	(0.093)	(0.044)
Mean Outcome	1.689	1.532	1.713
β / Mean	-0.038	-0.047	-0.051
Adjusted R2	0.685	0.684	0.687
Clustering	District	District	Province
Panel	Work week	Full	Full
Min Tower Coverage	28 Days	14 Days	28 Days
Observations	15809432	18381651	15809432

Notes: This table shows additional robustness checks to the main specification. Column 1 uses a work week panel where only calls occurring during the work week are used to identify firm presence. Column 2 uses the main paper's coding of firm presence and shows robustness of the main results to a more relaxed constraint of tower day coverage so that districts are only dropped that experience a month with less than 14 days of tower coverage. Column 3 shows robustness to provincial, instead of district, level clustering Each observation is a firm-district-month. All regressions include time fixed effects, district-firm fixed effects, district-season fixed effects, and district linear and quadratic trends. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A7: Heterogeneity: Firm Type

	(1)	(2)	(3)	(4)	(5)
Panel A: Any Presence	Construction	Trade	Manufacturing	Transport	Other
Violent Shock (=1)	-0.364***	0.067	0.041	-0.278*	-0.119**
	(0.134)	(0.121)	(0.145)	(0.163)	(0.054)
Mean Outcome	5.033	4.409	5.172	5.565	4.930
β / Mean	-0.072	0.015	0.008	-0.050	-0.024
Observations	3088396	1874801	2018045	1915456	6912734
Adjusted R2	0.555	0.554	0.590	0.604	0.596
Panel B: Intense Presence	Construction	Trade	Manufacturing	Transport	Other
Violent Shock (=1)	-0.117	0.047	-0.182***	-0.027	-0.100**
	(0.095)	(0.084)	(0.061)	(0.069)	(0.042)
Mean Outcome	1.592	1.290	1.664	1.827	1.864
β / Mean	-0.074	0.037	-0.109	-0.015	-0.054
Observations	3088396	1874801	2018045	1915456	6912734
Adjusted R2	0.667	0.706	0.692	0.683	0.693

Notes: This table shows the effects of violence on firm presence, splitting the sample by different firm industry (type) categories indicated at the top of each column. The estimation uses the specification in equation (1), including time fixed effects, district-firm fixed effects, tower controls, district-season fixed effects, and linear and quadratic trends. Panel A uses an indicator for any firm presence whereas the Panel B uses an indicator for intense firm presence as the outcome.

Table A8: Event Studies

	(1) Any	(2) Intense
Violent Shock (t+6)	-0.078	-0.017
	(0.110)	(0.043)
Violent Shock (t+5)	-0.077	-0.042
	(0.071)	(0.069)
Violent Shock (t+4)	0.062	0.006
	(0.075)	(0.047)
Violent Shock (t+3)	-0.168*	-0.174
	(0.100)	(0.120)
Violent Shock (t+2)	0.084	-0.035
	(0.065)	(0.056)
Violent Shock (t+1)	-0.055	-0.031
	(0.071)	(0.057)
Violent Shock (t=0)	0.011	0.011
	(0.076)	(0.031)
Violent Shock (t-1)	-0.231**	-0.128**
	(0.094)	(0.064)
Violent Shock (t-2)	-0.126	-0.102
	(0.077)	(0.083)
Violent Shock (t-3)	-0.128	-0.093
	(0.117)	(0.079)
Violent Shock (t-4)	-0.067	-0.083
	(0.089)	(0.057)
Violent Shock (t-5)	-0.110	-0.059
	(0.081)	(0.073)
Violent Shock (t-6)	-0.100	0.025
	(0.114)	(0.087)
Mean Outcome	5.164	1.759
F-Test of Leads	0.009	0.279
F-Test Sum of Leads	0.494	0.388
Observations	13445387	13445387
Adjusted R2	0.590	0.698

Notes: Observation is a firm-district-month. Column headers indicate the measure of firm presence used as the dependent variable. All regressions include time fixed effects, district-firm fixed effects, tower controls, district-season fixed effects, and district linear and quadratic trends. Standard errors clustered at district level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A9: Entry and Exit - Wider Windows

Dependent Variable:	Entry				Exit			
	Any Presence		Intense Presence		Any Presence		Intense Presence	
Pre Periods	1	3	1	3	1	3	1	3
Post Periods	3	1	3	1	3	1	3	1
Panel A:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Violent Shock (=1)	-0.015	-0.061**	-0.006	-0.028*	0.061	0.068**	0.051	0.030
	(0.020)	(0.029)	(0.014)	(0.015)	(0.037)	(0.032)	(0.034)	(0.019)
Mean Outcome	0.287	0.788	0.086	0.137	0.877	0.310	0.154	0.093
β / Mean	-0.052	-0.077	-0.067	-0.202	0.069	0.219	0.332	0.320
Observations	14620922	14620922	14620922	14620922	14620922	14620922	14620922	14620922
Adjusted R2	0.034	0.033	0.028	0.026	0.034	0.035	0.025	0.028
Panel B:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Violent Shock (=1)	-0.023	-0.083*	-0.003	-0.031	0.048	0.033	0.039	0.018
	(0.024)	(0.043))	(0.011)	(0.023)	(0.038)	(0.024)	(0.027)	(0.018)
Violent Shock x Primary	0.068	0.051	-0.021	0.024	0.238***	0.294***	0.227***	0.175***
	(0.042)	(0.066)	(0.031)	(0.051)	(0.075)	(0.063)	(0.055)	(0.049)
Mean Non-Prim	0.267	0.773	0.074	0.129	0.846	0.295	0.141	0.080
Mean Prim	1.098	0.685	1.319	0.827	1.435	1.833	1.634	2.094
β_1 / Mean Non-Primary	-0.087	-0.108	-0.039	-0.239	0.057	0.111	0.274	0.223
(β_1 + β_2) / Mean Primary	0.041	-0.047	-0.018	-0.008	0.200	0.178	0.163	0.092
P-value: $\beta_1 + \beta_2 = 0$	0.067	0.374	0.423	0.811	0.000	0.000	0.000	0.000
Observations	12672077	13445387	12672077	13445387	12672077	13445387	12672077	13445387
Adjusted R2	0.034	0.033	0.029	0.026	0.035	0.035	0.027	0.029

Notes: This table generalizes the analysis of firm entry and exit to consider longer periods of time before and after violent shocks. The number of periods considered is indicated, in months, at the top of the table. For example, the outcome measure in column (4) is defined as firm entry into a district (using the intense measure of firm presence) where that firm had NOT been present in any of the 3 previous periods and then WAS present in the current period. By contrast, the outcome in column (7) defines firm exit as occurring when a firm WAS present in the previous period and then WAS NOT present in any of the following three periods. Each observation is a firm-district-month. Binary dependent variables are all scaled by 100 for readability. All regressions include firm-district, time, and district-calendar month fixed effects, as well as district linear and quadratic time trends. Standard errors are clustered at district level. *** p<0.01, ** p<0.05, * p<0.1.

Table A10: Shocks, States, and Primary Locations - Full Interactions

	Firm Presence Measure (=100)	
	Any	Intense
	(1)	(2)
Insecure x Primary	-8.139***	-10.299***
	(0.452)	(0.482)
Insecure x Not Primary	-0.014	-0.044
	(0.078)	(0.036)
Violent Shock x Insecure Dist x Primary	-3.835	-2.155
	(2.670)	(2.979)
Violent Shock x Secure Dist x Primary	-1.116***	-1.630***
	(0.095)	(0.118)
Violent Shock x Insecure Dist x Primary	-0.027	-0.011
	(0.110)	(0.043)
Violent Shock x Secure Dist x Not Primary	-0.026	0.152
	(0.123)	(0.171)
Overall Mean Outcome	4.858	1.684
Mean Insecure Primary	51.507	48.493
Mean Secure Primary	76.446	73.252
Mean Insecure Non-Primary	2.375	0.815
Mean Secure Non-Primary	4.636	1.317
P-value: Primary Equality	0.314	0.861
P-value: Non-Primary Equality	0.993	0.317
Observations	13445387	13445387
Adjusted R2	0.603	0.713

Notes: This table shows the full set of interactions that would build on the results in Table 5. P-values test for equality of coefficients between primary interactions and then non-primary interactions. All regressions include firm-district, time, and district-calendar month fixed effects as well as district linear and quadratic time trends. *** p<0.01, ** p<0.05, * p<0.1.

A2 CDR Data Appendix

Our study relies on data from one of Afghanistan's largest private telecommunications operators. This appendix describes the three different sources of information that are used in our empirical analysis, and the main steps in the corresponding data processing. These data do not contain the content of the phone calls and text messages, but only certain metadata about the communication. This includes the parties involved in the communication (anonymized id-s), as well as time and location of the communication. As we treat these data as sensitive and confidential, all personally identifying information was removed prior to our analysis. All research was reviewed and approved by the internal review boards at our respective institutions.

A2.1 Three Different Data Sources

Call Detail Records The central data source for this analysis is *call detail records* (CDRs). These are datasets, originating from the operator's communication logs, that provide basic information about every call (and text message) in the network. We observe CDRs for 45 months, from April 2013 till December 2016. The most important features in CDRs are date and time, caller's id, receiver's id, and id of the network antenna where the call was initiated (only present for calls). Approximately 250 million calls and a similar number of text messages were conducted in the network each month during the analyzed period. As we do not observe the antenna id for text messages, most of our analysis is based on calls. CDRs are what allows us the deduce the location of every single cellphone over time, given it is used frequently.

Antenna Locations The second and complementary source of information is the spatial location of network antennas. Typically several antennas are attached to a single structure (such as cellphone tower) and we only use the tower location in this study. We have geographic coordinates of 1350 towers, located in 267 districts (out of total 398 districts in Afghanistan). The covered region includes all cities and most of other more densely populated areas (see Figure 1a).

Corporate Subscribers The final related dataset is the list of corporate phones. For each month, the provider lists which phone id's are registered as business phones, and provides basic information about the corresponding businesses. We exclude public and non-profit organizations, such as health, education and media groups, or foreign embassies. We refer to the remaining phones as "corporate subscribers".

As phone numbers occasionally move between different accounts, we disregard numbers that are assigned to multiple businesses, that do not have valid business account id, or that have other irregularities (this amounts to approximately 0.5% of the business phones). Over the observation period, slightly less than 200,000 phones belong to private organizations, out of approximately 10 million distinct phone numbers in data.

This information allows us to distinguish between general call activity and business-related activity. It also permits to assess the size of the firms (in terms of corporate phones), and their geographic and temporal activity patterns. We further categorize the firms into industry-related "segments" based on the operator's internal categorization. The segments are construction, finance, IT and telecommunication, manufacturing and trade, security, transportation, and "other". Note that we cannot use the standard ISIC codes because the operator's internal classification is different.

A2.2 Constructing Panel Data

Our central empirical approach relies on monthly panel data on firm activity by Afghanistan districts. We count all calls and distinct active subscribers by each firm in each spatio-temporal cell, usually district-month. Based on whether the firm was active in the given cell, we also define it's binary "firm presence" in the cell. We define presence in three different ways:

1. *total activity*, count of all calls and text messages in the relevant district-month.
2. binary *presence* indicator, equal to one if the business had any cellpone activity in the given district-month.
3. *intense presence*, an indicator for district-month where the phone was used most often.

Further, we order the districts according to how many phones have intense presence there. We call the district with the largest presence the "headquarter location". The top 5 districts found in this way show a reasonably good fit with the recorded locations of headquarters and regional offices in other administrative and survey data sources.

Activity distribution shows a prominent right tail while in time, there is no major trends in activity. As expected, Kabul region dominates the the spatial picture but the other major cities are also clearly present. The median value of firm size (phones the firm possesses) is 4, while the mean is 52.26 and the maximum value is 10686.