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

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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 based on employee locations. Using these measures, our main analysis shows that violent shocks reduce local firm presence by both increasing firm exit and decreasing entry. Firms respond most strongly to violence in their "headquarters" districts. We also 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. (*JEL* D22, K42, L11, L96, O18, R32)

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 has documented 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

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(Baranyi, Beaudet, and Locher 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 validates 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) as well as 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 percent. This is driven both by an increase in firm exit and a decrease in firm entry: Firms are 5–23 percent more likely to leave a district in the month after violence and are 7–16 percent 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

[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, Morten, and Peter 2020; Macchi and Stalder 2023).

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, Deschenes, and Sanders 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 United States (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) quantify 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, Macchiavello, and Morjaria (2016) study the effect of electoral violence on labor supply in Kenya; and Amodio and Di Maio (2018) show how conflict affects firms' access to inputs in Palestine.

More broadly, our approach connects to recent work using nontraditional "big" data to measure productivity and wealth (Henderson, Storeygard, and Weil 2012; Blumenstock, Cadamuro, and On 2015; Jean et al. 2016; Aiken et al. 2022), unemployment (Toole et al. 2015), urban mobility (Hanna, Kreindler, and Olken 2017), and migration (Blumenstock 2012). Blumenstock et al. (2024) 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 into 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—which limits 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] See Collier et al. (2003) and Blattman and Miguel (2010) for overviews of research linking aggregate economic activity and insecurity.

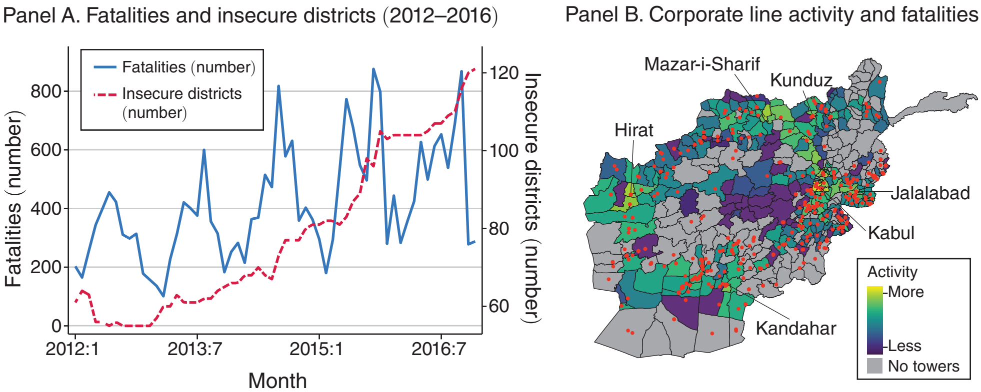

Figure 1. Rising Insecurity and Corporate Mobile Phone Activity

Notes: (Panel 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). (Panel 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.

I. Setting

While Afghanistan has experienced conflict for decades, we study a period of rising violence and economic downturn from 2013 to 2016 following relative stability and high growth. In 2009 the United States 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, US 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 1, panel A shows, the five years from 2012 to 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, Zhou, and Hallgren 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 percent of firms reported that they

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

viewed security as a primary obstacle to their businesses. In listing all challenges facing their businesses, 91 percent included security, while power cuts (86 percent), labor problems (82 percent), and infrastructure (76 percent) were the next three highest responses, all plausibly linked to issues of security.[4] Insecurity from insurgents remains the primary concern of firms: 78 percent 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 percent), small arms fire (84 percent), kidnappings (83 percent), and suicide bombers (93 percent) 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.

II. Measuring Firm Location 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.

A. *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 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 nonprofit organizations, we remain with a

[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.).

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 percent 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 Supplemental 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 IID provides further discussion on some of the nuances and limitations of these data.

B. *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 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 percent 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

[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.

Table 1—Location Validation

		% HQ match	
	Obs	Top 1 modal "primary"	Top 5 modal
Panel A. Headquarters			
AISA	110	81.82	91.82
CBR	934	72.06	82.98
Survey	406	78.08	87.93
All combined	1,119	73.28	84.45
			% Office match
	Obs	Num. of offices	Top 5 modal
Panel B. All offices			
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.

the median firm has only 4. In an average month, the average firm has any employee presence in 5 percent of the 173 districts in the estimation sample and intense presence in 1.7 percent of those districts. Supplemental Appendix Table A3 provides additional summary statistics for each firm, district-month, and firm-district-month.[6]

C. *Validation of CDR-Based Measures of Firm Location*

Figure 1, panel B 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 dataset and 110 to the AISA dataset. For each of these matched sets of firms, we compare the headquarters inferred from the CDR

[6] Supplemental Appendix B describes how the firm-district-month panel was constructed from the original CDR.
[7] With the fall of the government, AISA no longer exists, but their previous website has been archived here: https://www.loc.gov/item/lcwaN0003187/ (accessed August 15, 2025). CBR was managed by the former Ministry of Commerce and Industry. Its website was previously: https://acbr.gov.af/en (accessed May 27, 2016). In our own survey, we attempted to contact all firms in the CDR sample in spring of 2017.

Table 2—Aggregate Economic Validation with Nightlights Data

	Nightlight growth (%)		
	(1)	(2)	(3)
Panel A. Total active firms			
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	6,569	6,569	6,569
R^2	0.007	0.235	0.236
Month FE	NO	YES	YES
District FE	NO	NO	YES
Panel B. Total active subscribers			
Employee 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	6,569	6,569	6,569
R^2	0.007	0.235	0.236
Month FE	NO	YES	YES
District FE	NO	NO	YES

Notes: Observation is a district-month. Standard errors are clustered at district level. Columns include fixed effects for month of year and district as indicated. Nightlight growth (%), firms growth (%), and employee 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 employees is the count of active subscribers.

(see Section IIB) to the headquarters reported in the other traditional data source. We find that our method has a high success rate: 72 percent match to the CBR reports; 82 percent match to the AISA; and 78 percent 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, Storeygard, and Weil 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

[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, Aiken, and Blumenstock 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] Publicly available monthly composite VIIRS-DNB nighttime composite images for April 2013–December 2016 were clipped to the geography of Afghanistan districts and used to calculate the average intensity and total intensity of pixels at the district-month level. The original NOAA website hosting this dataset is no longer available since October 2019, but an archive is maintained by the Earth Observation Group at the Colorado School of Mines here: https://eogdata.mines.edu/products/vnl/monthly. Cao et al. (2012) describes the original data.

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.

D. *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 Supplemental Appendix Table A2). Second, firms 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 nonwork ("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

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

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.

III. How Do Firms Respond to Insecurity?

A. *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 IIB, 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 and firm presence:

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

where Y_{idt} is an indicator variable that equals 1 if firm i is present in district d in month t, and $\mathbf{1}\left(VS\right)_{dt-1}$ indicates whether district d experienced a Violent Shock in month $t-1$ (defined below in Section IIIB). 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. The identifying assumption 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 after presenting our main results.

[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.

B. *Measuring Violence*

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 percent 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] Supplemental 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. Supplemental 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 six months before or after. The figure plots the coefficients from a 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

[12] LaFree and Dugan (2007) provide an introduction to the Global Terrorism Database. 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 to 2016, UCDP records less than 5,000 civilian fatalities in Afghanistan, or 20 percent 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 Supplemental 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 3,050 confirmed fatalities that meet GTD's criteria. The most common types of attacks were armed assaults (53 percent) and bombings (47 percent), with less common categories including kidnappings (7 percent) and infrastructure attacks (5 percent)—percentages reflect the share of fatalities and are nonexclusive by type. Private citizens were the primary target in over half (51 percent) of fatalities, followed by police (30 percent), military (27 percent), government (11 percent), and businesses (4 percent).

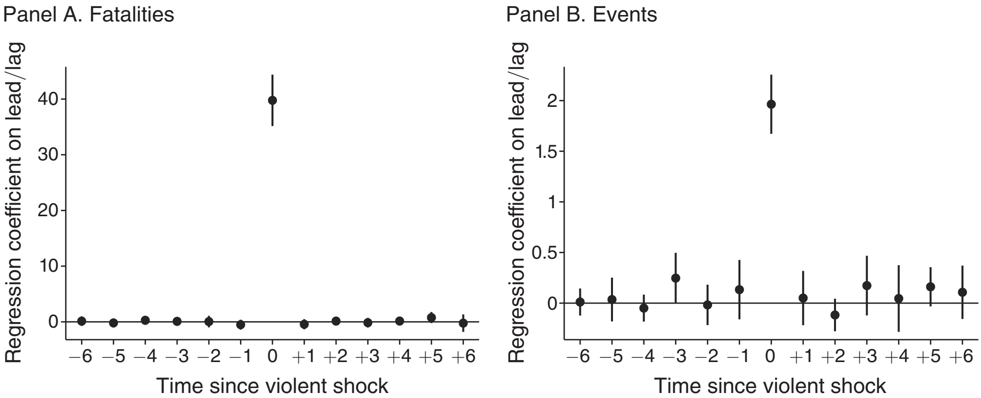

Figure 2. Violent Shocks and Surrounding Violence

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, of 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 percent confidence intervals.

anticipatory increase in violence leading up to these shocks nor is there a persistent change in levels of violence following them.

C. *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

TABLE 3—FIRM RESPONSE TO VIOLENT SHOCKS

	Firm employee ever present in district (= 100)			
	(1)	(2)	(3)	(4)
Panel A. Any presence				
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	15,809,432	15,809,432	15,809,432	15,809,432
Adjusted R^2	0.580	0.582	0.582	0.584
	Firm employee with primary location in district (= 100)			
Panel B. Intense presence				
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	15,809,432	15,809,432	15,809,432	15,809,432
Adjusted R^2	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 percent 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.

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 Supplemental Appendix A. In particular, Supplemental Appendix Table A4 shows widespread persistence of our main effects when varying our approach to coding violence. Supplemental Appendix Figure A3 shows that our results are qualitatively unchanged when using alternative thresholds of fatalities for our definition of a violent shock. Supplemental 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. Supplemental Appendix

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

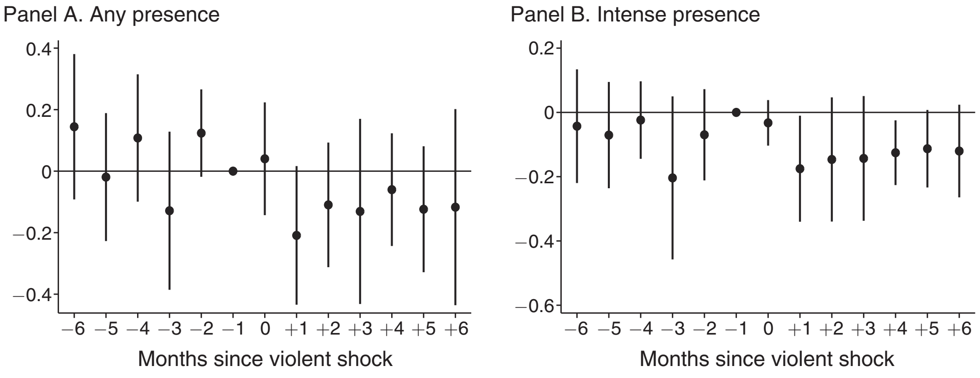

Figure 3. Event Studies of Firm Response to Violent Shocks

Notes: Event studies show the effect of violent shocks in relative time (indicated on the x-axis) on firm presence. The period $t = 0$ indicates the month in which violence occurred, $t = +1$ indicates firm presence in the month after violence, and so forth. The month before violence ($t = -1$) is the reference period; events more than six months in the future (past) are included as part of the sixth lead (lag). The outcome in panel A is "any" firm presence; in panel B it is "intense" firm presence. Dots represent coefficients, and bars indicate 95 percent confidence intervals.

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 Supplemental 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 and Anticipation.—Next, we examine the persistence of our main effects. For this, we use an event-study specification that includes six months of leads and lags of the violent shock treatment.[17] Figure 3 shows the coefficients and 95 percent confidence intervals; the corresponding coefficients and standard errors are in Supplemental Appendix Table A8, columns 1 and 2. While the estimates are imprecise, the results suggest that responses to major attacks are largest in the month following major attacks, then slowly attenuate over the next six months. Effects two to six months after violence are consistently negative, though typically not statistically significant.[18]

The event study also provides an opportunity to test for the presence of pre-trends (cf. Roth 2022). Looking at the significance of six leads across each of our two primary outcome measures, we find a single coefficient that is significant at $p < 0.1$ (the third lead of the specification using the "any" firm presence measure). This is

[17] We use the following event-study specification, where the first lead is dropped from the estimation and the sixth lead (lag) indicator also includes violent shocks in that district occurring more than six months in the future (past): $Y_{idt} = \sum_{k=-6}^{6} \beta_k \mathbf{1}(VS)_{dt-k} + \theta_{id} + \sigma_{dm} + \delta_t + g_d(t) + f(towers_{dt}) + \epsilon_{idt}$.

[18] The estimated effect of a shock in the previous month is similar in magnitude between the main regression specification (Table 3, column 4) and the event-study specification (Supplemental Appendix Table A8, columns 1 and 2). However, the event study results are slightly less precise ($p = 0.07$ in the first lag for the any presence measure in column 1). We suspect this is due to autocorrelation in firm presence in the months after the shock, so that some of the persistent reduction in presence in months 2–6 effectively gets loaded onto the single lag in the main specification.

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 0.63 (any presence) and 0.30 (intense presence).[19] Supplemental Appendix Table A8 provides an alternate specification of the event study (columns 3 and 4), which uses the period outside of the event study as the reference period (i.e., more than six months before and more than six months after the event), thus allowing us to estimate the first lead. We do not see evidence of anticipatory reduction in firm presence in the month immediately before violence.

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, 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 percent), and that firm exit increases by 6.1–8.0 percentage points (5.4–23.6 percent).

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 IIC.

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 Supplemental 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 Supplemental 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

[19] 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 0.46, 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.

Table 4—Firm Entry and Exit after Violent Shocks

	Any presence			Intense presence		
	Active (1)	Entry (2)	Exit (3)	Active (4)	Entry (5)	Exit (6)
Panel A. Base specification						
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	15,809,432	15,411,013	15,411,013	15,809,432	15,411,013	15,411,013
Adjusted R^2	0.584	0.091	0.092	0.688	0.069	0.069
Panel B. Heterogeneity						
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 × 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-primary	4.449	1.371	1.420	1.275	0.225	0.234
Mean primary	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	13,445,387	13,445,387	13,445,387	13,445,387	13,445,387	13,445,387
Adjusted R^2	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 one-month lag is needed to code the outcome variable. "Primary" indicates a firm's primary district, as defined over the first six months the firm appears in the data. Panel B has fewer observations than panel A because those six 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.

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

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 × primary		−8.542			−10.425	
		(0.428)			(1.451)	
Insecure district × non-primary		−0.011			−0.049	
		(0.080)			(0.036)	
Violent shock × insecure district			−0.046			−0.027
			(0.106)			(0.046)
Violent shock × 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	13,445,387	13,445,387	13,445,387	13,445,387	13,445,387	13,445,387
Adjusted R^2	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 six months the firm appears in the data. In columns 3 and 7, "Mean group 1" indicates the mean of the 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.

to visit in each month based on the presence of Taliban insurgents.[20] Relative to public assessments of district security by the Afghan government, US 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

[20] Data was provided to the research team by a private research firm, providing their monthly assessment of the state of security for each district across Afghanistan (Afghan Center for Socio-Economic and Opinion Research, 2017). This is the same measure used to construct Figure 1, panel A. Blair and Wright (2021) provide an in-depth discussion and application of this measure of insecurity.

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 percent 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 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.[21]

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 percent ($p = 0.08$). 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.

[21] Supplemental 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.

Table 6—Provincial and Neighbor Effects, District Trends

	Any presence		Intense presence	
	(1)	(2)	(3)	(4)
Violent shock (= 1)	−0.148	−0.065	−0.088	−0.003
	(0.054)	(0.075)	(0.040)	(0.022)
Violent shock × capital		−0.152		−0.157
		(0.097)		(0.054)
Violent shock elsewhere in province	−0.047		−0.008	
	(0.027)		(0.010)	
Violent shock elsewhere in province-capital		−0.071		−0.017
		(0.037)		(0.015)
Violent shock elsewhere in province-rural		−0.012		0.005
		(0.033)		(0.012)
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	15,809,432	15,809,432	15,809,432	15,809,432
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.

IV. 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 by 3–5 percent 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 nonstandard factors, including the risk of physical violence against staff and damage or destruction of firm assets and essential infrastructure. The existence of these nonstandard 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 percent. Aggregating this impact across the 68 violent shocks in our study, this would amount to 80–300 percent of local monthly GDP. However, this back of the envelope calculation of the immediate response to shocks in the targeted district is likely a considerable underestimate. In particular this one-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).[22]

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.[23] 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 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 Supplemental Appendix Table A1. These firms made clear that they perceive substantial physical risk to their employees, with 22.6 percent of respondents reporting that employees had been threatened by insurgents (anti-government groups), 8.2 percent reporting employee injuries from insurgents, and 5.2 percent reporting that employees had actually been killed. Correspondingly, 24.5 percent indicated insurgents had threatened or destroyed firm assets, and 58 percent indicated insurgents had destroyed public infrastructure crucial to business operations. Firms confirmed a range of economic responses to violence, with 45.7 percent investing in private security and 33.3 percent spending money on protection payments. Of firms, 35.7 percent said that insecurity reduced demand for their products or services, and 28.3 percent 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 percent said

[22] 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 percent 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 and 33 percent of the local, main effects.

[23] 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 IID for further discussion of these and other limitations.

that violence had caused them to move staff away from affected cities or districts, and 28.6 percent said that they decreased deliveries, while 39.8 percent changed transportation routes; 18.8 percent were forced to change suppliers, and 15.1 percent changed buyers; 30.8 percent said that they had been forced to temporarily halt local operations due to insecurity, while 7.5 percent 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.

V. 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 percent 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 US-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.

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