The Fortification Dilemma: Border Control and Rebel Violence*

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Abstract

Where cross-border sanctuaries enable rebels to marshal external support, classical theories of counterinsurgency extol the strategic value of border fortification. By sealing borders, counterinsurgents can erode transnational militants' resources, degrading the quality of rebellion. Extending resource-centric theories of conflict, I posit a fortification dilemma inherent in this strategy. Externally-supplied rebels can afford conventional attacks and civilian victimization. When border fortifications interdict their foreign logistics, insurgents compensate by cultivating greater local support. In turn, rebels prefer more irregular attacks and cooperative relations with civilians. Hence, counterinsurgent border fortification trades-off reduced rebel capabilities for greater competition over local hearts-and-minds. I test this theory using declassified microdata on border fortification and violence in Iraq. Results highlight the central link between border control and cross-border militancy, and show how governments can contest the transnational dimensions of civil wars, like external rebel sponsorship.

Word Count: 9,988

The data and materials required to verify the computational reproducibility of the results, procedures and analyses in this article are available on the *American Journal of Political Science* Dataverse within the Harvard Dataverse Network, at: https://doi.org/10.7910/DVN/I5SVQG.

1 Introduction

In June 2018, six months after declaring the defeat of the Islamic State (IS), Iraqi troops began fortifying the border with Syria, installing fences to inhibit cross-border infiltration (Sulaivany, 2018). Iraq's aim was to deny IS militants the ability to re-supply from bases in Syria, and thereby resurge. This strategy, involving the use of barriers to interdict transnational militancy, is known as counterinsurgent border fortification. In the past two decades, similar efforts aimed at disrupting cross-border rebellion have unfolded in Jordan, Kenya, Myanmar, and Pakistan, among others. The proliferation of counterinsurgent fortifications is part of a broader, global trend toward border hardening (Carter and Poast, 2017; Simmons and Kenwick, 2022; Blair, 2022).

The rationale behind counterinsurgent border fortification is simple: rebels need resources to survive and fight, and often secure them from sanctuaries and supporters in neighboring countries. By fortifying borders, counterinsurgents can deny militants the ability to move fighters and materiel from external sanctuaries—or at least raise the costs of doing so—thereby degrading rebels' capabilities and heightening the prospects of rebel defeat. This logic manifests in classical counterinsurgency theories (Galula, 2006; Leites and Wolf, 1970) and contemporary military doctrine (United States Army, 2006).

Unfortunately, we lack clear evidence that border fortification reduces violence. Though some scholars are sanguine (Staniland, 2005; Avdan and Gelpi, 2017), others argue barriers are symbolic (Andreas, 2000), with only modest impacts on security. Alternatively, fortification may backfire. By dislocating communities, fortifications can spur resentment and humiliation (Gade, 2020). In tandem with the disruption of cross-border markets (Getmansky, Grossman and Wright, 2019; Kim and Tajima, 2022), these impacts may exacerbate criminal and political violence. Mixed evidence warrants closer attention to bridge theoretical divides, unpack mechanisms, and address inferential challenges.

To this end, this article offers the first plausibly causal evidence on how border fortification shapes rebel violence. Extending political economy theories of conflict (Bueno de Mesquita, 2013; Wright, 2020; Qiu, 2022), I argue that border control generates discrete trade-offs for combatants. By raising the price of foreign support, fortification reduces transnational rebels' resources (a negative endowment shock).[1] Well-supplied rebels prefer conventional operations, but as fortifica-

[1] Counterinsurgents need not block all foreign support so long as fortification raises the costs to rebels of accessing

tion interdicts their foreign logistics, rebels substitute conventional attacks for less-costly irregular operations. Simultaneously, rebels move to compensate for fortification-induced resource losses. Militants cut off from external bases seek to recoup resources by cultivating greater support from civilians in the counterinsurgent's populace. These efforts manifest in the form of reduced civilian victimization and increased service provision. This is the *fortification dilemma*: by reducing rebels' access to foreign resources, border fortification trades-off reduced rebel capacity for greater competition between rebel and counterinsurgent forces over local civilian loyalties.

This theory emphasizes how border fortification affects the *quality* of rebellion, including the tactical portfolios insurgents employ and the nature of their relations with civilians. By moving beyond macrolevel characterizations of conflict, like the *onset* (Linebarger and Braithwaite, 2020) or *intensity* (Avdan and Gelpi, 2017; Nanes and Bachus, 2021) of violence, my approach offers new insights into how border fortification shapes microlevel conflict processes. The theory also offers a novel explanation for mixed findings in the empirical record. By altering the quantity and sources of rebel support, fortification causes a composition shift in violence. Only a disaggregated analysis, which distinguishes tactics and anti-government versus one-sided attacks, can detect these shifts. By impeding rebel access to resources from sanctuaries abroad, fortification reduces complex, conventional attacks while incentivizing irregular, harassing operations. Likewise, by increasing rebel reliance on local communities, fortification fosters restraint and reduced civilian victimization.

I test this theory in the context of US-led border fortification efforts during Operation Iraqi Freedom. I draw on declassified microdata from the Iraq Reconstruction Management System (IRMS) maintained by the US Army Corps of Engineers (Berman, Shapiro and Felter, 2011). These data document 73,600 reconstruction projects in Iraq, including 287 border forts. Because the data track the universe of US reconstruction spending, they offer a principled way to study the evolution of border enforcement. For identification, I leverage spatiotemporal variation in the implementation of fortification in a difference-in-differences setting. Plausibly exogenous bureaucratic delays and idiosyncratic reallocation of reconstruction money meant funds devoted to fortification were divorced from conflict trends across district-months. Rich data on construction timelines, violence, and concurrent policy changes allow me to address multiple threats to inference.

In line with the theory, I find that border fortification caused insurgents to substitute con-

transnational resources, for example by pushing militants to take riskier cross-border routes or raising the fees charged by smugglers.

ventional, direct fire operations for irregular, indirect fire attacks. This shift is consistent with rebel adoption of less effective tactics under a negative endowment shock. Yet, fortification also prompted reduced civilian victimization, implying rebel efforts to recoup resource losses through community-based mobilization. This latter effect was heightened in areas where rebel forces were co-ethnic with civilians, and hence, where their efforts to cultivate popular support were more credible. Several extensions provide further support for implications of the theory. Captured financial records documenting the expenditures of al-Qaeda in Iraq (AQI) reveal that fortification caused an increase in militant spending, mostly on smugglers' fees. This spending helped insurgents build support in borderland communities where access to informal, cross-border markets was disrupted. Data on insurgent ratlines reveal that effects attenuated where militants retained smuggling routes, which subverted the interdiction efficacy of fortification.

Overall, this paper makes several important contributions. By analyzing how counterinsurgents attempt to degrade transnational rebellion, I problematize an assumption in much existing work about the fixed character of rebel access to foreign support. Prominent models (Leites and Wolf, 1970; Weinstein, 2007) treat external resources as an exogenous source of rebel capabilities, and trace this support to static factors like interstate rivalry and ethnic geography (Salehyan, Gleditsch and Cunningham, 2011; Lee, 2020).[2] These accounts do not permit inference about how shifts in transnational resources affect violence within conflicts over time. While some recent work recognizes that rebel access to foreign sanctuaries may vary, this work focuses on how *gaining access* to external havens affects violence (Stewart and Liou, 2017).[3] Owing to border fortification, it is more common that rebels *lose access* to foreign support. Studying counterinsurgents' efforts to interdict rebels' cross-border logistics highlights the under-appreciated fact that the transnational dimensions of civil wars are the subject of contestation in themselves.

Further, while existing research considers the pathologies of transnational insurgency, including heightened risks of interstate conflict (Salehyan, 2009), this paper addresses antecedent questions about how governments can counter transnational insurgencies. Studying how states fight transnational rebels lends nuance to theoretical models showing why it is difficult to deter external support in civil war (Schultz, 2010). Border fortification represents an important means

[2]But see Hazen (2013).

[3]Zhukov (2017) studies the interdiction of external support, focusing on resource losses and government violence.

to counter militancy unilaterally, given inherent challenges in negotiating or coercing states to terminate rebel sponsorship.

This paper also provides new empirical evidence for political economy models of conflict, which emphasize how rebels' resources affect their technologies of rebellion (Kalyvas and Balcells, 2010; Bueno de Mesquita, 2013; Qiu, 2022). Back-end conflict processes, including logistics (Parkinson, 2013; Zhukov, 2017) and tactics (Wright, 2020; Biddle, 2021), remain a crucial, understudied field. My analyses contribute on both dimensions, and highlight how variation in insurgents' supply networks affect their repertoires of violence. One notable result, that rebels reduce civilian victimization following fortification, suggests an important modification to theoretical accounts predicting a positive association between resource losses and one-sided violence (Hultman, 2007; Wood, 2014). The fact that interdiction can spur greater rebel forbearance in relations with civilians reinforces accounts that emphasize how combatants anticipate civilian reactions and calibrate behavior accordingly (Polo and González, 2020).

Finally, as borders harden globally, a growing literature examines the political economy of border security. To date, most work has focused on the *macrolevel* determinants of enforcement (Carter and Poast, 2017; Linebarger and Braithwaite, 2022). This paper bolsters scholarship on the *microlevel* consequences of fortification, and especially on the effects of border hardening on conflict (Avdan and Gelpi, 2017). The evidence here suggests fortification can reduce rebel capabilities. Still, the costs required to control borders might be better spent on development and governance reforms (Berman, Shapiro and Felter, 2011). Unless states also invest in winning civilian loyalties, the reduction in rebel capacity stemming from border fortification may be compensated by a concomitant increase in rebels' local support. These insights extend strategic-interaction models of (counter-)insurgency (Bueno de Mesquita, 2005; Braithwaite and Johnson, 2012, 2015), which emphasize reciprocity of government enforcement and insurgent adaptation.

2 Transnational Resources and Rebellion

Rebel resilience is predicated on a host of factors, including social networks and internal political structures (Wood, 2003; Parkinson, 2013). But resources are a paramount constraint because it is costly to produce violence and provide services. Both of these outputs require recruits and materiel (Weinstein, 2007; Dube and Vargas, 2013). Increasing the production of violence and governance bolsters territorial control, endogenously increasing resources (Galula, 2006). Hence, combatants have incentives to seek larger resource endowments.

To secure resources, rebels often turn externally, seeking sanctuaries, cash, recruits, and weapons from co-ethnics, diasporas, and state sponsors (Byman, 2005; Salehyan, Gleditsch and Cunningham, 2011). 82% of insurgencies receive some form of outside support (Jones, 2017, 136). This external dimension of rebellion has become more important over time (Hazen, 2013), as globalization enhances militants' ability to operate transnationally (Hastings, 2010).

Insurgents' desire for resources induces them to seek control of territory across borderlands (Idler, 2019). Safe havens allow rebels to melt from the path of counterinsurgency, regroup, and dictate the terms-of-engagement (Byman, 2005; Salehyan, 2009). Recruitment, procurement, and training can all be organized with relative ease from border sanctuaries (Galula, 2006). Governing cross-border routes also provides revenue-generating opportunities. For cash-strapped rebels, these resources can help sustain operations even absent sponsorship. The rise of IS, for example, owed in part to the lucrative tax regime the group imposed at the border (Revkin, 2020). Beyond rebels' direct profits, siphoned taxes also represent lost income for state coffers, weakening government fighting capacity.

Border fortification is an appealing strategy for counterinsurgents precisely because resources are integral to rebellion. This strategy aims to interdict rebels' transnational logistical networks, reducing their material support. Counterinsurgent operations that remove fighters and arms from the battlefield degrade rebel fighting capacity (Braithwaite and Johnson, 2012; Weidmann and Salehyan, 2013). If fortification raises the cost to rebels of obtaining external support, it should reduce their overall resource base, and thereby weaken the rebellion. Crucially, to inflict resource losses, all fortification must do is reduce the quantity of foreign support rebels can obtain at a given cost. For instance, fortification may force rebels to take longer and more dangerous smuggling routes (Chambers et al., 2021), or pay higher smuggling fees and bribes. Efforts that channel cross-border traffic through government-controlled ports-of-entry can deprive rebels extortion opportunities, while increasing government rents. Fortification may also impose non-monetary costs, for instance sapping insurgent morale.

Resource-centric models imply that successful counterinsurgent border fortification will affect the *quantity* of violence rebels can produce (Leites and Wolf, 1970). But resources do not only affect how many attacks rebels conduct. Because different technologies of rebellion are priced differently (Kalyvas and Balcells, 2010), fortification may also affect the *quality* of rebel violence. The quality of violence hinges on tactics—the ways combatants organize and deploy their forces in battle. Tactical changes made by rebels in response to fortification create a salient trade-off for counterinsurgents.

3 The Fortification Dilemma

The tactical spectrum ranges from conventional to irregular violence (Biddle, 2021). Conventional tactics entail complex, coordinated, high-risk attacks on government forces.[4] Well-resourced rebels with access to external support—whether sanctuary, fighters, or materiel—can afford to produce more conventional violence (Bueno de Mesquita, 2013). Ceteris paribus, rebels prefer conventional operations, despite the greater risks involved, because these tactics are more effective for seizing territory and dealing governments decisive defeats (Biddle, 2021; Qiu, 2022). Controlling territory and capturing arms yield further opportunities for rent extraction, so conventional tactics endogenously beget conventional tactics.

Irregular tactics are predominantly used by resource-constrained rebels seeking to avoid a forceful state response (Carter, 2016). These are cheaper to employ because they typically entail lower risk to and coordination among perpetrating militants (Biddle, 2021). Irregular attacks can also be executed by small groups or even individuals. These operations allow rebels to harass government forces at minimal cost. A common irregular tactic in Iraq was the use of mortar and rocket fire against US bases. Called "shoot-and-scoot" operations, these attacks saw insurgents launch long-range projectiles at counterinsurgent sites, and then flee the launch area before suppressing fire was returned.

Endowment shocks enhancing rebel capacity increase conventional attacks, while those reducing rebel capacity increase irregular attacks (Wright, 2020). Border fortifications that interdict rebels' external support negatively shock rebel resources. Consequently, fortification should prompt rebels to substitute conventional for irregular tactics. Two factors are particularly relevant. First, fortification reduces rebel access to fighters and supplies from abroad, precisely the resources

[4]This conceptualization follows Biddle (2021), and focuses on combat tactics (i.e., the methods of anti-government violence), extending a more general view based on target hardness (Carter, 2016).

needed to perpetrate conventional violence.[5] Second, fortification attenuates access to safe havens, increasing rebels' need to avoid costly suppression.

> H_1: **Border fortification causes insurgents to substitute conventional for irregular attacks.**

From a counterinsurgent perspective, rebel substitution from conventional into irregular violence is a desirable consequence, since it implies that fortification leads rebels to adopt less effective combat methods.

In addition to tactics, resources also influence rebel behavior vis-á-vis civilians. Different endowments alter the extent to which rebels rely on civilians for extraction. External resources reduce rebel dependence on the local populace (Zhukov, 2017; Stewart and Liou, 2017), sapping incentives for restraint and governance (Stanton, 2016). Recruitment patterns compound this dynamic. Resource-rich rebels attract opportunists, who are more interested in loot than civilian protection (Weinstein, 2007), and struggle to embed themselves in local communities (Moore, 2019). Civilian victimization is correspondingly responsive to shifts in rebels' assets.

Shifting resources also matter apart from the content of rebels' endowments. Losses trigger predation. Following setbacks, civilian victimization is a cheap means to deter defection and enforce compliance (Wood, 2014). Violence also underscores the government's inability to protect the populace (Wood, 2010), and can help coerce concessions (Hultman, 2007). However, predation is counterproductive in the long-term (Kalyvas, 2006). Because civilians have agency, strategies of victimization to meet resource needs create incentives for civilians to collaborate with the government (Condra and Shapiro, 2012; Braithwaite and Johnson, 2012), exposing rebels to suppression.

These dynamics imply competing expectations about how fortification will affect insurgent-civilian relations. On one hand, if fortification interdicts rebels' *transnational* logistics, it should increase reliance on *local* civilians, incentivizing restraint. On the other hand, resource losses resulting from fortification threaten rebel capacity, incentivizing predation. I argue the former effect—rebel forbearance—predominates for three reasons.

First, because predatory strategies are counterproductive in the long-run, what matters is how losses affect rebels' time horizons. If they are not so hard-pressed by fortification that their

[5] I am theoretically agnostic about whether fortification has a cost hierarchy, reducing in-flows of materiel or personnel more. This is likely to vary across conflicts depending on the nature of fortification and sponsorship dynamics, and represents an important avenue for future research.

immediate survival is at risk, rebels should forgo victimization in favor of contractual bargaining with civilians, since the latter is optimal for resilience absent external support (Arjona, 2016). How resource losses affect time horizons is a function of the magnitude of the loss. Unlike major battlefield defeats, fortification is a more modest setback. No border controls are impermeable, and rebels will inevitably retain some access to foreign support through smuggling. Further, while imperfect, fortifications are durable. Fortifying rugged borderlands entails significant costs, making it a long-term investment.[6] The imperfect but durable nature of the setback imposed by fortification increases rebels' incentives to adapt. This means compensating for lost resources by cultivating new bases of support among civilians.

Second, while interdiction of their transnational networks increases militants' *need* to cultivate local support, it also shifts their recruitment patterns in a way that bolsters their *capacity* to do so. Without ample resources, groups attract fewer income-motivated opportunists (Weinstein, 2007) and more intrinsically-motivated locals, who are better equipped to cultivate civilian ties (Moore, 2019). Fortification also directly reduces inflows of foreign fighters, forcing increased reliance on local recruits (Tyson, 2006).[7] The impacts of fortification on civilian livelihoods compound these effects. Borderland civilians depend on cross-border markets (Idler, 2019). Fortifications impede market access, reducing trade and wages (Kim and Tajima, 2022). As US military advisors feared in Iraq, economic disruption resulting from fortification could empower "a [militant] financier who comes through and builds a cell in an impoverished border village" (Tyson, 2006).

Third, perpetrating civilian victimization requires fighters and materiel, so victimization reduces rebels' ability to produce anti-government violence. Because anti-government attacks are more effective at demonstrating rebel capacity and building civilian confidence in militant challengers (Wood, 2010), groups facing resource deficits should privilege these operations. In sum, border fortification should prompt rebels to reduce civilian victimization in order to cultivate civilian support, improve local extractive capacity, and recoup lost resources.[8]

[6]Figure D-3 considers whether terrain ruggedness conditions effects.

[7]Militants recognized that enforcement reduced recruitment and morale by "funneling" (Chambers et al., 2021) inflows to harder crossings. Worsening crossing conditions contributed to "struggling and suffering" among fighters (Harmony Program: NMEC-2007-612449). Lower morale is an important non-monetary cost of fortification.

[8]Insurgents may also build civilian support through governance (Arjona, 2016; Stanton, 2016). I bracket this expectation because I lack data on rebel governance in Iraq.

H_2: **Border fortification causes insurgents to reduce civilian victimization.**

From a counterinsurgent perspective, rebel efforts to cultivate local support are a troubling consequence of border fortification, since they imply that fortification increases competition over civilian hearts-and-minds. The fact that fortification disrupts civilian livelihoods in impacted communities also means the strategy may bolster insurgent recruitment in the long-run.

Broader societal cleavages impact insurgent-civilian relations apart from insurgents' resources (Wood, 2003). In particular, many civil wars have an identity-based dimension. In divided societies like Iraq, where society is cleaved along sectarian lines, rebel groups typically draw support from one primary community. In these settings, victimization dynamics are complicated by combatant identities (Lyall, Blair and Imai, 2013). Rebels exercise restraint toward their core constituencies while targeting out-groups (Fjelde and Hultman, 2014; Stanton, 2016).

Considerations about intergroup dynamics are especially pressing after resource losses. As Polo and González (2020, 2032) note, "[w]hen rebels expect a backlash they will not resort to terrorism, despite having suffered major military losses." This dynamic is most likely where rebels share identity ties with the civilian populace. In these areas, rebels' constituents will sanction predation, and their outreach efforts will be more credible (Moore, 2019). These factors reduce the costliness of community-based strategies predicated on forbearance. In contrast, rebels hold a higher threshold for cultivating support in out-group areas, making restraint less efficient. If out-group antagonism is high, rebels' prospective civilian supporters may even favor out-group victimization (Polo and González, 2020). This discussion suggests a conditional effect of fortification on insurgent-civilian relations. Insurgents' efforts to build support following interdiction should manifest most acutely in areas populated by prospective supporters.

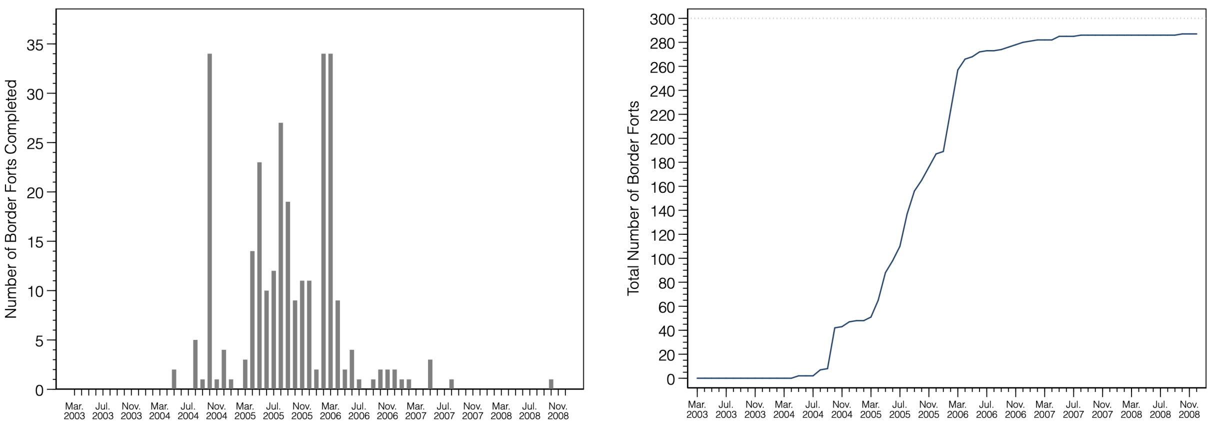
H_3: **Border fortification causes insurgents to reduce victimization of in-group civilians.**

In tandem, these arguments highlight the *fortification dilemma*. Counterinsurgent border control reduces rebel capabilities, inducing shifts from conventional to irregular combat. However, efforts counterinsurgents take to interdict rebels' transnational resources perversely incentivize them to invest in building civilian support. Hence, border fortification trades-off reduced rebel capabilities for greater competition over local hearts-and-minds. How governments manage this trade-off hinges on their abilities to capitalize on reduced rebel capacity on the battlefield, and to contest insurgent overtures to civilians in borderland communities.

4 Border Fortification in Iraq

To test my theory, I study border fortification during the Iraq War. When the US invaded in 2003, the primary focus was on Baghdad. However, as the insurgency evolved, the US quickly moved to reduce the flow of fighters, arms, and illicit goods across Iraq's historically-porous borders. Saddam Hussein's regime had maintained outposts along Iraq's frontiers and paid tribal militias to patrol (Demarest and Grau, 2005). However, the pre-invasion Iraqi border security apparatus was dismantled under de-Baathification.

Figure 1: Border Fortification over Time



Note: The number of forts completed each month (top) and the cumulative number of forts built (bottom).

Following de-Baathification, Iraq's borders went unsecured, and as the insurgency matured, many militant groups leveraged cross-border lines-of-communication, drawing on contacts in established smuggling networks, overt support from Iran, and tacit support from other neighboring states like Syria. In response to the transnationalization of the insurgency, the US-led Coalition fortified the border. On August 24, 2003, the Iraqi Directorate of Border Enforcement (DBE) was created, and between May 2004 and December 2009, US forces funded and built 287 border forts to deny insurgents' external support.[9] Figure 1 depicts the construction timeline and the total num-

[9]US-led fortification focused on interdicting insurgent resources (Skirlo, 2007). Yet, barriers may also bolster regime support or shape population movements (Linebarger and Braithwaite, 2022). Walls in Baghdad and Fallujah aimed

ber of forts built along Iraqi borders. Approximately 90% of all forts were built between May 2004 and March 2006, when the sectarian insurgency peaked.

Figure 2 maps variation in the implementation of US-led border fortification. Geographically, efforts were widespread, occurring in all 11 governorates contiguous to Iraq's borders, and 25 of 29 Iraqi border districts.[10] Fortification was predominately concentrated in four districts: Al-Rutba, bordering Syria, Jordan, and Saudi Arabia (37 forts); Khanaqin and Sulaymaniya, near Iran (21 and 18 forts); and Sinjar, bordering Syria (18 forts). On average, forts in border districts were spaced every 24 kilometers, with patrols, sensors, and aerial surveillance employed in monitoring. Forts took an average of 285 days to construct, with a median of 262 days. These projects began 9 days earlier and ended 1 day later than forecasted on average.

Between March 2003 and December 2009, US forces also constructed 52 non-fort border security facilities, including academies for training DBE troops, and wells and roads for DBE use. In total, border projects in Iraq cost $237,820,943, not including sums paid to train DBE guards. Adding estimated training costs, the total cost of American border initiatives in Iraq exceeds $1 billion. Still, individual forts were a relatively modest investment, costing just $571,969 to construct on average.

5 Research Design

Iraq is an ideal setting for identifying microlevel effects of border fortification on insurgent violence.[11] First, most rebel groups in Iraq were organized along lines matching Iraq's district borders, and managed finances locally (Bahney et al., 2010). These features make it is possible to identify how fortification affected insurgent tactics in discrete areas. Second, variation across Iraq's neighbors in the extent of support to insurgents presents a unique opportunity to compare the efficacy of border control when insurgents enjoy varying degrees of sponsorship (Tables D-2 – D-4).[12] Moreover, the porous nature of Iraq's borders meant all militant groups relied to some degree on foreign resources.

at controlling internal displacement. Iraqi leaders also deployed infrastructural investment to reward political allies (Demarest and Grau, 2005).

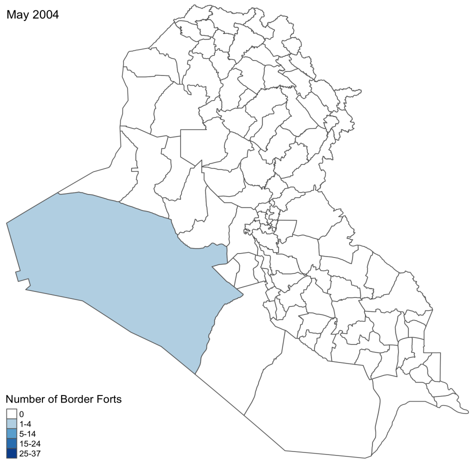
[10]The four never-fortified border districts were Amedi, Mergasur, Soran, and Zakho in Kurdistan.

[11]This design is optimized for identifying local, within-district shifts in response to fortification. Whether these effects translate into broader challenges for impacted groups is an avenue for future work.

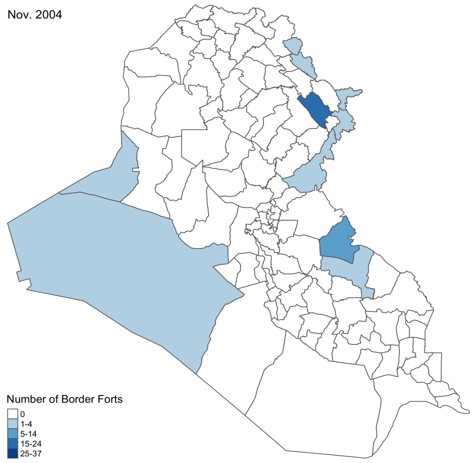
[12]Iraq's neighbors also varied in the extent of their own border enforcement—Kuwait had walled its border from 1991.

Figure 2: District-Level Border Fortification, May 2004 - November 2006

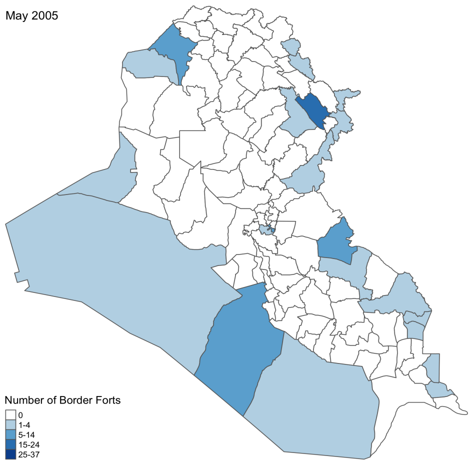
(a) Forts in May 2004



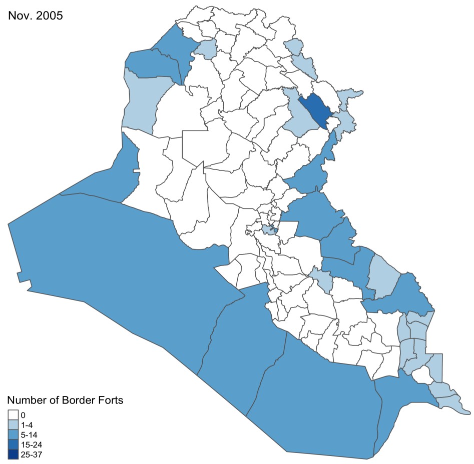
(b) Forts in November 2004



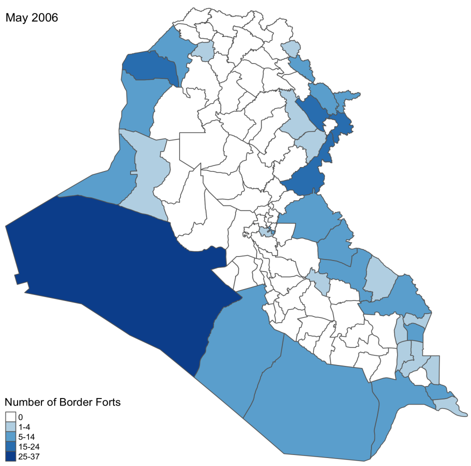
(c) Forts in May 2005



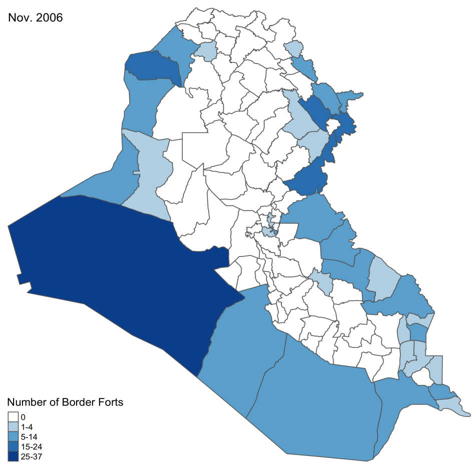
(d) Forts in November 2005



(e) Forts in May 2006



(f) Forts in November 2006



Note: Darker shades indicate more forts.

Across Iraq, insurgents tended to move materiel and fighters through the same ports-of-entry (Tyson, 2006).[13] To the east, Iran supported Shi'a militias, providing weapons and training, and engaging in active subversion of border enforcement through bribery (Felter and Fishman, 2008). Occasionally, Iranian troops maneuvered directly against border security operations. On Iraq's western border, Syria, Jordan, and Saudi Arabia were tacit conduits for insurgent support, used by fighters transiting into Iraq. Additionally, Syrian intelligence bribed border guards and facilitated arms transfers to AQI. Tribal smuggling in western Iraq was also integrated with militant logistical networks through ex-Baathist contraband networks (Malkasian, 2017). Along Iraq's northern border, Turkey cooperated with US-led efforts, but allowed some smuggling. In the south, Kuwait maintained a comprehensive border regime, denying transnational support.

5.1 Data

Border Fortification I leverage project-level data on border fortification from the Iraq Reconstruction Management System (IRMS) (Berman, Shapiro and Felter, 2011). This data represent a complete record of US reconstruction projects during Operation Iraqi Freedom. Specifically, the IRMS data describe the construction timelines, costs, and project details for 73,600 US-led aid projects.

This unique data allow me to chart the construction and completion of border fortifications in Iraq at the district-month level from 2003–2009. From the project data I construct the independent variable, border fortification, which takes a value of 1 in all district-months with a completed border fort, and 0 otherwise.[14] Because treatment never reverts, this approach is equivalent to an intent-to-treat design, mitigating concerns about endogeneity of the intensity of fortification to security conditions.[15]

Violence To assess the effect of border control on insurgent tactics, I use geocoded event data on the incidence of violence. Measures of insurgent-initiated attacks are drawn from the MNF-I SIGACT III database (Condra and Shapiro, 2012). These data are collated from reports filed by Coalition and Iraqi forces, and provide a rich set of information about the location, date, and type

[13]This makes it challenging to test cost hierarchy, since fortifying known crossings impacted resource and fighter flows similarly (CJSOTF–AP, 2007).

[14]This is a bundled treatment, which includes the presence of a border post and guards, plus sensors and surveillance devices (Skirlo, 2007).

[15]Results are similar for the intensive margin of fortification (Table D-5).

of insurgent violence. An advantage of using SIGACTs data is that they approximate the universe of anti-government conflict.[16]

To capture conventional tactics, I study direct fire attacks, where rebels engaged counterinsurgent forces within the line-of-sight. Most direct fire incidents are close-range firefights, which entail high levels of coordination and risk. To measure irregular violence, I study indirect fire attacks. Indirect fire incidents are those in which rebels engaged counterinsurgents beyond the line-of-sight (e.g., mortars, rockets). These are a good proxy for irregular tactics because they require less coordination and physical risk than direct engagements against Coalition forces (Berman, Felter and Shapiro, 2018, 202). Combining these measures gives the primary dependent variable, irregular share, which represents the proportion of projectile-fire SIGACTs that are indirect fires. This variable takes a value of 0 in all months with no projectile-fire SIGACTs, and otherwise equals $\frac{\text{Indirect Fires}}{\text{Indirect Fires} + \text{Direct Fires}}$.[17]

Civilian victimization outcomes are sourced from Iraq Body Count and the World Incidents Tracking System (Condra and Shapiro, 2012). The former compiles records of lethal incidents from local media and hospital reports provided by the Iraqi Ministry of Health. The latter is produced by the National Counterterrorism Center, and documents politically-motivated violence against civilians. A range of supplementary tests build confidence in the quality of these sources (Figure A-1, Table A-1).

Sectarianism In Iraq, militants' constituencies were defined by sectarianism (CJSOTF–AP, 2007). Sunni groups like AQI operated in Sunni regions of western and northern Iraq, while Shi'a militias like Jaish al-Mahdi (JAM) dominated southern and eastern Iraq. These competing groups clashed in mixed regions. Following Berman, Shapiro and Felter (2011), I use governorate-level voter returns from the 2005 parliamentary election to measure sectarianism. If a Shi'a, Sunni, or Kurdish party secured at least 66% of the voteshare in a district, it is defined as homogeneous and controlled by the respective sect; otherwise, the district is coded as mixed. Consistent results emerge if sectarianism is defined by population (Figure C-2).

This strategy operationalizes insurgents' intergroup ties on the basis of sectarian geography.

[16] I study SIGACTs against Iraqi/Coalition targets since these were comprehensively tracked.

[17] Results are substantively similar taking $\frac{\text{Indirect Fires}}{\text{Indirect Fires} + \text{Direct Fires} + \text{IEDs}}$, which captures the share of all insurgent-initiated SIGACTs that are indirect fires. Like direct fires, IEDs require more planning and coordination, and are more susceptible to civilian informing.

Because the conflict records do not attribute attacks to a specific group or identify victims' sect, this is a next-best approach. I assume that victimization in homogeneous districts targets in-group civilians, while violence in mixed districts targets out-groups. In the Iraqi context, this approach is reasonable because sectarian identity is not phenotypical. Instead, civilians and militants relied on geography as an identity marker (Haddad, 2014; Malkasian, 2017). Militants themselves bemoaned the challenge of operating in out-group strongholds. For instance, AQI leaders lamented the "[d]ifficulty of the muhajeer [Sunni fighters] to stay inside the land of Rafidayn [Shi'a], especially within the residential areas... ."[18] My analyses also include a range of covariaties, which vary across specifications but include measures like population, petroleum production, unemployment, and aid spending.[19] Table A-2 presents descriptive statistics.

6 Estimation Strategy

My empirical strategy leverages variation in border fortification over district-months, comparing fortified and non-fortified districts in border governorates. This approach requires that in the absence of fortification, fortified (treated) districts would experience the same changes in violence as non-fortified districts in border governorates (control). I present evidence of parallel trends below; however, identification is bolstered by plausibly exogenous, monthly variation in the implementation of fortification owing to bureaucratic wrangling. Border enforcement was funded in the context of the broader reconstruction. Within this effort, project funding was subject to numerous, idiosyncratic bureaucratic hurdles, rendering the timing of project completion divorced from violence trends across district-months (Sexton, 2016).

Border control efforts were first funded under the supplemental appropriation to the Iraq Reconstruction and Relief Fund (IRRF2) in November 2003. The slow initial roll-out of fortification from the time of the first appropriation to the time the first fort was completed in May 2004 is attributable to major wrangling between the CPA and the Office of Management and Budget (OMB) over the spending strategy. As Pentagon Comptroller Dov Zakheim noted, "OMB became kind of a black hole, from which funds would emerge on what appeared to be a *whimsical basis*..." (SIGIR,

[18]Harmony Program: NMEC-2007-612449. In the same document, AQI leadership implored fighters to forgo in-group victimization.

[19]Controlling for petroleum helps address concerns about how other lootable resources shape insurgent violence.

2009, 126).

After June 2004, the Defense Department took responsibility for security projects like fortification. Thereafter, the spending process was accelerated drastically, with contracts awarded in 90 days that would normally take 14-18 months to approve (SIGIR, 2009, 133). The drastic change in spending strategies fueled further bureaucratic variation in project implementation. Three reprogrammings between 2004 and June 2005, which saw previously allocated funds re-allocated on the basis of political priorities, shifted spending further. For instance, money was surged into governance activities before the 2005 election. Changes in the priority border security received during these reprogrammings created additional variation.

Leveraging these features, I estimate a least-squares, difference-in-differences model:

$$Y_{j,t+1} = \alpha_j + \beta_t + \delta(\mathrm{BorderFort}_{j,t}) + \gamma_{X_{j,t}} + \epsilon_{j,t}$$

where $Y_{j,t+1}$ are conflict-related outcomes of interest including the share of irregular insurgent-initiated attacks, and insurgent civilian victimization in district j in month $t+1$. α_j are district fixed effects; β_t are year-specific month fixed effects; and $X_{j,t}$ is a vector of covariates. $\mathrm{BorderFort}_{j,t}$ is a binary variable that equals 1 if district j has a completed border fort in month t. The coefficient δ recovers the effect of border fortification on insurgent violence. Main analyses compare fortified and non-fortified districts in border governorates, but results are substantively similar if I include all districts in Iraq. $\epsilon_{j,t}$ are heteroskedasticity-robust, district-clustered standard errors.

Several tests support the proposition that border fortification was unrelated to pre-existing conflict trends. In Table B-1 I show that violence does not predict differences in actual versus forecasted project start or completion dates, suggesting conflict did not have a distinguishable impact on construction. If violence caused frequent construction delays, I would anticipate projects taking longer than forecasted. In Table B-2 I also show that violence trends do not predict treatment, and in Table B-3 a temporal placebo check gives no evidence that fortification predicts past violence. In Figure B-1 I plot adjusted mean differences in pre-treatment outcomes between treated and control districts. There are no significant differences in pre-treatment means of the focal dependent variables.

6.1 Identifying Assumptions

The validity of this strategy hinges on two assumptions. First, I assume parallel trends in violence. Following the method introduced in Sun and Abraham (2021), I provide graphical evidence of parallel pre-trends in Figure 3. I specifically plot treatment leads from an event study estimation, excluding two pre-treatment periods. Given my expectation that insurgent civilian victimization also varies by the sectarian composition of a district, I plot pre-trends for these outcomes across homogeneous and mixed sectarian regions. Figure B-2 introduces comparable event studies with dynamic post-intervention effects.

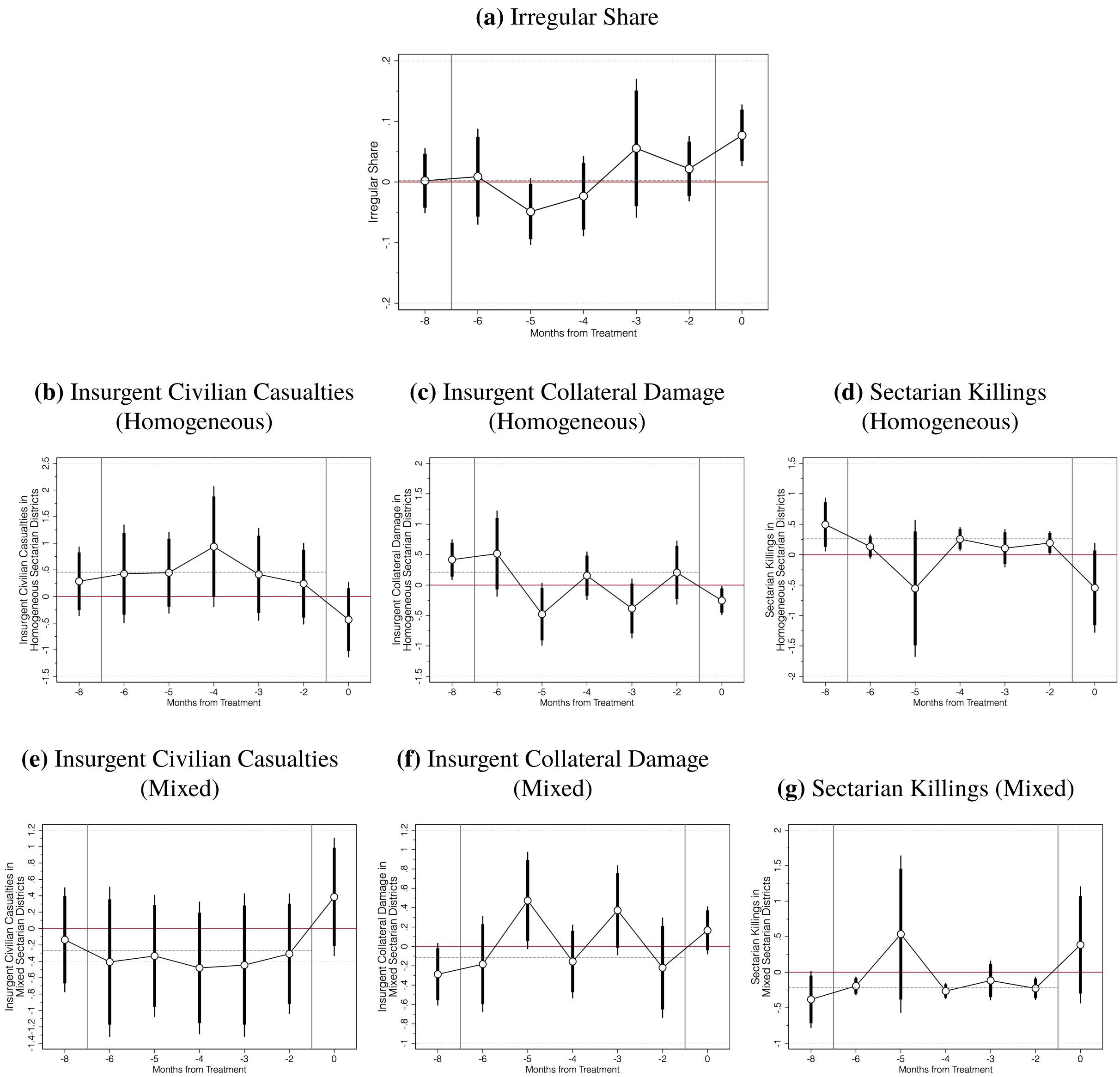
Second, to recover the causal effect of border fortification the design requires fortification not to coincide with other relevant policy changes. In Table B-4 I show that border control did not drive changes in: the number of battalions deployed, cellular network expansion, counterinsurgent spending on governance, petroleum production, or Coalition-caused civilian casualties, among others. Given my expectation that insurgents substitute into irregular attacks, another policy change that could confound the results would be shifts in the deployment of counter-indirect fire systems. Qualitative evidence (section B.7) does not indicate that deployments of these systems shifted with fortification. In sum, the identifying assumptions are met, supporting a causal interpretation of the results.

7 Results

7.1 Tactical Substitution

Table 1 offers a direct test of hypothesis 1, which predicts that border fortification induces rebel shifts into irregular tactics. Column 1 represents the most basic difference-in-differences specification with district and year-specific month fixed effects. Column 2 adds political and socioeconomic controls, and Sunni voteshare-by-year fixed effects, which absorb broad sectarian shifts over the conflict. Column 3 introduces additional security-related controls, column 4 introduces a spatial lag of the dependent variable to account for spatial autocorrelation, and column 5 adds a one period lag of the outcome. Column 6 adds district-specific linear trends. Finally, columns 7-10 shift the focal sample from districts in border governorates. Columns 7 and 8 restrict the analysis to areas where two insurgent movements—AQI and Sunni Rejectionist groups—held influence.

Figure 3: Parallel Pre-Trends



Note: Bars are 90 and 95% confidence intervals. Plots show the effect of treatment leads on the respective outcome. Vertical gray lines denote omitted base periods. Horizontal gray lines denote pre-treatment means. The first lead (-8) accumulates earlier pre-periods, and the first lag (0) accumulates subsequent post-periods (see also Figure B-2). The red line marks 0.

These groups relied heavily on cross-border sanctuaries, so fortification focused largely on interdicting their transnational resources. Finally, in column 9 I expand the analysis to all governorates except Baghdad, and in column 10 I study all districts in Iraq.

Across specifications I find that militants responded to fortification by substituting conventional for irregular attacks. Taking estimates from the fully-saturated specification in column 6 reveals border fortification caused a 6.9 percentage point (pp) increase in the proportion of irregular insurgent attacks, amounting to nearly a one-half standard deviation increase. The estimated effect size across models ranges from 3.1 – 9.6pp.

Table 1: Border Fortification and Tactical Substitution in Iraq

VARIABLES	Irregular Share	Irregular Share	Irregular Share	Irregular Share	Irregular Share	Irregular Share	Irregular Share	Irregular Share	Irregular Share	Irregular Share
Border Fortification	0.031*	0.049**	0.073**	0.072**	0.067**	0.069*	0.096*	0.086**	0.057*	0.049*
	(0.015)	(0.017)	(0.022)	(0.021)	(0.020)	(0.028)	(0.043)	(0.030)	(0.023)	(0.020)
District FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-Specific Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Sunni x Year FE		Y	Y	Y	Y	Y	Y	Y	Y	Y
Political/Socioeconomic Controls		Y	Y	Y	Y	Y	Y	Y	Y	Y
Security Controls			Y	Y	Y	Y	Y	Y	Y	Y
Spatial Lag				Y	Y	Y	Y	Y	Y	Y
Lagged DV					Y	Y	Y	Y	Y	Y
District-Specific Linear Trend						Y	Y	Y	Y	Y
Sample Includes Districts in:	Border Governorates	Border Governorates	Border Governorates	Border Governorates	Border Governorates	Border Governorates	AQI Areas	Rejectionist Areas	All but Baghdad	All of Iraq
Constant	0.027**	0.808**	1.501†	1.487†	1.398†	2.550*	-0.443	0.217	1.408	0.774
	(0.005)	(0.227)	(0.864)	(0.836)	(0.792)	(0.980)	(1.287)	(1.411)	(0.962)	(0.864)
Observations	4,148	3,788	2,109	2,109	2,109	2,109	1,767	2,166	3,078	3,591
R^2	0.139	0.167	0.221	0.223	0.227	0.253	0.342	0.311	0.252	0.274
Log-Likelihood	2426	2137	1031	1034	1040	1076	905.2	1088	1435	1769
AIC	-4848	-4257	-2020	-2024	-2033	-2107	-1764	-2130	-2824	-3492

Note: Robust, district-clustered standard errors are in parentheses. Political/socioeconomic controls are population, population density, urbanicity, unemployment rate, oil reserves, oil production, and CERP spending. Security controls are nighttime lights, total and new cell phone towers, Sons of Iraq, police station density, Coalition maneuver battalions, Coalition collateral damage, condolence spending, police spending, Provincial Reconstruction Teams, Civil Military Operations Centers, and provincial Iraqi control. The mean of irregular share is 0.056, with a standard deviation of 0.166. †p<0.1; * p<0.05; ** p<0.01.

To probe the robustness of these results, in Table 2 I conduct a number of additional tests, all of which corroborate the large, positive effect of border fortification on tactical substitution. Columns 1 and 2 adjust for spatial dependence by allowing for clustering across districts within governorates and DBE regions.[20] In column 3, estimates are scaled using population weights, which identify heterogeneous treatment effects by district population. In column 4, I scale estimates by the intensive margin of violence. Column 5 excludes district-months in which no projectile-fired SIGACTs occurred, and column 6 includes IEDs in the denominator of the dependent variable. I verify the results are robust to controlling for spending on non-fort border security projects and the number of border forts in a district-month in column 7.

Because the dependent variable is a proportion, OLS estimates could fall outside the unit interval. In column 8 I re-estimate the core specification using a two-limit tobit estimator. Tobit estimates are substantively larger, suggesting the main results are conservative. Finally, in columns 9 and 10 I estimate the effect of border fortification on per capita levels of indirect and direct fire attacks, disaggregating the proportion variable into its constituent terms. All tests confirm that

[20]DBE units were organized into 5 areas of responsibility.

Table 2: Robustness of Tactical Results in Iraq

VARIABLES	Irregular Share	Irregular Share	Irregular Share	Irregular Share	Irregular Share	Irregular Share	Irregular Share	Irregular Share	Indirect Fires/Capita	Direct Fires/Capita
Border Fortification	0.067*	0.067†	0.047*	0.101**	0.118**	0.038*	0.064**	0.250*	0.003†	-0.006†
	(0.020)	(0.022)	(0.018)	(0.025)	(0.035)	(0.016)	(0.021)	(0.100)	(0.001)	(0.003)
District FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-Specific Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Sunni x Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Political/Socioeconomic Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Security Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Spatial Lag	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Lagged DV	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Governorate Clustered SEs	Y									
DBE Region Clustered SEs		Y								
Population Weights			Y							
Violence Weights				Y						
Excluding Districts-Months w/o SIGACTs					Y					
Including IEDs in Denominator						Y				
Additional Border Controls							Y			
Two-Limit Tobit								Y		
Constant	1.398†	1.398	1.864†	-1.193	6.614*	1.266†	1.496†	9.818†	0.018	0.023
	(0.583)	(0.639)	(1.066)	(2.090)	(3.096)	(0.698)	(0.791)	(5.673)	(0.047)	(0.055)
Observations	2,109	2,109	2,109	1,312	852	2,109	2,109	2,109	2,109	2,109
R^2	0.227	0.227	0.260	0.422	0.370	0.219	0.227	0.383	0.325	0.760
Log-Likelihood	1040	1040	1169	976.8	352.4	1831	1040	-605.3	6215	3339
AIC	-2033	-2033	-2291	-1908	-658.8	-3615	-2030	1285	-12385	-6631

Note: Robust, district-clustered standard errors are in parentheses unless otherwise noted. Models except column 8 are estimated with OLS. The sample includes all districts in border governorates. Column 8 reports pseudo R^2. Controls are described in Table 1. The mean of irregular share is 0.056, with a standard deviation of 0.166. The mean of indirect fires per capita is 0.004, with a standard deviation of 0.016. The mean of direct fires per capita is 0.033, with a standard deviation of 0.102. †p<0.1, * p<0.05; ** p<0.01.

border fortification causes rebel shifts from conventional to irregular violence.

The logic of the fortification dilemma implies that rebels shift into irregular tactics as fortification reduces their external resources. An alternative mechanism, information-sharing, potentially operates in parallel. Civilian informing is a key constraint on insurgent violence (Kalyvas, 2006). Direct fire and IED attacks are susceptible to exposure if civilians alert counterinsurgent forces. Indirect fires are less vulnerable to informing because they can be set-up at long-range (Berman, Felter and Shapiro, 2018). As such, insurgent substitution from direct into indirect fires is consistent with a shift into cheaper tactics (the resource mechanism), and a shift into less collaboration-sensitive tactics (the information mechanism).

I investigate the information-sharing mechanism in Table C-1, where I study suicide attacks. Suicide bombings are highly resistant to exposure, and so should increase in fortification if the information-sharing mechanism predominates. Instead, results show that border fortification has a precise null effect on suicide attacks. While relatively cheap, these attacks were primarily perpetrated by foreign fighters in Iraq, whose travel into the country was impeded by fortification. This finding is more consistent with the resource mechanism. Still, the information mechanism may complement the resource-centric logic of the fortification dilemma.

7.2 Insurgent-Civilian Relations

Hypotheses 2 and 3 anticipate that rebels respond to border fortification by reducing civilian victimization, particularly of in-group civilians. Table 3 tests these expectations, studying three per-capitized outcomes: insurgent civilian casualties, insurgent collateral damage, and sectarian killings.[21] Parameters follow the main specification from column 5 of Table 1. Columns 1-3 test the main effect of fortification on victimization. Estimates are negatively-signed but small and imprecise, offering weak support for hypothesis 2.

Table 3: Border Fortification and Civilian Victimization in Iraq

	Hypothesis 2			**Hypothesis 3**		
VARIABLES	Insurgent Civilian Casualties	Insurgent Collateral Damage	Sectarian Killings	Insurgent Civilian Casualties	Insurgent Collateral Damage	Sectarian Killings
Border Fortification x In-Group				-0.531*	-0.398**	-0.265†
				(0.221)	(0.095)	(0.132)
Border Fortification	-0.044	-0.099	-0.052	0.439†	0.265*	0.189
	(0.080)	(0.077)	(0.064)	(0.243)	(0.118)	(0.152)
District FE	Y	Y	Y	Y	Y	Y
Year-Specific Month FE	Y	Y	Y	Y	Y	Y
Sunni x Year FE	Y	Y	Y	Y	Y	Y
Political/Socioeconomic Controls	Y	Y	Y	Y	Y	Y
Security Controls	Y	Y	Y	Y	Y	Y
Spatial Lag	Y	Y	Y	Y	Y	Y
Lagged DV	Y	Y	Y	Y	Y	Y
Constant	1.190	-0.017	2.959	2.085	0.622	3.438
	(2.664)	(1.457)	(2.720)	(2.474)	(1.294)	(2.643)
Observations	2,109	2,109	2,109	2,109	2,109	2,109
R^2	0.496	0.487	0.667	0.498	0.488	0.667
Log-Likelihood	-2097	-1990	-2457	-2092	-1987	-2456
AIC	4240	4026	4961	4232	4022	4961

Note: Robust, district-clustered standard errors are in parentheses. The sample includes all districts in border governorates. In-group is an indicator for homogeneous sectarian districts–the constituent term is absorbed by district fixed effects. Controls are described in Table 1. Outcomes are z-standardized. †p<0.1, * p<0.05; ** p<0.01.

Hypothesis 3 anticipates that the reduction in civilian victimization following interdiction of rebels' transnational logistics should manifest most acutely in areas where rebels' prospective civilian supporters are concentrated. In these areas, shared identity ties create affinity, and render rebels' overtures more credible. Ethnic geography provides a heuristic for rebels and potential civilian supporters. Insurgent predation in homogeneous districts dominated by in-group civilians

[21] Outcomes are z-standardized for interpretability.

is likely to target co-sectarians, alienating (prospective) supporters. Victimization in mixed areas is more likely to target out-groups, against whom in-group civilians may tolerate violence.

To test this proposition, in columns 4-6 of Table 3 I interact fortification with an indicator for homogeneous districts, defined as districts where a Sunni, Shi'a, or Kurdish party won at least 66% of the voteshare in the 2005 election. Insurgents operating in homogeneous districts are likely to share in-group identity ties with the dominant sect (Haddad, 2014). Correspondingly, fortification in homogeneous districts is associated with a 0.53 standard deviation (sd) reduction in insurgent civilian casualties, a 0.40sd reduction in insurgent collateral damage, and a 0.27sd reduction in sectarian killings. These effects reverse in mixed districts, where rebel violence can more easily target out-group civilians. Fortification causes a 0.44sd increase in insurgent civilian casualties, a 0.27sd increase in insurgent collateral damage, and a 0.19sd increase in sectarian killings in mixed areas.

One possible concern is that fortification caused a reduction in insurgent civilian victimization because it impeded rebel production of violence, not because insurgents adapted by cultivating civilian support. Sectarian heterogeneity in the effect of fortification is inconsistent with this view. I would not observe increasing victimization in mixed sectarian districts if border fortification simply reduced militants' ability to produce violence generally. Figure C-1 also yields no evidence of an overall decline in violence in response to fortification. The insurgents' ability to reduce collateral damage in homogeneous areas, despite adopting less precise tactics (indirect fires), is strongly suggestive of conscious effort to minimize civilian harm.

Several additional tests confirm the robustness of these results. To address concerns about under-reporting, I re-estimate results focusing on the extensive margin of one-sided violence (Table C-2) and find similar effects. In Figure C-2 I disaggregate the results by sect. The negative effect of fortification on victimization is largest in Sunni districts. By contrast, fortification had little distinguishable effect in Shi'a districts, and a positive effect in mixed districts.[22] As noted above, fortification efforts chiefly focused on interdicting external support to AQI and Rejectionist groups, which operated mostly in Sunni areas. Figure C-2 also confirms that results are robust to operationalizing districts' sectarian composition using population rather than voteshares. In Table C-3 I show results are robust to alternate specifications and estimators.

[22] I also find consistent evidence studying sectarian transborder settlements (Figure C-3).

These findings bolster extant models of the sectarian war in Iraq. As Weidmann and Salehyan (2013) show, insurgent groups deliberately targeted civilians in mixed areas, driving segregation into homogeneous sectarian enclaves. By using violence in mixed regions, insurgents polarized the population along ethnic lines. In turn, this effort enhanced insurgents' ability to obtain succor from in-group civilians. Sectarian violence forced civilians to turn to in-group rebels for security. Particularly in mixed areas around Baghdad, Sunni civilians relied on Sunni insurgents for protection against Shi'a militias. Security is a key resource insurgents can provide to cultivate civilian support (Wood, 2010). From this perspective, insurgents' response to fortification was about creating a problem only they could solve. Border fortification contributed to increasing civilian victimization in mixed sectarian areas, bolstering in-group civilian support for insurgents in homogeneous areas, where civilians turned to rebels for protection.

8 Robustness and Extensions

Rich data from Iraq permit a variety of additional tests, which illustrate further implications of the fortification dilemma.

8.1 Rebel Surveillance

Insurgents concerned about border fortification should focus intelligence-gathering efforts on counterinsurgent enforcement. For example, by spying on border infrastructure, insurgents could identify safer crossings. Captured AQI documents (Figure 4) reveal militants did just that. AQI established a "Border Emirate" to manage its foreign logistics, and compiled weekly reports about enforcement, including documenting the ease of crossing in various locations.

8.2 Rebel Finances

The fortification dilemma should emerge whenever fortification increases the price of external support. Unique data from captured insurgent financial records (Bahney et al., 2010) permit an exploratory test. The records detail fiscal transfers from AQI's provincial administration to Anbar sectors from June 2005–October 2006. Results in Table D-1 suggest that fortification increased province-to-sector transfers by 0.23sd. This implies about $31,353 per month in spending by the average cell. Other records reveal why enforcement increased militant expenditures—fortification

Figure 4: Insurgents Surveilled Border Fortification

بسم الله الرحمن الرحيم

تقرير الحدود
تقرير اسبوعي

المنطقة:

القاطع:

أمير الحدود:

إسم المنطقة الحدودية:

تواجد العدو بالقرب من المنفذ: (إن وجد)

درجة العدائية:

الحالة الحالية: (يوجد منفذ جيد – لا يوجد إطلاقا – يوجد بصورة متقطعة – يوجد ولكن خطر)

عدد النقاط الحدودية المعادية (إحداثياتها – وصفها – قوتها – درجة الإنتباه والعدائية)

الإحتياجات: (أجهزة إحداثيات – ثريا - سيارات)

تقرير العبور الاسبوعي: (الاخوة – السلاح – الأموال - البريد)

In the name of God the most Merciful, the most Compassionate

Borders Report
Weekly Report

Area:
Sector:
Borders' Emir:
Name of the border area:
Enemy locations near the port of entry: (if any)

Level of hostility:
Current situation: (there is a good port of entry – there is none at all – there is an intermittent port of entry – there is a port of entry but it's dangerous.)

Number of enemy border checkpoints (coordinates – description – strength – level of attention and hostility.)

Equipments required: (coordinates systems – cell phones – vehicles.)

The weekly crossing report: (the brothers – weapons – funds – mail.)

End of document / translation

Note: The document (left) is a template of border reports compiled by AQI spies and captured by US forces in 2007. The translation (right) is provided by the Combating Terrorism Center. Harmony Program: NMEC-2007-658008.

raised smugglers' fees. AQI financial ledgers indicate cells were paying up to $4,985 to smugglers biweekly, with an average expenditure of $3,425 per month.[23]

Military officials recognized that insurgent spending in communities where fortification disrupted local livelihoods facilitated militants' efforts to build support. Troops in Anbar noted, "[t]he geographically remote villages and tribes assist in smuggling weapons and Foreign Fighters (FF) because it provides basic life sustainment for these villages that have little or no local industry or commercial potential" (CJSOTF–AP, 2007). This effort was facilitated by many militant commanders' prior experience in Baathist smuggling networks.[24] These results also underscore synergy between territorial control and smuggling (Asal, Rethemeyer and Schoon, 2019). Fortification degraded insurgents' foreign control, constraining trafficking. Consequently, insurgents sought greater local control in receptive borderland communities, which afforded new smuggling opportunities.

[23] Harmony Program: NMEC-2007-657731; NMEC-2007-657777; NMEC-2007-657860.

[24] Prior experience was an advantage for coordinating logistics; however, leaders feared ex-Baathists were vulnerable to foreign influence by virtue of past contacts with regional intelligence services (Harmony Program: NMEC-2007-612449).

8.3 Rebel Smuggling

Iraqi militants leveraged historical trafficking networks, through which they could continue accessing foreign support. Tactical shifts along smuggling routes could cause conflict spillovers, biasing estimates. Spatial lags in the analyses account for spillovers, but to further probe smuggling dynamics, I study ratlines geotraced from a declassified map (Figure D-1). If tactics hinge on insurgents' abilities to sustain external resources, the effect of fortification should attenuate where insurgents maintain alternate lines-of-communication.

I test this implication in Figure D-2. Consistent with the main logic of the fortification dilemma, fortification caused insurgent shifts into irregular tactics and reduced civilian victimization where insurgents did not have access to ratlines that could facilitate external re-supply. Fortification caused precisely the opposite effects—more conventional attacks and civilian victimization—where insurgents maintained hard-to-interdict ratlines. Along high-density trafficking nodes where insurgents could access multiple routes but counterinsurgent surveillance was concentrated, fortification had no distinguishable effect on violence.

8.4 Sponsorship and Subversion

Subversion by hostile neighbors frequently undermines state capacity (Lee, 2020). In Iraq, Iran actively countered border enforcement, using bribes and incursions to ensure resources reached their militant surrogates (Felter and Fishman, 2008). An implication is that fortification should have weaker effects in areas dominated by Iranian proxy groups, which could rely on Iranian subversion to sustain external resources. Table D-2 explores this implication, focusing on JAM-dominated areas contiguous to Iran. In these regions, the effect of border fortification was substantively small and indistinguishable.

Tables D-3 and D-4 study other neighboring countries—Saudi Arabia, Syria, Jordan, and Kuwait. The former three tacitly-sponsored militants, but did not engage in overt subversion. Effects of fortification are large and precise in districts adjacent to these states. In contrast, Kuwait was not an important conduit for insurgent support because it sealed its border with Iraq after 1991. Negligible effects of fortification in areas near Kuwait reflect this dynamic. Together, these findings suggest the effect of fortification is conditional on interstate dynamics between target and sanctuary countries (Gavrilis, 2008). Overt (versus tacit) sponsorship attenuates the efficacy of fortification.

The interaction of border security regimes is also relevant. Iraq's 2007 National Border Strategy emphasized the value of "regional engagement... to synchronize border efforts."

8.5 Temporal Dynamism

The effect of border fortification could decay over time as insurgents find new bases of support. On the other hand, without alternate smuggling routes or overt support from a state sponsor, insurgents may be unable to recoup external resources. This would imply durable tactical shifts. I take several approaches to understand temporality in the effect of fortification. First, Figure B-2 plots event study estimates. Second, in Figure D-4 I re-estimate a series of regressions of progressively longer leads of outcomes on border fortification. The results suggest that tactical shifts emerge quickly and persist for roughly a year. Effects on civilian victimization emerge somewhat more slowly, and attenuate in the longer-run (12-22 months post-treatment). That tactical substitution attenuates before victimization effects may indicate that insurgents' efforts to build support succeed in mobilizing civilians, relaxing constraints on production of conventional violence (Bueno de Mesquita, 2005, 2013).

The prospect of temporal heterogeneity in the effect of fortification raises questions about the constant effect assumption. With staggered treatment, difference-in-differences estimators based on two-way fixed effects yield a variance-weighted average treatment effect. When already-treated units act as controls, changes in treatment effects over time may bias the overall estimate (Goodman-Bacon, 2021). Figure D-5 depicts results based on new classes of difference-in-differences estimators. The findings are unchanged.

8.6 Placebo Tests

I argue that border fortification affects violence by interdicting insurgents' external resources. One concern is that tactical substitution could owe to a composition shift in government targets (Braithwaite and Johnson, 2015) rather than insurgent resources. Border forts are fixed installations that pose a convenient target for indirect fires. Another concern is that the observed effects are generic to all Iraqi counterinsurgent presence. Placebo tests using the construction of non-fort security infrastructure—e.g., military bases and police facilities—help rule out these possibilities. If substitution into indirect fires occurred solely because forts are opportune targets, I would anticipate similar effects of other fixed infrastructure. Likewise, while other security facilities afforded a greater Iraqi role in counterinsurgency, they were not used to interdict borders, and so should not affect insurgents' endowments. Encouragingly, results in Figure D-6 confirm that the effects are unique to border forts, bolstering the resource-centric logic I posit.

9 Conclusion

While the conventional wisdom on counterinsurgency strategy suggests border fortification is critical for defeating transnational insurgents, I argue that this unqualified prescription neglects important tactical dynamics. To the extent fortification efforts degrade transnational rebels' external resources, rebels are likely to adapt by seeking to cultivate better relations with the civilian population in the target state. As a result, border fortifications, while reducing the fighting capacity of insurgents, can also induce greater competition between rebels and counterinsurgents for the loyalties of the civilian populace. Counterinsurgents contemplating pursuing border control must weigh whether the good consequences—reduced insurgent capabilities—outweigh the bad—increased competition over hearts-and-minds.

The relationship between resources and military power is a first-order question for political economy theories of conflict. Results presented in this article extend important theories linking rebel resources and tactics, and offer some of the first plausibly causal evidence about how resources impact combat capacity. The results also challenge prevailing accounts about how resource losses spur rebel predation. If civilians are central to rebel recovery, rebels may engage in greater forbearance, not victimization, after losses. Perhaps most critically, this paper highlights the importance of viewing transnational dimensions of civil war as a subject of contestation in themselves. External sanctuaries and resources are not exogenous or incontestable characteristics of rebellion, and efforts to reduce rebels' transnational support bear crucial consequences for the microdynamics of conflict.

The policy implications are clear. While border fortification can help degrade transnational insurgents' capabilities, counterinsurgents must be prepared to endure irregular campaigns, and to invest in hearts-and-minds initiatives designed to raise living standards and civilian livelihoods. Otherwise, fortification-induced competition from insurgents over civilians' loyalties may ultimately make counterinsurgents' tasks more difficult. Population-centric programs should be employed in tandem with counterinsurgent border fortification.

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Supplementary Materials for The Fortification Dilemma: Border Control and Rebel Violence

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Contents

A Data and Measurement

A.1 Potential Biases in Civilian Victimization Data

In the main text I study insurgent violence against civilians using data from Iraq Body Count (IBC) and the World Incidents Tracking System (WITS). For reference, IBC data are described in greater detail in (Condra and Shapiro, 2012), while WITS data are introduced in (Wigle, 2010). IBC records violent incidents resulting in death, and captures the date and location, at a minimum, for each incident. IBC events are coded from English language commercial media reports, including reports originating in non-English languages and translated by major Middle Eastern and Iraqi press agencies, along with NGO reports, and hospital and morgue records provided by Iraqi Medico-Legal Institutes and the Iraqi Ministry of Health.

WITS records incidents of politically-motivated violence against civilians, and captures the date, location, and number killed, at a minimum, for each incident. WITS data are maintained by the US National Counterterrorism Center (NCTC), and represent the source for the data on terrorism reported in Congressionally-mandated annual terrorism reports, including the State Department's Country Reports on Terrorism and the NCTC Report on Terrorism. WITS events are machine coded from commercial newswires, the US Government's Open Source Center, and local press reports, and then cross-checked by human researchers at the NCTC. A common set of sources and search strings is maintained by NCTC for quality control.

I rely on IBC and WITS for data on civilian victimization because insurgent violence against civilians is undercounted in the MNF-I SIGACT III database, from which I draw measures of insurgent-initiated violence against Coalition and Iraqi forces. As Berman, Shapiro and Felter (2011, 790) explain, the SIGACT data "capture violence against civilians and between nonstate actors only when U.S. forces are present and so dramatically undercount sectarian violence... ." While IBC and WITS are hence preferable to MNF-I SIGACT III for measuring civilian victimization, because these alternative data sources are coded from media reports it is possible that they are subject to reporting bias. Recent scholarship shows that reporting biases in media focus can affect statistical results (Dafoe and Lyall, 2015; Weidmann, 2016), raising concerns about bias in the IBC and WITS data I study.

Overall, I am sanguine that reporting biases in the IBC and WITS data are unlikely to drive the observed negative effect of border fortification on civilian victimization for several reasons. First, consider situations where reporting bias in IBC and WITS data could be systematically correlated with border fortification. This could happen if the implementation of border fortification led to the deployment of more Coalition troops and embedded reporters, in turn improving media reporting of insurgent civilian victimization. Alternatively, what if the implementation of border fortification meant improved security conditions, such that cell phone service providers could expand coverage of the cell network in peripheral border regions, in turn improving reporting of insurgent civilian victimization by facilitating mobile penetration. In both of these plausible scenarios, the direction of bias between border fortification and reporting bias in IBC and WITS is positive. In other words, I would be more likely to observe a spurious positive effect of border fortification on insurgent civilian victimization if the roll-out of border forts led to increased media or troop presence or expansion of the cell network. I identify precisely the opposite effect in the main text: border fortification reduces insurgent civilian victimization, at least in homogeneous sectarian districts. Second, all of the arguments I can think of for reporting bias in IBC and WITS point in the same direction, whereas I find heterogeneous effects of border fortification on insurgent civilian victimization by district sectarianism. Third, in Table B-4, I find no significant correlations between border fortification and deployments of Coalition troops or changes in cell coverage. These results suggest that border fortification did not induce policy changes that could also affect reporting bias in IBC and WITS data. Fourth, IBC and WITS draw extensively on local Iraqi media, which operated widely throughout the conflict. It is unlikely that local press reporting varied much month-to-month within districts. Hence, while IBC and WITS may contain some measurement error orthogonal to the relationship of interest, this is an issue of statistical precision, not bias.

To more formally probe potential biases in IBC and WITS I take a few steps. First, I estimate coefficients of proportionality (δ) for the models reported in columns 4-6 of Table 3 using the method described in Oster (2019). Conceptually, δ represents the degree of selection on unobservables relative to observables (i.e., controls) required to explain away an estimated effect.[25] For the insurgent civilian casualties outcome (column 4 of Table 3), δ = -3.593 for the effect of border fortification in homogeneous districts and 0.674 for the effect of border fortification in mixed districts. For the insurgent collateral damage outcome (column 5 of Table 3), δ = -3.636 for the effect of border fortification in homogeneous districts and 2.205 for the effect of border fortification in mixed districts. For the sectarian killings outcome (column 6 of Table 3), δ = -0.589 for the effect of border fortification in homogeneous districts.[26] Negative values of δ across the border fortification $\times$ homogeneous interaction term indicate that controlling for observables strengthens the estimated negative effect of border fortification on insurgent civilian victimization in homogeneous districts relative to a model without controls. Negative δs are uninformative about the size of potential bias, but they do indicate that results are unlikely to be driven by omitted variables like reporting biases in IBC and WITS data. In mixed districts, positive δs indicate that unobservables would have to be 0.67 to 2.2 times more important than observables in order to attrite the observed positive point estimate of border fortification on insurgent civilian casualties and insurgent collateral damage to 0. These tests build confidence that our results are not driven by unobserved bias in the IBC or WITS data.[27]

Second, in Figure A-1 I employ a variant of the test suggested by Weidmann (2016) to determine the influence of mobile coverage on reporting bias in the IBC and WITS data. The logic of the test is that if reporting bias owing to cell phone coverage is affecting data, we should see the effect of cell phones on violence significantly differ for less severe attacks than for more severe attacks. As Weidmann (2016, 214-215) explains: "a small event with one casualty is likely to go unreported due to difficulties in communication, but a major attack that leaves 15 people dead will be reported no matter whether cellphone coverage exists at the location of the attack. This means that if selective reporting affects our results, a positive effect of cellphone coverage should be weaker or even disappear if we analyze high-fatality events as compared to low-fatality ones, since the former will suffer less from reporting being driven by cellphone coverage."

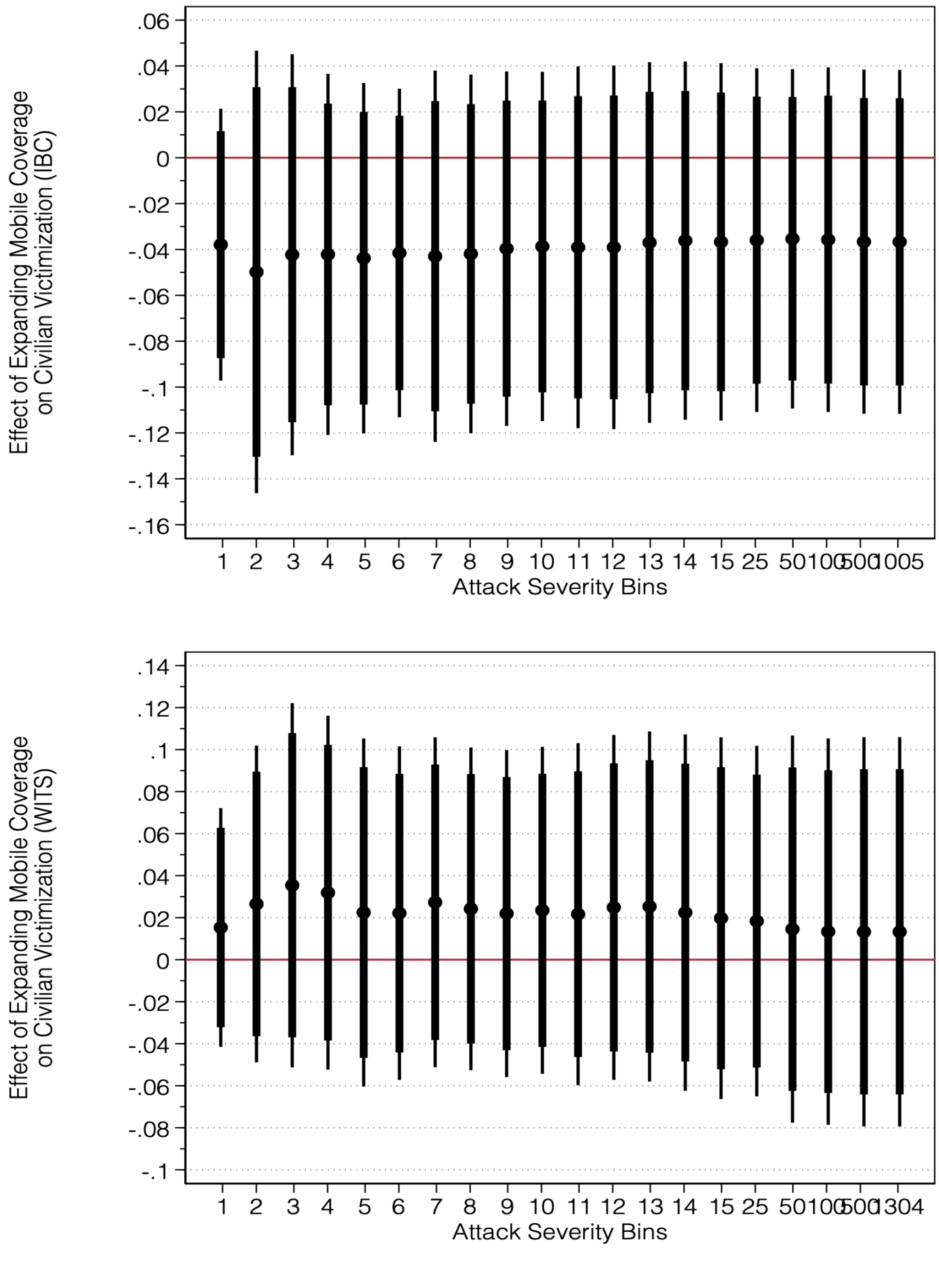
I implement this test for IBC data on insurgent collateral damage and WITS data on insurgent civilian casualties. The specific procedures for the results reported in Figure A-1 are as follows. First, I split IBC and WITS data by the reported severity of each attack. For IBC this means splitting

[25]Per Oster (2019)'s recommendation, I base the calculation of δ on a maximum R^2 of $1.3 \times R^2_{Full}$, where R^2_{Full} equals the within-district R^2 from the full model with controls reported in the respective column 4-6 of Table 3 in the main text.

[26]I do not estimate δ for the effect of border fortification on sectarian killings in mixed sectarian districts because the estimated effect is not statistically significant.

[27]I am not concerned about reporting bias in the irregular share dependent variable based on MNF-I SIGACT III data, but I estimate δ for models of the effect of border fortification on irregular tactics anyway to assess their sensitivity. For the main irregular share model (column 4 of Table 1), δ = -1.203. As with the civilian victimization outcomes, this indicates that the irregular share results are unlikely to be driven by omitted variables.

Figure A-1: The Effect of Cell Coverage on Civilian Victimization Does Not Vary Over Incident Severity



Note: Bars are 95% confidence intervals based on robust, district-clustered standard errors. Estimates are from OLS models, and show the effect of the lagged first-difference in the number of new cell phone towers built in a district on insurgent civilian victimization from IBC (top panel) and WITS (bottom panel). Each model includes controls for population, population density, the urban population share, a spatial lag of the insurgent civilian victimization dependent variable, and district and year-specific month fixed effects.

the data by the maximum number of deaths in each event, and for WITS this means splitting the data by the total number of casualties in each event. Then, I subset the data to include all attacks at or below each severity level, and collapse these attacks, summing their incidence at the district-month level. Finally, I estimate the effect of expanding cell tower coverage on the number of attacks in a least squares regression framework. I repeat this procedure for successive severity bins, moving in increments. For instance, for the 10 casualty bin I subset the data to include all attacks that caused 10 or fewer casualties for the IBC and WITS variables. Then, I regress the count of attacks of a given severity level on the lagged first-difference in the number of new cell phone towers built in a district, repeating this approach for each severity bin. Mean severity increases over successive bins. Results show no evidence that the effect of expanding cell coverage on either civilian victimization measure significantly differs for high severity versus low severity attacks.

Table A-1: Correlations Between IBC/WITS and SIGACTs Data on Civilian Victimization

	Civilian Victimization at the Military Division-Month				Coalition-Caused Civilian Casualties at the Governorate-Month	
VARIABLES	(1) Insurgent Civilian Victimization (WITS)	(2) Insurgent Civilian Victimization (WITS)	(3) Sectarian Killings (IBC)	(4) Sectarian Killings (IBC)	(5) Coalition-Caused Civilian Casualties (IBC)	(6) Coalition-Caused Civilian Casualties (IBC)
Sectarian Incidents (SIGACTs)	0.395**	0.174**	0.322**	0.081**		
	(0.086)	(0.022)	(0.052)	(0.016)		
Coalition-Caused Civilian Casualties (SIGACTs)					0.006**	0.003*
					(0.002)	(0.001)
Unit FE	N	Y	N	Y	N	Y
Year-Specific Month FE	N	Y	N	Y	N	Y
Constant	46.996	58.556**	29.701	42.295**	4.517†	5.025**
	(27.281)	(1.143)	(15.409)	(0.854)	(2.334)	(0.197)
Observations	224	224	224	224	1,000	1,000
R^2	0.352	0.923	0.397	0.775	0.010	0.154
Log-Likelihood	-1278	-1039	-1211	-1100	-4817	-4739
AIC	2560	2082	2426	2205	9639	9482

Note: Robust standard errors clustered by military division (columns 1-4) and governorate (columns 5-6) are in parentheses. Unit fixed effects are for military divisions in columns 2 and 4, and for governorates in column 6. Models estimated using OLS. †p<0.1, * p<0.05; ** p<0.01.

Finally, in Table A-1 I compare data from WITS and IBC to data on civilian victimization contained within the MNF-I SIGACT III database. The US military have released SIGACTs data on sectarian incidents at the military division-month level for January 2006 through August 2008, and SIGACTs data on Coalition and Iraqi forces-caused civilian casualties at the governorate-month level for January 2004 through August 2008. These newly-released data are based on instances of violence against civilians observed directly by or locally reported to Coalition and Iraqi military forces, which were deployed across Iraq, and whose reporting was not affected by the availability of cellular communications technologies or the presence of embedded reporters. If the variation in the WITS/IBC data on killings of civilians are broadly consistent with these administrative sources, concerns about systematic measurement error in WITS and IBC are reduced. Regressing WITS incidents and sectarian incidents recorded in IBC on sectarian/insurgent civilian victimization SIGACTs (columns 1-4) shows that SIGACTs victimization data are highly correlated with WITS/IBC data, and explain a high proportion of total model variability. A similarly strong correlation emerges between SIGACTs and IBC data on Coalition-caused civilian casualties. Shaver and Shapiro (2021) also validate a high correlation between IBC data and not-yet-publicly-available SIGACTs data on civilian victimization.

A.2 Descriptive Statistics

Descriptive statistics for variables used in the main analysis can be found here.

Table A-2: Summary Statistics

	Observations	Mean	Std. Dev.	Minimum	Maximum
Dependent Variables:					
Irregular Share	2109	0.056	0.166	0	1
Insurgent Civilian Casualties	2109	0.057	0.219	0	2.723
Insurgent Collateral Damage	2109	0.002	0.005	0	0.066
Sectarian Killings	2109	0.005	0.018	0	0.411
Independent Variables:					
Border Fortification	2109	0.427	0.495	0	1
Number of Border Forts	2109	3.009	5.832	0	37
Non-Fort Border Infrastructure	2109	0.069	0.253	0	1
Directorate of Border Enforcement Academy	2109	0.035	0.183	0	1
In-Group (Homogeneous Sectarian District)	2109	0.865	0.342	0	1
Control Variables:					
Population	2109	5.675	1.004	3.090	7.599
Population Density	2109	0.135	0.222	0	1.160
Urban Population	2109	0.536	0.194	0.156	0.936
Sunni Share	2109	0.189	0.392	0	1
Shi'a Share	2109	0.676	0.468	0	1
CERP Spending	2109	2312.542	8605.321	0	185458.284
Nighttime Lights	2109	0.048	0.047	0.003	0.320
Unemployment Rate	2109	0.107	0.065	0	0.333
Price-Weighted Oil Reserves	2109	8.832	6.669	0	17.588
Price-Weighted Oil Production	2109	15.050	11.804	0	27.355
Cell Phone Towers	2109	9.531	19.834	0	172
New Cell Phone Towers	2109	0.340	1.459	0	35
Sons of Iraq	2109	0.087	0.282	0	1
Police Density	2109	0.011	0.018	0	0.112
Coalition Maneuver Battalions	2109	0.728	1.279	0	15.5
Coalition Collateral Damage	2109	0.001	0.003	0	0.051
Condolence Spending	2109	131.384	808.449	0	22510.368
Police Spending	2109	1093.505	4427.149	0	87363.273
Provincial Reconstruction Team	2109	0.045	0.206	0	1
Civil Military Operations Center	2109	0.142	0.349	0	1
Provincial Iraqi Control	2109	0.185	0.389	0	1

Note: Descriptive statistics are calculated from the main estimating sample in column 5 of Table 1 and columns 1-6 of Table 3.

B Identification Strategy

B.1 Violence and Construction Timelines

Using project-level data, I study the relationship between violence and the difference between actual and forecasted project start and finish dates. None of the focal violence outcomes are significantly correlated with construction timelines.

Table B-1: Violence Trends and Construction Timelines

	Insurgent-Initiated SIGACTs				Irregular Share				Insurgent Civilian Casualties			
VARIABLES	(1) Start	(2) Start	(3) Finish	(4) Finish	(5) Start	(6) Start	(7) Finish	(8) Finish	(9) Start	(10) Start	(11) Finish	(12) Finish
Violence Trend (6 Month MA)	11.953		57.859		-43.930		-123.419		25.087		-2.121	
	(28.108)		(59.690)		(63.786)		(112.765)		(106.343)		(48.780)	
Violence Trend (9 Month MA)		52.259		-97.093		68.909		-72.858		609.618*		-36.556
		(35.132)		(95.365)		(90.660)		(105.785)		(243.184)		(87.929)
Constant	15.220	-22.667	263.281*	224.370*	4.475	44.614	262.879*	220.776†	263.132	-223.818	268.071*	262.582*
	(263.044)	(243.002)	(116.742)	(108.761)	(249.698)	(227.391)	(115.861)	(111.852)	(361.942)	(285.007)	(118.835)	(117.093)
District FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-Specific Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	220	183	272	232	220	183	272	232	267	212	272	266
R^2	0.548	0.557	0.831	0.823	0.548	0.557	0.831	0.822	0.555	0.566	0.830	0.816
Log-Likelihood	-1096	-926.1	-1362	-1172	-1096	-926.1	-1362	-1173	-1401	-1055	-1362	-1331
AIC	2198	1858	2729	2350	2198	1858	2729	2351	2807	2117	2731	2668

Note: Robust standard errors clustered by district are in parentheses. Models are estimated using OLS. MA = moving average. Estimates are from the cross-section of border fortification projects. Dependent variables are the differences in $\mathtt{actual} - \mathtt{forecasted}$ project start (columns 1, 2, 5, 6, 9, 10) and finish dates (columns 3, 4, 7, 8, 11, 12). Violence trends reflect trends in the respective header variable. For instance, in column 1 "Violence Trend (6 Month MA)" captures the six-month lagged moving average of insurgent-initiated SIGACTs prior to the project start date. Higher values indicate the project finished later than expected. Year-specific month fixed effects are for the month the project began in the "Start" models, and for the month the project ended in the "Finish" models. District fixed effects absorb time-invariant characteristics of districts that could affect construction (e.g., weather, soil type, access to construction materials). †p<0.1, * p<0.05; ** p<0.01.

B.2 Violence and Treatment Onset

Using panel data, I study the relationship between violence trends and initial border fortification. None of the focal violence outcomes are significantly correlated with treatment onset.

Table B-2: Violence Trends and the Onset of Border Fortification

	Insurgent-Initiated SIGACTs				Irregular Share				Insurgent Civilian Casualties			
VARIABLES	(1) Cox PH	(2) OLS	(3) Cox PH	(4) OLS	(5) Cox PH	(6) OLS	(7) Cox PH	(8) OLS	(9) Cox PH	(10) OLS	(11) Cox PH	(12) OLS
Violence Trend (6 Month MA)	-1.134	-0.003			-2.496	-0.044			-1.364	-0.003		
	(1.782)	(0.005)			(2.201)	(0.033)			(1.431)	(0.003)		
Violence Trend (9 Month MA)			-1.347	-0.003			-1.113	-0.025			-2.671	-0.004*
			(1.943)	(0.004)			(2.202)	(0.034)			(2.197)	(0.002)
Constant		0.037**		0.034**		0.034**		0.033**		0.031**		0.028**
		(0.016)		(0.016)		(0.016)		(0.015)		(0.013)		(0.014)
Year-Specific Month FE	N	Y	N	Y	N	Y	N	Y	N	Y	N	Y
Observations	2,261	2,261	2,063	2,063	2,261	2,261	2,063	2,063	2,643	2,643	2,443	2,443
Log-Likelihood	-124.1	1679	-90.70	1731	-123.7	1680	-90.84	1731	-124	2169	-115.8	1992

Note: Robust standard errors clustered by district are in parentheses. Cox proportional hazards models study time until fortification, with exponentiated coefficients reported. OLS models study the probability of fortification up to the period of treatment onset. All models subset to the sample of districts in border governorates. MA = moving average. Violence trends reflect trends in the respective header variable. All models also control for district population. †p<0.1, * p<0.05; ** p<0.01.

B.3 Temporal Placebo Check

A temporal placebo check gives no evidence that contemporary border fortification predicts past violence.

Table B-3: Fortification Does Not Predict Past Violence

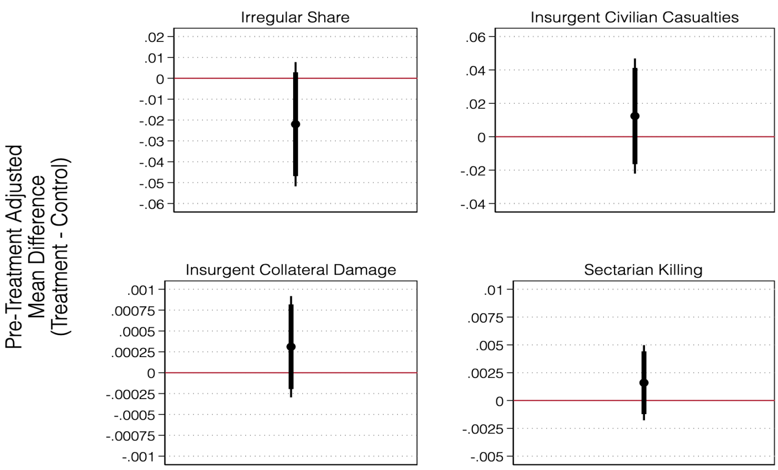
	Insurgent-Initiated SIGACTs				Irregular Share				Insurgent Civilian Casualties			
VARIABLES	(1) 1 Month Lag	(2) 3 Month MA	(3) 6 Month MA	(4) 9 Month MA	(5) 1 Month Lag	(6) 3 Month MA	(7) 6 Month MA	(8) 9 Month MA	(9) 1 Month Lag	(10) 3 Month MA	(11) 6 Month MA	(12) 9 Month MA
Border Fortification	-0.014	-0.014	-0.011	-0.010	0.024	0.028	0.029	0.027	-0.015	-0.016	-0.013	-0.017
	(0.017)	(0.017)	(0.017)	(0.018)	(0.015)	(0.017)	(0.018)	(0.017)	(0.018)	(0.017)	(0.016)	(0.017)
Constant	0.037	0.048	0.062	0.078†	0.204*	0.192†	0.157†	0.100	0.016	0.020	0.037	0.058
	(0.104)	(0.091)	(0.067)	(0.042)	(0.096)	(0.099)	(0.091)	(0.087)	(0.054)	(0.051)	(0.043)	(0.058)
District FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-Specific Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Sunni x Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	4,148	4,012	3,808	3,604	4,148	4,012	3,808	3,604	4,828	4,692	4,488	4,284
R^2	0.675	0.723	0.765	0.805	0.148	0.285	0.406	0.472	0.499	0.691	0.770	0.803
Log-Likelihood	2874	3170	3338	3493	2448	4280	5220	5673	1882	3668	4351	4550
AIC	-5742	-6334	-6671	-6980	-4890	-8554	-10434	-11341	-3758	-7329	-8696	-9095

Note: Robust standard errors clustered by district are in parentheses. Models are estimated using OLS. All models subset to the sample of districts in border governorates. MA = moving average. Dependent variables are the specified lags of the violence variables described in the header. For instance, in column 3 "6 Month MA" captures the six-month lagged moving average of insurgent-initiated SIGACTs. All models also control for district population. †p<0.1, * p<0.05; ** p<0.01.

B.4 Assessing Covariate Balance

To probe covariate balance across treatment and control districts in the pre-treatment period, I regress each outcome on an indicator for fortified districts prior to the intervention. Adjusted mean differences are calculated from these regressions. None of the focal outcomes are distinguishable from 0 (p > .1).

Figure B-1: Adjusted, Pre-Treatment Mean Differences in Dependent Variables



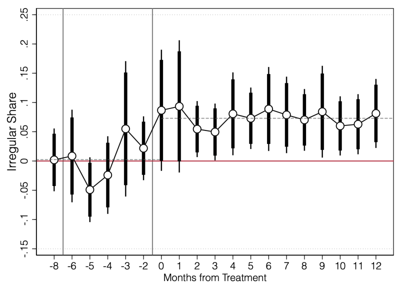
Note: Bars are 90 and 95% confidence intervals based on robust, district-clustered standard errors. Mean differences are calculated from OLS regressions of treatment status on the respective outcome, with district, year-specific month, and Sunni voteshare-by-year fixed effects.

B.5 Panel Event Study Estimates

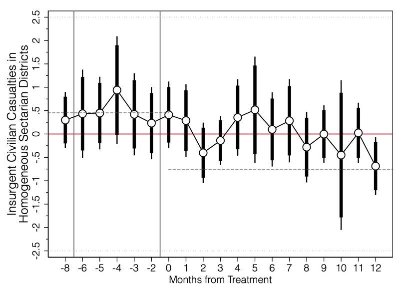
I provide graphical evidence of parallel pre-trends in Figures 3 and B-2. Following the method introduced in Sun and Abraham (2021), I exclude two pre-policy periods (one and seven months before treatment). Eight leads (j) and twelve lags (k) are included, and final leads and lags "accumulate" subsequent effects beyond the j and k periods, as in Freyaldenhoven et al. (2021) and Clarke and Schythe (2021). Violence is parallel in the pre-treatment period, building confidence in the design. Treatment lags indicate that the positive effect of fortification on the share of irregular insurgent attacks emerges within 2 months and persists for at least a year. In homogeneous (mixed) areas, insurgent civilian casualties distinguishably decline (increase) after a year; insurgent collateral damage incidents decline (increase) 2 and 9 months after treatment, as well as durably after a year; and sectarian killings decline (increase) quickly between 2-4 months and 8 months after treatment.

Figure B-2: Dynamic Event Study Estimates

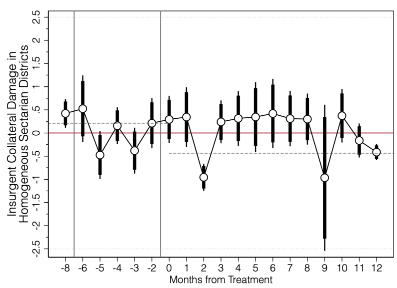
(a) Irregular Share



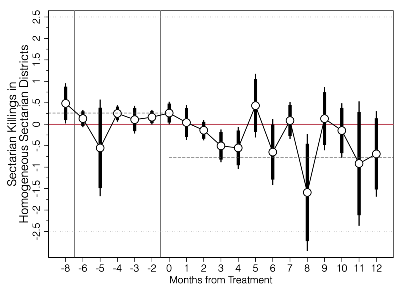
(b) Ins. Civilian Casualties (Homog.)



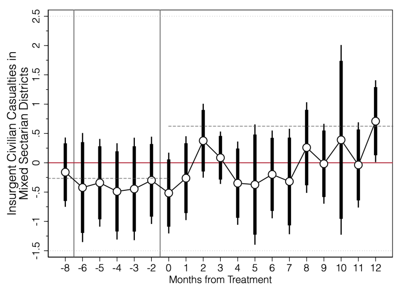
(c) Ins. Civilian Casualties (Homog.)



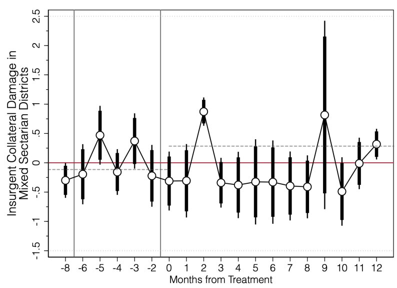
(d) Sectarian Killings (Homog.)



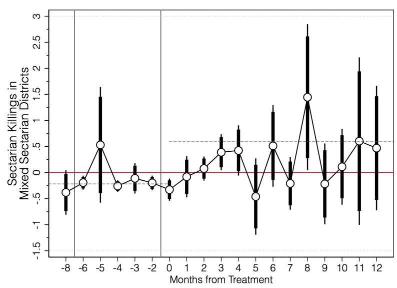
(e) Ins. Civilian Casualties (Mixed)



(f) Ins. Collateral Damage (Mixed)



(g) Sectarian Killings (Mixed)



Note: Bars are 90 and 95% confidence intervals. Each plot shows the effect of treatment leads and lags on the respective outcome. Horizontal gray lines denote pre- and post-treatment means. Vertical gray lines denote omitted base periods. The red line marks 0.

B.6 Fortification Did Not Cause Other Policy Changes

Key to my identification strategy is that border fortification did not cause other policy changes that could explain the focal effects. I regress a range of pertinent outcomes on fortification, and find no distinguishable effects. In particular, fortification did not systematically coincide with the deployment of more maneuver battalions or Provincial Reconstruction Teams. Nor did it affect other counterinsurgent security spending. Evidence that fortification was uncorrelated with the expansion of the Iraqi mobile network helps assuage concerns about reporting bias discussed in Figure A-1.

Table B-4: Border Fortification Does Not Predict Key Policy Changes

VARIABLES	(1) Maneuver Battalions Deployed	(2) Total Cell Towers	(3) New Cell Towers	(4) CERP Spending	(5) Oil Production	(6) Coalition-Caused Civilian Casualties	(7) Condolence Payments	(8) Police Stations	(9) Nighttime Lights	(10) Sons of Iraq Spending	(11) Provincial Reconstruction Team	(12) Civil-Military Operations Center	(13) Provincial Iraqi Control
Border Fortification	0.146	-0.175	-0.021	0.122	0.003	-0.017	0.112	-0.013	-0.0004	-0.058	-0.056	-0.002	0.007
	(0.105)	(0.148)	(0.047)	(0.175)	(0.005)	(0.020)	(0.181)	(0.014)	(0.0548)	(0.051)	(0.036)	(0.038)	(0.039)
District FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-Specific Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Constant	-1.769	6.017	0.071	0.771†	-0.398**	0.006	0.299	-0.181**	5.3010**	0.577	-0.326	-0.124	0.592*
	(1.326)	(4.580)	(1.019)	(0.447)	(0.065)	(0.083)	(0.271)	(0.057)	(1.0635)	(0.791)	(0.253)	(0.188)	(0.234)
Observations	2,109	2,220	2,220	5,508	5,508	5,032	5,508	5,508	5,508	5,508	4,080	4,216	5,508
R^2	0.743	0.724	0.214	0.356	1.000	0.376	0.133	0.647	0.8655	0.157	0.541	0.906	0.755
Log-Likelihood	-1050	-330.5	-2346	-7548	15085	573.3	-8134	9733	-349.9	-4482	2616	3587	321.6
AIC	2106	666.9	4698	15102	-30165	-1140	16273	-19460	705.7	8969	-5226	-7167	-637.2

Note: Robust standard errors clustered by district are in parentheses. Columns 1-4 and 6-13 include a control for population. Column 5 includes a control for price-weighted oil reserves. All models subset to the sample of districts in border governorates. Outcomes are z-standardized for interpretability. †$p<0.1$, * $p<0.05$; ** $p<0.01$.

B.7 Border Fortification and Counter-Indirect Fire Systems

It is difficult to gather data on all possible policy shifts in fortified districts. One particularly acute concern is that districts with border forts could have been more likely to receive deployments of counter-battery (CB) radar and counter-rocket/artillery/mortar (C-RAM) systems. These systems were an integral part of U.S. force protection in Iraq, and were designed to provide warning (and potentially neutralize) incoming indirect fires. If border fortification affected CB/C-RAM deployments, effects on indirect fires could owe to these changes, rather than border control-induced insurgent tactical shifts. Data on the dates and locations of CB/C-RAM deployments are unavailable due to classification. Fortunately, qualitative evidence suggests border fortification did not affect CB/C-RAM deployments. Instead, CB/C-RAM systems were deployed at forward operating bases (FOBs) in all Multi-National Division (MND) commands. FOB locations, in turn, were determined by a variety of logistical constraints unrelated to border control efforts (Multi-National Corps–Iraq, 2007*d*).

C Robustness of Main Results

C.1 Information-Sharing and Tactical Substitution

The effect of border fortification on tactical substitution could owe to an information-sharing mechanism, whereby counterinsurgent pressure leads insurgents to prefer attacks resistant to civilian informing. To assess this possibility, I repeat the core models with per capita suicide attacks as the outcome. Suicide attacks are planned under high secrecy by motivated militants, making them resistant to exposure. The information-sharing mechanism would expect border fortification to increase suicide attacks. On the other hand, the resource mechanism predicts null effects of border fortification on suicide attacks because such attacks were cheap (Hoffman, 2003), but relied on an important external resource, foreign fighters, to conduct (Multi-National Corps–Iraq, 2005).

Table C-1: Border Fortification and Suicide Attacks

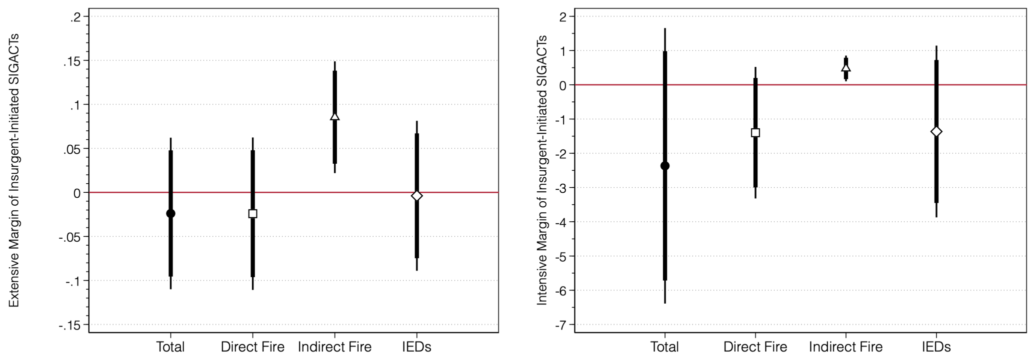
VARIABLES	(1) Suicide Attacks/ Capita	(2) Suicide Attacks/ Capita	(3) Suicide Attacks/ Capita	(4) Suicide Attacks/ Capita	(5) Suicide Attacks/ Capita	(6) Suicide Attacks/ Capita	(7) Suicide Attacks/ Capita	(8) Suicide Attacks/ Capita	(9) Suicide Attacks/ Capita	(10) Suicide Attacks/ Capita
Border Fortification	-0.031	-0.010	0.008	0.007	0.006	-0.002	0.006	0.010	-0.010	-0.003
	(0.025)	(0.016)	(0.020)	(0.019)	(0.017)	(0.026)	(0.048)	(0.030)	(0.024)	(0.020)
District FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-Specific Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Sunni x Year FE		Y	Y	Y	Y	Y	Y	Y	Y	Y
Political/Socioeconomic Controls		Y	Y	Y	Y	Y	Y	Y	Y	Y
Security Controls			Y	Y	Y	Y	Y	Y	Y	Y
Spatial Lag				Y	Y	Y	Y	Y	Y	Y
Lagged DV					Y	Y	Y	Y	Y	Y
District-Specific Linear Trend						Y	Y	Y	Y	Y
Sample Includes Districts in:	Border Governorates	Border Governorates	Border Governorates	Border Governorates	Border Governorates	Border Governorates	AQI Areas	Rejectionist Areas	All but Baghdad	All of Iraq
Constant	0.059**	0.328	0.841	0.734	0.716	1.648	3.610†	1.875	0.283	0.212
	(0.009)	(0.488)	(1.042)	(1.013)	(0.931)	(1.589)	(1.914)	(1.609)	(1.402)	(1.331)
Observations	4,148	3,788	2,109	2,109	2,109	2,109	1,767	2,166	3,078	3,591
R^2	0.134	0.176	0.215	0.219	0.224	0.246	0.242	0.237	0.226	0.219
Log-Likelihood	-744	-752.1	-681.3	-675.9	-668.9	-638.1	-754.1	-721.4	-580.3	-535.3
AIC	1492	1522	1405	1396	1384	1322	1554	1489	1207	1117

Note: Robust standard errors clustered by district are in parentheses. Controls are described in Table 1. The mean of suicide attacks is 0.001, with a standard deviation of 0.004. †p<0.1, * p<0.05; ** p<0.01.

C.2 Border Fortification and Overall Insurgent Violence

There is no reason to suspect the decline in insurgent violence reflects a decline in insurgents' ability to produce violence in general. Repeating the core specifications from column 5 of Table 1, I find no distinguishable effects on the extensive or intensive margins of insurgent violence.

Figure C-1: Fortification Did Not Reduce Violence Overall



Note: Bars are 90 and 95% confidence intervals. Dashed lines mark 0. Specifications follow Table 1.

C.3 Civilian Victimization and Sectarian Geography

Table 3 studies the effect of border fortification on insurgent civilian victimization per 1000 of district population. Measuing civilian victimization in civil war is difficult, and we may be concerned about measurement error. To assuage concerns, I study the extensive margin of one-sided violence and find similar results.

Table C-2: Fortification and the Extensive Margin of Victimization

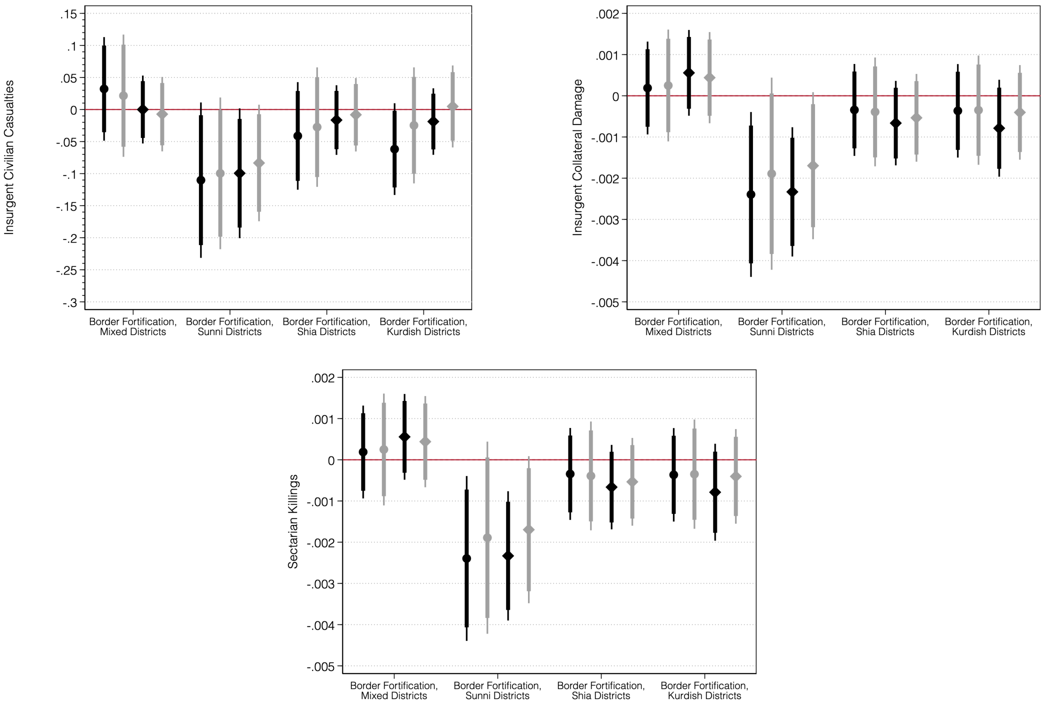
VARIABLES	(1) Insurgent Civilian Casualties	(2) Insurgent Collateral Damage	(3) Sectarian Killings	(4) Insurgent Civilian Casualties	(5) Insurgent Collateral Damage	(6) Sectarian Killings
Border Fortification x In-Group				-0.483**	-0.227**	-0.314**
				(0.107)	(0.032)	(0.064)
Border Fortification	0.011	-0.022	-0.048	0.453**	0.185**	0.237**
	(0.041)	(0.028)	(0.045)	(0.111)	(0.041)	(0.071)
District FE	Y	Y	Y	Y	Y	Y
Year-Specific Month FE	Y	Y	Y	Y	Y	Y
Sunni x Year FE	Y	Y	Y	Y	Y	Y
Political/Socioeconomic Controls	Y	Y	Y	Y	Y	Y
Security Controls	Y	Y	Y	Y	Y	Y
Spatial Lag	Y	Y	Y	Y	Y	Y
Lagged DV	Y	Y	Y	Y	Y	Y
Constant	-0.565	-0.578	0.085	0.152	-0.222	0.560
	(1.461)	(0.783)	(1.265)	(1.272)	(0.645)	(1.202)
Observations	2,109	2,109	2,109	2,109	2,109	2,109
R^2	0.564	0.444	0.533	0.571	0.447	0.536
Log-Likelihood	-551.4	-276.2	-543.6	-533.1	-270.9	-535.8
AIC	1149	598.4	1133	1114	589.8	1120

Note: Robust, district-clustered standard errors are in parentheses. The sample includes all districts in border governorates. In-group is an indicator for homogeneous sectarian districts–the constituent term is absorbed by district fixed effects. Controls are described in Table 3. Outcomes are the extensive margin of the designated header variable. †p<0.1, * p<0.05; ** p<0.01.

In Figure C-2 I further disaggregate the effect of border fortification across sectarian areas. Taking the core specifications, I interact border fortification with separate indicators for Sunni, Shi'a, and Kurdish districts. To verify the robustness of the results to the operationalization of district-level ethnicity, I take two strategies. First, as in the main text, I define districts using results of the 2005 Iraqi parliamentary election. Second, I define districts using ethnic maps and fine-grained population data from LandScan. Results show that the negative effect of border fortification on civilian victimization is largest in Sunni districts for all outcomes—insurgent civilian casualties, insurgent collateral damage, and sectarian killings. Effects are less precise in Kurdish and Shi'a districts. In mixed sectarian districts, effects are positive but imprecise.

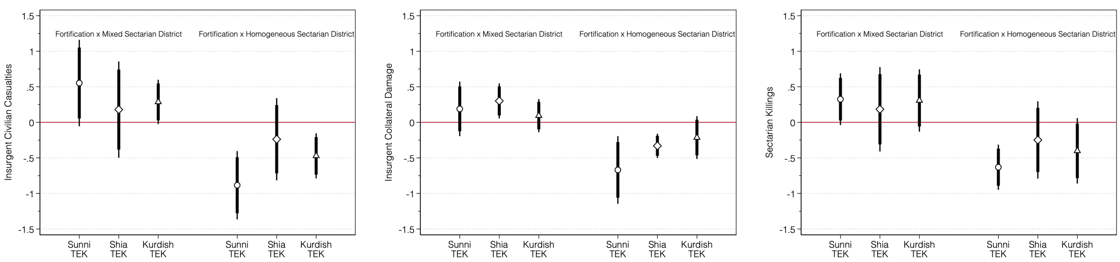
In a final set of models in Figure C-3, I also consider heterogeneity in relation to cross-border ethnic linkages. Using a shapefile from the GeoEPR dataset (Vogt et al., 2015), I overlay polygons corresponding to settlement areas of transnational ethnic kin (TEK) groups that reside in Iraq and neighboring countries: Sunni tribes, Shi'a tribes, and Kurds. Each of these groups' settlement areas extend across international borders. I re-estimate the core victimization models in sub-samples of Iraqi districts within these different (Sunni, Shi'a, and Kurdish) transborder settlement polygons. As above, the main effects are most pronounced in districts linked to cross-border Sunni TEK.

Figure C-2: Sectarian Heterogeneity and Insurgent Civilian Victimization



Note: Thick and thin bars are 90 and 95% confidence intervals based on robust, district-clustered standard errors. Controls are described in Table 3. To avoid dropping Kurdish regions of northern Iraq due to covariate missingness, models omit controls for cell towers and Coalition maneuver battalions, though this choice is not consequential for the results. Circles denote estimates from the sample of districts in border governorates. Diamonds denote estimates from the sample of all Iraqi districts. Black markers denote estimates that define ethnic composition based on voteshare. Gray markers denote estimates that define ethnic composition based on LandScan population data.

Figure C-3: Transnational Ethnic Kinship and Insurgent Civilian Victimization



Note: Thick and thin bars are 90 and 95% confidence intervals based on robust, district-clustered standard errors. Controls are described in Table 3. Sunni, Shi'a, and Kurdish TEK denote sub-samples of districts overlapped by the EPR settlement polygons for Sunni tribes, Shi'a tribes, and Kurds respectively.

C.4 Robustness of Civilian Victimization Results

Dependent variables vary across panels: insurgent civilian casualties (A), insurgent collateral damage (B), and sectarian killings (C). Columns 1 and 2 cluster standard errors by governorate. Columns 3 and 4 cluster standard errors by Directorate of Border Enforcement (DBE) region. Columns 5 and 6 scale estimates using population weights. Columns 7 and 8 scale estimates using violence weights. Columns 9 and 10 add controls for the total number of border forts and per capita spending on non-fort border security projects. Columns 11 and 12 use a Poisson estimator and count outcomes.

Table C-3: Robustness of Civilian Victimization Results

Panel A	DV: Insurgent Civilian Casualties/1000 Pop.										DV: Insurgent Civilian Casualties	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Border Fortification x In-Group		-0.531*		-0.531†		-0.472**		-0.216		-0.461*		-1.301†
		(0.204)		(0.201)		(0.141)		(0.289)		(0.185)		(0.727)
Border Fortification	-0.044	0.439	-0.044	0.439	-0.006	0.419*	-0.266†	-0.098	-0.081	0.345†	0.435	1.102
	(0.056)	(0.269)	(0.055)	(0.261)	(0.073)	(0.183)	(0.152)	(0.340)	(0.053)	(0.193)	(0.332)	(0.679)
Constant	1.190	2.085†	1.190	2.085†	2.272	3.209	8.334	9.376	2.503	3.116	12.870	15.972
	(0.803)	(1.036)	(0.760)	(0.868)	(2.222)	(1.951)	(19.704)	(18.964)	(2.006)	(1.851)	(28.415)	(26.984)
Observations	2,109	2,109	2,109	2,109	2,109	2,109	1,312	1,312	2,109	2,109	1,881	1,881
Log-Likelihood	-2097	-2092	-2097	-2092	-1479	-1475	-1865	-1865	-2090	-2086	-13508	-13435
AIC	4240	4232	4240	4232	3004	2997	3776	3777	4230	4224	27062	26918
Panel B	DV: Insurgent Collateral Damage/1000 Pop.										DV: Insurgent Collateral Damage	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Border Fortification x In-Group		-0.398**		-0.398*		-0.327**		-0.492†		-0.415**		-1.814**
		(0.100)		(0.105)		(0.092)		(0.263)		(0.066)		(0.529)
Border Fortification	-0.099†	0.265†	-0.099	0.265†	-0.024	0.271*	-0.264†	0.118	-0.075	0.309**	-0.229	0.932†
	(0.049)	(0.122)	(0.046)	(0.103)	(0.057)	(0.115)	(0.138)	(0.330)	(0.067)	(0.078)	(0.372)	(0.541)
Constant	-0.017	0.622	-0.017	0.622	0.711	1.328	37.979	40.313	-0.217	0.297	-19.488	-5.368
	(0.540)	(0.855)	(0.505)	(1.189)	(1.825)	(1.662)	(26.837)	(26.236)	(1.433)	(1.280)	(45.391)	(39.962)
Observations	2,109	2,109	2,109	2,109	2,109	2,109	1,312	1,312	2,109	2,109	1,596	1,596
Log-Likelihood	-1990	-1987	-1990	-1987	-1633	-1631	-2137	-2136	-1985	-1981	-938.1	-934.3
AIC	4026	4022	4026	4022	3312	3310	4319	4320	4020	4015	1922	1917
Panel C	DV: Sectarian Killings/1000 Pop.										DV: Sectarian Killings	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Border Fortification x In-Group		-0.265*		-0.265†		-0.321*		-0.884		-0.284*		-1.604**
		(0.097)		(0.108)		(0.136)		(0.524)		(0.107)		(0.501)
Border Fortification	-0.052	0.189	-0.052	0.189	-0.082	0.206	0.033	0.723	-0.043	0.218†	-0.143	0.742*
	(0.027)	(0.124)	(0.023)	(0.131)	(0.071)	(0.147)	(0.183)	(0.601)	(0.059)	(0.113)	(0.255)	(0.320)
Constant	2.959	3.438	2.959	3.438	5.326	5.982	80.207†	84.662†	2.736	3.152	14.350	25.944
	(2.303)	(2.724)	(2.442)	(2.927)	(4.314)	(4.334)	(45.701)	(45.591)	(2.336)	(2.313)	(24.231)	(24.205)
Observations	2,109	2,109	2,109	2,109	2,109	2,109	1,312	1,312	2,109	2,109	1,881	1,881
Log-Likelihood	-2457	-2456	-2457	-2456	-2303	-2302	-2597	-2596	-2457	-2456	-1740	-1733
AIC	4961	4961	4961	4961	4653	4653	5239	5239	4964	4964	3527	3513
District FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year-Specific Month FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Sunni x Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Political/Socioeconomic Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Security Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Spatial Lag	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Lagged DV	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Governorate Clustered SEs	Y	Y										
DBE Region Clustered SEs			Y	Y								
Population Weights					Y	Y						
Violence Weights							Y	Y				
Additional Border Controls									Y	Y		
Poisson											Y	Y

Note: Robust, district-clustered standard errors are in parentheses unless otherwise noted. The sample includes all districts in border governorates. In-group is an indicator for homogeneous sectarian districts—the constituent term is absorbed by district fixed effects. Controls are described in Table 1. Outcomes are z-standardized. †$p<0.1$, * $p<0.05$; ** $p<0.01$.

D Empirical Extensions

D.1 Border Fortification and Insurgent Spending

Bahney et al. (2010) describe financial records captured by U.S. forces from al-Qaeda in Iraq (AQI). One subset of the data detail revenues and expenditures of AQI in Anbar governorate. The data record transfers from the Anbar provincial administration to local AQI sectors in the province. If border control efforts increase the price insurgents pay for accessing external resources, border forts should be positively correlated with local requirements for funding. Data described in Bahney et al. (2010) were recovered from figures in the manuscript using digital extraction software because the authors no longer have access to replication materials.

Consistent with a border fortification-induced price effect, local AQI spending is increasing in border fortification. Because controls are included for Coalition maneuver battalions and per capita CERP spending in sectors, we can rule out that the effect of border fortification owes solely to increased AQI spending in response to greater counterinsurgent deployments. It is also unlikely that increased spending is solely geared at compensating fighters for increased local operations against the Coalition because compensation in AQI was not based on risk (Bahney et al., 2013), and because border control spurred insurgents to engage in fewer high-risk direct fire attacks and more low-risk indirect fire attacks.

Table D-1: Border Fortification and Provincial AQI Transfers to Local Sectors

VARIABLES	(1) Sector Transfers/1000 Pop.	(2) Sector Transfers/1000 Pop.	(3) Sector Transfers/1000 Pop.	(4) Sector Sector Transfers/1000 Pop.
Number of Border Forts	0.056**	0.137*	0.133*	0.226**
	(0.007)	(0.037)	(0.043)	(0.029)
Sector FE	Y	Y	Y	Y
Year-Specific Month FE	Y	Y	Y	Y
Covariates		Y	Y	Y
Lagged DV			Y	Y
Sector-Specific Linear Trend				Y
Constant	-0.308**	47.537†	46.088*	48.475†
	(0.032)	(19.789)	(15.194)	(21.833)
Observations	80	80	80	80
R^2	0.484	0.623	0.624	0.671
Log-Likelihood	-76.64	-64.07	-63.92	-58.60
AIC	157.3	160.1	161.8	151.2

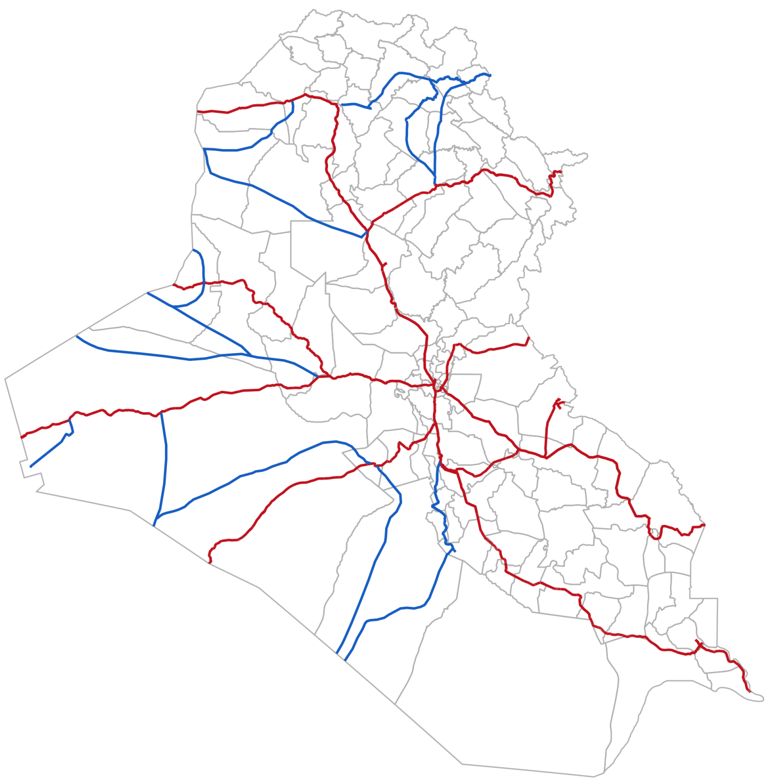
Note: Robust standard errors clustered by AQI sector are in parentheses. The sample includes al-Qaeda in Iraq (AQI) sectors in Anbar governorate. Covariates are political/socioeconomic and security controls described in Table 1. Sector transfers per 1000 refer to the amount of funds the AQI provincial administration transfered to sector commanders in a given sector-month, normalized by sector population in thousands. Outcomes are z-standardized. †p<0.1, * p<0.05; ** p<0.01.

D.2 Insurgent Smuggling Networks

Using a declassified document created by Multi-National Corps Iraq (MNC-I) Headquarters in 2005 and provided by U.S. Central Command (Multi-National Corps–Iraq, 2005), I geotraced primary and secondary insurgent ratlines, or smuggling routes. Whereas some primary ratlines follow the Iraqi highway network, secondary ratlines do not typically follow existing paved roads, but rather denote historical smuggling trails and informal paths. The Iraqi road network overlaid on the map comes from the United Nations Office for the Coordination of Humanitarian Affairs (OHCA) in collaboration with the U.S. National Imagery Mapping Agency (NIMA), and reflects roads designated by OHCA as "primary routes" as of January 2003, three months prior to the U.S. invasion of Iraq.

Insurgent smuggling through districts otherwise unaffected by counterinsurgent border control could cause conflict spillovers if insurgents respond to border fortification by shifting patterns of violence along smuggling routes. Military sources indicate this occurred: "[c]ontrol and secure the border anywhere and smugglers, criminals, AQI, FF [foreign fighters] will detour to one of many other border crossing locations" (Multi-National Corps–Iraq, 2007*c*). I control for spillovers in the main analyses using spatial lags. As an additional test, I show that, consistent with Getmansky, Grossman and Wright (2019) and Laughlin (2019), access to alternate smuggling routes relaxed insurgents' tactical adaptations to border fortification.

Figure D-1: Geotraced Insurgent Ratlines



Note: Primary ratlines are marked in red and secondary ratlines are marked in blue. Dark gray lines mark sections of the Iraqi road network not used as primary or secondary trafficking routes.

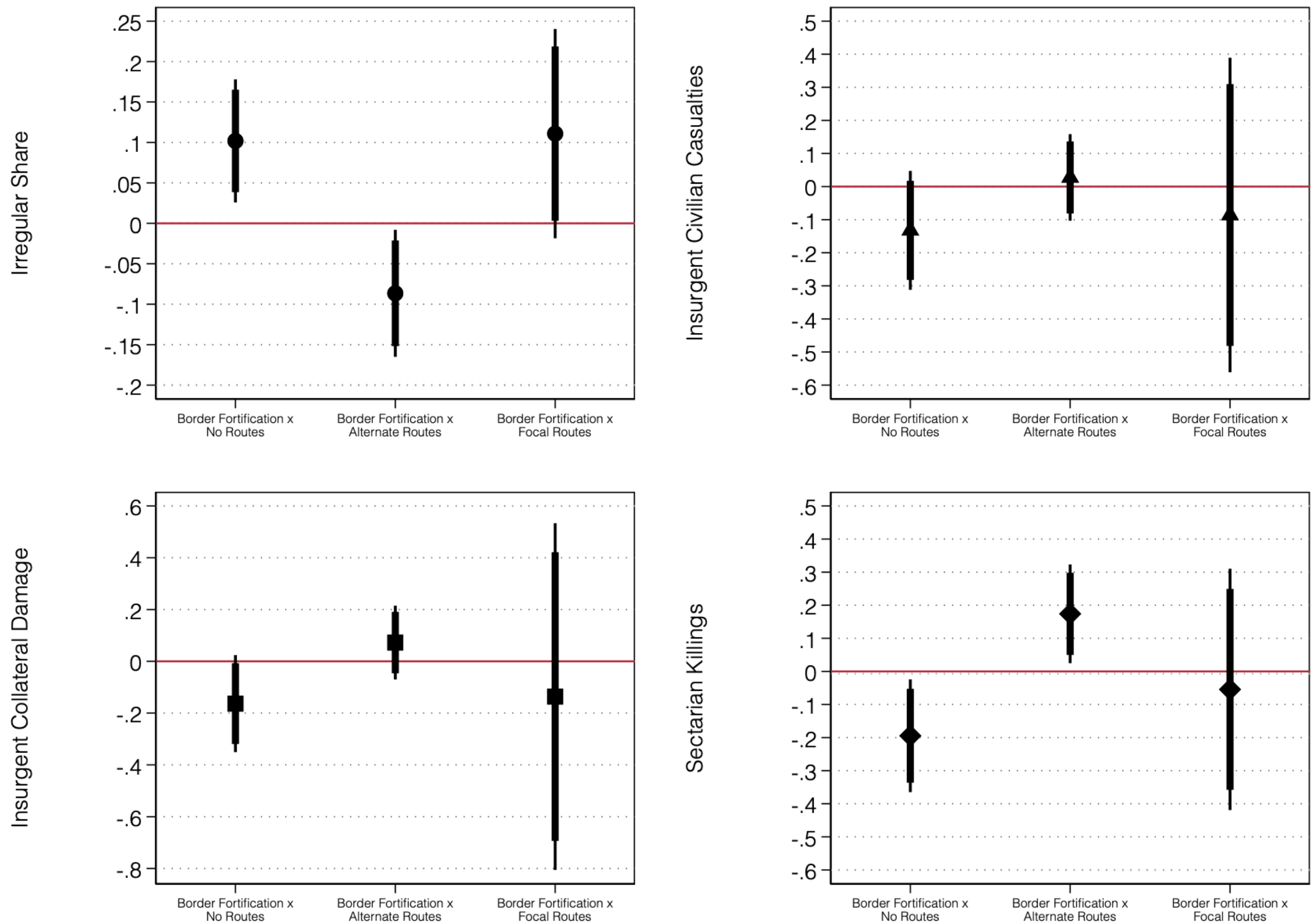
Laughlin (2019) shows that US border control efforts raised the value of trafficking routes in un-walled sections of the US-Mexico border, increasing violence in those areas as cartels competed for control of cross-border routes. At the same time. Chambers et al. (2021) show that border enforcement on the US-Mexico border induced a "funnel effect," forcing migrants to take longer and more dangerous cross-border routes. Getmansky, Grossman and Wright (2019) show that in response to the Israel-Palestine border wall, criminal gangs increased car thefts in non-fortified areas, while those whose smuggling routes were interdicted shifted into criminal activities that did not rely on cross-border smuggling. These analyses imply that the effect of border fortification on insurgent tactics should be conditioned by insurgent access to trafficking routes.

In districts where insurgents lack convenient and well-established ratlines for cross-border trafficking, border fortification should increase the proportion of insurgent attacks that are irregular (H1) and reduce insurgent civilian victimization (H2, H3), as insurgents have no other convenient means of recouping external resource losses. In districts with a high-density of primary and secondary routes (i.e., focal routes), insurgents have some means of subverting border control by leveraging smuggling routes, but counterinsurgent pressure is also greatest, as surveillance assets associated with border fortification intensely monitored high-density trafficking nodes (Williams, 2007, 521). Relative to districts with less-trafficked, alternate routes only, high-density trafficking nodes in focal districts were significantly more likely to be classified by US forces as "controlled" by August 2007 (Multi-National Corps–Iraq, 2007*a*). In focal districts, then, border fortification should have a weak or insignificant effect on insurgent tactics, since insurgents can subvert border fortification, but face higher costs to doing so owing to greater counterinsurgent attention. Finally, in districts with low-density, alternate smuggling routes, where insurgents can subvert border fortification by shifting trafficking to less heavily surveilled and harder-to-interdict routes, border fortification does not affect insurgents' foreign logistics, as alternate routes provide a means of sustaining foreign support. In these areas, insurgents retain resources and have to cultivate less local civilian support, meaning they can continue to produce conventional violence and victimize civilians.

I test these expectations in Figure D-2. I cannot calculate optimal smuggling routes and trafficking equilibria a la Dell (2015) because most secondary ratlines do not follow defined roads, but rather use unpaved and historical paths and shepherds' trails. Instead, I repeat the main analyses while interacting border fortification with indicators for the status of district smuggling routes. These regressions reveal support for the expectations outlined above. The hypothesized effects—increasing irregular attacks and reduced civilian victimization—consistently emerge in fortified districts without smuggling routes. Fortification in districts without ratlines significantly increases the proportion of attacks that are irregular ($p = 0.010$), and reduces the number of sectarian killings ($p = 0.026$) and insurgent collateral damage ($p = 0.086$). The reduction in insurgent civilian casualties is nearly statistically significant ($p = 0.144$).

Opposite effects emerge in districts with alternate routes, where insurgents could subvert border controls by leveraging cross-border trafficking networks. Fortification in these significantly reduces the proportion of attacks that are irregular ($p = 0.032$), and increases the number of sectarian killings ($p = 0.023$). Effects on insurgent civilian casualties and insurgent collateral damage are imprecisely estimated but consistently positively signed. In comparison, focal smuggling districts with a high-density of routes but expansive counterinsurgent monitoring see generally insignificant effects. Here, however, insurgents do still shift toward irregular attacks ($p = 0.091$).

Figure D-2: Heterogeneity in the Effect of Border Control Along Smuggling Routes



Note: Bars are 90 and 95% confidence intervals based on robust, district-clustered standard errors. Models include district, year-specific month, and Sunni voteshare-by-year fixed effects, political/socioeconomic and security controls, spatial lags, a lagged dependent variable, and district-specific linear trends. Controls are described in Table 1. No routes denote districts without insurgent ratlines. Alternate routes denote districts with primary or secondary ratlines but not both. Focal routes denote districts known to be high-density trafficking nodes, with both primary and secondary ratlines. These areas were a focus of US counterinsurgent surveillance. Victimization outcomes are z-standardized.

D.3 Border Fortification and Foreign Subversion

Iran engaged in extensive subversion of U.S. border enforcement. For instance, Iranian forces were coordinating smuggling into Iraq via bribery (Multi-National Division–Central, 2007). US troops also engaged in several direct clashes with Iranian special forces in Diyala in 2006-2007 (Combined Joint Special Operations Task Force–Arabian Peninsula, 2007). Consistent with these accounts, effects are attenuated in districts near Iran and influenced by Jaish al-Mahdi (JAM), the main Iranian proxy.

Table D-2: Iranian Sponsorship Subverted the Efficacy of Border Fortification

VARIABLES	(1) Irregular Share	(2) Insurgent Civilian Casualties	(3) Insurgent Collateral Damage	(4) Sectarian Killings
Border Fortification x Iran	-0.054†	0.297*	0.299*	0.256*
	(0.031)	(0.128)	(0.132)	(0.105)
Border Fortification	0.107**	-0.258†	-0.315*	-0.237*
	(0.031)	(0.140)	(0.143)	(0.114)
District FE	Y	Y	Y	Y
Year-Specific Month FE	Y	Y	Y	Y
Sunni x Year FE	Y	Y	Y	Y
Politica/Socioeconomic Controls	Y	Y	Y	Y
Security Controls	Y	Y	Y	Y
Spatial Lag	Y	Y	Y	Y
Lagged DV	Y	Y	Y	Y
Constant	1.590†	0.206	-1.041	2.258
	(0.807)	(2.697)	(1.569)	(2.539)
Observations	2,109	2,109	2,109	2,109
R^2	0.228	0.497	0.488	0.667
Log-Likelihood	1041	-2094	-1987	-2456
AIC	-2035	4237	4022	4960

Note: Robust, district-clustered standard errors are in parentheses. Iran is an indicator for districts in governorates contiguous to Iran. Controls are described in the notes for Table 1. Victimization outcomes are z-standardized. †p<0.1, * p<0.05; ** p<0.01.

Unlike Iran, other neighboring states around Iraq—Syria, Saudi Arabia, and Jordan—offered tacit/covert support to militants, allowing some logistical activities (e.g., fundraising, smuggling, recruiting). However, these tacit sponsors did not actively interfere with US and Iraqi border interdiction efforts (Combined Joint Special Operations Task Force–Arabian Peninsula, 2007; Malkasian, 2017). I examine the efficacy of fortification against tacit sponsors in Table D-3. Across specifications, marginal effects of fortification on the irregular share and civilian victimization are generally distinguishable and precise. Effects are particularly pronounced for areas near Saudi Arabia.

Table D-3: Fortification Was Effective Against Tacit Sponsorship

	DV: Irregular Share				DV: Insurgent Civilian Casualties				DV: Insurgent Collateral Damage				DV: Sectarian Killings		
VARIABLES	(1) Syria	(2) Saudi Arabia	(3) Jordan		(4) Syria	(5) Saudi Arabia	(6) Jordan		(7) Syria	(8) Saudi Arabia	(9) Jordan		(10) Syria	(11) Saudi Arabia	(12) Jordan
Border Fortification x Neighbor	0.046	0.054†	0.046	-0.221		-0.297*	-0.221	-0.435*	-0.299*		-0.435*	-0.248†	-0.256*	-0.248†	
	(0.036)	(0.031)	(0.036)	(0.182)		(0.128)	(0.182)	(0.181)	(0.132)		(0.181)	(0.136)	(0.105)	(0.136)	
Border Fortification	0.059*	0.052*	0.059*	-0.002		0.038	-0.002	-0.017	-0.016		-0.017	-0.005	0.019	-0.005	
	(0.022)	(0.020)	(0.022)	(0.071)		(0.073)	(0.071)	(0.058)	(0.061)		(0.058)	(0.060)	(0.056)	(0.060)	
District FE	Y	Y	Y		Y	Y	Y		Y	Y	Y		Y	Y	Y
Year-Specific Month FE	Y	Y	Y		Y	Y	Y		Y	Y	Y		Y	Y	Y
Sunni x Year FE	Y	Y	Y		Y	Y	Y		Y	Y	Y		Y	Y	Y
Political/Socioeconomic Controls	Y	Y	Y		Y	Y	Y		Y	Y	Y		Y	Y	Y
Security Controls	Y	Y	Y		Y	Y	Y		Y	Y	Y		Y	Y	Y
Spatial Lag	Y	Y	Y		Y	Y	Y		Y	Y	Y		Y	Y	Y
Lagged DV	Y	Y	Y		Y	Y	Y		Y	Y	Y		Y	Y	Y
Constant	1.379†	1.590†	1.379†	1.329		0.206	1.329	0.183	-1.041		0.183	3.239	2.258	3.239	
	(0.794)	(0.807)	(0.794)	(2.671)		(2.697)	(2.671)	(1.360)	(1.569)		(1.360)	(2.722)	(2.539)	(2.722)	
Observations	2,109	2,109	2,109		2,109	2,109	2,109		2,109	2,109	2,109		2,109	2,109	2,109
R^2	0.227	0.228	0.227		0.496	0.497	0.496		0.488	0.488	0.488		0.667	0.667	0.667
Log-Likelihood	1040	1041	1040		-2096	-2094	-2096		-1987	-1987	-1987		-2457	-2456	-2457
AIC	-2032	-2035	-2032		4240	4237	4240		4022	4022	4022		4961	4960	4961

Note: Robust, district-clustered standard errors are in parentheses. Neighbor is an indicator for districts in governorates contiguous to the neighboring state identified in the header. Controls are described in the notes for Table 1. Victimization outcomes are z-standardized. †p<0.1, * p<0.05; ** p<0.01.

In Table D-4, I repeat the approach detailed in Tables D-2 and D-3 for Iraq's other neighbor against which fortifications were directed: Kuwait. Effects are substantively small and indistinguishable. This is unsurprising because Kuwait was neither a sponsor of Iraqi insurgents nor a conduit for safe haven.

Table D-4: Fortification Had Little Discernable Effect in Areas Near Kuwait

VARIABLES	(1) Irregular Share	(2) Insurgent Civilian Casualties	(3) Insurgent Collateral Damage	(4) Sectarian Killings
Border Fortification x Kuwait	-0.025	-0.032	0.000	0.062
	(0.023)	(0.088)	(0.057)	(0.072)
Border Fortification	0.074**	-0.035	-0.099	-0.068
	(0.023)	(0.095)	(0.087)	(0.077)
District FE	Y	Y	Y	Y
Year-Specific Month FE	Y	Y	Y	Y
Sunni x Year FE	Y	Y	Y	Y
Political/Socioeconomic Controls	Y	Y	Y	Y
Security Controls	Y	Y	Y	Y
Spatial Lag	Y	Y	Y	Y
Lagged DV	Y	Y	Y	Y
Constant	1.387	1.223	0.009	3.141
	(0.833)	(2.731)	(1.485)	(2.755)
Observations	2,109	2,109	2,109	2,109
R^2	0.227	0.496	0.487	0.667
Log-Likelihood	1040	-2097	-1990	-2457
AIC	-2032	4242	4028	4962

Note: Robust, district-clustered standard errors are in parentheses. Kuwait is an indicator for districts in governorates contiguous to Kuwait. Controls are described in the notes for Table 1. Victimization outcomes are z-standardized. †p<0.1, * p<0.05; ** p<0.01.

Kuwait is the only one of Iraq's neighbors that had walled its border in the period under study. One may wonder whether joint fortification enhanced the efficacy of border enforcement. It is difficult to determine this absent fine-grained data on Kuwaiti enforcement operations. However, qualitative evidence suggests that Kuwaiti fortification efforts actually deterred insurgents from pursuing sanctuary in Kuwait in the first place. Joint enforcement did not magnify the effect of Iraqi fortification because earlier Kuwaiti fortification reduced insurgents' incentives and ability to pursue cross-border support in Kuwait at the outset of the Iraq War (Multi-National Corps–Iraq, 2005). Still, qualitative sources highlight the general importance of cross-border cooperation, and synergies between border security programs undertaken in Iraq and by its neighbors. For instance, US officials lauded "regional engagement initiative[s] in order to stabilize border areas with neighboring countries..." (Multi-National Corps–Iraq, 2007*b*).

D.4 Border Fortification and the Intensity of Enforcement

The main models study the extensive margin of fortification, which averages over substantive, scale effects in the intensive margin of fortification. To examine these, I re-estimate the focal regressions while focusing on the number of border fortifications in a given district. The main results are generally robust, though effects on sectarian killings are modestly imprecise.

Table D-5: The Intensive Margin of Fortification and Violence

VARIABLES	(1) Irregular Share	(2) Insurgent Civilian Casualties	(3) Insurgent Collateral Damage	(4) Sectarian Killings
Number of Border Fortifications x In-Group		-0.025*	-0.031***	-0.010
		(0.014)	(0.009)	(0.009)
Number of Border Fortifications	0.002†	0.039†	0.022	0.004
	(0.001)	(0.022)	(0.015)	(0.014)
District FE	Y	Y	Y	Y
Year-Specific Month FE	Y	Y	Y	Y
Sunni x Year FE	Y	Y	Y	Y
Political/Socioeconomic Controls	Y	Y	Y	Y
Security Controls	Y	Y	Y	Y
Spatial Lag	Y	Y	Y	Y
Lagged DV	Y	Y	Y	Y
Constant	1.392†	2.960	0.282	3.066
	(0.767)	(1.985)	(1.386)	(2.392)
Observations	2,109	2,109	2,109	2,109
R-squared	0.222	0.499	0.489	0.667
Log-Likelihood	1033	-2089	-1986	-2457
AIC	-2019	4227	4021	4962

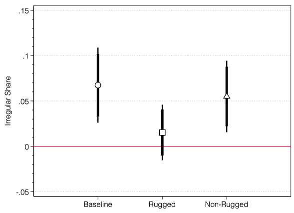
Note: Robust, district-clustered standard errors are in parentheses. The sample includes all districts in border governorates. In-group is an indicator for homogeneous sectarian districts–the constituent term is absorbed by district fixed effects. Controls are described in Table 1. Civilian victimization outcomes are z-standardized. †p<0.1, * p<0.05; ** p<0.01.

D.5 Border Fortification and Terrain Ruggedness

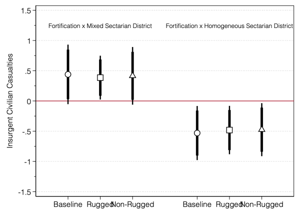
The efficacy of fortification may be conditioned by terrain (Aleprete Jr. and Hoffman, 2012; Linebarger and Braithwaite, 2020). Ruggedness also exacerbates the danger of border crossing (Chambers et al., 2021). If harsh terrain impedes the ability of militants to cross the border, it may magnify the negative effect of fortification on insurgent resources. On the other hand, if harsh terrain inhibits the ability of counterinsurgent forces to patrol the border, it may attenuate the efficacy of fortification. In Figure D-3 I explore heterogeneous effects of terrain ruggedness. Using an elevation raster, I calculate district-level ruggedness as the standard deviation of elevation. I define rugged districts as those in the top quartile of ruggedness. Re-estimating the focal regressions in rugged and non-rugged sub-samples gives little evidence of heterogeneity. The civilian victimization results are substantively similar, while the share of irregular insurgent attacks is marginally smaller in rugged districts. One likely possibility is that indirect fires are less militarily-effective in mountainous regions. For instance, distinct wind variations occur over short distances in mountains, and this effect increases with elevation, complicating targeting calculations. Terrain roughness also reduces mobility, inhibiting employment of artillery.

Figure D-3: Border Fortification and Terrain Ruggedness

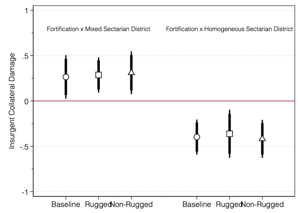
(a) Irregular Share



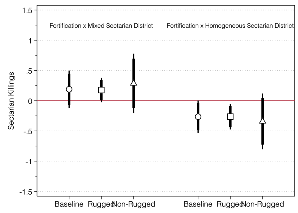
(b) Insurgent Civilian Casualties



(c) Insurgent Collateral Damage



(d) Sectarian Killings



Note: Bars are 90 and 95% confidence intervals. Each plot shows the effect of border fortification on the respective outcome. Specifications follow Table 3. Baseline models repeat the core estimates for reference. Rugged and non-rugged denote sub-samples of rugged and non-rugged districts respectively. Victimization outcomes are z-standardized. The red line marks 0.

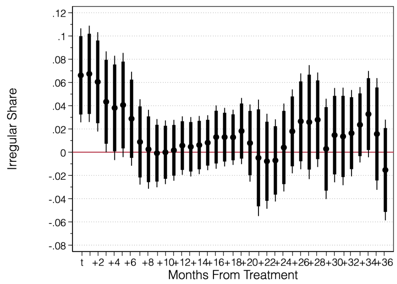
D.6 Temporal Dynamism in the Effect of Border Fortification

In Figure D-4 I re-estimate the focal equation with successively longer leads of outcomes vis-á-vis treatment. Formally, $\forall\ n\ \big(0, 1, 2, ...36\big)$ I estimate

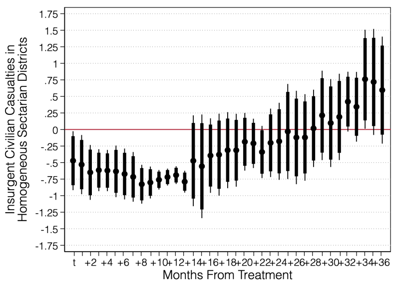
$$Y_{j,t+n} = \alpha_j + \beta_t + \delta(\text{BorderFort}_{j,t}) + \gamma_{X_{j,t}} + \epsilon_{j,t}$$

Figure D-4: Temporal Dynamism in the Effect of Border Fortification

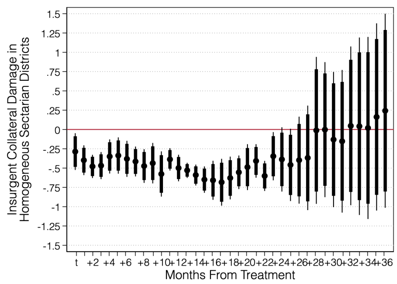
(**a**) Irregular Share



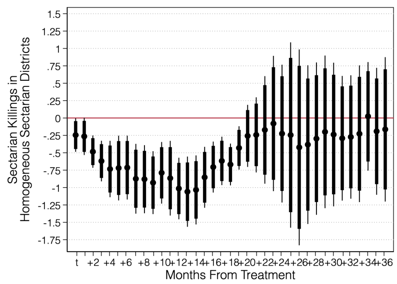
(**b**) Insurgent Civilian Casualties



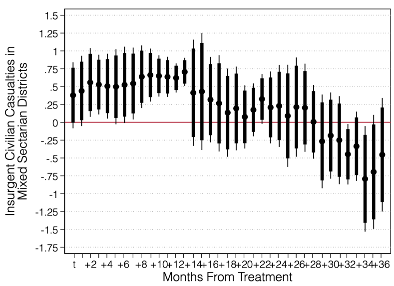
(**c**) Insurgent Collateral Damage



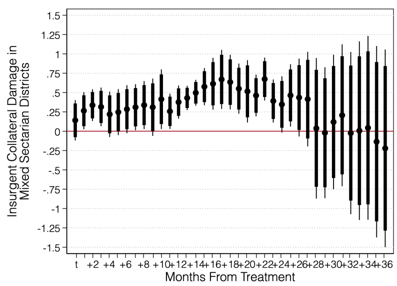
(**d**) Sectarian Killings



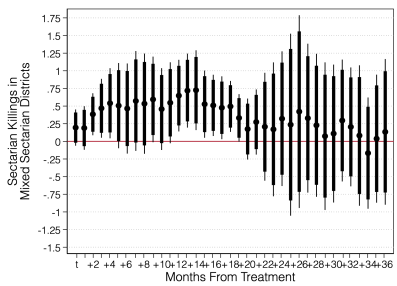
(**e**) Insurgent Civilian Casualties



(**f**) Insurgent Collateral Damage



(**g**) Sectarian Killings

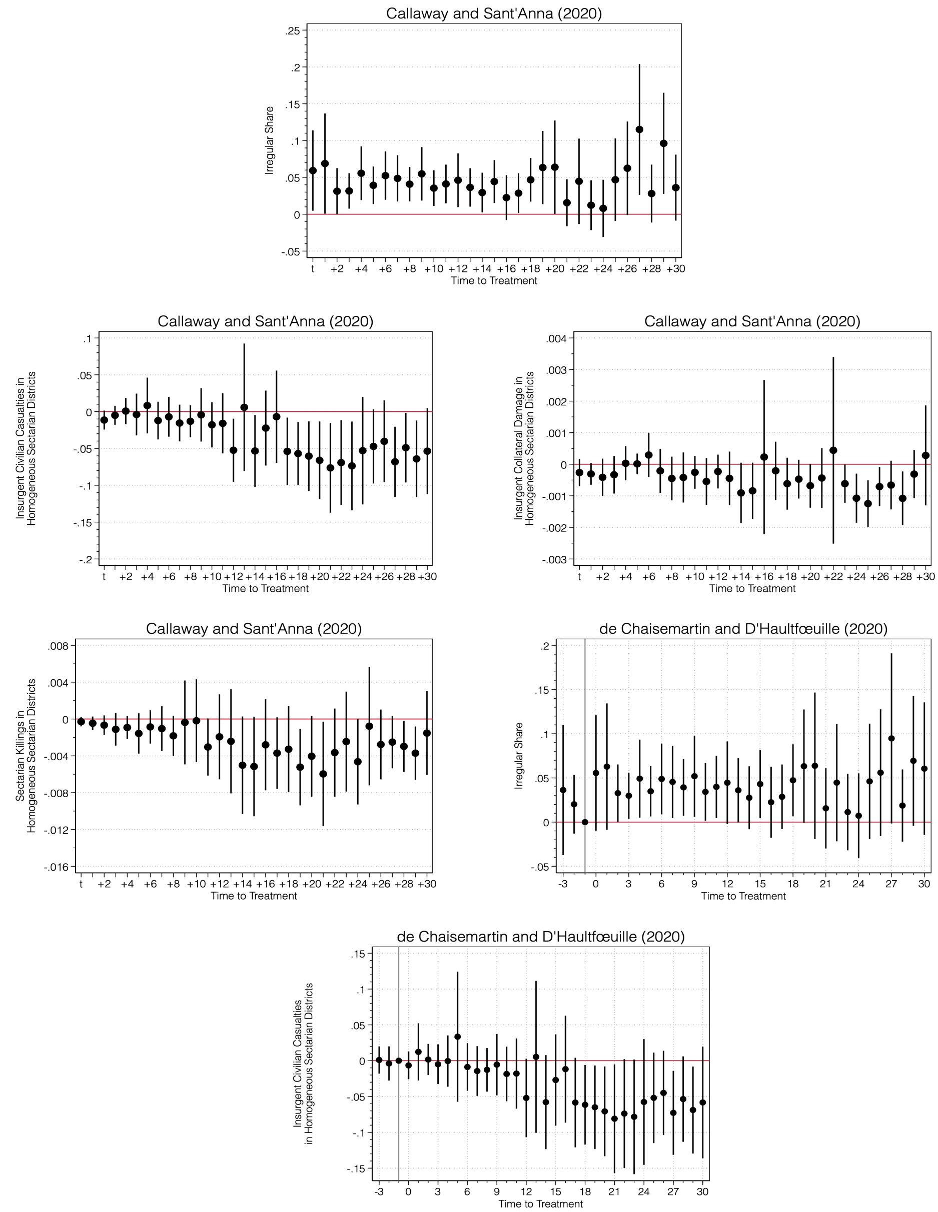


Note: Bars are 90 and 95% confidence intervals based on robust, district-clustered standard errors. Plots show coefficients from regressions of progressively longer leads of the respective outcome on border fortification. I study all periods from treatment onset t to 36 months after treatment $t + 36$. Specifications follow Table 3. Victimization outcomes are z-standardized. The red line marks 0.

D.7 Alternative Difference-in-Differences Estimators

Two-way fixed effects estimators give a variance-weighted average treatment effect. When already-treated units act as controls, changes in treatment effects over time may bias the overall effect estimate (Goodman-Bacon, 2021). New classes of estimators introduced in Callaway and Sant'Anna (2021) and de Chaisemartin and D'Haultfoeuille (2020) address issues with the two-way fixed effects estimator. Callaway and Sant'Anna (2021) propose a method to calculate group-time average treatment effects, which represent the average treatment effect for group g at time t, where a "group" is defined by the time period when units are first treated. de Chaisemartin and D'Haultfoeuille (2020) propose an estimator that calculates the average treatment effect across all the group-time cells whose treatment changes from $t-1$ to t. Results using these alternative estimators are substantively similar.

Figure D-5: Alternative Difference-in-Differences Estimators

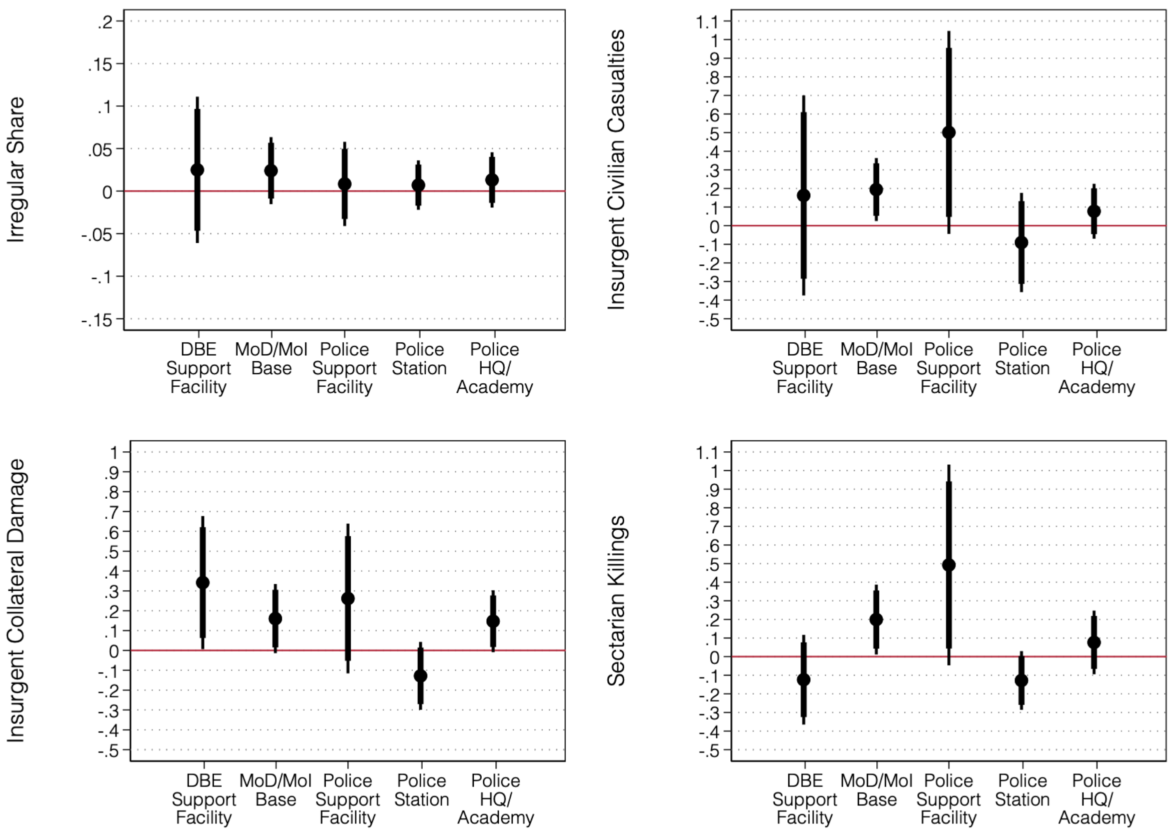


Note: Bars are 90% confidence intervals based on robust, district-clustered standard errors. Plots show coefficients from regressions of violence on border fortification. Estimates of civilian victimization come from models interacting border fortification with an indicator for homogeneous districts.

D.8 Placebo Tests with Non-Fort Security Infrastructure

I anticipate that border fortification affects insurgent tactics by interdicting insurgents' transnational resources. If this is the case, non-fort security infrastructure, which does not affect insurgents' foreign logistics, should have null or more modest effects on the focal outcomes. I focus on five other infrastructure types as placebo tests: DBE wells and roads (support facilities), Ministry of Defense (MoD) and Ministry of Interior (MoI) bases, police support facilities, police stations, and police headquarters or academies. Border forts have distinct effects from non-fort security infrastructure.

Figure D-6: Placebo Tests with Non-Fort Security Infrastructure



Note: Bars are 90 and 95% confidence intervals based on robust, district-clustered standard errors. Points are difference-in-differences estimates of the effect of placebo, non-border fort security infrastructure on the focal outcomes. Specifications follow Table 1. I also include a control for border fortification, though results are substantively similar without this control. Victimization outcomes are z-standardized.

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