The Fortification Dilemma: Border Control and Rebel Violence*

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February 8, 2022

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 foreign rebel sponsorship.

*I thank Beth Simmons, Michael Horowitz, David Carter, Guy Grossman, Michael Kenwick, Melissa Lee, Sara Mitchell, Jack Paine, Evan Perkoski, Lauren Pinson, Sarah Polo, Brad Smith, Austin Wright, and participants in the 2019 Borders and Boundaries Conference, the 2020 Online Peace Science Colloquium, the 2020 American Political Science Association Conference, the 2020 Empirical Studies of Conflict (ESOC) Annual Meeting, the 2020 Households in Conflict Network (HiCN) Workshop, and the University of Pennsylvania Internal IR Workshop for helpful comments and advice. This paper was previously circulated under the titles: "Border Control and Insurgent Tactics" and "Border Fortification and Insurgent Tactics."

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1 Introduction

In June 2018, just six months after declaring the defeat of the Islamic State (ISIS), Iraqi security forces began fortifying the border with Syria, installing fences, trenches, and surveillance cameras to inhibit cross-border infiltration (Sulaivany, 2018). Iraq's aim was to deny ISIS militants the ability to shelter and re-supply from the group's bases in Syria, and thereby to resurge. This strategy, involving the use of barriers to detect and interdict transnational rebel operations, is known as counterinsurgent border fortification. In the past two decades, similar efforts aimed at disrupting cross-border militant networks have unfolded in Jordan, Kenya, Myanmar, Tunisia, Pakistan, and Ukraine, among others (Table A-1). The proliferation of counterinsurgent border fortifications is part of a broader, global trend toward border hardening (Carter and Poast, 2017; Simmons and Kenwick, 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 matériel 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 and Marine Corps, 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; Vallet, 2016), with only modest impacts on security. Alternatively, fortification may backfire. By dislocating borderland communities (Gade, 2020), fortifications can spur resentment and humiliation (Longo, Canetti and Hite-Rubin, 2014). In tandem with the disruption of cross-border markets (Getmansky, Grossman and Wright, 2019), 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; Qiu, 2022), I argue that counterinsurgent border fortification generates discrete trade-offs for combatants. By raising the price of obtaining foreign support, border control reduces transnational rebels' resources.[1] Well-supplied rebels prefer conventional operations, but as fortification 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, counterinsurgent 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 matériel, border fortification causes a composition shift in violence. Only a disaggregated analysis, which distinguishes rebel tactics and anti-government versus one-sided attacks, can detect these shifts. By impeding rebel access to arms and fighters from sanctuaries abroad, fortification reduces complex conventional attacks while incentivizing irregular, low-risk, harassing operations. Likewise, by increasing rebel reliance on local communities, fortification fosters restraint and reduced civilian victimization.

I test the theory in the context of the US-led border fortification effort during Operation Iraqi Freedom. I draw on declassified microdata from the Iraq Reconstruction Management System (IRMS) maintained by the U.S. Army Corps of Engineers' Gulf Region Division (Berman, Shapiro and Felter, 2011). These data document 73,600 reconstruction projects in Iraq, including 333 border security projects and 287 border forts.[2] Because the data track the universe of US reconstruction spending in Iraq, they offer a novel and principled way to study the evolution of

[1]Counterinsurgents need not block all foreign support to rebels so long as fortification raises the costs to rebels of accessing transnational resources, for example by pushing militants to take riskier cross-border routes or raising the fees charged by smugglers.

[2]Apart from border forts, other border security projects include efforts to improve cargo monitoring equipment at ports of entry, to build training academies for Iraqi Directorate of Border Enforcement (DBE) personnel, and to construct roads and wells for DBE forces.

counterinsurgent border fortification. For identification, I leverage spatio-temporal variation in the implementation of border fortification in a difference-in-differences setting. Plausibly exogenous bureaucratic delays and idiosyncratic reallocation of reconstruction funds meant the resources devoted to fortification were divorced from conflict trends across district-months. Rich data on fort construction timelines, conflict events, 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 conventional, direct fire operations for irregular, indirect fire attacks. This tactical shift is consistent with rebel adoption of less effective forms of combat in the face of resource losses. Yet, fortification also prompted reduced civilian victimization, implying rebel efforts to recoup resource losses through community-based mobilization. This latter effect is heightened in areas where rebel forces are co-ethnic with the civilian population, and consequently, where their efforts to cultivate civilian support are 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 border fortification caused an increase in militant spending, mostly on smugglers' fees. This spending helped insurgents build support in borderland communities whose access to informal, cross-border markets was disrupted by fortification. Data on insurgent ratlines reveal that effects are attenuated where militants retain accessible smuggling routes, which subvert 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).[3] 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 (Martínez, 2017; Stewart and Liou, 2017).[4] Ow-

[3]But see Hazen (2013).

[4]Zhukov (2017) studies the interdiction of external support, but focuses on how external resource losses affect government violence.

ing to counterinsurgent 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 often the subject of contestation in themselves.

Further, while existing research focuses on 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; Carter, 2015). Counterinsurgent border fortification represents an important means to counter transnational 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 fronts, and highlight how variation in insurgents' transnational supply networks affect their repertoires of violence. One notable result, that rebels reduce civilian victimization in the face of border fortification, suggests an important modification to extant 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 around the world, a growing literature examines the political economy of border security. To date, however, most work has focused on the *macrolevel* determinants of border control (Carter and Poast, 2017; Simmons and Kenwick, 2022). This paper builds on a burgeoning research program on the *microlevel* consequences of fortification (Getmansky, Grossman and Wright, 2019), and especially on the effects of border hardening on conflict (Laughlin, 2019; Nanes and Bachus, 2021). The evidence here suggests counterinsurgent border fortification can effectively reduce rebel capabilities. Still, in a context where basic internal security is threatened, the costs required to control international borders might be better spent on development and governance reforms (Berman, Shapiro and Felter, 2011), or securing elite bargains to sap insurgent support (Hazelton, 2021). 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.

2 Transnational Resources and Rebellion

Rebel resilience is predicated on a host of factors, including social networks (Parkinson, 2013), internal political structures (Wood, 2003), and socialization (Hoover Green, 2018). But resources are perhaps the paramount constraint because it is costly to produce violence and provide services. Both of these outputs require recruits, funds, and matériel (Taber, 1965; Weinstein, 2007; Bueno de Mesquita, 2013; Dube and Vargas, 2013). For instance, carrying out attacks requires, at minimum, fighters and arms. Service provision, likewise, requires funds to disburse and personnel to administer projects. Increasing the production of violence and governance bolsters territorial control, endogenously increasing resources (Wood, 2003). Hence, combatants have incentives to seek larger resource endowments.

To secure additional 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, 136-137). This transnational dimension of civil war has become more important over time (Hazen, 2013), as globalization enhances militants' abilities to operate across borders (Hastings, 2010).

Insurgents' desire for access to external resources induces them to seek control of territory across international borders (Idler, 2019). Transnational safe havens allow rebels to melt from the path of domestic counterinsurgency, regroup, and dictate the subsequent terms of engagement (Byman, 2005; Sinno, 2008). Recruitment, procurement, and training can all be organized with relative ease from border sanctuaries (Galula, 2006). Governing cross-border routes also provides lucrative revenue-generating opportunities, including smuggling and taxation. For cash-strapped groups, these resources can help sustain operations, even if rebels receive no direct external sponsorship. The rise of ISIS, 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 supplies and military power. If fortification raises the cost to rebels of obtaining external support, it should degrade the overall resource base rebels can marshal, and thereby weaken the rebellion. Crucially, to inflict resource losses, all border 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, or pay higher smuggling fees and bribes. Similarly, efforts that channel cross-border traffic through government-controlled ports-of-entry can deprive rebels the ability to extort this traffic, while increasing government rents.

Resource-centric models of conflict 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; Bueno de Mesquita, 2013), border 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 border fortification create a salient trade-off for counterinsurgents.

3 The Fortification Dilemma

The tactical spectrum ranges from conventional to irregular attacks (Biddle, 2021).[5] Conventional tactics entail direct, complex, high-risk attacks on government forces. Well-resourced rebels with access to external support—whether sanctuary, fighters, training grounds, funds, or matériel—can afford to produce more conventional violence (Kalyvas and Balcells, 2010; Bueno de Mesquita, 2013). Especially for militants facing powerful counterinsurgents like the US, initiating direct attacks is risky, requiring substantial resources and coordination to execute. These sorts of attacks are easiest when rebels have more resources, and particularly more external resources, like cross-border havens to which they can flee, military-grade equipment from state sponsors, and a supply of foreign fighters. Ceteris paribus, rebels prefer conventional operations, despite the greater risks

[5] Biddle (2021) shows most militants fall in the mid-range of this spectrum. He calls conventional tactics Napoleonic and irregular tactics Fabian.

involved, because these tactics are more effective for seizing territory and dealing governments decisive defeats (Biddle, 2021; Qiu, 2022). Controlling territory and capturing government arms yield further opportunities for rent extraction (Taber, 1965; Wood, 2003), so conventional tactics endogenously beget conventional tactics.

In contrast, irregular tactics are predominantly used by resource-constrained rebels seeking to avoid a forceful state response (Carter 2016; Wright 2020). Irregular tactics are cheaper to employ because they typically entail lower risk to perpetrating militants (Biddle, 2021). Unlike conventional tactics, irregular tactics are also suitable for small groups or even individuals to carry out, and can generally be executed with less planning and coordination. These operations allow rebels to harass government forces at minimal cost. A common irregular tactic in Iraq was the use of mortar and rocket attacks against US bases. Called "shoot-and-scoot" operations, these attacks saw insurgents quickly fire long-rang projectiles at counterinsurgent sites, and then flee the launch area before suppressing fire could be returned.

Resource shocks enhancing rebel capacity increase conventional attacks, while those reducing rebel capacity increase irregular attacks (Wright, 2020; Sonin and Wright, 2022). Counterinsurgent 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 needed to perpetrate conventional violence. Second, fortification attenuates access to safe havens, increasing rebels' need to avoid costly suppression.

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

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

In addition to tactical choice, resources also influence rebel behavior vis-á-vis civilians. Different endowments alter the extent to which rebels rely on civilians for extraction. Greater access to external resources reduces rebel dependence on the local civilian populace (Zhukov, 2017; Stewart and Liou, 2017; Fortna, Lotito and Rubin, 2018), 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 who struggle to embed themselves in local communities (Moore, 2019; Schram, 2019). Civilian victimization is correspondingly responsive to shifts in rebels' assets and matériel.

Shifts in rebels' resource bases also matter apart from the content of their endowments. Losses trigger predation. Following setbacks, civilian victimization is a cheap means for rebels 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; Thomas, 2014). However, predation is counterproductive in the long-term (Kalyvas, 2006). Because civilians have agency, strategies of victimization to meet local resource needs create incentives for civilians to collaborate with the government (Condra and Shapiro, 2012), exposing rebels to suppression.

These dynamics imply competing expectations about how border 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 rebel strategies are counterproductive in the long-run, what matters is how resource losses affect rebels' time horizons. If rebels are not so hard-pressed by fortification that their immediate survival is at risk, they should forgo victimization in favor of contractual bargaining with civilians, since the latter is optimal for long-term resilience in the absence of external support (Arjona, 2016). How resource losses affect time horizons is a function of the magnitude of the loss. Unlike major battlefield defeats, counterinsurgent border fortification is a more modest setback. No border controls are impermeable, and rebels will inevitably be able to retain some access to foreign support through smuggling networks. Further, while imperfect, border fortifications are durable. Fortifying rugged, peripheral regions entails significant costs, making fortification 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, namely among civilians.

Second, while interdiction of their transnational networks increases militants' *need* to culti-

[6] In 1980, Morocco sunk 40% of its annual GDP into a fortified berm along the Mauritanian frontier (Damis, 1983).

vate local civilian support, it also shifts their recruitment patterns in a way that bolsters their *capacity* to do so. Without ample, external resources, groups attract fewer income-motivated opportunists (Weinstein, 2007) and more intrinsically-motivated locals (Schram, 2019), who are better equipped to cultivate civilian support (Moore, 2019). Fortification also directly reduces inflows of foreign fighters, forcing increased reliance on local recruits (Tyson, 2006). The impacts of fortification on civilian livelihoods compound these effects. Civilians residing in borderlands frequently depend on informal smuggling economies and cross-border markets (Idler, 2019). Fortification efforts impede licit and informal trade, reducing reservation wages. 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, engaging in civilian victimization requires resource expenditures in manpower and matériel, so victimization reduces rebels' ability to produce anti-government violence. Because attacks on counterinsurgent forces are more effective at demonstrating rebel capacity and building civilian confidence in the viability of militant challengers (Wood, 2010), groups facing resource deficits should privilege these operations. In sum, counterinsurgent border fortification should prompt rebels to reduce civilian victimization in order to cultivate greater civilian support, and thereby to improve local extractive capacity and recoup lost resources.[7]

> H_2: **Counterinsurgent 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 medium- and long-term.

Broader societal cleavages impact insurgent-civilian relations apart from insurgents' resources (Wood, 2003). In particular, many civil wars take on an identity-based dimension. In contexts where stark boundaries exist between groups, such as in Iraq, where society is divided along sectarian lines, rebel groups often draw support from one primary community or group (e.g. ethnicity, religion, political party). In these settings, rebel choices about civilian victimization are

[7] A complementary way insurgents may build civilian support is by increasing service provision to earn the goodwill of the populace (Arjona, 2016; Stanton, 2016). I bracket this expectation in this paper because I lack data on rebel governance in Iraq.

complicated by combatant identities (Lyall, Blair and Imai, 2013). Rebels are likely to exercise restraint toward their core constituencies while targeting out-groups (Fjelde and Hultman, 2014; Stanton, 2016).

Considerations about intergroup dynamics are especially pressing in the face of 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 civilian support in areas populated by out-groups, making restraint less attractive. If out-group antagonism is high, meaning civilians express systematic bias against other communities, then rebels' prospective civilian supporters may even favor out-group victimization (Polo and González, 2020). This discussion suggests that the effect of border fortification on insurgent-civilian relations is conditional. Insurgents' efforts to cultivate support following interdiction of their foreign logistics should manifest most acutely in areas populated by prospective civilian supporters (i.e. in-groups).

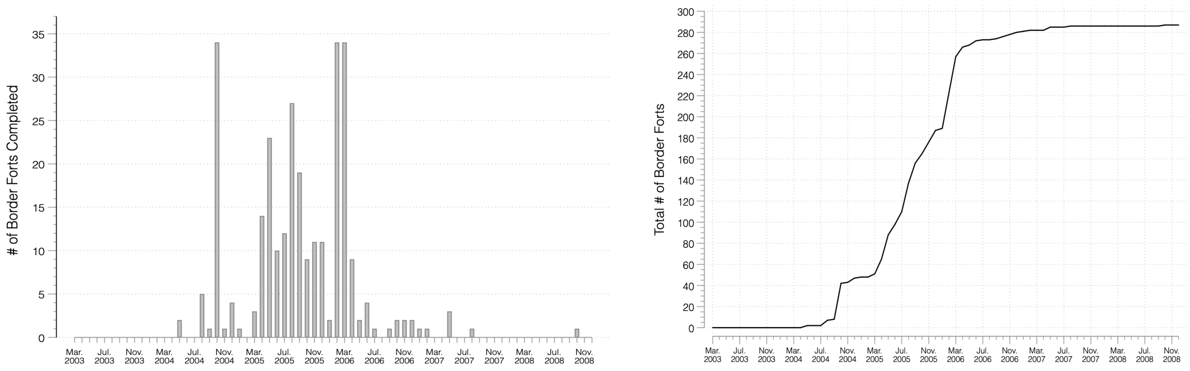
> H_3: **Counterinsurgent border fortification causes insurgents to reduce victimization of in-group civilians.**

In tandem, these arguments highlight the fortification dilemma. Counterinsurgent border control efforts reduce rebel capabilities, inducing shifts from conventional to irregular combat tactics. However, efforts counterinsurgents take to fortify international borders and isolate rebels from transnational resources perversely incentivize rebels to invest in building local support. Hence, counterinsurgent border fortification trades-off reduced rebel capabilities for greater competition over local hearts-and-minds. Fortification-induced tactical shifts may complicate counterinsurgents' own pacification efforts.

4 Border Fortification in Iraq

In order to test my theory, I study border fortification during the Iraq War. When the US invaded Iraq in 2003, the primary focus was on Baghdad. However, as the insurgency evolved, the US quickly moved to implement border controls to reduce the flow of fighters, arms, and illicit goods across Iraq's historically-porous borders. Saddam Hussein's regime had maintained hundreds of small border posts along Iraq's frontiers, and paid tribal militias to patrol remote sectors (Demarest and Grau, 2005). However, the pre-invasion Iraqi border security apparatus was dismantled under de-Baathification pursuant to Coalition Provisional Authority (CPA) Order #2, which disbanded the Saddam-era security forces.

Figure 1: Border Fortification Over Time



Note: The left panel shows the number of border forts completed each month. The right panel shows the cumulative number of forts built.

In the wake of de-Baathification, Iraq's borders went unsecured, and as the insurgency matured, many insurgent groups leveraged cross-border havens and supply lines, drawing on contacts in established smuggling networks, overt support from Iranian security forces, and tacit support from other neighboring states, especially Syria and Jordan. In response to the transnationalization of the insurgency, the US-led Coalition invested in border fortification. 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 interdict and deny insurgents' external support. Figure 1 depicts the timeline of the construction effort and the cumulative number of forts built along Iraqi borders over time. Approximately 90% of all Iraqi border forts were built between May 2004 and March 2006, when the sectarian insurgency reached its near-peak.

Figure 2 depicts spatial variation in the implementation of US-led border fortification. Geographically, fortification efforts were widespread, occurring in all 11 governorates contiguous to Iraq's international borders, and 25 of 29 Iraqi border districts.[8] Fortification efforts were predominately concentrated in four districts: Al-Rutba, bordering Syria, Jordan, and Saudi Arabia

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

(37 forts); Khanaqin and Sulaymaniya, near Iran (21 and 18 forts); and Sinjar, bordering Syria (18 forts). On average, forts in districts along Iraq's borders were spaced every 24 kilometers, with mobile patrols, electronic sensors, and aerial surveillance employed to monitor border areas between forts. Forts took an average of 285 days to construct, with a median of 262 days. These projects began 9 days earlier than forecasted and ended 1 day later than forecasted on average.

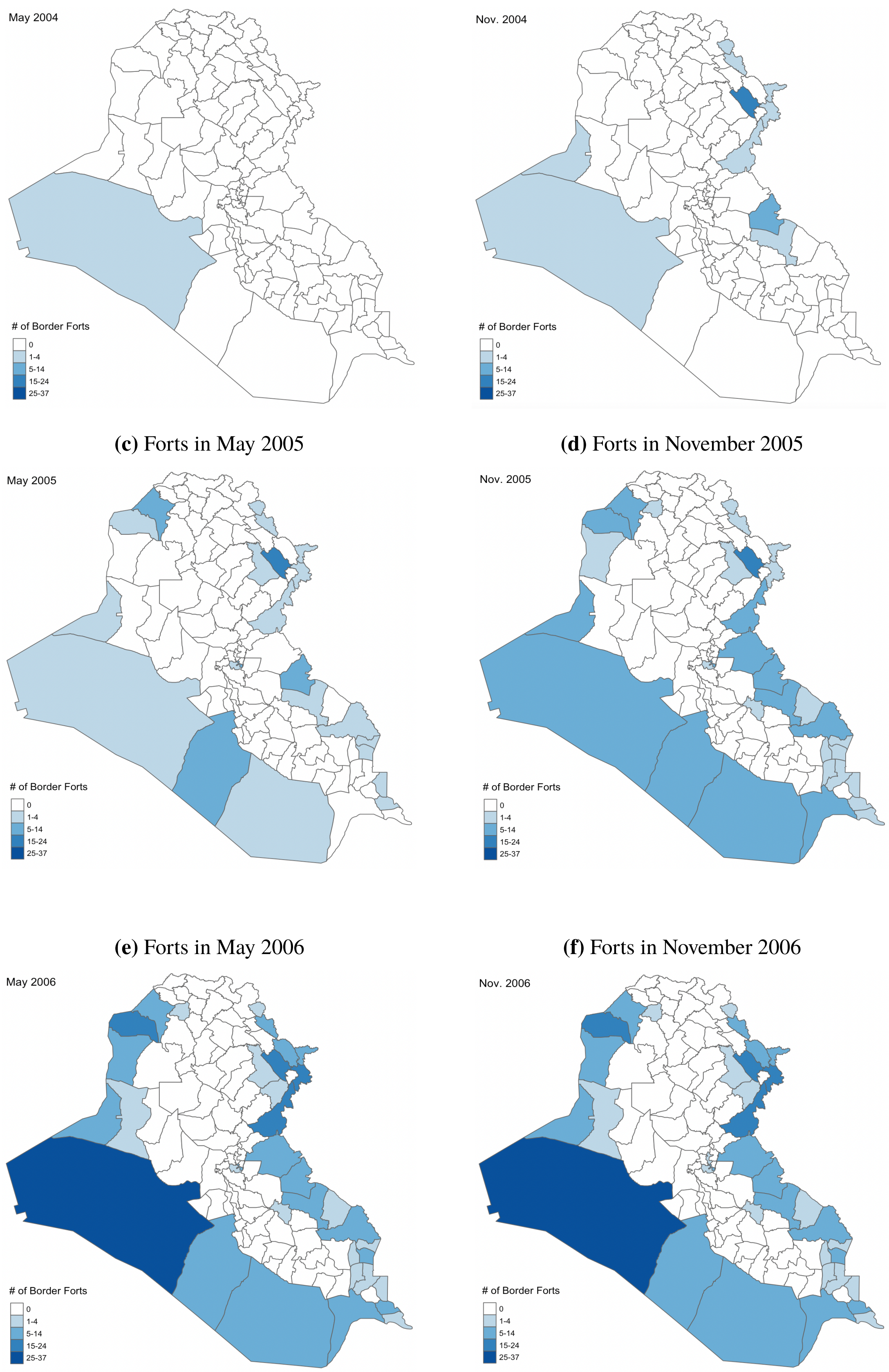
Between March 2003 and December 2009, US forces also rebuilt or constructed 52 non-fort border security facilities, including academies or headquarters for training troops of the DBE, and wells and roads for DBE use. In total, US border control projects in Iraq cost $237,820,943, not including sums paid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

5 Research Design and Data

The Iraqi case is an ideal for setting for identifying the effects of counterinsurgent border fortification on insurgent violence. First, most rebel groups in Iraq were organized along lines closely matching Iraq's district borders, and managed finances at the local level (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 external support. Moreover, the porous nature of Iraq's borders meant virtually all militant groups relied to some extent on foreign resources.

On Iraq's eastern border, Iran supported a range of Shi'a militias, providing weapons and training, and also engaging in active subversion of Coalition and Iraqi security forces (Felter and Fishman, 2008). In some instances, Iranian operatives maneuvered directly against troops engaged in border control operations. On Iraq's western border, Syria, Jordan, and Saudi Arabia were more tacit conduits for insurgent support. These countries allowed some insurgent logistical activities, and were used by couriers and foreign fighters transiting into Iraq. In addition, Syrian intelligence facilitated the transfer of weapons and suicide vests to AQI and other Sunni groups. Tribal smug-

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.

gling between Iraq and Syria was also extensive. Along Iraq's northern border, Turkey generally cooperated with US-led border security efforts, but was a conduit for the smuggling and sale of Iraqi oil stolen by insurgent groups, namely AQI. Along Iraq's southern border, Kuwait maintained a comprehensive border security regime. Kuwait effectively denied insurgent cross-border logistics, though it did produce foreign fighters who entered Iraq via Saudi Arabia.

5.1 Data

To assess my hypotheses, I leverage project-level data on US border fortification from the Iraq Reconstruction Management System (IRMS) (Berman, Shapiro and Felter, 2011). These data represent a near-complete record of US reconstruction projects during Operation Iraqi Freedom.[9] Specifically, the IRMS data describe the construction timelines, costs, project details, and funding sources for 73,600 individual projects undertaken by US forces.

This unique data allow me to chart the construction and completion of border fortifications in Iraq at the district-month level between 2003 and 2009. From the project data I construct my core independent variable, **border fortification**, which takes a value of 1 in all district-months with a completed border fort, and 0 otherwise. This is a bundled treatment that includes the presence of a border post and troops manning it, as well as berms and barriers extending out from border garrisons, and enhanced surveillance and reconnaissance capabilities employed by Coalition and Iraqi DBE forces in border monitoring.[10] In this sense, border fortification is best thought of as a system-of-systems (Skirlo, 2007).

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 of insurgent violence. An advantage of using SIGACT data is that they approximate the "universe" of anti-government violence (Weidmann, 2016, 211).

To capture conventional tactics, I study direct fire attacks, where rebels engaged counterin-

[9]IRMS describe the universe of US reconstruction spending but for $8 billion disbursed in 2003. None of this unmeasured funding was allocated to border control operations. The first border projects were funded under the supplemental Iraq Relief and Reconstruction Fund (IRRF2). IRRF2 funds were appropriated in November 2003, but not released until January 2004, at which point they were recorded in IRMS.
[10]Coalition forces used drones extensively to monitor Iraq's borders.

surgent 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 counterinsurgent forces beyond the line-of-sight (e.g., mortars, rocket attacks). These are a good proxy for irregular tactics because they require less coordination and far less 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.[11] This variable takes a value of 0 in all months with no insurgent-initiated, projectile-fire SIGACTs, and otherwise equals $\frac{\text{Indirect Fires}}{\text{Indirect Fires} + \text{Direct Fires}}$.[12]

My analyses also include a range of covariaties, which vary across specifications, but include variables like population, oil production, unemployment, spending on reconstruction programs, and levels of Coalition-caused civilian casualties. Given my expectation that insurgent-civilian relations vary by the sectarian composition of districts, this is perhaps the most important covariate. Following Berman, Shapiro and Felter (2011), I use governorate-level voter returns from the December 2005 parliamentary election to measure sectarianism. If a Shi'a, Sunni, or Kurdish party secured at least 66% of the vote share in a district, it is defined as homogeneous and controlled by the respective sect; otherwise, the district is coded as mixed sectarian. Substantively similar results emerge when sectarianism is defined according to population rather than vote shares. Table B-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 and completion of fortification owing to bureaucratic wrangling. Border fortifi-

[11] Wright (2020) employs a similar measure.

[12] Results are substantively identical if I define the measure as $\frac{\text{Indirect Fires}}{\text{Indirect Fires} + \text{Direct Fires} + \text{IEDs}}$, which captures the share of all insurgent-initiated SIGACTs that are indirect fires. Like direct fires, IEDs require relatively more planning and coordination, and are more susceptible to civilian informing than indirect fires.

cation was funded in the context of the broader US reconstruction of Iraq. Within this massive effort, project funding was subject to numerous and 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 in November 2003 to the time the first fort was completed in May 2004 is attributable to major bureaucratic 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, 2009, 126).[13]

After June 2004, the Department of Defense took responsibility for security projects like border fortification. Under Defense oversight, the slow process of reconstruction spending was accelerated drastically, with contracts awarded in 90 days that would have taken 14-18 months to approve under normal circumstances (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, funds were surged into governance activities just before the 2005 parliamentary election. Changes in the priority border security received during these reprogrammings created additional, plausibly exogenous variation.

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

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

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 that varies across specifications. $\text{BorderFort}_{j,t}$ 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 tactics. Main analyses compare fortified and non-fortified districts in border governorates, but results are

[13] Emphasis added.

substantively similar when 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-3 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 timelines. If violence caused frequent construction delays, we would expect to observe projects taking longer than initially forecasted. In Table B-4 I also show that violence trends do not predict treatment, and in Table B-5, a temporal placebo check gives no evidence that fortification predicts past violence. In Figure B-2 I plot adjusted mean differences in pre-treatment covariates between treated and control districts. There are no significant differences in pre-treatment means of the four focal dependent variables.

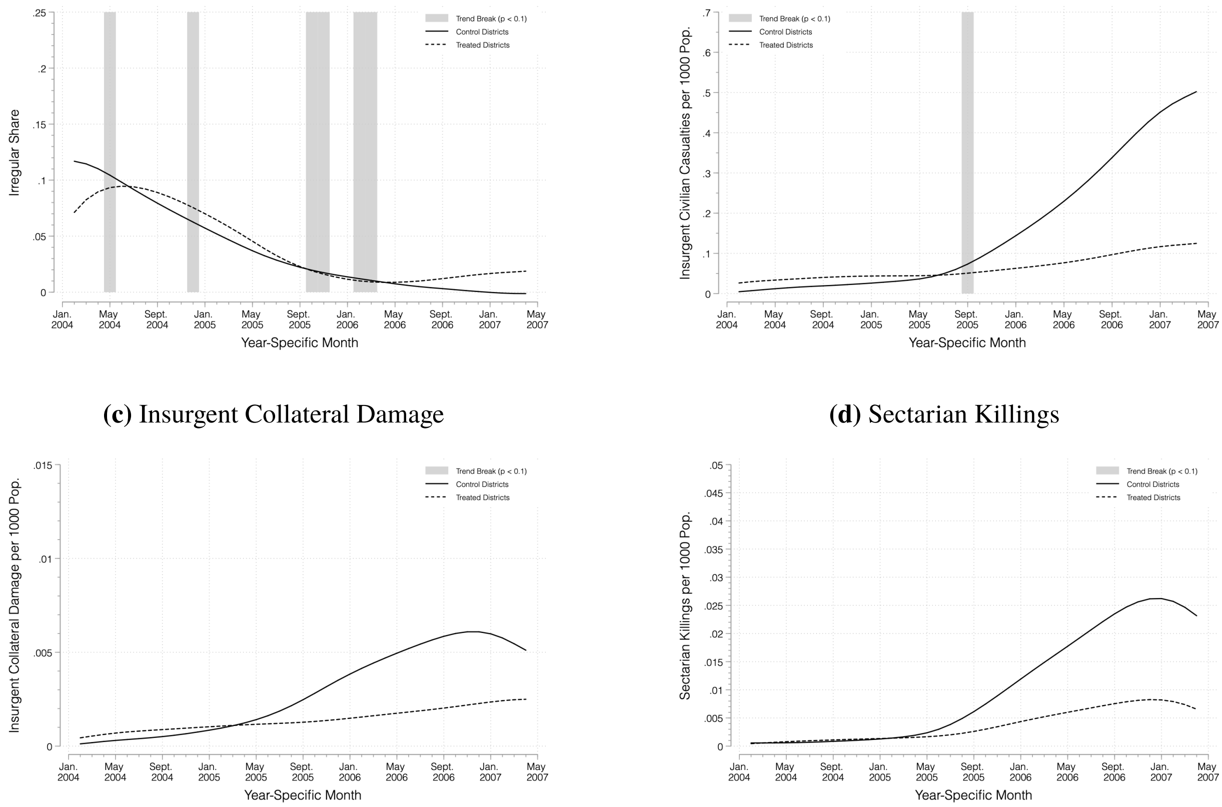
6.1 Identifying Assumptions

The validity of this estimation strategy hinges on two assumptions. First, I assume parallel trends in insurgent tactics prior to fortification. As reflected in Figure 3, this assumption appears reasonable. Values of the pre-treatment outcome variables are symmetric in trends and levels. Difference-in-slopes tests show that fewer than 8% of periods are distinguishably non-parallel, and only for two outcomes—the irregular share and insurgent civilian casualties.[14] In Figure B-3, panel event-study results also show pre-treatment effects are indistinguishable from zero.

Second, to recover the causal effect of border fortification my empirical strategy requires fortification not to coincide with other pertinent policy changes. In Table B-6 I show that border control in Iraq did not drive changes in: the number of Coalition maneuver battlions deployed, expansion of the cellular communications network, counterinsurgent spending on service provision or security, petrol production, Coalition-caused civilian casualties, Provincial Reconstruction Team presence, or provincial Iraqi control. Given my expectation that insurgents substitute into irregular attacks, namely indirect fires, another obvious policy change that could confound the results would be shifts in the deployment of counter-indirect fire systems. Qualitative evidence (section B.9) does not indicate that deployments of counter-indirect fire systems shifted with fortification. In sum, the

Figure 3: Parallel Trends in Insurgent Tactics

(a) Irregular Share **(b)** Insurgent Civilian Casualties **(c)** Insurgent Collateral Damage **(d)** Sectarian Killings



Note: Each plot shows pre-treatment trends in the corresponding outcome variable. Lines are locally weighted scatterplot smoothing. Treatment districts are districts where border fortification occurred. Control districts are non-fortified districts in border governorates. Gray bars denote statistically significant trend breaks at the 10% level based on difference-in-slopes tests.

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 year by Sunni vote share fixed effects, which absorb broad sectarian shifts over the conflict. Column 3 introduces additional, security-related controls, column 4 in-

[14] All results are robust to dropping these non-parallel periods.

troduces a spatial lag of the dependent variable to account for spatial autocorrelation, and column 5 adds a one period lag of the outcome. Given non-parallel trends in a small number of periods, column 6 verifies that the core results are robust to excluding non-parallel pre-periods. Column 7 adds district-specific linear trends. Finally, columns 8-11 shift the focal sample from districts in border governorates. Models 8 and 9 restrict the analysis to districts where two different insurgent movements—AQI and the Sunni Rejectionist groups—held influence. Both AQI and Rejectionist groups relied heavily on cross-border support, so fortification was largely focused on interdicting these groups' bases of transnational support. Finally, in model 10 I expand the analysis to all governorates except Baghdad, and in model 11 I study all districts in Iraq.

Across models there is evidence that militants responded to fortification by substituting conventional, direct fire attacks for irregular, indirect fire attacks. Taking estimates from the fully saturated specification in column 7 reveals border fortification caused a 6.9 percentage point (pp) increase in the proportion of insurgent attacks that are irregular, amounting to nearly a one-half standard deviation increase. The estimated effect size across all models ranges from 3.1 to 9.6pp.

Table 1: Border Fortification and Tactical Substitution in Iraq

VARIABLES	(1) Irregular Share	(2) Irregular Share	(3) Irregular Share	(4) Irregular Share	(5) Irregular Share	(6) Irregular Share	(7) Irregular Share	(8) Irregular Share	(9) Irregular Share	(10) Irregular Share	(11) Irregular Share
Border Fortification	0.031**	0.049***	0.073***	0.072***	0.067***	0.058**	0.069**	0.096**	0.086***	0.057**	0.049**
	(0.015)	(0.017)	(0.022)	(0.021)	(0.020)	(0.024)	(0.028)	(0.043)	(0.030)	(0.023)	(0.020)
District FE	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
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
Spatial Lag				Y	Y	Y	Y	Y	Y	Y	Y
Lagged DV					Y	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 No Trend Breaks	Border Governorates	AQI Areas	Rejectionist Areas	All but Baghdad	All of Iraq
Constant	0.027***	0.808***	1.501*	1.487*	1.398*	1.387	2.550**	-0.443	0.217	1.408	0.774
	(0.005)	(0.227)	(0.864)	(0.836)	(0.792)	(0.865)	(0.980)	(1.287)	(1.411)	(0.962)	(0.864)
Observations	4,148	3,788	2,109	2,109	2,109	1,961	2,109	1,767	2,166	3,078	3,591
R^2	0.139	0.167	0.221	0.223	0.227	0.226	0.253	0.342	0.311	0.252	0.274
Log-Likelihood	2426	2137	1031	1034	1040	999.4	1076	905.2	1088	1435	1769
AIC	-4848	-4257	-2020	-2024	-2033	-1953	-2107	-1764	-2130	-2824	-3492

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by district are in parentheses. Political/socioeconomic controls are population, population density, urbanicity, unemployment rate, oil reserves, oil production, and CERP spending/capita. Security controls are nighttime lights, total cell phone towers, new cell phone towers, Sons of Iraq, police station density, Coalition maneuver battalions, Coalition collateral damage, condolence spending/capita, police spending/capita, Provincial Reconstruction Teams, Civil Military Operations Centers, and provincial Iraqi control. The mean of irregular share is 0.051, with a standard deviation of 0.157.

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 within DBE regions.[15] In column 3, estimates are scaled using population weights, which identify heterogeneous treatment effects by district population. In column 4, I exploit variation in the intensive margin of violence, scaling estimates by up-weighting districts with more per capita insurgent-initiated SIGACTs. 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 per capita spending on non-fort border security projects and the total number of border forts in a district-month in column 7.

Table 2: Robustness of Tactical Results in Iraq

VARIABLES	(1) Irregular Share	(2) Irregular Share	(3) Irregular Share	(4) Irregular Share	(5) Irregular Share	(6) Irregular Share	(7) Irregular Share	(8) Irregular Share	(9) Indirect Fires/Capita	(10) 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: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by district 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 0.051, with a standard deviation of 0.157. The mean of indirect fires per capita is 0.003, with a standard deviation of 0.015. The mean of direct fires per capita is 0.033, with a standard deviation of 0.117.

Because the dependent variable is a proportion, least squares 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 and more precise, suggesting the main results are conservative. Finally, in columns 9 and 10 I directly estimate the effect of border fortification on per capita levels of indirect fire and direct fire attacks, disaggregating the proportion variable into its constituent terms. All tests confirm that border fortification causes rebel shifts from conventional to irregular violence.

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

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 fire attacks 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 fire into indirect fire attacks is consistent with a shift into cheaper tactics (the resource mechanism), and a shift into less collaboration-sensitive tactics (the information-sharing mechanism).

I investigate the information-sharing mechanism in Table B-7, where I study per capita insurgent 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, suicide attacks were primarily perpetrated by foreign fighters in Iraq, whose travel into the country was impeded by counterinsurgent border control. This finding is more consistent with the resource mechanism. Still, the information mechanism may complement the resource-centric logic of tactical substitution under 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 victimization outcomes: insurgent civilian casualties, insurgent collateral damage, and sectarian killings.[16] Parameters follow the main specification from column 5 of Table 1. Columns 1-3 test the main effect of fortification on civilian victimization. While coefficients are negatively signed, estimated effects are substantively small and imprecise. These initial results offer weak support for hypothesis 2.

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 between rebels and civilians provide a convenient base, and render rebel efforts to build support more credible. Rebel

[16] Outcomes are z-standardized for interpretability.

Table 3: Border Fortification and Civilian Victimization in Iraq

	Hypothesis 2			Hypothesis 3		
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				-0.531**	-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
R2	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: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors clustered by district 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.

forbearance in areas populated by out-group civilians is less efficient, since intergroup biases make out-group civilians skeptical of rebels' overtures. If out-group antagonism is sufficiently high, rebels seeking to cultivate local support may even find that in-group civilians favor retributive attacks against out-groups. Ethnic geography provides a heuristic for rebels and potential civilian supporters. Insurgent violence against civilians in homogeneous districts dominated by in-group civilians is likely to target in-group members, alienating (prospective) supporters. Civilian victimization in mixed areas is more likely to target out-groups, against whom in-group civilians may tolerate or support violence.

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

One possible concern is that fortification causes a reduction in insurgent civilian victimization because it impedes rebel production of violence, not because insurgents adapt by cultivating civilian support. Sectarian heterogeneity in the effect of border fortification is inconsistent with this view. I would not observe a significant increase in insurgent civilian victimization in mixed sectarian districts if border fortification simply reduced the ability of insurgents to produce violence generally. Figure B-4 also yields no evidence of an overall decline in violence in response to fortification. The ability of insurgents 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 of civilian victimization, I re-estimate results, focusing on the extensive margin of one-sided violence (Table B-8) and find similar effects. In Figure B-5 I disaggregate the results by sect. The negative effect of border fortification on victimization is largest in Sunni districts. By contrast, border fortification had little distinguishable effect in Shi'a districts, and a positive effect in mixed districts.[17] As noted above, US border fortification efforts chiefly focused on interdicting external support to AQI and Rejectionist groups, which operated mostly in Sunni areas of western and northern Iraq. Figure B-5 also confirms that results are robust to operationalizing districts' sectarian composition using population rather than vote shares. In Table B-9 I show results are robust to alternate specifications and estimators.

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.[18] By using violence in mixed regions, insurgents aimed to polarize 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 of the Baghdad Belts, Sunni civilians relied on Sunni

[17] I probe effects in Shi'a districts further when discussing Iranian subversion below.

[18] In Sunni-dominated areas of western Iraq, this dynamic was less relevant (Biddle, Friedman and Shapiro, 2012).

insurgents for protection against Shi'a militia violence. 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 additional implications of the fortification dilemma, and establish the robustness of the core results.

8.1 Rebel Surveillance

Figure 4: Insurgents Compiled Intelligence on Counterinsurgent 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: (أجيزة 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 scanned document on the left is a template of the weekly border activity reports compiled by AQI spies. The document was captured by US forces in western Iraq in 2007. Text on right is a translation provided by the Combating Terrorism Center at the US Military Academy. Harmony Program: NMEC-2007-658008.

For insurgents interested in retaining access to external support and concerned about the effects of fortification, a natural reaction would be to focus intelligence-gathering efforts on counterinsurgent border security operations. For instance, by collecting intelligence on where border infrastructure and personnel were deployed, insurgents could identify relatively safer and cheaper smuggling routes. Captured AQI documents released by the US military (Figure 4) reveal the group did just that. AQI established a "Border Emirate" to manage its foreign logistics, and compiled weekly reports about border security, including documenting counterinsurgent checkpoints and the ease of crossing in various locations.

8.2 Rebel Finances

The fortification dilemma should emerge whenever counterinsurgent border control increases the price to rebels of obtaining external support. For example, as it becomes more expensive to maintain cross-border smuggling routes and bribe border guards, expenses will necessarily increase. Unique data based on captured insurgent financial records (Bahney et al., 2010) permit an exploratory test of the relationship between fortification and rebel expenditures. The records detail fiscal transfers from AQI's province-level financial administration to cells in sectors of Anbar between June 2005 and October 2006.

Results in Table B-10 suggest that increasing sector-level border fortification increased province-to-sector monetary transfers. Each additional border fort increased total per capita fiscal transfers from the AQI provincial administration by up to one-quarter of a standard deviation. This implies about $31,353 per month in additional spending in the average sector. Other records reveal why border control increased local militant expenditures—fortification raised smugglers' fees. AQI financial ledgers indicate the group was paying as much as $4,985 to smugglers every two weeks, with an average expenditure of $3,425 per month from April to August 2007, not including costs for vehicles used in trafficking.[19] Military officials recognized that insurgent spending in communities where fortification disrupted local livelihoods facilitated militants' efforts to cultivate civilian support. A special operations report from Anbar noted, "The geographically remote villages and tribes assist Al Qaida 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" (Combined Joint Special Operations Task Force–Arabian Peninsula, 2007).

8.3 Rebel Smuggling

Insurgents in Iraq maintained expansive smuggling networks. By using illicit trafficking routes, insurgents could continue to access foreign support even after border fortification impeded

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

access through formal crossings. Tactical shifts along smuggling routes could cause conflict spillovers, biasing estimates. Spatial lags in the main analyses help account for spillovers, but to further probe smuggling dynamics I study ratlines geotraced from a military map of insurgent logistical networks (Figure B-6). If tactics hinge on insurgents' abilities to sustain external resource flows, then the effect of border fortification should attenuate where insurgents have access to alternate smuggling routes.

I test this implication in Figure B-7. Consistent with the main logic of the fortification dilemma, border 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 trafficking routes. Along high-density trafficking nodes where insurgents could access multiple smuggling routes but counterinsurgent surveillance was concentrated, border fortification had no significant effect on insurgent violence. These intuitive results comport with evidence that border fortification can affect violence by impacting smuggling networks (Getmansky, Grossman and Wright, 2019; Laughlin, 2019).

8.4 Foreign Subversion

Subversion by hostile neighboring states frequently undermines state capacity in peripheral regions (Lee, 2020). During Operation Iraqi Freedom, Iran actively countered border interdiction efforts, using bribes and occasional military incursions to ensure resources continued to reach their militant surrogates (Felter and Fishman, 2008). An implication is that border control should not cause tactical shifts in areas dominated by Iranian proxy groups, which could rely on Iranian subversion to sustain external resource flows.

I explore this implication in Table B-11, focusing on areas contiguous to Iran, where Jaish al-Mahdi (JAM), the primary Iranian-supported militia, was active. There is no effect of counterinsurgent border control in JAM-influenced, Iranian border districts. This suggests that the effect of fortification is conditional on interstate dynamics between fortifying states and neighboring, sanctuary countries.

8.5 Temporal Dynamism

Insurgent learning means the effect of border fortification on violence could decay over time, as insurgents find new means of accessing transnational support. On the other hand, without alternate smuggling routes or active support from a state sponsor capable of subverting border controls, insurgents may be unable to fully restore external resources. This would imply that so long as counterinsurgents continue to police border access, rebel tactical shifts will endure.

I take two approaches to understanding temporal dynamism in the effect of border fortification. In Table B-12 I replicate the core results over district-quarters, district-half years, and district-years. All results hold over these longer windows. In Figure B-8 I take a more flexible approach, re-estimating the effect of border fortification for each period from treatment onset to 36 months post-treatment. These results suggest the effect of border fortification on irregular attacks attenuates within about 6 months, while the effects on civilian victimization attenuate between 12 and 22 months. The fact that the tactical substitution effect attenuates before the civilian victimization effect may indicate that insurgents' efforts to cultivate local support succeed in mobilizing civilians, relaxing constraints on production of conventional violence (Bueno de Mesquita, 2013).

The prospect of temporal heterogeneity in the effect of border 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 treatment effect estimate (Goodman-Bacon, 2021). Figure B-9 depicts results based on new classes of difference-in-differences estimators. The core findings are unchanged.

8.6 Placebo Tests

The logic of the fortification dilemma implies that border fortification affects insurgent tactics by interdicting insurgents' foreign logistics. One concern is that the observed effects of border fortification merely capture generic effects of Iraqi counterinsurgent presence. Placebo tests using the construction of non-fort security infrastructure—DBE support facilities (e.g. wells) and academies, Ministry of Defense and Interior bases, and police support facilities, stations, and academies—help rule out this possibility. While the expansion of these security facilities meant a greater Iraqi role in counterinsurgency, they were not used to interdict the borders, and so should not have the same effects as border forts. Results in Figure B-10 confirm that hypothesized effects are unique to border forts, which were intended to interdict insurgents' transnational logistics. These tests also verify that increasing indirect fire attacks as a result of border fortification do not occur simply because border forts are fixed installations, which pose a convenient target.

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 border 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 whether or not to pursue border control must weigh whether the good consequences—reduced insurgent capabilities and civilian victimization—outweigh the bad—increased irregular attacks and competition for 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, border control-induced competition from insurgents over civilians' loyalties may ultimately make the counterinsurgents' task 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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February 8, 2022

Contents

A Descriptive Appendix

A.1 Cross-National Data on Counterinsurgent Border Fortification

Table A-1 describes counterinsurgent border fortifications constructed between 1945 and 2018. Data are based on original data collection and collation of multiple sources, including Hassner and Wittenberg (2015), Avdan and Gelpi (2017), and Carter and Poast (2017).

Table A-1: Cases of Counterinsurgent Border Fortification, 1945-2018

Name	Counterinsurgent (Patron)	Neighbor/Sanctuary	Start Year	End Year	Confidence
De Lattre Line	Indochina (France)	China	1950	1954	High
	Malaya (U.K.)	Thailand	1950	1960	Low
Pedron Line	Algeria (France)	Morocco	1957	1962	High
Morice Line	Algeria (France)	Tunisia	1957	1962	High
Cordon Sanitaire	Zimbabwe (Rhodesia)	Zambia	1966		High
McNamara Line	South Vietnam (U.S.)	Cambodia	1967	1968	High
McNamara Line	South Vietnam (U.S.)	Laos	1967	1968	High
McNamara Line	South Vietnam (U.S.)	North Vietnam	1967	1968	High
	Israel	Egypt	1968	1973	High
	Angola (Portugal)	Democratic Republic of the Congo	1971	1975	Low
	Israel	Syria	1973		High
Hornsbeame Line	Oman (U.K., Iran)	South Yemen	1973	1976	High
Cordon Sanitaire	Zimbabwe (Rhodesia)	Mozambique	1974		Moderate
	Israel	Lebanon	1975		High
	South Africa	Mozambique	1975		Moderate
The Berm	Western Sahara/Morocco	Mauritania	1980		High
	Nigeria	Cameroon	1981		Low
	Israel	Jordan	1981		Moderate
	Afghanistan (USSR)	Pakistan	1981	1989	Moderate
	Afghanistan (USSR)	China	1981	1989	Moderate
Cordon Sanitaire	Nicaragua	Honduras	1981	1990	Low
	Afghanistan (USSR)	Iran	1982	1989	Moderate
	Egypt	Gaza	1982		Moderate
Dogob Defensive Line	Ethiopia	Somalia	1982	1982	Low
Vat Cong Defensive Line	Cambodia (Vietnam)	Thailand	1983	1991	High
	Turkey	Iran	1985		Low
	Turkey	Iraq	1985		Low
	India	Bangladesh	1986		High
	South Africa	Swaziland	1986		Moderate
	Azerbaijan	Armenia	1991		Low
	Armenia	Azerbaijan	1991		Low
	Iran	Pakistan	1991		Low
	India	Pakistan	1992		High
	Malaysia	Thailand	1993		Moderate
	Israel	Gaza	1994		High
	Uzbekistan	Kyrgyzstan	1999		Moderate
	Iran	Afghanistan	2000		High
	Israel	West Bank	2000		High
	Uzbekistan	Afghanistan	2001		High
	India	Myanmar	2003		Moderate
	Thailand	Malaysia	2004		Moderate
	Pakistan	Afghanistan	2005		High
	Iraq (Coalition)	Syria	2005		High
	Iraq (Coalition)	Iran	2005		High
	Iraq (Coalition)	Saudi Arabia	2005		High
	Iraq (Coalition)	Kuwait	2005		High
	Iraq (Coalition)	Jordan	2005		Moderate
	Jordan	Iraq	2006		High
	Saudi Arabia	Yemen	2008		High
	Georgia	Russia/South Ossetia	2008		High
	Georgia	Russia/Abkhazia	2008		High
	Myanmar	Bangladesh	2009		High
	India	Myanmar	2010		High
European Wall/Great Wall	Ukraine	Russia	2013		High
	Turkey	Syria	2013		High
	Tunisia	Libya	2015		High
	Morocco	Algeria	2015		Low
Jordan Great Wall	Jordan	Syria	2016		High
Jordan Great Wall	Jordan	Iraq	2016		High
Al Shabaab Wall	Kenya	Somalia	2016	2019	High
	Iraq	Syria	2018		Moderate

B Empirical Appendix: Iraq

B.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-6, 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.[20] 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.[21] 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.[22]

Second, in Figure B-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."

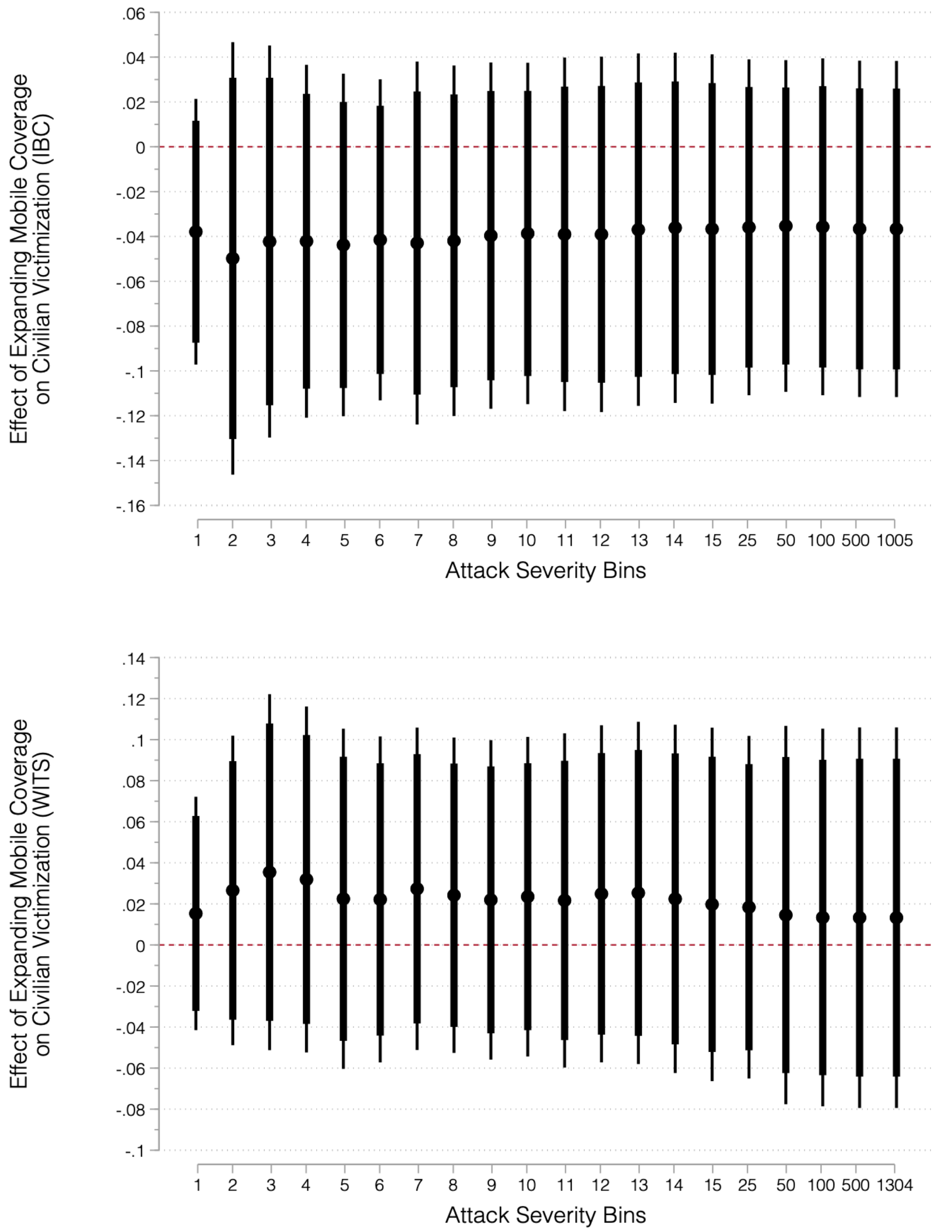
I implement this test for IBC data on insurgent collateral damage and WITS data on insurgent civilian casualties. The specific procedures for the results reported in Figure B-1 are as follows. First, I split IBC and WITS data by the reported severity of each attack. For IBC this means splitting

[20] 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.

[21] 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.

[22] 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 B-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 B-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*** (0.086)	0.174*** (0.022)	0.322*** (0.052)	0.081*** (0.016)		
Coalition-Caused Civilian Casualties (SIGACTs)					0.006*** (0.002)	0.003** (0.001)
Unit FE	N	Y	N	Y	N	Y
Year-Specific Month FE	N	Y	N	Y	N	Y
Constant	46.996 (27.281)	58.556*** (1.143)	29.701 (15.409)	42.295*** (0.854)	4.517* (2.334)	5.025*** (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: *** p<0.01, ** p<0.05, * p<0.1. 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.

Finally, in Table B-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.

B.2 Descriptive Statistics

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

Table B-2: Summary Statistics for Iraq Analyses

	Observations	Mean	Std. Dev.	Minimum	Maximum
Dependent Variables:					
Irregular Share	6344	0.051	0.157	0	1
Insurgent Civilian Casualties	7488	0.060	0.237	0	3.957
Insurgent Collateral Damage	7904	0.001	0.006	0	0.240
Sectarian Killings	7904	0.004	0.013	0	0.411
Independent Variables:					
Border Fortification	8528	0.243	0.429	0	1
Cumulative Number of Border Forts	8528	1.747	4.597	0	37
Border Fort Construction	8528	0.071	0.257	0	1
Non-Fort Border Infrastructure	8528	0.053	0.224	0	1
Directorate of Border Enforcement Academy	8528	0.039	0.194	0	1
Control Variables:					
Population	8528	5.803	1.042	2.546	8.113
Population Density	8528	0.431	1.611	0	13.939
Urban Population	8068	0.530	0.221	0.088	1
Sunni Share	8528	0.208	0.284	0	0.917
Shi'a Share	8528	0.409	0.384	0	0.902
Kurdish Share	8528	0.245	0.383	0	0.993
CERP Spending	8528	1197.209	4618.205	0	185,458.284
Nighttime Lights	8528	0.046	0.065	0.001	1.048
Unemployment Rate	8528	0.085	0.070	0	0.509
Price-Weighted Oil Reserves	8528	7.597	7.004	0	17.588
Price-Weighted Oil Production	8528	12.368	11.750	0	27.355
Cell Phone Towers	3780	17.903	38.540	0	296
New Cell Phone Towers	3780	0.519	1.833	0	35
Sons of Iraq	8528	0.121	0.326	0	1
Police Density	8528	0.046	0.225	0	2.180
Coalition Maneuver Battalions	3591	0.929	1.629	0	15.500
Coalition Collateral Damage	7904	0.001	0.004	0	0.118
Condolence Spending	8528	58.009	445.994	0	22,510.368
Police Spending	8528	674.959	3331.430	0	87,363.273
Provincial Reconstruction Team	6240	0.108	0.310	0	1
Civil Military Operations Center	6448	0.174	0.380	0	1
Provincial Iraqi Control	8528	0.275	0.446	0	1

B.3 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-3: 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: *** p<0.01, ** p<0.05, * p<0.1. 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 $\text{actual} - \text{forecasted}$ 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).

B.4 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-4: 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: *** p<0.01, ** p<0.05, * p<0.1. 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.

B.5 Temporal Placebo Check

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

Table B-5: Fortification Does Not Predict Past Violence

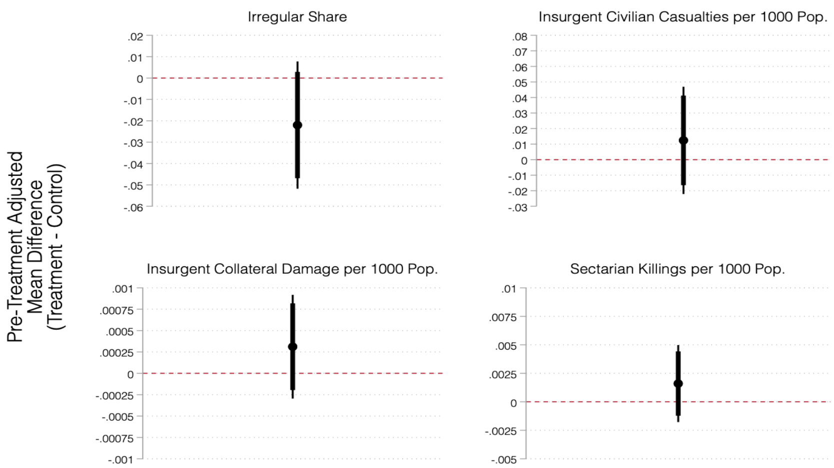
	Insurgent-Initiated SIGACTs				Irregular Share				Insurgent Civilian Casualties			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	1 Month Lag	3 Month MA	6 Month MA	9 Month MA	1 Month Lag	3 Month MA	6 Month MA	9 Month MA	1 Month Lag	3 Month MA	6 Month MA	9 Month MA
Border Fortification	-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: *** p<0.01, ** p<0.05, * p<0.1. 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.

B.6 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-2: Adjusted, Pre-Treatment Mean Differences in Dependent Variables

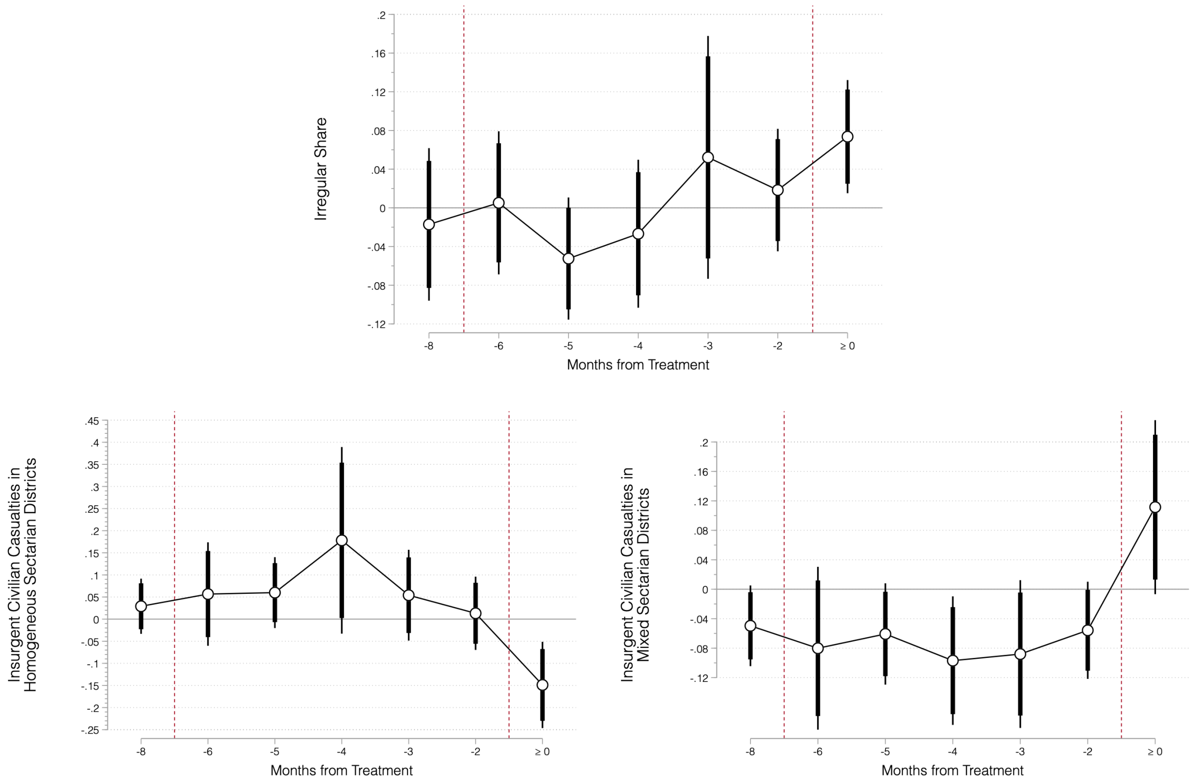


Note: Bars are 90 and 95% confidence intervals based on robust, district clustered standard errors. Mean differences are calculated from OLS regressions of treatment status on the respective outcome, with district, year-specific month, and Sunni vote share by year fixed effects.

B.7 Assessing Parallel Trends With Leads and Lags

The key identifying assumption is that treated districts do not experience differential trends in violence prior to fortification. Differential trends could be driven by a number of factors, including anticipation of the policy. I follow the suggestion of Freyaldenhoven, Hansen and Shapiro (2019), and provide graphical evidence of parallel pre-trends in Figure 3. Differences-in-slopes tests suggest that trends in two outcomes, the irregular share and insurgent civilian casualties, are distinguishably non-parallel in a small number of periods. To assuage concerns about these outcomes and further probe parallel trends, I illustrate pre-intervention trends using treatment leads and lags (Autor, 2003). Following the method introduced in Sun and Abraham (2020), I exclude two pre-policy periods (one and seven months before treatment). Eight leads (j) and one lag (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 our strategy.

Figure B-3: Assessing Parallel Trends With Leads and Lags



Note: Bars are 90 and 95% confidence intervals. Dashed vertical lines mark two excluded base periods: 7 months and 1 month before treatment.

B.8 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 B-1.

Table B-6: 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: *** p<0.01, ** p<0.05, * p<0.1. 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 in columns 1-5 and 7-10 are z-standardized for interpretability.

B.9 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) rader 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*b*).

B.10 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 B-7: Border Fortification and Suicide Attacks

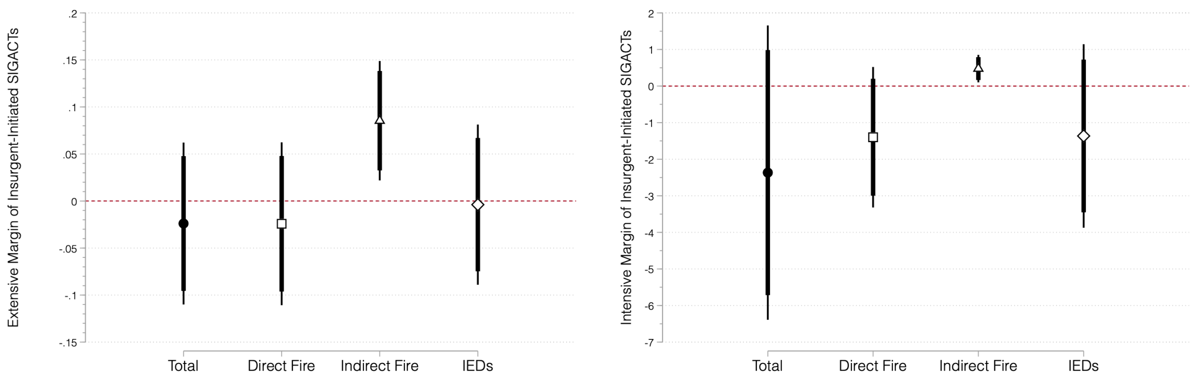
VARIABLES	(1) Suicide Attacks/ Capita	(2) Suicide Attacks/ Capita	(3) Suicide Attacks/ Capita	(4) Suicide Attacks/ Capita	(5) Suicide Attacks/ Capita	(6) Suicide Attacks/ Capita	(7) Suicide Attacks/ Capita	(8) Suicide Attacks/ Capita	(9) Suicide Attacks/ Capita	(10) Suicide Attacks/ Capita	(11) Suicide Attacks/ Capita
Border Fortification	-0.031	-0.010	0.008	0.007	0.006	0.008	-0.002	0.006	0.010	-0.010	-0.003
	(0.025)	(0.016)	(0.020)	(0.019)	(0.017)	(0.018)	(0.026)	(0.048)	(0.030)	(0.024)	(0.020)
District FE	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
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
Spatial Lag				Y	Y	Y	Y	Y	Y	Y	Y
Lagged DV					Y	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 No Trend Breaks	Border Governorates	AQI Areas	Rejectionist Areas	All but Baghdad	All of Iraq
Constant	0.059***	0.328	0.841	0.734	0.716	0.732	1.648	3.610*	1.875	0.283	0.212
	(0.009)	(0.488)	(1.042)	(1.013)	(0.931)	(0.919)	(1.589)	(1.914)	(1.609)	(1.402)	(1.331)
Observations	4,148	3,788	2,109	2,109	2,109	1,961	2,109	1,767	2,166	3,078	3,591
R^2	0.134	0.176	0.215	0.219	0.224	0.224	0.246	0.242	0.237	0.226	0.219
Log-Likelihood	-744	-752.1	-681.3	-675.9	-668.9	-690.9	-638.1	-754.1	-721.4	-580.3	-535.3
AIC	1492	1522	1405	1396	1384	1428	1322	1554	1489	1207	1117

Note: *** p<0.01, ** p<0.05, * p<0.1. 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 0.003.

B.11 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 found no distinguishable effects on the extensive or intensive margins of insurgent violence.

Figure B-4: Fortification Did Not Reduce Violence Overall



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

B.12 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. Consistent with the conditional logic outlined in the paper, insurgents in homogeneous districts are significantly less likely to victimize civilians in response to border fortification.

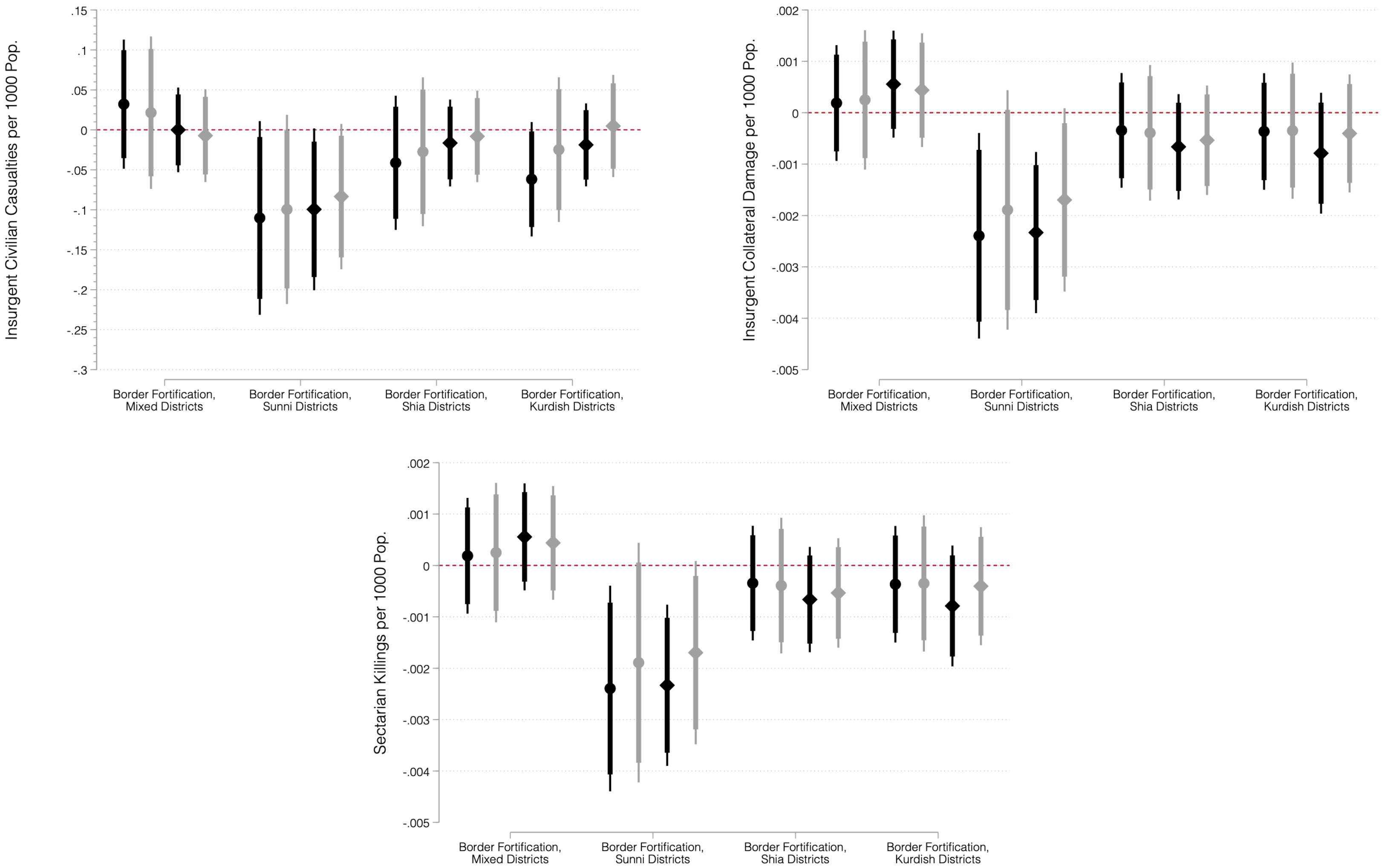
Table B-8: Fortification and the Extensive Margin of Victimization

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: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by district 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 the extensive margin of the designated header variable.

In Figure B-5 I further disaggregate the effect of border fortification across sectarian areas. Taking the core specifications, I interact border fortification with separate indicators for Sunni districts, Shia districts, 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 provincial election. Districts are defined as belonging to the respective sect if a clearly sectarian party captured $\geq 66\%$ of the vote (Berman, Shapiro and Felter, 2011). 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 significantly largest in Sunni districts for all outcomes—insurgent civilian casualties, insurgent collateral damage, and sectarian killings. Effects are less precise in Kurdish and Shia districts. In mixed sectarian districts, effects are positive but imprecise.

Figure B-5: Sectarian Heterogeneity and Insurgent Civilian Victimization



Note: Thick and thin bars are 90 and 95% confidence intervals based on robust, district clustered standard errors. Models include district, year-specific month, and year by Sunni vote share fixed effects, political/socioeconomic and security controls, spatial lags, and lagged dependent variables. 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 vote share. Gray markers denote estimates that define ethnic composition based on LandScan population data.

B.13 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. Columns 13 and 14 drop trend breaks identified in difference-in-slopes tests.

Table B-9: Robustness of Civilian Victimization Results

Panel A	DV: Insurgent Civilian Casualties/1000 Pop.										DV: Insurgent Civilian Casualties		DV: Insurgent Civilian Casualties/1000 Pop.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Border Fortification x In-Group		-0.531**		-0.531*		-0.472***		-0.216		-0.461**		-1.301*		-0.514**
		(0.204)		(0.201)		(0.141)		(0.289)		(0.185)		(0.727)		(0.240)
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	-0.051	0.417
	(0.056)	(0.269)	(0.055)	(0.261)	(0.073)	(0.183)	(0.152)	(0.340)	(0.053)	(0.193)	(0.332)	(0.679)	(0.085)	(0.262)
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	1.326	2.205
	(0.803)	(1.036)	(0.760)	(0.868)	(2.222)	(1.951)	(19.704)	(18.964)	(2.006)	(1.851)	(28.415)	(26.984)	(2.588)	(2.368)
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	2,072	2,072
Log-Likelihood	-2097	-2092	-2097	-2092	-1479	-1475	-1865	-1865	-2090	-2086	-13508	-13435	-2043	-2038
AIC	4240	4232	4240	4232	3004	2997	3776	3777	4230	4224	27062	26918	4132	4124
Panel B	DV: Insurgent Collateral Damage/1000 Pop.										DV: Insurgent Collateral Damage			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
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)	(13)	(14)
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	Y	Y
Year-Specific Month FE	Y	Y	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	Y	Y
Political/Socioeconomic Controls	Y	Y	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	Y	Y
Spatial Lag	Y	Y	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	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		
No Trend Breaks													Y	Y

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by district 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.

B.14 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 B-10: 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 Transfers/1000 Pop.
# 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

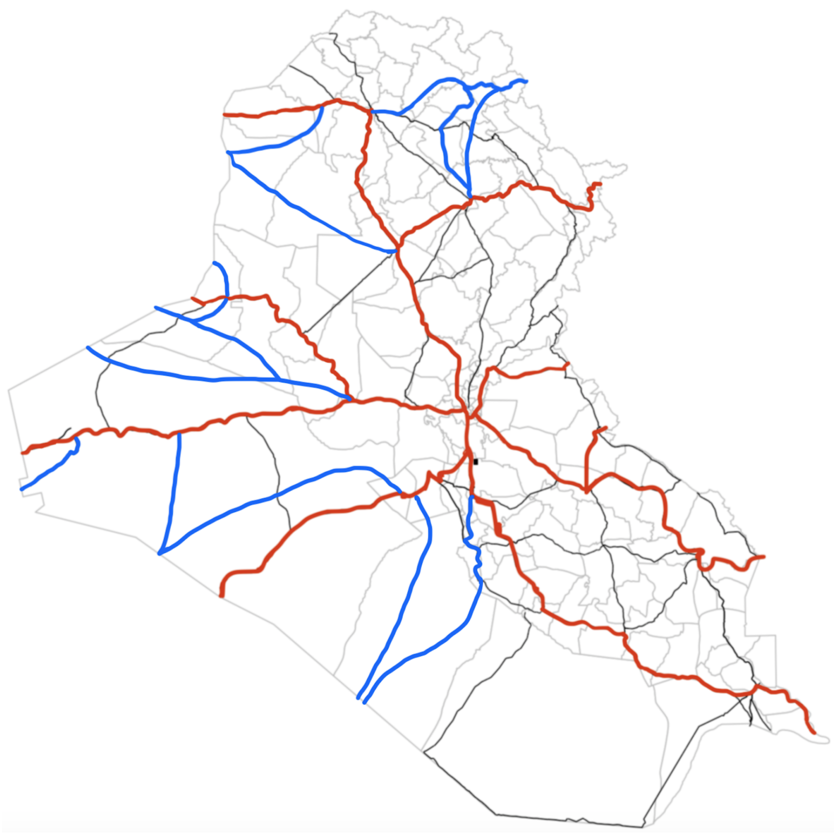
Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by AQI sector are in parentheses. The sample includes al-Qaeda in Iraq (AQI) sectors in Anbar governorate. Controls are political/socioeconomic and security controls described in Table 1. Sector transfers per 1000 refer to the amount 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.

B.15 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. 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 B-6: 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.

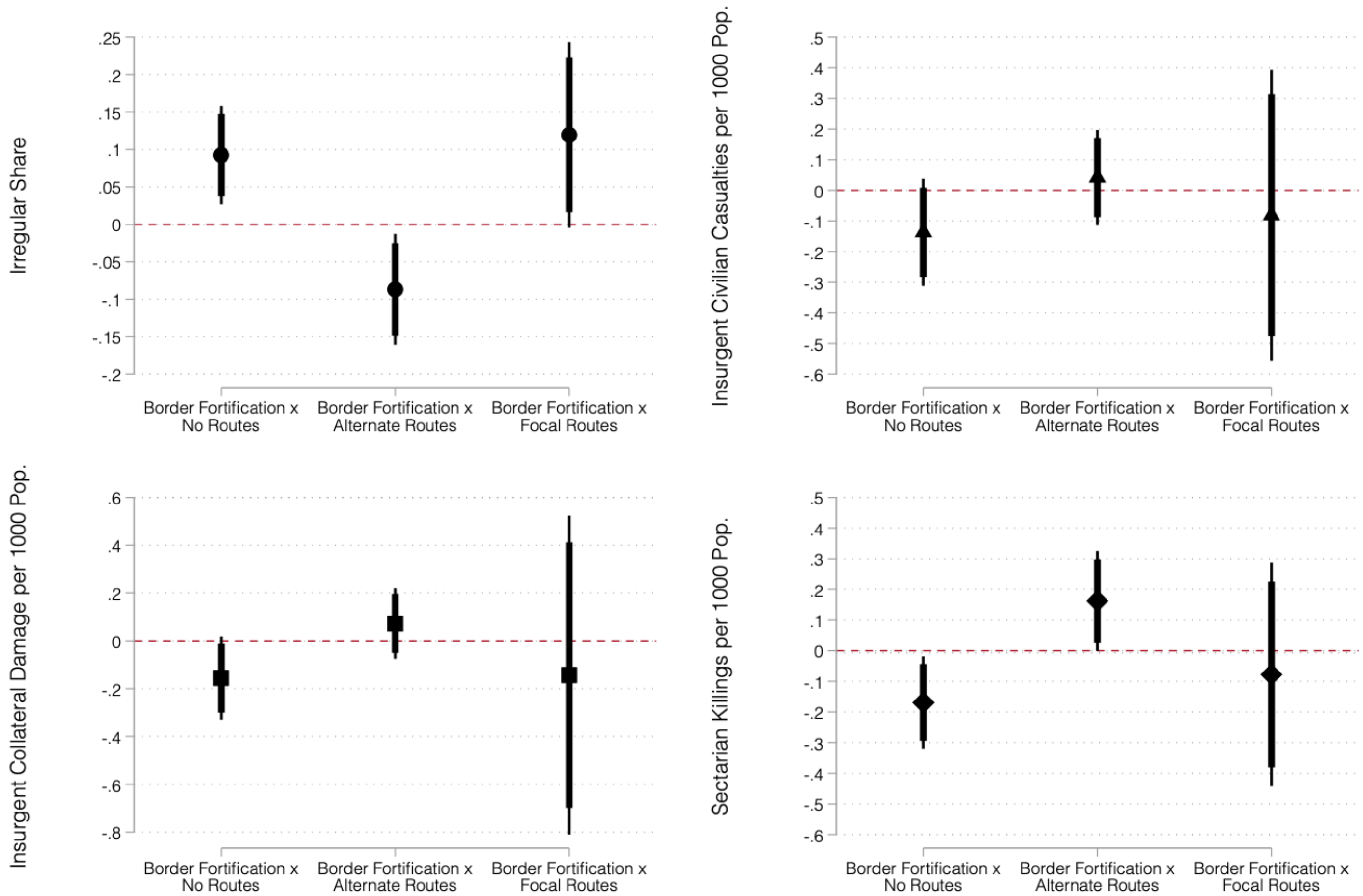
Laughlin (2019) shows that US border control efforts raised the value of trafficking routes in un-walled sections of the US-Mexico border, increasing violence in those areas as cartels competed for control of cross-border routes. Getmansky, Grossman and Wright (2019) show that in response to the Israel-Palestine 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" in 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 B-7. 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 ($p = 0.007$), and reduces the number of sectarian killings ($p = 0.029$) and insurgent civilian casualties ($p = 0.079$). The reduction in insurgent collateral damage is nearly statistically significant ($p = 0.121$).

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 ($p = 0.023$), and increases the number of sectarian killings ($p = 0.051$). Effects on insurgent collateral damage and insurgent civilian casualties 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 ($p = 0.058$).

Figure B-7: 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 year by Sunni vote share 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.

B.16 Border Fortification and Foreign Subversion

Counterinsurgent border fortification cannot affect insurgent tactics if insurgents have state sponsors that actively subvert counterinsurgent efforts such that border control does not affect flows of external resources. Qualitative accounts suggest Iran engaged in extensive subversion of U.S. border control in this manner. For instance, US forces began adding observation towers to border forts along the Iran border after it emerged that Iranian forces were coordinating arms smuggling into Iraq via cargo trucks (Multi-National Division–Central, 2007), through which rockets, guns, and explosively-formed penetrators (EFPs) and other IED components were shipped to Iranian-sponsored groups. US special forces also engaged in several direct clashes with Iranian Revolutionary Guard Corps–Quds Force (IRGC-QF) operatives in Diyala province in 2006-2007 (Combined Joint Special Operations Task Force–Arabian Peninsula, 2007). Consistent with these accounts, I observe opposite effects of border forts on militant tactics in districts near Iran and influenced by Jaish al-Mahdi (JAM), the main Iranian proxy in Iraq. These results indicate Iran often successfully subverted border fortification.

Table B-11: Iranian Sponsorship Subverted the Efficacy of Border Fortification

VARIABLES	(1) Insurgent Civilian Casualties	(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
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	1.590*	0.160	-1.065	2.102
	(0.807)	(2.659)	(1.561)	(2.511)
Observations	2,109	2,109	2,109	2,109
R^2	0.228	0.497	0.4882	0.6673
Log-Likelihood	1041	-2094	-1987	-2456
AIC	-2035	4237	4022	4960

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered by district 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.

B.17 Temporal Dynamism in the Effect of Border Fortification

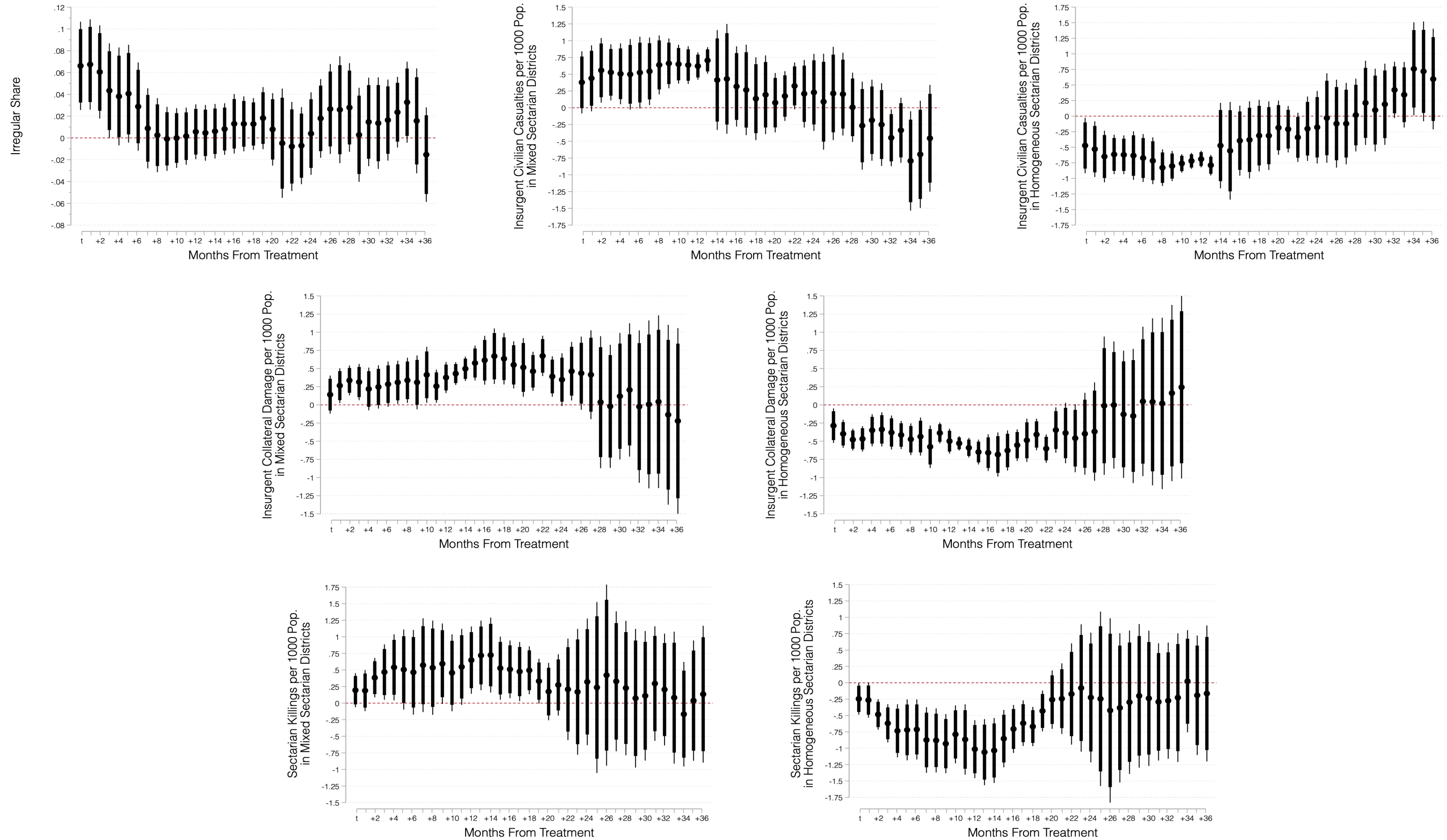
I take two approaches to understanding temporal dynamism in the effect of border fortification. In Table B-12 I replicate the core results over district-quarters, district-half years, and district-years, rather than district-months. In Figure B-8 I re-estimate the effect of border fortification for each period from treatment onset to 36 months post-treatment.

Table B-12: Border Fortification Over Longer Temporal Windows

Panel A	Unit of Analysis: District-Quarters			
VARIABLES	(1) Irregular Share	(2) Insurgent Civilian Casualties	(3) Insurgent Collateral Damage	(4) Sectarian Killings
Border Fortification x In-Group		-0.718*** (0.160)	-0.587*** (0.124)	-0.653*** (0.195)
Border Fortification	0.049* (0.029)	0.656*** (0.182)	0.440*** (0.125)	0.551*** (0.184)
Constant	2.948* (1.696)	2.647 (1.691)	1.093 (1.255)	4.956 (2.991)
Observations	740	740	740	740
R^2	0.309	0.727	0.660	0.743
Log-Likelihood	360.7	-537.3	-579.5	-784.3
AIC	-675.5	1123	1207	1617
Panel B	Unit of Analysis: District-Half Years			
VARIABLES	(5) Irregular Share	(6) Insurgent Civilian Casualties	(7) Insurgent Collateral Damage	(8) Sectarian Killings
Border Fortification x In-Group		-0.645*** (0.168)	-0.606*** (0.169)	-0.836** (0.370)
Border Fortification	0.060* (0.032)	0.544*** (0.174)	0.414*** (0.146)	0.681** (0.331)
Constant	2.106 (1.745)	2.942* (1.738)	1.001 (1.946)	4.211 (3.803)
Observations	370	370	370	370
R^2	0.404	0.816	0.755	0.736
Log-Likelihood	256.6	-194.6	-241.2	-397.5
AIC	-467.3	437.2	530.4	843
Panel C	Unit of Analysis: District-Years			
VARIABLES	(9) Irregular Share	(10) Insurgent Civilian Casualties	(11) Insurgent Collateral Damage	(12) Sectarian Killings
Border Fortification x In-Group		-0.881*** (0.272)	-0.824** (0.336)	-1.741*** (0.553)
Border Fortification	0.042 (0.036)	0.707*** (0.159)	0.504** (0.236)	1.112*** (0.301)
Constant	-0.012 (1.206)	-1.213 (3.859)	-3.520 (4.572)	-7.389 (5.116)
Observations	185	185	185	185
R^2	0.430	0.776	0.804	0.715
Log-Likelihood	178.1	-116.7	-95.93	-205.4
AIC	-310.3	281.3	239.9	458.8
District FE	Y	Y	Y	Y
Time 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

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Robust standard errors clustered by district are in parentheses. The sample includes all districts in border governorates. Controls are described in Table 1. Time fixed effects are for year-specific quarters (A), half years (B), and for years (C). Victimization outcomes are z-standardized.

Figure B-8: Temporal Dynamism in the Effect of Border Fortification

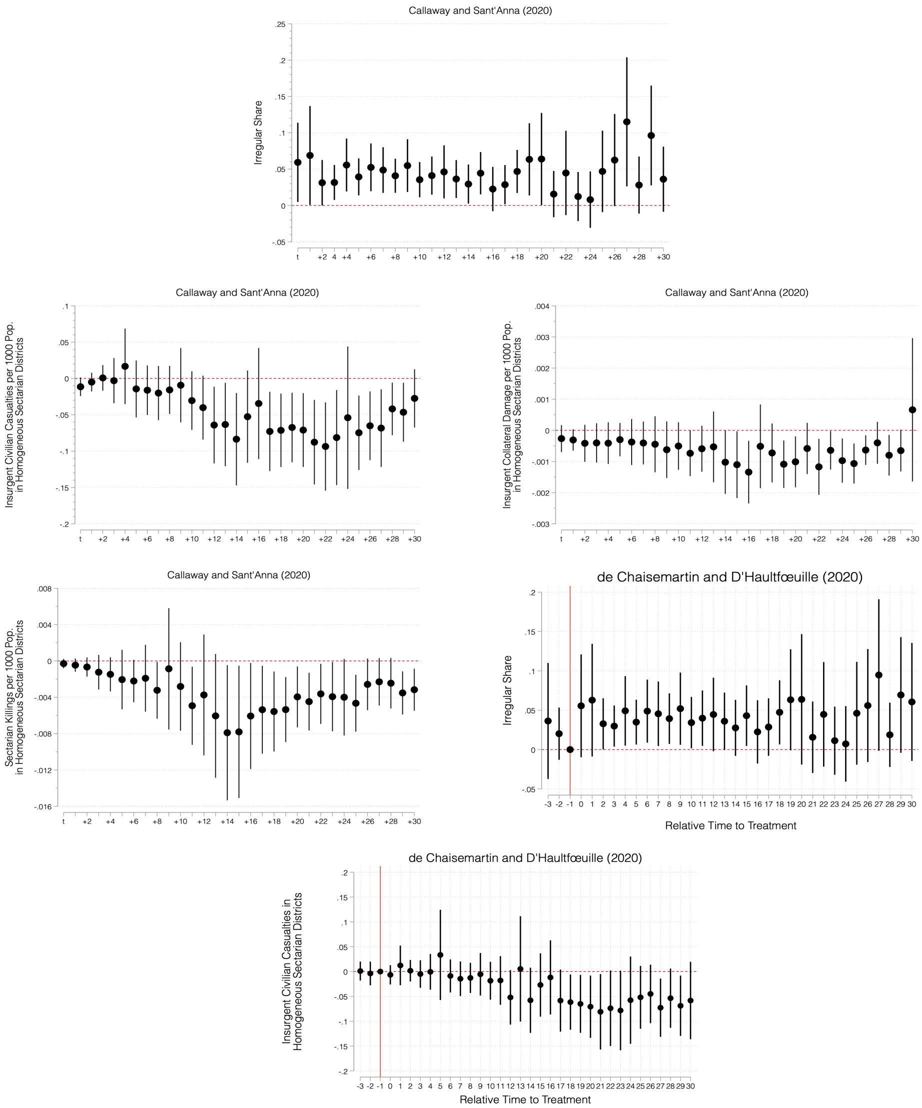


Note: Bars are 90 and 95% confidence intervals based on robust, district clustered standard errors. Plots show coefficients from regressions of border fortification on leads of the respective outcome for each period from treatment onset t to 36 months after treatment $t + 36$. Specifications follow Table 3. Estimates of civilian victimization come from models interacting border fortification with an indicator for homogeneous districts. This yields two plots for each civilian victimization outcome—one for the effect of border fortification in mixed sectarian districts and one for the effect of border fortification in homogeneous districts. Victimization outcomes are z-standardized.

B.18 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 (2020) and de Chaisemartin and D'Haultfoeuille (2020) address issues with the two-way fixed effects estimator. Callaway and Sant'Anna (2020) 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 B-9: Alternative Difference-in-Differences Estimators

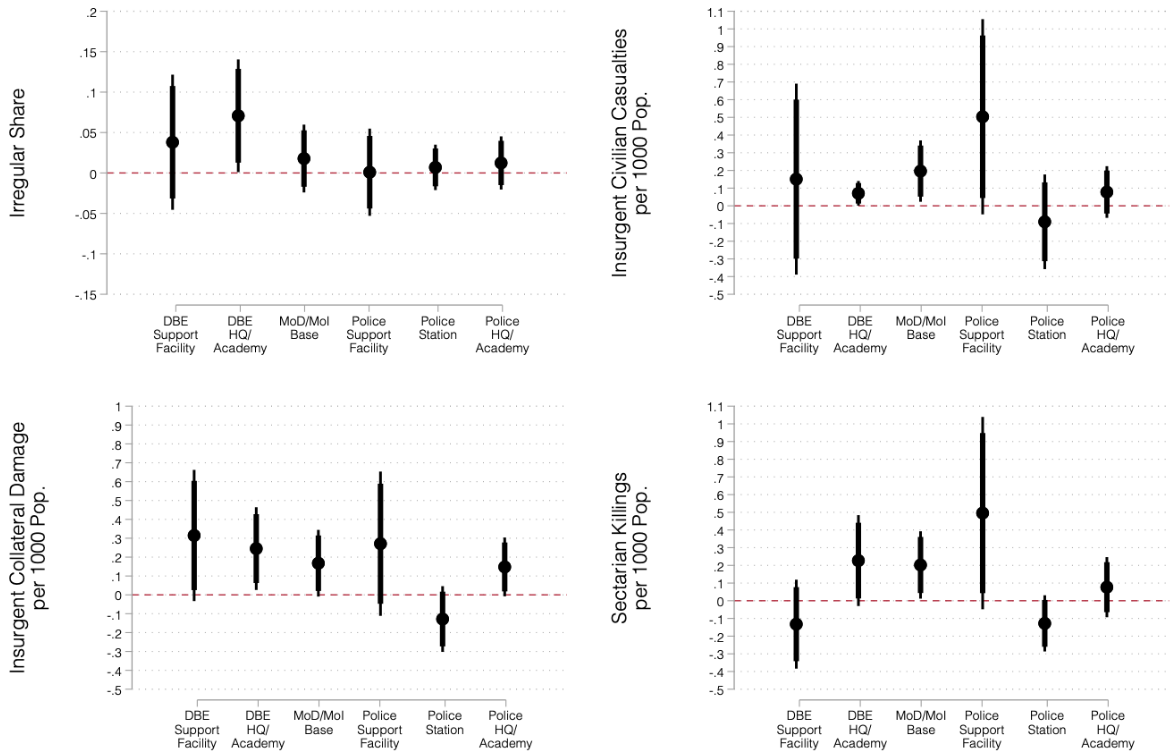


Note: Bars are 95% confidence intervals based on robust, district clustered standard errors. Plots show coefficients from regressions of border fortification on violence outcomes. Specifications follow Table 3. Estimates of civilian victimization come from models interacting border fortification with an indicator for homogeneous districts.

B.19 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 six other infrastructure types as placebo tests: DBE wells and roads (support facilities), DBE headquarters or academies, 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 B-10: 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. Victimization outcomes are z-standardized.

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