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

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Contents

A Data and Measurement

A.1 Potential Biases in Civilian Victimization Data

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

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

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

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

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

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

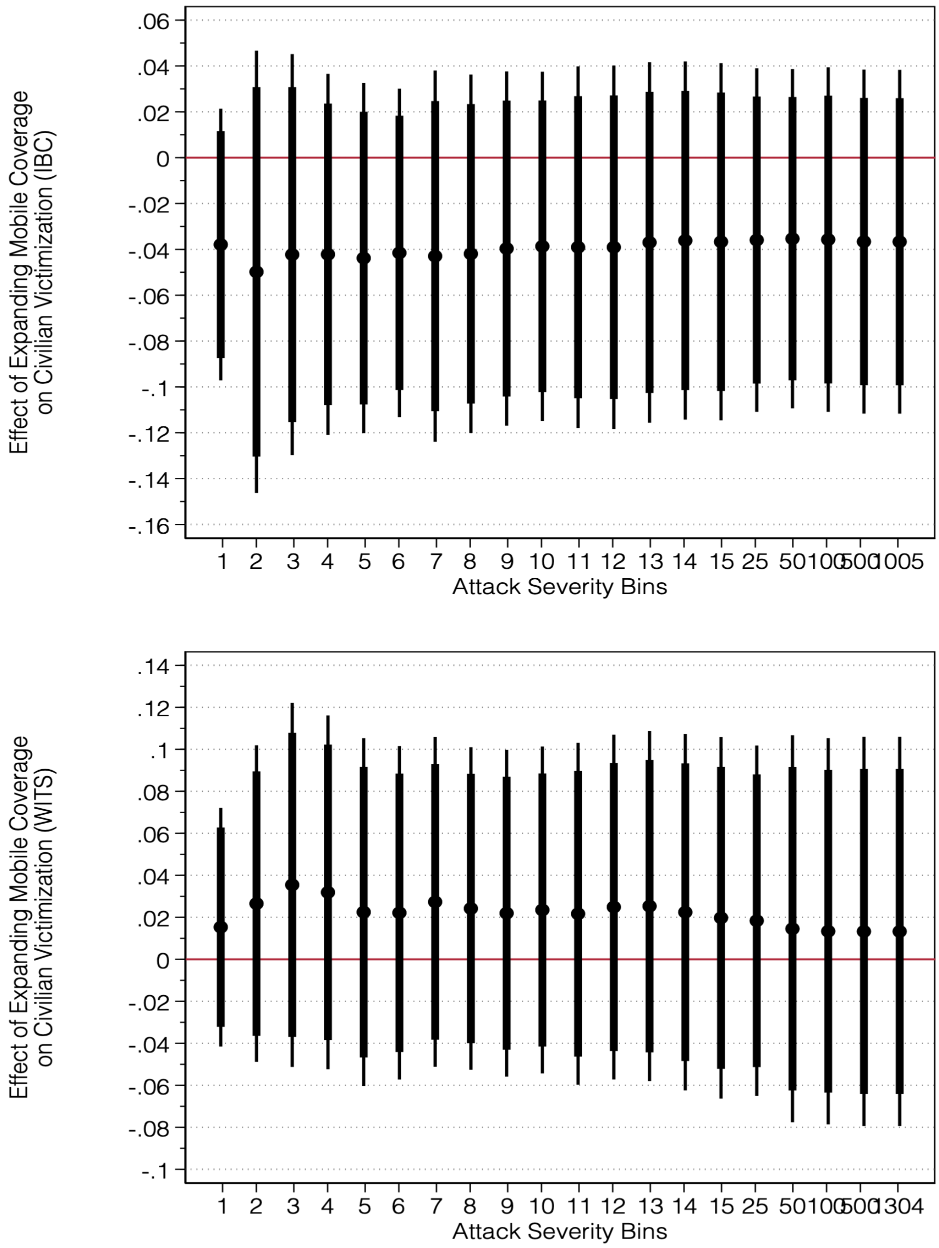
I implement this test for IBC data on insurgent collateral damage and WITS data on insurgent civilian casualties. The specific procedures for the results reported in Figure A-1 are as follows. First, I split IBC and WITS data by the reported severity of each attack. For IBC this means splitting

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

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

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

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



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

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

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

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

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

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

A.2 Descriptive Statistics

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

Table A-2: Summary Statistics

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

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

B Identification Strategy

B.1 Violence and Construction Timelines

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

Table B-1: Violence Trends and Construction Timelines

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

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

B.2 Violence and Treatment Onset

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

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

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

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

B.3 Temporal Placebo Check

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

Table B-3: Fortification Does Not Predict Past Violence

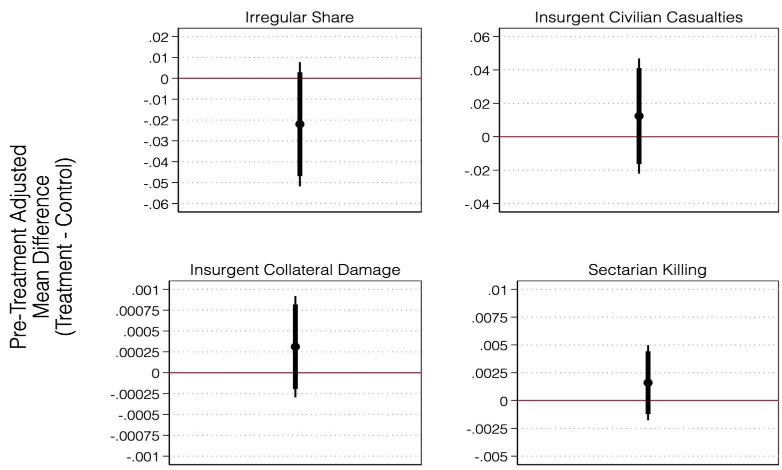
	Insurgent-Initiated SIGACTs				Irregular Share				Insurgent Civilian Casualties			
VARIABLES	(1) 1 Month Lag	(2) 3 Month MA	(3) 6 Month MA	(4) 9 Month MA	(5) 1 Month Lag	(6) 3 Month MA	(7) 6 Month MA	(8) 9 Month MA	(9) 1 Month Lag	(10) 3 Month MA	(11) 6 Month MA	(12) 9 Month MA
Border Fortification	-0.014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

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

B.4 Assessing Covariate Balance

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

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



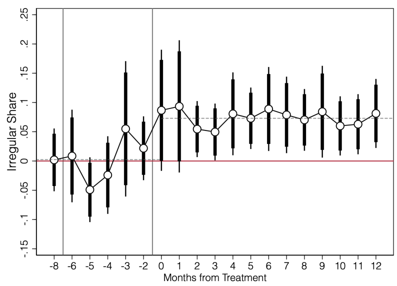
Note: Bars are 90 and 95% confidence intervals based on robust, district-clustered standard errors. Mean differences are calculated from OLS regressions of treatment status on the respective outcome, with district, year-specific month, and Sunni voteshare-by-year fixed effects.

B.5 Panel Event Study Estimates

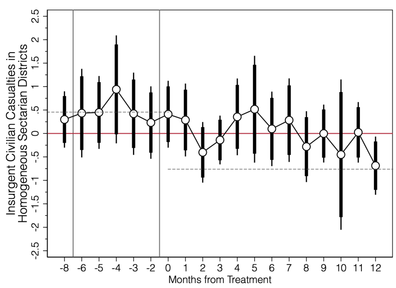
I provide graphical evidence of parallel pre-trends in Figures 3 and B-2. Following the method introduced in Sun and Abraham (2021), I exclude two pre-policy periods (one and seven months before treatment). Eight leads (j) and twelve lags (k) are included, and final leads and lags "accumulate" subsequent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

Figure B-2: Dynamic Event Study Estimates

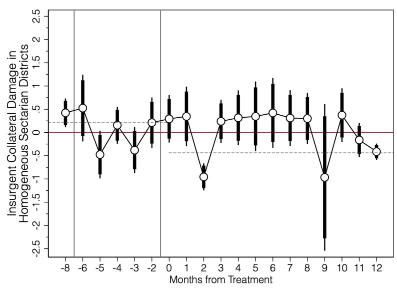
(a) Irregular Share



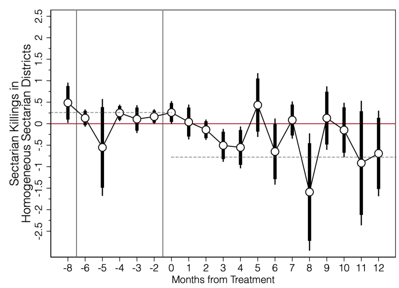
(b) Ins. Civilian Casualties (Homog.)



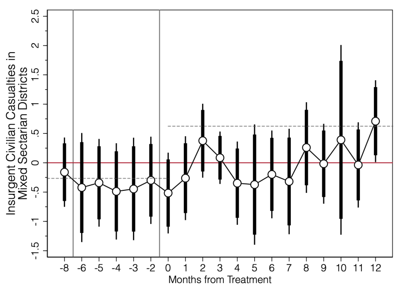
(c) Ins. Civilian Casualties (Homog.)



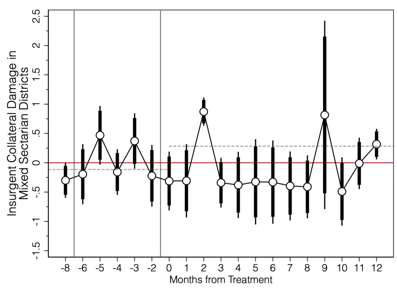
(d) Sectarian Killings (Homog.)



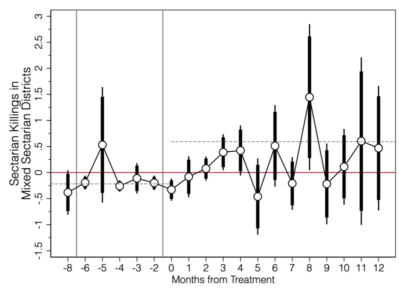
(e) Ins. Civilian Casualties (Mixed)



(f) Ins. Collateral Damage (Mixed)



(g) Sectarian Killings (Mixed)



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

B.6 Fortification Did Not Cause Other Policy Changes

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

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

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

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

B.7 Border Fortification and Counter-Indirect Fire Systems

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

C Robustness of Main Results

C.1 Information-Sharing and Tactical Substitution

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

Table C-1: Border Fortification and Suicide Attacks

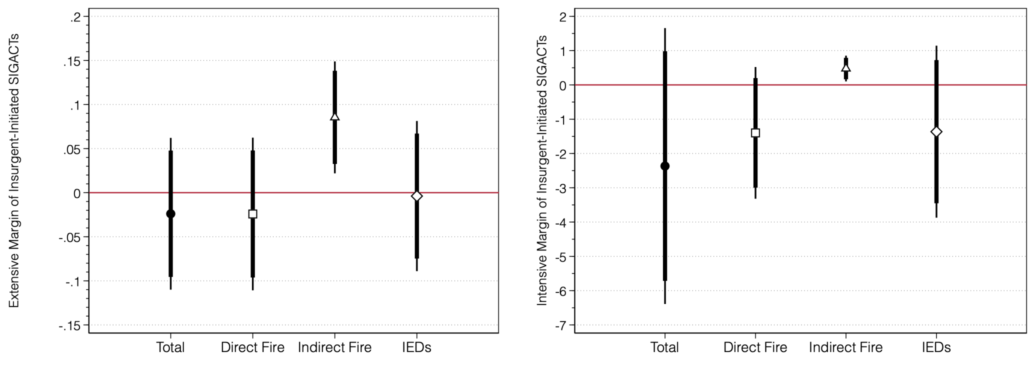
VARIABLES	(1) Suicide Attacks/ Capita	(2) Suicide Attacks/ Capita	(3) Suicide Attacks/ Capita	(4) Suicide Attacks/ Capita	(5) Suicide Attacks/ Capita	(6) Suicide Attacks/ Capita	(7) Suicide Attacks/ Capita	(8) Suicide Attacks/ Capita	(9) Suicide Attacks/ Capita	(10) Suicide Attacks/ Capita
Border Fortification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

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

C.2 Border Fortification and Overall Insurgent Violence

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

Figure C-1: Fortification Did Not Reduce Violence Overall



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

C.3 Civilian Victimization and Sectarian Geography

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

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

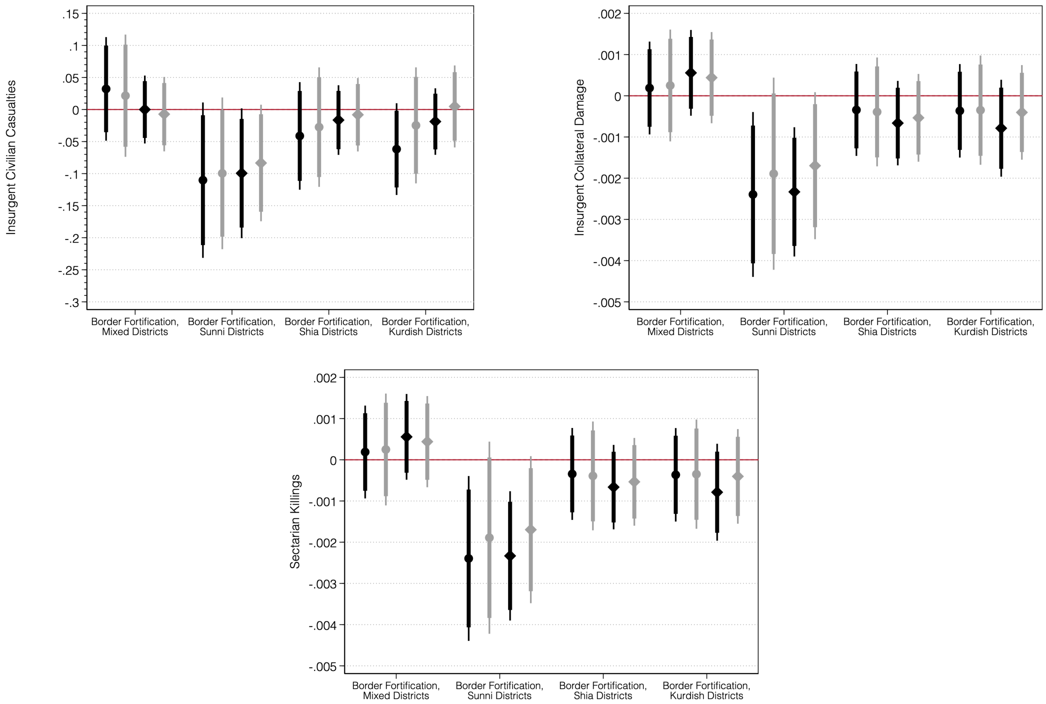
VARIABLES	(1) Insurgent Civilian Casualties	(2) Insurgent Collateral Damage	(3) Sectarian Killings	(4) Insurgent Civilian Casualties	(5) Insurgent Collateral Damage	(6) Sectarian Killings
Border Fortification x In-Group				-0.483**	-0.227**	-0.314**
				(0.107)	(0.032)	(0.064)
Border Fortification	0.011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

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

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

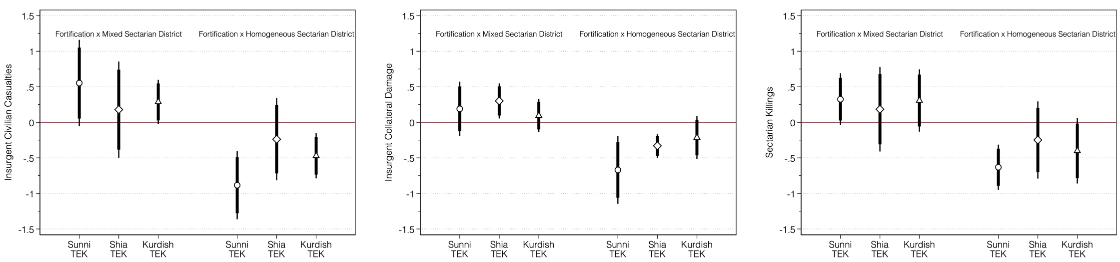
In a final set of models in Figure C-3, I also consider heterogeneity in relation to cross-border ethnic linkages. Using a shapefile from the GeoEPR dataset (Vogt et al., 2015), I overlay polygons corresponding to settlement areas of transnational ethnic kin (TEK) groups that reside in Iraq and neighboring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

Figure C-2: Sectarian Heterogeneity and Insurgent Civilian Victimization



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

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



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

C.4 Robustness of Civilian Victimization Results

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

Table C-3: Robustness of Civilian Victimization Results

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

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

D Empirical Extensions

D.1 Border Fortification and Insurgent Spending

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

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

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

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

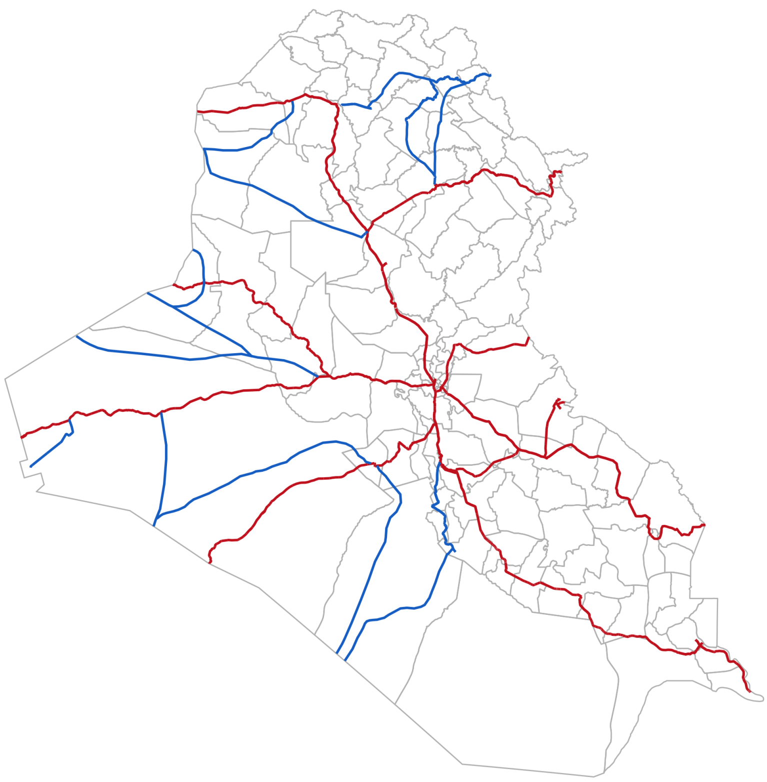
Note: Robust standard errors clustered by AQI sector are in parentheses. The sample includes al-Qaeda in Iraq (AQI) sectors in Anbar governorate. Covariates are political/socioeconomic and security controls described in Table 1. Sector transfers per 1000 refer to the amount of funds the AQI provincial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

D.2 Insurgent Smuggling Networks

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

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

Figure D-1: Geotraced Insurgent Ratlines



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

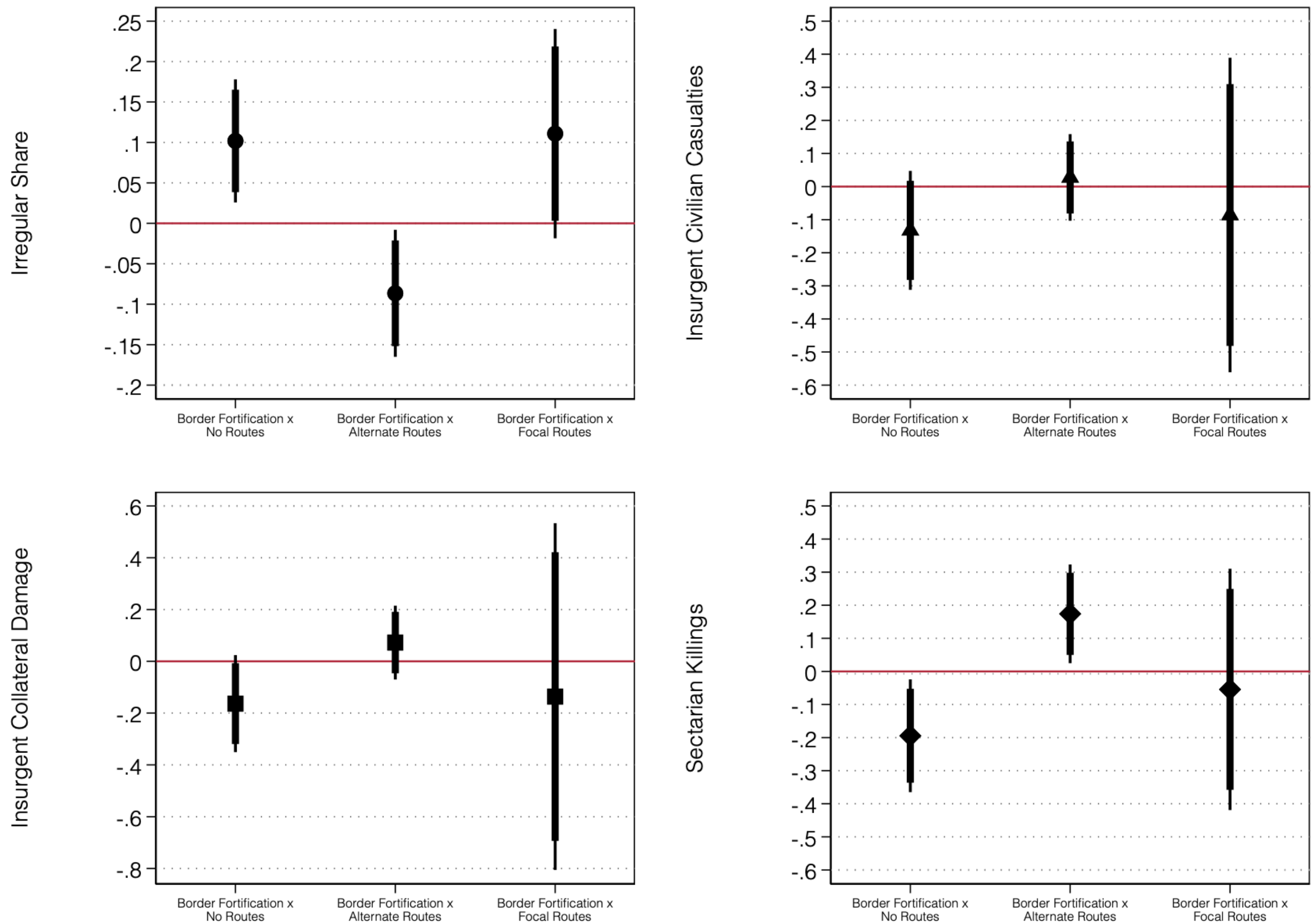
Laughlin (2019) shows that US border control efforts raised the value of trafficking routes in un-walled sections of the US-Mexico border, increasing violence in those areas as cartels competed for control of cross-border routes. At the same time. Chambers et al. (2021) show that border enforcement on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

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

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

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

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



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

D.3 Border Fortification and Foreign Subversion

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

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

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

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

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

Table D-3: Fortification Was Effective Against Tacit Sponsorship

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

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

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

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

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

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

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

D.4 Border Fortification and the Intensity of Enforcement

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

Table D-5: Border Fortification and the Intensive Margin of Fortification

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

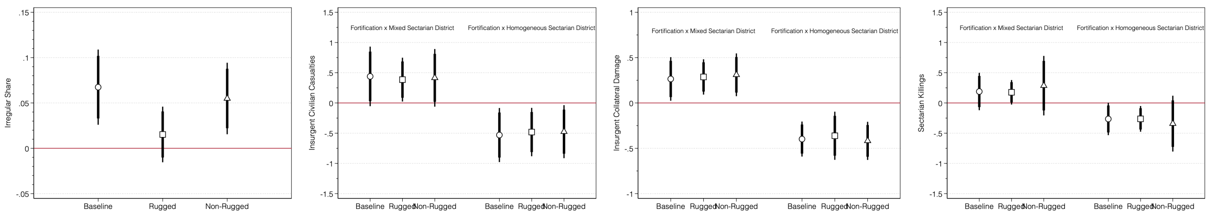
Note: Robust, district-clustered standard errors are in parentheses. The sample includes all districts in border governorates. In-group is an indicator for homogeneous sectarian districts–the constituent term is absorbed by district fixed effects. Controls are described in Table 1. Civilian victimization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

D.5 Border Fortification and Terrain Ruggedness

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

Figure D-3: Border Fortification and Terrain Ruggedness

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



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

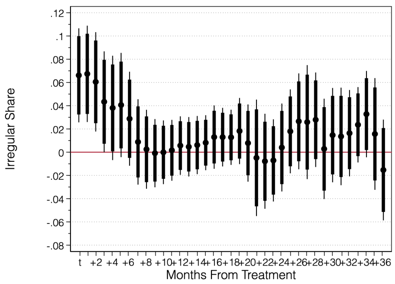
D.6 Temporal Dynamism in the Effect of Border Fortification

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

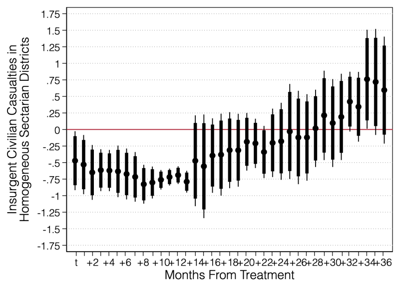
$$Y_{j,t+n} = \alpha_j + \beta_t + \delta(\text{BorderFort}_{j,t}) + \gamma_{X_{j,t}} + \epsilon_{j,t}$$

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

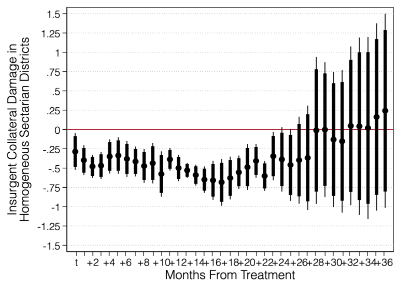
(**a**) Irregular Share



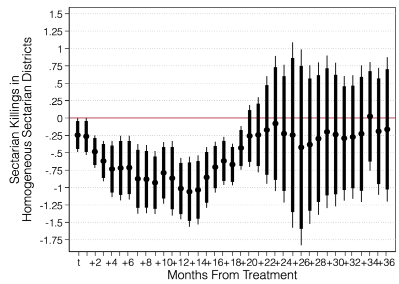
(**b**) Insurgent Civilian Casualties



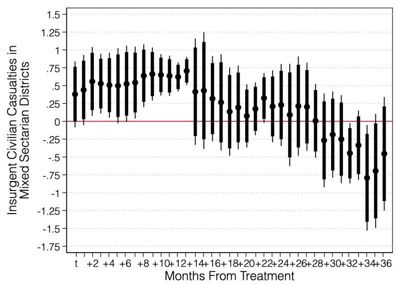
(**c**) Insurgent Collateral Damage



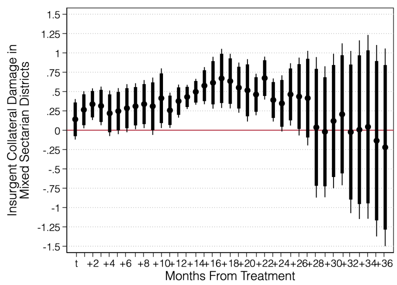
(**d**) Sectarian Killings



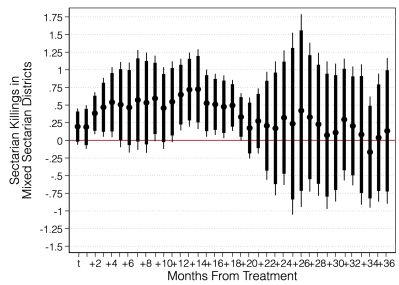
(**e**) Insurgent Civilian Casualties



(**f**) Insurgent Collateral Damage



(**g**) Sectarian Killings

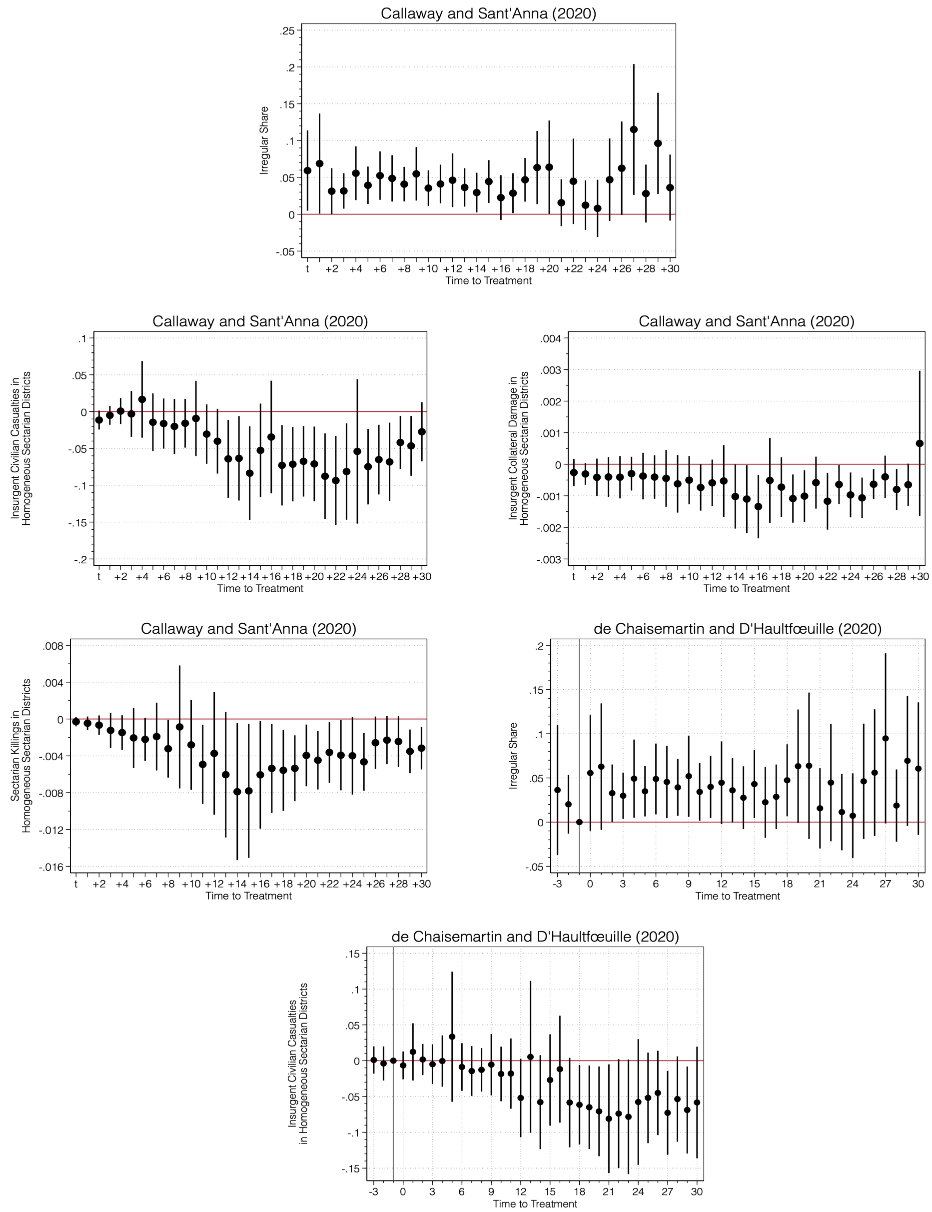


Note: Bars are 90 and 95% confidence intervals based on robust, district-clustered standard errors. Plots show coefficients from regressions of progressively longer leads of the respective outcome on border fortification. I study all periods from treatment onset t to 36 months after treatment $t + 36$.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

D.7 Alternative Difference-in-Differences Estimators

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

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

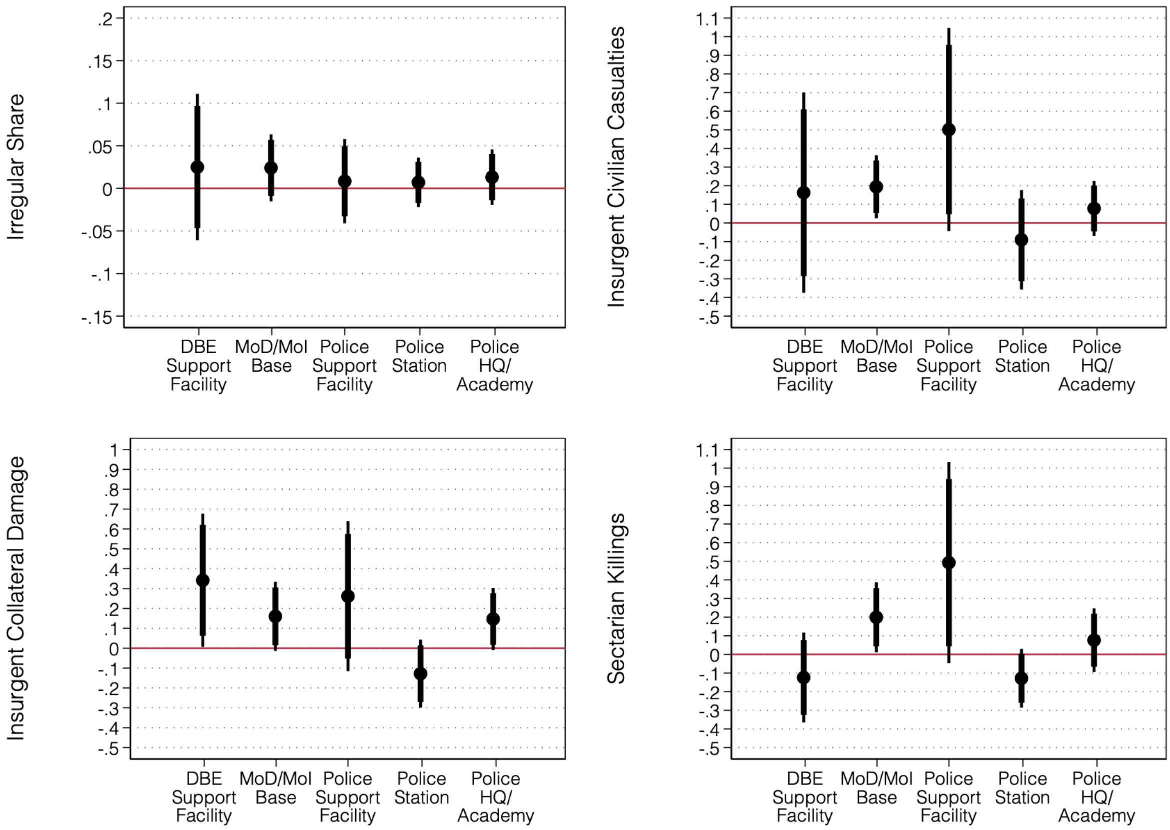


Note: Bars are 90% confidence intervals based on robust, district-clustered standard errors. Plots show coefficients from regressions of violence on border fortification. Estimates of civilian victimization come from models interacting border fortification with an indicator for homogeneous districts.

D.8 Placebo Tests with Non-Fort Security Infrastructure

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

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



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

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