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Conflict and counterinsurgency aid: Drawing sectoral distinctions ☆

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ABSTRACT

We examine the impact of counterinsurgency aid on conflict in Afghanistan from 2005 to 2009. To enable this analysis we combine unique aid project data from NATO, household data from the Afghan government, and conflict data from US government sources. Our panel data analysis accounts for district and time period fixed effects across 398 districts and 57 months. Projects in the health sector successfully promote stability, whereas those in the education sector actually provoke conflict. Our findings are robust to reverse causation, confounding aid programs, and other sources of endogeneity. The results shed new perspective on the 'hearts and minds' theory commonly discussed in this vein of inquiry.

1. Introduction

More than 15 years of Western engagement in Afghanistan and Iraq has brought peace and security to neither country. To date, these conflicts have collectively claimed the lives of over 200,000 civilians and 8000 coalition troops.[1] In Afghanistan, Taliban influence may have reached its peak since 2001 (Sadat and McChrystal, 2017), and suicide attacks continue to rock both nations' capitals (New York Times, 2018; Fox News, 2018). Together with hard counterinsurgency, a major cornerstone of both interventions has been the delivery of counterinsurgency aid for state-building and economic growth. To this end, the US Government alone has doled out over USD 80 billion in Iraq (SIGIR, 2013), and over USD 120 billion in Afghanistan (SIGAR, 2018). Historically, development aid from Western nations has been channeled into post-conflict settings via myriad development banks, national development agencies, supranational organizations, international NGOs, and local partners. But in Iraq and Afghanistan, the militaries of coalition member states joined the list of development actors. Provincial Reconstruction Team (PRT) military bases were erected across both countries to administer counterinsurgency aid (hereafter referred to interchangeably as *reconstruction*) in the domains of traditional aid providers, including health, education, and security. This paper examines how

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[1] Coalition deaths are calculated from http://icasualties.org, reporting 4853 deaths in Iraq and 3546 deaths in Afghanistan. Civilian deaths are aggregated across https://www.iraqbodycount.org and United Nations Assistance Mission in Afghanistan figures (UNAMA, 2009; 2018). Between 180,476-202,347 deaths are reported for Iraq, and 31,932 deaths are documented in Afghanistan. Civilian deaths in Afghanistan from 2001 to 2006 remain uncounted. Figures are current as of March 1 2018.

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conflict is influenced by reconstruction in each of these various sectors.

The US Army's Counterinsurgency Field Manual explicitly incorporates reconstruction as a key strategic pillar.[2] This effort is guided by a 'hearts and minds' credo, which maintains that counterinsurgency aid attracts community cooperation in the fight against rebels, and provides alternative economic opportunities for would-be insurgents. This belief is widely espoused by government officials, military personnel, analysts, and many academics. Berman et al. (2011) formalize hearts and minds theory with an information-sharing model of counterinsurgency in which reconstruction mitigates violence by winning popular support. In exchange for development projects, the community provides tips (i.e. intelligence) to counterinsurgents which help to capture or kill rebel forces.

In recent years, hearts and minds theory has gained traction while scholars seek to test its validity. Berman et al. (2011) show the US military-led Commander's Emergency Response Program (CERP) reduced conflict in Iraq during a period of increased troop strength.[3] In Afghanistan, Chou (2012), Child (2014), and Adams (2015) each attempt to replicate that analysis, but the impact of CERP across several time periods is generally indistinguishable from zero. Beath et al. (2017) suggest a large community development program in Afghanistan (NSP) improved security and pro-government attitudes. But Bohnke and Zurcher (2013) offer no such evidence in Afghanistan when examining aid more broadly defined. In the Philippines, Crost et al. (2014) find community-driven development spurred conflict; they suggest insurgents (in the battle for hearts and minds) strategically targeted program areas to disrupt community-government cooperation. Khanna and Zimmermann (2017) proffer strategic insurgent behaviour to explain why an anti-poverty program fuelled conflict in India. In Afghanistan, authors again invoke strategic behaviour to explain the destabilizing effects of CERP in contested areas (Sexton, 2016), and of NSP near the Pakistan border (Beath et al., 2017).

Taken together, the aforementioned literature suggests aid can build community support, but it can also attract conflict. Disruptions by insurgents can offset the stabilization from community. The net marginal impact of an aid project on conflict thus depends on two potentially countervailing forces: (i) enhanced community cooperation, and (ii) greater incentives for insurgency. The strength of each channel depends in turn on development program characteristics. In what follows we explain a number of reasons why the net effect of a project can depend on its sector.

Insurgents can perceive aid as a tool to consolidate government control and challenge rebel authority (Findley, 2018). So in the battle for hearts and minds, opposition groups in Afghanistan have attempted to frustrate development efforts by targeting program areas (Narang and Stanton, 2017; Wood and Sullivan, 2015). Security sector projects pose the most obvious threat to rebel control over territory. But education delivery is a key statebuilding activity that can also threaten rebel influence. The Taliban waged an active campaign of violence against educational institutions during our study period (HRW, 2006; Giustozzi and Franco, 2011). Those attacks were justified citing a number of strategic concerns: the curriculum is influenced by occupiers; teachers deliver political lectures against the resistance; schools extend the reach of government, are used as polling stations, and are visited by US forces (HRW, 2006; Giustozzi and Franco, 2011). Meanwhile, the Taliban did not object to health interventions because they were deemed *less* political, and even physically beneficial (AREU, 2016). The importance of clinics was recognized by the Taliban, and vaccination programs were approved and encouraged (Jackson and Giustozzi, 2012).[4]

Notwithstanding the above, a number of education-related grievances from the Taliban seemed quite ideological in essence. For example, Taliban complained about mixed-gender classrooms, girls schooling, secularization, penetration of Christianity, and failure to enforce the veil (HRW, 2006; Jackson and Giustozzi, 2012). This suggests the violent opposition to development efforts in education may have been driven as much by ideology than by tactical/strategic concerns. But in either case, there are good reasons to expect insurgent responses to vary across development sectors.

The community response to aid provision can be similarly contingent. Child and Scoones (2015) suggest innocuous projects help build community support, while controversial projects actually deter cooperation. Consistent with that theory, elders often expressed hostility to aid providers for disrespecting Afghan culture (Jackson and Giustozzi, 2012). Even while demand for modern education was significant, conservatives resisted programs due to secularization, girls education, curriculum design, foreign influence, and mixed-gender classrooms (HRW, 2006; Giustozzi, 2010; Giustozzi and Franco, 2011). It therefore seems community and rebel responses may *both* be sensitive to the development sector. Our analysis aims to capture the implications of that heterogeneity as a first-order point of interest.

Our study is set in Afghanistan, where raw data on aid and district characteristics were accessible only through physically available hardcopies in Kabul. Aid project data was procured from a rare extant DVD of NATO C3 Agency's *Afghanistan Country Stability Picture*. District characteristics were gleaned from the Afghanistan Central Statistics Organization's *National Risk and Vulnerability Assessments*. We merge these unique sources with conflict data from the National Counterterrorism Center's *Worldwide Incidents Tracking System*, and the US Department of Homeland Security's *Global Terrorism Database*. The combined dataset includes over 30,000 development projects for analysis, of which over 10,000 are PRT reconstruction projects led by coalition forces. Our panel incudes 398 districts which experienced more than 4500 attacks by insurgents between January 2005 and September 2009.

We measure the impact of sector-specific reconstruction on conflict by examining monthly changes using a district-level fixed effects model. Through our identification strategy (following Berman et al., 2011), we control for fixed district-level characteristics, recent district-level violence, contemporaneous nationwide trends, and the total volume of reconstruction activity. We find that PRT health projects improve stability, whereas PRT education projects actually provoke conflict. We demonstrate our findings are not explained by reverse causation, nor by confounding bias from civil aid programs. Importantly, our findings are also economically significant. In an average-sized district of 63,000 inhabitants, a one-standard-deviation increase in the number of health projects (corresponding to 1.6 projects/month) led to a reduction in expected violence by one third (from an average of 0.2 incidents/month). By contrast, a one-standard-deviation increase in education programming (1.4 projects/month) is associated with a 20% escalation in violent incidents.

We contribute to a growing literature using micro-level data to test the relationship between aid and conflict.[5] A sectoral distinction is drawn to accommodate heterogeneous responses to aid from both insurgents and communities. By demonstrating opposing marginal effects

[2] Civil Security, Civil Control, *Essential Services*, Governance, and *Economic and Infrastructure Development* comprise the Stability pillar of COIN strategy (see Figure 1-1, U.S. Army, 2006, emphasis added).

[3] The empirical and theoretical results of Berman et al. (2011) are consistent with the notion of complementarity between military control and the effectiveness of reconstruction, which is demonstrated in Iraq and Afghanistan (Berman et al., 2013; Sexton, 2016).

[4] The Pakistani Taliban, by contrast, explicitly targeted vaccinators. This difference may be attributable to the group's weak political strength in comparison to their Afghan counterparts, or because fake vaccinators helped surveil bin Laden in Pakistan (Abimbola et al., 2013).

[5] Works cited above constitute the relevant empirical studies examining aid's impact on conflict at the subnational level.

of reconstruction projects across development sectors, we provide a novel empirical contribution to the literature. Our conceptual framework offers a nuanced perspective on insurgent behaviour and community support, consistent with hearts and minds theory. Heterogeneity across sectors can arise from: (i) community preferences, (ii) insurgent ideology, or (iii) insurgent tactics. These three interpretations constitute unique (non mutually exclusive) causal mechanisms potentially underlying our results.

The remainder of this paper is structured as follows. Section 2 describes our data, section 3 presents main results, section 4 conducts robustness, and section 5 interprets our findings. Section 6 concludes, and is followed by a technical appendix.

2. Data

Throughout the analysis, our primary unit of observation is the district-month. Our sample covers 398 districts over 57 months (from January 2005 to September 2009), and thus contains a total of 22,686 observations.[6] Reconstruction volumes for a district-month are calculated as the mean number of projects in progress. Conflict levels are obtained by aggregating all attacks over the corresponding period. Reconstruction volumes are lagged one period to ensure we measure the impact of recent (not future) projects on violence. Both conflict and reconstruction variables are expressed in per-capita terms. For descriptive purposes we scale these measures to the average-sized district (63,000 inhabitants). Population data is for 2011/12, and obtained from the Central Statistics Organization of Afghanistan.

2.1. Reconstruction

Reconstruction data is from NATO C3 Agency's Afghanistan Country Stability Picture (ACSP). The ACSP is an ostensibly comprehensive database on reconstruction and development projects across Afghanistan (from January 2002 to September 2009). The database covers all projects funded by PRTs, USAID, Combined Security Transition Command, and a host of other donors (including World Bank, WHO, and UN agencies). The ACSP contains detailed information on timing, location, and sector of projects. Due to inconsistent transliteration of location names, we invoke the ESRI World Gazetteer and digital mapping software to district-locate a considerable share of projects included in our sample.

While the ACSP falls short of providing complete data coverage of all reconstruction programs, PRT data is particularly well documented. From 2002 to 2009, the ACSP contains data on 22,351 PRT projects (included among these is CERP, which comprise more than one third of PRT projects in the ACSP). The spatial distribution of mean PRT projects across districts is mapped in Fig. 1, and Fig. 2 depicts that distribution by sector (notice between education and health, no regional specializations are apparent). The left panel of Fig. 3 plots the level of PRT projects in our sample, across all provinces over time. Including all donors, the ACSP contains a total of 118,322 projects, and documents $28.2 billion spent.[7] A considerable share (73%) of all projects are not coded with accurate dates in the ACSP. Among PRT projects, 54% are missing the start and/or end date. Throughout the main analysis, projects with missing dates are dropped from our sample. Accordingly, we are left with 31,486 projects in total, of which 10,357 are PRT-led.[8] We have no reason to believe measurement error is nonrandom with respect to conflict, and differentially so across sectors. Nevertheless, we address this concern in Appendix A.1, and our results remain intact when incorporating partial data for an additional 3969 PRT projects.

Reconstruction activity is disaggregated by sector, and project examples under each sector group are offered in Table 1. Descriptive statistics of reconstruction and aid volumes (as well as conflict and district characteristics) are presented in Table 2. Table 3 contains variable definitions.

2.2. Conflict

Throughout the analysis our dependent variable is conflict (i.e. violence). We measure conflict primarily through the Worldwide Incidents Tracking System (WITS) - a US government database assembled by National Counterterrorism Center analysts. Data are gleaned manually from open media sources, including local media in foreign languages where linguistic capabilities permit.[9] The WITS catalogues all publicly known, premeditated, politically motivated violence directed at police, military, government, and civilians 'outside of war-like settings', but including ambushes, suicide attacks, and improvised explosive devices. The data cover 3222 incidents in Afghanistan from January 2005 to August 2009, and have been geocoded by the Empirical Studies of Conflict Project.

WITS data are supplemented with the Global Terrorism Database (GTD) managed by the US Department of Homeland Security's START Center at the University of Maryland. The GTD covers terrorist attacks across Afghanistan from 2001 to 2011. A terrorist attack is defined by the GTD as 'the threatened or actual use of illegal force and violence by a non-state actor to attain a political, economic, religious, or social goal through fear, coercion, or intimidation'. Although the GTD covers a longer time horizon, its coverage is more sparse. We were able to district-locate only 1428 incidents over the sample period corresponding to that of the WITS.

Because there is significant overlap between the two sources of conflict data, we merged databases to avoid double counting.[10] Specifically, for each day we invoked the source reporting the larger number of incidents. In line with related research, our measure of violence does not capture actions initiated by the state, such as police raids or counterinsurgency operations. Moreover, in keeping with the previous focus on government-targeted attacks, the vast majority of incidents in both databases involve non-civilian casualties (often exclusively). The spatial distribution of violence, averaged across the sample period, is reflected in Fig. 1. A monthly time-series of violent incidents by province is offered in the right panel of Fig. 3.

[6] We follow the 2005 Afghan Ministry of Interior administrative designation of 398 districts spanning 34 provinces.

[7] While other authors have examined project values (e.g. Berman et al., 2011), we use project counts because reliable cost data is available only for a subset of projects in the ACSP. Replicating our analysis using dollar-weighted metrics yields no obvious contradictions with the results presented here (but the explanatory power of each statistical model *is* considerably reduced).

[8] Over half the ACSP database consists of projects funded through either Afghanistan's Ministry for Rural Rehabilitation and Development (MRRD), or the Ministry of Finance (MOF). MRRD data do not contain project end dates, while MOF data are not geographically coded at the district level. As such, domestically funded reconstruction projects do not form part of our analysis. NSP funding (examined by Beath et al., 2017) was channeled through the line ministries, and therefore cannot form part of our analysis.

[9] Weidmann (2015) examines the accuracy of media-sourced conflict event data relative to military records in Afghanistan. The findings suggest measurement error is greater for incidents occurring in remote or less populated areas. But in general, reported event locations are accurate down to the district level of detail. WITS data have previously been used by Krueger and Maleckova (2003). See Wigle (2010) for full introduction to the database.

[10] WITS data have since been folded into the GTD officially, and the combined dataset is available from START.

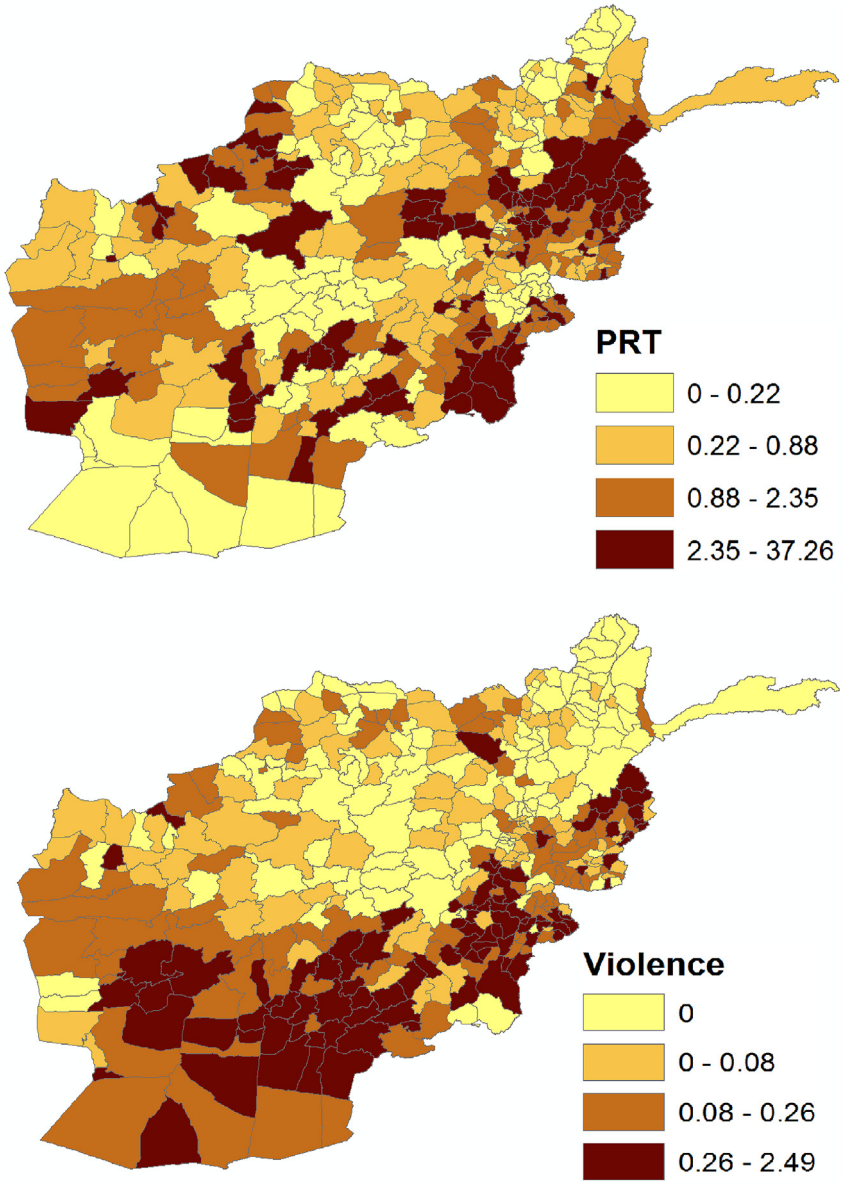

Fig. 1. Spatial distribution of PRT projects and violence. Maps reflect average ongoing number of Provincial Reconstruction Team (PRT) projects, and average rate of violent incidents. Rates are calculated per month, across 57 months for 398 districts. Both measures are expressed in per capita terms, and scaled to the average district population. For comparison, an average sized district is expected to witness 2.22 PRT projects and incur 0.20 violent incidents per month. The ranges provided in the legend are based on quartiles. Data are gleaned from the ACSP, WITS, and GTD.

2.3. District characteristics

To better understand the spatial distribution of reconstruction and conflict, we construct district characteristics with household data from the National Risk and Vulnerability Assessment (NRVA) survey carried out by the Central Statistics Organization of Afghanistan. We use two survey waves, from 2005 to 2007/8. Both surveys are statistically representative at the province level, but district sample sizes are conveniently large, such that data permit district-level inference. NRVA 2005 surveyed 392 districts, covering 385,519 individuals; NRVA 2007/8 surveyed 395 districts, covering 152,284 individuals. The time elapsed between survey waves is too large to confidently identify the impact of conflict or reconstruction on community attributes (or vice versa). As such, we construct district characteristics only to examine the spatial (cross-sectional) distribution of reconstruction and conflict. To pool our two periods for cross-sectional analysis, we need comparable indicators across survey waves for each field of interest. Some survey questions are identical across waves, and others are not. District characteristics for which the NRVA does not provide a consistent measure across waves are approximated by principal component analysis. These include the level of schooling, religiosity, women's rights, and access to health services.[11] Other characteristics include development preferences, hunger, road access (remoteness), and the presence of community development councils administering non-PRT aid (see Table 3).

3. Analysis

3.1. Spatial distribution

To begin our analysis, Table 4 investigates the spatial correlates of violence and project outlays (see Figs. 1 and 2). To this end, we stack

[11] For each such field of interest, we calculate the district-average response for each relevant survey question. Then we extract the first principal component score by district, and use its percentile rank (vis-a-vis other districts' scores) as the district indicator. The requisite theoretical assumption here is that the underlying factor approximated by the first principal component is robust to marginal changes in the input survey questions. It is important to note however, the resulting characteristics are used only in cross-sectional analysis, comparing ranks produced using identical factor loadings.

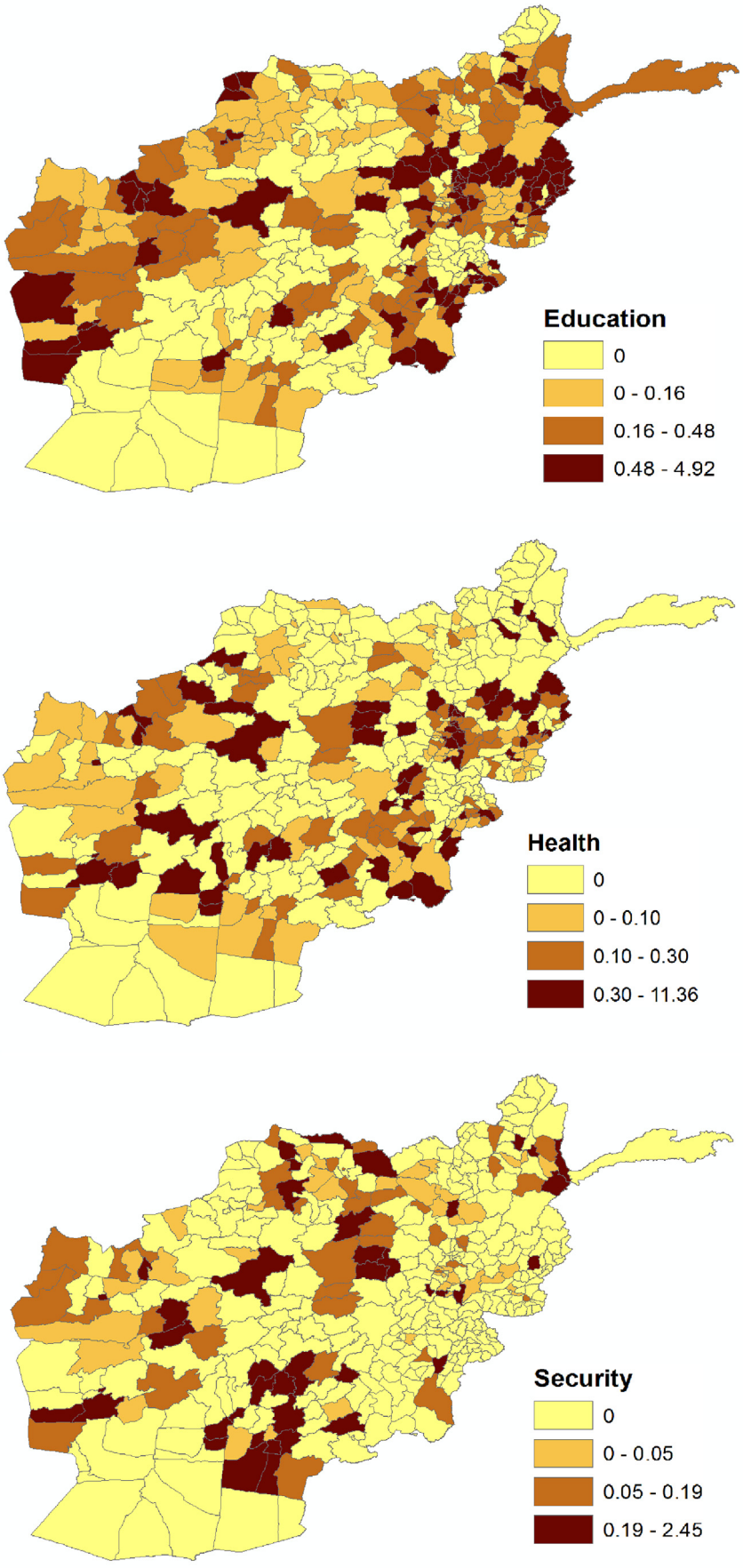

Fig. 2. Spatial distribution of PRT sectors. Map reflects average ongoing number of sector-specific Provincial Reconstruction Team (PRT) projects, calculated across 57 months for 398 districts. The measure is expressed in per capita terms, and scaled to the average district population. For comparison, an average size district is expected to witness, per month: 0.37 education projects, 0.19 health projects, and 0.07 security projects. The ranges provided in the legend are based on quartiles. Data are gleaned from the ACSP.

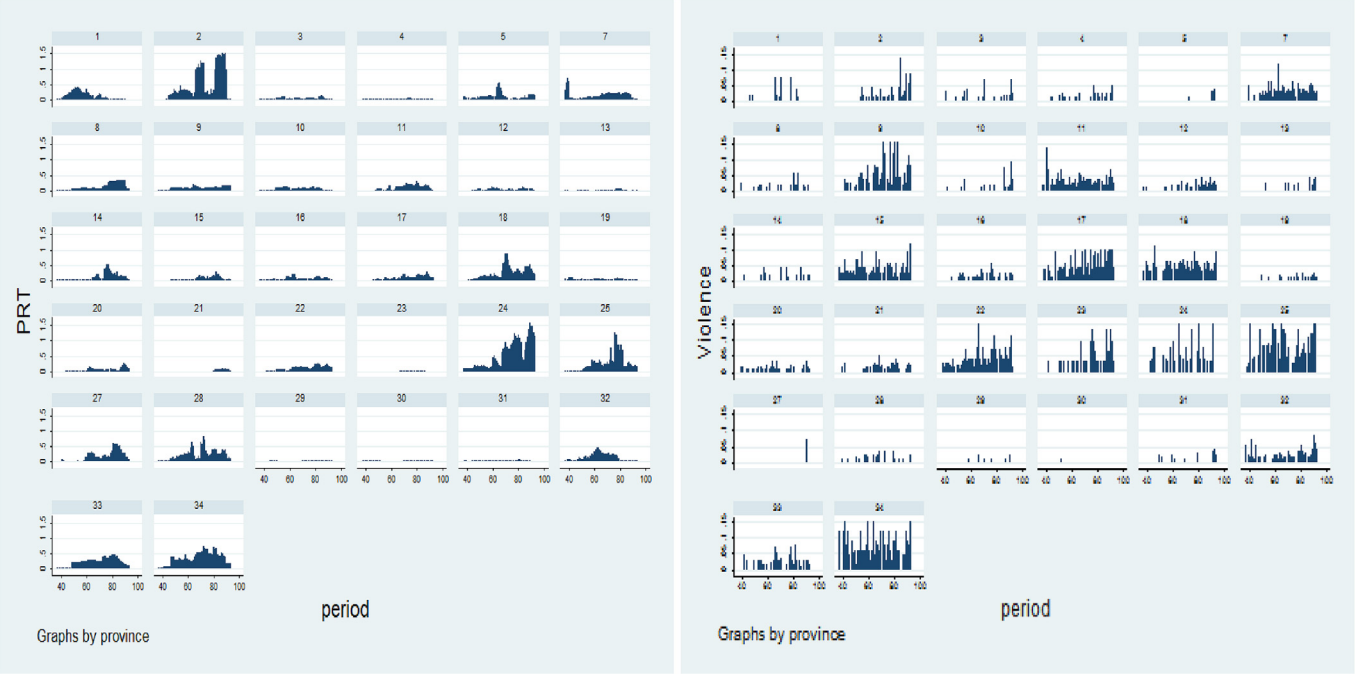

Fig. 3. Monthly variation in reconstruction and violence. Left panel of figure depicts monthly volumes of per-capita PRT projects across 34 provinces. Right panel reflects the incidence of violence, at the same level of aggregation. Data are gleaned from the ACSP, WITS, and GTD.

Table 1
Sector descriptions.

Sector	Typical Projects
Education	boys/girls schools; supplies; teacher training; vocational courses
Health	clinics; hospitals; supplies; medical training
Security	police stations; army barracks; checkpoints; fortification of civilian targets; prison repair
Commerce & Industry	market/bazaar infrastructure; training workshops; enterprise development
Agriculture	irrigation; livestock treatment; seed & fertilizer distribution
Energy	generators; wells; hydroplants
Water & Sanitation	wells; waterpumps
Environment	floodwalls; environmental protection; snow removal
Transportation	roads; bridges; highways
Emergency Assistance	refugee camps; humanitarian relief; compensation
Capacity Building	town hall; civic center; post office; district office
Governance	court facilities; district offices; governor compounds
Community Development	clothes; food; blankets; sports facilities; mosques; radio
Unknown	other

Sector groups are from Afghanistan Standard Industrial Classification of Activities maintained by Afghanistan Information Management Services. 'Typical Projects' describe common projects falling under each sector classification.

two cross-sections corresponding to the NRVA survey waves, yielding 777 district-wave observations. We then estimate the following statistical model using OLS:

$$Y_{iw} = \boldsymbol{\beta}\mathbf{X}_{iw} + \gamma_w + \epsilon_{iw}$$

where the outcome Y_{iw} varies across columns, from (per capita) violence to measures of (per capita) reconstruction activity. $\mathbf{X}_{iw}$ is a vector capturing a host of characteristics in district i, gleaned from NRVA wave w (as described in section 2.3). Standard errors are clustered at the province level.

Taken together, columns 1–2 of Table 4 indicate that both violence and reconstruction are more pronounced in districts with greater food security; greater road access; no community development council; and a smaller population. That both violence and reconstruction are concentrated in similar districts reflects a need to control for location when attempting to identify causal effects. Still, an identification challenge persists insofar as violence and reconstruction are correlated with omitted time-varying factors, and the correlation between those factors and reconstruction *differs across sectors*. Columns 3–5 examine the spatial allocation of sector-specific projects. There are very few significant determinants of individual sector programming, and there are none whose direction of correlation significantly changes across sectors. This helps allay our concerns regarding adverse project selection

Table 2
Descriptive statistics.

	Levels					Differences			
	N	Mean	SD	Min	Max	Mean	SD	Min	Max
Violence	22686	0.20	0.8	0	34	0.0015	1.06	−26	34
Reconstruction & Development:									
PRT projects	22686	2.22	6.9	0	342	0.0225	2.58	−118	143
Education (PRT)	22686	0.37	1.4	0	62	0.0028	0.84	−41	56
Health (PRT)	22686	0.19	1.6	0	96	0.0020	0.65	−39	41
Security (PRT)	22686	0.07	0.5	0	13	0.0002	0.24	−9	7
Aid projects	22686	12.34	30.8	0	1305	−0.2297	11.50	−1122	665
Education (Aid)	22686	5.60	18.8	0	228	0.0073	2.35	−107	85
Health (Aid)	22686	1.77	5.2	0	65	−0.0552	1.50	−42	53
Security (Aid)	22686	0.76	1.9	0	32	0.0180	0.51	−17	31
Commerce & Industry (All)	22686	0.33	1.0	0	42	−0.0076	0.47	−27	30
Agriculture (All)	22686	2.49	21.0	0	1288	−0.0488	10.29	−1120	665
Energy (All)	22686	0.46	3.0	0	145	−0.0002	1.30	−67	77
Water & Sanitation (All)	22686	0.61	8.3	0	383	−0.1129	4.03	−313	16
District Characteristics:									
Education preference	777	0.39	0.34	0	1				
Health preference	777	0.40	0.35	0	1				
Security preference	777	0.07	0.20	0	1				
Hunger	777	1.86	0.86	0	4.37				
Road access	777	0.84	0.94	0	4.97				
CDC presence	777	0.51	0.43	0	1				
Population (thousands)	777	63.0	170.3	2	3289				

Sample includes 398 districts across Afghanistan, and spans 57 months. Data are gleaned from the ACSP, WITS, GTD, and NRVA. Violence data are measured as incidents per average district population (per 63,000 inhabitants). Reconstruction and development (R&D) data are measured as mean concurrent projects per average district population. Unit of observation for violence and R&D data is the district-month. Projects in unmentioned sectors are tallied in the appropriate total project subcategories (either 'PRT projects' or 'Aid projects'). Unit of observation for district characteristics data is the district-survey wave.

Table 3
Variable descriptions.

Variable	Definition	Unit	Source	Frequency
Violence	acts of terrorism, as per WITS and GTD definitions	no. of incidents	WITS/GTD	daily
Reconstruction & Development:				
PRT projects	Provincial Reconstruction Team (PRT) led projects	no. of projects	ACSP	daily
Education (PRT)	PRT projects in the education sector	no. of projects	ACSP	daily
Health (PRT)	PRT projects in the health sector	no. of projects	ACSP	daily
Security (PRT)	PRT projects in the security sector	no. of projects	ACSP	daily
Aid projects	civil aid projects	no. of projects	ACSP	daily
Education (Aid)	civil aid projects in the education sector	no. of projects	ACSP	daily
Health (Aid)	civil aid projects in the health sector	no. of projects	ACSP	daily
Security (Aid)	civil aid projects in the security sector	no. of projects	ACSP	daily
Commerce & Industry (All)	all projects in commerce and industry	no. of projects	ACSP	daily
Agriculture (All)	all projects in agriculture	no. of projects	ACSP	daily
Energy (All)	all projects in energy	no. of projects	ACSP	daily
Water & Sanitation (All)	all projects in water and sanitation	no. of projects	ACSP	daily
District Characteristics:				
Education preference	education facilities are top-3 dev. priority (*shura*)	average (0–1)	NRVA	2 waves
Health preference	health facilities are top-3 dev. priority (*shura*)	average (0–1)	NRVA	2 waves
Security preference	disarmament of militias is top-3 dev. priority (*shura*)	average (0–1)	NRVA	2 waves
Hunger	unable to satisfy food needs of household (hh)	times/year	NRVA	2 waves
Road access	distance to nearest road (*shura*)	km	NRVA	2 waves
CDC presence	Community Development Council is present (*shura*)		NRVA	2 waves
Population	inhabitants	1,000s	CSO	2011/2012

Data are gleaned from Afghanistan Country Stability Picture (ACSP), Worldwide Incidents Tracking System (WITS), Global Terrorism Database (GTD), and National Risk and Vulnerability Assessment (NRVA). District characteristics variables are computed from male questionnaires, generally at the *shura* level (female questionnaires do not cover same topics). *All* category of projects includes those led by PRT, USAID, and other civil aid providers.

on time-varying characteristics correlated with violence.[12]

3.2. General reconstruction

To begin our panel analysis, we first follow previous authors by imposing homogeneous effects across reconstruction sectors. Conflict intensity in Afghanistan is highly seasonal, with the Taliban announcing the beginning of 'spring offensive' around April–May each year. If reconstruction activity is disproportionately concentrated in the fighting (off)season, we could erroneously attribute violence (stability) to reconstruction in the absence of time controls. We thus incorporate (57)

[12] Column 3 shows PRT education projects are strongly correlated with the presence of community development councils. In light of the negative correlation between these councils and violence (found here in column 1, and in Beath et al., 2017), any result suggesting PRT education projects *increase* violence could actually be understated, subject to confound from this separate source of development programming (for which we cannot control dynamically).

Table 4
Spatial allocation of reconstruction and violence.

	(1) Violence	(2) PRT	(3) Education	(4) Health	(5) Security
Preference			1.818*	0.895	1.803
			(0.0667)	(0.161)	(0.367)
Schooling	−1.01*	1.93	1.16	1.25	0.18
	(0.054)	(0.699)	(0.240)	(0.114)	(0.733)
Healthiness	−0.459	2.89	0.165	−0.788	−0.471
	(0.375)	(0.574)	(0.884)	(0.305)	(0.453)
Religiosity	0.467	8.31	1.33	1.87	1.02**
	(0.437)	(0.267)	(0.342)	(0.145)	(0.023)
Women	−1.75***	−5.73	−0.867	−0.309	0.557
	(0.000254)	(0.330)	(0.474)	(0.738)	(0.218)
Hunger	−0.876***	−5.384***	−0.679*	−0.368	−0.333
	(1.49e-06)	(0.00613)	(0.0848)	(0.104)	(0.118)
Roads	−0.406**	−3.405**	−0.337	−0.408*	−0.0966
	(0.0150)	(0.0117)	(0.289)	(0.0660)	(0.465)
CDC	−0.932**	5.513*	1.836***	−0.405	−0.121
	(0.0103)	(0.0995)	(0.00875)	(0.451)	(0.696)
Population	−0.515***	−4.48**	−0.634*	−0.733***	−0.339*
	(0.000)	(0.013)	(0.077)	(0.006)	(0.052)
Observations	777	777	777	777	777
R-squared	0.139	0.085	0.067	0.035	0.057

Sample includes 398 districts across Afghanistan, and covers two NRVA survey periods (2005 and 2007/8). Data are gleaned from the ACSP, WITS, GTD, and NRVA. Dependent variable is either violent incidents or reconstruction projects, per average-sized district (63,000 inhabitants). *Schooling*, *Healthiness*, *Religiosity*, and *Women* are expressed as percentile ranks, normalized between 0 and 1 (as discussed in section 2.3). Population is expressed in millions. Regressions are weighted by district population, and survey period effects are included. Standard errors are clustered by province. P-values are reported in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

month-specific dummies into our model, to fully condition our effects on nationwide trends. In column 1 of Table 5 we estimate the cross-sectional relationship between (lagged) reconstruction and violence, clustering errors at the province level. Mean projects are significantly positively correlated with violence in the cross-sectional setting. The incidence of greater violence in areas more concentrated with reconstruction activity may simply reflect the spatial selection of projects reflected in Table 4. Because reconstruction is viewed as a tool for peace, projects may be set where their benefits are most needed - in volatile regions. To address this concern, we use a first-difference approach in column 2:

$$\Delta V_{it} = \beta \Delta R_{it-1} + \gamma_t + \Delta \epsilon_{it} \quad (1)$$

By estimating the above equation, we evaluate the change in violence (ΔV) stemming from a within-district change of PRT outlays (ΔR). Districts are indexed by *i*, and months are indexed by *t*. Time-invariant district characteristics influencing both violence and reconstruction are swept out through first-differencing.

Table 5
Impact of reconstruction.

	(1)	(2)
Time controls	Y	Y
First differences		Y
PRT	0.0133**	−0.00500
	(0.025)	(0.118)
Observations	22,288	21,890
R-squared	0.033	0.012

Sample includes 398 districts across Afghanistan, and spans 57 months. Data are gleaned from the ACSP, WITS, and GTD. Dependent variable is violent incidents per capita. Reconstruction variable is lagged one period. Regressions are weighted by district population, and standard errors are clustered by province. P-values are reported in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

When estimating within-district effects in column 2, the amount of projects becomes an insignificant determinant of violence. This suggests the spatial correlation in column 1 may be driven by omitted time invariant factors. PRT projects are now *negatively* correlated with violence (p-value 0.118), which is broadly consistent with Berman et al. (2011). It is possible reconstruction generally mitigates violence in Afghanistan, although such an interpretation would be premature at this stage of the analysis. To ascertain whether the impact of projects differs across sectors, we next adopt a more disaggregated approach.

3.3. Sector-specific reconstruction

Based on our conceptualization of insurgent behaviour and community support, we expect the impact of reconstruction projects to vary by sector. Projects in education, health, and security are particularly interesting to examine from this vantage point. The ACSP also contains data on projects from ten other sectors (see Table 1). But qualitative research on this topic offers little guidance regarding how projects in those sectors may affect conflict differentially. So for ease of reporting, and to avoid attributing economic meaning to potentially spurious correlations, we have refrained from analyzing those sectors individually. We nevertheless control for their combined volume in the analysis that follows.

In Table 6 we disaggregate PRT projects into three mutually exclusive sectors and a catch-all residual category (suppressed). In this way we allow for heterogeneous effects across education, health, and security projects. We report cross-sectional results in column 1, which are purely correlational as noted in the preceding subsection. Next in column 2 we account for selection on time invariant unobservable characteristics by first-differencing (estimating equation (1), with **R** now a vector). The results provide compelling evidence that the effect of reconstruction activity varies by sector. PRT education projects lead to an uptake in violence, whereas PRT health and security projects are effective at improving stability.

By removing district effects through first-differencing, we overcome endogeneity from selection on fixed district characteristics. A dynamic

Table 6
Sector-specific impact of reconstruction.

	(1)	(2)	(3)	(4)
First differences		Y	Y	Y
Pre-existing trend			Y	Y
District-specific trend				Y
Education	0.0179	0.0243**	0.0286***	0.0286***
	(0.234)	(0.012)	(0.008)	(0.008)
Health	0.0560	−0.0585**	−0.0416*	−0.0419*
	(0.100)	(0.024)	(0.070)	(0.071)
Security	0.0509	−0.0295*	−0.0433*	−0.0433*
	(0.328)	(0.077)	(0.066)	(0.069)
Observations	22,288	21,890	21,890	21,890
R-squared	0.036	0.012	0.262	0.263

Sample includes 398 districts across Afghanistan, and spans 57 months. Data are gleaned from the ACSP, WITS, and GTD. Dependent variable is change in violent incidents per capita. Reconstruction variables are lagged one period. All specifications include controls for time period and residual PRT (reconstruction projects in sectors not explicitly reported in table). Regressions are weighted by district population, and standard errors are clustered by province. P-values are reported in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

source of endogeneity may still run through violence though, if decisions regarding project outlays are made on a continual basis, and related to the contemporaneous state of instability. To exclude the possibility that our result is a byproduct of dynamic selection on time-varying conflict, we include lagged violence as a control variable in column 3. Our results are similar under this specification.[13]

Next in column 4 we allow for district-specific trends in violence. Under a scenario in which increasingly conflicted districts undergo intensive education programming for unrelated reasons, we would be wrong to attribute violence to education projects. When including district-specific trends our results remain nearly identical, so we can rule out this scenario. Because district trends do not add explanatory power, whilst lagged violence does improve model fit, we opt for column 3 as our preferred specification:

$$\Delta V_{it} = \boldsymbol{\beta}\Delta\mathbf{R}_{it-1} + \gamma_t + \theta\Delta V_{it-1} + \Delta\epsilon_{it} \quad (2)$$

where i is the district index, t is the month index, V is violent incidents, $\mathbf{R}$ is a vector of reconstruction volumes (mean concurrent projects), and Δ is the difference operator. This identification strategy follows Berman et al. (2011) and Berman et al. (2013), except we aggregate observations to the district-month, rather than the district-half year.[14]

Our results are economically significant. For an average sized district, a one-standard-deviation increase in PRT education programming leads to an escalation in expected violence by approximately one fifth. By contrast, a one-standard-deviation increase in the level of PRT health projects brings about a reduction in expected violence by approximately one third. A one-standard-deviation increase in PRT security projects, however, appears to reduce expected violence by only 10%.

Using the statistical model of equation (2), we rule out selection on fixed district-level characteristics, recent district-level violence, contemporaneous nationwide violence, and the total volume of reconstruction activity. Added variable plots (unreported) confirm our results are not driven by a handful of outliers. Still, a remaining concern is that time varying district-level variables could influence both aid allocation and conflict. We do control for the overarching volume of reconstruction activity, however, so the concern here is restricted to time varying covariates which influence both violent outcomes and the project *mix* (as opposed to its level). In particular, one concern is that education projects are targeted at areas with increasing propensity for violence, and health projects are targeted at districts becoming more safe. This issue is examined thoroughly in the next section.

4. Robustness

4.1. Reverse causality

One issue with the results of Table 6 concerns the direction of causation. Fieldwork by the author and others (Adams, 2014; Sexton, 2016) suggests practical concerns regarding within-district reverse causality at low levels of temporal aggregation are limited. A combination of bureaucratic rigidity, idiosyncratic preferences of commanders, logistical limitations, and limited foresight effectively renders project outlays sufficiently exogenous with respect to contemporaneous violence. This is especially true once results are conditioned on district, month, total outlays, and trends in recent violence (as in equation (2)). Furthermore, the sectoral composition of programming is broadly predetermined, can be legislatively mandated as the outcome of lobbying/negotation by various donor government branches, and can altogether exclude security as a selection criterion.[15] In effect, short-run violence reduction is likely to be neither the only nor largest principle guiding project allocation. Nevertheless, we empirically test for reverse causality throughout the remainder of this section.

Importantly, it can be shown that reconstruction projects in education, health, and security Granger-cause violence. The converse is not true, which is evident from Panel A of Table 7. There we effectively re-estimate equation (2), but with reconstruction projects as the outcome, and (lagged) violence as the predictor. The first column examines the impact of violence on the total volume of PRT projects. Total programming intensity does not appear to be affected by recent violence. Next in columns 2, 3, and 4, we test whether education, health, or security projects (respectively) are selected dynamically on the basis of violence. There is no evidence to suggest conflict determines sector-specific project outlays. In fact, the signs of the coefficients actually suggest education projects are steered *away* from increasingly violent districts, while health (and security) projects are geared *towards* those districts.

Still, PRTs could allocate projects based on longer-term patterns of violence. To address this possibility, we aggregate observations to six-month blocks, and rerun our tests in Panel B. Reconstruction does not generally follow violent half-years, as indicated by the null result in column 1. Columns 2 to 4 again provide no evidence for the existence of strategic sector-specific outlays. As such, conflict (neither recent nor medium-run) does not appear to determine the timing of reconstruction outlays.

As a final check on reverse causality, we consider the possibility that sector-specific programs are allocated on the basis of expected (if not recent) violence. To verify this, we extract predicted violence from a simple forecasting model, and test whether next period's expected

[13] Including further lags of violence does not reduce the explanatory power of reconstruction variables, nor meaningfully change their effect sizes. In Appendix A.2 we address complications associated with including the lagged dependent variable as a control in this dynamic setting.

[14] A number of time-varying district-level factors (including some cross-sectional controls in Table 4) may constitute omitted variables in equation (1). We only possess NRVA data for two cross-sections spaced several years apart, so we cannot (without interpolation) incorporate those controls into a panel analysis at such fine level of temporal granularity. For lack of data we are unable to examine local economic conditions as a potential time-varying confound influencing conflict and aid allocation. Due to censorship, we are also unable to access data on troop movements, which could alleviate potential bias from omitted hard counterinsurgency. But since we measure the marginal impact of sector-specific projects (while controlling for total reconstruction), this bias is not of first-order concern.

[15] These claims are based on field interviews conducted by the author with reconstruction stakeholders in Kabul, November 2013.

Table 7
Reverse causality.

	(1) PRT	(2) Education	(3) Health	(4) Security
Panel A: 1-month *intervals*				
Violence	0.00646	−0.00830	0.00563	0.00282
	(0.641)	(0.173)	(0.132)	(0.251)
Observations	21,890	21,890	21,890	21,890
R-squared	0.017	0.008	0.006	0.006
Panel B: 6-month *intervals*				
Violence	0.0201	−0.0156	−0.00185	0.00688
	(0.641)	(0.309)	(0.797)	(0.124)
Observations	1617	1617	1617	1617
R-squared	0.117	0.053	0.032	0.036
Panel C: predicted violence				
Violence	−0.389	−0.0939	0.0929**	0.0113
	(0.214)	(0.386)	(0.016)	(0.653)
Observations	21,094	21,094	21,094	21,094
R-squared	0.017	0.009	0.006	0.006

Sample includes 398 districts across Afghanistan, and spans 57 months (9 half-years). Data are gleaned from the ACSP, WITS, and GTD. Dependent variable is change in mean concurrent daily projects per capita. All specifications are first-differenced. Violence variable is lagged one period. Time controls, and residual PRT (reconstruction projects in sectors not explicitly reported in table) are controlled for in all specifications. Regressions are weighted by district population, and standard errors are clustered by province. P-values are reported in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

violence is a determinant of contemporaneous project outlays. For this purpose, we predict violence on the basis of all significant lags (three, in a level equation), time period dummies, and district fixed effects. The correlation between our forecast measure and real outcomes is 0.31. In Panel C, predicted violence is forward-lagged, and included as a regressor to estimate its impact on contemporaneous project outlays. We find the coefficient on expected violence to be insignificant in most cases. In columns 2 and 3, the coefficient actually points in the direction *opposite* to that which supports a selection story. Panel C therefore strengthens our causal interpretation of the results in Table 6. Remarkably, even if we add expected contemporaneous violence as a control measure when estimating equation (2), our results from Table 6 remain robust (unreported). We therefore argue our results are not the consequence of sector allocations differentially selected on conflict patterns.

4.2. Confounding aid

Development aid from non-military donors exceeds reconstruction aid in Afghanistan by a factor of 5:1 (by project count, see Table 2). Insofar as civil aid projects are allocated on the basis of local security (or some correlated characteristic), and PRTs coordinate with civil donors, then a selection problem persists. Related work examining (in isolation) the impact of single development programs has failed to account for the slew of development agents active in conflict areas (Berman et al. 2011, 2013; Crost et al., 2014; Sexton, 2016). We contend this is an important potential source of confounding bias, and therefore control for non-PRT programming.[16]

Column 1 of Table 8 controls for projects funded by USAID - the largest donor in Afghanistan. Column 2 includes all remaining civil donors, including various UN agencies, development banks, IFIs, international NGOs, and so on. Column 3 includes the Combined Security Transition Command - a multinational effort to train Afghan security forces. Column 4 combines all non-PRT projects across the first three columns, and examines their aggregate effects. All our sector-specific results reported earlier are robust to the inclusion of civil aid controls. Interestingly, civil aid does not appear to have a significant effect on conflict. Education projects funded and administered by non-military development actors are not destabilizing, nor do health and security projects appear to alleviate conflict.

Table 8
Civil aid donors.

	(1) USAID	(2) Other	(3) CSTCA	(4) Aid
Education (PRT)	0.0287***	0.0286***	0.0285***	0.0286***
	(0.007)	(0.007)	(0.008)	(0.007)
Health (PRT)	−0.0417*	−0.0420*	−0.0423*	−0.0425*
	(0.069)	(0.068)	(0.066)	(0.066)
Security (PRT)	−0.0454*	−0.0432*	−0.0431*	−0.0451*
	(0.067)	(0.068)	(0.066)	(0.069)
Education (Aid)	−0.00152	−0.0111		−0.00186
	(0.141)	(0.346)		(0.110)
Health (Aid)	0.00650	0.00320		0.00260
	(0.416)	(0.198)		(0.153)
Security (Aid)	−0.0216	−0.0108	−0.0138	−0.0124
	(0.488)	(0.451)	(0.343)	(0.144)
Observations	21,890	21,890	21,890	21,890
R-squared	0.262	0.262	0.262	0.262

Sample includes 398 districts across Afghanistan, and spans 57 months. Data are gleaned from the ACSP, WITS, GTD. Dependent variable is change in violent incidents per capita. All specifications are first-differenced. Reconstruction and aid variables are lagged one period. Time controls, pre-existing trends, civil aid project volumes, and residual PRT (reconstruction projects in sectors not explicitly reported in table) are controlled for in all specifications. Regressions are weighted by district population, and standard errors are clustered by province. P-values are reported in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

5. Discussion

The foregoing results are interpreted in three ways below. Each interpretation is consistent with a generalized hearts and minds theory, and all are observationally equivalent in our study. The first interpretation offers a more complex appreciation of the community support mechanism underpinning development's stabilization potential. The second interpretation permits insurgents to pursue development goals directly. The third interpretation qualifies the 'strategic insurgent' extension recently proffered in the literature.

Hearts and minds theory suggests counterinsurgents build popular support by providing public goods in exchange for intelligence. Earlier work has theorized conditions under which projects are less likely to be successful at eliciting support (Berman et al. 2011, 2013). But our results suggest PRT education projects are actually *counter*productive to stability - a result not accommodated by prevailing formal theory. We thus offer a more careful interpretation of community preferences consistent with our findings. Favourable (PRT health) projects build support for pro-government forces and thereby improve security. Controversial (PRT education) projects degrade that support and shift public favour toward insurgents.

Perhaps more relevant than development preferences of community members, however, are the ideological perceptions of insurgents. The hearts and minds literature has thus far restricted the political ambitions of insurgents to contesting government power. A more flexible theory consistent with our findings could permit insurgents to focus on development agendas directly. Given ideological opposition to (PRT education) projects frustrating their cultural ambitions, insurgents may

[16] Confound from unrelated development programs is an outstanding concern in both observational and experimental studies (e.g. due to compensatory efforts of donors unaffiliated with RCT implementing agents). By virtue of our relatively comprehensive database, we are able to address such complications.

simply attack related institutions to remove them or influence development trajectories. In accordance with existing theory, (PRT health) projects not arousing ideological opposition still mitigate conflict by gaining popular support.

Recent work has extended the logic of hearts and minds theory by suggesting insurgents strategically target program areas to preclude pro-government forces from gaining footholds in the community (Crost et al., 2014; Khanna and Zimmermann, 2017). This argument can explain how development efforts generally exacerbate conflict, but deeper consideration is required to account for our mixed results. We suggest (PRT education and security) projects perceived as statebuilding activity are fiercely opposed by insurgents. Other (PRT health) projects do not threaten rebel authority, and again subdue conflict through community engagement.

The violence-reducing effect of security projects observed does run counter to the strategic rebel interpretation above. But aside from influencing rebel incentives, security projects also carry a direct material impact on the balance of power. Security projects may bolster government capacity to deter or pre-empt attacks by opposition forces (the WITS/GTD data catalogue only insurgent-initiated events). Hence, we interpret this result as evidence for the success of hard (rather than soft) counterinsurgency.

It is also worth noting civil aid had no significant effect on conflict in our setting. But civil aid differs from PRT aid in important ways to explain this discrepancy. The provision of civil aid is typically not conditioned on counterinsurgent control of an area, and is therefore not expected to strengthen community cooperation on theoretical grounds (Berman et al. 2011, 2013). Moreover, the involvement of foreign militaries may spark controversy around a project otherwise benign under civil leadership (PRT projects may draw ire from community, and resistance from rebels, in ways that civil aid does not).

6. Conclusion

Our sample period (2005–2009) coincides with an intense phase of Taliban assault on educational institutions, which subsided considerably from 2010 onward (Giustozzi and Franco, 2011). Alongside this transition was a clear shift in official policy. While the Taliban code of conduct (*Layha*) legitimated attacks on nonconforming schools in 2006, it dropped that approval in 2009 (see Munir, 2011). Thereafter, schools reopened through local-level negotiations which ceded significant control to the Taliban for curriculum design, textbook choice, and teacher selection (AREU, 2016). The causal pathway between Taliban policy, attacks, and negotiation is not especially well understood. As such, the reduction in attacks may have ultimately been driven by either: (i) pressure from communities to allow schooling; (ii) Taliban success in negotiating key ideological sticking points; or (iii) strengthened political power through territorial expansion (Giustozzi and Franco, 2011). So while three interpretations remain inseparable in our study, examining post-2009 data could help disentangle the underlying channels of influence.

Along similar lines, more work is needed to draw out conditions under which aid is likely to reduce conflict. With growing availability of high-quality granular data on conflict, aid, and public opinion, scholars are increasingly able to shed light on causal processes linking these phenomena. Within the hearts and minds conceptual framework, both community preferences and rebel incentives (be they strategic or ideological) remain largely unexplored. Theoretical advances focussing on these dimensions will be helpful in guiding further empirical analyses in this vein of inquiry.

A. Appendix

A.1. Missing data

Although NATO C3 Agency proclaimed the ACSP to be a comprehensive list of development activities in Afghanistan from 2002 to 2009, it is doubtful all projects were individually coded into the database. However, we have no reason to suspect miscoding is systematically related to violence and reconstruction in a way that could explain differential effects across sectors. Such bias would require education projects to be included in the database when unanticipated conflict is on the immediate horizon, and health projects to be included prior to sudden improvements in stability.

Many projects in the ACSP are missing start or end dates. Because our main analysis is based on first differences, it is actually possible to incorporate partial project data we have thus far excluded. Our identification is leveraged off the timing and location of project commencements and project completions. Missing a start (end) date for a project does not preclude us from incorporating its end (start) date into our analysis. Of course, these partial data may be less reliable than complete project data, and including data subject to classical measurement error can attenuate our coefficient estimates. On the other hand, excluding these data amounts to systematic underreporting of project volumes, leading to overstated effect sizes at best. At worst, this could result in directional bias if data-coding errors are systematically related to imminent violence, and differentially so across sectors. We thus revisit our main analysis including all available partial project data.

When we incorporate these additional projects, our database coverage expands significantly. The total amount of projects increases from 31,486 to 36,947; PRT projects in particular increase from 10,357 to 14,326. The results obtained from this expanded database qualitatively match those presented in the main analysis. In Tables A1 and A2, we reproduce Tables 2 and 6, respectively. The descriptive statistics in Table A1 closely resemble the corresponding figures in Table 2.[17] In Table A2, the coefficients on PRT projects are somewhat smaller than in Table 6. This potentially reflects attenuation bias from (classical) measurement error in the revised sample, systematic under-representation of projects in the main sample, or both. Because all coefficients shrink in absolute value, there is little reason to believe directional bias induced by measurement error could explain our differential results across sectors.

A.2. Anderson-Hsiao 2SLS

Following the estimation method of two seminal papers in this line of inquiry, we include a lagged DV to account for short-term, district-specific trends (Berman et al. 2011, 2013). When naively estimating equation (2) *without* the lagged DV term, we account for both district-specific and month-specific factors (through first-differencing, and through the inclusion of γ_t, respectively). But under that specification there remains serial correlation in violence at the district level. We estimate level equations (unreported) in which violence is a function of district FE, and multiple lags of the DV. We find that three lags of violence are significant predictors of contemporaneous violence, conditional on time-invariant

[17] Note we only report the descriptive statistics in first differences in Table A1 because of missing start/end dates.

district characteristics. Following Heckman (1981), we therefore consider a state-dependence model to be most appropriate in this empirical setting.[18]

Anderson and Hsiao (1982) indicate that estimating the state-dependence model in equation (2) directly by OLS can yield inconsistent estimators since the error term is correlated with the lagged DV by construction. They suggest an alternative consistent IV estimator in which higher order lags (in levels or differences) of the DV are used as instruments for the first order lag. Under this strategy the structural correspondence between the error term and the endogenous variable (our lagged DV) is effectively broken. In this spirit we estimate equation (2) using various lag structures as instruments for the lagged (differenced) DV. In column 1 of Table A3 we re-present our OLS baseline results for ease of comparison. In subsequent columns we instrument for the lagged DV term in equation (2) with differenced violence: lagged twice (in column 2); lagged twice and thrice (in column 3); and lagged three and four times (in column 4). We find the coefficients of interest (the effects of reconstruction activity) to be relatively stable across columns. Notably, using even more distant lags as instruments yields very similar results.[19]

That our results are essentially unchanged under the Anderson and Hsiao (1982) correction is perhaps unsurprising since the inconsistency is sourced from the estimate on lagged violence, while we are interested in the effects of PRT projects. Simulation findings by Judson and Owen (1999) suggest bias in PRT project coefficients would be small in this case. We include lagged violence in part to reduce variance in the error term, thereby sharpening precision for our coefficients of interest. To the extent that lagged violence is correlated with our reconstruction variables, a bias would be reflected in coefficients for the latter. It is therefore reassuring that the contemporaneous correlation coefficient between violence and reconstruction is only −0.002, 0.003, and −0.011, for education, health, and security, respectively. Importantly, none of these correlations are significant (the corresponding p-values are 0.74, 0.71, and 0.11).

Table A1
Descriptive statistics (extended sample).

	N	Mean	SD	Min	Max
PRT projects	22288	0.0275	2.97	−118	143
Education (PRT)	22288	0.0060	0.93	−41	56
Health (PRT)	22288	−0.0018	0.68	−39	41
Security (PRT)	22288	0.0070	0.42	−9	24
Aid projects	22288	−0.1580	11.65	−1122	665
Education (Aid)	22288	0.0078	2.36	−107	85
Health (Aid)	22288	−0.0552	1.50	−42	53
Security (Aid)	22288	0.0353	0.66	−17	31
Commerce (All)	22288	−0.0065	0.48	−27	30
Agriculture (All)	22288	−0.0285	10.43	−1120	665
Energy (All)	22288	−0.0048	1.32	−67	77
Water (All)	22288	−0.1071	4.04	−313	16

All values are expressed in first-differences. Sample includes 398 districts across Afghanistan, and spans 57 months. Data are gleaned from the ACSP. Reconstruction and development data are measured as mean concurrent projects per average district population. Unit of observation is the district-month. Projects in unmentioned sectors are tallied in the appropriate total project subcategories (either 'PRT projects' or 'Aid projects').

Table A2
Sector-specific impact of reconstruction (extended sample).

	(1)	(2)	(3)
First differences	Y	Y	Y
Pre-existing trend		Y	Y
District-specific trend			Y
Education	0.019**	0.020**	0.020**
	(0.020)	(0.037)	(0.040)
Health	−0.051**	−0.038**	−0.038**
	(0.022)	(0.037)	(0.040)
Security	−0.018	−0.023*	−0.024*
	(0.138)	(0.082)	(0.079)
Observations	21,890	21,890	21,890
R-squared	0.012	0.262	0.263

Sample includes 398 districts across Afghanistan, and spans 57 months. Data are gleaned from the ACSP, WITS, and GTD. Dependent variable is change in violent incidents per capita. All specifications are first-differenced. Reconstruction variables are lagged one period. All specifications include controls for time period and residual PRT (reconstruction projects in sectors not explicitly reported in table). Regressions are weighted by district population, and standard errors are clustered by province. P-values are reported in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

[18] The foregoing test estimates the equivalent of equation (1) (p.98) from Heckman (1981).

[19] In columns 2 and 3 of Table A3 we are able to easily reject the hypothesis that our instruments are weak, by comparing the Kleibergen-Paap rk Wald F-stat to critical values (from Stock and Yogo, 2005) approximately two orders of magnitude smaller. The highly significant negative coefficient on the lagged difference of violence (in columns 1 and 4) is mechanical. It is an artifact of mean-reversion in a data process characterized by intermittent violence at the district level.

Table A3
Anderson-Hsiao 2SLS-IV.

	(1) OLS	(2) IV (L2)	(3) IV (L2,L3)	(4) IV (L3,L4)
Education	0.0286***	0.0240***	0.0247***	0.0285***
	(0.008)	(0.008)	(0.007)	(0.005)
Health	−0.0416*	−0.0585**	−0.0575**	−0.0419*
	(0.070)	(0.015)	(0.016)	(0.050)
Security	−0.0433*	−0.0328**	−0.0349**	−0.0490**
	(0.066)	(0.047)	(0.041)	(0.021)
Violence (Lag)	−0.515***	0.007	−0.025	−0.498**
	(0.000)	(0.766)	(0.242)	(0.043)
Observations	21,890	21,492	21,094	20,696
Kleibergen-Paap rk Wald F-stat		4941	1637	3.97

Sample includes 398 districts across Afghanistan, and spans 57 months. Data are gleaned from the ACSP, WITS, and GTD. Dependent variable is change in violent incidents per capita. All specifications are first-differenced. Reconstruction variables are lagged one period. All specifications estimate equation (2). Column 1 replicates OLS baseline results in column 3 of Table 6. In columns 2, 3, and 4, we instrument for (differenced) lagged violence with, respectively: its second lag (differenced); its second and third lags (differenced); and, its third and fourth lags (differenced). Time controls and residual PRT (reconstruction projects in sectors not explicitly reported in table) are controlled for in all specifications. Regressions are weighted by district population, and standard errors are clustered by province. P-values are reported in parentheses (***p < 0.01, **p < 0.05, *p < 0.1).

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