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Modest, Secure, and Informed: Successful Development in Conflict Zones[†]

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Development programs are often tasked not only with improving human welfare, but also with helping local governments stabilize insecure or fragile environments (World Bank 2011; Berrebi and Olmstead 2011; US Army 2007).[1] Yet theory and evidence are both inconclusive on how development generates stability (Blattman and Miguel 2010, World Bank 2011). Increased economic activity in poorly controlled spaces might attract predatory violence (Hirshleifer 1989; Collier and Hoeffler 2004). Empirically, some aid is violence reducing (Berman, Shapiro, and Felter 2011—henceforth BSF). The literature has not explored optimal program design for violence reduction.

Using a panel on development assistance and violent incidents over the first five years of the Iraq War, we compare the effects of several development programs with different characteristics. We are guided by the predictions of an information-centric, or "hearts and minds" theory of counterinsurgency in looking for characteristics of effective program design in conflict or fragile environments, where assistance might be captured, extorted, or destroyed.

I. Complementarity of Service Provision and Security in Counterinsurgency

Imagine an environment in which insurgents ambush military patrols, or set improvised explosive devices (IED) to attack them. Preparations for these actions are likely to be observed by noncombatant community members who could report the insurgents to government forces. Those reports strongly complement government attacks on insurgents—government forces have superior technology and equipment—raising the government's chances of controlling the territory.

BSF model that environment formally as a three-sided game: rebels attack government; government attacks/captures rebels (*enforcement*) and provides services to communities—including development—communities choose whether to share information with government. The BSF model implies that increased service provision limits rebel violence by motivating community members to share information. BSF confirm that implication in data on Iraq.

Three aspects of that model are relevant to effective program design. First, the violence-reducing property of service provision requires *conditional* provision: the community benefits from services only if the government controls the territory. If the community benefited from services regardless of who won, provision would not motivate information sharing. Second, development programs are more violence-reducing the greater their value to the community, so programs informed by development experts will be more violence-reducing. Third, service provision and enforcement are complements in reducing violence. Berman et al. (2013)—henceforth BFST—discuss, and prove that

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[1] The 2011 *World Development Report* reports 1.5 billion people in countries affected by fragility, conflict, or violence. Berman et al. (2013) provide a more complete survey.

result analytically. Intuitively, services are more valuable when better protected from extortion, capture, or destruction. Conversely, enforcement is more effective the greater the flow of information motivated by service provision. This paper tests that first implication indirectly and the other two directly.

II. Data and Institutions

Insurgent violence is measured as attacks per capita against Coalition and Iraqi government forces (sometimes including civilians) as in BSF. "Significant activity" (SIGACT) reports provide the location and date of incidents between February 2004 and December 2009. (See BSF for full details and BFST for descriptive statistics.)

Extending BSF, we measure aid spending in Iraq using data from the US Army Corps of Engineers on several programs, including large-scale reconstruction spending as well as community level spending. Commanders Emergency Response Program (CERP) funds were allocated in small amounts, mostly by units at the brigade level and below. CERP spending was designed to work like government service provision, allowing military commanders to fund small projects for local communities with the aim of garnering the population's support and cooperation. CERP funded a broad range of projects, from salary payments to infrastructure, though the majority went to small-scale local public goods. Non-CERP projects were often large, typically infrastructure for water and sanitation, or transportation. The US Agency for International Development (USAID) also provided small-scale project funding in Iraq, through two programs, the $560M Community Action Program (CAP) and the $644M Community Stabilization Program (CSP). CAP created and trained local community groups who identified and implemented small infrastructure projects. CSP funded job creation through training and small infrastructure projects in key cities. CSP branded projects as being conducted by the local government. While not explicitly tied to the military campaign, as was CERP, CAP and CSP represented the civilian-administered programs most like CERP in project size and duration.

Provincial Reconstruction Team (PRT) locations are coded from maps provided by the State Department PRT office. US PRTs peaked at 20 by the second half of 2008. We count only US PRTs, as they are most relevant for US development program spending.

Troop strength is compiled by Lee Lindsay (2013) from press reports, counting maneuver battalions (forces responsible for territory) for February 2004 through December 2008. The possibility of endogenous force levels in a particular district $\times$ half-year is limited by organizational constraints. Despite an intention to allocate troops dynamically to the area of greatest need, the combination of troop rotation schedules, an organizational interest in maintaining unit cohesion (and developing local knowledge), staffing, and logistics constraints would tightly constrain a commanders' ability to reallocate troops in groups of less than about 800. Thus the marginal cost of additional enforcement was quite high. The establishment of PRTs would similarly require substantial lead time for recruiting, security, and logistics.

III. Results

Our empirical strategy is a first-differences design. We regress changes in violence on changes in aid spending, by program, controlling for lagged changes in violence and force levels:

$$\begin{aligned} v_{i,t} - v_{i,t-1} = {} & \beta_1(g_{i,t} - g_{i,t-1}) \\ & + \beta_2(m_{i,t} - m_{i,t-1}) \\ & + \beta_3(v_{i,t-1} - v_{i,t-2}) \\ & + \delta_t + \varepsilon_{i,t} \end{aligned}$$

where $v_{i,t}$ is the number of insurgent attacks (incidents) in district i in period t, $g_{i,t}$ is the amount of development spending, δ_t is a year effect to account for the secular trends in the war, and $m_{i,t}$ captures the number of maneuver battalions. The lagged change in violence, $v_{i,t}$, allows for short district-specific trends. Broad trends in violence differed in Sunni areas so we include a separate year effect interacted with the proportion of Sunni voters (see BSF for details).

The major challenge to identifying causal effects of g is possible endogeneity bias if, conditional on trends and other controls, increases in residual violence are anticipated and responded to by application of more services, enforcement or PRT resources (below) within a half-year

TABLE 1—DEVELOPMENT PROGRAMS AND VIOLENCE SUPPRESSION: ALTERNATIVE PROGRAMS

LHS variable Incidents/capita	(1)	(2)	(3)	(4)	(5)	(6)	(7)	Variable mean
CERP < $50K	−0.0639*** (0.0186)							$1.32
CERP > $50K		−0.0108* (0.00559)						$8.75
Non-CERP > $100K			0.000899 (0.000565)					$33.18
Non-CERP < $100K				0.00636 (0.0228)				$0.31
CSP					−0.0470* (0.0242)			$0.44
CAP						−0.0118 (0.0279)		$0.18
USAID							−0.00248 (0.00334)	$12.51
Troop strength	0.0479 (0.0349)	0.0451 (0.0400)	0.0386 (0.0325)	0.0349 (0.0337)	0.0466 (0.0328)	0.0354 (0.0333)	0.0301 (0.0339)	1.12
Lagged troop strength	0.0162 (0.0602)	−0.0373 (0.0704)	−0.0411 (0.0729)	−0.0421 (0.0732)	−0.0205 (0.0705)	−0.0424 (0.0728)	−0.0400 (0.0731)	1.13
R^2	0.222	0.204	0.180	0.179	0.194	0.179	0.180	

Notes: An observation is a district (N=103) × half year. District Karkh is excluded as national CSP programs are confounded with local programs there. Means are for levels (NT=927) though regressions are estimated in first differences (NT=824). Incidents are measured per 1000 population. Their mean is 0.587. Troop strength is measured in battalions per district. Regressions are weighted by population and include year effects, Sunni vote-year interactions, lagged incidents, and a constant. Standard errors are clustered by district.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

interval. That would cause our estimates to *understate* the violence-reducing effects of both g and m (and the complementarities below). The logistic constraints and lags built into the allocation and implementation of both development spending and effective patrols to a district make that type of rapid response (within a half-year) difficult but not impossible. BFST report additional specifications with various controls and find results to be robust.

Table 1 reports results: some small scale development spending is violence reducing ($\beta_1 < 0$) but other types are often not. Columns 1 and 2 show that the CERP coefficient is robust to including troop strength (m), which BSF did not—violence-reduction is not due to CERP proxying for troop presence.[2] Small CERP is six times more violence-reducing per dollar spent than is large. Columns 3–7 report the effect of other programs. Of those, only CSP shows evidence of being violence-reducing, while the rest do not (large non-CERP reconstruction, small non-CERP reconstruction, USAID spending through CAP, overall USAID spending). None of those programs show effects statistically different from zero. Excepting CSP, all

[2] The coefficient on troop strength suggests troops *increasing* violence—in contrast to BSF's prediction. We do not think that this refutes the theory for two reasons. First, SIGACTs are reported by troops, creating a spurious correlation with troop strength. Second, the negative coefficient after half a year suggests that new troops are initially challenged by insurgents, who then withdraw as the troops develop local situational awareness. Summed coefficients on contemporaneous and lagged troops are generally statistically zero (not shown), and negative in some specifications. Thus, accounting for upward reporting bias and positive endogeneity bias, described above, the cumulative effect of troop strength appears to be violence reduction (by an unknown amount) after about six months.

TABLE 2—COMPLEMENTARITY OF DEVELOPMENT SPENDING WITH TROOP STRENGTH

LHS variable incidents/capita	(1)	(2)	(3)	Variable mean
CERP < $50K	−0.0145			$1.32
	(0.0262)			
CERP < $50K × troops	**−0.0120****			
	(0.00542)			
CERP > $50K		0.000353		$8.75
		(0.00438)		
CERP > $50K × troops		**−0.00639*****		
		(0.00228)		
CSP			0.0325*	$0.44
			(0.0172)	
CSP × troops			**−0.0291*****	
			(0.00480)	
Troops	0.0655*	0.136**	0.0746*	1.12
	(0.0389)	(0.0517)	(0.0393)	
Lagged troops	0.0106	−0.0335	−0.0164	
	(0.0607)	(0.0684)	(0.0693)	
R^2	0.231	0.231	0.217	

Note: See notes to Table 1.

are statistically less violence-reducing than is small CERP. This contrast echoes the literature: economic activity is sometimes associated with more violence and sometimes with less.

Why are CERP and CSP violence-reducing while other programs are not, and why is small CERP especially so? The results in Table 1 are broadly consistent with the logic of BSF's "information-centric" approach: programs are effective when secure enough to be implementable; when their design is sufficiently informed about community preferences to be valued; and when implementation is conditional on government controlling the territory, which implies cooperation (i.e., information sharing). CERP programs are secure by design, as they are administered by military units. Small programs are also more easily conditioned on cooperation of communities, in contrast to large infrastructure projects, which are not easily revoked.[3]

Smaller programs such as CSP, CAP, and small CERP tend to be better informed, because they tend to be designed in consultation with the community.

Having discussed how those criteria can rationalize the results in Table 1, we turn now to testing those mechanisms. We examine whether security and development expertise complement programs for which we have evidence of effectiveness from Table 1, CERP and CSP.

Table 2 tests the hypothesis that security and development are complements by estimating whether the coefficient on an interaction of g and m is negative for small CERP, large CERP, and CSP (columns 1, 2, and 3). As implied by complementarity, estimated interaction terms are negative. All three coefficients are precisely estimated, rejecting the hypothesis of a zero coefficient in favor of complementarity.[4] Recalling our discussion of the possible endogeneity of both g and m, since that bias (despite our best efforts) would be positive, these estimates, if anything, understate complementarity. This evidence of complementarity between development

[3] Moreover, survey evidence from Afghanistan shows that CERP was generally implemented conditionally (see BSF, footnote 11), in contrast to how a development agency would typically implement programs—in accordance with a development objective rather than a security objective.

[4] BFST check robustness to measuring troop strength per capita rather than per district. That evidence for complementarity is weaker, but consistent with the previous results: two of the three interaction coefficients are negative, including the one which is statistically significant (for CSP).

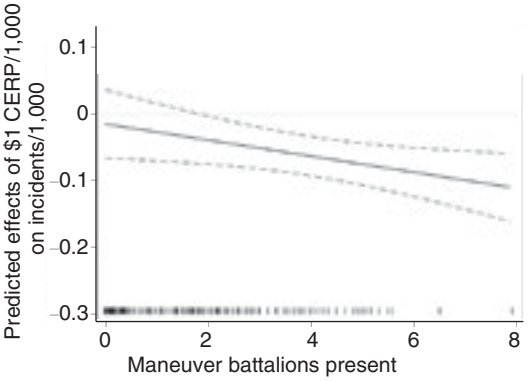

FIGURE 1. TROOP STRENGTH COMPLEMENTS DEVELOPMENT SPENDING

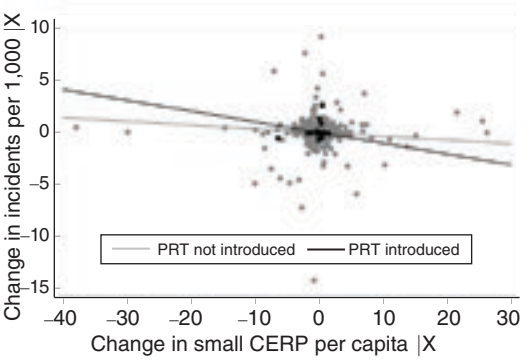

FIGURE 2. DEVELOPMENT EXPERTISE COMPLEMENTS SPENDING

spending and troop strength is illustrated in Figure 1, which plots the predicted marginal effects for small CERP by battalions present. Spending is more violence-reducing as battalions per district increases. Battalion density is illustrated in a rug plot on the x axis.

A final testable implication of the model concerns expertise. We seek to distinguish the mechanism in the model, which relies on delivering a service, g, of value to the community, from a class of "opportunity cost" models in which injecting wages and employment reduces violence regardless of whether services are valued. In other words, we test whether violence reduction is linked to quality of development programs.

We test the hypothesis that expertise complements spending by estimating the interaction of spending in the three effective programs with PRT presence in the district. As in our discussion of m and g, endogeneity bias would cause us to understate any violence-reducing effect if we thought that PRTs were instituted in anticipation of increased $\varepsilon_{i,t}$. Establishing a PRT would often involve recruitment, security, logistics and possibly construction, which would make response within a half year unlikely but not impossible.

Columns 1–3 of Table 3 report interaction tests. In all cases (small CERP, large CERP, and CSP) the interaction of PRT with spending yields a negative coefficient; the violence-reducing effects of spending are enhanced by PRTs. Interaction effects are statistically significant for large CERP (at $\alpha = 0.1$), and for small CERP ($\alpha = 0.01$). Small CERP projects are almost three times as violence-reducing with a PRT in the district. The reported coefficients are robust to removing troop strength and lagged violence (not shown). Figure 2 illustrates complementarity, showing increased violence reduction (slope) with a PRT present.

IV. Conclusion

Development spending is most violence-reducing when it is small, secure, and informed by experts, which can explain the effectiveness of small CERP relative to other programs (Table 1). Small projects are more likely to be violence-reducing, perhaps because they are better informed, or because they are conditional. Conversely, troop strength is more violence-reducing when small-scale development spending is present. Overall, evidence from Iraq suggests that, consistent with an information-centric ("hearts and minds") framework, aid spending which is small, conditional, secure, and informed creates incentives for community cooperation with government that less thoughtful spending does not. More generally, the theoretical and empirical evidence for complementarity suggests a practical lesson for aid programs in insecure spaces: a government wishing to suppress violence or improve welfare, at minimal cost, would mix development efforts with enforcement, rather than exclusively using one or the other.

TABLE 3—EXPERTISE COMPLEMENTS DEVELOPMENT PROGRAMS

LHS variable incidents/capita	(1)	(2)	(3)	Variable mean
CERP < $50K	−0.0357**			$1.32
	(0.0174)			
CERP < $50K × PRT	**−0.0667*** **			$0.42
	(0.0176)			
CERP > $50K		−0.00594		$8.75
		(0.00476)		
CERP > $50K × PRT		**−0.0164***		$3.42
		(0.00958)		
CSP			−0.0222	$0.44
			(0.0152)	
CSP × PRT			**−0.0354**	$0.31
			(0.0397)	
PRT	0.0750	0.109	−0.00337	0.29
	(0.0885)	(0.121)	(0.0840)	
R^2	0.236	0.217	0.196	

Note: See notes to Table 1.

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