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Do Working Men Rebel? Insurgency and Unemployment in Afghanistan, Iraq, and the Philippines

Eli Berman[1], Michael Callen[1], Joseph H. Felter[2], and Jacob N. Shapiro[3]

Abstract

Most aid spending by governments seeking to rebuild social and political order is based on an opportunity-cost theory of distracting potential recruits. The logic is that gainfully employed young men are less likely to participate in political violence, implying a positive correlation between unemployment and violence in locations with active insurgencies. The authors test that prediction in Afghanistan, Iraq, and the Philippines, using survey data on unemployment and two newly available measures of insurgency: (1) attacks against government and allied forces and (2) violence that kill civilians. Contrary to the opportunity-cost theory, the data emphatically reject a positive correlation between unemployment and attacks against government and allied forces ($p < .05$ percent). There is no significant relationship between unemployment and the rate of insurgent attacks that kill civilians. The authors identify several potential explanations, introducing the notion of insurgent precision to adjudicate between the possibilities that predation on one hand, and security

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measures and information costs on the other, account for the negative correlation between unemployment and violence in these three conflicts.

Keywords
Insurgency, opportunity costs, rebel recruitment, unemployment

The vast majority of aid money spent to reduce political violence is motivated by an opportunity-cost theory of distracting recruits. Two causal logics underlie this theory (United States Army 2006). The most commonly cited is that gainfully employed young men are less likely to participate in insurgent violence.[1] A slightly less prominent argument is that unemployment creates grievances, generating support for insurgent violence (Brainard and Chollet 2007, 3). This support could lead to more violence directly—through more recruits or enhanced fundraising—or indirectly—by reducing the willingness of a population to share information with counterinsurgents. Whichever causal pathway is posited, the testable implication is the same: a positive correlation between unemployment and insurgent violence.

The opportunity-cost approach is generally based upon a number of often implicit assumptions about the production of insurgent violence. Some of these include

- participation in insurgency is a full-time occupation, in the sense that individuals cannot be legitimately employed and active insurgents at the same time.
- insurgency is a low-skill occupation so that creating jobs for the marginal unemployed reduces the pool of potential recruits.
- the supply of labor is a binding constraint on insurgent organizations.

Each of these assumptions is questionable in some contexts, suggesting first that empirical testing is warranted, and second, that the relationship between unemployment and insurgency may be more complex than is commonly assumed.

Other causal channels predict a *negative* correlation between unemployment and violence. Suppose, for example, that the main constraint on the production of violence is the extent to which noncombatants share information about insurgents with the government (Kalyvas 2006; Berman, Shapiro, and Felter 2008). If counterinsurgents spend money to buy intelligence—as they routinely do—then as the local employment picture worsens and household incomes drop, the marginal dollar spent to buy information will go further and violence will fall. Alternatively, suppose that security efforts—establishing checkpoints and the like—reduce violence but also increase unemployment by impeding the movement of goods and services (Hendawi 2008). That would imply a negative correlation between unemployment and violence. Or fighting a perceived occupying force might be something people do out of belief in the cause but can do only once their basic needs are satisfied. If insurgency is a normal "good" in this narrow sense, then an improved economic situation

could lead to greater levels of participation and hence greater violence so that reduced unemployment causes more violence.

We investigate the relationship between unemployment and political violence using panel data on local unemployment and insurgent violence in three countries: Afghanistan, Iraq, and the Philippines.[2] These countries vary greatly both in geography and in the nature and intensity of the insurgencies they face. Yet, they yield broadly similar results.

The data rule out a positive correlation between unemployment and violence for all three countries: if there is an opportunity-cost effect, it is not dominant in any of them. Using micro-data from three countries provides plenty of inferential leverage. Assuming that the internal relationships between violence and unemployment within these countries are independent—a reasonable assumption, given their geographic spread and the different periods observed in each—then combining our results across countries allows us to reject the null hypothesis of a positive correlation at the 99.95 percent confidence level.

Why is the correlation of unemployment and violence generally negative? Existing data do not allow us to fully adjudicate between possible reasons, but we offer evidence that it is due to the relationship between local economic conditions and counterinsurgents' efforts to combat violence. Our findings are consistent with two hypotheses concerning counterinsurgency: (1) as local economic conditions deteriorate, government forces and their allies are able to buy more intelligence on insurgents (i.e., the price of information falls), and (2) actions taken to enhance security—establishing checkpoints, building walls, and the like—damage the economy.

The remainder of this article describes our effort to study the relationship between unemployment and insurgent violence in Afghanistan, Iraq, and the Philippines. First, we briefly review the existing literature. We then lay out a theoretical framework—including some alternatives, describe our data, report estimation results, and conclude.

Literature Survey

Three major theoretical arguments link unemployment and violence at the local level. The first is the opportunity-cost approach that first surfaces in Becker's theory of crime (Becker 1968). Grossman (1991) applies it to rebels' time allocation, predicting that as opportunities for potential rebels to work in legitimate occupations improve, the amount of time they will provide to insurgency declines.

The opportunity-cost approach is incorporated in Fearon's (2008) model that predicts insurgent violence will increase in income inequality, as relatively poor rebels see more to gain from expropriating resources from the relatively rich. This model links opportunity costs to a second theoretical mechanism—appropriation or rent capture—the idea that the greater the economic gains associated with controlling an area, the greater the effort rebels will invest in violent capture. Dube and

Vargas (2008), for example, report evidence that violence increases in oil-rich areas of rural Colombia when the price of oil increases. Similarly, Hildalgo et al. (2010) provide empirical evidence that economic shocks drive the rural poor in Brazil to invade large landholdings and that this effect is especially pronounced in areas of high land inequality and in areas with fixed-rent tenurial arrangements, which do not provide peasants insurance against income shocks. Blattman and Miguel (2009) provide a general survey.

A third major theoretical argument is the hearts-and-minds approach, which states that the key predictor of violence is the attitude of the population toward the government. That attitude in turn predicts whether insurgents can survive to conduct attacks against a militarily superior foe. This strain of thinking has been most prominent among practitioners of insurgency and counterinsurgency. Mao Tse-Tung famously argued the people are "the sea in which rebels must swim" (Mao 1937). Counterinsurgency theorists from the post-Colonial wars relied on similar arguments about the criticality of the population's attitudes,[3] as did the Iraq/Afghanistan cohort of Western counterinsurgents.[4] Importantly, this literature stresses that it is not the ability to recruit combatants that constrains insurgents but rather the ability to induce noncombatants to withhold information from counterinsurgents. Akerlof and Yellen (1994) present an analytical statement of this approach, arguing that excessive punishment will fail to deter urban street gangs if the community responds by withholding information police need to catch gang members. Evidence from captured internal documents of Al Qaeda in Iraq indicates analogous orders to avoid killing noncombatants (Combating Terrorism Center 2006).[5] A Taliban code of conduct released in the summer of 2009 contains explicit directives to "bring the hearts of civilian Muslims closer to them . . . " and to avoid civilian casualties.[6] Berman, Shapiro, and Felter (2008) apply "hearts and minds" logic to analyze the response of violence to reconstruction and social service provision programs in Iraq, testing the logic that these programs cause noncombatants to favor the government side, inducing them to share information with counterinsurgents.

Evidence generally supports opportunity-cost theory at the subnational level with respect to crime. Studies show that in the United States crime rates increase, as wages in the legal economy fall and as unemployment rises (Grogger 1998; Gould, Weinberg, and Mustard 2002; Raphael and Winter-Ebmer 2001). A similar pattern has been observed with respect to insurgency in rural Columbia where increases in prices of agricultural commodities predict reduced insurgent violence (Dube and Vargas 2008) and with respect to uprisings in Brazil where economic shocks drive land invasions by the rural poor (Hildalgo et al. 2010). These findings are consistent with cross-country evidence that low gross domestic product (GDP)/capita predicts civil wars (Collier and Hoeffler 2004; Fearon and Laitin 2003); that correlation holds even when using rainfall to identify exogenous variation in GDP/capita (Miguel, Satyanath, and Sergenti 2004). A notable exception is Benmelech, Berrebi, and Klor (2010), who—consistent with our results for Afghanistan, Iraq, and the Philippines below—find that unemployment and the incidence of suicide attacks in

Palestine are negatively correlated, even after including fixed effects and a rich set of covariates.

Little formal quantitative research has been reported which tests opportunity-cost theory in the context of political violence, or that tests hearts-and-minds theory, though the literature cited above is rife with supportive anecdotal evidence. This is unfortunate, as determining which mechanism is dominant—hearts and minds or opportunity costs—is critical to properly designing economic aid programs in efforts to rebuild social and political order.

Theoretical Framework

The opportunity-cost approach is a theory of rebel recruitment and retention along the lines of Becker's (1968) theory of crime. Imagine a potential insurgent choosing between work at wage, w, and insurgency. Many factors might influence the decision of those individuals, including commitment to a cause, risk aversion, attitudes toward violence, and so on, all of which would influence the threshold market wage, w^*, above which they would prefer work to insurgency. Let $F(w^*)$ be the cumulative distribution of threshold wages across both potential and current insurgents, and let N be the number of such individuals. For simplicity assume a single wage rate, w, in the economy. Then $N(1 - F(w))$ is the number of insurgents, a decreasing function of wages in the legitimate economy. Assuming that variation in unemployment is due to variation in labor demand, a negative correlation between wage rates and unemployment rates would yield a positive correlation between unemployment and the number of insurgents.

In this fairly general version of the opportunity-cost theory, insurgents are drawn not only from among the unemployed but also from among individuals in low-wage employment or who are out of the labor force. One could easily generalize to a model in which insurgency is a not a full-time job by adding an intensive (hours) margin, in which insurgent hours would also increase with market wages and decrease with unemployment. Even among individuals who are currently out of the labor force, the expectation of a high wage in some future employment, which would be precluded by being caught engaging in insurgency would yield the same insurgency-reducing effect of high wages. Overall, these complementary mechanisms all yield the same overall prediction of a positive correlation of unemployment and the number of insurgents.

We do not directly observe the size of the insurgent force, only the amount of violence. Does a larger insurgent force imply more violence? For very small insurgent forces that must be true, but the relationship could turn out to be non monotonic. If an insurgency became strong enough in numbers, it may reach a point at which more insurgents may induce the government (and/or its allies) to stop contesting the space, yielding *less* violence and *much less* measured violence (as measurement generally requires some degree of presence, with the notable exception of raids and air strikes).[7] For a full analysis of the extent to which space is contested in a "hearts

and minds" counterinsurgency, see Berman, Felter, and Shapiro (2008). They show that under fairly general conditions optimal effort by counterinsurgents increases monotonically with insurgent strength until the extreme case of uncontested regions is reached. Of the three conflicts we examine, the Afghan case is the only one in which there was substantial uncontested space. We treat this in our empirical analysis by separately examining the Pashtun majority regions that contain the areas that went uncontested because of a combination of insurgent strength and low Afghan and North Atlantic Treaty Organization (NATO) troop levels (Rhode and Sanger 2007).

To summarize in terms of an estimating equation, across regions $r = 1, \ldots, R$, an opportunity-cost theory predicts that the correlation between violence, v, and unemployment rates, u, will be positive, except if the region is uncontested, in which case the correlation should be zero (since the amount of violence is unrelated to the size of the insurgent force).

$$v_r = \alpha + \beta u_r + \varepsilon_r \tag{1}$$

The first thing to note about this equation is that if violence increases unemployment by reducing access to markets or depressing investment, then an estimate of β will be biased upward, exaggerating the true opportunity-cost effect. Alternatively, that coefficient could be biased in a negative direction—and in fact the estimated correlation between unemployment and violence could be negative—if rent capture, security measures, or information cost mechanisms are in play. While we do not observe those directly, it is useful to examine the implications for a regression involving observables. We examine those ideas in turn.

Rent capture, or predation, is the idea that violence is directed at the expropriation of economic rents, in the absence of some nonviolent means of dispute adjudication. Economic rents would then be an omitted variable with a positive coefficient in equation (1), plausibly having a negative correlation with unemployment—as economic activity is unemployment reducing. That would imply a negative bias on our estimated coefficient on unemployment, which, if rent capture dominated the opportunity-costs mechanism, could make it negative. If insurgency were a normal good, the bias and testable implications would be the same as those of rent capture.

Security measures provide a second avenue relating unemployment and violence. Successful security measures reduce violence by definition. At the same time, roadblocks, curfews, barriers, spot inspections, and other efforts to control the movement of goods and people might reduce economic activity, even accounting for the countervailing increase in economic activity due to improved security of people and property. That was the case in the West Bank and Gaza in the 1990s, for example, where the benefits of security measures imposed through movement and access restrictions accrued mostly to Israelis, while the economic costs were borne by Palestinians, generating very high unemployment rates (Fischer, Alonso-Gamo, and Erickson von Allmen 2001; Kanaan 1998). Kanaan emphasizes that even in a very

agricultural economy, uncertainty about the duration of a trip to market can severely depress investment, causing economic damage. Security measures, especially the building of large blast walls, also had widely discussed negative repercussions for the flow of goods, services, and people in Baghdad in 2007 (Crain 2007). If security measures are positively correlated with unemployment, then they imply a negative bias on an estimate of β in equation (1), which, if damage due to security measure dominates the opportunity-cost mechanism, could make the estimate negative.

Finally, information costs might decrease when unemployment rises, allowing for violence reduction through a separate security mechanism—the market for information, or tips. Counterinsurgents routinely use rewards to buy information on insurgents. The British government in Malaysia, for example, paid informants on a sliding pay scale based on the organizational role of the people killed or captured as a result of tips, informing on platoon commanders paid more than informing on rank-and-file soldiers and so on.[8] The U.S. Central Command REWARDS program, for example, was set up "to facilitate the capture of wanted persons or weapons" among other purposes (Multi-National Corps Iraq 2009, D-3). High unemployment is a symptom of low income, and the lower is income, the more information counterinsurgents can purchase (either in cash or in kind) with a given budget.[9] In other words, high unemployment should indicate a low price per tip. If those tips allow reductions in violence, and low tip prices are negatively correlated with the unemployment rate, then the estimate of β in equation (1) would again be biased downward, and could be negative if this information cost mechanism dominates the opportunity-cost effect.

All three of these alternatives predict a negative bias in the relationship between unemployment and insurgent violence. Anticipating a consistently negative coefficient, we need a way to distinguishing between explanations. Security and information mechanisms share the characteristic that unemployment proxies for factors that limit the operational effectiveness of insurgents. To measure that effectiveness, we calculate *insurgent precision*, the proportion of attacks on coalition forces that kill no civilians.

Here it is important to bear in mind that insurgents generally aim to avoid killing civilians. That is a direct implication of "hearts and minds" theory, in which noncombatants control operationally crucial information. In the literature survey, we provided a few examples from captured documents. One of those, the Taliban code of conduct (reportedly approved by the Mullah Omar) is quite explicit on this matter:

> Governors, district chiefs and line commanders and every member of the Mujahideen must do their best to avoid civilian deaths, civilian injuries and damage to civilian property. Great care must be taken . . . . . Suicide attacks should only be used on high and important targets. A brave son of Islam should not be used for lower and useless targets. The utmost effort should be made to avoid civilian casualties.[10]

These clear admonitions against civilian casualties imply that our insurgent precision measure is a reasonable proxy for the ease with which insurgents can attack.

Both the security effect and the information cost mechanisms predict a negative relationship between unemployment and insurgent precision while the predation mechanism has no firm prediction. Both security and information effects should be strongest in more densely populated areas where the risk of killing civilians in any attack is greater, and hence, the negative correlation between unemployment and insurgent precision should be strongest in densely populated areas.

In terms of an estimating equation, we have

$$p_{r,t} = \mu + \pi u_{r,t} + \delta d_{r,t} + \theta u_{r,t} d_{r,t} + \nu_{r,t} \tag{2}$$

where p is insurgent precision, d is population density, ud is the interaction of unemployment and population density, with the subscripts indexing by region and time. The shared predictions of both the security measures mechanism and the information cost mechanism are that precision and unemployment (proxying for improved security and/or low information costs) will be negatively correlated in the simple regression, equation (1), and the coefficient θ will be negative in equation (2), as high unemployment (again, as a proxy) reduces precision more in regions with dense populations. Unfortunately, we are unable to distinguishing between the security and information mechanisms using the data we have in hand and must leave that investigation to future work.

Data

We study the relationship between unemployment and violence at the local level in Afghanistan, Iraq, and the Philippines. In all three countries, we collected observations of these variables for the smallest geographical units for which reliable population and unemployment data were available, the district ($n = 398$) in Afghanistan, the district ($n = 104$) in Iraq, and the province ($n = 76$) in the Philippines.[11]

Our key dependent variable is the intensity of insurgent activity measured as the rate of attacks per capita against government forces and their allies. We generate these measures by aggregating incident-level data and focus on the rate of incidents because tightly geo-located data on Coalition and insurgent casualties are not publicly available for Iraq or Afghanistan. To maintain comparability of our estimates across countries, we use incident rates as our primary dependent variable.

For Iraq, we use two data sources on violence. The first are data drawn from "significant activity" (SIGACT) reports submitted by Coalition forces. These capture a wide variety of information about "executed enemy attacks targeted against coalition, Iraqi Security Forces (ISF), civilians, Iraqi infrastructure and government organizations" (Government Accountability Office 2007; Department of Defense 2008). Unclassified data drawn from the Multi-National Forces Iraq SIGACTS III Database provide the location (to approximately 100 meters), date, and time of attack for incidents between February 2004 and July 2008.[12] We filtered these data to exclude violence not directed at Coalition and Iraqi government targets leaving a data set of 148,546 incidents spanning February 2004–December 2007.

The unclassified information from the SIGACT data do not measure the consequences of attacks, so we supplement them with data from Iraq Body Count (IBC), which uses press reporting to identify incidents that kill noncombatants. The IBC data capture 13,335 incidents in which civilians were killed (that can be accurately geo-located). These incidents account for 49,391 civilian deaths. Each incident includes a reported target. We divide these killings into three categories, which will provide analytical leverage on the relationship between unemployment and violence:

1. Insurgent killings of civilians in the course of attacking Coalition or Iraqi government targets
2. Coalition killings of civilians
3. Sectarian killings, which includes all killing of civilians not falling in the other categories, capturing ethnic cleansing, reprisal killings, and the like

Insurgent precision is measured as the proportion of SIGACT attacks in a district/quarter that do not result in IBC insurgent-caused civilian casualties. That proportion averages 92.3 percent, weighted by population.

The analogues to SIGACTS incidents in Afghanistan are reported in two databases, the Joint Operations—INTEL Information System (JOIIS) and the Combined Information Data Network Exchange (CIDNE); these differ because of different recording practices and definitions of incidents. JOIIS incidents reported are very broad, including counterinsurgency incidents, criminal events, economic events, enemy action, explosive hazards, friendly action, friendly fire, noncombatant events, and "suspicious" events. A subset of these, designated "AGE," are enemy actions and explosive hazard events. CIDNE events include small arms fire, prematurely detonated explosives, mine strikes, mines found and/or cleared, improvised explosive devices (IED) found and/or cleared, IED explosions, IED hoaxes, suicide attacks, and indirect fire events. As in Iraq, incidents have time and location fields. We aggregate to district-months to match unemployment data in six separate months spread over 2008 and 2009.

To generate data on insurgent attacks in the Philippines, we coded unclassified details of over 22,245 individual internal security incidents reported by the Armed Forces of the Philippines from 1997 to 2006. These data were compiled from the original field reports of every operational incident reported during this period to the Armed Forces of the Philippines' Joint Operations Center by units conducting counterinsurgency and other internal security operations. Information coded from these reports include the date, location, and description of each incident, including the number of civilian casualties and who initiated the incident.[13] Each incident was assigned a unique location identification number that allows it to be plotted at the village level.

The lack of fine-grained data on unemployment is the limiting factor in our analysis in all three countries. In Afghanistan, we use five waves of the Afghan National

Quarterly Assessment Report (ANQAR) household surveys, which we have from September 2008 through September 2009.[14] In Iraq, three surveys capture unemployment at the district level: the Iraq Living Conditions Survey (ILCS), which was fielded in March and April 2004, the 2005 World Food Program Food Security and Vulnerability Analysis in Iraq (June and July 2005), and the World Food Program Food Security and Vulnerability Analysis in Iraq (November and December 2007). In the Philippines, we obtained provincial level unemployment rates based on the Republic of the Philippines Census Organization's quarterly Labor Force Survey (LFS) for 1997–2003 and 2006.[15]

To maximize the accuracy of our estimates, we focus on periods when the data on unemployment are available. For Afghanistan, we have between 363 and 365 (of the 398) Afghan districts for six separate months in 2008 and 2009. (The two dropped districts were lost because of unmatched district names.) For Iraq, we have 297 observations: 99 districts over three quarters when surveys were in the field (Q1:04, Q2:05, and Q4:07).[16] For the Philippines, this approach yields 546 observations: 78 provinces over seven years during which we observe both unemployment and violence (1997–1999, 2001–2003, and 2006). Table 1 provides population-weighted summary statistics for incident counts and other key variables.

Several facts stand out from Table 1. First, the insurgency in Iraq is substantially more intense than that in Afghanistan or the Philippines. When measured in incidents per thousand, the civilian casualty rate is an order of magnitude higher in Iraq than in the Philippines. Second, provinces in the Philippines are larger than districts in Iraq and much larger than those in Afghanistan. This means our estimates for the latter two countries allow more precise matching of incidents to unemployment and other demographic variables that may predict violence. In Iraq, our proxy for the proportion Sunni in a district is the vote share of political parties associated with the Sunni denomination at the governorate level, which averages 21 percent.[17] In the Philippines, we measure 5.4 percent of the population as Muslim, concentrated in the southern islands. In Afghanistan, we measure the percent Pashtun (averaging 38.5 percent), the proportion of males in a household (which may vary because of migration and averages 52 percent), and household size (which averages 9.6 persons).

Estimation

We seek to estimate the relationship between violence and unemployment in the equation (1), reproduced here with time subscripts and with a region specific fixed effect,

$$v_{r,t} = \alpha_r + \beta u_{r,t} + \gamma_t + \varepsilon_{r,t}, \tag{1}$$

where v measures the incidence of violence, u is the unemployment rate, r indicates region (districts in Iraq and Afghanistan, provinces in the Philippines), α_r are region-specific fixed effects, and γ_t are period effects. Bearing in mind that violence is

Table 1. Summary Statistics for Population, Unemployment, and Violence

	Variable	Mean	Std. Dev.	Min	Max
	Population (district)	605,340	464,106	10,966	1,624,058
	Unemployment (rate)	0.117	0.063	0	0.495
	Sunni vote share (governorate)	0.207	0.250	0	0.917
	Population density (1,000/km^2)	1.53	3.13	.000286	13.62
	SIGACT incidents/1,000	0.258	0.504	0	8.54
Iraq (district/quarter)	Iraq Body Count (IBC) incidents/1,000				
	Insurgent	0.005	0.011	0	0.240
	Sectarian	0.013	0.026	0	0.340
$N = 312$	Coalition	0.002	0.006	0	0.140
	IBC civilian casualties/1,000				
	Insurgent	0.018	0.043	0	0.611
	Sectarian	0.046	0.159	0	1.70
	Coalition	0.015	0.102	0	1.32
	Insurgent precision	0.923	0.159	0	1

(continued)

Table 1. (continued)

	Variable	Mean	Std. Dev.	Min	Max
	Population (province)	1,002,281	1,271,405	15,095	11,321,875
	Unemployment (rate)	0.105	0.039	0	0.201
	Percent Muslim	0.053	0.181	0	0.993
Philippines	Rebel initiated incidents/1,000	0.010	0.024	0	0.213
(province/year)	All noncriminal incidents/1,000	0.034	0.070	0	1.13
$N = 624$	Civilian casualties/1,000	0.015	0.042	0	0.659
	Insurgent precision	0.816	0.309	0	1
	Population (district)	77,7804	1,410,510	2,462	4,017,898
	Unemployment (rate)	0.241	0.206	0	1
	Percent Pashtun	0.385	0.389	0	1
Afghanistan (district/month)	Percent males	0.522	0.094	0	1
	Average household size	9.56	1.73	5.84	21.1
$N = 2{,}160$	AGE incidents/1,000	0.049	0.152	0	2.18
	JOIIS incidents/1,000	0.083	0.225	0	2.90
	CIDNE incidents/1,000	0.046	0.154	0	2.13

Source: Unemployment and population: Iraq Living Conditions Survey (ILCS; March and April 2004), World Food Program Food Security and Vulnerability Analysis in Iraq (June and July 2005; November and December 2007), Republic of the Philippines Census Organization, Labor Force Survey (LFS) for 1997 through 2003 and 2006, Afghan National Quarterly Assessment Report (ANQAR) surveys wave 1 (September 2008) through wave 5 (September 2009), LandScan™ population. Violence: Empirical Studies of Conflict Philippine project, internal security incidents, Armed Forces of the Philippines, 1975–2008; Multi-National Forces Iraq SIGACTS III Database; Joint Operations—INTEL Information System (JOIIS), Combined Information Data Network Exchange (CIDNE). Iraq Body Count. Other: Sunni vote share in Iraq is based on the December 2005 elections. Muslim population share in the Philippines is from the 1995, 2000, and 2007 censuses. Pashtun population share, male population share, adult population without any formal schooling share, and average household size in Afghanistan are from ANQAR. Note: All means are population weighted.

likely to reduce employment (by discouraging investment, consumption, and production), we interpret our estimate of the best linear predictor, β, as an underestimate of the causal effect of unemployment on violence.

Table 2 reports regression analysis for Afghanistan, Iraq, and the Philippines. The dependent variable in all specifications is the number of attacks against government forces—a category that includes both Coalition and Iraqi government forces in the Iraqi SIGACTS data. The key independent variable is the unemployment rate in that district/quarter (Iraq and Afghanistan) or province/year (Philippines). In Table 2, we report linear regressions on the number of attacks per 1,000 population.

We control for time-invariant region-specific characteristics in two ways. First, since both conflicts have an ethnic component we employ ethnicity controls: the Sunni vote share in the December 2005 election for Iraq, the Muslim population share for the Philippines, and the Pashtun population share for Afghanistan. Second, we employ region fixed effects that control for all time-invariant region-specific factors (including ethnicity measures). In all regressions, we use time fixed effects to control for secular trends and seasonality affecting the entire country. We also rerun the analysis in regions where the relationship may differ across regions. In Iraq, we focus on Baghdad (where population density may constrain rebels and coalition forces—as we discuss below). For the Philippines, we focus on provinces with more than 5 percent Muslim population as much of the insurgent violence during the period we study is driven by the secessionist movement in Mindanao, which is organized around religious grievances; the main insurgent group is the Moro Islamic Liberation Front (MILF). In Afghanistan, we report results separately for majority Pashtun areas, where violence is concentrated. As a robustness check, we also report separate estimates in Afghanistan for three different measures of violent incidents.

Our key finding is reported in Table 2: the estimated coefficient on unemployment is consistently negative in all three conflicts. Higher unemployment predicts less violence. This result remains true even after controlling for a wide range of possible confounding factors using time and space fixed effects. In Iraq, we reject a positive coefficient in three of the four specifications at the 95 percent confidence level, and the fourth at a 90 percent confidence level, as reported in the upper panel. The results are stronger for the entire Philippines (second panel), where a positive correlation is rejected with 99 percent confidence and consistently negative but insignificant in the more Muslim provinces where the southern insurgency is concentrated. (A separate and ongoing, communist insurgency was concentrated in the northern Philippines during the sample period.) In Afghanistan (third panel), the coefficient is positive in the cross-sectional regression, which includes only the ethnicity controls but omits other time invariant factors. The estimate is consistently negative and statistically significant once we control for all fixed effects, regardless of the measure of violence used. The p values for a one-tailed test for the AGE, JOIIS, and CIDNE measures, using fixed effects, are all .08 (by coincidence).

Table 2. Unemployment and Violent Incidents in Iraq, the Philippines, and Afghanistan

		Linear Regression			
	DV Region	Incidents /1,000 All	Incidents /1,000 Baghdad	Incidents /1,000 All	Incidents /1,000 Baghdad
Iraq (district/quarter)	Unemployment	−1.118** (0.542)	−4.254*** (1.460)	−0.709* (0.552)	−1.847*** (0.566)
	Observations	312	27	312	27
	R^2	0.23	0.35	0.06	0.16
	Controls	Ethnicity	Ethnicity	District FE	District FE
	DV	Incidents/1,000	Incidents/1,000	Incidents/1,000	Incidents/1,000
	Region	All	Muslim > 5%	All	Muslim > 5%
Philippines (province/year)	Unemployment	−0.131*** (0.031)	−0.148 (0.174)	−0.152*** (0.049)	−0.113 (0.132)
	Observations	624	96	624	96
	R^2	0.33	0.35	0.141	0.354
	Controls	Ethnicity	Ethnicity	Province FE	Province FE
	DV	AGE/1,000	AGE/1,000	JOIIS/1,000	CIDNE/1,000
Afghanistan (district/month)	Unemployment	0.032** (0.018)	−0.049 (0.035)	−0.063* (0.044)	−0.049 (0.035)
	Observations	2,160	2,160	2,160	2,160
	R^2	0.17	0.08	0.105	0.075
	Controls	Ethnicity	District FE	District FE	District FE
	DV	AGE/1,000	AGE/1,000	JOIIS/1,000	CIDNE/1,000
. . . of which Afghanistan majority Pashtun	Unemployment	0.092** (0.054)	−0.158** (0.085)	−0.191* (0.106)	−0.163* (0.088)
	Observations	923	917	917	917
	R^2	0.10	0.22	0.25	0.21
	Controls	Ethnicity	District FE	District FE	District FE

Note: All regressions include time/wave fixed effects and are weighted by population. The Afghanistan sample has five waves of ANQAR data and all waves span two months, except wave 5 spanning one month (results are similar using wave-month fixed effects). Robust standard errors clustered by district/province reported in parentheses. Variables described in note to Table 1. Iraq has 104 districts, the Philippines have 78 provinces, and Afghanistan has 363 districts included in the samples above. ***$p < .01$. **$p < .05$. *$p < .1$, all for one-tailed test with H_0: $\beta > 0$.

The statistical power of Table 2 lies not in the individual coefficient estimates but in their combination. Separated by time and space, each case yields additional information about the relationship between unemployment and insurgent violence. Interpreting exactly how that information should affect our beliefs requires being specific about the sampling scheme and correlation between countries. The three countries examined are a sample of opportunity, so they are not representative of the full population of countries experiencing insurgent violence. Nevertheless, our criterion for sampling was not the correlation of unemployment and violence, so that there is no particular bias incurred by conducting inference by combining information across these countries. (If there were, inference based on micro-data from any given country would be subject to the same critique.) Samples within countries are representative, so combining information across countries can be understood as testing a null hypothesis about the correlation of unemployment and insurgent violence in countries that were at risk of inclusion in our sample of opportunity.[18]

With parameter estimates from three separate conflicts, we can jointly test the hypothesis that the relationship between conflict and unemployment is nonzero, that is, H_0: $\beta_{Afghanistan} = \beta_{Philippines} = \beta_{Iraq} = 0$, using a Wald test. Assuming our asymptotic inference is valid in the three individual cases, then, by the Cramer-Wold device, a vector of the three elements will converge in distribution to a multivariate normal distribution. Because the countries in our sample all had a U.S. occupation or heavy involvement, the probability that we reject the null may be correlated, so it makes sense to explicitly incorporate that correlation into a joint hypothesis test. Figure 1 uses that distribution to graph the p value at which we reject the null that the relationship is nonnegative in all three conflicts against the correlation coefficient between the standard error estimates.[19] If the parameter estimates are perfectly uncorrelated, then each case truly constitutes an additional test. If estimates are perfectly correlated, the additional tests yield no additional information. The figure illustrates that moving from zero correlation to the coefficient of correlation, which maximizes the p value (.44) causes our p value to increase from .0041 to .0224.[20] However, we still reject the null in this two-tailed test with 95 percent confidence.

An alternative approach makes the statistical power that comes from combining estimates across countries more intuitive. If we assume complete independence of the t-ratios across countries, we can treat them as three independent hypothesis tests. Because the probability of three independent events occurring—in this case, falsely rejecting the null in each of the three countries—is just a product of the probability that the individual events occur, we can multiply probabilities to form a joint test. If we start with the prior that unemployment and violence are positively correlated in all three countries, that is, H_0: $\beta > 0$, then, based on the fixed effects estimates for the entire countries in the second column from the right, top three panels, the three joint one-tailed tests would be rejected at a significance level of $(1 - .1 * .01 * .1 =)$ 99.99 percent.[21] Alternatively, if the prior were that the relationship is nonnegative, that is, H_0: $\beta = 0$, then the three joint two-tailed tests would reject the null at $(1 - .2 * .01 * .2 =)$ 99.96 percent, again based on the second column from the right, top three

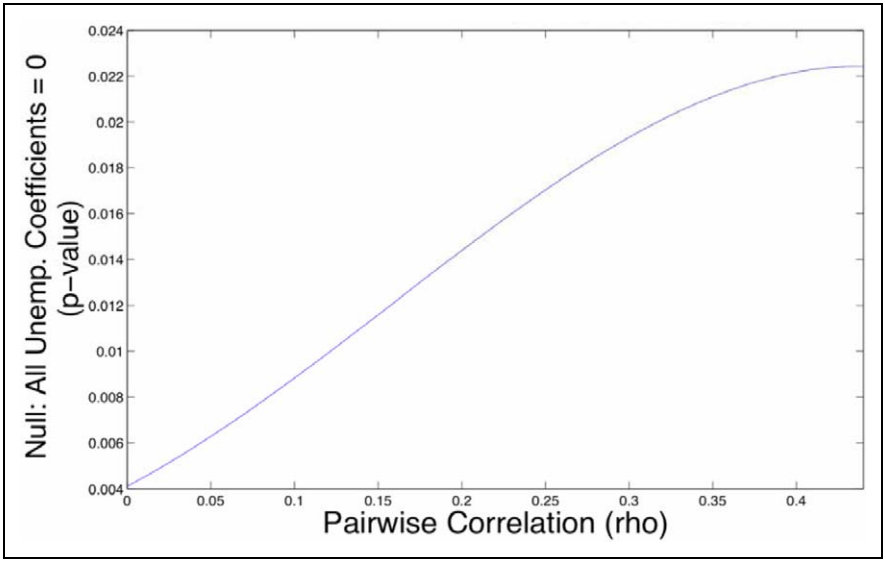

Figure 1. Rejection *p* value and test correlation

Note: "Rho" is the first multiplying parameter in the (*i*,*j*)th off-diagonal term of the variation covariance matrix ($\rho SE(\beta_i)SE(\beta_j)$) for the asymptotic joint distribution of the three individual country parameter estimates. The figure assumes that the pairwise correlation ρ is the same across the three country pairs. The *p* value of the joint test declines as rho increases above .45 because rejection in the high-*t* country (the Philippines) begins to dominate the inference. For a detailed explanation, see NBER WP #15547.

panels.[22] Even if we assume the *p* value maximizing degree of correlation in country parameter estimates above, we still reject the null with 97.8 percent confidence. The likelihood of falsely rejecting the null of a nonnegative relationship between unemployment and violence, when these results are combined across countries, is very small.

An alternative estimation approach when using count data is to assume a functional form and estimate using maximum likelihood methods. Table A1 of the appendix takes that approach, assuming a negative binomial probability distribution for the data generating process—a generalization of a Poisson process that allows for arbitrary variance. The advantage of this approach is a gain in both precision and consistency, if the assumed functional form is correct. The disadvantages are inconsistency if the distribution is not negative binomial and no latitude to allow for serial correlation in the error terms—running the risk of overstating precision. In practice, Table A1 reports qualitatively similar results. In the fixed-effects specification, which is preferred because it allows for the greatest control over unobserved district-specific contributors to violence, the correlation of unemployment and violence is negative in all the national samples. The basic finding is robust to nonlinear specifications.

Recall that while the prediction of the opportunity-cost model on the link between wages and the number of insurgents was unambiguously negative in the discussion above, the link to violence was muted by the possibility of uncontested spaces, allowing the possibility that increased unemployment (by reducing wages) may have no effect on violence. That is of particular interest in Afghanistan over the sample period, where the violence is concentrated in Pashtun majority regions that also contain a large number of areas that go uncontested by the governments for long periods. To ensure that our results are not driven by lack of contestation in some areas with high unemployment, Table A2 reports estimates including an interaction term for Pashtun majority districts. The negative correlation of unemployment and violence is in fact concentrated in these regions, providing strong evidence that the results for Afghanistan are not spurious. The coefficients on the interaction term are strongly negative. In a regression using only Pashtun regions (bottom panel of Table 2), we reject the positive null at statistical levels of .05. (In the remaining Pashtun minority regions, the fixed-effects estimates are two statistical zeros and one small positive coefficient, and for the CIDNE incident measure, the coefficient is .0033 and the *t*-ratio is 1.60). Those estimates are consistent with the idea that any positive correlation between unemployment and violence is being muted in the Pashtun majority regions by a weak correlation between insurgent strength and violence, but that idea cannot explain the negative coefficient estimates. Some other mechanism must be in play to explain these results.[23]

Figure 2 illustrates the mild negative correlation in Afghanistan between changes in AGE incidents and changes in unemployment, conditional on covariates. A regression line reproduces the coefficient in the second column of results of the third panel of Table 2, with slope −.049. The size of the circle for each district is proportional to population. It is clear from the figure that this negative correlation is not due to a few large outliers, but that the pattern is consistent across many districts.[24]

These negative coefficients are particularly striking when we consider that they are probably biased upward by a reverse-causal relationship in which violence increases unemployment through the damage it does to the economy. They are also large, at least in Iraq, indicating that a 10 percent increase in unemployment from the mean level (from 10 percent to 11 percent) is associated with .74 less attacks per 1,000 per district quarter for the entire country, about three times the sample mean. In Baghdad, the associated decrease is over twice that large, at 1.96 less attacks. While we cannot learn too much statistically from three waves of nine districts in Baghdad, it does illustrate the pattern we see throughout Iraq.[25]

These results do not imply that policies that increase employment rates cause violence, since the variation in unemployment rates that is negatively correlated with violence is not necessarily due to exogenous changes in labor demand. For instance, it may instead be due to enhanced intrusive security efforts that reduce both employment and violence. Yet, this negative correlation must lead us to doubt whether job creation policies actually decrease violence. What they certainly suggest is that the relationship between employment and violence is perhaps more complex than has

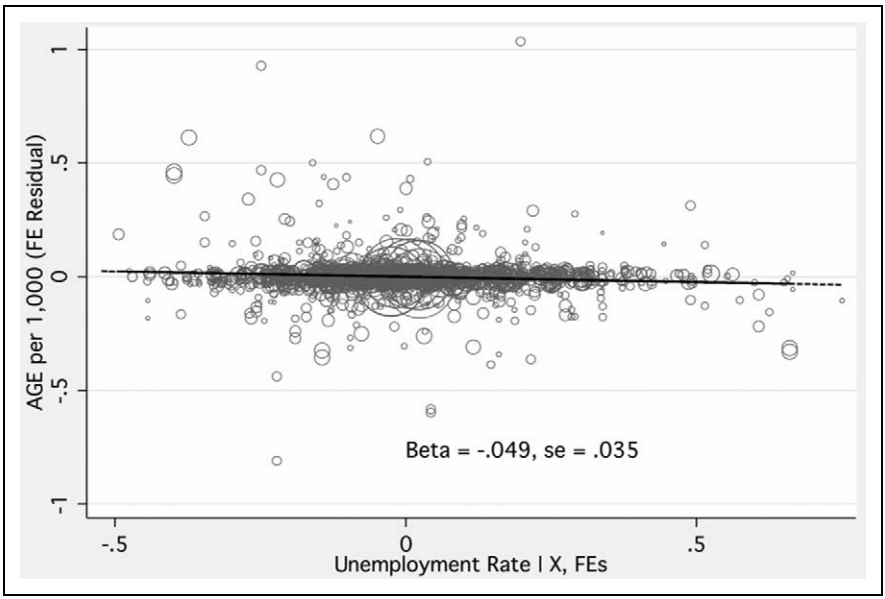

Figure 2. Unemployment vs. Armed Group Events (AGE) per 1,000 in Afghanistan.
Note: Variables on axes are residuals from Table 2, panel 3, column 2 regression that includes fixed effects and regressors ("X"): percent Pashtun, percent male, percent with no schooling, and average household size. Fixed effects (FEs) include both wave and district effects.

been commonly assumed. To probe possible explanations for this pattern, we now turn to a closer examination of the Iraqi insurgency where the negative correlation between unemployment and violence is strongest.

The "Surge" and the "Anbar Awakening"

The first obvious concern with the results from Iraq in Table 2 is that they may be driven by factors not controlled for by region and year fixed effects. Suppose, for example, that the "surge" in Baghdad (which began in January 2007) not only reduced violence but also strangled the local economy as military units built walls around specific neighborhoods and established checkpoints through the city. We would then observe a negative correlation between unemployment and violence not because unemployment increases violence but because the surge increased the former while reducing the latter. Alternatively, we might have spurious effects because of the politically driven reduction in violence in Sunni areas between August 2006 and December 2007, due to the "Anbar awakening," though it is not clear why these would be associated with increased unemployment. To explore these possibilities, we reran the basic fixed-effect regressions for Iraq but separated the sample by period and region. Table 3 reports these results.

Table 3. Unemployment and Violent Incidents in Iraq, by Period

Dependent variable: SIGACT incidents	2004–2007		2004–2005		2005–2007	
	All	Baghdad	All	Baghdad	All	Baghdad
Unemployment	−0.709*	−1.847***	−0.581	−2.431***	−1.321	−1.696**
	(0.552)	(0.566)	(0.514)	(0.933)	(1.060)	(0.905)
Observations	312	27	208	18	208	18
R^2	0.062	0.162	0.068	0.164	0.040	0.135

Note: Includes year and district fixed effects and are weighted by population. Robust standard errors clustered by district in parentheses. Variables described in note to Table 1.
***$p < .01$. **$p < .05$. *$p < .1$, all for one-tailed test with H_0: $\beta > 0$.

As the table shows, we can again reject the null hypothesis of a positive correlation between unemployment and violence at the 90 percent confidence level for the full three years observed. In Baghdad, the data strongly reject a positive correlation in both 2004–2005 and over the entire sample period. The negative correlation becomes substantially weaker in Baghdad during the 2005–2007 interval that spans both the pre-"surge" and "surge" periods, allowing us to rule out the possibility that our results reflect either (1) the building of walls and placement of additional troops in Baghdad in 2007, which caused both high unemployment and low violence, or (2) the major changes in patterns of violence from mid-2006 on in Baghdad. The data do not allow us to rule out a spurious effect associated with the Anbar awakening, as data from one province provide insufficient precision, yet it is unclear why that process—which reduced violence—would be associated with increased unemployment.

Replication

Are these results somehow particular to officially collected incident data? The top panel of Table 4 replicates the results in Table 3 using the Iraq Body Count data. These are included here in an effort to expose our core results to the possibility of refutation, using a second Iraqi data set, this one based on press reports rather than on administrative data. The table reports the results of a regression of incidents in which civilians were killed on unemployment rates, whether those incidents were insurgent, sectarian, or coalition induced. (Coalition-induced civilian casualty incidents are relevant because they often occur when coalition forces are unexpectedly faced with a strong insurgent threat, inducing the use of imprecise methods such as air strikes to extract themselves safely.) The results are not very informative, as they are not statistically significant—regardless of perpetrator; but they certainly do *not* show a positive correlation of unemployment and violence.

The lower panel of Table 4 repeats the exercise using as an outcome measure the number of civilian casualties rather than the number of incidents involving civilian casualties. Here a positive correlation appears for insurgent-perpetrated casualties,

Table 4. Unemployment and Violence—Iraqi Civilian Casualties Incidents, by Perpetrator

Perpetrator of Incident	Insurgent			Sectarian			Coalition		
Dependent variable: Civilian casualty incidents									
	All	Not Baghdad	Baghdad	All	Not Baghdad	Baghdad	All	Not Baghdad	Baghdad
Unemployment	0.013	0.016	0.011	0.009	0.011	−0.023	0.003	0.005	−0.010
	(.011)	(.013)	(.010)	(.031)	(.039)	(.020)	(.008)	(.010)	(.010)
Observations	312	285	27	312	285	27	312	285	27
R^2	0.085	0.067	0.380	0.179	0.183	0.665	0.011	0.015	0.134
Dependent variable: Civilian casualties									
	All	Not Baghdad	Baghdad	All	Not Baghdad	Baghdad	All	Not Baghdad	Baghdad
Unemployment	0.037	−0.022	0.292***	−0.449	−0.003	−2.096	0.013	0.104	0.004
	(0.047)	(0.038)	(0.054)	(0.430)	(0.111)	(1.731)	(0.046)	(0.099)	(0.061)
Observations	312	285	27	312	285	27	312	285	27
R^2	.03	.004	.53	.06	.06	.24	.02	.03	.03

Note: Iraq Body Count data. Includes year and district fixed effects and weighted by population. Robust standard errors clustered by district in parentheses. Variables described in note to Table 1.
***$p < .01$. **$p < .05$. *$p < .1$, all for one-tailed test with H_0: $\beta > 0$.

though only in Baghdad. This exception in Baghdad may appear to be supportive evidence for an opportunity-cost theory, but it is more likely evidence of a tactical failure by insurgents. Recall that these are incidents in which insurgents targeted coalition forces but killed civilians. We know from internal insurgent documents that many groups regard collateral damage—as distinct from intentionally targeting civilians—as politically problematic (Fishman and Moghadam 2008), a fact we use in the next section to help distinguish possible mechanisms for our results.

Why Does Unemployment Correlate with Less Violence?

The negative correlation between unemployment and violence directed at government forces is inconsistent with opportunity costs being the dominant mechanism in play. Yet, what is the first order connection between labor markets and violence? The result is consistent with at least three theories: (1) predation—insurgent violence rises in economically advantaged periods and areas because those areas become more valuable, (2) security effects—government security efforts simultaneously suppress both economic activity and insurgent violence, and (3) information—counterinsurgents can operate more effectively in areas with high unemployment because the cost of information is lower.

Following the logic of the Theoretical Framework, we attempt to distinguish between these possibilities using data from Iraq and the Philippines, where data on civilian casualties are available. The logic is that if high unemployment proxies for either security measures or low costs of information, low insurgent precision will be predicted by (a) unemployment and (b) the interaction of unemployment and population density. Predation has no prediction in this regression. Recall that our insurgent precision variable is calculated as the proportion of attacks on coalition forces that kill no civilians. In Iraq, this is calculated as the difference between SIGACT and IBC incidents directed at Coalition and Iraqi forces, divided by SIGACT incidents. In the Philippines, civilian casualties are reported directly in the incident data.

Our estimating equation is then

$$p_{r,t} = \mu_r + \pi u_{r,t} + \delta d_{r,t} + \theta u_{r,t} d_{r,t} + \gamma_t + \nu_{r,t} \qquad (2')$$

where p is insurgent precision, d is population density, u is measured as before (and we have included a full set of indicators for periods). Security effect and information-cost theories predict that the coefficient on unemployment, π, will be negative in a short regression (equation 1) and that the coefficient on the interaction, θ, will be negative in this long regression. As before, we do not think of these estimated coefficients as causal effects, but assuming that any reverse causality between precision and unemployment is second order, we are confident in interpreting the coefficients as tests of the theory.

Table 5 reports the results of this analysis. Beginning with the upper panel, on Iraq, three facts stand out. First, high unemployment is weakly associated with low insurgent precision in all sample periods, though not significantly

Table 5. Insurgent Precision, Unemployment, and Population Density

		2004–2007		2004–2005		2005–2007	
Dependent variable: Insurgent Precision = 1 – (Insurgent killings/SIGACTs)							
Panel A: Iraq	Unemployment	−0.541 (0.493)	0.230 (0.179)	−0.556 (0.683)	0.202 (0.133)	−0.733 (0.658)	0.281 (0.330)
	Population Density		−0.243*** (0.053)		−0.345*** (0.108)		−0.302*** (0.051)
	Unemployment × Pop. Density		−0.616*** (0.041)		−0.597*** (0.041)		−0.717*** (0.054)
	Observations	312	312	208	208	208	208
	R^2	0.087	0.460	0.054	0.680	0.158	0.623
	Joint *F*-Test on unemployment and interaction term	237.90***		219.38***		277.39***	
		1997–2003 and 2006		1997–2000		2001–2003 and 2006	
Panel B: The Philippines	Unemployment	−1.196 (1.064)	−0.582 (1.075)	0.050 (2.130)	1.828 (2.163)	−3.475** (1.784)	−1.848 (1.878)
	Population Density		0.143*** (0.051)		−1.941** (0.87)		0.350** (0.19)
	Unemployment × Pop. Density		−0.646*** (0.13)		0.425 (0.59)		−2.288*** (0.68)
	Observations	624	624	312	312	312	312
	R^2	0.26	0.28	0.41	0.46	0.07	0.19
	Joint *F*-test on unemployment and interaction term	25.99***		1.21		17.91***	

Note: Includes year and district/province fixed effects and weighted by population. Robust standard errors clustered by district in parentheses. Precision set to 1 if no attacks and no civilian casualties, 0 if no attacks and civilian casualties. Other variables described in notes to Table 1.
***$p < .01$. **$p < .05$. *$p < .1$, two-tailed.

so. Second, if we include population density and an interaction term in the regression, we find that insurgent precision is lower in densely populated areas during both subperiods.[26] This makes sense, as civilians are unfortunately more likely to be affected by shrapnel, overpressure, and stray small arms fire in densely populated areas. Third, and most important, once we control for this density effect, we find a strong negative coefficient on the interaction term between unemployment and population density, indicating that in the dense urban districts of Iraq unemployment was associated with reduced precision in both subperiods (columns 4 and 6) and over the entire sample period (column 2). This last result is consistent with both the security effects and the information-costs explanations for the negative correlation.

The results from the Philippines are quite similar. Unemployment predicts significantly less violence in the pooled regression and in second sample period, 2001–2003 and 2006 (column 5). The coefficient in the first sample period is positive but not statistically significant. When the interaction of unemployment and violence is inserted, the coefficient on that term is negative and highly significant in the second period (column 6), as predicted by both security effects and information cost theories. The same is true in the regression that pools both periods (column 2).

Taken as a whole, these results are consistent with the conjecture that insurgents switch tactics when unemployment is high, restricting themselves to methods that reveal less information but allow less precise targeting of coalition forces (e.g., sensor-activated IEDs vs. command-detonated ones) and thus inadvertently kill more civilians. What we cannot determine from these data is whether that tactical switch is due to (1) increased security pressure, such as checkpoints, barriers, and patrols—that raise unemployment by restricting the movement of goods and services, or (2) improved information flows to coalition forces about insurgent activities, as the price of leaks declines when unemployment rises. These results are neutral with respect to the predation hypothesis.[27] Recalling a grievance-based mechanism that motivated our original discussion, it is worth noting that Table 5 provides additional evidence refuting the idea that it is a dominant force; assuming that unhappy or unemployed noncombatants are less likely to share tips with the government—just as they are thought to be less likely to join the insurgency—one would expect insurgent precision to be high in areas with high unemployment. Table 5 shows the opposite.

Conclusion

Our findings on the relationship between unemployment and insurgency in Afghanistan, Iraq, and the Philippines call into question the opportunity-cost theory that dominates thinking in policy circles. These results suggest that any opportunity-cost effects—at least in these three cases—are overshadowed by other forces.

Combining data from three insurgencies allows us to emphatically reject a positive correlation between unemployment and violence.

Why is higher unemployment associated with less violence? While we cannot say for certain, a closer look at the data from Iraq and the Philippines suggests that the pattern stems from the relationship between local economic conditions and counter-insurgents' efforts to combat violence not from the labor market for insurgents. The data are consistent with two possibilities. The first is that as local economic conditions deteriorate, the price of information falls and government forces and their allies can buy more intelligence on insurgents, undermining insurgent operations. The second is simply that the techniques used to enhance security—establishing checkpoints, building barriers, and conducting raids—inhibit commerce and damage the economy. Distinguishing between those alternatives, and establishing the importance of predation, is a task for future research. What we can say with confidence is that while it may still be true that increasing unemployment causes greater political violence on the margins that effect is swamped by other mechanisms. Greater unemployment in these three conflicts predicts less political violence, not more.

Our research presents two serious policy implications for academics, donor countries, and aid organizations. First, the negative correlation of unemployment with violence indicates that aid and development efforts that seek to enhance political stability through short-term job creation programs may well be misguided. Instead, development funds are likely to buy more "no bang for the buck" when directed at small-scale projects that improve the quality of local government services, thereby inducing noncombatants to share intelligence about insurgents with their government and its allies. We find evidence for just such an effect in related research on the impact of U.S. reconstruction spending in Iraq (Berman, Felter, and Shapiro 2008).

Second, much more basic research is required to guide development aid spent in efforts to rebuild social and political order. These programs are not methodically evaluated the way comparable domestic programs would be. This is a tragedy. Aid resources are scarce and the needs massive. A better understanding of how, when, and where aid spending helps reduce political violence will both further our understanding of insurgencies, while helping to guide practitioners in applying limited development aid in conflict and post conflict societies.

Appendix

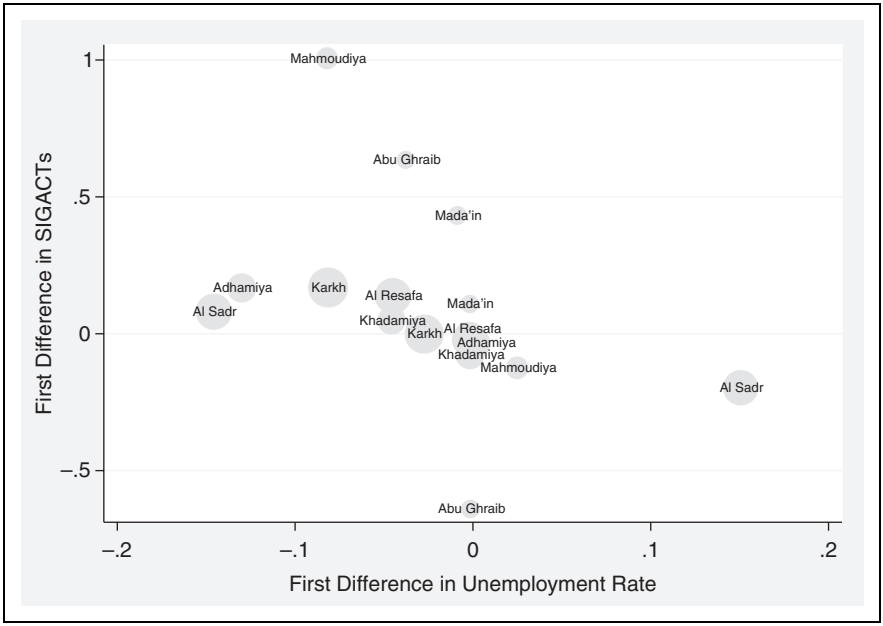

Figure A1. Unemployment and SIGACTs in Baghdad without Tarmis. Black labels correspond to 2005 and gray labels correspond to 2007.
Note: SIGACT ¼ data drawn from significant activity reports submitted by Coalition forces.

Table A1. Unemployment and Violent Incidents in Iraq and the Philippines, Negative Binomial Regression

		Negative Binomial Regression			
	DV	Incidents	Incidents	Incidents	Incidents
Iraq (district/quarter)	Region	All	Baghdad	All	Baghdad
	Unemployment	–5.313*** (1.371)	–13.66*** (2.093)	–2.718** (1.172)	–6.672*** (2.115)
	Observations	312	27	279	27
	Controls	Ethnicity, Population	Ethnicity, Population	District FE, Population	District FE, Population
Philippines (province/year)	Region	All	Muslim > 5%	All	Muslim > 5%
	Unemployment	–8.199*** (2.012)	–7.161* (4.654)	–1.156 (2.026)	1.816 (3.981)
	Observations	624	96	552	96
	Controls	Ethnicity, Population	Ethnicity, Population	Province FE, Population	Province FE, Population
Afghanistan (district/year) "AGE" incidents)	Region	All	Pashtun > 50%	All	Pashtun > 50%
	Unemployment	–0.424*** (0.176)	0.501** (0.237)	–0.106 (0.103)	–0.164* (0.119)
	Observations	2160	923	1697	854
	Controls	Ethnicity, Population	Ethnicity, Population	District FE, Population	District FE, Population

Note: All regressions include time effects. The coefficient on population is fixed to unity, in order to control for scale (using the Stata "exposure" option). Sample sizes vary as regions with no incidents during the entire sample period are excluded. Standard errors in parentheses. Variables described in note to Table 1.
***$p < .01$. **$p < .05$. *$p < .1$, one-tailed with H_0: $\beta > 0$.

Table A2. Unemployment and Violent Incidents in Afghanistan with Extra Controls

Dependent Variable	AGE/ 1000	AGE/ 1000	JOIIS/ 1000	CIDNE/ 1000	AGE/ 1000	JOIIS/ 1000	CIDNE/ 1000
Unemployment	−0.049* (0.035)	−0.050* (0.035)	−0.066* (0.044)	−0.051* (0.036)	0.004 (0.008)	−0.004 (0.012)	0.006 (0.008)
% Pashtun		0.010 (0.011)	0.027* (0.017)	0.011 (0.011)	0.047* (0.030)	0.069* (0.039)	0.049* (0.031)
% Pashtun × Unemp.					−0.148* (0.094)	−0.170* (0.118)	−0.155* (0.096)
% Male		0.118** (0.039)	0.188** (0.076)	0.099** (0.035)	0.127** (0.042)	0.199** (0.078)	0.108** (0.038)
% No Schooling		0.004 (0.011)	−0.003 (0.020)	0.004 (0.010)	0.005 (0.011)	−0.002 (0.020)	0.005 (0.010)
Avg. Household Size		−0.003 (0.002)	−0.008** (0.004)	−0.004 (0.003)	−0.004* (0.002)	−0.008** (0.004)	−0.004* (0.002)
District FE	–	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,146	2,146	2,146	2,146	2,146	2,146	2,146
R-squared	0.081	0.092	0.123	0.083	0.108	0.133	0.101

Note: All regressions include ANQAR wave fixed effects and are weighted by population. Robust standard errors clustered by district reported in parentheses. Variables described in note to Table 1. Afghanistan has 363 districts included in the samples above.
***$p < .01$. **$p < .05$. *$p < .1$, one-tailed test with H_0: β 0.
First differences: 04–05 (red), 05–07(blue) (Table 3, column 2).[28]

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Authors' Note

The opinions, findings, and recommendations in this document are the authors' and do not reflect views of the United States Department of Homeland Security or Department of Defense. All mistakes are the authors'. Replication data are available on the authors' websites and at http://jcr.sagepub.com/.

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Notes

1. General Chiarelli, the U.S. Army Commander of Multinational Forces in Iraq, made this argument in a press briefing, December 8, 2006.
2. Our data are based on labor force surveys and three newly available data sets that include two subnational measures of insurgency: (1) attacks against government and allied forces; and (2) violence that kills civilians.
3. This view is largely based on the British experience in Malaya, the French in Algeria, and the U.S. in Vietnam. It is explained in Trinquier (1961), Taber (1965), Galula (1964), Clutterbuck (1966), Thompson (1966), Kitson (1971), and Popkin (1979).
4. Articles by practitioners in this vein include Sepp (2005), Petraeus (2006), Cassidy (2006), and McMaster (2008). This is distinct from Berman and Laitin (2008), and Berman (2009), who argue certain rebel clubs do not share information with noncombatants and are thus unaffected by actions of noncombatants. Economic development and improved governance still play a role in countering these clubs, though, as their social service providing organizational bases are vulnerable to competition.
5. "Stop the killing of people unless they are spying, military, or police officers. If we continue using the same method, people will start fighting us in the streets." From,

"Instructions to Abu-Usamah" from the collection "Captured Zarqawi Network Letters from Iraq" posted April 20 2006, www.ctc.usma.edu/harmony/harmony.asp, accessed June 5, 2010.

6. Al Jazeera (2009), http://english.aljazeera.net/news/asia/2009/07/200972775236982270.html, accessed June 5, 2010.
7. Kalyvas (2006) offers the strongest version of this argument. An alternative possibility is Fearon's (2008) suggestion that the insurgency could reach a size where additional members create more detection risk for the organization, leading to decreasing levels of realized violence.
8. Formally, the British paid insurgents to surrender with established payments by position. Informants who provided information that led to the capture or killing of insurgents earned 75 percent of those individuals' surrender values (Long 2006, 48 cited in Komer 1972, 72-74).
9. For a colorful summary of U.S. activity buying tips in Afghanistan and elsewhere, see Joby Warrick "CIA buys Afghan chief's loyalty with Viagra," in the *Washington Post*, 26 December 2008. "In their efforts to win over notoriously fickle warlords and chieftains, the officials say, the agency's operatives have used a variety of personal services. These include pocket knives and tools, medicine or surgeries for ailing family members, toys and school equipment, tooth extractions, travel visas and, occasionally, pharmaceutical enhancements for aging patriarchs with slumping libidos, the [U.S. intelligence] officials said."
10. Al Jazeera (2009), http://english.aljazeera.net/news/asia/2009/07/200972775236982270.html, accessed June 5, 2010.
11. All data provided by the Empirical Studies of Conflict (ESOC) Project.
12. The information provided in the unclassified SIGACT data are limited to the fact of and type of terrorist/insurgent attacks (including improvised explosive devices [IEDs]) and the estimated date and location they occurred.
13. Data for other conflicts do not reliably count government-initiated incidents and so in our analysis we focus on insurgent-initiated incidents for the Philippines to maximize comparability.
14. ANQAR survey waves were fielded as follows: September. 14–25, 2008; December 23, 2008 to January 2, 2009; February 25 to March 9, 2009; June 4–13, 2009; September 2009.
15. The quarterly labor force surveys were aggregated to the provincial level by the Philippine Human Development Network to obtain reliable province-level estimates. Summary statistics are from their 2005 and 2008 reports and include figures for 1997–2003 and 2006. Data available at http://hdn.org.ph/philippine-human-development-reports/#hdrs.
16. Five districts in Kurdish regions were not surveyed. These districts suffered little to no insurgent violence.
17. Using district-level ethnicity measures created by combining maps with remote-sensing data on population does not effect the results.
18. Those would be countries where subnational panel data about both unemployment and rebel violence are available or could be constructed—the population that researchers can possibly use for statistical inference. To our knowledge, this list includes Afghanistan, Colombia, Guatemala, India, Iraq, Israel, Nepal, Northern Ireland, Pakistan,

the Philippines, Spain, and Vietnam. We thank an anonymous referee for emphasizing the need to clarify the sampling scheme.

19. A convenient feature of multivariate normal distributions is that the covariance in the (i,j)th position of the variance–covariance matrix is equal to $\rho SE(\beta_i)SE(\beta_j)$ where $SE(\beta_i)$ is the estimated standard parameter error for country i.
20. The relationship between the degree of correlation between country coefficients and the p value at which the null of joint zero is rejected can be non-monotonic. Please see NBER working paper #15547 for a discussion.
21. The actual product of p values from a one-tailed test is .0000069.
22. Note rejection at 99.96 percent confidence implies a p value with zero correlation of .04, which is greater than the .0156 we found assuming zero correlation in the Wald test. This is principally because our p values in the individual country t-tests are not perfectly equal to .1, .01, and .1 cutoffs.
23. Appendix Table A2 reports on further robustness checks for the Afghan regressions reported in Table 2.
24. Figure 2 also provides additional evidence against the relationship being a spurious result of trying to fit a linear model when the true relationship is one where violence is decreasing at high levels of unemployment because areas under insurgent control go uncontested. If that were the case, we would see outliers clustered below the regression line at high and low levels of unemployment.
25. Appendix figure 1 plots the data for Baghdad to illustrate the fixed effects regression in the rightmost column, with changes in incidents plotted against changes in unemployment rates in both 2004–2005 and 2005–2007. The very small district of Tarmia is omitted from the Figure as it is an outlier and forces rescaling of the graph. It is included in the regression analysis in Table 2. It has no substantial effect on the results. The figure shows our Iraq results are not driven by any particular outlier but rather by the pattern that played out in some of Baghdad's largest districts: violence fell while unemployment rose in Sadr City, Al-Resafa, and Adhamiya in 2004–2005 but subsequently rose while unemployment fell in the same three districts in 2005–2007.
26. Note also that population density is sufficiently time-varying in Iraq during the war to allowing coefficients to be precisely estimated in a fixed-effects regression. This reflects tragically high rates of internal displacement and refugee migration.
27. The insurgent precision results are also neutral with respect to the additional possibility that increased violence induces migration disproportionately among working-aged adults, which in turn reduces the unemployment rate, generating a negative correlation. We think that this is unlikely, especially in low-mobility rural communities but cannot rule out the possibility with these data.
28. 9 Districts $\times$ 3 Years $=$ 18 observations of differences.

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