

The Seasonality of Conflict

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Abstract

This paper investigates whether poor employment prospects of potential insurgents help to fuel conflict. The paper provides a new test of this “opportunity cost mechanism” using one of the largest shocks to labor demand in agricultural societies: harvest. Theoretically, the paper shows that because seasonal harvest shocks are temporary and anticipated, they change opportunity costs while keeping the dynamic benefits of fighting constant, yielding unbiased estimates even if those benefits are unobserved. In contrast, many other shocks in the conflict literature are persistent

and unanticipated, thus also varying the dynamic benefits of fighting that confound estimates of the opportunity cost mechanism. Empirically, the paper estimates the effect of harvest shocks on conflict intensity in Afghanistan, Iraq, and Pakistan using subnational variation in the timing and intensity of harvest driven by local climatic conditions. Consistent with the opportunity cost mechanism, the results show that the onset of harvest usually reduces the number of insurgent attacks.

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THE SEASONALITY OF CONFLICT

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An Online Appendix is available at: [link](#).

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“Peasant recruitment and desertions in all the [Russian] civil war armies fluctuated with the farming seasons. Peasants joined up in the winter, only to desert the following summer. In the central agricultural region, the weekly rate of desertion was up to ten times higher in summer than winter.” Figes (1996: 596)

1. INTRODUCTION

Understanding why civil conflict occurs is of utmost policy importance given its toll on human lives and on broader development prospects. While traditional two-state conflicts have been relatively rare post-WW2, around 40% of countries have had at least one civil war that has killed more than 1,000 people (Fearon 2008). The best predictor of civil war is low per capita income (Fearon and Laitin 2003), leading to a commonly held view that poverty and poor employment prospects are key drivers of conflict (World Bank 2011). Indeed, a number of theoretical models establish a connection between the availability and rewards for work and the onset and intensity of conflict, known as the opportunity cost mechanism (Becker 1968; Grossman 1991; Dal Bo and Dal Bo 2011).

However, the empirical evidence on the opportunity cost mechanism is mixed. On one hand, a number of studies show that conflict intensifies with negative income shocks driven by rainfall (Miguel et al. 2004) or commodity prices (Bruckner and Ciccone 2010; Dube and Vargas 2013; Guardado 2016; Hodler and Raschky 2014, among others). But on the other hand, other studies find a more mixed relationship between commodity prices and conflict (Blattman and Bazzi 2014; Crost and Felter 2016), and some even find that conflict intensity increases with labor availability (Berman et al. 2011b) or income windfalls from aid (Nunn and Qian 2014; Crost et al. 2014; Weintraub 2016).¹

In this paper, we make three contributions to the opportunity cost literature. First, we argue theoretically that the mixed empirical results in the literature may be because the highly persistent shocks often studied change the unobserved dynamic benefits of fighting at the same time as the opportunity costs, confounding empirical estimates. Second, we propose using harvest shocks as a novel alternative, which are both temporary and anticipated and are also one of the largest and most common shocks to labor demand in developing countries. Third, we estimate the effect of

¹In a similar vein, Iyengar et al. (2011) and Beath et al. (2017) find both a positive and negative impact of development assistance programs in Iraq and Afghanistan, respectively.

harvest shocks on subnational conflict intensity in Iraq, Afghanistan, and Pakistan and find support for the role of opportunity cost considerations.

Conceptually, shock persistence is important because in many conflict settings, the costs of fighting are born today, whereas the benefits of winning a conflict are mostly in the future. For example, for conflict over an oil-rich region—motivated by “greed”—the future benefits are the flow of rents after the oil wells are captured. In this context, a persistent oil price shock that stimulates an oil-exporting economy and increases the opportunity cost of fighting will also increase future profits from capturing oil wells. As both the opportunity costs and future benefits of fighting increase, estimates of the strength of the opportunity cost mechanism driven by this shock will be biased toward zero.

Given that many commodity price shocks are highly persistent (and are often indistinguishable from a random walk; see, e.g., Cashin et al. 2000), this argument suggests that the opportunity cost effect captured by commodity prices is likely to be even stronger (more negative) than shown in current studies (e.g., Bruckner and Ciccone 2010; Dube and Vargas 2013; Blattman and Bazzi 2014; Guardado 2018; Hodler and Raschky 2014). We also show that the same concern arises in other models where conflict is motivated by grievances or depends on counterinsurgency informants (Berman et al. 2011a), albeit through a different mechanism. In these grievance/counterinsurgency models, persistent shocks change the marginal utility of consumption, which then affects the subjective value attached to grievances or tip-off payments.

Our theory suggests that the ideal shocks to uncover the strength of the opportunity cost mechanism are temporary or anticipated changes in labor demand. Seasonal shocks, like harvest, are both. Temporary or anticipated shocks do not affect the expected benefits of fighting, like the rents from capturing an oil well, because those rents are in the future (for temporary shocks) or the effect is already built in (for anticipated shocks). In a grievance/counterinsurgency framework, temporary or anticipated shocks are smoothed by the permanent income hypothesis (PIH), and so they do not change the marginal utility of consumption and the subjective value of the grievance/tip-off payment. Harvest has the added practical advantage of being a large shock to labor demand: harvests are common throughout the world in different conflict settings and their exact timing and size are determined by exogenous local climatic conditions.

Our theoretical argument builds on that of Fearon (2008) and Chassang and Padro-i-Miquel (2009), who argue that permanent changes in incomes in a greed-motivated model will not change the level of conflict, as both the costs and benefits of fighting increase. Chassang and Padro-i-Miquel (2009) also argue that temporary shocks will affect conflict via the opportunity cost mechanism. Relative to those papers, we generalize this argument using a quantitative regression framework using simulated data, where the future value of winning is an omitted (unobserved) variable. We also introduce a specific definition of the actual and measured strength of the opportunity cost mechanism, which allows us to precisely characterize the size and direction of the bias in opportunity cost regressions with seasonal or persistent auto-regressive shocks. We likewise show that the same results hold (for different reasons) in grievance/counterinsurgency models not considered by Fearon (2008) and Chassang and Padro-i-Miquel (2009).

Beyond theory, there are hundreds of years of narrative evidence that harvest affects conflict intensity through the opportunity cost mechanism. For example, during the American Civil War (1861–1865), desertions from the Confederate Army increased in the months of June and July, the harvesting times for tobacco—an important Southern crop at the time (Giuffre 1997). The introductory quote also suggests that during the Russian Civil War (1917–1922), desertion rates in the Red and White armies—largely formed of peasants—were higher during the summer harvest (Figes 1996, cited by Dal bo and Dal bo 2011: 657).

The sensitivity of conflict intensity to harvest stems from the fact that insurgencies in civil wars are largely fought by part-time fighters (see Appendix Figure A.3). There are many advantages for an insurgency to hire part-time fighters: they have valuable local knowledge, they may be cheaper than full-time fighters, and they can help protect the more skilled professional insurgents from the risks of day-to-day fighting. For example, Vietcong guerrilla members famously worked as farmers during the day but fought US forces at night. Cline (2000) finds that the Moro Islamic Liberation Front in the Philippines filled battalions with part-time fighters on a monthly rotational basis. In Afghanistan, Taliban forces have been known to organize in village cells, each containing around 10 to 50 part-time fighters (Afsar et al. 2008). Part-time fighters were also common among Iraqi insurgents fighting against the US military presence and among the Shining Path insurgency in Peru in the 1980s (McClintock 1998).

In the empirical section of this paper, we estimate the effect of the wheat harvest on subnational conflict intensity in Iraq (2004–2009), Pakistan (2002–2010), and Afghanistan (2004–2014). Wheat is the main (legal) crop in all three countries and is generally harvested annually using labor-intensive methods. Unsurprisingly, household survey data show that during harvesting months, agricultural workers tend to have higher employment rates than other rural workers, suggesting a positive seasonal labor demand shock at harvest time. As monthly data on local wages or employment are unavailable, we estimate a reduced-form relationship between the timing and size of the area harvested and the number of attacks in that location. As conflict intensity varies widely across settings and years due to other factors, we use a normalized measure of the number of attacks occurring in the month as a share of the annual total in that district, which can be compared across the three samples. We also use several different datasets of conflict intensity—based on US government records as well as on local media reports—to ensure our findings are robust to measurement error in recording conflict intensity.

While the use of transitory and anticipated shocks keep constant the dynamic benefits of fighting (as in the theory), our identification strategy also removes remaining concerns about reverse causality and omitted variable bias. To ensure against reverse causality (running from conflict to harvest), we use pre-conflict data on local climatic conditions that determine the timing and intensity of harvest at the subnational level. Other omitted variables are controlled for by district-by-year fixed effects that partial out local characteristics—even those changing slowly over time—and monthly time effects to control for any aggregate political and economic shocks and for the season. Our results are also robust to controls for temperature and precipitation (among others).

Our main empirical finding is that in all three countries, the intensity of conflict is lower at harvest than at other times of the year, with greater falls in areas with larger areas under cultivation. Specifically, we find that at the mean intensity of wheat cultivation, the onset of harvest reduces the average number of monthly attacks by around 6%–22% in Iraq, 20% in Pakistan, and 8%–18% in Afghanistan. Moreover, dynamic specifications show that most of the reduction in conflict is generally in the harvesting month itself; there is little evidence that the harvest may be promoting or financing conflict in subsequent months. Overall, these results provide some support for the opportunity cost playing an important role as a determinant of conflict, even

for insurgencies thought to be driven by ideology, rather than economic factors, like the Taliban and Al-Qaeda.

The remainder of the paper is organized as follows. Section 2 presents our theoretical argument illustrating the importance of shock persistence, and the advantages of seasonal shocks in estimating the true strength of the opportunity cost mechanism. Section 3 discusses our empirical approach in estimating harvest’s effect on conflict intensity. Section 4 presents our empirical results for Iraq, Afghanistan, and Pakistan. Section 5 concludes by discussing some policy implications.

2. THEORY: DYNAMIC BENEFITS OF SEASONAL SHOCKS

In this section, we seek to understand why the literature finds mixed results on the importance of the opportunity cost mechanism in explaining conflict. We argue that the estimated strength of the opportunity cost mechanism depends on the type of shock studied: highly persistent shocks can lead to systematically biased estimates, while temporary and anticipated seasonal shocks can uncover the true magnitude of the opportunity cost mechanism.

The intuition is as follows: in standard dynamic models of conflict motivated by greed, the costs of fighting are temporary and are incurred today, whereas the benefits of victory are persistent and are received in the future (Chassang and Padro-i-Miquel 2009). Since seasonal harvest shocks are both temporary (only a few weeks a year) and anticipated, they change the costs of fighting today while keeping constant its unobserved dynamic future benefits, thus properly capturing the strength of the opportunity cost mechanism. In contrast, many of the unanticipated and persistent shocks studied in the literature (e.g. persistent commodity prices or development aid) may affect *both* today’s opportunity costs and tomorrow’s gain from conflict. These income shocks would thus bias estimates of the strength of the opportunity cost mechanism toward zero—particularly when the dynamic benefits are hard to observe—explaining the wide variety of estimates in the literature.

This argument is robust to different motivations for conflict. Below we present a dynamic “greed” model of conflict, where insurgents engage in violence in order to capture a resource that has some monetary value, but we show in Appendix 2 that this argument also holds in models where conflict has intrinsic motivations (“grievances”) or is driven by counterinsurgency information. For each model, we identify the size of the bias and provide a precise definition of the strength of the opportunity cost mechanism—namely, the elasticity of time allocated to violence with respect to wages.

2.1. Greed Model. What we call the “greed” model of conflict is one of the most popular in the literature (Haavelmo 1954; Hirshleifer 1988, 1989; Garfinkel 1990; Skaperdas 1992; Garfinkel and Skaperdas 2007; Fearon 2008; Chassang and Padro-i-Miquel 2009; Dal bo and Dal bo 2011), with our paper building on the last three papers. Similar to Chassang and Padro-i-Miquel (2009), we model the gains from victory as dynamic, whereas the costs are static, meaning that temporary but not permanent shocks affect violence, and that winning is decisive. As in Fearon (2008), conflict varies at the intensive, rather than extensive, margin. Like Dal bo and Dal bo (2011), our appropriation/fighting technology is concave in labor (reflecting congestion effects); our production function is non-linear in labor (such that real wages depend on the allocation of labor between working and fighting); and we abstract from the government’s response to violence.

2.1.1. Model Setup. We study the problem of a representative insurgent fighter, who has one unit of time to split between labor (L) or violent activities (V). The benefit of time spent fighting is to increase the insurgent’s probability of victory, represented by a “contest success function” $p(V_t) = \psi V_t^{1-\phi^{-1}}$, where $\phi > 1$ measures the strength of the opportunity cost mechanism and ensures positive but diminishing marginal returns to fighting.²

Output in this economy Y_t is produced with labor L_t and a fixed factor of production \bar{N} (such as land or a natural resource), which we normalize to one so that $Y_t = \theta_t L_t^\alpha$.³ θ_t measures productivity in terms of consumption goods, which would capture commodity price shocks, seasonal shocks, or land productivity that varies with rainfall, among others.⁴ An increase in θ_t increases output. We study persistent or seasonal (temporary and anticipated) shocks to θ_t .

The reward for working is the wage, which is the marginal product of labor $W_t = \alpha Y_t / L_t$. As such, total labor income $W_t L_t$ is simply αY_t . The remainder of output is

²This is a good approximation of the “power form” of the contest function (Garfinkel and Skaperdas 2007, Equation 3) in a low-level insurgency where the strength of the government is large (and constant). That is: $p(V_t, G) = V_t^m / (V_t^m + \bar{G}^m) \approx \psi V_t^{1-\phi^{-1}}$ with $m = 1 - \phi^{-1}$ and $\psi \approx 1 / (\bar{V}^m + \bar{G}^m) \approx 1 / \bar{G}^m$ if the government’s strength \bar{G}^m is very large relative to the insurgent’s (and constant). For example, with our default calibration, $V = 0.065$, $\bar{G} = 1900$, and $\phi = 3$ such that the probability of the insurgents winning is $\approx 0.1\%$ (per quarter).

³That is, the production function is Cobb-Douglas $Y_t = \theta_t L_t^\alpha \bar{N}^{1-\alpha}$ with $\bar{N} = 1$.

⁴In this former case, let the price of domestic consumption goods be the numeraire and the international price of the commodity be θ_t , and assume that the volume of cash crops is produced for export is L_t^α . Then the amount of consumption goods that can be purchased is $\theta_t L_t^\alpha$.

rents Π to the fixed factor of production (land or a natural resource) $\Pi_t = (1 - \alpha)Y_t$, which accrue its owner. The motivation for the insurgent to fight is to capture these rents and to gain Π_t .

We present two submodels that vary in their dynamics: a two-period model that we can solve analytically and an infinite horizon model that we solve numerically. The two models are very similar. The size of the prize of winning, denoted as $\Pi_{t+1}^{Prize} \equiv U_{t+1}^{Win} - U_{t+1}^{Lose}$ equals Π_{t+1} in the two-period model, but is more complicated in infinite horizon model. More formally, the fighter's problem is

$$(1) \quad U_t^L(\theta_t) = \underbrace{max_{V,L} W_t L_t}_{Wage\ Inc} + \underbrace{p(V_t)}_{ProbWin} \times E\beta U_{t+1}^{Win}(\theta_{t+1}) + \underbrace{[1 - p(V_t)]}_{ProbLose} E\beta U_{t+1}^L(\theta_{t+1})$$

s.t. $p(V_t) = \psi V_t^{1-\phi^{-1}}$, $L_t + V_t = 1$, (taking $W_t = \alpha Y_t / L_t$ and $Y_t = \theta_t L_t^\alpha$ as given)

2.1.2. *Measuring the Opportunity Cost Mechanism.* The first-order condition of the insurgent's problem is $W_t = p'(V_t)\beta E_t \Pi_{t+1}^{Prize}$, where the right hand side is the gain from fighting an extra hour: the increase in the probability of winning $p'(V_t)$ times the expected discount prize from winning $\beta E_t \Pi_{t+1}^{Prize}$. The left hand side is the foregone wage from working (i.e., the opportunity cost of spending an extra hour fighting). Substituting the functional forms and taking logs, we obtain an expression that can be potentially taken to the data:⁵

$$(2) \quad \ln V_t = \kappa_1 - \phi \ln W_t + \phi \ln E_t \Pi_{t+1}^{Prize}.$$

Definition 1. The *true strength of opportunity cost mechanism* is the elasticity of violence with respect to wages, keeping everything else constant: $\beta_{Opp}^{True} \equiv \frac{\partial \ln V_t}{\partial \ln W_t} = -\phi$.

Definition 2. The *measured strength opportunity cost mechanisms* is the elasticity of violence with respect to wages, allowing the value of the prize of fighting to change endogenously: $\beta_{Opp}^{Meas.} \equiv \frac{d \ln V_t}{d \ln W_t} = \frac{\partial \ln V_t}{\partial \ln W_t} + \frac{\partial \ln V_t}{\partial \ln \Pi_{t+1}^{Prize}} \frac{\partial \ln \Pi_{t+1}^{Prize}}{\partial \ln W_t}$.

⁵The constant $\kappa_1 = \phi \ln \psi (1 - \phi^{-1}) + \phi \ln \beta$. In equation 4, $\kappa_2 = \kappa_1 - \phi \ln \alpha + \phi \ln (1 - \alpha)$.

To illustrate, consider a standard univariate regression of changes in violence on some measure of opportunity cost of fighting, like wages:

$$(3) \quad \ln V_t = \alpha_0 + \beta_{Opp} \ln W_t + e_t.$$

The objective is to estimate $\beta_{Opp}^{True} = -\phi$ as in Definition 1. However, the value of the prize for fighting $\ln E_t \Pi_{t+1}^{Prize}$ is typically unobserved, and so it enters the error term e_t in Equation 3, yielding an estimate of $\beta_{Opp}^{Meas.}$, which includes the effect of endogenous changes in $\ln E_t \Pi_{t+1}^{Prize}$, namely $\partial \ln V_t / \partial \ln \Pi_{t+1}^{Prize}$, which is usually positive. As we show in the next section, temporary and anticipated seasonal shocks satisfy $\partial \ln \Pi_{t+1}^{Prize} / \partial \ln W_t = 0$ such that $\beta_{Opp}^{Meas.} = \beta_{Opp}^{True} = -\phi$. In contrast, for persistent shocks, $\partial \ln \Pi_{t+1}^{Prize} / \partial \ln W_t > 0$, biasing estimates of the opportunity cost mechanism upward toward zero: $\beta_{Opp}^{Meas.} > \beta_{Opp}^{True}$.

2.1.3. Two-Period Analytical Greed Model. The simplest version of this model has only two periods. Conflict, labor, and production occur in the first period t , and the outcome of conflict is decided at the end of this period. In the second period $t+1$, there is peace and all of the insurgent's time is spent working ($L_{t+1} = 1$). In the two-period model, the insurgent's gain from winning are profits Π_{t+1} from production in the second period: $U_{t+1}^{Win} = \Pi_{t+1} + W_{t+1} \times 1$ and $U_{t+1}^L = W_{t+1} \times 1$, so $\Pi_{t+1}^{Prize} \equiv U_{t+1}^{Win} - U_{t+1}^L = \Pi_{t+1}$. However, profits and wages are driven by shocks to productivity θ . $\Pi_{t+1} = (1 - \alpha)Y_{t+1} = (1 - \alpha)\theta_{t+1}$ such that $\ln E_t \Pi_{t+1}^{Prize} = \ln(1 - \alpha) + \ln E_t \theta_{t+1}$. We assume that we start with a very low fraction of time spent on violent activities, so $\ln W_t \approx \ln \alpha + \ln \theta_t$ (wages are approximately proportional to productivity).⁶ Then Equation 2 can be rewritten as

$$(4) \quad \ln V_t \approx \kappa_2 - \underbrace{\phi \ln \theta_t}_{\text{"Wage"}} + \underbrace{\phi \ln E_t \theta_{t+1}}_{\text{"Prize win"}}.$$

Proposition 1 Consider an autoregressive process for productivity $\ln \theta_{t+1} = \kappa_3 + \rho \ln \theta_t$,⁷ where $\beta_{Opp}^{True} = -\phi$ as in Equations 2 and 4. Then the measured strength of the opportunity cost mechanism will

- (A) equal its true strength $\beta_{Opp}^{Meas.} = \beta_{Opp}^{True} = -\phi$ in the event of temporary shocks, $\rho = 0$ (such as seasonal shocks);
 - (B) be zero ($\beta_{Opp}^{Meas.} = 0$) with permanent shocks $\rho = 1$ (i.e., $\ln \theta_{t+1} = \ln \theta_t$);
- and

⁶That is, $L_t \approx 1$ so $L_t^{\alpha-1} \approx 1$ and $W_t = \alpha \theta_t L_t^{\alpha-1} \approx \alpha \theta_t$.

⁷ $\kappa_3 = (1 - \rho) \ln \bar{\theta}$, where $\ln \bar{\theta}$ is the log mean productivity.

(C) be “biased” upward ($\beta_{Opp}^{Meas.} > \beta_{Opp}^{True}$) by persistent shocks $\rho > 0$.

Proof: Substituting the shock process, Equation 4 becomes $\ln V_t \approx (\kappa_2 + \kappa_3) - \phi(1 - \rho)\ln\theta_t$, and hence $\beta_{Opp}^{Meas.} = d\ln V_t/d\ln W_t = d\ln V_t/d\ln\theta_t = -\phi(1 - \rho)$. The result follows from substituting different values of ρ . \square

Proposition 1A shows our main motivation for using seasonal shocks: variation in labor market outcomes driven by temporary shocks, including seasonal shocks, yield the true strength of the opportunity cost mechanism—even if variation in the prize of fighting is unobserved. This argument is related to Chassang and Padro-i-Miquel’s (2009) finding that only temporary negative income shocks will increase violence with a different model.

Proposition 1B is a restatement of Fearon’s (2008) and Chassang and Padro-i-Miquel’s (2009) result that permanent changes in the level of economic development, or income, increase both the opportunity cost of violence and the spoils of war, leaving the level of violence unchanged.

Finally, Proposition 1C applies to many shocks used to assess the role of opportunity cost considerations, such as many commodity price shocks, which are well known to be highly persistent. It states that using variation in persistent shocks will generate estimates of the strength of the opportunity cost mechanism that are too small in absolute value. This might explain the wide variety of estimates of the strength of the opportunity cost mechanism in the literature. We quantify this result in the next subsection.

2.1.4. *Dynamic Quantitative Greed Model.* The second submodel is an infinite horizon one and allows for richer dynamics but can only be evaluated quantitatively. Just like in the two-period model, the part-time insurgent allocates time between working and fighting in the first period t and the outcome of conflict is decided at the end of the first period. If the insurgents win, we assume that victory is decisive and there is peace forever with all time allocated to working ($L = 1$). The expected future utility of the victorious insurgent U_{t+1}^{Win} is the present discounted value of future profits from the capture of the natural resource ($\sum_{i=1}^{\infty} \beta^i E_t \Pi_{t+i}$) plus the present discounted value of future labor income $\sum_{i=1}^{\infty} \beta^i (E_t W_{t+i})$.

If the insurgents lose, the game resets and the part-time fighter allocates time between working and fighting again, with the utility of the out-of-power fighter U_{t+1}^L defined recursively in Equation 1. The violence continues forever or until the insurgents win. The gain from winning in the dynamic model is $\Pi_{t+1}^{Prize} \equiv U_{t+1}^{Win} - U_{t+1}^L = (\sum_{i=1}^{\infty} \beta^{i-1} \Pi_{t+i} + \sum_{i=1}^{\infty} \beta^{i-1} W_{t+i}) - U_{t+1}^L$. That is, the value of the prize of winning

today depends on the present value of future profits $\sum_{i=1}^{\infty} \beta^i E_t \Pi_{t+i}$ but also on other terms that do not have a closed-form solution.⁸ Now that we have a full dynamic model, we can fully categorize shocks as seasonal (alternating high and low) versus persistent shocks following an AR(1) process (Equation 5):

(5)

$$\text{Seasonal : } \theta_t = \ln \bar{\theta} + \chi \mathbf{1}(t = k), \quad k = 1, 3, 5, \dots \quad \text{AR(1) : } \ln \theta_{t+1} = \kappa_3 + \rho \ln \theta_t + e_{t+1}.$$

The model is not analytically tractable, so instead we simulate data when productivity is driven by persistent shocks (like commodity price shocks) or by anticipated temporary seasonal variation, and we estimate a regression of simulated violence on simulated wages. The model is solved by log-linearizing the winning and losing value functions and the first-order conditions around a non-stochastic steady state (where $\theta_t = \bar{\theta} \forall t$).⁹ See Appendix 1.2 for a list of equations.

Calibration. We calibrate the strength of the true opportunity cost mechanism $-\phi = -3$ to match the estimated elasticity of violence with respect to wages in Colombia driven by coffee price shocks, using data from Dube and Vargas (2013) and estimated using indirect inference.¹⁰ This means that, other things equal, a 1% increase in wages should lead to a 3% fall in time spent allocated to violence. We calibrate relative strength of the insurgent forces ψ so that given the other parameters, the insurgents have a 0.1% chance of winning each quarter.¹¹ See Appendix 1.3 for further details on the calibration.

⁸More formally, note that in the infinite horizon setup, Equation 1 is a Belman equation (with some abuse of notation for consistency with the two-period model). $U^L(\theta)$ is the value (discounted lifetime expected utility) of an out-of-power fighter. The state is θ . $U^{Win}(\theta)$ is the value of the part-time fighter who is in power, which can be written recursively as $U^{Win}(\theta) = \theta + \beta E U^{Win}(\theta')$, where a prime denotes the next period's value.

⁹This is a first-order Taylor series approximation of the model's FOCs and value functions but with respect to $\log X_t$ rather than to X_t .

¹⁰The indirect inference approach involves choosing ϕ so that estimating Equation 3 in the simulated and actual data generates the same β_{Opp}^{Meas} . Such an approach is needed because we know β_{Opp}^{Meas} will be biased upward. We cannot calibrate using our empirical results, as we do not observe wages at seasonal frequencies. See Online Appendix 1.2.1.

¹¹That is, $\psi = 0.0065$. Other parameters: $\alpha = 0.5$, $\beta = 0.99$ and $\bar{\theta} = 1$.

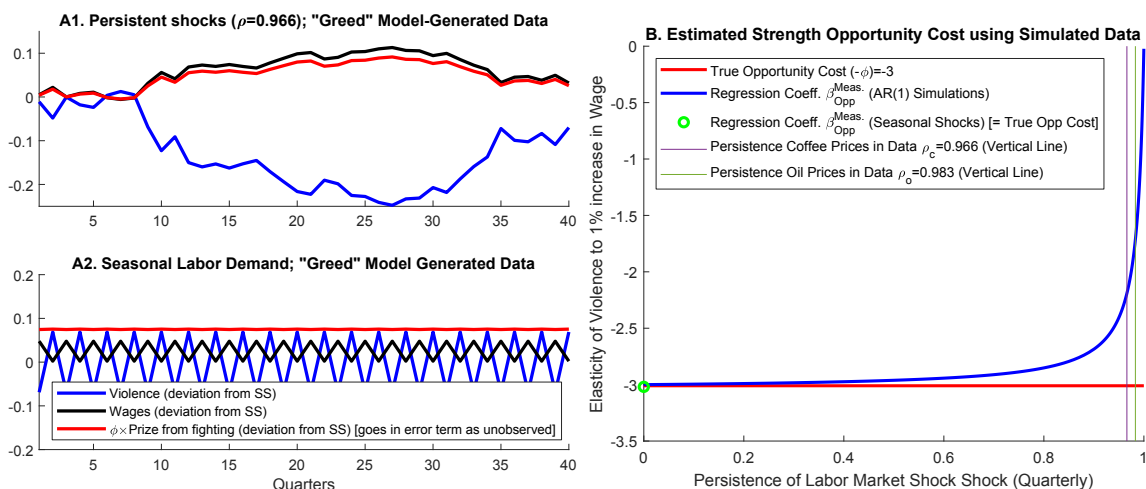


FIGURE 1. Panel A (LHS) shows the simulated data, and Panel B (RHS) shows the estimated coefficient.

Results. The results are best illustrated with an example shown in Figure 1. Panel A1 (top) of Figure 1 is simulated random data generated by shocks as persistent as coffee prices ($\rho = 0.966$ quarterly). One can see that around quarter 10, a large shock raises wages substantially (black line), increasing the opportunity cost of fighting and resulting in a fall in time allocated to violence (blue line). However, as this shock is unanticipated and persistent, it also raises the value of the prize of winning (red line). The higher prize of winning *increases* violence, other things equal. Combining the higher wages and the larger prize, the graph shows that violence still falls overall but not as much as it should based on the opportunity cost mechanism. For example, by period 25, wages are up by 10%, but violence has only fallen slightly more than 20% rather than the 30% fall expected by the opportunity cost mechanism ($-3 \times 10\%$).

Simulations driven by seasonal shocks are shown in Panel A2 (bottom) of Figure 1. The figure shows that wages go up and down with the seasons (black lines) and violence goes down and up (blue lines) with three times the volatility, as expected, based on $-\phi = -3$. In contrast with results in Panel A1, the value of the prize from winning is unaffected by the anticipated and temporary seasonal shock and is thus completely constant (red line).

What does this mean for the *estimated* strength of the opportunity cost mechanism? Given that the value of winning is typically unobserved, researchers estimate $\beta_{Opp}^{Meas.}$ in univariate Equation 3 with the prize of winning going in the error term e_t . Panel B

of Figure 1 plots $\beta_{Opp}^{Meas.}$ estimated using simulated data generated by AR(1) shocks of different degrees of persistence (blue curve). The true strength of the opportunity cost mechanism ($-\phi = -3$) is plotted in red. $\beta_{Opp}^{Meas.}$ is close to -3 when shocks are almost temporary, while $\beta_{Opp}^{Meas.}$ increases sharply as shocks become persistent. Namely, as $\rho \rightarrow 1$ (more permanent), $\beta_{Opp}^{Meas.} \rightarrow 0$, as in Proposition 1B above. Unfortunately, the region on the right of the persistence parameter space with the largest bias is where a number of shocks studied in the literature reside, such as coffee and oil price shocks (marked with pink and green vertical lines, respectively). This means that labor market shocks with the persistence of coffee prices lead to an upward bias of around $1/4$ (i.e. $\beta_{Opp}^{Meas.} = -2.2$ rather than $\beta_{Opp}^{True} = -3$).¹² For oil prices, things are even worse due to higher persistence, as there would be an upward bias of almost $1/2$ ($\beta_{Opp}^{Meas.} = -1.7$ rather than rather than $\beta_{Opp}^{True} = -3$).¹³ The large upward bias is able to rationalize why many studies find mixed evidence of the opportunity cost mechanism using commodity prices (Blattman and Bazzi 2013) or even a positive impact for other types of shocks, such as those driven by development programs.

In contrast, a regression run on seasonal shocks delivers almost exactly the true opportunity cost estimate of $\beta_{Opp}^{Meas.} = \beta_{Opp}^{True} = -3$ (green circle), because the value of the prize of victory is kept constant. This finding motivates the empirical work in the next section.

2.1.5. *Anticipated Shocks.* The second advantage of seasonal shocks is that they are not only temporary but also *anticipated*. The unobserved value of the prize of winning in Equation 2 is the expected discounted value of future profits, much like a share price. Just as for shares, *anticipated* changes in future profits have less of an effect on the current prize—even if those changes are persistent—because they are already “priced in”. Hence the estimated size of the opportunity cost mechanism is close to unbiased using anticipated shocks, even when those shocks are persistent and the value of the prize is unobserved (see Appendix 1.1).

¹²To be clear, the results in Dube and Vargas (2013) for coffee prices are correct but are possibly even stronger than estimated if there is fighting to control the revenues from coffee plantations. Dube and Vargas (2013) argue that this channel is relatively weak because coffee production is more labor intensive, and the earnings are difficult to appropriate.

¹³Moreover, our estimates of the persistence of commodity prices are conservative—with a different sample and the Andrews (1993) median-unbiased estimator, Cashin et al. (2000) find that commodity price persistence is often $\rho = 1$, suggesting $\beta_{Opp}^{Meas.} = 0$.

2.2. Grievance Model. Although the motivation for fighting is very different, a “grievance” model generates very similar results for seasonal and persistent shocks as the “greed” model presented above. The model’s details are presented in Appendix 2, but we sketch the argument here. In the grievance model, there is no monetary benefit from “winning,” but rather the insurgent holds a grievance which means she gains utility from time spent allocated to violence (V). The insurgent also needs to work at wage W to consume (C).

We make the standard assumption that she likes both consumption and time allocated to violence but has diminishing marginal utility in both. Higher wages have two effects in the model: (i) they increase the opportunity cost of fighting (a substitution effect) which implies a reduction in V and (ii) they make the agent wealthier (higher consumption), which makes her want to increase V , as the grievance is now relatively more subjectively important (an income effect).

As consumption is typically not controlled for in conflict regressions, the income effect enters the error term, and univariate opportunity cost regressions with persistent shocks will be biased upward (towards zero). More specifically, we can write the agent’s first-order condition in the grievance model in the same form as in Equation 4 but where $\phi \ln E_t \Pi_{t+1}^{Prize}$ is replaced by $\phi \ln C_t$.¹⁴ Households smooth consumption over time by the PIH. Hence, persistent unanticipated increases in wages lead to an increase in $\phi \ln C_t$, which increases violence and leads univariate estimates of the opportunity cost mechanism to be biased upward toward zero.

In contrast, consumption remains constant under temporary and anticipated seasonal shocks (households save temporary income shocks under the PIH), keeping $\phi \ln C_t$ constant and yielding unbiased univariate estimates of the opportunity cost mechanism. Appendix 2.3 also includes a counterinsurgency information model of violence, which generates similar results through the same income effect mechanism.¹⁵

¹⁴Here we assume log preferences for consumption, so $\sigma = 1$ drops out. See Appendix 2 for the complete form.

¹⁵In that counterinsurgency information model, the level of violence depends negatively on the counterinsurgency information provided to the government. Households get paid to provide that information, which they view as “snitching” and intrinsically dislike providing. An increase in wages that increases the opportunity cost of fighting also means that the richer households want to provide less counterinsurgency information, increasing violence and biasing univariate opportunity cost estimates.

3. HARVEST AND CONFLICT: EMPIRICAL METHODOLOGY

The theoretical framework above suggests that seasonal shocks are well suited to gauge the true importance of the opportunity cost mechanism in conflict settings. We now investigate this empirically in Iraq, Afghanistan and Pakistan. These three countries were chosen because they have long-lasting conflicts, experience a large seasonal shock to labor demand due to the harvesting of wheat, and have fine-grained data on both harvest and conflict intensity. This section describes our methodology and data, and Section 4 presents our main results.

3.1. Empirical Specification. Ideally, we would run a regression of time spent fighting on variation in monthly wages driven by harvest shocks, as suggested by our theoretical model. Unfortunately, in conflict settings there are typically no detailed monthly panels of time use or local wages. Hence we estimate the reduced-form effect of the onset of harvest (and the amount harvested) on conflict intensity. In this context, a negative coefficient on harvest intensity is consistent with increases in local labor demand due to harvest reducing the attractiveness of fighting (the opportunity cost mechanism).

Civil war conflict intensity is typically measured as the number of conflict incidents (“attacks”) in a particular subnational region over a certain period, as in Dube and Vargas (2013) and Berman et al. (2011). However, a key practical challenge is the wide variation in measures of conflict intensity across countries, within countries, over time, and in different datasets. For example, descriptive statistics in Appendix Table A.4 suggest the mean number of attacks per district-year in Iraq ranges from 19 to 317, depending on the dataset and definition of attack (e.g., whether it involves casualties). In Pakistan, there are, on average, 2–4 terrorist/militant attacks per district-year, and in Afghanistan there are 3–45 attacks per district-year, again depending on the definition and dataset. The intensity of conflict also varies substantially across years depending on geopolitical shocks.

For these reasons, we seek to produce a *normalized* measure of attacks that is comparable across datasets and countries with widely varying conflict intensity, with the number of attacks per district-year being the normalizing variable. In other words, our outcome of interest, $\%Attacks_{imt} = \frac{Attacks_{imt}}{Attacks_{it}} \times 100$, is the percentage of attacks in district i in month m in year t ($Attacks_{imt}$) relative to the total number of attacks in the same district in year t ($Attacks_{it}$). By construction, the mean of $\%Attacks_{imt}$

is close to $1/12 = 8.3\%$, though it can depart slightly from $1/12$ when datasets start or finish mid-year.

Our key independent variable seeks to capture *when* a region is in harvest and the *intensity* of harvest (as a proxy, the size of the shock to labor demand). It is constructed as the fraction of district i in harvest in a particular month m ($Harv_{im}$) interacted with the land area harvested ($Prod_i$). $Prod_i$ is based on pre-war wheat production data and is thus time invariant, but $Harv_{im}$ varies within each country due to local climatic conditions (though it is also based on pre-war data). Hence our independent variable $Harv_{im} \times Prod_i$ is the number of hundred square kilometers of wheat in harvest in district i , month m , and year t . Our main estimated equation is

$$(6) \quad \%Attacks_{imt} = \alpha_{it} + \gamma_{mt} + \beta(Harv_{im} \times Prod_i) + \delta \mathbf{x}_{imt} + e_{imt},$$

where α_{it} is a district-by-year fixed effect to account for district- and year-specific factors affecting conflict, γ_{mt} is a month-of-the-year fixed effect (e.g., June 2005), and \mathbf{x}_{imt} is a vector of monthly district characteristics such as temperature and precipitation. In all specifications we also control for the effect of the timing of planting on conflict intensity (not shown).¹⁶ The parameter of interest is β , which captures the effect of harvest on conflict intensity. Standard errors are clustered at the district level, which accounts for serial correlation in the error terms for that spatial unit.

For Afghanistan, opium production is a key confounding factor but one that appears less common in areas where wheat is rain-fed rather than irrigated (discussed further in Section 3.2.1). Hence, for Afghanistan, we include separate variables for areas where wheat is predominantly rain-fed (our focus) or irrigated. That is, $Harv_{im} \times Prod_i$ in Equation 6 is replaced by $RHarv_{im} \times RProd_i$, which is the area of rain-fed wheat (100 square kilometers) in harvest in district i , month m , and year t . We add the analogous variable for irrigated wheat $IHarv_{im} \times IProd_i$ as a control.

¹⁶Planting is also a temporary and anticipated labor demand shock, and indeed we find some evidence that conflict intensity falls at planting (and with a larger area cultivated). However, the results for planting are not as strong or robust as those for harvest, so in the interests of brevity, we do not report them. This might be because planting is a less urgent task than harvest, and possibly it is less labor intensive. Our planting variable is constructed in the same way as the harvest variable, as the interaction between a local planting month dummy variable and the local area under cultivation.

3.2. Identification Strategy. There are two challenges to identifying the effect of temporary harvest shocks on conflict intensity: reverse causality and omitted variable bias. Reverse causality is where changes in conflict for other reasons (changes in strategy, geopolitical shocks, and blind luck in the fog of war) drive changes in harvest timing or intensity $Harv_{im} \times Prod_i$. We rule out reverse causality because our harvest timing and intensity variables are constructed using pre-war data. More specifically, wheat production is driven by a range of time-invariant agricultural suitability indicators, such as local climate, rainfall, and soil type. The pre-war harvesting calendar $Harv_{im}$ is also driven by a combination of geographic and climate factors. By using pre-war data on the size of the local harvest rather than contemporaneous data, we rule out (for example) conflict affecting the ability of farmers to plant or harvest wheat.¹⁷ Because harvest is very time-sensitive—crops will not be ready or will start to rot—there is a limited ability for conflict to move the harvesting month away from that dictated by the pre-war calendar based on climate and geography.

Perhaps a more important threat to identification comes from omitted variable bias, where variables that are correlated with harvest timing or intensity can also affect conflict. In Section 2, we showed that in many dynamic models of conflict, shocks to opportunity cost are correlated with other unobservables (e.g., the value of the prize of fighting), biasing estimates of the strength of the opportunity cost mechanism. We also showed this was not a problem for harvest shocks given that they were temporary and anticipated. Nonetheless, other omitted variables may remain.

The first way we deal with omitted variable bias is to include district-by-year fixed effects α_{it} . These fixed effects will capture any time-invariant determinants of conflict (e.g., strategic location) as well as most other trends in conflict intensity in each district (e.g., how far away the district is from the “front line”). All remaining aggregate variation in conflict intensity is removed by month-of-the-year fixed effects γ_{mt} (e.g., the “fighting season” in the country, aggregate commodity prices, and geopolitical shocks). These fixed effects mean that any remaining confounding variables must vary both at the district level *and* within the year, greatly reducing the set of potential confounding variables.

Two of the variables that might meet these criteria are precipitation and temperature. Burke et al. (2009), Hsiang et al. (2013), and many others argue that

¹⁷Put another way, the amount harvested in a location in a given month has an endogenous component—due to conflict disrupting agricultural production (for example)—as well as an exogenous component due to the local climate, geography, and soil suitability. We only consider variation due to the second exogenous component.

temperature tends to increase violence. Miguel et al. (2004) and others argue that rainfall shocks affect conflict in Africa.¹⁸ For this reason, all specifications include controls for local temperature and precipitation (contemporaneously) in \mathbf{x}_{imt} .¹⁹

The final general threat to identification is where the timing and intensity of harvest does affect conflict but not through the opportunity cost mechanism posited. For example, it could be that insurgents try to capture the harvest during transportation, leading to an increase in conflict just after the harvest month, which makes conflict look lower during harvest by comparison. We address this threat by estimating dynamic specifications for conflict intensity in the months around harvest.

3.2.1. *Opium Production in Afghanistan.* One identification challenge specific to Afghanistan is the presence of opium production, as opium poppy grows in many of the same places as wheat and has a similar harvesting calendar. It is well known that opium production funds the Taliban (and other insurgent groups): they tax opium harvest and transportation, and are also involved in opium storage, trading and barter (Peters 2009).²⁰ This increases the ability of the Taliban to conduct attacks around harvest time, and indeed Piazza (2012) finds that Afghan provinces with higher opium production have higher levels of terrorist attacks. Thus opium production is a confounding factor that would bias our estimates of the strength of the opportunity cost mechanism toward zero.

We address this concern in two ways. First, as mentioned above, we take advantage of the fact that poppy—due to its high value—is mainly cultivated in irrigated areas, whereas wheat is cultivated in both irrigated and rain-fed ones.²¹ As such, we focus on the coefficient of rain-fed wheat as the cleanest measure of the opportunity cost

¹⁸In our context Appendix Figure A.4 shows how the onset of harvesting (roughly from May to July) is indeed accompanied by an increase in temperature and a decrease in precipitation in Iraq. Without controls, these factors would tend to bias our estimated strength of the opportunity cost.

¹⁹Our precipitation data (in millimeters) and average temperature (in degrees Celsius) are from Willmott and Matsuura (2015) for Iraq, Afghanistan, and Pakistan. See Appendix Figure A.7.

²⁰“[Opium] proceeds from ushr [a 10% Islamic tax applied to harvest], as well as commodities collected in barter deals, appear to supply village and district-level Taliban with the bulk of their operational needs, everything from salaries for fighters and transport, fuel, food, weapons, and explosives.” (Peters 2009: 19). While the Taliban announced a ban on opium cultivation in 2000-01, this was temporary. By 2002, opium production increased almost 10-fold (relative to 2001), making Afghanistan again the world’s largest producer (Peters 2009). According to the UNODC (2010: 5) “planting opium poppies is six times more profitable than growing wheat.”

²¹See Appendix Figure A.10 portraying the relative prevalence of wheat versus opium in rain-fed versus irrigated areas according to the UNODC (2004) report.

mechanism. Second, we limit the sample in Afghanistan to those districts for which the median opium production during our sample period is zero. This does not mean there was no production of poppy in that district but rather that it occurred in less than half of the sample months.²²

3.3. Conflict Data, Samples, and Background. Given the well-known difficulties in measuring the intensity of violence, we use different datasets on violence for each conflict setting to avoid assigning disproportionate weight to a single data collection procedure. We use both very precisely geolocated datasets of attacks (e.g., latitude, longitude) and datasets where attacks are aggregated to the district level. However, in the former case we always aggregate data to the district level to reduce measurement error. Additional details on data sources and variables used are described in Appendix 3.1. All of our datasets indicate that violence is geographically and temporally concentrated in particular district-years.²³

As discussed above, our main dependent variable is the percentage of attacks in district i and month m relative to its total in year t ($\frac{Attacks_{i,m,t}}{Attacks_{i,t}} \times 100$), which is not defined if the total number of attacks in a district-year is zero, and can be noisy when the number of attacks is very low. As such, we limit the sample to either district-years that are at least above the median of conflict intensity or above the mean (if the median is zero).

3.3.1. Iraq. Between 2003–2011, Iraq experienced a civil conflict against coalition forces and along sectarian lines. The intensity of the conflict, its large contingent of part-time fighters—at least 200,000 according to some estimates²⁴—coupled with the strong reliance on wheat cultivation in rural areas makes it an ideal setting to explore the importance of seasonal labor markets for violence intensity.

For Iraq, we use four conflict datasets over a common sample of 2004–2009. Coalition forces invaded Iraq in March and April 2003. However, the initial invasion was a regular interstate war and not a subnational conflict, which is the focus of this paper, so we start our sample in 2004. Our period of study covers the initial insurgency against the coalition forces during 2004–2006 as well as its evolution into a sectarian

²²48% of district-year observations between 2004–2009 (WITS data) and 70% of observations between 2008–2014 (SIGACTS data) meet this criteria.

²³As noted in Appendix Table A.4, the median number of attacks in a district-year across datasets is zero or one.

²⁴The same source estimates around 40,000 “core” or full-time fighters. See (last accessed February 2, 2020): https://www.globalsecurity.org/military/ops/iraq_insurgency.htm.

conflict following the bombing of the Shia al-Askari mosque in February 2006. Our sample includes the surge of US troops in 2007–2008. We finish our sample at the end of 2009, in anticipation of the withdrawal of US combat forces from Iraq by the middle of 2010. Although the US maintained a large number of troops until the end of 2011, they were used mostly to support the Iraqi military, and 2010–2011 reported lower conflict intensity than earlier years.²⁵

We use four datasets in Iraq because each one provides an incomplete (and noisy) measure of the conflict intensity. Our first data source is the Iraq Body Count dataset (IBC), which tracks civilian deaths due to sectarian conflict (the vast majority), insurgent-coalition conflict, and coalition forces directly. The IBC data are collected by a non-profit organization, are based on media and administrative sources and are sourced from the Empirical Studies of Conflict (ESOC) Project’s conflict data repository at Princeton University (<https://esoc.princeton.edu/>). Our main results use the total number of deadly events, but we also consider disaggregated data by perpetrator. One advantage of the IBC data is that attacks do not have to be witnessed by coalition forces, though the disadvantage is that many serious attacks only involve combatants (not civilians) and so would not be included in the IBC.

Our second data source for Iraq is the World Incident Tracking System (WITS), which exclusively focuses on terrorism events based on media reports. The WITS data were collected by the National Counter-terrorism Center for the US State Department until they were discontinued in 2012 (they were also download from ESOC). We focus on total attacks across all categories, though we also consider types of attacks that might be more labor intensive (armed attacks) or less labor intensive (bombings). The WITS dataset captures terrorist attacks against civilians and noncombatant targets that are both premeditated and politically motivated, which may end up excluding many attacks against coalition forces (and is limited to those reported in the media).

Our final two data sources are based on coalition reports of events of “significant activity” (SIGACTS). The first one is more general/aggregate from Berman et al. (2011a) (SIGACTS-BFS), which includes all “enemy activity” (deadly and not deadly). We also use a version of the same underlying dataset disclosed by Wikileaks to *The Guardian* that focuses on “significant activity” that results in deaths on either side of the conflict (SIGACTS-WIKI).²⁶ Berman et al. (2011a) report their results

²⁵Our sample does not cover the Iraqi civil war from 2014 following the rise of ISIL.

²⁶Last accessed February 2, 2020: <https://www.theguardian.com/news/datablog/2010/oct/23/wikileaks-iraq-data-journalism#data>.

weighted by population, so we report both weighted and unweighted results using the SIGACTS-BFS dataset.

We also report heterogeneity by the type of significant attack: more labor intensive (direct fire), less labor intensive (explosives/IEDs), and capital intensive (indirect fire). The advantage of the SIGACTS datasets is that they mostly record conflict incidents that are closest to those in theory—combat between the government and an insurgent group—and the data are official. The disadvantages are that they only capture events witnessed by coalition forces, and so they would undercount, for example, sectarian conflict. Moreover, the quality of this dataset could greatly vary across the combat units collecting it (Berman et al. 2011a: 790).

3.3.2. Afghanistan and Pakistan. Following the 9/11 terrorist attacks, the US and its allies invaded Afghanistan in late 2001 to overthrow the Taliban, which had been harboring Al-Qaeda. Many of the Taliban and Al-Qaeda fighters were not captured but instead escaped into rural or mountainous areas or moved across the porous Pakistani border. In Afghanistan, the Taliban launched an insurgent movement to regain power. The insurgency waged asymmetric warfare against the US and its allies, known as the International Security Assistance Force (ISAF), as well as against members of the Afghan military and the Afghan government. The ISAF ceased combat operations at the end of 2014.²⁷

Afghanistan is a good location to test the opportunity cost mechanism through harvest given the Taliban’s reliance on rural part-time fighters. Most of the Taliban recruits come from poor *madrassas*, motivated by local grievances, and participate only on a part-time basis due to their work as farmers or laborers (Qazi 2011: 10). Taliban cells are composed of around 10 to 50 part-time fighters (Afsar et al. 2008: 65) who periodically gather to launch attacks but then return to work as laborers or farmers.

Ideally, our Afghanistan sample would cover the whole ISAF period from 2002–2014, but in practice our sample is dictated by sample period of publicly available data. Our first dataset for Afghanistan is the WITS, which focuses exclusively on terrorism events based on media reports. It is available for 2004–2009 and was downloaded from the ESOC repository. Like the WITS data for Iraq, our default variable is “total attacks,” but we also report results for more labor-intensive types of attacks (firearms or attacks excluding bombings) or less labor-intensive attacks (bombings).

²⁷Since 2015, the NATO-led mission “Resolute Support” has been focused on training and supporting the Afghan military.

The second dataset for Afghanistan is on “significant activity,” reported by the ISAF troops in Afghanistan from 2008 to 2014. This dataset is similar to the SIGACTS dataset for Iraq, though it is a little more detailed. Our main variable is total enemy attacks, but as for Iraq we also use direct fire as a more labor-intensive attack type and use bombings as a less labor-intensive type. The data were declassified in 2014 and can be downloaded from Centcom FOIA library files or from Vincent Bauer’s website.²⁸ In the appendix we consider a placebo test using counterinsurgency actions.

For Pakistan, our sample starts in 2002 following the arrival of the ISAF in neighboring Afghanistan, and the start of a rising trend of violence in a number of provinces in Pakistan (Shapiro and Gulzar 2012). Our sample ends in 2010 due to data availability. As in the other contexts, conflict in Pakistan is concentrated geographically, though unlike Iraq, it is of lower intensity, perpetrated by several groups, and concentrated in urban areas (Blair et al. 2013: 32). Our first data source for Pakistan is the BFRS Political Violence Dataset (Bueno de Mesquita et al. 2015), which is available at the district level over 2002–2010 and was collected from local newspaper reports (as opposed to only those in English). This dataset codes all instances of political violence, including those perpetrated by militant and state forces, and classifies them into whether they are conventional (more labor intensive) or asymmetric (e.g., less reliant on labor—IEDs, suicide bombs). Our main variable from the BFRS dataset is the number of militant attacks, which can be defined by either attack type or attack perpetrator. We also present results for more labor-intensive attacks (conventional attacks), less labor-intensive attacks (asymmetric attacks), and placebo tests using attacks by the state or foreign forces.

Our second dataset for Pakistan’s terrorist attacks is the Global Terrorism Dataset (GTD) for the same period, which includes events defined as “the threatened or actual use of illegal force and violence by a non-state actor to attain a political, economic, religious, or social goal through fear, coercion, or intimidation.” It is available over 2002–2010 and was collected by the University of Maryland.²⁹ Our main variable is total attacks, though we also consider labor-intensive attacks (total attacks excluding bombings) and less labor-intensive attacks (bombings). Both the BFRS and GTD datasets were downloaded from the ESOC data repository.

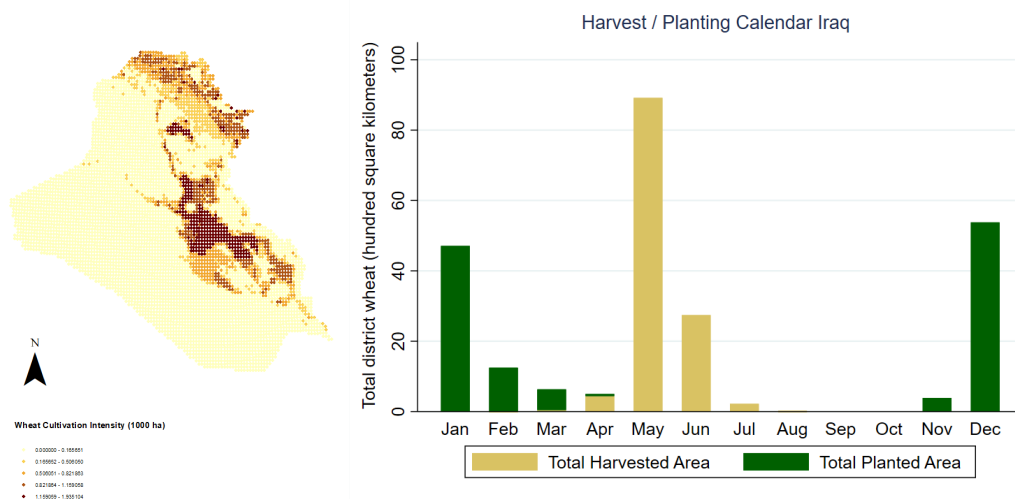
²⁸<https://stanford.edu/~vbauer/data.html>.

²⁹See <http://start.umd.edu/gtd/>. Starting in 2012, GTD became the dataset used by the US State Department to report terrorist incidents, as WITS was discontinued. Because WITS is similar to GTD in sources and motivation, for each country, we either use WITS or GTD.

3.4. Harvest Calendar and Harvest Intensity. Based on the intensity of wheat cultivation and the timing for harvesting for each district-month, our main explanatory variable is the interaction of the size of the area harvested with a dummy variable for whether that district that is in harvested in that month. The right panels of Figures 2–4 show the distribution of this variable for Iraq, Afghanistan, and Pakistan such that each column represents the total weighted number of square kilometers at harvest (or planting) in a given month. The intensity of harvesting varies across months within the year. For Iraq (Figure 2), most districts harvest wheat in May–June, yet some areas also harvest as early as April or as late as July.

Wheat Cultivation Intensity. Wheat intensity is measured in hundreds of square kilometers and is calculated by the Food and Agriculture Organization (FAO) as the historical average of the period 1960–1990, which clearly precedes our sample. The left panels of Figure 2–4 illustrates the data by showing the raw images provided by the FAO Global Agro-Ecological Zones³⁰ (GAEZ v3.0) of the intensity of wheat harvesting at the 5x5 arc-minute grid cell level. For Afghanistan, the images are divided into areas of rain-fed wheat and irrigated wheat (for an example, see Appendix Figure A.5). This fine-grained information is then aggregated at the district level to calculate the intensity with which a given district is “in harvest.”

FIGURE 2. Wheat Production (L) and Calendar (R) in Iraq



³⁰ Available at: <http://www.gaez.iiasa.ac.at/>.

FIGURE 3. Wheat Production (L) and Calendar (R) in Pakistan

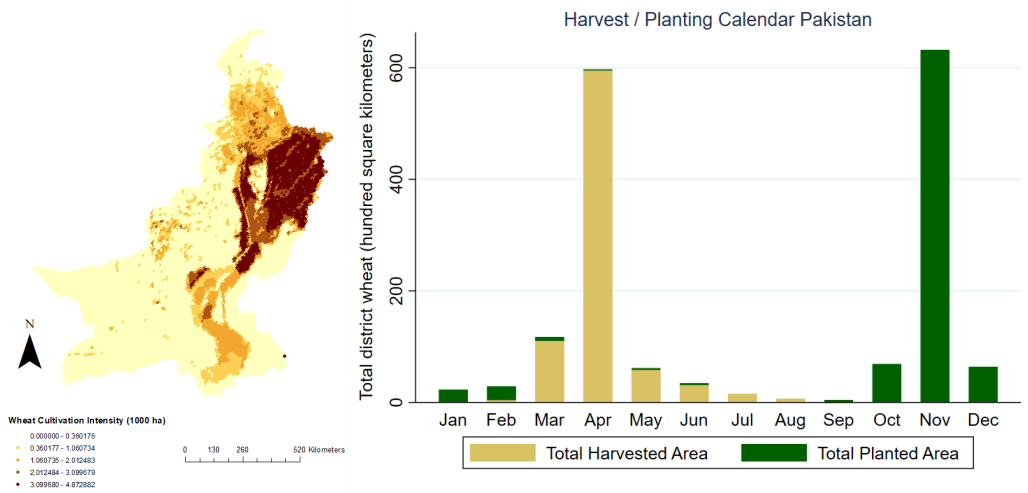
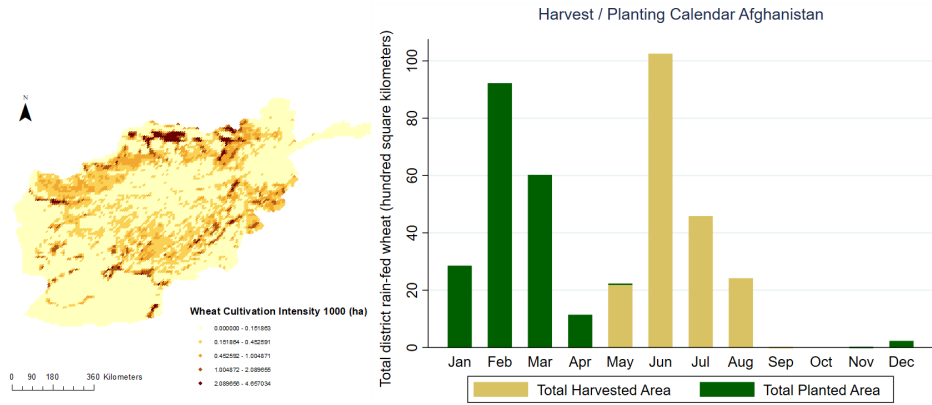


FIGURE 4. Wheat Production (L) and Calendar (R) in Afghanistan



Harvesting and Planting Calendars. To calculate the *timing* of planting and harvest, we again rely on the FAO-GAEZ data, which provide high resolution maps for the start and length of the wheat growing cycle depending on the level of inputs used (high, medium, or low) and water sources (rain-fed or irrigated by rivers).³¹ Specifically, we combine the calendar start day of the cycle (e.g., day 30, meaning late January) with how long wheat is expected to grow in that grid cell (e.g., 60,

³¹For an example and additional details, see Appendix Figure A.6.

90, or more days) to identify the month of harvest (and planting). Our planting and harvesting indicator thus generally takes the value of 1 in the month the growing cycle starts and ends, respectively (and zero otherwise).

However, in some grid cells there can be more than one harvesting and planting month, depending on the inputs used. For example, while high-input wheat in a grid cell might be harvested in June, low-input wheat in the same grid may be harvested in May. For these very few cases, we created an average harvest indicator by dividing equally across these months (e.g., weighting by 0.5 if there are two months of harvest or 0.33 for three months). As with the wheat intensity data, we then aggregate this variable to the district level, as these encompass numerous 5x5 arc-minute grid cells.

4. HARVEST AND CONFLICT: EMPIRICAL RESULTS

In this section we present empirical estimates of the extent to which harvest reduces conflict intensity. We present the results for Iraq first in more detail (Section 4.1) and then the results for Afghanistan (Section 4.2) and Pakistan (Section 4.3) more concisely, as they are broadly similar. In all three countries we find that harvest tends to reduce conflict intensity.

4.1. Iraq. Table 1 presents our main empirical results of the effect of harvest on conflict intensity in Iraq. In sum, four of our five estimates suggest that the onset of harvest (and greater harvest intensity) leads to a statistically significant reduction in the monthly share of attacks.

In column 1, our dependent variable is the monthly share of deadly events resulting in civilian deaths (relative to the district-year total), taken from the IBC dataset. The coefficient of interest on harvest intensity is -1.58 , significant at the 1% level. This can be interpreted as follows: an increase of 100 square kilometers of wheat production at harvest leads to a reduction in the share of deadly episodes by around 1.6 percentage points. Given that the average wheat cultivation intensity per district is 1.26 hundred square kilometers in our sample, and the mean share of attacks in a month is around 9%, this represents a 22% reduction ($-1.58 \times 1.26/9.0$). In column 2, which uses total terrorist attacks from the WITS dataset, the estimated coefficient is slightly smaller such that an extra 100 square kilometers of wheat at harvest reduces the share of attacks in the month by around 1ppt (significant at the 5% level), representing a 15% fall in the number of monthly attacks. Column 3, which uses the monthly share of deadly enemy actions recorded by coalition forces (SIGACTS-WIKI), generates

almost identical results to the IBC dataset in column 1 (also significant at the 1% level).

Columns 4–5 present results using the monthly share of total attacks—including those that are not deadly—as recorded by coalition forces and used in Berman et al. (2011a). Column 4 presents unweighted estimates, as in the rest of the paper, with column 5 reporting estimates weighed by population, as in Berman et al. (2011a).³² The weighted estimates indicate that an extra 100 square kilometers of wheat at harvest reduces the monthly share of attacks by around 0.5ppts, significant at the 5% level (a 6% fall in the number of monthly attacks). The unweighted estimates—while still negative in sign—are smaller than absolute value and are insignificant. The fact that both sets of point estimates are smaller in absolute value than those in columns 1–3 could indicate that terrorist attacks and more serious incidents (resulting in casualties) are more sensitive to labor demand shocks than broad categories of all events used by Berman et al. (2011a).

4.1.1. *Dynamics.* The estimates in Table 1 indicate that conflict is lower at harvest relative to the rest of the year. But this difference does not tell us whether harvest reduces conflict in an absolute sense or whether insurgents conduct the same number of overall attacks but simply delay them until the part-time fighters return from harvest (insurgents could also bring forward attacks, in anticipation of upcoming reductions in fighting strength).

Moreover, a relative fall in conflict intensity at harvest time can also mask direct effects of harvest on conflict intensity through channels other than the opportunity cost mechanism. For example, conflict could be lower at harvest time, not because of the opportunity cost mechanism but because violence spikes after harvest, as its proceeds are used to finance insurgent activity.³³ Alternatively, insurgents could try to capture the harvest itself while it is being stored and transported.

To test these hypotheses, Table 2 presents the estimates from a full dynamic specification where conflict intensity is regressed on harvest with two leads and lags. If conflict is simply moved to surrounding months, we would expect to see positive and significant leads and lags. Alternatively, if the proceeds of harvest were financing

³²Note that all other estimates are similar when weighting by yearly district population (see Appendix Table A.6). But because we lack similar population data for Pakistan and Afghanistan, we prefer the unweighted estimates for consistency.

³³Likewise, the government could use taxes on harvest to finance counterinsurgency operations, leading to more violence.

TABLE 1. Seasonal Labor and Conflict Intensity in Iraq (2004–2009)

	(1)	(2)	(3)	(4)	(5)
Data Source:	IBC	WITS	SIGACTS-WIKI	SIGACTS-BSF	SIGACTS-BSF
Dep. Variable:	Deadly Events	Total Attacks	Deadly Enemy Activity	Enemy Activity	Enemy Activity
					Pop. Weight
$Harv_{int} \times Prod_i$	-1.583*** (0.584)	-0.975** (0.487)	-1.526*** (0.466)	-0.213 (0.169)	-0.452** (0.181)
Observations	4,110	4,416	4,680	3,215	3,175
District X Year FEs	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Clusters	89	88	96	66	66
Mean DV	8.978	8.333	8.333	8.989	8.472
Mean Wheat Intensity	1.262	1.252	1.185	1.168	1.166
Avg. Effect %	-22.24	-14.64	-21.69	-2.762	-6.223
Controls	Temperature, Precipitation, $Planting_{int} \times Prod_i$				
Sample	Jan2004- Jun2009	Jan2004- Dec2009	Jan2004- Dec2009	Feb2004- Feb2009	Feb2004- Feb2009

Clustered robust standard errors at the district level are in parentheses. Wheat Intensity $Prod_i$ is measured in 100 square kilometers. DV is in monthly %. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

conflict (or fought over), only the lags of harvest would be positive and significant. However the estimates in Table 2 suggest that, in most cases, there is only a significant fall in conflict at harvest time.³⁴ In fact, in no dataset do we see significantly *higher* levels of conflict immediately before or after the conflict, which rules out the possibility that harvest in some ways finances conflict or creates it.³⁵

4.1.2. *Tactics and Perpetrators.* In addition to looking at aggregate measures of insurgent activity, we also examine the *types* of attacks employed by insurgents. Our

³⁴In the IBC dataset, there is some evidence of a reduction in conflict the month before and after harvest, but it is only significant at the 10% level and is half the size of the reduction in the harvest month itself. This may be due to some measurement error in the timing of harvest, which can vary slightly from year to year.

³⁵This pattern also makes other explanations unlikely, for example, that the drop in conflict is driven by insurgents “letting” the harvest take place as a way to curry their favor. If that were the case, we would expect higher levels of conflict before and after the harvest, and there would also be no difference in the type of attack (e.g., labor intensive versus others).

TABLE 2. Dynamic Specification in Iraq (2004–2009)

	(1)	(2)	(3)	(4)	(5)
Data Source:	IBC	WITS	SIGACTS-WIKI	SIGACTS-BSF	SIGACTS-BSF
Dep. Variable:	Deadly Events	Total Attacks	Enemy Activity	Enemy Activity	Enemy Activity
					Pop. Weighted
$Harv_{im-2} \times Prod_i$	-0.54 (0.80)	-0.10 (0.68)	-0.54 (0.59)	0.00 (0.16)	-0.03 (0.14)
$Harv_{im-1} \times Prod_i$	-1.07* (0.55)	-0.61 (0.55)	-0.06 (0.70)	0.24 (0.15)	0.15 (0.14)
$Harv_{im} \times Prod_i$	-2.02*** (0.72)	-0.94 (0.60)	-1.76** (0.73)	-0.30* (0.16)	-0.57*** (0.16)
$Harv_{im+1} \times Prod_i$	-1.39* (0.75)	-0.09 (1.10)	-0.53 (0.84)	0.15 (0.28)	0.01 (0.34)
$Harv_{im+2} \times Prod_i$	0.85 (0.87)	0.05 (0.44)	-0.12 (0.66)	0.21 (0.24)	0.14 (0.27)
Observations	4,110	4,416	4,680	3,215	3,175
District X Year FEs	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Clusters	89	88	96	66	66
Mean DV	8.978	8.333	8.333	8.989	8.472
Mean Wheat Intensity	1.262	1.252	1.185	1.168	1.166
Avg. Effect	-28.45	-14.07	-24.98	-3.917	-7.859
Controls	Temperature, Precipitation, $Planting_{imt} \times Prod_i$				
Sample	Jan2004- Jun2009	Jan2004- Dec2009	Jan2004- Dec2009	Feb2004- Feb2009	Feb2004- Feb2009

Clustered robust standard errors at the district level are in parentheses. Wheat Intensity $Prod_i$ is measured in 100 square kilometers. DV is in monthly %. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

idea is that insurgents might change tactics depending on the availability of labor versus other inputs (Bueno de Mesquita 2013). Specifically, we focus on *labor-intensive* attacks: those that require greater manpower to be carried out, e.g., direct/armed attacks involving groups of individuals using small arms or rocket-propelled grenades (Condra et al. 2018: 3208). The first three columns of Table 3 replace total attacks in our main specification with measures of labor-intensive attacks (which vary by dataset): armed attacks from the WITS dataset (column 1), direct fire resulting in casualties from the SIGACTS-WIKI datasets (column 2), and all direct fire attacks

from the SIGACTS-BSF dataset (column 3).³⁶ In each case, harvest results in a reduction in the monthly share of labor-intensive attacks, which is consistent with the opportunity cost mechanism.

In contrast, *asymmetric* attacks with no exchange of fire generally have lower manpower requirements (such as bombings and IEDs). These type of attacks can act as a placebo test for our mechanism. Appendix Table A.7, columns 2–4 shows that, indeed, for a range of less labor-intensive attacks, harvest has no effect on conflict intensity.³⁷ Combined with the findings in Table 3 (columns 1–3), these results suggest that harvest is more likely to reduce labor-intensive attacks and not other attacks, which is what the opportunity cost mechanism would predict.

The IBC dataset does not record the type of attack but does record the attack’s perpetrators. Our theory suggests that only the capacity of non-state actors like insurgents or sectarian militias are likely to be constrained by a shortage of part-time labor at harvest. In contrast, the US-led coalition forces use only professional full-time soldiers, and so their fighting capacity will be unaffected by harvest. The final two columns in Table 3 show that a reduction in conflict intensity at harvest is driven by insurgent groups and sectarian violence, consistent with the opportunity cost mechanism. However, there is also some weak evidence that harvest also reduces the number of attacks by coalition forces (Appendix Table A.7, column 1). This could also be due to the coalition responding to insurgent attacks, and so a reduction in the latter also affects the former.³⁸

4.1.3. *Corroborating Evidence.* In this subsection, we discuss some suggestive evidence in favor of the effect of harvest on conflict intensity through the opportunity cost mechanism.

Employment and Harvest. The mechanism in our theoretical model suggests that harvest affects conflict intensity through the demand for labor. However, there

³⁶The attack variable is the percentage of total annual labor-intensive attacks in a district that occur in a given month.

³⁷The results for capital intensive attacks—indirect fire in the SIGACTS dataset—are mixed; see Appendix Table A.7, columns 5–6. In the SIGACTS-BFS dataset, (population-weighted) harvest significantly reduces the intensity of indirect fire, but this is not robust to indirect fire resulting in casualties in the SIGACTS-WIKI dataset.

³⁸For example, the First Battle of Fallujah (Operation Vigilant Resolve) was launched in April 2004 in response to insurgent attacks that killed four Blackwater US security contractors. If the insurgent attacks did not happen due to harvest, then likely the US-led coalition response would not have happened either.

TABLE 3. Heterogeneity by Attack Type and Perpetrator (Iraq 2004–2009)

	(1)	(2)	(3)	(4)	(5)
Data Source:	WITS	SIGACTS-WIKI	SIGACTS-BSF	IBC	IBC
	Labor-Intensive Types			Perpetrator	
Dep. Variable:	Armed Attacks	Deadly Direct Fire	Direct Fire Pop. Weighted	Insurgent Attacks	Sectarian Attacks
$Harv_{imt} \times Prod_i$	-1.137* (0.640)	-1.033** (0.421)	-0.727** (0.291)	-0.905* (0.521)	-1.530** (0.681)
Observations	3,828	4,320	3,229	1,494	3,948
District X Year FEs	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Clusters	84	93	69	34	86
Mean DV	8.333	8.333	8.486	8.568	8.992
Mean Wheat Intensity	1.294	1.181	1.195	1.264	1.254
Avg. Effect %	-17.66	-14.64	-10.24	-13.35	-21.34
Controls	Temperature, Precipitation, $Planting_{imt} \times Prod_i$				
Sample	Jan2004-	Jan2004-	Feb2004-	Jan2004-	Jan2004-
Sample	Dec2009	Dec2009	Feb2009	Jun2009	Jun2009

Clustered robust standard errors at the district level are in parentheses. Wheat Intensity $Prod_i$ is measured in 100 square kilometers. DV is in monthly %. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

are no monthly panel data on employment or wages in our conflict settings to show this directly. Nonetheless, there is suggestive evidence from the 2006 World Bank Living Standards Measurement Survey (LSMS) that rural agricultural employment increases at harvest (and also slightly at planting).

Appendix Figure A.8 shows the difference in the monthly probability of employment for rural agricultural workers relative to rural non-agricultural workers.³⁹ These differences roughly follow the harvesting calendar in Iraq even after controlling for a number of individual factors and governorate fixed effects. This W-shaped pattern is consistent with the idea that harvest affects conflict by boosting demand for labor.

Migration. Our test of the opportunity cost mechanism requires that harvest and reduced conflict intensity happen in the same district. If instead people migrated to other districts to work on the harvest, the harvest location would be disconnected from the location of any reduction in violence, making it impossible to identify the

³⁹More specifically, the coefficient is from a regression of monthly employment status on a dummy for agricultural workers (the excluded category are rural non-agricultural workers). The LSMS is a pooled cross-section, so the monthly employment status dummies are retrospectives.

mechanism, even it was present. Luckily, the LSMS above asks about migration, and only 3.67% of agricultural workers report an absence from home for an extended period.⁴⁰

Harvest Income. Another concern is whether a reduction in conflict intensity could be due to the income received from harvest rather than to an increase in labor demand. Theoretically, our grievance model (see Appendix 2) predicts that the payment from harvest—which is temporary and anticipated—should be saved and so it will not affect the marginal utility of consumption or the valuation of the grievance.⁴¹ Practically, there is also likely to be some delay between harvest and payment, as most farmers sell their grain to the governmental Iraqi Grain Board, which only issues a receipt once all harvest is collected and stored in silos. The farmer then has to cash the receipt at a bank, adding further delays.

Religious Calendar. The timing of religious events can affect the intensity of conflict in either direction. All of our specifications include month-year fixed effects, which will remove the aggregate effects of Islamic religious festivities common to all districts, even if their exact dates changes each year. However, month-year fixed effects will be less effective if agricultural areas are more religious than others. Nonetheless, the 2008 Iraqi time-use survey suggests there is no significant difference in religiosity between agricultural and non-agricultural workers (see Appendix Figure A.9).

4.2. Afghanistan. The Taliban’s reliance on rural part-time fighters suggests the opportunity cost mechanism is likely to be important in Afghanistan, and indeed we find that conflict intensity is much more sensitive to harvest than in Iraq (or Pakistan). However, as harvested areas are smaller, the average effect turn out to be broadly similar. Recall that as opium cultivation is a potential confounding factor in Afghanistan (see Section 3.2.1), we focus on rain-fed wheat (where opium cultivation is less common) and restrict the sample to districts with zero median cultivation.

Our main results for Afghanistan are shown in the first two columns of Table 4. Column 1 reports the effect of rain-fed wheat harvest on the monthly share of terrorist attacks over 2004–2009 from the WITS dataset. We find that an extra hundred square

⁴⁰There is little evidence that individuals switch occupations, which would allow harvest demand to be met by non-agricultural workers. In the LSMS, only 4.67% reported more than one occupation throughout the year.

⁴¹Even if it was spent, this would reduce the marginal utility of consumption and hence encourage an *increase* in violence.

kilometers of rain-fed wheat at harvest reduces conflict intensity by 22 percentage points, more than 20 times the size of the effect in Iraq (or Pakistan). Districts in the WITS sample cultivate around 0.07 hundred square kilometers of rain-fed wheat, so harvest reduces conflict intensity by about 18%, on average. Column 2 reports the effect of rain-fed harvest on the share of monthly total enemy attacks over 2008–2014 from the SIGACTS dataset. An extra hundred square kilometers of rain-fed wheat at harvesting areas reduces conflict intensity by 4.4 percentage points, which reduces average conflict intensity in the average district by 8%. The size of the coefficient (−4.4) is still much larger in absolute value than in Iraq or Pakistan but is smaller than using the WITS dataset and sample. Unlike our estimates for Iraq (and Pakistan), the WITS and SIGACTS samples cover different years, and the difference in these estimates may be due to shifts in the intensity and nature of conflict occurring between these two periods. We also find the wheat harvest in irrigated areas has a smaller or insignificant effect on conflict intensity, which (as flagged above) is unsurprising given that coincident poppy harvesting will confound those estimates.

Dynamics. In the first two columns of Appendix Table A.8, we reestimate with two lags and two leads of harvest to test whether violence might be moved to the months surrounding harvest or if harvest might be funding conflict. The results for both the WITS (2004–2009) and SIGACT (2008–2014) datasets suggest a significant fall in conflict intensity at harvest (similar in size to that in Table 4), with no effect before or after.

Tactics and Perpetrators. As for Iraq (and Pakistan), we also explore the effect of harvest on different types of labor-intensive attacks, which are likely more sensitive to the supply of part-time fighters. As one can see in columns 3–5 of Table 4, harvest leads to reduction in attacks involving firearms (column 3, WITS dataset), attacks excluding bombings (column 4, WITS dataset), and direct fire attacks (column 5, SIGACTS dataset). The size and significance of harvest effects on labor-intensive attacks is similar to that on total attacks.

As a placebo test, Appendix Table A.9 estimates the effect of rain-fed harvest on the number of less-labor-intensive attacks, such as those involving bombs (mainly IEDs). For both the WITS and SIGACTS datasets, the harvest variable is insignificantly different from zero. Column 4 of Appendix Table A.9 investigates the effect of harvest on the intensity of counterinsurgency operations (attacks by the coalition forces between 2008–2014). Unsurprisingly, the harvest variable is also insignificant,

TABLE 4. Main Results and Heterogeneity in Afghanistan (2004–2014)

	(1)	(2)	(3)	(4)	(5)
Data Source:	WITS	SIGACTS	WITS	WITS	SIGACTS
	Main Results		Type of Attacks		
Dep. Variable:	Total Attacks	Enemy Attacks	Firearm Attack	Attacks excl. Bombing	Direct Fire
$RHarv_{im} \times RainProd_i$	-22.176*** (5.903)	-4.461*** (1.111)	-29.618*** (8.895)	-28.865*** (7.282)	-5.010*** (1.832)
$IHarv_{im} \times IrrigProd_i$	3.423 (3.377)	-1.570** (0.737)	4.613 (4.003)	3.169 (3.774)	-0.682 (1.022)
Observations	2,961	9,888	3,225	3,546	17,652
District X Year FEs	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Clusters	88	171	98	100	264
Mean DV	8.815	8.333	8.775	8.799	8.333
Mean Wheat Intensity	0.0718	0.148	0.0727	0.0727	0.157
Avg. Effect %	-18.06	-7.915	-24.55	-23.85	-9.443
Controls	Temperature, Precipitation, $Planting_{imt} \times Prod_i$				
Sample	Jan2004- Sep2009	Jan2008- Dec2014	Jan2004- Sep2009	Jan2004- Sep2009	Jan2008- Dec2014

Clustered robust standard errors at the district level are in parentheses. Wheat Intensity $Prod_i$ is measured in 100 square kilometers. DV is in monthly %. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

given that the coalition uses only full-time professional soldiers and not part-time fighters.

4.3. **Pakistan.** As for Iraq and Afghanistan, our results for Pakistan suggest that the onset of harvest reduces conflict intensity, mostly driven by labor-intensive attacks.

The first three columns of Table 5 present our main empirical results of the effect of harvest on conflict intensity in Pakistan. The first column shows that the onset of harvest is associated with a reduction in the monthly share of attacks when the identity of the militant perpetrator is known (ethnic, Islamist, sectarian, or other militant group) in the BFRS dataset, significant at the 5% level. The second column shows that the onset of harvest is associated with a reduction in all types of attacks typically used by militants (terrorism, conventional attacks such as direct fire or ambushes, or guerrilla attacks) using the BFRS dataset, significant at the 1% level. The size of the estimated effects are similar in Columns 1 and 2, suggesting that an

increase of a 100 square kilometers of wheat production at harvest leads to a reduction in the monthly share of militant attacks in a district by around 0.6 percentage points. Given that the average wheat cultivation intensity per district is 2.5–3.1 hundred square kilometers (depending on the sample), these estimates imply a 20% fall in the monthly share of attacks at average harvest intensity. The coefficient sizes are slightly smaller than those in Iraq though, as there is more wheat being harvested, the average effects is similar. The results in column 3 using a different dataset—terrorist attacks using the GTD dataset—are similar in size, significance, and average magnitude as those using the BFRS dataset.

TABLE 5. Main Results and Heterogeneity in Pakistan (2002–2010)

	(1)	(2)	(3)	(4)	(5)
Data Source:	BFRS	BFRS	GTD	BFRS	GTD
Dep. Variable:	Militant Perpetrator	Militant Attack Types	Total Attacks	Conventional Attacks	Attacks excl. Bombing
$Harv_{im} \times Prod_i$	-0.652** (0.283)	-0.545*** (0.177)	-0.537** (0.243)	-0.542*** (0.201)	-0.760*** (0.218)
Observations	2,100	2,652	2,700	2,268	2,400
District X Year FEs	Yes	Yes	Yes	Yes	Yes
Time Fixed Effects	Yes	Yes	Yes	Yes	Yes
Clusters	47	60	60	68	74
Mean DV	8.333	8.333	8.333	8.333	8.333
Mean Prod	2.456	3.149	2.921	3.504	3.701
Avg. Effect %	-19.21	-20.61	-18.82	-22.78	-33.76
Controls	Temperature, Precipitation, $Planting_{imt} \times Prod_i$				
Sample	Jan 2002-Dec 2010				

Clustered robust standard errors at the district level are in parentheses. Wheat Intensity $Prod_i$ is measured in 100 square kilometers. DV is in monthly %. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Dynamics. As for Iraq and Afghanistan, we examine the dynamic changes in conflict intensity around harvest. Columns 3–5 of Appendix Table A.8 re-estimates the main specifications in Table 5 with two monthly leads and two monthly lags of harvest intensity. In general, we find that the largest and most significant falls in conflict intensity occur at harvest time, but the results are more mixed than for the other countries. For militant attacks in the BFRS dataset (classified by perpetrator,

column 3), the only significant change in conflict intensity is at harvest, with a coefficient almost significant at the 5% level ($t\text{-stat} = 1.9$). For total attacks in the GTD dataset (column 5), we also find the largest and most significant fall in conflict intensity at harvest month. However, for the GTD dataset, we also find some evidence of a reduction in harvest intensity in the month before harvest, which might be due to harvest occurring on the border of two months.⁴² More important for our mechanism, there appears to be an increase of attacks following harvest for BFRS militant attacks classified by type (column 4), raising the possibility that harvest delays conflict or could fund it. However, as we do not see any increase in post-harvest conflict intensity in either of the other two specifications (in columns 3 or 5), we do not put too much weight on this result.

Tactics and Perpetrators. The final two columns in Table 5 show results for labor-intensive attacks in Pakistan. For the BFRS dataset, these are conventional militant attacks in which there is a larger chance of exchanging fire: “ambushes, direct fire, artillery, pitched battle and troop captures” (Bueno de Mesquita et al. 2015: 544). For the GTD dataset, labor-intensive attacks are total attacks excluding bombings (which are thought to be less labor intensive). In both cases the coefficients are of similar (or larger) magnitude as the main results using all attacks, and they are negative and significant at the 1% level.

We also consider several placebo tests, organized by attack type and perpetrator. Columns 7 and 8 of Appendix Table A.9 consider attacks that are less labor intensive—bombings and asymmetric attacks. The results are more mixed than for Iraq and Afghanistan: it appears that these type of attacks are sensitive to the onset of harvest in the BFRS dataset (column 8) but not in the GTD dataset (column 7). Turning to attacks organized by perpetrator, column 4 of Appendix Table A.9 shows that the onset of harvest has no impact on the intensity of attacks from foreign parties (mainly the US, India, Afghanistan, or multilateral), which is not surprising, as these parties are not reliant on part-time fighters.

For state-initiated attack—initiated by the military, paramilitary, or police against civilians or militants—results are mixed. Column 5 of Appendix Table A.9 shows state-initiated attacks increase with harvest onset (and harvest intensity), providing some tentative evidence that the state may be trying to protect the harvest from

⁴²For example, if harvest usually took place in the first few days of a month, then climate shocks could move it into the previous month in some years. This would explain the negative coefficient in the month before harvest in Appendix Table A.8, column 3.

militants or are perhaps taking strategic advantage of their reduced fighting capacity. However these results are based on a sample of district-years where the state attacks, which is different from the district-year sample of militant attacks used in the main results. If the state were responding to the threat of militants, the effect of harvest on state attacks should be even stronger in the sample with militant attacks. However, when we restrict the sample to district-years based on militant attacks, the effect disappears (column 6). As such, we do not put too much weight on these results.

5. CONCLUSIONS AND POLICY IMPLICATIONS

This paper studies variation in the opportunity costs of fighting as a key mechanism to explain the intensity of armed conflict across different settings. Our theoretical framework suggests that the wide range of estimates in the literature might be caused by variation in shock persistence, with estimates of the opportunity cost driven toward zero for the more persistent shocks. Instead, we propose using seasonal income shocks due to harvest—which are temporary and anticipated—as a more accurate way to estimate the effect of opportunity cost on conflict when the dynamic gains from fighting are hard to observe.

Applying our methodology in Iraq, Pakistan, and Afghanistan, we find that at average cultivation intensity, harvest reduces the average share of monthly attacks by around 6% to 22%, depending on the country and dataset used. Results are robust to a wide array controls, and dynamic specifications generally suggest an actual fall in conflict at harvest time rather than a shift in conflict to adjacent non-harvest months. We also find some evidence that the reduction in total attacks is driven by a fall in labor-intensive attacks.

Our results have three sets of policy implications. First, our empirical evidence in favor of the opportunity cost mechanism suggests that, in principle, employment programs or development aid can reduce the intensity of conflict by increasing the opportunity cost of fighting.

However, our second implication is that how these policies interact with the dynamic incentives to fight is crucial, and more permanent policies can have no effect on conflict intensity or can even increase it. For example, permanent transfers (in cash or food) may encourage fighting over the rents from these schemes or mean that households are wealthy enough to devote time to fight for causes they care about. For this reason, we are wary of policy prescriptions based on long-lasting employment programs or on permanent forms of development aid, as their persistence across periods may lead

to unintended consequences. This may explain the counterintuitive results of several recent studies (Nunn and Qian 2014; Crost et al.2014; Weintraub 2016).

Our final implication is that it might be possible to design more sophisticated policies that increase the opportunity cost of fighting without changing its dynamic benefits. Closest to our empirical results, a *temporary* employment program at a critical junction in a conflict, or in key fighting months, may increase the opportunity cost of fighting without affecting its dynamic benefits or subjective valuations of grievances.⁴³ A thorough investigation of these implications would be an interesting area for future research.

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⁴³A wage subsidy funded by local taxation (or a reduction in food/fuel subsidies) would boost the incentive to work without increasing the general level of income (affecting the value of the cause) or creating additional local rents to fight over. Making employment programs conditional on a successful counterinsurgency would mean that the extra income generated is disconnected from the value of winning.

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