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The Fiscal Cost of Conflict: Evidence from Afghanistan 2005-2016

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The Fiscal Cost of Conflict: Evidence from Afghanistan 2005-2016

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Abstract

I use a monthly panel of provincially-collected central government revenues and conflict fatalities to estimate government revenues lost due to conflict in Afghanistan since 2005. I identify causal effects by instrumenting for conflict using pre-sample ethno-linguistic share. Headline estimates are very large, implying total revenue losses since 2005 of $3bn, and future revenue gains from peace of about 6 percent of GDP per year. Reduced collection efficiency, rather than lower economic activity, appears to be the key channel. OLS estimates understate the causal effect by a factor of four. Comparing to estimates from Powell's (2017) generalized synthetic control method suggests that this bias results from omitted variables and measurement error in equal share. The findings underscore the considerable economic loss due to conflict, and the importance of careful identification in measuring this loss.

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1 Introduction

A violent conflict is one of the largest shock that an economy can face. Other factors, such as changes to technology, policy errors, and financial panics, all undoubtedly generate economic costs. But few events have the capacity to shatter economies and ruin living standards like an extended major conflict. This paper is an attempt to quantify one aspect of the economic impact of such a conflict: the fiscal loss from conflict in Afghanistan during 2005-16.

The ongoing conflict in Afghanistan is just one chapter in a much longer war. While some years have been less violent than others, for barely one year in the last 40 has the country been truly at peace. The socialist coup of 1978 presaged the subsequent Soviet invasion, Mujahideen resistance, post-Soviet-withdrawal civil war, Taliban ascendancy, and finally the invasion in 2001 by a US-led coalition. This history means that Afghanistan is perhaps the single most important case study for understanding the economic impact of conflict.

The loss of domestic government revenue is an important measure of the cost of conflict, as it captures directly the loss of the state's ability to provide basic services to its citizens, such as security, education, and health care. The economic cost of conflict is therefore an important part of the broader humanitarian impact of conflict. Fiscal records also have the added benefit of being a source of regular, consistently comparable information, a rare object in an otherwise data-poor environment.

In order to tackle this question, I construct a panel of central government revenue collected in the 34 provinces of Afghanistan between 2005 and 2016 – nearly 5,000 observations. I match this to data on provincial conflict fatalities and estimate a dynamic panel model to capture the impact of per capita fatalities on domestic revenues. I use three complementary methods: ordinary least squares (OLS – both standard dynamic equation estimation, and local projection in the style of Jordà (2005)), instrumental variables, and generalized synthetic controls (GSC) following Powell (2017). The instrument follows Bartik (1991) in interacting a cross-sectional share – entho-linguistic composition – with an aggregate time series – national conflict fatalities.

The strength of the instrument relies on the concentration of violence in the those provinces which have a historical ethnic link with the Taliban.

In this environment, a variety of factors may prevent OLS from revealing the causal impact of conflict on revenues. First, some shocks might simultaneously impact both conflict and revenues within a province – omitted variables bias.[1] While I try to account for this in the OLS setting, the challenges of data collection in Afghanistan during the sample period mean that even the richest specifications are likely to suffer from this problem. The instrumental variables and generalized synthetic control approaches address exactly this issue, identifying the causal effect of conflict if omitted variables bias is present. This is a particular concern given evidence that poor economic outcomes cause increased conflict. The instrumental variables approach is also valid in the presence of further challenges from reverse causality and attenuation bias: the former arises if low tax receipts preempt violence (perhaps by lowering state capacity to respond to insurgent attacks), and the latter if realized conflict fatalities are a noisy measure of the underlying security situation.

The main result is that conflict in Afghanistan has had a large and statistically significant effect on revenues. Counterfactual simulations suggest that estimated fiscal peace dividend is large. Conservative IV estimates suggest that a return to the low levels of violence in 2004 would result in an increase in annual revenues of around 50 percent, or approximately 6 percent of GDP per year. The historic cost of the Afghan conflict is similarly large. The same coefficients imply an estimated cumulative cost to the Afghan treasury of the rise in violence since 2005 of around 140 percent of GDP – approximately 3 billion US dollars in 2016.[2] This is equivalent to the total

1. For example: shocks to local economic output, or time-varying administrative capacity.

2. The focus on foregone revenues as measure of the cost of conflict omits changes in the composition of expenditure due to conflict. This is usually thought of as a key part of any "peace dividend"; funds used for security spending can be reallocated to other functions. In the case of Afghanistan, though, this is likely a near net zero effect, as elevated Afghan security spending is funded almost entirely funded by foreign grants, which will decline along with spending in the event of peace. For example, in 2016 Afghanistan spent 11.6 percent of GDP on security and received 8.6 percent of GDP from its major two security grants. So even if security spending in Afghanistan were to decline to the NATO target of 2 percent of GDP, this would leave only a 1 percent GDP peace dividend on the expenditure side after accounting for reduced security grants.

contributions since 2002 of the United States to the main multilateral development fund.

By manipulating the timing assumptions for the conditioning variables, I decompose this loss into two channels. I find that the main driver is not changing local economic activity, but rather a reduced revenue share of given local activity. This is consistent with past work, which often finds that local economic activity increases in response to conflict.

In order to emphasize the importance of a careful identification strategy, I also document and decompose bias in the OLS estimates. I show that OLS understates the causal impact of conflict violence on government revenues, and produces estimates around one quarter the size of those calculated using instrumental variables. I compare these results to those obtained from the GSC approach, and interpret the differences between them as a decomposition of the OLS bias. Because the GSC estimator is robust to a wide array of omitted variables, the difference between it and the OLS estimator corresponds to the contribution of omitted variables alone to the OLS bias. And the difference between GSC and the IV estimates can then be attributed to other sources of bias – most plausibly, I argue, measurement error. As the GSC estimates are approximately halfway between the OLS and IV estimates, I attribute about half of the OLS bias each to omitted variables and to measurement error.

Section 2 reviews related literature. Section 3 introduces the main data sources. Section 4 presents results from OLS estimation, Section 5 those from an instrumental variables approach, and Section 6 from the generalized synthetic control method. Section 7 provides counterfactual simulations, and Section 8 concludes.

2 Related Literature

Measures of the economic impact of conflict have a rich history. Early attempts used cross-country variation in conflict to measure a variety of outcome variables: Collier (1999) computes the impact of conflict on the level and composition of output; Gupta et al. (2004) calculate losses to growth and tax revenues; and Knight, Loayza, and Villanueva (1996) estimates a peace dividend from reduced military spending.[3] However, cross-country studies suffer from rather serious challenges to identification. Countries in conflict differ not only in their duration and intensity of the conflicts, but also in many ways which may affect both conflict and economic outcomes. Examples include economic structure, geography, and political systems. And so it is very hard to convincingly control for confounding factors, in conflicts as diverse as Colombia, Sri Lanka, and Sudan (to name just a few).[4]

More recent work on the cost of conflict have studied outcomes at partly or fully sub-national levels. Besley and Mueller (2012) and Mueller (2016) estimate the cost of conflict in Northern Ireland and Africa respectively, both using sub-national panels. And Brodeur (2018) uses plausibly random variation in the success of terror attacks in the US during 1970-2013 to identify the causal impact of terror attacks on local activity.[5]

In the Afghan setting, this identification challenge has mostly been addressed by using short panels of individual- or household-level data, with largely inconclusive results. Floreani, López-Acevedo, and Rama (2016) show that after accounting for the impact of increased local troops due to conflict, household consumption does not respond significantly when conflict rises. Blumenstock et al. (2018) use mobile phone data in 2013-16 to show that corporate phone activity declines following a major violent incident. And Ciarli, Kofol, and Menon (2015) demonstrate that conflict causes households engaging in more, yet less productive, economic activities. Other studies focus on the behavior of prices during conflict. D'Souza and Jolliffe (2012) study the impact of changing food prices on household-level consumption patterns. And Bove and Gavrilova (2014) estimate the impact of conflict on food prices in a sample of provinces in 2003-2009.

Ciarli, Kofol, and Menon (2015) is also particularly relevant in that it uses an instrument to go beyond identification via fixed effects in a panel. As I do, the authors use a Bartik-style

3. A more recent study in a similar vein is IMF (2017)

4. See Blattman and Miguel (2010) for a detailed overview of the limitations of this approach.

5. An alternative approach to identification is to specify a structural economic model. For example, World Bank (2017) measure the cost of the recent Syrian conflict by performing counterfactual simulations in an estimated multi-region DSGE model.

instrument for conflict, with the cross-sectional variation coming from violence during the Soviet occupation of 1979-1989. In spite of (or perhaps because of) their care in identifying the causal impact of conflict, the estimated responses are quite small, in contrast to the results reported here. This difference might arise because households are more adept than the government at substituting into other income-generating activitie, especially at short notice.. Or it might occur because Ciarli, Kofol, and Menon (2015) study only 2005 and 2008, a period with smaller and less geographically varied changes in conflict. The specific relationship that I exploit in my instrument – between ethnicity and conflict – is not new. Alesina et al. (2003), Cunningham and Weidmann (2010) and Berman, Shapiro, and Felter (2011) are just a few examples of many which make this link.

When measuring the economic impact of conflict in the Basque country, Abadie and Gardeazabal (2003) propose a synthetic control approach to identify the causal impact of conflict. This is designed to address bias due to omitted variables which drive both conflict and economic activity. Applying Powell's (2017) generalized version of this framework not only extends Abadie and Gardeazabal's approach in a technical sense, but also expands the field of application, in this case to a low-income country.[6]

The use of an instrument in disentangling the causal relationship between economic outcomes and conflict has been more common in the study of the reverse relationship: how economic performance affects the likelihood of conflict. In their seminal study, Miguel, Satyanath, and Sergenti (2004) use rainfall as an instrument to identify exogenous variation in economic activity – and hence its causal impact on conflict – in a sample of sub-Saharan African countries. Dube and Vargas (2013) use international terms of trade shocks to identify similar variation in economic activity in commodity-producing regions in Colombia.[7] These papers not only provide a methodological point of comparison to this paper, but their findings also motivate my concerns about identifica-

6. This is important because conflict in rich countries is rare. The Uppsala Conflict Database recorded conflict in 33 countries in 2016, or which 19 were low income countries, and only one an advanced economy.

7. These papers are only the most recent in a long literature studying this direction of causality, including Collier and Hoeffler (1998), Fearon and Laitin (2003), and Sambanis (2002)

tion. As negative economic shocks robustly predict conflict, omitted variables (and perhaps also reverse causality) is likely to be a threat to identification in this setting.

3 Data

I construct a panel of monthly provincial revenues and conflict fatalities in Afghanistan between March 2005 and December 2016. With 34 provinces, this produces 4,828 observations.

3.1 Data on conflict

My source of data on conflict is the Uppsala Conflict Data Program Georeferenced Event Dataset (UCDP GED). The UCDP GED is a commonly used dataset in conflict research across a wide range of disciplines[8], and includes observations worldwide dating during 1989-2016. The unit of observation is an incident of conflict violence with at least one fatality, such as a bomb in a public place or an attack on a police checkpoint. Each record contains detailed information on location, fatalities, actors, sources, and type of incident. In order to guarantee data quality, the dataset draws from a wide range of primary sources, including official documents, media sources, NGO reports, and archives.

The protagonists in the current conflict have been involved in Afghanistan since late 2001, when a US-led coalition invaded Afghanistan and deposed the Taliban government. In the period since, the UCDP GED records 19,955 separate conflict incidents in Afghanistan, resulting in 97,902 deaths.[9] Figure 1 shows the nationwide fatality rate from violence per 10,000 of national population, a measure of conflict intensity. Using nationwide conflict intensity as a guide, one can tell the history of the Afghan conflict since the fall of the Taliban in four approximate periods: 2002-2005, 2006-2009, 2010-2013, and 2014-2016. The divisions between these periods are indicated by the vertical lines in the Figures 1-3.

8. Examples include Michalopoulos and Papaioannou (2016) (economics), Hultman, Kathman, and Shannon (2013) (political science), and Themnér and Wallensteen (2014) (conflict research)

9. The UCDP GED provides three estimates of fatalities for each incident: an upper bound, a lower bound, and a "best guess" estimate. Throughout, I use the best guess estimate.

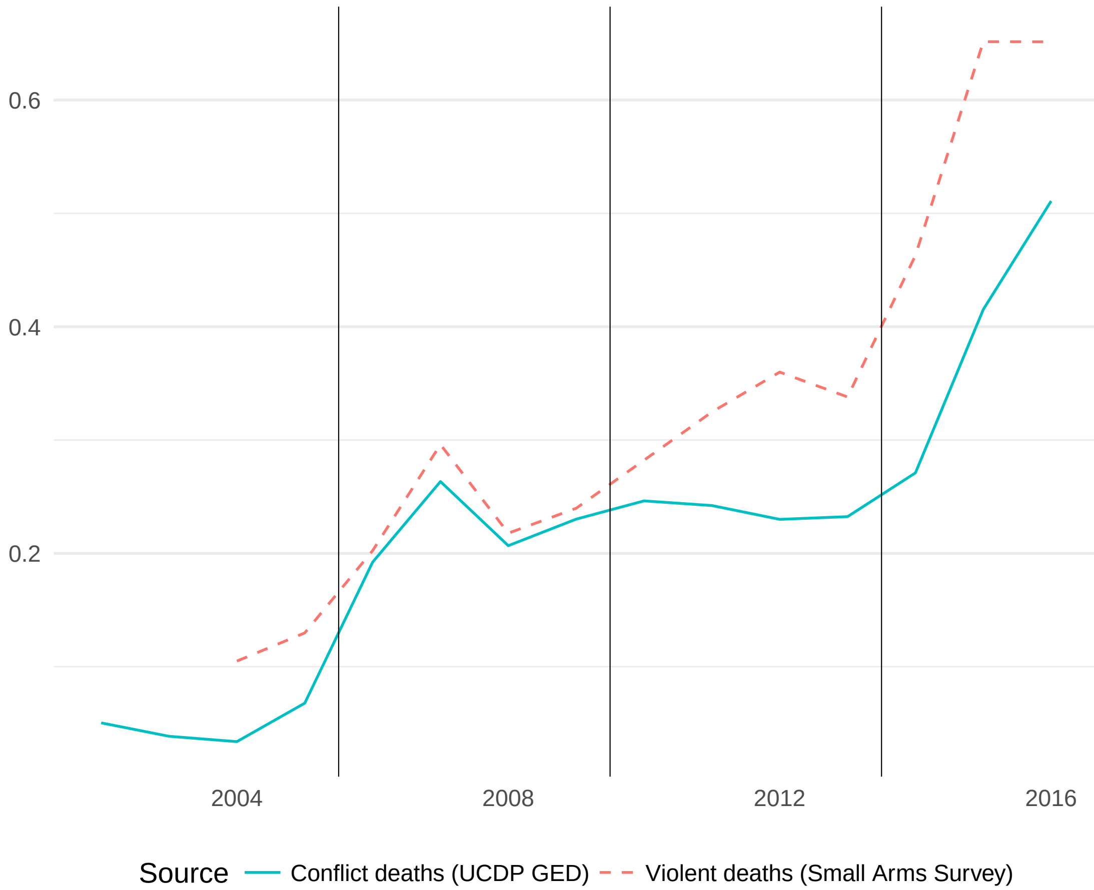


Figure 1: Nationwide deaths 10,000 in Afghanistan

Country	2002-2005	2006-2009	2010-2013	2014-2016
Afghanistan	0.05	0.22	0.23	0.40
Iraq	0.08	0.10	0.06	0.34
Sudan	0.13	0.05	0.05	0.05
Sri Lanka	0.00	0.29	0.00	
Somalia	0.05	0.15	0.17	0.11
Syria	0.00	0.00	1.42	2.96

Table 1: Deaths from conflict per thousand, international comparison

From 2002 to 2005, the country was relatively peaceful. Deaths from conflict were no higher than one per 10,000 nationwide. By this measure, the Afghan conflict was not particularly intense relative to other concurrent conflicts. Table 1 illustrates this point, presenting the UCDP GED conflict fatality rate for a sample of conflict-affected countries during this time.[10]

Conflict intensified markedly during 2006-2009. Increased violence was concentrated in the South (see Figure 2). This region – overwhelmingly Pahshto-speaking, and a longtime stronghold of the Taliban – accounted for over half of the 31,169 deaths recorded during 2006-2009.[11]

Between 2010 and 2013, foreign troop levels surged, as shown in Figure 3..[12] And while the intensity of the conflict in the South subsided during this time, increasing violence elsewhere meant that national conflict deaths fell little.

Since the withdrawal of international troops in 2014, violence has once again escalated. Compared to the the earlier peak in 2007, this most recent increase is much more widespread, with dramatic increases in fatalities outside the Taliban heartlands, including in the predominantly Dari-speaking North (see Figure 2). The UCDP GED data suggests that, during this period conflict deaths per capita were higher in Afghanistan than in Iraq, even though much of the latter was under the control of Islamic State during this time (see Table 1).[13]

Comparing Figure 1 and 2 to Figure 3 emphasizes a further advantage of using the UCDP GED data. Because the UCDP GED records *all* fatalities from conflict, it gives the broadest possible measure of conflict violence. In contrast, using only military records, such as ISAF

10. Of course, given the obvious difficulties of data collection, international comparisons of conflict intensity are only very approximate. UDCP GED data on Sudan in particular are very hard to match with other sources. In 2005, the UN estimated that 10,000 civilians per month had died in Darfur over the preceding 18 months (Vasagar and MacAskill (2005)). With a population of around 30 million, this alone would have produced an average fatality rate of 2 per 1,000 per year ($180,000 \div 30,000,000 \times 1,000 = 6$ over three years).

11. Principally bordering Pakistan, the Southern region of Afghanistan comprises Helmand, Kandahar, Nimroz, Uruzgan, and Zabul Provunces, and includes around 11 percent of the national population.

12. Data on from US troop levels is US Central Command, reported by the New York Times (Fairfield, Quealy, and Tse (2009), covering 2002-09), and by the Congressional Research Service (Peters, Schwartz, and Kapp (2017), covering 2007-16). Data on other international troop levels comes from ISAF's "Resolute Support Facts & Figures" series, which covers separately US and other coalition troops during 2007-16.

13. Although according to the UCDP GED data, since 2009 the conflict in Syria has been much more intense than in either Iraq or Afghanistan.

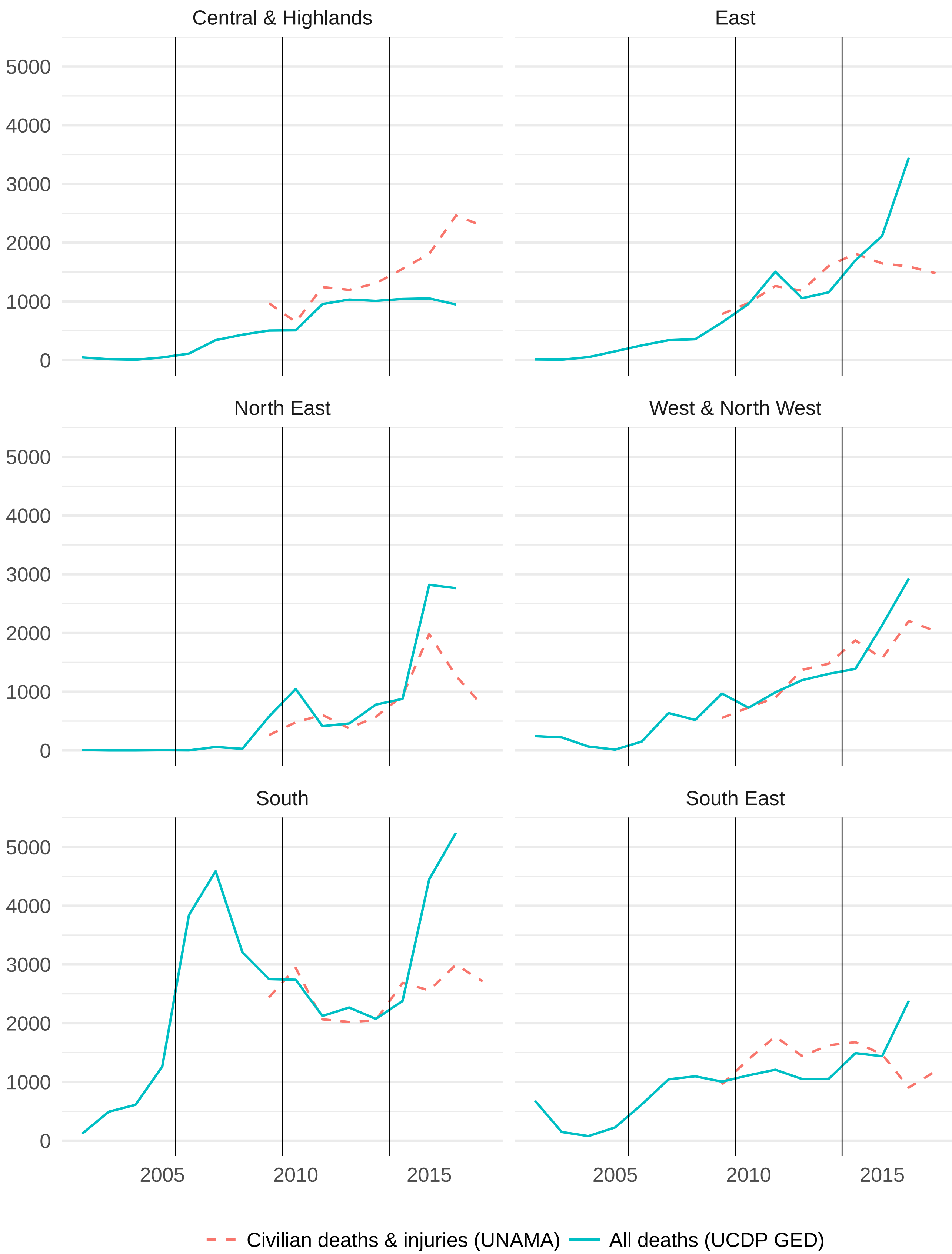


Figure 2: Deaths and injuries from conflict in Afghanistan 2002-16

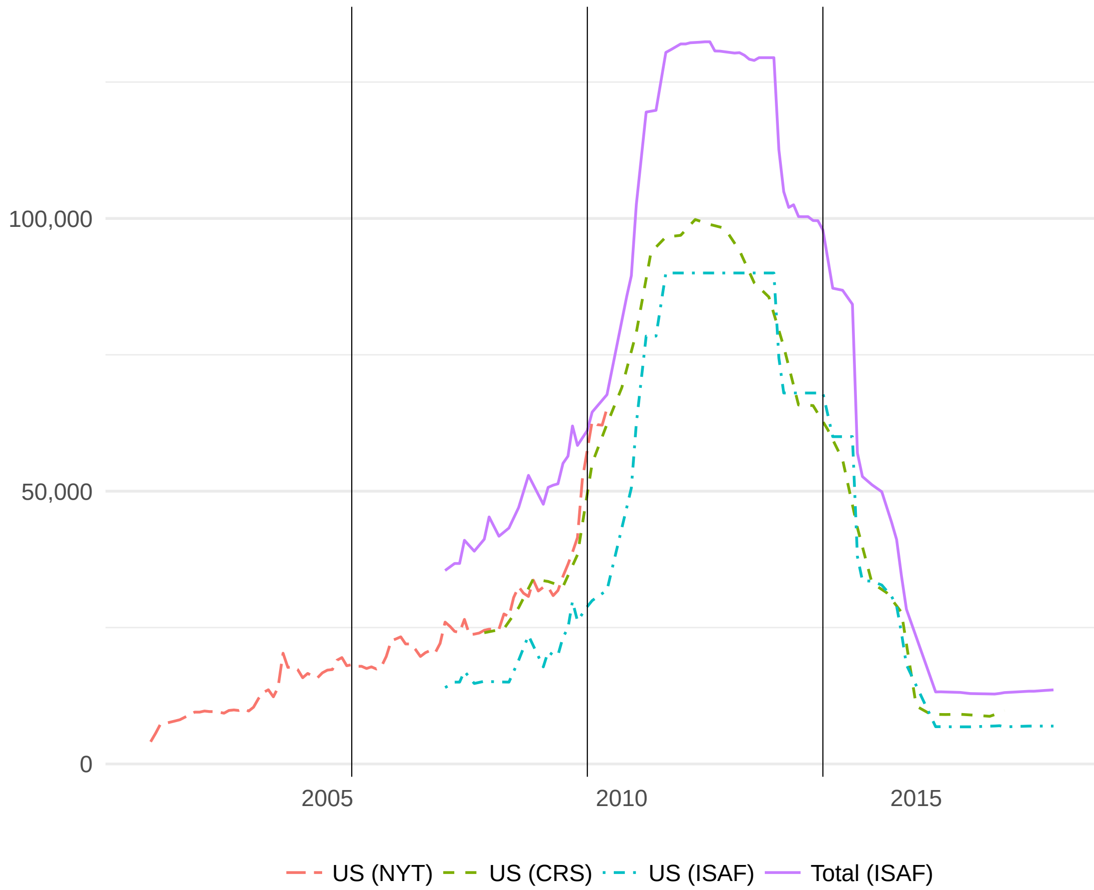


Figure 3: Foreign troop levels

coalition deaths, will capture a changing share of fatalities, and hence mis-represent the true extent of violence in Afghanistan.[14]

In Appendix A.2 I corroborate the UCDP-GED data against two independent sources: the Small Arms Survey Database on Violent Deaths, and data on civilian casualties from the United Nations Assistance Mission to Afghanistan.

14. Bove and Gavrilova (2014) rely only ISAF casualties as their measure of conflict when trying to estimate responses of local prices to conflict. This is likely appropriate for the time period that they study, 2003-09. But in subsequent years, changes in ISAF fatalities will reflect large fluctuations in aggregate troop levels as much as the variation in actual conflict intensity.

3.2 Data on revenues

I compile data on provincially-collected domestic revenues of the central government. This covers all domestic revenues collected by the central government that have a specific provincial origin. Examples of revenues covered by this measure include customs duties collected at border crossings and corporate income tax paid by firms registered at provincial tax offices. This measure excludes nationally collected domestic revenues[15] and external revenues, such as foreign grants.[16] Data are published in the Afghan Ministry of Finance Monthly Fiscal Bulletins, available publicly until 2017.

Domestically collected revenues capture the Afghan government's ability to provide self-sustaining funding for ongoing expenditures. As such, losses to domestic revenues represent an economic cost of conflict, measuring services that cannot be provided by the government. Grants, in contrast, are a transfer from overseas citizens. Figure 4, shows annual domestic revenues as a share of GDP, which grew over the sample period. Following the draw-down of international forces in 2014 revenues dropped, but measures introduced subsequently – including a 10 percent tax on mobile phone top-ups – have offset these losses.

I restrict inquiry to only provincially collected domestic revenues as they provide a source of spatial variation. The share of domestic revenues with specific provincial origin is large and stable, averaging 59 percent of total revenues during the sample period. And as shown in Figure 5, the provincially-collected and total revenues move broadly in line. Furthermore, the central government is the only revenue-collecting agency in the provinces. There are no local taxes which might respond to or affect with either central government revenues; provincial governments are funded solely by transfers from the center.

There is considerable heterogeneity in provincial revenues, as shown in Table 2 which reports

15. Nationally collected domestic revenues are those which have no definite provincial origin. This typically refers to revenues received by government entities with nationwide scope. For example: fees charged for issuing passports, which are collected by the (single, nationwide) Foreign Ministry.

16. Grants were a large source of funding for the government of Afghanistan during this period, averaging over 12 percent of GDP. Afghanistan has no meaningful net foreign debt issuance during this period.

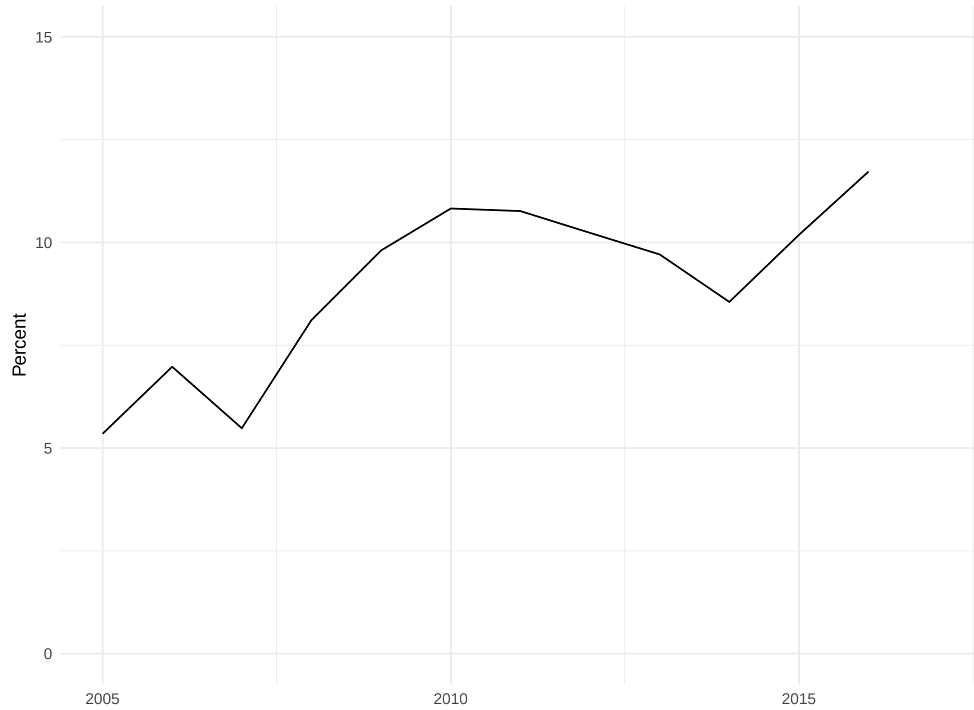


Figure 4: Central government domestic revenues as a share of GDP

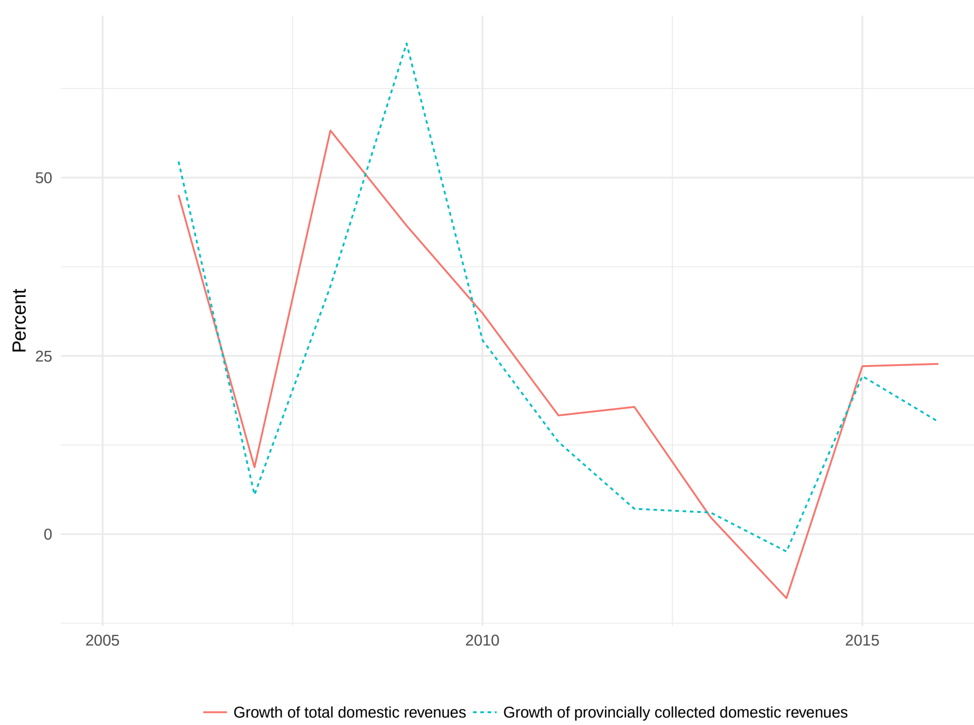


Figure 5: Nominal growth rates of total and provincially collected revenues

summary statistics for province-level revenue data, aggregated annually. Unsurprisingly, more populous provinces report higher revenues. Revenues are also highly concentrated; over 60 percent of revenues are collected in just three provinces: Balkh, Herat, and Nangahar. This reflects the dependence of the Afghan government revenues on taxes on corporations and trade; all three provinces are major regional trading centers, with strong cultural and transport links to Uzbekistan, Iran, and Pakistan, respectively.

There are several obvious outliers in the revenue data, the largest due to apparently misplaced decimal points. Full details of outlier identification are given in Appendix A.3.

3.3 Other data

Given that Afghanistan has not conducted a census since before the Soviet invasion of 1979, accurate provincial population data are hard to come by. Appendix A.1 discusses the difficulties of using subsequent published provincial population estimates. So for national populations, I use data from the World Bank, and for provincial shares I use the 2003-05 nationwide household listing, reported in UNFPA and CSO (2007). The 2003-05 household listing also includes provincial data on the share of speakers of each of the major languages. I use this in Section 5, where I construct a Bartik-style instrumental variable by interacting language share with nationwide conflict fatalities.

As controls in the dynamic panel estimation, I use eight other data series, from three sources: four provincial meteorological series (precipitation, average temperature, days of frost, and average clous cover) come from the Centre for Environmental Data Analysis (CEDA); opium prices for five regions are listed in the UN Office on Drugs and Crime (UNODC) Afghanistan Opium Survey; and regional wheat, sheep and daily unskilled labor costs come from the World Food Program's Vulnerability Analysis and Mapping project (WFP VAM). These controls are all available monthly, although coverage varies across each series. Table 3 presents summary statistics of the control variables. In robustness checks with annually aggregated data I also use data on

Province	Pop share	Prov rev share		Revenue growth	
	(2003-2005)	Mean	St. dev.	Mean	St. dev.
Badakhshan	3.6	0.4	0.1	26.3	33.3
Badghis	2.2	0.1	0.0	19.9	25.2
Baghlan	3.2	0.6	0.3	11.6	47.5
Balkh	4.8	16.5	3.8	21.2	44.5
Bamyan	1.5	0.2	0.0	31.1	36.3
Daykundi	2.0	0.1	0.0	47.2	63.5
Farah	2.1	2.6	2.3	40.2	56.5
Faryab	3.6	3.8	2.2	20.9	58.8
Ghazni	4.7	0.4	0.1	23.2	37.9
Ghor	2.8	0.2	0.0	22.0	26.0
Helmand	6.0	0.7	0.3	21.4	46.2
Herat	7.7	26.7	2.9	13.5	22.9
Jowzjan	1.8	0.3	0.1	24.0	31.2
Kabul	10.4	6.0	3.3	10.4	27.3
Kandahar	4.3	7.1	1.6	14.0	29.2
Kapisa	1.6	0.2	0.1	28.6	38.1
Khost	2.7	1.7	1.0	5.4	57.6
Kunar	1.8	0.2	0.1	26.0	78.2
Kunduz	3.4	1.5	0.6	20.9	39.3
Laghman	1.6	0.2	0.1	24.5	25.6
Logar	1.4	0.2	0.1	29.5	29.0
Wardak	2.3	0.2	0.1	31.7	28.5
Nangarhar	5.8	19.1	2.7	23.1	27.1
Nimruz	0.5	8.4	3.5	36.4	42.6
Nuristan	0.6	0.1	0.0	26.0	68.5
Paktia	2.2	1.1	0.6	27.1	41.2
Paktika	3.3	0.3	0.1	23.1	31.1
Panjshir	0.5	0.1	0.0	28.3	41.8
Parwan	2.1	0.3	0.1	16.4	30.2
Samangan	1.4	0.2	0.0	23.8	15.7
Sar-e Pol	1.9	0.1	0.0	30.6	25.5
Takhar	3.6	0.4	0.1	23.0	31.1
Urozgan	1.4	0.1	0.0	21.7	56.2
Zabul	1.5	0.1	0.0	25.3	39.2
Provinces		100.0	0.0	18.7	19.0
Total				20.3	16.2
GDP				13.1	9.0

Table 2: Provincial revenue summary statistics, anuual 2005-2016

nighttime lights visible from space. A full description of this, and all other data sources, can be found in Appendix A.1.

4 Ordinary least squares estimates

In this section I present baseline estimates of the impact of conflict on government revenues in Afghanistan using ordinary least squares.

4.1 Conflict intensity

I use the UCDP data to generate a province-period measure of conflict intensity, defined as:

$$s_{it} = \frac{1000 \times \text{Annualized UCDP conflict fatalities in province } i \text{ in period } t}{\text{Province } i \text{ popuation share 2003-05 } \times \text{ National population in period } t}$$

I use the start-of-sample population share for two reasons: the unreliability of published provincial population estimates (discussed in Appendix A.1) and because even accurately scaling by the current population would overstate increases in violence if people migrate away from areas affected by conflict.

Applying the panel stationarity tests of Levin, Lin, and Chu (2002) and Im, Pesaran, and Shin (2003) with province-specific intercepts, I find that both strongly reject (i.e. $p \simeq 0$) non-stationarity for the monthly and quarterly-aggregated data. While stationary, fatalities from conflict are serially correlated at the provincial level. A dynamic regression of revenues on s_{it} alone will be hard to interpret, as not all variation in s_{it} can be interpreted as a contemporaneous shock – some fraction will be the persistent impact of previous shocks. So I apply a filtering transformation to s_{it}, fitting a panel auto-regression to the conflict intensity data and interpreting the residuals, x_{it} as the shocks to conflict intensity. Appendix A.4 describes further the results of this this process.

Series	Spatial unit	Measurement	Units	Periods	No. obs	Start	End	Mean	Within sd	Across sd
Precipitation	Province	Milimeters, total	34	168	5712	2003	2016	38.3	34.7	22.9
Ave temperature	Province	Celcius, average	34	168	5712	2003	2016	12.2	8.6	5.4
Cloud cover	Province	Percent, average	34	168	5712	2003	2016	40.2	14.1	7.5
Frost	Province	Days, total	34	168	5712	2003	2016	9.7	9.5	4.7
Opium price	Region	Afghanis/kg, growth	5	144	720	2005	2016	0.2	11.1	39.3
Sheep price	Region	Afghanis/animal, growth	5	154	770	2004	2016	0.5	6.1	31.7
Unskilled labor	Region	Afghanis/day, growth	5	154	770	2004	2016	0.8	8.8	20.9
Wheat price	Region	Afghanis/kilo, growth	5	154	770	2004	2016	0.7	7.6	18.8

Table 3: Covariate summary statistics, monthly data. “Within sd” and “Across sd” represent the mean within-province and across-province standard deviations respectively for each series.

4.2 Dynamic equation estimates

To measure the impact of conflict intensity on provincial revenues, I estimate a full dynamic panel model for provincial revenues

$$y_{it} = \beta x_{it} + \sum_{m=1}^{M} \rho_m y_{i,t-m} + \sum_{l=0}^{L} \alpha_l' z_{i,t-l} + \mu_i + \delta_t + e_{it} \tag{1}$$

where y_{it} is the log of provincial revenues in local currency, and x_{it} is the shock to provincial conflict intensity. The z_{it}'s represent a vector of meteorological and price controls, as described in Table 3. The μ_i represent a full set of province-specific fixed effects, which will absorb permanent differences between provinces, and the δ_t are a full set of time fixed effects, which will absorb an national-level time series variation. The error term e_{it} absorbs all remaining time-province specific variation.

Estimation of equation (1) by OLS will produce an unbiased estimate of the casual effect of violence on local revenues if the usual sequential exogeneity condition holds. Here, this requires that: 1) the error terms e_{it} are serially uncorrelated; and 2) the error term e_{it} is orthogonal to $\{x_{it}, \ldots, x_{i0}, y_{i0}, \ldots, y_{i0}, z_{i,t}, \ldots, z_{i0}\}$ for all t.

The inclusion of lagged revenue represents a simple partial control for reverse causality. If deteriorating economic conditions cause subsequent violence, then omitting lags of y_{it} will induce correlation between e_{it} and x_{it}, causing bias in the OLS estimates. Specifically, regressing revenues on violence without accounting for past revenue performance will produce estimates which measure not only the causal impact of violence on revenues but also the selection of low-revenue province-year pairs into violent episodes. In the language of a difference-in-differences framework, this would violate the 'parallel trends' assumption. Including lags of the dependent controls for this effect.

Inclusion of the price and weather controls aims to satisfy sequential exogeneity by reducing the likelihood of omitted variables. If other local conditions affect both provincial revenues and security simultaneously, then the error term e_{it} will again be correlated with provincial conflict fatalities s_{it}. The meteorological series are controls for weather-related phenomena which might affect both local economic activity[17] and the probability of conflict.[18] Prices also absorb variation due to local demand shocks which may affect both economic activity and conflict.

The parameter β measures the contemporaneous impact of an increase in conflict intensity, and is a key target of inquiry. But it is not the only object of interest. The parameter γ, defined as

$$\gamma = \frac{\beta}{1 - \sum_m^M \rho_m} \tag{2}$$

is also important. This measures the cumulative revenue loss due to a shock to conflict.

Columns (1)-(7) of table 4 presents the results of estimating equation 1 on quarterly data via OLS using progressively richer specifications. In column (1) estimates β with only month and province fixed effects. This already reveals a negative and weakly significant association between conflict and revenues. However, applying the Wooldridge (2010) residual autocorrelation test in a panel rejects the null of zero serial correlation, even at significance levels below 1 percent.[19].

Autocorrelated errors are a sign of model mis-specification and, when lags of the dependent variable are included as regressors, a violation of strict exogeneity. To address this, specifications (2) and (3) include six and 24 lags of revenues respectively. These both fail to reject stationarity, although the estimated impact of the conflict intensity shock is much smaller. Six lags of revenues are sufficient to fail to reject uncorrelated residuals. However, with monthly data, at least 12 lags are required to guarantee that the results are robust to any residual seasonality remaining after

17. According to the 2003 National Risk and Vulnerability Assessment survey, agriculture was the principal sector of employment for 64 percent of male heads of household. The same survey finds that lack of irrigation was the most-cited constraint reported by Afghan farmers, suggesting that production is particularly sensitive to weather.

18. Afghanistan is well known for having a "fighting season", as the difficulty of living outdoors and the need to tend crops reduce manpower both for insurgents and potentially government forces. During 2005-16 nearly two thirds (64 percent) of conflict fatalities occurred in the six months from May to October. Local variation in weather is therefore likely to affect not only local revenues but also the length and timing of this fighting season (and thus measured conflict deaths).

19. This test regresses first-difference residuals Δe_{it} on their own first lags $\Delta e_{i,t-1}$. Under the null hypothesis that the residuals are not serially correlated, their first differences should have an AR(1) coefficient of -0.5. The Wooldridge test is then just a Wald test of this null. As the test is pooled across units, the test statistic follows a chi-squared statistic with one degree of freedom. The standard deviation used in the Wald test is itself robust to autocorrelation.

Table 4: Effect of conflict on provincial revenues

	Dependent variable: Monthly provincial (log) revenues						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Conflict intensity shock	−0.025	−0.016	−0.015	−0.014	−0.017	−0.017	−0.018
	(0.016)	(0.010)	(0.015)	(0.010)	(0.014)	(0.012)	(0.014)
Revenue lag 1		0.071**	0.038	0.059**	0.032	0.055**	0.033
		(0.029)	(0.032)	(0.026)	(0.027)	(0.026)	(0.026)
Revenue lag 2		0.050**	0.036	0.038	0.033	0.060**	0.037
		(0.024)	(0.030)	(0.023)	(0.029)	(0.028)	(0.030)
Revenue lag 3		0.123***	0.078***	0.118***	0.080***	0.114***	0.079***
		(0.017)	(0.023)	(0.018)	(0.024)	(0.018)	(0.024)
Revenue lag 4		0.122***	0.081***	0.112***	0.081***	0.108***	0.079***
		(0.019)	(0.019)	(0.021)	(0.020)	(0.021)	(0.019)
Revenue lag 5		0.113***	0.086***	0.114***	0.082***	0.106***	0.079***
		(0.020)	(0.021)	(0.021)	(0.020)	(0.023)	(0.020)
Revenue lag 6		0.072***	0.055**	0.094***	0.062***	0.086***	0.057**
		(0.019)	(0.025)	(0.023)	(0.024)	(0.025)	(0.024)
Revenue lags	0	6	24	6	24	6	24
Last significant lag	0	6	24	6	21	6	21
Revenue persistence	0.00	0.55	0.72	0.54	0.72	0.53	0.71
	(0.000)	(0.072)	(0.048)	(0.069)	(0.039)	(0.071)	(0.041)
Meteorological control lags	0	0	0	24	24	24	24
Meteorological ave, lags	0	0	0	36	36	36	36
Price control lags	0	0	0	0	0	24	24
Price ave, lags	0	0	0	0	0	36	36
Opium price lags	0	0	0	0	0	24	24
Cumulative impact	-0.025*	-0.035*	-0.056	-0.031*	-0.061	-0.037*	-0.061*
	(0.016)	(0.021)	(0.053)	(0.021)	(0.049)	(0.024)	(0.048)
Autocorrelation test stat	14.7	0.0	0.1	0.0	0.1	0.0	0.1
p-value	0.00	0.98	0.79	0.92	0.76	0.92	0.77
Residual standard deviation	0.762	0.706	0.685	0.696	0.673	0.677	0.666
Observations	4,896	4,692	4,080	4,454	4,080	4,062	4,062
F Statistic	1.530	67.580***	21.162***	5.327***	5.347***	2.983***	3.323***

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by province.
All specifications include a full set of time and province fixed effects.
AR tests from Wooldridge (2010).
Significance of cumulative impact based on a one-sided test

including the time fixed effects. Accordingly, and due to evidence of persistence at long lags (see the "last significant lag" line of Table 4), I include 24 lags of revenue in the most sophisticated specifications.

Using lagged dependent variables as a control for reverse causality is imperfect. If there is contemporaneous feedback between revenues and conflict within a period then it will fail. Given lags in reporting, this is unlikely to be a major problem with monthly data. However, regressing on lagged dependent variables does itself violate the strict exogeneity condition necessary for consistency in another way. Because lagged regressors are dependent variables in previous periods, this induces a correlation between the period t regressors and the period $t - m$ residuals (for $0 < m \leq M$). Nickell (1981) shows that the resulting bias is asymptotically of order $1/T$. With at least 120 observations for each province for even the longest auto-regressive specification, the resulting bias will be small. But when I corroborate the headline results using annual data (in Appendix C.4) this is a concern. So in that setting I also use a GMM estimator in the style of Arellano and Bond (1991) in order to address this issue. This has little effect on the main results.

Specifications (4) to (7) add further controls to address omitted variables bias. In specification (4) I add 24 lags of each of the four meteorological variables, as well as the rolling average of each over the preceding 36 months. This is a means of capturing both the short- and medium-run effect of changes in the weather without excessively reducing the degrees of freedom. It allows current revenues to be affected by the exact seasonal variation of the weather in the preceding two years, but by the overall weather prior to that.

In specification (6) and (7) I also include a similar hybrid lag structure for regional prices of wheat, sheep and labor – including 24 lags of each, and a rolling average prior to that. Because the opium price data starts in 2005, I include 24 lags of the regional opium price, without any averaging. To include a lag length longer than the autoregressive component of revenues would reduce the number of observations in the sample.

Specification (7) also exhibits the largest and most statistically significant impact of conflict intensity, with a long-run impact of a decline in revenues in the order of 6 percent in response to a unit increase in conflict intensity. Section 7 performs a series of experiments to measure the counterfactual decline in revenues from increased violence in Afghanistan. But as a rule of thumb, one can approximate the per period revenue gain from a permanent reduction in average violence to 2004 levels by multiplying the cumulative impact by one quarter.[20] This gives a total estimated revenue loss of 0.02 log points, implying that period revenues are around 2 percent lower than they would be if violence were to permanently return to 2004 levels.[21]

This is a tiny loss for such a large conflict, and out of proportion with other estimates.[22] So as a cross-check, I also compute the long-run impact directly using a local projection approach in the style of Jordà (2005).

4.3 Local projections

In the specification in equation (1), estimation with lagged dependent variables is used to make inference about the very long run impact of a shock. However, if the model is not correctly specified, the long-run estimates can be very badly wrong as the mis-specification errors compound over time (see Ramey (2016) for further discussion of this issue). In particular, if the impact of a shock is delayed, or nonlinear, then a simple dynamic equation setting, such as that proposed in equation (1), is not well-suited to identifying the long-run impact of the shock.

An alternative approach is to use local projections, first proposed by Jordà (2005). This computes impulse responses not by propagating forward the dynamic estimation, but instead by estimating the k-period-ahead response to a shock directly, thus avoiding compounding mis-

20. This comes from taking the aggregate increase in per capita fatalities (roughly 0.5 per 1,000, see Figure 1), and dividing by the inverse of the cumulative impact of the conflict intensity shock (roughly 2, see Figure 15)

21. As $0.07 \div 4 \simeq 0.02$. Note that annualization in the the definition of s_{it} means that we do not have to convert to quarterly fatalities – we can just apply the increase in the annual rate.

22. Mueller and Tobias (2016) estimate that a four-year civil war substantially less severe than that in Afghanistan entails a per capita GDP loss of around 18 percent. While the relationship between government revenues and output may not be stable in the event of conflict, it it hard to see how such small estimates of the impact of conflict can be consistent with this.

specification errors. In this setting, this involves estimating

$$y_{i,t+k-1} = b_k x_{it} + \sum_{m=1}^{M} \rho_m y_{i,t-m} + \sum_{l=0}^{L} \alpha_l' z_{i,t-l} + \mu_i + \delta_t + e_{i,t+k} \tag{3}$$

for $k = 1, \ldots, K$, where K is some maximum impulse horizon. In this case, the impulse response at horizon k is b_k. The cost of a local projection approach, however, is decreased precision at long forecast horizons; as $k \to T$, the number of observations declines to one per province.

Figure 6 shows the resulting estimates for b_k.[23] While these are on average negative, they are also very variable, reflecting the high degree of noise in the underlying data. So in Figure 7, I plot the cumulative impact of a on local revenues of a unit increase in conflict intensity, re-estimating equation (3) with cumulative revenue as the dependent variable. The effect appears to be stable after around six quarters, and has a p-value of less than 0.05 for all horizons of seven months or longer. The fact that the response is so delayed does make some intuitive sense – delays in reporting or in local investment may mean that an uptick in local violence may not feed through into reported revenues for some time.

The magnitude of this effect is much more plausible than that estimated by the dynamic panel model of equation 1, which I also plot in Figure 7. The average cumulative effect in the period 16-30 months after the shock is -0.26 log points. Dividing by four gives an approximate per period gain from reduced violence of about 6 percent of revenues.

The local projection approach also shines some light on why the dynamic panel specification of equation (1) fails to detect any sizable effect. The true impact of conflict on local revenues appears to be delayed – the cumulative impact is not statistically significant until half a year after the shock. While including lags of past revenues should amplify initial responses to account for some of this effect, if the contemporaneous response is so close to zero then there is little to amplify. As a result, this dynamic panel approach fails to infer the long-run response of provincial revenues to conflict. In Section 4.5, I show how equation 1 can be augmented with lagged conflict shocks to recover the same estimated impact.

23. The control set is the same as that in the most general case Table 4 – specification (7).

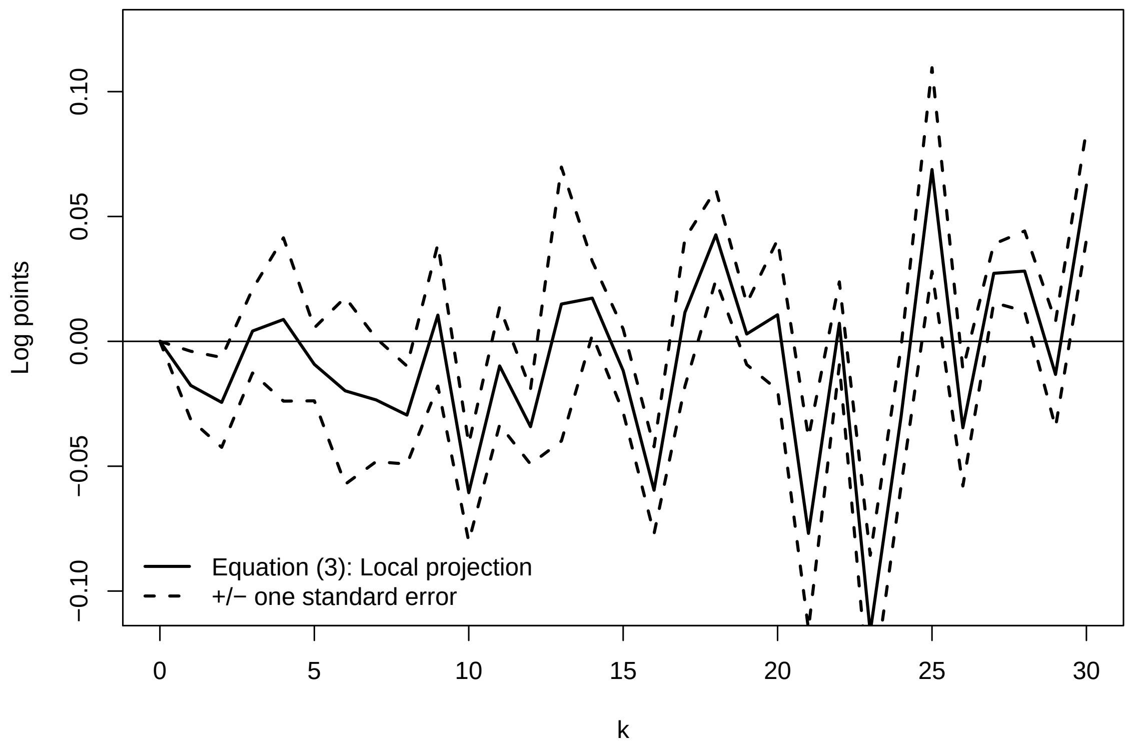


Figure 6: Period revenue loss: dynamic panel and local projection. Monthly aggregation.

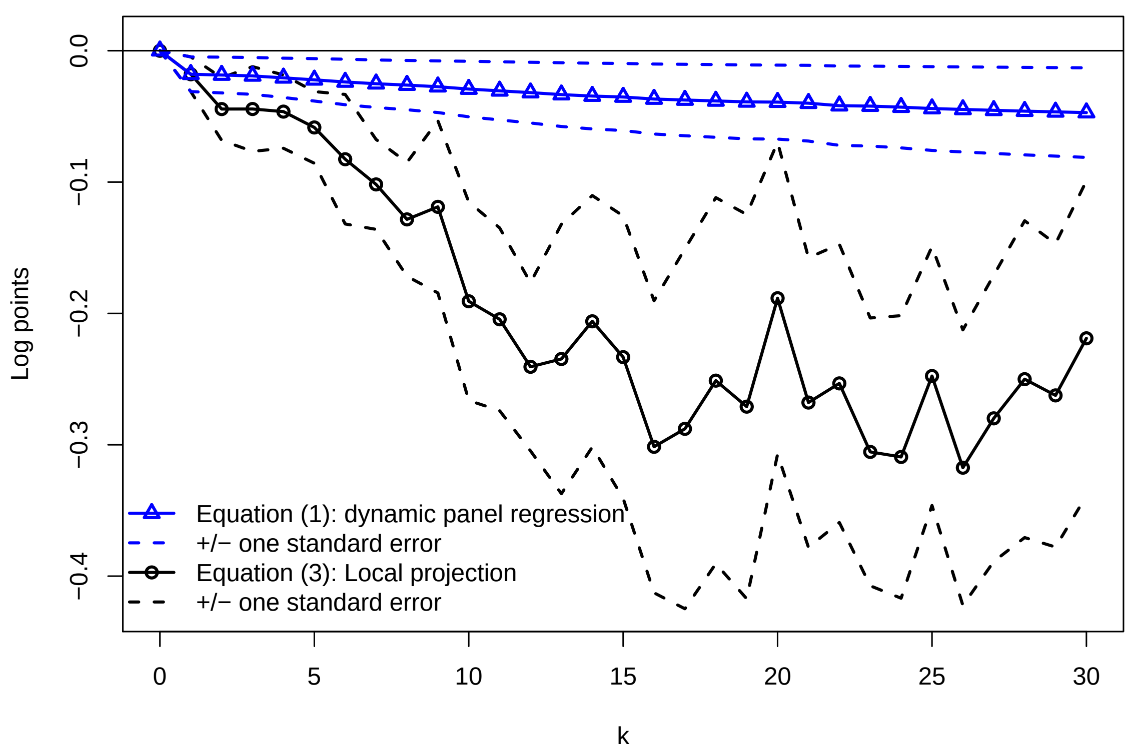


Figure 7: Cumulative revenue loss: dynamic panel and local projection. Monthly aggregation.

4.4 Decomposing the sources of revenue losses

In the linear projection setting, the revenue loss due to conflict can be decomposed into two channels. The revenue base effect is the decline in revenues due to changing local economic activity. And the revenue efficiency effect is the decline in the share of given local activity that authorities can collect as revenues.

To decompose losses into these two channels, I estimate for $k = 1, \ldots, K$:

$$y_{i,t+k-1} = b_k x_{it} + \sum_{m=1}^{M} \rho_m y_{i,t-m} + \sum_{l=-k}^{L} \alpha_l' z_{i,t-l} + \mu_i + \delta_t + e_{i,t+k} \tag{4}$$

This differs from equation (3) due to the inclusion of the control variables $z_{i,t-l}$ for periods subsequent to the shock up until the the outcome in period $t+k$. These variables – meteorological observations and regional prices – are proxies for local economic activity. The impulse response estimated in question (4) is therefore conditioned not only on the state of the local economy at time t, but also on its subsequent evolution. As a result, the resulting estimates exclude the impact of changes in the revenue base, producing estimates of b_k which capture the revenue efficiency effect alone.

Figure 8 presents the results of estimating equation (4) using the same lag structure as in Figure 7. The difference between the local projection with and without the future controls is essentially zero. This suggests that the entirety of the revenue loss comes from an efficiency effect, and that the local activity effect is unimportant. This is not inconsistent with other work that suggests there are several channels through which conflict may positively impact local activity. Examples include: the purchase of goods and services by fighters moving into the area (Floreani, López-Acevedo, and Rama (2016)), government spending on development projects to counter insurgent support (Berman, Shapiro, and Felter (2011)), and weakened state control relaxing constraints on private sector activity (Guidolin and La Ferrara (2007)). So one interpretation of Figure 8 is that such channels offset any negative impact of conflict on local activity, resulting in a zero revenue base effect.

There is another interpretation of Figure 8. It might simply be the case that the future controls simply do not capture local fluctuations in activity very well. In appendix C.4 I test this possibility, estimating equation (4) using annual satellite data on the intensity of visible electric light at night as an additional control. As satellite data is only available monthly from 2012, adding the extra control comes at the cost of reducing the frequency of observation. Nevertheless, the resulting estimates cannot reject the hypothesis that the revenue base effect is zero, in agreement with the monthly estimates.

4.5 Dynamic equation with lagged shocks

In order to recover a similar impact in the usual dynamic panel setting, I augment equation (1) with lags of the conflict intensity shock in order to capture the delayed impact. Specifically, I estimate:

$$y_{it} = \sum_{k=0}^{K} \beta_k x_{i,t-k} + \sum_{m=1}^{M} \rho_m y_{i,t-m} + \sum_{l=0}^{L} \alpha_l' z_{i,t-l} + \mu_i + \delta_t + e_{it} \tag{5}$$

Figure 7 suggests that much of the impact of conflict on revenues comes during the first six months after a shock. So I set $k = 6$, reporting the results in Table 5. This shows that there is indeed a large and highly statistically significant delayed impact of conflict on revenues. The long-run cumulative impact of a transitory shock to conflict is given by the analogue of equation (2):

$$\gamma = \frac{\sum_{k=0}^{K} \beta_k}{1 - \sum_{m}^{M} \rho_m}$$

Point estimates for the cumulative effect are larger even than those produced by the local projection approach, and highly significant. Figure 9 also includes the estimated cumulative response, in red, showing broad agreement between this method and the local projection approach.

Appendix C contains further robustness checks, including estimates using province-specific time trends, allowing for spillovers from neighboring provinces, omitting potentially influentially observations, and detailed results from annual data with more controls. And Appendix B uses the Southern insurgency of 2007-08 as a case study, estimating the impact of violence only in the

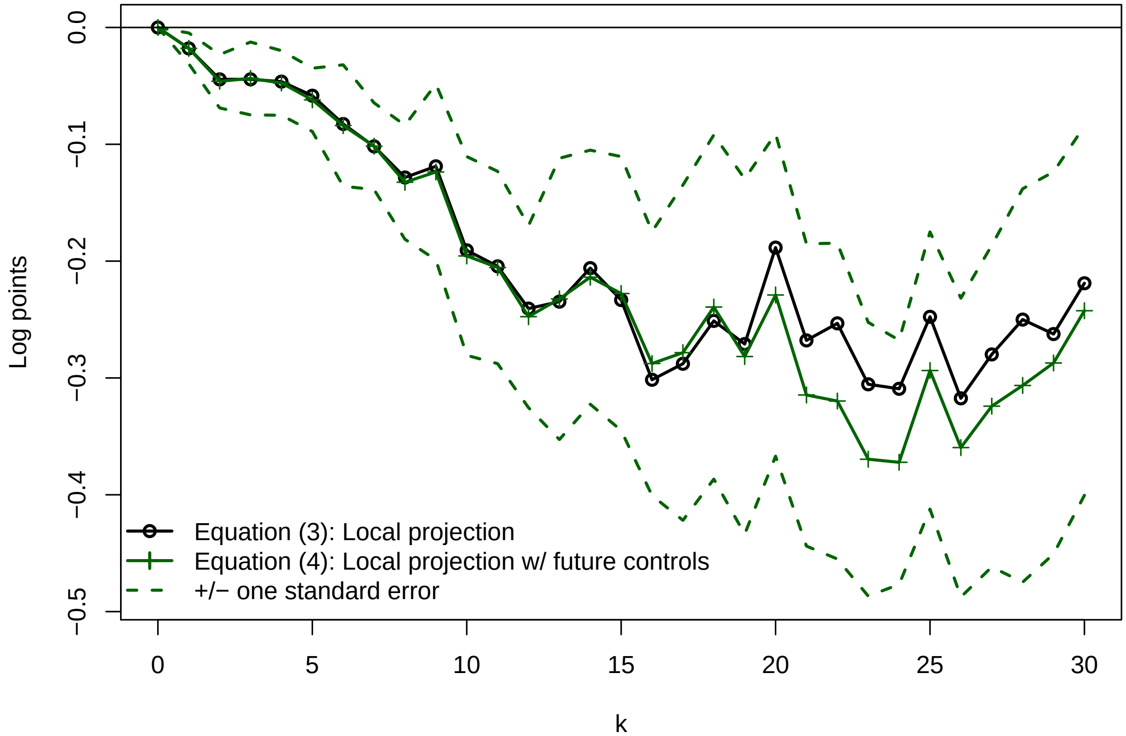


Figure 8: Cumulative revenue loss: local projection with future controls. Monthly aggregation.

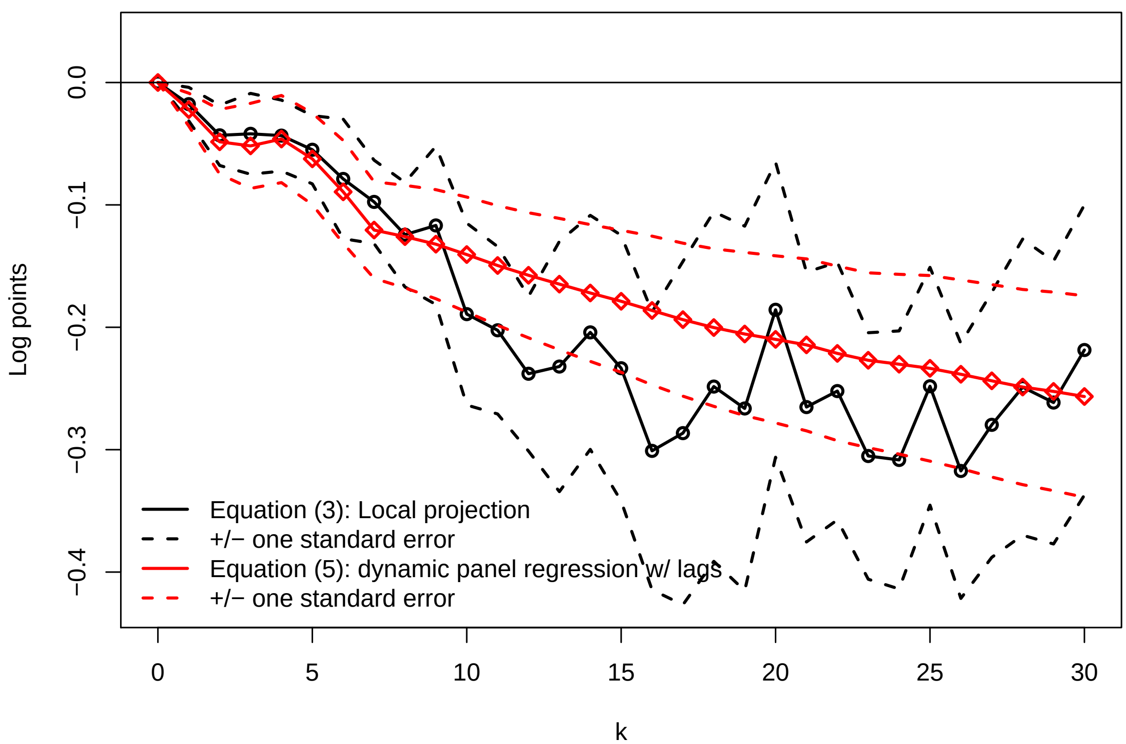


Figure 9: Cumulative revenue loss: dynamic panel and local projection. Monthly aggregation.

cross-section. The corresponding results all further corroborate the main results in this section.

Table 5: Effect of conflict on provincial revenues

	Dependent variable: Monthly provincial (log) revenues						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Conflict shock, lag 0	−0.029*	−0.017	−0.019	−0.017	−0.022*	−0.020*	−0.022*
	(0.018)	(0.011)	(0.014)	(0.010)	(0.013)	(0.011)	(0.013)
Conflict shock, lag 1	−0.036*	−0.016	−0.024	−0.022*	−0.027	−0.016	−0.026
	(0.019)	(0.014)	(0.017)	(0.013)	(0.018)	(0.015)	(0.019)
Conflict shock, lag 2	−0.012	0.007	−0.001	0.009	−0.003	0.008	−0.002
	(0.016)	(0.016)	(0.014)	(0.015)	(0.016)	(0.017)	(0.017)
Conflict shock, lag 3	0.008	0.020	0.010	0.027	0.011	0.013	0.008
	(0.028)	(0.032)	(0.030)	(0.031)	(0.029)	(0.031)	(0.028)
Conflict shock, lag 4	−0.013	−0.001	−0.010	−0.005	−0.014	−0.011	−0.012
	(0.019)	(0.015)	(0.016)	(0.015)	(0.016)	(0.015)	(0.015)
Conflict shock, lag 5	−0.026	−0.015	−0.020	−0.022	−0.024	−0.016	−0.023
	(0.033)	(0.035)	(0.033)	(0.038)	(0.033)	(0.037)	(0.035)
Conflict shock, lag 6	−0.025	−0.019	−0.022	−0.022	−0.025	−0.023	−0.027
	(0.033)	(0.031)	(0.030)	(0.026)	(0.027)	(0.026)	(0.025)
Revenue lags	0	6	24	6	24	6	24
Last significant lag	0	0	21	1	21	0	21
Revenue persistence	0.00	0.55	0.72	0.54	0.71	0.53	0.71
	(0.000)	(0.072)	(0.049)	(0.069)	(0.040)	(0.072)	(0.043)
Meteorological control lags	0	0	0	24	24	24	24
Meteorological ave, lags	0	0	0	36	36	36	36
Price control lags	0	0	0	0	0	24	24
Price ave, lags	0	0	0	0	0	36	36
Opium price lags	0	0	0	0	0	24	24
Cumulative impact	-0.133	-0.093	-0.305**	-0.108	-0.365***	-0.140*	-0.354***
	(0.166)	(0.110)	(0.148)	(0.093)	(0.125)	(0.099)	(0.111)
Autocorrelation test stat	14.3	0.0	0.1	0.0	0.1	0.0	0.1
p-value	0.00	0.99	0.80	0.94	0.77	0.93	0.78
Residual standard deviation	0.762	0.706	0.685	0.696	0.673	0.676	0.666
Observations	4,896	4,692	4,080	4,454	4,080	4,062	4,062
F Statistic	1.092	36.620***	17.176***	5.093***	5.137***	2.911***	3.254***

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered at the province.
All specifications include a full set of time and province fixed effects.
AR tests from Wooldridge (2010).
Significance of cumulative impact is one-sided.

5 Instrumental variables

Some important further challenges to the causal interpretation of OLS remain. In this section I attempt to address these challenges with an instrumental variables strategy.

The most obvious challenge is reverse causality, that changes in local revenues might feed back to affect the local security situation. This might be direct, for example, if local government revenues are tied to current or subsequent local spending on security. The fact that revenue and spending policies are determined centrally does weaken this possibility, but does not eliminate it entirely. Or it might be indirect, if insurgent commanders see the efficiency of local government services such as the ability to collect revenues as a proxy for the strength of the local governance.

There is also considerable evidence that poor economic performance can causally impact the likelihood of conflict[24]. Given that local economic activity is likely a key driver of local revenues, then this gives rise to the second challenge to the causal interpretation of OLS: that local economic activity may jointly determine violence and revenues, violating the exogeneity assumption and confounding inference. Of course, the price and weather controls included in the OLS specification so far will partly address this problem. But these controls are imperfect, and as local economic performance is unobserved, some correlated variation is inevitably left over.

Mis-measurement of violence poses a third challenge to the causal interpretation of OLS. Such mis-measurement will cause attenuation bias, which pushes the OLS coefficients towards zero. This might arise from two sources. First, literal measurement error. Accurate recording of conflict fatalities is inherently very difficult in an unsafe environment, particularly when various actors have incentives to distort reality. The cliché that "truth is the first casualty of war" seems particularly relevant here. Second, the true causal impact of conflict on relationship may stem not from incidents of conflict *per se*, but from a a deteriorating security environment more generally, of which specific incidents are merely a noisy signal. For example, foiled insurgent attacks may dissuade business investment in a province, but will not show up in the conflict fatalities indicator.

24. Such as Miguel, Satyanath, and Sergenti (2004) and Dube and Vargas (2013)

These challenges can all be addressed by use of an instrumental variable. In particular, I use a Bartik-style instrument, interacting pre-sample linguistic share with nationwide average conflict fatalities, in a two stage least squares setting.

5.1 The instrument

A valid instrument must satisfy two requirements: relevance and exogeneity. Relevance requires that the instrument is a statistically significant determinant of the outcome variable – here provincial conflict fatalities – after controlling for the other independent variables. Exogeneity requires that the the instrument not be correlated with the shocks to the dependent variable, here provincial revenues.

A Bartik, or shift-share instrument is a type of instrumental variable constructed by multiplying cross-sectional shares with time-series shifts. Named after Bartik (1991), and popularized by Eichengreen et al. (1992), the early applications multiplied state-specific industry shares by national industry employment growth to create an instrument for local labor demand.

In a recent paper, Goldsmith-Pinkham, Sorkin, and Swift (2018) show that with a full set of time and unit fixed effects, a Bartik-style instrument can be recast as a GMM estimator where the instruments are the shares interacted with time fixed effects, and a weighting matrix determined by the time-varying shifter. As a result, when there is just one shift and one share (as here), the exogeneity and relevance criteria split in line with the instrument's two components. Relevance is determined by the cross-sectional share and the time-varying shifter together, but the exogeneity is determined by the share alone.[25]

In the application at hand, the role of the cross-sectional share is played by the pre-sample fraction of Pashto-speakers within a province, and that of the time series shift by average nationwide conflict fatalities per 1,000 people, shown in Figure 1. So the instrument for period t in

25. With many shifts and many shares then this split is not so clean. Correlation between the cross-sectional shares and subsequent variation in the respective shifts can indicate unobserved confounders. See Goldsmith-Pinkham, Sorkin, and Swift (2018) for further details.

province i is:

$$Instrument_{it} = Pre\text{-}sample\ Pashto\text{-}speaking\ share\ _i \times National\ conflict\ fatalities\ per\ 1,000\ _t$$

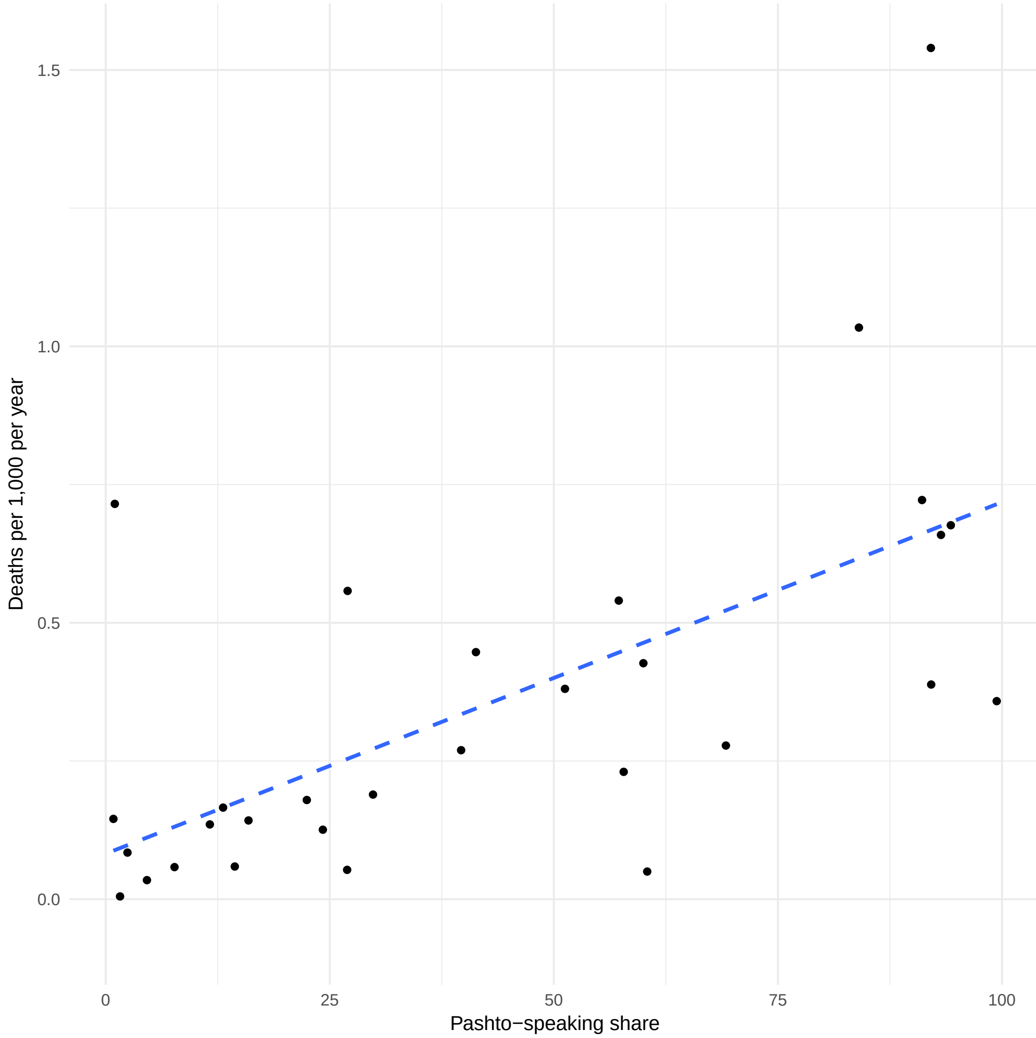
Pashto is the language of the Pashtuns, the largest ethnic group in Afghanistan, and UNFPA and CSO (2007) estimate that it is the first language of around half the population. Pashtuns live principally in the southern part of Afghanistan, as well as in northern Pakistan. While the links between Pashtun nationalism and the Taliban are complex, the Taliban undoubtedly was and continues to be an overwhelmingly Pashtun-dominated organization. For example, Johnson and Mason (2007) list the eleven most senior Taliban figures during the Taliban government, and identifies them all as Pashtuns. And Esposito (2004) cites Pashtun tribal traditions as a key influence on Taliban governance. In contrast, in the north of Afghnistan, where the population predominantly speaks Dari (a dialect of Persian), Taliban influence has always been much weaker. Even when the Taliban operated a government out of Kabul during 1996-01, the their control did not extend to the North, where an alliance of principally non-Pashtun forces held sway.[26]

Given the overwhelmingly Pashtun composition of the Taliban, and given the Taliban's key role in the post-2005 insurgency, it is therefore not entirely surprising that the spatial distribution of conflict violence during 2005-16 mirrors Afghanistan's ethno-linguistic composition. Figure 10 presents the cross-sectional correlation of provincial pre-sample Pashto-speaking share with conflict intensity during 2005-016. The correlation is very strong.[27] It is also a statistically significant predictor; the F-statistic in the simple bivariate regression in Figure 10a is 24.

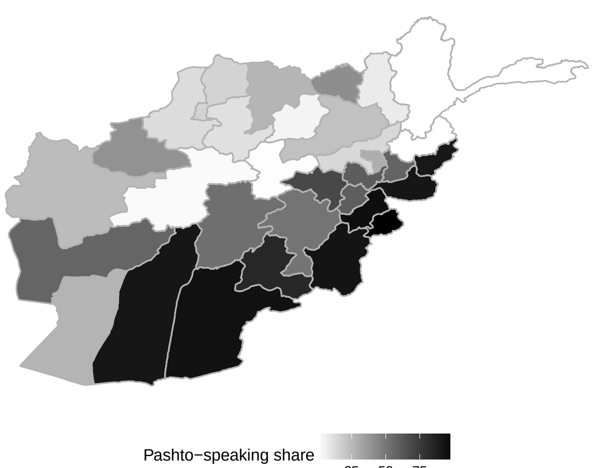
This spatial correlation suggests that the cross-sectional variation in ethnic composition might

26. Indeed, Symon (2001) reports that the Northern Alliance even retained Afghanistan's seat at the UN, as well as several dozen embassies during the Taliban rule.

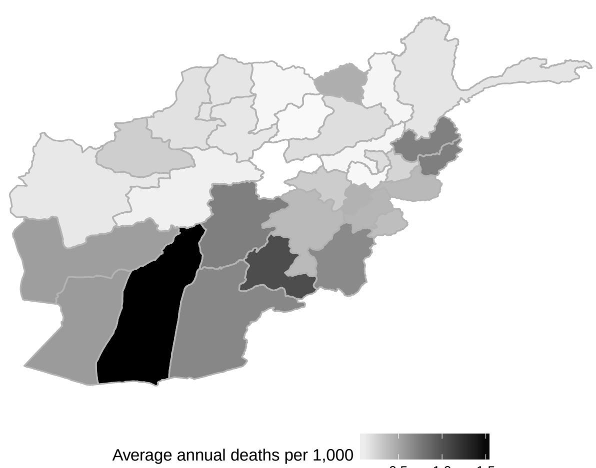
27. Berman, Shapiro, and Felter (2011) find a similar relationship in Iraq, where local voting patterns – considered a proxy for Sunni-Shia shares – predict local violence. In cross-country settings, though, other authors have found that measures of heterogeneity, rather than the prevelnace of just one ethnic group, are important predictors of poor outcomes. Cunningham and Weidmann (2010) find that conflict is highest in sub-national units where the degree of dominance of the largest local ethno-linguistic group – defined as the share in excess of the next largest group – is near 0.5. And Alesina et al. (2003) find that the degree of fractionalization – also a measure of heterogeneity – of both language and ethnicity predicts lower subsequent growth. Appendix C.5 shows how in the Afghan case, these are all far inferior predictors of cross-sectional conflict violence than the simple Pashtun share, presumably due to the degree of Pashtun influence in the major insurgent group.



(a) Pashto-speaking share vs. conflict fatalities



(b) Pashto-speaking share 2003-05



(c) Conflict fatalities 2005-16

Figure 10: Spatial and cross-sectional variation of: Pashto-speaking share (2003-05) and mean annual conflict fatalities per thousand (2005-16) by province.

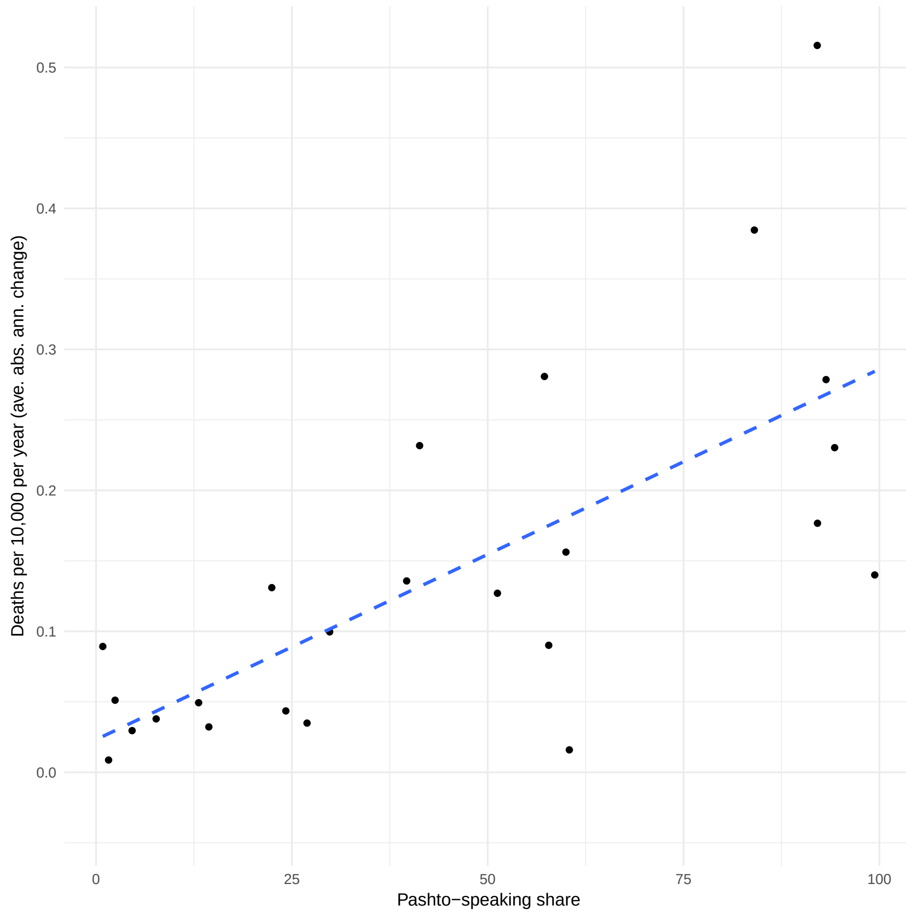


Figure 11: Pashto-speaking share 2003-05 and mean annual absolute change in provincial conflict fatalities 2005-16

be relevant. If *changes* in local conflict intensity correlate with provincial language share, then this cross-sectional variation contributes to instrument relevance in specifications with province fixed effects. Chart 11 shows that this is indeed the case, plotting provincial language share against a measure of provincial conflict volatility – the average absolute annual change in conflict fatalities. Furthermore, the distribution of ethno-linguistic groups across Afghanistan is exogenous to subsequent spatial variation in revenues: the ethno-linguistic data is collected pre-sample. The ethnic distribution of Afghanistan is also stable over time, allaying concerns that 2003-05 data are merely a one-off snapshot. To this end, Figure 19 in Appendix C.5 presents data from Atlas Narodov Mira (1964), a Soviet ethnographic atlas,[28] which shows that the spatial concentration

28. This atlas is the primary source for ethnographic information in many studies of the role of ethnicity in

of Pashtuns has changed little in the intervening half-century.

The preceding evidence suggests that the instrument is likely to be valid ex ante. Exogeneity holds because the cross-sectional share is pre-determined. And while relevance must be tested for empirically (which I do in the next section), there is at least a strong *prima facie* reason to believe that it will be satisfied: the share is correlated with variation in violence (Figure 11), and the shift – national levels of conflict violence – must be correlated with local fluctuations in violence.

5.2 The estimates

Table 6 presents the first stage regressions of a two stage least squares approach. As expected, the coefficient on the instrument is positive; increases in nationwide conflict intensity are correlated with larger relative increases in violence in provinces with more Pashto speakers. This coefficient can be interpreted as a differential response to nationwide conflict. For example, specifications (6) and (7) illustrate that (after accounting for weather, lagged revenues, and local prices) an nationwide increase in conflict intensity rate of 1 death per 1,000 is associated with an increase a province with only Pashto speakers which is 2 deaths per 1,000 larger than in one with no Pashto speakers at all.

A critical issue for the reliability of the two stage least squares estimates is whether the instrument is sufficiently strong. When an instrument is an insufficiently significant predictor of the endogenous variables, instrumental variables estimates are biased towards OLS, and the instrument is considered "weak'. The proportionate bias is of order $1/F$, where F is the F-statistic for the test that the instruments are all zero in the first stage regression. And so a sufficiently high F statistic is typically considered evidence that the instrument is strong, and the bias minimal. A commonly-applied rule of thumb is that $F > 10$ is sufficient to avoid bias due to weak instrument.

The F-test statistics reported in Table 6 are typically a little lower than 10, so there could be some concern that the instrument is weak. However, when the model is just-identified (i.e.

conflict and development, including Alesina et al. (2003) and Cunningham and Weidmann (2010) the number of instruments equals the number of endogenous variables, as here), the $F > 10$ rule is overly restrictive. In Stock, Yogo, and Wright (2002) – the original basis for the rule – the 5 percent critical value for a relative bias of 15 percent is 8.96. This means that in specifications (6) and (7) we can reject at the 5 percent level the possibility that the two stage least squares estimates will be biased by more than 15 percent. More recently, Angrist and Pischke (2008) and Angrist and Pischke (2009) provide simulation evidence that just-identified IV is unbiased in all but the very worst cases. In the latter, the authors also show that 95 percent confidence intervals have coverage ratios of at least 85 percent, even when the F-statistic gets very close to zero. They conclude that in the just-identified case, instrument weakness will show up in large standard errors rather than bias of point estimate.[29]

Table 7 presents the two stage least squares instrumental variables estimates of the impact of conflict on government revenues. Given that six lags of conflict produced similar estimates to the local projection approach, I focus on this specification here. The long-run impacts are large and consistently negative – much larger than under OLS – and statistically significant at the 5 percent threshold or higher in the most sophisticated specifications (7) and (8). Notably, the point estimates are much larger than OLS even when only one lag of the conflict shock is included (specifications (5) and (6)). In contrast, under OLS the one-lag specification produced essentially no discernible impact whatsoever. The large effects are all the more striking given concerns that the instrument might be weak. Instrument weakness induces bias *towards* the OLS estimates, meaning that the estimates in Table 7 would be, if anything, under-estimates of the true causal impact of conflict on government revenues.

What might explain the large differences between the OLS and IV estimates? Earlier in this section, I identified three factors which might undermine causal inference in the OLS setting: reverse causality, omitted variables, and measurement error. However, the most plausible form of reverse causality is likely to result in OLS estimates which overstate the causal impact of

29. Note that k-estimators, which are otherwise robust to weak instrument bias, are applicable only to over-identified models. This includes the often-used Limited Information Maximum Likelihood (LIML) estimates.

Table 6: Two stage least squares estimates: First stage

	Dependent variable: Monthly provincial conflict intensity shock						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Instrument	1.315***	1.456***	1.570***	1.631***	1.725***	2.014***	2.047***
	(0.412)	(0.489)	(0.539)	(0.603)	(0.612)	(0.672)	(0.658)
Revenue lag 1		−0.022	−0.027	−0.021	−0.028	−0.027	−0.027
		(0.019)	(0.019)	(0.020)	(0.019)	(0.018)	(0.017)
Revenue lag 2		−0.020	−0.026	−0.025	−0.029	−0.026	−0.028
		(0.020)	(0.025)	(0.020)	(0.024)	(0.022)	(0.024)
Revenue lag 3		−0.024	−0.035	−0.027	−0.035	−0.029	−0.034
		(0.023)	(0.027)	(0.023)	(0.026)	(0.025)	(0.027)
Revenue lag 4		0.011	0.007	0.008	0.005	0.009	0.008
		(0.019)	(0.018)	(0.019)	(0.018)	(0.020)	(0.020)
Revenue lag 5		0.044*	0.038	0.043	0.037	0.039	0.036
		(0.026)	(0.025)	(0.027)	(0.026)	(0.027)	(0.025)
Revenue lag 6		0.003	−0.009	−0.002	−0.011	−0.002	−0.009
		(0.012)	(0.014)	(0.014)	(0.015)	(0.016)	(0.015)
Revenue lags	0	6	24	6	24	6	24
Meteorological control lags	0	0	0	24	24	24	24
Meteorological ave, lags	0	0	0	36	36	36	36
Price control lags	0	0	0	0	0	24	24
Price ave, lags	0	0	0	0	0	36	36
Opium price lags	0	0	0	0	0	24	24
Instrument F stat	10.20	8.85	8.50	7.33	7.96	8.98	9.68
p-value	0.00	0.00	0.00	0.01	0.00	0.00	0.00
Residual standard deviation	0.501	0.549	0.558	0.549	0.55	0.548	0.546
Observations	5,950	4,692	4,080	4,454	4,080	4,062	4,062

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by province. All specifications include a full set of time and province fixed effects.

conflict on local revenue collect. Declining revenues would, if anything, mean fewer resources to combat violence, increasing conflict and resulting in a more negative OLS estimate. In the case of omitted variables, one can think of factors which might cause bias in either direction. For example, if poor economic outcomes simultaneously cause more violence and lower revenues then the OLS estimates will be more negative than the causal effect. But if the government responds to local violence by increasing local expenditure – a strategy that Berman, Shapiro, and Felter (2011) find empirical and theoretical support for in Iraq – then OLS estimates will capture part of this offsetting policy response, understating the causal impact of violence alone. In contrast, the impact of measurement error is unambiguous. It biases coefficients to zero, and is thus known as "attenuation bias". This might arise if local revenues respond to local security conditions, of which realized local violence is simply a noisy signal. In this case, nationwide violence – which averages across many such signals – will better capture the true change in local security conditions. As such, using national violence as the time series variation in a Bartik-style instrument will extract the parts of local variation in violence which are meaningful determinants of local revenues, ignoring those that are merely noise.[30]

So the most reasonable interpretation of the divergence between the OLS and IV estimates is that the OLS estimates exhibit attenuation bias due to some combination of omitted variables and measurement error. In Sections 6 and 7, I use an alternative estimation strategy to separate out the two sources of bias.

6 Generalized synthetic control estimation

In this section I present estimates of the impact of conflict on government revenues from a generalized synthetic control estimation approach. I show that these results are statistically significant, and roughly midway between the OLS and IV estimates.

30. Ciarli, Kofol, and Menon (2015) also find that an instrumental variables approach produces larger responses of household consumption to conflict – something that they attribute to attenuation bias.

Table 7: Effect of conflict on provincial revenues: Instrumental variables estimates

	Dependent variable: Monthly provincial (log) revenues							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conflict shock, lag 0	−0.390	−0.333	−0.259	−0.259	−0.238	−0.191	−0.286	−0.309
	(0.394)	(0.299)	(0.197)	(0.221)	(0.223)	(0.175)	(0.226)	(0.238)
Conflict shock, lag 1	0.064	0.115	0.111	0.175			0.211	0.272
	(0.172)	(0.160)	(0.198)	(0.274)			(0.238)	(0.275)
Conflict shock, lag 2	0.199	0.135	0.315*	0.204			0.036	0.042
	(0.197)	(0.157)	(0.186)	(0.199)			(0.255)	(0.276)
Conflict shock, lag 3	−0.271	−0.348	−0.387	−0.303			−0.352	−0.312
	(0.278)	(0.321)	(0.307)	(0.333)			(0.395)	(0.356)
Conflict shock, lag 4	−0.251	−0.188	−0.210	−0.117			−0.072	−0.004
	(0.232)	(0.199)	(0.227)	(0.261)			(0.247)	(0.285)
Conflict shock, lag 5	−0.164	−0.059	0.046	0.105			−0.026	−0.015
	(0.222)	(0.172)	(0.186)	(0.179)			(0.184)	(0.180)
Conflict shock, lag 6	−0.198	−0.062	0.091	−0.097			−0.156	−0.256
	(0.450)	(0.309)	(0.199)	(0.181)			(0.211)	(0.170)
Revenue lags	0	6	24	24	6	24	6	24
Last significant lag	0	6	21	21	6	21	6	24
Revenue persistence	0.00	0.54	0.72	0.72	0.52	0.71	0.51	0.71
	(0.000)	(0.084)	(0.054)	(0.045)	(0.081)	(0.045)	(0.002)	(0.049)
Meteorological control lags	0	0	0	24	24	24	24	24
Meteorological ave, lags	0	0	0	36	36	36	36	36
Price control lags	0	0	0	0	24	24	24	24
Price ave, lags	0	0	0	0	36	36	36	36
Opium price lags	0	0	0	0	24	24	24	24
Cumulative impact	-1.010	-1.599	-1.046	-1.024	-0.494	-0.652	-1.317**	-2.012**
	(1.946)	(1.601)	(1.482)	(1.297)	(0.419)	(0.587)	(0.782)	(1.198)
Instrument F statistic	10.20	8.85	8.50	7.96	8.98	9.68	8.98	9.68
Residual standard deviation	0.825	0.766	0.768	0.733	0.688	0.673	0.735	0.738
Observations	4,896	4,692	4,080	4,080	4,062	4,062	4,062	4,062

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered at the province.
All specifications include a full set of time and province fixed effects.
AR tests from Wooldridge (2010).
Significance of cumulative impact is one-sided.

6.1 Generalized synthetic controls: a brief introduction

The generalized synthetic control estimator, proposed by Powell (2017), extends the synthetic control approach of Abadie and Gardeazabal (2003) to a setting with a continuous treatment. The key econometric challenge that both seek to overcome is a failure of the exogeneity assumption: that there may be correlation between a dependent variable the assignment of a treatment. In regression settings, one can seek to address these issues by controls, including possibly lagged dependent variables and (in a panel) time and/or unit fixed effects. Indeed, it is exactly this concern that motivates the regression specifications of Sections 4 and 5. However, this approach is fairly restrictive, relying on the hope that the controls "soak up" all correlated shocks, and limiting the possibility of arbitrary spatial correlation.

These challenges are particularly pertinent in the current setting, as controls are somewhat sparse. Price controls are only available at the regional level, and while the meteorological controls are quite rich, there is much non-climatic variation which they may fail to pick up. In particular, there may be good reason to think that there is spatial correlation of time fixed effects in Afghanistan, which the aggregate time fixed effects will not control for. Provinces vary considerably in ways that that are likely to produce common but complex variation in revenues: some provinces are economically and culturally close integrated with Pakistan, others with Iran, or central Asia; in some provinces wheat is the principal crop, and others maize; some have airports, or customs houses, or dams, and others do not; and some are snowy and mountainous, whereas others depend on upstream precipitation.

The insight of the synthetic control approach, and which provides estimates robust to these concerns, is that one need not account for all possible forms or sources of spatially correlated shocks. Instead, what matters for inference is the overall impact of the other covariates on the dependent variable. The synthetic control estimator applies this insight by constructing a "synthetic" comparator for each unit i by weighting the other units according to some matching criterion. If the match is good then the synthetic and real units should exhibit the same fluctuations due to the omitted variables. Subtracting the real from synthetic outcomes removes the impact of the omitted variables from both, leaving only the causal effect of the difference in their treatments. In the usual binary treatment setting, matching only happens in pre-treatment periods. But in in the general case, matching happens in all units at all times, but accounts for estimated impact of the treatment variable D_{it}.

Formally, in a synthetic control framework, the data is assumed to be generated by:

$$Y_{it} = \beta' D_{it} + \mu_i' \lambda_t + e_{it}$$

Where Y_{it} the outcome variable, D_{it} is the treatment, λ_{it} a vector of time-varying factors, and μ_i is a vector of unit-specific factor loadings. In the current setting Y_{it} is log provincial revenue relative to 2005 and D_{it} is the provincial conflict intensity shock.[31] As in Powell (2017), D_{it} is continuous, in contrast to Abadie and Gardeazabal (2003) where it is binary.

For each period and province, a synthetic comparison can be constructed as a weighted average of the other provinces with constant weights w_{ij} (for $j \neq i$). Given such weights, and given a candidate value of β, we can construct "untreated" outcomes for both the real and synthetic provinces in each period t. These are given by $Y_{it} - \beta D_{it}$ and $\sum_{j \neq i} w_{ij}(Y_{jt} - \beta' D_j)$. The estimator then minimizes the (square) difference between these untreated outcomes, analogous to the "pre-treatment" matching in a standard synthetic control model.

Thus, the generalized synthetic control estimator is a vector of coefficients $\hat{\beta}$, and a $N \times N$ matrix of coefficients $\hat{W} = \{\hat{w}_{ij}\}$ solving:

$$(\hat{\beta}, \hat{W}) = \arg\min_{\beta, W} \left\{ \frac{1}{2NT} \sum_{i=1}^{N} \sum_{t=1}^{T} \left[Y_{it} - \beta' D_{it} - \sum_{j \neq i} \left(\hat{w}_{ij} \left(Y_{jt} - \beta' D_{jt} \right) \right) \right]^2 \right\} \tag{6}$$

6.2 Estimates

Table 8 present several estimates using the synthetic control method. The estimation technique guards against omitted variables bias, so the controls in equation (1) are omitted. However, the

31. So in the notation of equation (1), $Y_{it} = y_{it} - y_{i0}$, and $D_{it} = x_{it}$

motive for including the lagged dependent variable – as a partial control for reverse causality – remains. So by default I include six lags of revenue.[32] Column (1) presents a one step estimate, solving exactly the problem given in equation (6). Column (2) presents a two step estimator, where differences between the true and synthetic untreated provinces are weighted by the inverse of the province-specific errors from the one step estimate. And column (3) shows the "individual-weighted" estimate, which similarly weights the real-synthetic differences, but allows for province-specific fit in the first step. These weighting schema, suggested by Powell (2017), down-weight provinces where the model fit is poor, preventing them from dominating the estimation.

Columns (2) and (3) produce estimated long-run impacts of around -0.3. These are larger than the equivalent OLS results in Table 4, but similar to the local projection estimates (see Figure 7).

The long-run effects are also statistically significant at the 5 percent level, at least for the individual-weighted two step estimator. The p-values for the long-run impact are computed using the asymptotic test suggested by Powell (2017). This is valid for fixed N as $T \to \infty$, and is robust to arbitrary correlation across provinces.[33] This provides an extra cross-check on the results of sections 4 and 5, where standard errors are clustered at the province – permitting only within-province correlation of errors.

Columns (4)-(7) extend this estimation approach further, including lags of the conflict shock in the vector of treatments D_{it}. Column (4) presents estimates without the dynamic lags of revenues. And columns (5)-(7) include revenue lags, using increasingly robust weighting schemes. The long-run estimates are substantially larger than the equivalent impacts reported in Section 4, and statistically significant at the 5 percent level, suggesting the estimates presented there are

32. With 24 lags, the optimization fails to converge. Furthermore, the estimated autocorrelation of revenues in the GSC setting is very high, suggesting that additional lags would add little.

33. Mechanically, the test is implemented by re-solving the model subject to the constraint that the long-run impact is zero (the null hypothesis). Under the null, the gradient of the objective function at this solution is zero. Bootstrapping the score across provinces generates a distribution of test statistics, against which the sample statistic can be assessed, producing a p-value. Because hypothesis testing requires resolving the model, standard errors on individual coefficients are not reported: this would require resolving the model for each parameter.

a lower bound on the magnitude of the impact of conflict. The effect of the weighting in both columns (1)-(3) and (5)-(7) suggest a further factor which might impede estimation in the OLS and IV settings: that a small number of points where the model fits poorly might have outsize influence on the results. In Appendix C, I try to account for this by excluding provinces which might be, ex ante, thought of as particularly influential. The weighting approach used in Table 8 does this more scientifically.

As a way to inspect the mechanism of this estimator a little, Figure 12 shows the weights from the individual-weighted two step estimator in column (7) of Table 8 for the six provinces with the highest average revenue on average: Balkh, Kandahar, Kabul, Herat, Nangarhar, and Nimruz (see Table 2 for a full breakdown of provincial revenues). Two key characteristics standout. First, that all of the major six provinces have considerable weight on each other in their synthetic matches. The synthetic matches for these six provinces put, on average, more than 40 percent of their weight on the other five major provinces. Second, that weight which is not assigned to other major six provinces tends to be placed on neighboring provinces: see in particular Balkh, Kabul, and Herat.

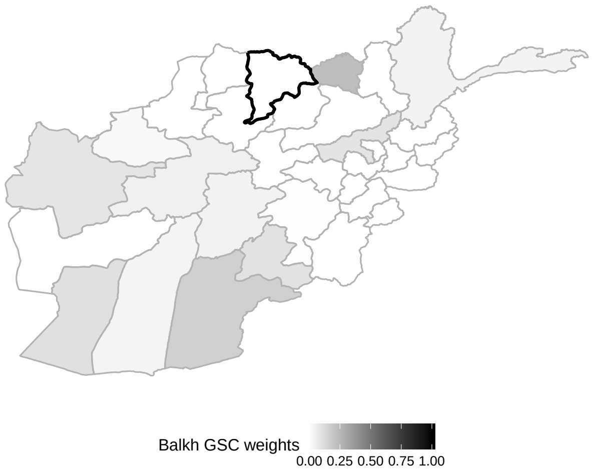
These patterns are intuitively reassuring – we should not be surprised that the natural comparator for a major province is some combination of neighboring and other major provinces. The advantage of the synthetic control method is that we can be agnostic about the exact combination of neighbors and similar provinces when forming a control, instead letting the data speak to the appropriate mix.

7 Measuring the cost of conflict

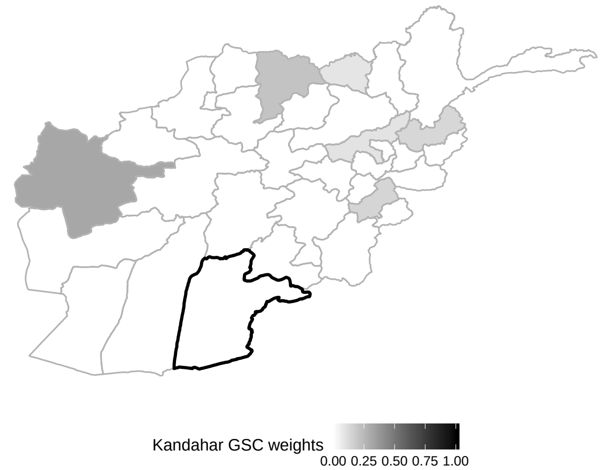
Using the estimates computes in the preceding sections, I compute two counterfactual measures of the cost of conflict in Afghanistan. The evidence presented in Appendix C.2 suggests that there are no spillover effects of violence in one province on the revenue collected in its neighbors. So adding up local impacts will give a valid measure of the national revenue loss from conflict.

	One step	Two step	Two step	Two step	One step	Two step	Two step
		Agg.	Indiv.	Indiv.		Agg.	Indiv.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Conflict intensity shock, lag 0	-0.01	-0.02	-0.02	0.02	-0.01	-0.02	-0.01
Conflict intensity shock, lag 1				-0.00	-0.02	-0.00	-0.00
Conflict intensity shock, lag 2				0.00	0.03	0.01	0.02
Conflict intensity shock, lag 3				-0.03	0.03	-0.03	-0.02
Conflict intensity shock, lag 4				0.01	0.01	0.01	0.01
Conflict intensity shock, lag 5				0.03	-0.01	-0.00	-0.01
Conflict intensity shock, lag 6				-0.03	-0.03	-0.01	-0.03
Revenue lags	6	6	6	0	6	6	6
Revenue persistence	0.91	0.95	0.95	0.00	0.91	0.95	0.95
Long-run impact	-0.08	-0.32	-0.34	0.01	-0.10	-0.80	-0.79
p-value	0.18	0.12	0.03	0.01	0.16	0.01	0.00

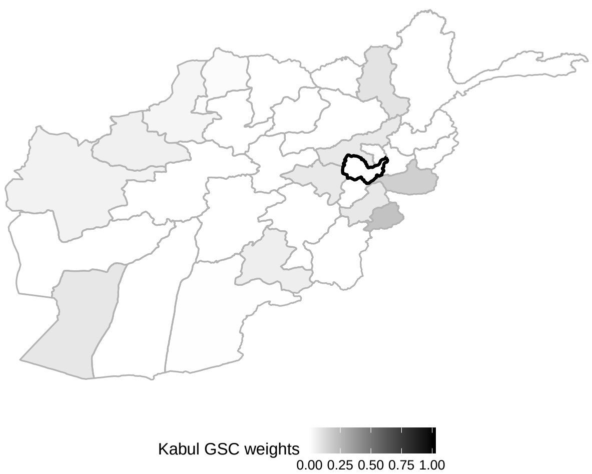
Table 8: Generalized synthetic control estimates. One-sided test of zero long-run impact from computed via 1,000-point bootstrap using Powell (2017).



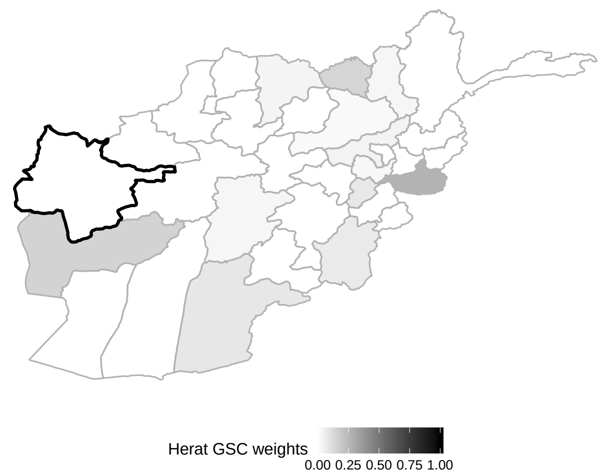
(a) Balkh



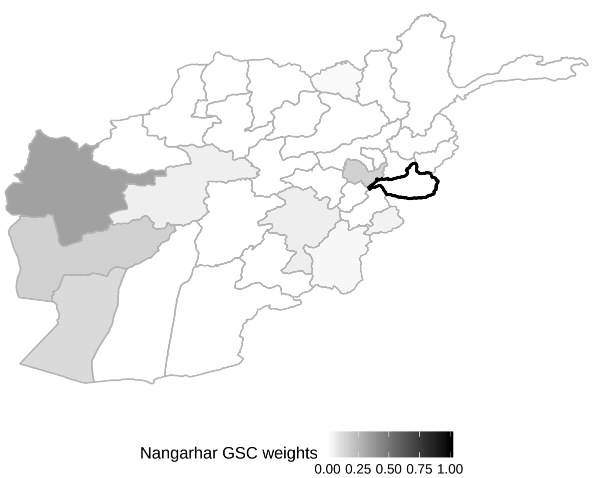
(b) Kandahar



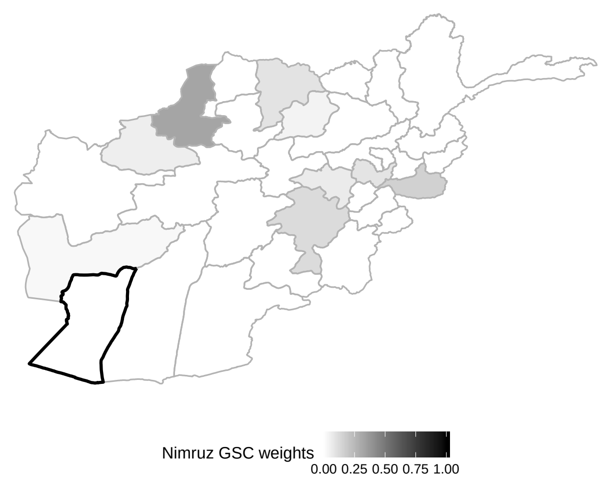
(c) Kabul



(d) Herat



(e) Nangarhar



(f) Nimruz

Figure 12: Generalized synthetic control weights for four largest revenue provinces. Province to which weights apply outlined in black.

The first measure is forward-looking. Given an estimated cumulative impact of conflict γ this is defined as:

$$M_1(\gamma) = \sum_i w_{i,2016}\ e^{\gamma d_i} \tag{7}$$

where i indexes provinces, $w_{i,2016}$ is the 2016 of share provincially collected revenues in province i, and $d_i = \left(\sum_{t \in 2004} x_{i,t} - \sum_{t \in 2016} x_{i,t}\right)/12$ is the difference in average conflict shocks between 2016 and 2004. $M_1(\gamma)$ computes the estimated per-year increase in relative revenues if violence permanently returns back to 2004 levels, relative to 2016. This is the direct equivalent of approximate revenue loss discussed in Section 4. The differences are that here d_i is measured exactly, and that the relative effect is scaled by the revenue share.

The second measure is backward-looking. It is given by:

$$M_2(\gamma) = \frac{1}{rev_{2016} \times usd_{2016}} \sum_{i,t} (1+r)^{(T-t)} usd_t \times rev_i\ e^{\gamma(x_{i,t} - x_{i,0})} \tag{8}$$

where r is a per-period interest rate, $rev_{i,t}$ is provincially collected revenues in province i in period t, rev_{2016} is *total* provincially-collected revenues in 2016, usd_t is the period t Afghani-US dollar exchange rate, and $x_{i,0}$ is the conflict shock to province i at the start of the sample, March 2005. Converting to contemporaneous US dollars allows me to use an international (rather than Afghan) interest rate to accumulate the period costs – here I set $r = 4\%$ per year. $M_2(\gamma)$ is thus the net present value of past losses from conflict, expressed as a share of most recent revenues. Where $M_1(\gamma)$ is a flow gain to future peace, $M_2(\gamma)$ is a stock of costs from past conflict.

Table 9 presents computed values for $M_1(\gamma)$ and $M_2(\gamma)$, using point estimates from the approaches pursued in previous sections. Columns (1)-(4) present the OLS estimates, from both the local projection and dynamic equation specifications. As discussed previously, these estimates likely understate the causal impact of conflict on revenues. And so the numbers presented in Table 9 should be thought of as lower bounds on the true effect.

Columns (5)-(8) use long-run effects from two stage least squares and generalized synthetic controls, which better-identify the causal impact of conflict on revenues. So as a headline measure, I focus on the results in column (6). This is because the estimated effect here is plausibly causal (as the two stage least squares approach is to many forms of bias) statistically significant (see Table 7) and conservative (in that it is smaller than the estimates in column (7)).

The headline results imply a very large annual peace dividend – around 50 percent of revenues, or about 6 percent of GDP. This is roughly the same as the Afghan government's combined annual spending on development projects in infrastructure, education, health, and agriculture (which amounted to 6.3 percent GDP in 2017). Even the OLS estimates are in the order of 10 percent of revenues (more than 1 percent of GDP).

In all cases, the exact impact of conflict on revenues is a little larger than the approximate one computed previously, which involved dividing by four. This is because there is a positive association between average revenues and increased conflict over the sample period; provinces which generate more revenue have seen larger increases in violence. And so the total losses are a little larger than simply scaling the aggregate by the average effect. The unweighted version of $M_1(\gamma)$, which weights all provinces by $1/N$, confirms this, giving an overall loss which is indeed approximately $\gamma/4$.

The historical loss calculations, represented by $M_2(\gamma)$ are much larger – somewhat unsurprisingly as this is a accumulated stock. The headline estimate is over 140 percent of revenues. This is equivalent to nearly 17 percent of GDP, or a little more than \$3bn. This is more than total non-discretionary developments grants[34] to the Afghan government cumulative from 2011 to 2017, and is similiar to the United State's contribution to the Afghanistan Reconstruction Trust Fund[35]. The lower bound on the cumulative cost of conflict coming from the OLS estimates is still very large, typically more than \$800m.

Gathering together the estimates in Table 9 also highlights the differences between the three

34. This is the main channel through which foreign donors disburse fund for specific development projects (as opposed to discretionary development grants, which can be used by the government for any purpose).

35. The ARTF is the main vehicle by which donor governments provide budgetary support to the government of Afghanistan. As of April 2018, the United States was the largest contributor, providing \$3.2bn of the \$10.9bn total contributions.

estimation approaches. The IV estimates are larger than the GSC estimates, which in turn are larger than the OLS estimates. These differences can be interpreted as separating out the contributions of the various ways in which the OLS identifying assumptions can be violated. As the GSC estimates are robust to very general forms of omitted variables, the difference between the GSC and OLS estimates can be attributed to omitted variables. And as the IV estimates are robust to not only omitted variables bias but also measurement error, then the difference between the IV and GSC estimates can be attributed to measurement error.[36] As the headline GSC estimate (in column (8) of Table 9) falls approximately halfway between the headline IV and OLS estimates (columns (6) and (4) respectively), this decomposition suggests that omitted variables and measurement error are roughly equal contributors to bias in the OLS estimates.

8 Conclusion

In this paper I present estimates of government revenue loss due to conflict in Afghanistan. I find that the fiscal peace dividend is likely to be large, at around 50 percent of current revenues. The historic cost of the Afghan conflict is even larger, at around 140 percent current revenues – roughly 3 billion US dollars. I show that OLS estimates under-estimate the magnitude of this cost, due in roughly equal share to omitted variables and attenuation bias. I also present evidence that this loss arises due to a reduction in revenue share of local activity, rather than reduced local economic activity.

These estimates are valuable in their own right as a measure of the gains that peace could bring to a country long riven by violence. But they also have a broader applicability to international attempts to increase state capacity and public living standards in conflict-affected areas. In such settings, provision of security competes for finite resources with many other valuable activities such as infrastructure, education, health care, and the like. The evidence presented here suggests that finding ways to reduce the intensity of conflict violence may prove a highly effective way to

36. While reverse causality is also a potential source of bias, as discussed in Section 5.2, measurement error is likely to be a more important factor in explaining the OLS bias.

Table 9: Cost of conflict: share of annual revenues

	LP-OLS	LP-OLS	DE-OLS	DE-OLS	TSLS	TSLS	TSLS	GSC-2I
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
γ	-0.32	-0.26	-0.36	-0.35	-0.65	-1.32	-2.01	-0.79
$M_1(\gamma)$	0.09	0.07	0.11	0.10	0.21	0.52	1.03	0.26
$M_1(\gamma)$, unweighted	0.08	0.06	0.09	0.08	0.16	0.35	0.58	0.20
$M_2(\gamma)$	0.35	0.28	0.40	0.38	0.71	1.43	2.19	0.86
$M_2(\gamma)$, USD bn	0.81	0.65	0.91	0.89	1.63	3.29	5.03	1.98
Security shock lags	0	0	6	6	0	6	6	6
Revenue lags	24	24	24	24	24	6	24	6
Meteorological controls	No	Yes	Yes	Yes	Yes	Yes	Yes	No
Regional price controls	No	Yes	No	Yes	Yes	Yes	Yes	No
Regional opium price controls	No	Yes	No	Yes	Yes	Yes	Yes	No

Note: LP-OLS: local projection OLS, DE-OLS: dynamic equation OLS, TSLS: Two stage least squares, GSC-2I: Generalized synthetic control, individual weighted two step boost the capacity of the state to provide services for its citizens.

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A Data sources and cleaning

A.1 Detailed data description

Conflict fatalities *UDCP GED:* Event-level data on conflict fatalities, available for download from http://ucdp.uu.se/downloads/. See Sundberg and Melander (2013) for many further details on the dataset, including definitions and sources. *Small Arms Survey:* National fatality rate for violent deaths, available for download from http://www.smallarmssurvey.org/gbav. See Mc Evoy and Hideg (2017), especially Annex 3, for details on sources and methodology. *UNAMA:* Civilian deaths and injuries by region, 2009-17, taken from UNAMA (2018).

Cross-sectional covariates These come from the 2003 National Risk and Vulnerability Assessment (NRVA) in Rural Afghanistan. This collected data survey from 11,757 households, containing 85,577 individuals across all provinces of Afghanistan. The sample is then reweighted to be representative of the rural population (over 80 percent of the national population, according to the 2003-05 household listing).

Foreign troop levels *ISAF:* The "Resolute Support Facts & Figures" series is published near-monthly, and covers both US and other coalition troops from 2007 to 2017. An archive is available to download from https://www.nato.int/cps/en/natolive/107995.htm. *Congressional Research Service:* Peters, Schwartz, and Kapp (2017) present figures from US Central Command covering US troops from 2007 to 2016. *New York Times:* Fairfield, Quealy, and Tse (2009) also cite US Central Command as their primary source for US troops between 2002 and 2009. Links to the original Central Command reports fora both the CRS and NYT data no longer work, but data from 2007-09 largely overlap (see Figure 3).

Language shares UNFPA and CSO (2007) include data on the majority language of sampled villages in the 2003-05 household listing. This measure excludes urban areas. See note on Provincial population shares for further details about the household listing.

National Populations World Bank, compiled from UN World Population Prospects and national censuses. Countries used: Afghanistan, Iraq, Libya, Sudan, Sri Lanka, Somalia, and Syria

Nighttime lights data Annual nighttime lights data are collected by the Operational Linescan System of the US Air Force Defense Meteorological Satellite Program. From 1992 until 2013, data were archived by the National Oceanic and Atmospheric Administration (NOAA), and are available for download from https://ngdc.noaa.gov/eog/dmsp/downloadV4composites.html. Since the launch of Suomi National Polar-orbiting Partnership satellite in 2011, NOAA produces average nighttime lights composite images using data from the Visible Infrared Imaging Radiometer Suite Day-Night Band. Monthly data are available starting in April 2012 and can be downloaded from https://ngdc.noaa.gov/eog/viirs/download_dnb_composites.html

Opium prices Collected from Afghan Drug Price Monitoring monthly reports, published jointly by UNODC and the Afghan Ministry of Counter Narcotics. These report farm gate prices of of dry opium in the five regions used by the UNODC: Northeast, Northwest, South, East, and West.

Provincial population shares The National Household Listing, conducted during 2003-05, records every household in Afghanistan. This was a precursor to an intended census in 2008, and also includes basic information about households and individuals living therein. Due to deteriorating security conditions, the 2008 survey was never conducted. UNFPA and CSO (2007) report various statistics from the listing, including provincial populations. The Central Statistics Office does publish annual provincial population estimates, but I do not use them due to data quality concerns. These estimates are computed by growing forward population shares at annual growth rates which sometimes differ across provinces. The CSO provides no further details on source data for these growth rates; they appear to be based purely on judgment instead.

Provincial revenues Monthly Fiscal Bulletins including provincial revenues are available online from http://www.budgetmof.gov.af/index.php/en/2012-12-06-22-51-13/fiscal-bulletin.

Precipitation From "CRU TS4.01: Climatic Research Unit (CRU) Time-Series (TS) version 4.01 of high-resolution gridded data of month-by-month variation in climate", published online by CEDA at http://catalogue.ceda.ac.uk/uuid/58a8802721c94c66ae45c3baa4d814d0. This uses weather satellites to produce global data for a grid of one half-degree latitude by one half-degree longitude boxes. At Afghanistan's latitude, these are rectangles of approximately 25 by 35 miles. See Harris et al. (2014) for further details about this data. I convert the boxes to average provincial precipitation by averaging across CEDA gridpoints within each province. My figures match the ground-measured NOAA normals for major cities reasonably well for lowland parts of Afghanistan. For example the Herat City NOAA mean is 239mm, whereas my average for Herat province is 246mm. But for the more mountainous regions, the numbers are often much larger than the NOAA measures. For instance, NOAA mean precipitation for Kabul City is 312mm, whereas my measure for Kabul province is 726mm. Presumably this occurs because cities in upland areas are located in valleys, and so have less precipitation (snowfall in particular) than the province as a whole. Given the importance of annual snowfalls to Afghan agriculture, using the satellite data is likely to provide a better control for local economic conditions.

Wheat & rice prices From the World Food Program's Vulnerability Analysis and Mapping project. This samples prices monthly in markets in ten provinces. Six provinces have data starting in 2005 (for wheat), and 2006 (for rice): Badakhshan, Faryab, Herat, Kabul, Kandahar, and Nangarhar. As at least one province is in each of the five UNODC regions, I create regional wheat and rice prices by averaging across the markets in each region.

A.2 Corroborating the UCDP-GED data on conflict

I corroborate the UCDP GED data using two other data sources. The first is the Small Arms Survey Database on Violent Deaths (see Figure 1). This covers all violent deaths, including those resulting from homicides and legal interventions, as well as from conflict. As should be expected, this provides an upper bound on the UCDP GED fatality rates. It also matches the overall level and year-to-year movements fairly well. This both provides an independent cross-check on the UCDP GED data, as well as verifying that conflict violence is the major risk to life during this period.[37]

The second corroborating dataset provides a cross-check on the spatial variation in the UCDP GED data, albeit imperfectly. Figure 2 includes plot civilian deaths and injuries, as measured by the United Nations Assistance Mission to Afghanistan (UNAMA). This is narrower than the UCDP data in one dimension (it covers only civilians), but broader in another (it includes both deaths and injuries). For much of the sample, these two differences largely offset, suggesting that number of civilian injuries and non-civilian deaths have been roughly equal over this period. Moreover, the agreement of the levels across provinces makes clear that the spatial distribution of violence across these two very different measures is similar: violence in one region *relative* to another is very similar across the two measures. This provides reassurance that the spatial variation in the UCDP data is reliable. The divergence of the UCDP GED and UNAMA data in the last few years matches anecdotal reports that insurgent groups have shifted away from targeting civilians, focusing instead on attacks on the security forces.

A.3 Outliers and timing errors

The provincial revenue data include numerous obvious outliers. Typically, these involve a coding error in the monthly reports, such as a misplaced decimal point. This is then corrected in

37. Of course, the two series diverge somewhat from 2012 onward. While this might reflect difficulties of gathering accurate data in a war-torn country, it could also be because non-conflict deaths have indeed increased, perhaps reflecting a more general breakdown in government control.

subsequent reports, but only by an offsetting error. Figure 13 shows the raw monthly data for four provinces[38], which all show this pattern of errors during 2006 and 2009. While such outliers are obvious to the naked eye, it is less obvious how to treat them systematically, as the timing and magnitude of the errors is not quite the same across provinces and periods. The negative values for revenues are particularly troublesome, as they become missing values under a log-transformation. With lagged revenues a regressor, this removes many sample observations.

In order to systematically remove outliers, I compute for each monthly observation a local mean and standard deviation using a two-sided twelve-month window (the point itself is excluded from the window). Any point which deviates from the local mean by more than 10 times the local standard deviations is considered an outlier, and replaced with the local mean. The process is repeated until there are no remaining outliers. This replaces 204 of the 4,828 month-province observations, or almost exactly six outliers per province. Given that outliers usually come in pairs, and that one can easily identify two or three candidate pairs for each province in Figure 14 by eye alone, this process seems to be reasonably accurately eliminating mis-coded points. Quarterly and annual averages are then computed from the cleaned monthly underlying data.

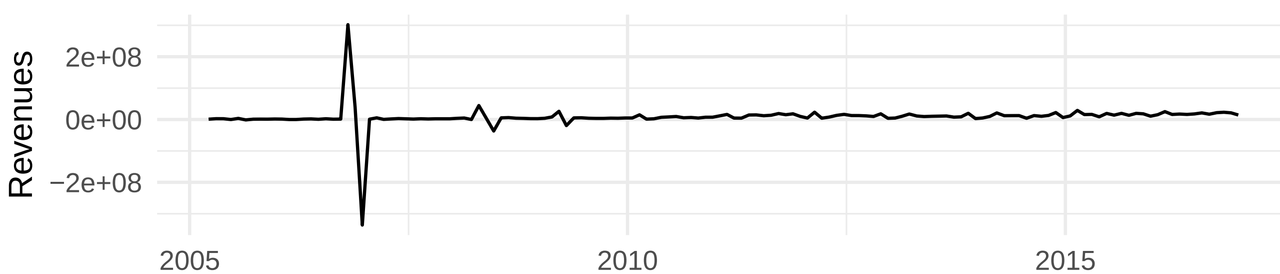
The results of the outlier removal process for the sample provinces are shown in Figure 14. Even though the data no longer include obvious mistakes, they are very volatile, with several months still recording zero revenues. As a result, I decide to aggregate the data to quarterly frequency for the main empirical analysis, although I do conduct some robustness checks with the monthly data.

Table 10 describes the effect on key moments of the revenue data, after aggregating to quarterly averages. The median level and growth rate are little unchanged, but the top and bottom of the distribution are substantially affected, suggesting that the basic properties of much of the data are little changed, as one would hope.

38. Two which collect a large share of provincial revenues – Herat and Nangarhar – and two which collect less – Farah and Laghman.

(a) Farah (4 outliers detected)

(b) Herat (4 outliers detected)



(c) Laghman (7 outliers detected)

(d) Nangarhar (5 outliers detected)

Figure 13: Raw revenue data for selected Provinces.

(a) Farah

(b) Herat

(c) Laghman

(d) Nangarhar

Figure 14: Cleaned revenue data for selected Provinces.

	Original Data	Outliers Removed	Difference
Level (10m Afghanis)			
Mean	11.3	11.5	0.1
Median	1.1	1.2	0.1
1st percentile	-2.7	0.0	2.7
5th percentile	0.0	0.1	0.1
10th percentile	0.1	0.1	0.1
25th percentile	0.3	0.3	0.0
75th percentile	5.3	5.2	-0.1
90th percentile	34.2	33.3	-0.9
95th percentile	78.0	76.2	-1.8
99th percentile	156.2	144.1	-12.1
Med. within-province coeff. of variation	1.25	0.89	-0.36
Med. across-province coeff. of variation	2.26	2.23	-0.03
Number weakly negative	327	0	-327
Growth rate			
Mean	0.03	0.01	-0.02
Median	0.03	0.01	-0.02
1st percentile	-1.98	-1.56	0.43
5th percentile	-0.87	-0.81	0.05
10th percentile	-0.55	-0.51	0.04
25th percentile	-0.16	-0.15	0.01
75th percentile	0.22	0.20	-0.02
90th percentile	0.54	0.49	-0.06
95th percentile	0.83	0.72	-0.10
99th percentile	2.25	1.53	-0.72
Med. within-province std. dev.	0.54	0.41	-0.13
Med. across-province std. dev.	0.41	0.39	-0.01
Number NAs	150	0	-150

Table 10: Revenue data with and without outliers, 4828 monthly observations. Aggregated from 4828 monthly observations, of which 123 classified as outliers. Growth rates computed as log difference of Revenues + 1.

A.4 The filtering transformation for conflict intensity

I transform the data for conflict intensity s_{it} by estimating a panel auto-regression of the form:

$$s_{it} = a_i + b_t + \sum_{l=]}^{N} \gamma_l s_{i,t-l} + x_{it}$$

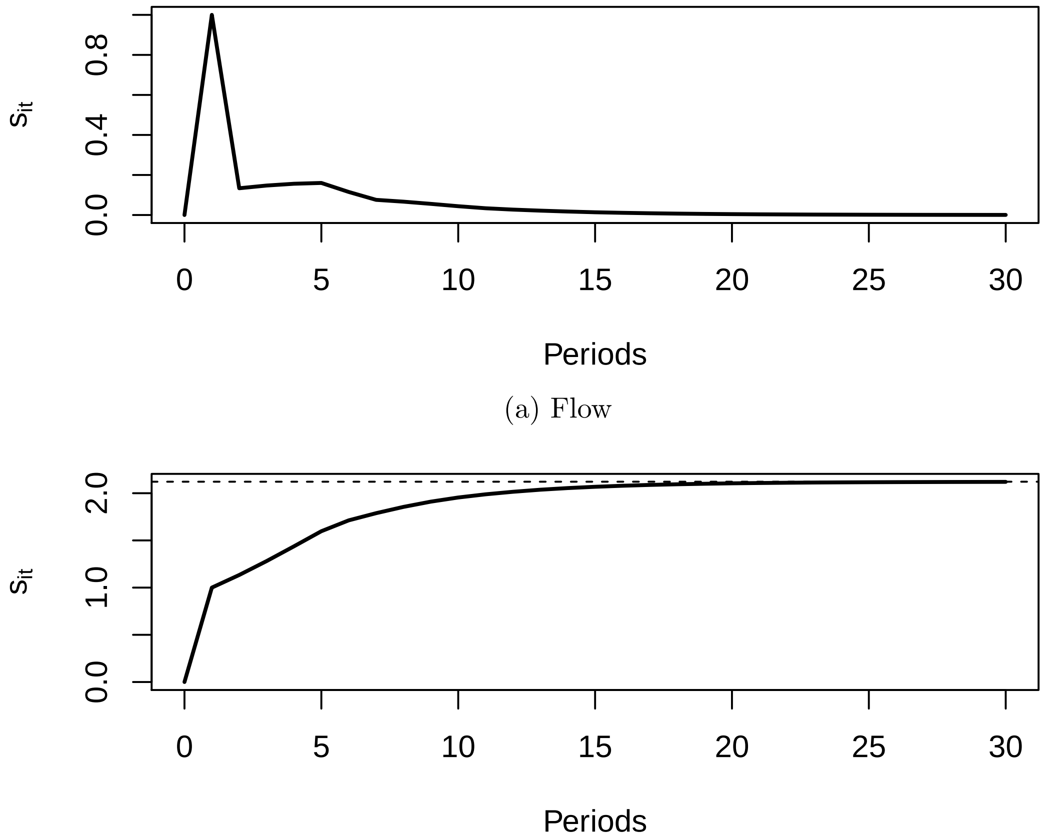
where a_i is a province-specific intercept, and b_t is a seasonal dummy. The resulting error, x_{it} is a proxy for the unpredictable component of s_{it}.

In Table 11, I report the results of this filtering procedure, using the Akiake Information Criterion to select the lag length. Note that in all cases, the cumulative effect of a shock to conflict fatalities is a little over 2, as described in the line labeled "Long-run impact". In other words, an immediate increase equivalent to one conflict fatality per year is typically associated with a persistent effect of similar magnitude. Figure 15 illustrates this graphically, showing the impulse responses of s_{it} to a unit increase in x_{it}.

Table 11: Conflict intensity filter transformation

	Dependent variable: Conflict intensity				
	Annual	Quarterly	Quarterly	Monthly	Monthly
	(1)	(2)	(3)	(4)	(5)
Conflict intensity, lag 1	0.696***	0.437***	0.439***	0.151***	0.134***
	(0.052)	(0.023)	(0.023)	(0.013)	(0.013)
Conflict intensity, lag 2	−0.110**	0.073***	0.104***	0.134***	0.129***
	(0.052)	(0.023)	(0.022)	(0.013)	(0.013)
Conflict intensity, lag 3				0.114***	0.119***
				(0.013)	(0.013)
Conflict intensity, lag 4				0.090***	0.104***
				(0.013)	(0.013)
Conflict intensity, lag 5				0.024*	0.042***
				(0.013)	(0.013)
Seasonal dummies	No	No	Yes	No	Yes
Observations	442	1972	1972	5950	5950
AIC statistic	-1242	-4168	-4236	-7546	-7625
Long-run impact	2.42	2.04	2.19	2.05	2.12

Note: *p<0.1; **p<0.05; ***p<0.01
Lag length selected by AIC



(a) Flow

(b) Stock, cumulative

Figure 15: Impulse response for a panel autoregression on conflict intensity, s_{it}, monthly frequency.

B Case study: The southern insurgency of 2007-08

In this section I estimate the impact of conflict on government revenues using only cross-sectional data. I do this by studying only the provincial changes in conflict and revenues during the period of the southern insurgency, compressing the panel to a cross-section. This episodes saw large, and spatially heterogeneous increases in conflict violence. The aim of this approach is twofold. First, it provides an additional cross-check on the estimation in the dynamic panel. Second, the simplicity of the sample mechanisms of the estimation strategy are much clearer, throwing additional light on the instrumental variables strategy used in the dynamic panel.

I take 2005 as a baseline pre-insurgency year, and compare it to the average during 2007-08. I average across two post-insurgency years to reduce noise in the revenue data, which we know to be large, and because the result ins in section 4 suggest that there is a delayed impact of conflict on revenues. In a two-period setting, taking first differences is identical to including province fixed effects in the level.

Figure 16 plots the change in log revenues between 2005 and 2007-08 against the change in provincial conflict fatalities per 1,000 over the same period. The negative correlation is large, and statistically significant at the 5 percent level (see column (1) of Table 12). Adding a subset of the pre-insurgency covariates controls partly for omitted variables – prior differences in the provinces that might cause correlation between changes in conflict and revenues. These have only a small impact on the relationship between conflict and revenues. Note that size of the estimated response is a little larger the OLS estimates in Section 4.

Table 12 also shows the instrumental variables estimates, using only provincial Pashto share as an instrument. In common with the more sophisticated dynamic panel estimates, the IV coefficients are around twice as large as the OLS estimates, again suggesting that attenuation bias is at work here.

This simple approach has many drawbacks – few sample points, few covariates, standard errors unlikely to be robust to correlation. But in spite of these limitations, the estimates from the case study back up those in the more sophisticated estimation specifications.

Table 12: Cross-sectional regressions: 2005 vs 2007-08 average

	Dependent variable: Provincial revenues, log difference			
	(1)	(2)	(3)	(4)
Ordinary least squares				
Deaths per 1,000, difference	-0.554^{**}	-0.466^{*}	-0.533^{*}	-0.732^{**}
	(0.236)	(0.274)	(0.280)	(0.292)
Temperature, 2005			0.0002	-0.001
			(0.001)	(0.001)
Precipitation, 2005		-0.0005	0.004	-0.011
		(0.017)	(0.020)	(0.020)
Log opium price, 2005		-0.448		-0.920^{***}
		(0.288)		(0.314)
Log wheat price, 2005				13.585^{**}
				(5.421)
Instrumental variables				
Deaths per 1,000, difference	-0.905^{*}	-0.848	-1.302^{**}	-2.722^{*}
	(0.493)	(1.032)	(0.628)	(1.440)
Residual std dev	0.44	0.42	0.44	0.37
Instrument F stat	10.3	2.4	10.0	3.4
p-value	0.00	0.13	0.00	0.07
Observations	34	34	34	34
Adjusted R^2	0.121	0.132	0.069	0.280

Note: *p<0.1; **p<0.05; ***p<0.01
Spherical standard errors in parentheses

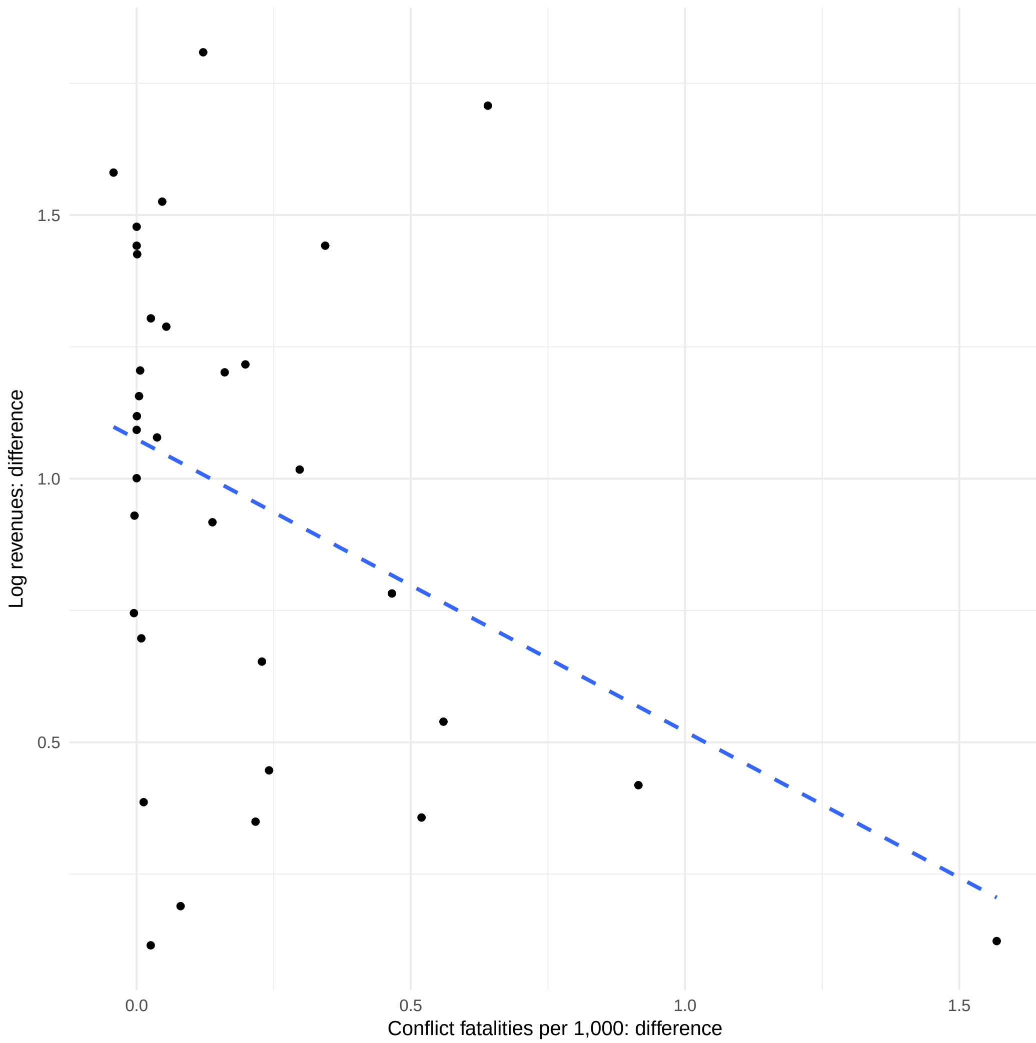


Figure 16: Change in conflict fatalities and revenues during the southern insurgency

C Robustness of the main estimates

C.1 Province-specific time trends

As a robustness check, Table 13 reports coefficient estimates including Province-specific time trends. This accounts for the possibility that the time-invariant difference in the provinces may extend to growth rates, as well as level differences. The long-run estimates remain negative, stable, and statistically significant. The long-run impact is slightly smaller than that reported in Table 4. However, this is potentially the result of the time trend absorbing some meaningful long-run variation in revenues which is correlated with conflict. If the provinces which became more violent over the sample also had lower revenue growth, then this would be absorbed into the Province=specific time trend. To the extent that this effect is causal, we can treat the smaller effects in Table 13 as a plausible lower bound on the impacts measured in Table 4.

C.2 Cross-province spillovers

The impact of shocks may not be felt purely locally. Violence in one province may impact revenue collection in its neighbors in many ways. For example, violence may interrupt inter-provincial trade, or displace people across provincial borders, or act as a signal of local insurgent strength. This is particularly important when using local estimates to infer national, aggregate responses to increased violence. If local violence has a spillover effect on revenue collection in other provinces, then this needs to be taken into account when computing counterfactual revenues under alternative levels of conflict violence.

As a means of addressing this issue, Table 14 presents the results of augmenting equation (5) with lags of population-weighted violence in neighboring provinces. The inclusion of violence in neighboring provinces appears to have no statistically significant effect at any lag. In fact, the coefficients are very poorly estimated, with standard errors far in excess of those on the local conflict shock. In addition, there is no noticeable change in the local conflict shock effect, or on the cumulative revenue loss from violence – the lags on the conflict shocks and the cumulative impact are near-identical to those in Table 5. Taken together, these observations are consistent with a zero impact of violence in neighboring provinces on local revenue collection. Accordingly, I compute the counterfactual revenue losses assuming that there is no spillover of violence from neighboring provinces.

C.3 Influential observations

To verify that particularly influential observations are not driving the results, I recalculate the OLS estimates excluding the data for Helmand and Kandahar provinces. These two neighboring provinces experience the most conflict fatalities during the sample, accounting for 28 percent

Table 13: Effect of conflict on provincial revenues: Including time trends

	Dependent variable: Monthly provincial (log) revenues						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Conflict shock, lag 0	−0.042***	−0.025**	−0.016	−0.022**	−0.019	−0.021*	−0.019
	(0.014)	(0.011)	(0.014)	(0.010)	(0.012)	(0.011)	(0.013)
Conflict shock, lag 1	−0.049***	−0.026	−0.021	−0.029*	−0.024	−0.019	−0.022
	(0.018)	(0.016)	(0.020)	(0.015)	(0.019)	(0.018)	(0.020)
Conflict shock, lag 2	−0.022	−0.001	0.004	0.001	0.001	0.006	0.002
	(0.018)	(0.017)	(0.014)	(0.014)	(0.015)	(0.016)	(0.016)
Conflict shock, lag 3	−0.004	0.012	0.015	0.020	0.014	0.010	0.011
	(0.029)	(0.031)	(0.030)	(0.030)	(0.029)	(0.029)	(0.028)
Conflict shock, lag 4	−0.024	−0.007	−0.004	−0.009	−0.008	−0.010	−0.008
	(0.016)	(0.014)	(0.015)	(0.014)	(0.014)	(0.013)	(0.014)
Conflict shock, lag 5	−0.036	−0.021	−0.014	−0.025	−0.018	−0.015	−0.018
	(0.033)	(0.035)	(0.033)	(0.039)	(0.034)	(0.037)	(0.035)
Conflict shock, lag 6	−0.036	−0.025	−0.016	−0.026	−0.020	−0.023	−0.023
	(0.031)	(0.031)	(0.031)	(0.025)	(0.026)	(0.025)	(0.024)
Revenue lags	0	6	24	6	24	6	24
Last significant lag	0	0	20	0	18	0	18
Revenue persistence	0.00	0.34	0.46	0.34	0.47	0.32	0.48
	(0.000)	(0.089)	(0.080)	(0.089)	(0.075)	(0.087)	(0.073)
Meteorological control lags	0	0	0	24	24	24	24
Meteorological ave, lags	0	0	0	36	36	36	36
Price control lags	0	0	0	0	0	24	24
Price ave, lags	0	0	0	0	0	36	36
Opium price lags	0	0	0	0	0	24	24
Cumulative impact	-0.212*	-0.143**	-0.098	-0.137**	-0.143**	-0.105**	-0.146***
	(0.159)	(0.083)	(0.079)	(0.072)	(0.064)	(0.058)	(0.061)
Autocorrelation test stat	7.7	0.0	0.1	0.0	0.1	0.0	0.1
p-value	0.01	0.88	0.78	0.87	0.75	0.85	0.78
Residual standard deviation	0.731	0.695	0.677	0.686	0.665	0.665	0.659
Observations	4,896	4,692	4,080	4,454	4,080	4,062	4,062
F Statistic	10.091***	13.810***	9.802***	4.836***	4.681***	3.104***	3.197***

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by province.
All specifications include a full set of time and province fixed effects and
Province-specific linear time trends.
AR tests from Wooldridge (2010).
Significance of cumulative impact based on a one-sided test

Table 14: Effect of conflict on provincial revenues: Including neighboring provinces

	Dependent variable: Monthly provincial (log) revenues						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Conflict shock, lag 0	-0.035^{***}	-0.019^{**}	-0.021	-0.019^{**}	-0.023^{*}	-0.020^{*}	-0.022^{*}
	(0.012)	(0.009)	(0.015)	(0.009)	(0.014)	(0.011)	(0.013)
Conflict shock, lag 1	-0.041^{***}	-0.018	-0.025	-0.024^{*}	-0.028^{*}	-0.017	-0.026
	(0.015)	(0.012)	(0.016)	(0.013)	(0.017)	(0.015)	(0.018)
Conflict shock, lag 2	-0.019	0.005	-0.003	0.005	-0.005	0.006	-0.003
	(0.019)	(0.019)	(0.015)	(0.017)	(0.016)	(0.017)	(0.017)
Conflict shock, lag 3	0.003	0.018	0.009	0.026	0.010	0.013	0.009
	(0.029)	(0.033)	(0.031)	(0.031)	(0.030)	(0.031)	(0.028)
Conflict shock, lag 4	-0.020	-0.003	-0.012	-0.008	-0.017	-0.014	-0.015
	(0.016)	(0.015)	(0.016)	(0.014)	(0.015)	(0.014)	(0.014)
Conflict shock, lag 5	-0.031	-0.017	-0.022	-0.023	-0.024	-0.016	-0.023
	(0.034)	(0.037)	(0.035)	(0.039)	(0.035)	(0.038)	(0.036)
Conflict shock, lag 6	-0.031	-0.021	-0.025	-0.024	-0.028	-0.025	-0.028
	(0.030)	(0.030)	(0.029)	(0.025)	(0.025)	(0.025)	(0.025)
Neighboring conflict, lag 0	0.572	0.241	0.266	0.192	0.119	0.061	0.062
	(0.807)	(0.602)	(0.464)	(0.652)	(0.496)	(0.552)	(0.484)
Neighboring conflict, lag 1	0.601	0.324	0.331	0.471	0.382	0.365	0.366
	(0.467)	(0.330)	(0.299)	(0.320)	(0.274)	(0.327)	(0.278)
Neighboring conflict, lag 2	0.206	-0.055	-0.018	0.061	0.046	0.204	0.152
	(0.507)	(0.395)	(0.416)	(0.392)	(0.408)	(0.420)	(0.403)
Neighboring conflict, lag 3	0.097	-0.037	-0.029	0.064	0.059	0.133	0.109
	(0.584)	(0.484)	(0.461)	(0.400)	(0.386)	(0.377)	(0.394)
Neighboring conflict, lag 4	0.370	0.212	0.211	0.545	0.527	0.540	0.487
	(0.510)	(0.390)	(0.377)	(0.393)	(0.377)	(0.412)	(0.392)
Neighboring conflict, lag 5	0.193	-0.050	-0.076	-0.235	-0.304	-0.128	-0.170
	(0.424)	(0.345)	(0.356)	(0.425)	(0.370)	(0.335)	(0.362)
Neighboring conflict, lag 6	0.557	0.334	0.527	0.265	0.497	0.484	0.428
	(0.647)	(0.399)	(0.499)	(0.422)	(0.462)	(0.600)	(0.503)
Meteorological control lags	0	0	0	24	24	24	24
Meteorological ave, lags	0	0	0	36	36	36	36
Price control lags	0	0	0	0	0	24	24
Price ave, lags	0	0	0	0	0	36	36
Opium price lags	0	0	0	0	0	24	24
Cumulative impact	-0.174	-0.123	-0.353***	-0.144*	-0.398***	-0.154*	-0.370***
	(0.155)	(0.115)	(0.143)	(0.104)	(0.133)	(0.101)	(0.114)
Residual standard deviation	0.761	0.706	0.685	0.695	0.673	0.676	0.666
Observations	4,896	4,692	4,080	4,454	4,080	4,062	4,062
F Statistic	1.241	23.885***	14.079***	4.827***	4.903***	2.834***	3.168***

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by province.
All specifications include a full set of time and province fixed effects
Significance of cumulative impact based on a one-sided test

of all conflict deaths during this time, despite being home to only 10 percent of the national population. And in terms of per capita fatalities, Helmand by some distance the most violent province (several other smaller provinces have similar fatality rates to Kandahar). They are also economically important provinces, contributing 7 percent of provincially collected revenues.

The resulting estimates results are shown in Table 15. These are little changed from full-sample results in Table 4. Indeed, if anything, the long-run impacts are a little larger.

C.4 Annual frequency data

Here, I recompute the main results using annual frequency data. This has two main advantages. First, annual aggregation is a useful check that very volatile monthly observations are not driving the results. Second, because nightlights data are available over the sample at only an annual frequency – monthly data start in April 2012 – the annual data have an additional control for local activity.[39] This is important for the results of Section 4.4.

C.4.1 Dynamic equation estimates

Table 16 presents the headline results, which measures the impact of a shock to annual conflict intensity. This shows a large and significant effect of conflict at all horizons in all specifications, and a long run effect of around 0.4 for the most sophisticated specifications. This is comparable to results in Table 5, where a similar (two year) lag length for revenues produces estimates of a similar size. With fewer observations, the standard errors are necessarily larger, though.

Table 17 reports the results including Province-specific time trends. As in Table 13, this results in slightly smaller, but still statistically significant, effects. With annual data, the panel is short: there are only 12 periods. And so Nickell bias is might be a problem for this specification. The similarity with the monthly estimates is one indication that this is not a problem (as the long monthly panel means Nickell bias will be small). As further confirmation, I compute the results using an Arellano and Bond (1991) style estimator. This addresses Nickell bias by using differences

39. Night lights are a commonly used as a proxy for local economic activity, particularly in data-poror countries. See Henderson et al. (2018) for a recent example.

Table 15: Effect of conflict on provincial revenues: Dropping most violent provinces

	Dependent variable: Monthly provincial (log) revenues						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Conflict shock, lag 0	−0.047***	−0.028***	−0.027*	−0.029***	−0.032**	−0.029**	−0.032**
	(0.011)	(0.009)	(0.016)	(0.010)	(0.015)	(0.012)	(0.015)
Conflict shock, lag 1	−0.038*	−0.010	−0.025	−0.017	−0.028	−0.016	−0.027
	(0.020)	(0.016)	(0.020)	(0.017)	(0.020)	(0.017)	(0.020)
Conflict shock, lag 2	−0.017	0.007	−0.009	0.007	−0.011	0.002	−0.010
	(0.019)	(0.017)	(0.014)	(0.016)	(0.016)	(0.018)	(0.018)
Conflict shock, lag 3	0.016	0.033	0.018	0.040	0.018	0.024	0.017
	(0.025)	(0.028)	(0.029)	(0.026)	(0.026)	(0.027)	(0.025)
Conflict shock, lag 4	−0.024	−0.007	−0.019	−0.009	−0.021	−0.017	−0.020
	(0.018)	(0.017)	(0.018)	(0.017)	(0.017)	(0.016)	(0.015)
Conflict shock, lag 5	−0.028	−0.013	−0.020	−0.020	−0.023	−0.015	−0.023
	(0.036)	(0.040)	(0.037)	(0.042)	(0.036)	(0.040)	(0.038)
Conflict shock, lag 6	−0.033	−0.024	−0.027	−0.023	−0.028	−0.027	−0.031
	(0.035)	(0.034)	(0.033)	(0.029)	(0.029)	(0.029)	(0.028)
Revenue lags	0	6	24	6	24	6	24
Last significant lag	0	3	18	3	18	0	18
Revenue persistence	0.00	0.51	0.71	0.49	0.69	0.48	0.69
	(0.000)	(0.079)	(0.062)	(0.074)	(0.052)	(0.078)	(0.055)
Meteorological control lags	0	0	0	24	24	24	24
Meteorological ave, lags	0	0	0	36	36	36	36
Price control lags	0	0	0	0	0	24	24
Price ave, lags	0	0	0	0	0	36	36
Opium price lags	0	0	0	0	0	24	24
Cumulative impact	-0.172	-0.085	-0.372**	-0.101	-0.411***	-0.150*	-0.401***
	(0.164)	(0.091)	(0.162)	(0.081)	(0.138)	(0.094)	(0.120)
Autocorrelation test stat	11.2	0.0	0.1	0.0	0.1	0.0	0.1
p-value	0.00	0.96	0.82	0.91	0.78	0.90	0.79
Residual standard deviation	0.741	0.693	0.67	0.683	0.659	0.661	0.651
Observations	4,608	4,416	3,840	4,192	3,840	3,822	3,822
F Statistic	1.732*	27.748***	13.904***	4.162***	4.284***	2.479***	2.794***

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by province.
All specifications include a full set of time and province fixed effects.
AR tests from Wooldridge (2010).
Significance of cumulative impact based on a one-sided test

of the dependent variable at lags longer than M as instruments. With a short sample, there are insufficiently many moment conditions to estimate both individual and time fixed effects, so the results are not directly comparable to those in Table 16. Nevertheless, the resulting long-run estimates are all negative, and typically statistically significant. The magnitudes of the estimates are typically a little different to those computed by OLS. However, with so few moment conditions, less precise estimates are to be expected particularly for the more sophisticated specifications.

Table 16: Effect of conflict on provincial revenues

	Dependent variable: Annual provincial (log) revenues						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Conflict intensity shock	-0.128^{*}	-0.231^{**}	-0.249^{***}	-0.244^{***}	-0.254^{***}	-0.211^{*}	-0.258^{**}
	(0.072)	(0.095)	(0.084)	(0.095)	(0.086)	(0.124)	(0.109)
Revenue lag 1		0.544^{***}	0.442^{***}	0.524^{***}	0.417^{***}	0.439^{***}	0.331^{***}
		(0.076)	(0.090)	(0.069)	(0.085)	(0.074)	(0.083)
Revenue lag 2			0.107^{*}		0.107^{**}		0.078
			(0.058)		(0.054)		(0.055)
Revenue lags	0	1	2	1	2	1	2
Last significant lag	0	1	2	1	2	1	2
Revenue persistence	0.00	0.54	0.55	0.52	0.52	0.44	0.41
	(0.000)	(0.076)	(0.081)	(0.069)	(0.071)	(0.074)	(0.078)
Meteorological control lags	0	0	0	3	3	3	3
Price control lags	0	0	0	0	0	3	3
Nightlight lags	0	0	0	0	0	3	3
Opium price lags	0	0	0	0	0	1	1
Cumulative impact	-0.128**	-0.508**	-0.551***	-0.512**	-0.534***	-0.377*	-0.437**
	(0.072)	(0.244)	(0.220)	(0.224)	(0.199)	(0.244)	(0.210)
Autocorrelation test stat	72.5	0.0	2.1	0.0	2.3	0.0	1.7
p-value	0.00	0.85	0.15	0.83	0.13	0.94	0.19
Residual standard deviation	0.353	0.281	0.259	0.273	0.249	0.264	0.236
Observations	408	374	340	374	340	311	283
F Statistic	2.125	76.134^{***}	44.174^{***}	9.524^{***}	8.388^{***}	3.755^{***}	3.369^{***}

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by province.
All specifications include a full set of time and province fixed effects.
AR tests from Wooldridge (2010).
Significance of cumulative impact based on a one-sided test

Table 17: Effect of conflict on provincial revenues: Including time trends

	Dependent variable: Annual provincial (log) revenues						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Conflict shock, lag 0	-0.180^{**}	-0.188^{**}	-0.212^{***}	-0.205^{***}	-0.216^{***}	-0.237^{**}	-0.241^{**}
	(0.072)	(0.076)	(0.080)	(0.075)	(0.080)	(0.120)	(0.094)
Conflict shock, lag 1	-0.127	-0.046	-0.049	-0.050	-0.063	-0.090	-0.081
	(0.106)	(0.104)	(0.098)	(0.096)	(0.096)	(0.105)	(0.106)
Revenue lags	0	1	2	1	2	1	2
Last significant lag	0	0	2	0	2	0	2
Revenue persistence	0.00	0.22	0.15	0.21	0.17	0.12	0.06
	(0.000)	(0.088)	(0.094)	(0.086)	(0.096)	(0.076)	(0.096)
Meteorological control lags	0	0	0	3	3	3	3
Price control lags	0	0	0	0	0	3	3
Nightlight lags	0	0	0	0	0	3	3
Opium price lags	0	0	0	0	0	1	1
Cumulative impact	-0.307**	-0.302***	-0.310***	-0.322***	-0.338***	-0.371**	-0.342***
	(0.178)	(0.127)	(0.118)	(0.118)	(0.119)	(0.170)	(0.105)
Autocorrelation test stat	11.8	0.3	0.0	0.3	0.0	0.2	1.0
p-value	0.00	0.60	0.92	0.56	0.82	0.63	0.31
Residual standard deviation	0.258	0.243	0.213	0.236	0.209	0.227	0.196
Observations	408	374	340	374	340	311	283
F Statistic	8.313^{***}	7.861^{***}	8.028^{***}	5.776^{***}	5.683^{***}	3.181^{***}	3.173^{***}

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by province.
All specifications include a full set of time and province fixed effects and
Province-specific linear time trends.
AR tests from Wooldridge (2010).
Significance of cumulative impact based on a one-sided test

Table 18: Effect of conflict on provincial revenues: GMM estimates

	Dependent variable: Annual provincial (log) revenues						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Conflict intensity shock	-0.253^{***}	-0.220^{*}	-0.164	-0.277^{*}	-0.158	-0.114	-0.019
	(0.080)	(0.119)	(0.115)	(0.145)	(0.165)	(0.233)	(0.242)
Revenue lags	0	1	2	1	2	1	2
Last significant lag	0	1	2	1	2	0	2
Revenue persistence	0.00	0.49	0.66	0.39	0.83	0.03	0.58
	(0.000)	(0.078)	(0.097)	(0.244)	(0.199)	(0.230)	(0.162)
Meteorological control lags	0	0	0	3	3	0	0
Price control lags	0	0	0	0	0	3	3
Nightlight lags	0	0	0	0	0	3	3
Opium price lags	0	0	0	0	0	1	1
Cumulative impact	-0.253***	-0.434***	-0.479***	-0.457***	-0.913***	-0.118	-0.046
	(0.080)	(0.119)	(0.115)	(0.145)	(0.165)	(0.233)	(0.242)
Residual standard deviation	0.367	0.408	0.341	0.354	0.336	0.284	0.288
Observations	34	34	34	34	34	34	34

Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered by province.
All specifications include province fixed effects and linear time trend.
AR tests from Wooldridge (2010).
Significance of cumulative impact based on a one-sided test

C.4.2 Jorda projection

Figure 17 reports the result of estimating equations 3 and 4 at an annual frequency. Direct comparisons of the magnitude of the responses with the monthly estimates shown in Figure 7 are difficult, as the conditioning variables are different and because the conflict shocks are not directly comparable[40]. While the difference between the black and green lines in figure 17 is substantial – about half the long-run effect – it is not statistically significant. More precisely, the wide standard errors on the green line mean that we cannot reject the hypothesis that the true three-year effect including future controls is -0.48 (i.e. the long-run effect without future controls)

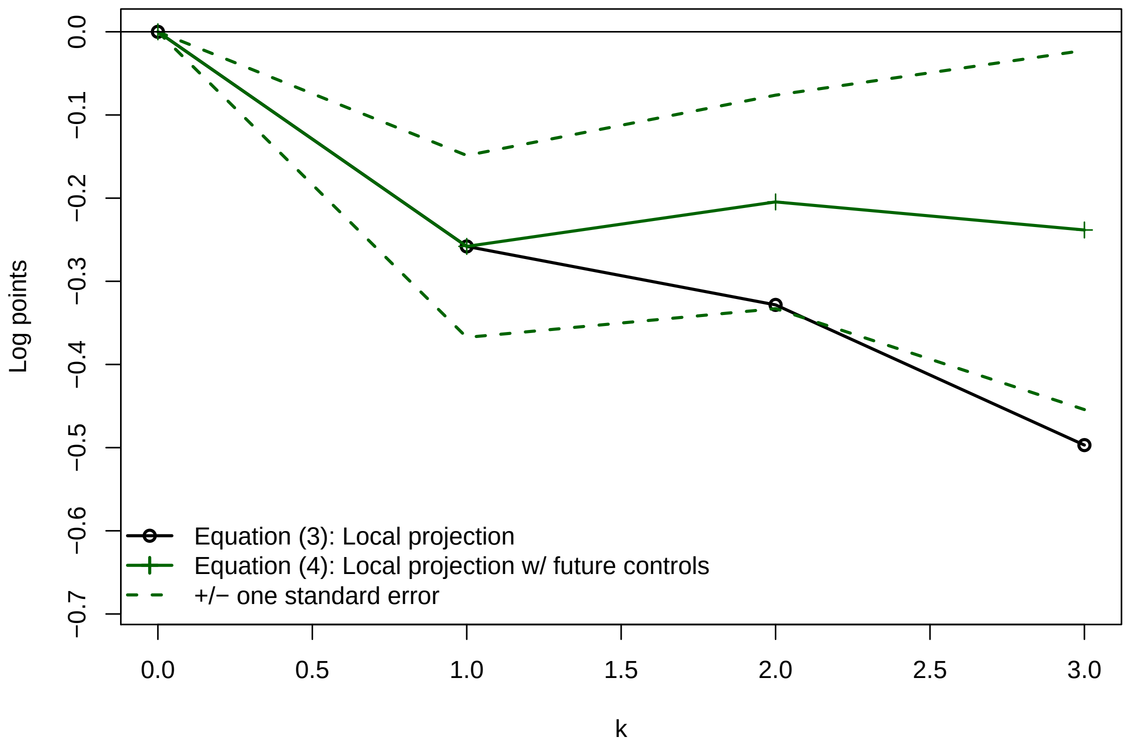


Figure 17: Cumulative revenue loss: local projection with future controls, including nightlights. Annual aggregation.

40. The shocks are of the same intensity but different duration, in that the monthly shock represents one-twelfth the number of fatalities of the annual one, but all in one month. These are equivalent if the response to 12 monthly unit shocks is the same as one annual shock. But if the response is nonlinear then the annual and monthly estimates will differ.

C.5 Alternative instruments

C.5.1 US Troop levels

Table 19 presents results using an alternative instrument, using US troop levels for the time-series component of the Bartik instrument. The instrument is not as strong a predictor as the one used in Section 5, but is plausibly independent of violence in any given province, buttressing the exogeneity requirement of the instrument.

The weakness of the instrument results in occasionally very large standard errors on the estimates, particularly when the specification includes many controls or lagged dependent variables. Nevertheless, the point estimates of the long run effect are comparable to those shown in Table 7, and in specifications (6) and (7) are statistically significant.

C.5.2 Cross-sectional predictors of violence

Figure 18 presents a variety of alternative cross-sectional shares and their correlation with conflict fatalities. Only the Dari-speaking share comes close to the predictive power of the Pashto-speaking share, largely because Dari and Pashto are the main languages spoken in most provinces.

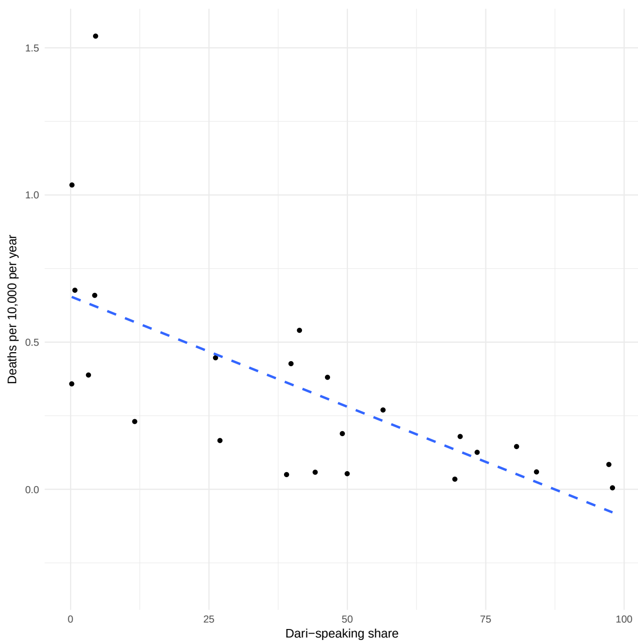
C.6 Alternative ethnographic data

Atlas Narodov Mira (1964) provides worldwide spatial data on ethno-linguistic groups. Each region lists a primary ethno-linguistic group, as well as sometimes a second, minor group. This means that any given ethnicity can be coded as the sole (primary) group, the primary group of two, the secondary group of two, or not at all. Figure 19 shows this coding for Pashtuns. For ease of comparison, Figure 20 reproduces Figure 10b. The distribution of Pashtuns in Figure 19 and of Pashto-speakers in Figure 19 are clearly very similar. The spatial distribution of ethnicities in Afghanistan has barely changed.

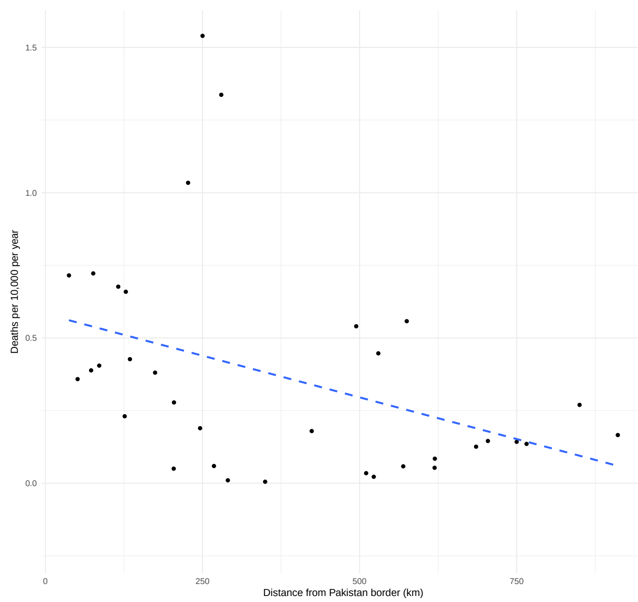
Table 19: Effect of conflict on provincial revenues: Instrumental variables estimates

	Dependent variable: Monthly provincial (log) revenues							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Conflict shock, lag 0	1.292	1.722	5.133	3.932	−0.892	−1.101	30.458	0.426
	(8.022)	(4.872)	(320.607)	(27.686)	(0.663)	(0.674)	(11,207.290)	(9.377)
Conflict shock, lag 1	−1.373	−1.545	−3.959	−2.245			−26.196	−1.110
	(11.173)	(5.680)	(272.602)	(18.555)			(9,570.632)	(10.879)
Conflict shock, lag 2	0.940	−0.983	−23.072	−6.855			−43.012	−1.181
	(5.025)	(3.646)	(1,428.476)	(44.082)			(15,810.920)	(14.808)
Conflict shock, lag 3	−1.024	1.782	37.010	6.153			13.550	1.796
	(5.925)	(4.607)	(2,305.975)	(35.681)			(4,671.768)	(9.505)
Conflict shock, lag 4	1.103	−0.573	−0.274	0.919			26.755	0.172
	(4.436)	(2.138)	(68.437)	(10.482)			(10,095.380)	(9.626)
Conflict shock, lag 5	−1.437	0.471	31.330	5.373			25.594	0.524
	(4.146)	(3.064)	(1,997.900)	(38.740)			(9,517.051)	(10.982)
Conflict shock, lag 6	0.778	−0.842	−36.008	−5.897			−25.580	−1.511
	(3.702)	(2.317)	(2,288.132)	(36.980)			(9,212.246)	(11.765)
Revenue lags	0	6	24	24	6	24	6	24
Last significant lag	0	4	0	0	6	21	0	0
Revenue persistence	0.00	0.51	1.31	0.79	0.49	0.69	-1.34	0.71
	(0.000)	(0.159)	(37.953)	(0.407)	(0.098)	(0.055)	(0.003)	(0.062)
Meteorological control lags	0	0	0	24	24	24	24	24
Meteorological ave, lags	0	0	0	36	36	36	36	36
Price control lags	0	0	0	0	24	24	24	24
Price ave, lags	0	0	0	0	36	36	36	36
Opium price lags	0	0	0	0	24	24	24	24
Cumulative impact	0.279	0.064	-32.308	6.482	-1.765*	-3.579**	0.671	-3.032
	(42.429)	(2.661)	(1689.765)	(69.980)	(1.108)	(2.106)	(188.108)	(6.860)
Instrument F statistic	0.00	3.12	3.22	3.72	6.71	6.53	6.71	6.53
Residual standard deviation	1.782	1.877	35.708	7.081	0.813	0.869	40.398	1.695
Observations	4,760	4,556	3,944	3,944	3,926	3,926	3,926	3,926

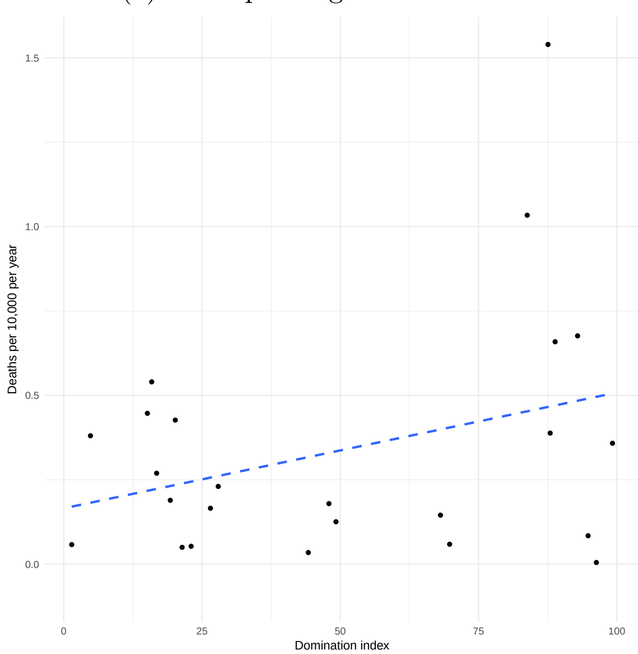
Note: *p<0.1; **p<0.05; ***p<0.01
Standard errors clustered at the province.
All specifications include a full set of time and province fixed effects.
AR tests from Wooldridge (2010).
Significace of cumulative impact is one-sided.



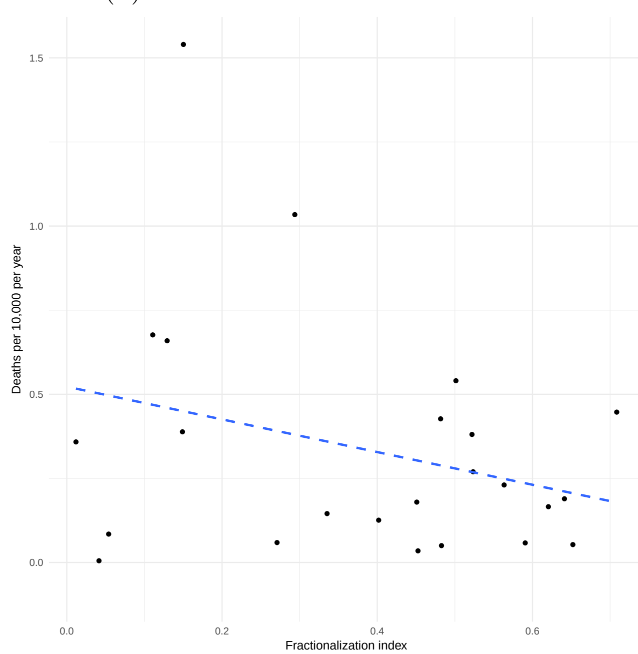
(a) Dari-speaking share 2003-05



(b) Distance from Pakistani border



(c) Cunningham and Weidmann (2010) domination index



(d) Alesina et al. (2003) fractionalization index

Figure 18: Mean annual conflict fatalities and alternative cross-sectional shares, by province.

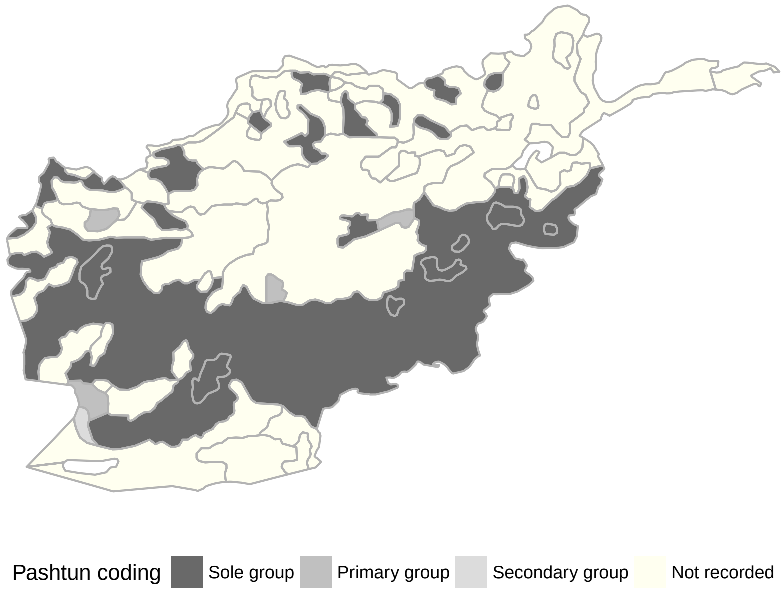


Figure 19: Atlas Narodov Mira (1964) ethnographic classification

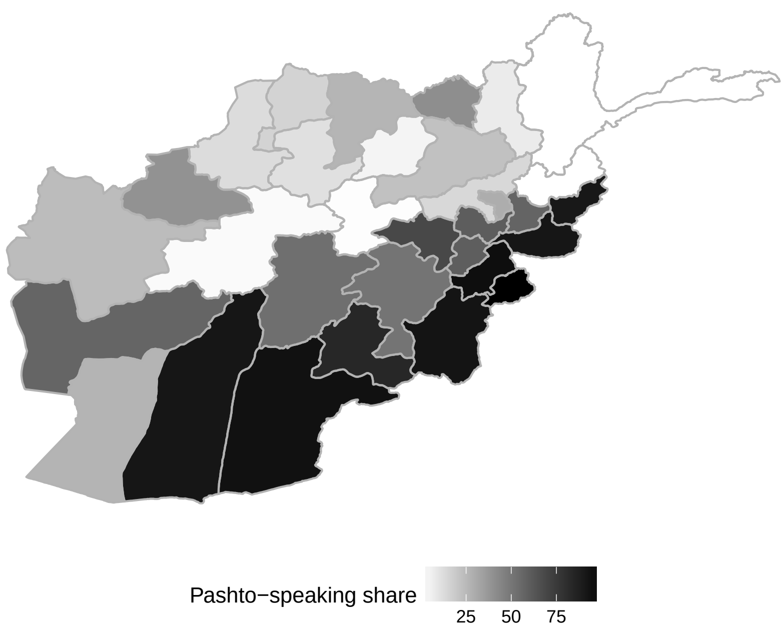


Figure 20: UNFPA and CSO (2007) provincial language share