

Did the Invasion of Afghanistan Deter Ungoverned Spaces? Not in Africa

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Abstract

Many of the world's poorest citizens live in peripheral spaces their states' have essentially chosen not to control. Leaving these spaces ungoverned is bad for residents' welfare and lets them to become hot beds for terrorist and other insurgent groups. Can the international community induce countries to invest in controlling these territories? In this paper, I consider the George W. Bush Administration's foreign policy, which, following the September 11th attacks, demanded countries take active steps to reduce terrorist safe havens or risk a US invasion. Building on recent work on the determinants of government control, I develop a difference-in-difference strategy to test for evidence of government expansions, and implement this test using subnational conflict data from Africa. Across a wide range of specifications and measures, I consistently find that African states *did not* engage in these expansions. The results suggest that deterrence is an ineffective policy strategy to reduce ungoverned space.

Keywords: Ungoverned spaces; Conflict; Deterrence; Africa; National Security

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“Terrorist groups like al Qaeda depend upon the aid or indifference of governments... Some governments still turn a blind eye to the terrorists, hoping the threat will pass them by. They are mistaken... We’re asking for a comprehensive commitment to this fight. We must unite in opposing all terrorists... Any government that rejects this principle... will know the consequences.”

– President George W. Bush (November 10, 2001)

1 Introduction

Academics and policymakers are increasingly concerned with “ungoverned spaces,” swaths of territory where formal governments have little or no influence or presence. This general concern is founded on two specific worries. First, ungoverned (or, perhaps more accurately, minimally governed) spaces have powerful negative impacts on the rest of the world by creating safe spaces for terrorists and other violent groups to operate. As recent years in Syria have demonstrated, these safe spaces can facilitate devastating attacks throughout the world. The second issue is that some degree of governance appears to be essential for development and improving citizen welfare. Much of what we see as development, from broad gains like education quality and financial markets to specific interventions like cash transfers and distribution of bed nets, requires some degree of stability and government authority. More fundamentally, even private market trading requires security and protection of property rights.

Increasingly, the importance of having a formal government presence is being appreciated. However, what can be done? In much of the world, governments make few investments in establishing this presence in peripheral, low-income, resource-poor territories. A growing literature shows that this investment is a rational choice that governments make based on the calculated costs and benefits of establishing control in a region. Clear incentives like agricultural productivity (Callen et al., 2015), potential mining revenues (Vanden Eynde, 2015), and risks of interstate conflict (Lee, 2018) drive states’ decision to invest in building authority. However, these responses offer little policy guidance. What can be done, in particular by Western states, to encourage the extension of governance?

This paper evaluates one of the most significant Western actions meant to incentivize governments to expand their authority: The 2001 invasion of Afghanistan. In the next section, I use statements from senior officials in the Bush administration to demonstrate two key points. First, the invasion of Afghanistan was not meant to punish the Taliban (the acting government of Afghanistan) for action or involvement in the September 11th attacks, but for *inaction* in failing to prevent al-Qaeda from planning the attack and failing to track down and capture senior organization members afterwards. These statements make clear that the Taliban was

never being held responsible for the attack, but for failing to prevent it and failing to act afterwards.¹ Second, the statements show that, from the earliest weeks of the invasion, the dialog was clearly focused on deterring other countries from allowing terrorists to operate within their borders. In other words, the message was always centered around encouraging weak states to establish control throughout their countries.

I ask whether this invasion, where the acting Afghan government was removed from power in a matter of weeks, effectively led sub-Saharan governments to push their authority and influence into previously ungoverned spaces. Specifically, following the 2001 invasion, is there evidence of increased government engagement in less-governed provinces, relative to more-extensively governed ones?² In answering this, there are two major challenges. First, one must determine which provinces were more or less extensively governed before the invasion. Second, one must observe a proxy for increased government engagement.

To determine which provinces were more or less likely to be directly governed before the invasion, I build on a large literature studying determinants of and correlates with government presence. For instance, as discussed above, agricultural productivity (Callen et al., 2015) and mining revenue (Vanden Eynde, 2015) both increase incentives for government engagement. Thus, I treat provinces with low pre-2001 agricultural productivity and without mines as those which were less likely to be directly governed. I then ask whether evidence of government engagement increased in these provinces, relative to others in the same country. In total, I use eight such proxies and extensive discussion of these proxies and the evidence behind them is presented in Section 3.

The second challenge is observing the expansion of government presence. To do this, I build on theoretical (Hirshleifer, 1989; Skaperdas, 1996) and empirical (Berman, Downey, and Felter, 2016; Crost and Felter, 2016; Kalyvas, 2006) evidence that violence rises when control of territory shifts. This notion, implicit in commonly used “contest functions” of conflict and supported by evidence from Northern Ireland and the Philippines, suggests that a territory that is unambiguously controlled by *either* rebels or the government tends to be peaceful. Instead, violence rises when the government tries to take control from rebels or vice versa. Thus, to determine whether governments try to expand authority into minimally governed territories following the 2001 invasion, I ask whether violence rises disproportionately in minimally governed regions.

With this strategy for measuring the extent of pre-2001 government presence and for ob-

¹Both of these failures may have stemmed from the Taliban’s ideological unwillingness to act, but the challenges in establishing governance since the invasion demonstrate the Taliban never *could* have prevented the attacks or captured al-Qaeda officials afterwards.

²Throughout the paper, I use “province” to refer to the first subnational administrative unit within each country.

serving expansions of authority, I use a difference-in-difference strategy (with and without propensity scores), multiple proxies for pre-2001 control, and a range of measures and sub-samples. Across a rich set of specifications, I consistently find no evidence that sub-Saharan African governments expanded authority in response to the invasion (and likewise no evidence for key sub-samples of countries). Parallel pre-trends, supporting the identification strategy, are matched by equally parallel post-trends. My estimates are fairly precise, and given the variety of empirical specifications employed, tell a consistent story that there was no effect. I conclude that military invasions do not deter ungoverned spaces.

Importantly, this was a dramatic example of intended deterrence. The Taliban was removed from power with overwhelming force weeks after the invasion began. Worldwide media focused on the invasion and its aftermath throughout, and the Bush administration’s emphasis on deterring other states from becoming terrorist “safe havens” was always part of the conversation (as the next section demonstrates). If this sort of intervention did not deter ungoverned spaces, then it is unlikely that any military action could.

The rest of the paper is organized as follows. Section 2 presents a range of excerpts from senior Bush administration officials to underpin two key elements of my argument: That the Taliban was being removed from power because of inaction rather than involvement in the September 11th attacks, and that deterrence was always a key focus of the administration. Section 3 summarizes evidence important for predicting and identifying ungoverned spaces. Section 4 summarizes the data and methods. Section 5 presents results, including a range of specifications and robustness checks, before Section 6 concludes.

2 Deterrence as Theme Following the Invasion

Here, I summarize statements by senior officials in the Bush administration in order to illustrate the following two points. First, the Taliban was removed from power because it failed to prevent al-Qaeda from launching the September 11th attack and failed to apprehend al-Qaeda and secure its territory afterwards. Second, from early on, policymakers repeatedly emphasized that other countries’ governments were at risk if they failed to prevent similar terrorist action. In other words, the administration was always consciously focused on raising other states’ perceptions of the costs of leaving space ungoverned.

The basic demands and expectations for the Taliban were made clear as early as September 21, 2001, when President George W. Bush said:

And tonight, the United States of America makes the following demands on the Taliban: Deliver to United States authorities all the leaders of al Qaeda who hide in your land... Protect foreign journalists, diplomats and aid workers in your

country. Close immediately and permanently every terrorist training camp in Afghanistan, and hand over every terrorist, and every person in their support structure, to appropriate authorities. Give the United States full access to terrorist training camps, so we can make sure they are no longer operating. These demands are not open to negotiation or discussion. The Taliban must act, and act immediately. They will hand over the terrorists, or they will share in their fate.

In short, then, these expectations included: 1) capturing al-Qaeda leadership, 2) protecting important foreign citizens, 3) closing terrorist training camps, and 4) dismantling terrorist organizations' infrastructure. These are strong demands, and they require a strong and well-functioning state. The statement demanded immediate action from the Taliban, but few would have considered the Taliban a sufficiently strong or well-functioning state to have implemented these demands, particularly given its weak presence in the areas where al-Qaeda operated.

The Taliban's response, within a week, was the focus of much media attention. The Taliban focused on the demand to turn over al-Qaeda leader Osama bin Laden, and refused to do so without additional evidence (and officials were divided over whether or not they would do so even in light of evidence). Popular discussion about this response, however, seems to miss the point. On September 28, President Bush made clear that bin Laden was only one element of US demands: "First, there is no negotiations with the Taliban. They heard what I said. And now they can act. And it's not just Mr. bin Laden that we expect to see and brought to justice; it's everybody associated with his organization that's in Afghanistan. And not only those directly associated with Mr. bin Laden, any terrorist that is housed and fed in Afghanistan needs to be handed over. And finally, we expect there to be complete destruction of terrorist camps." Regardless of the refusal to capture bin Laden, it is clear that the expectations placed on the Taliban were impossible for a weak government like theirs to satisfy.

Within two months, the Taliban was removed from power. That November, Secretary of State Colin Powell was explicit that this had nothing to do with their involvement in the attack, but their failure to help capture al-Qaeda leadership afterwards: "To get to terrorists, we had to go after the Taliban regime in Afghanistan that was protecting them. We warned them. We warned the Taliban to turn over Bin Laden or we would make them pay. They refused, and we have now made them pay. We have driven them from power."

In sum, then, the Taliban was removed from power for refusing to exercise the powers of an effective and legitimate government in Northern Afghanistan. From the earliest days, this inaction was emphasized and highlighted, and a clear, consistent message was sent out worldwide that other countries who failed to act would similarly be punished. On October 7, President Bush made this clear: "Today we focus on Afghanistan, but the battle is broader. Every nation

has a choice to make. In this conflict, there is no neutral ground. If any government sponsors the outlaws and killers of innocence, they have become outlaws and murderers themselves. And they will take that lonely path at their own peril.”

The emphasis on proactive suppression of terrorism was enshrined in a UN resolution in November, which President Bush summarized as follows:

In this war of terror, each of us must answer for what we have done **or what we have left undone... The time for action has now arrived.** The most basic obligations in this new conflict have already been defined by the United Nations... Every United Nations member has a responsibility to crack down on terrorist financing... We have a responsibility to share intelligence and coordinate the efforts of law enforcement... And when we find the terrorists, we must work together to bring them to justice. **We have a responsibility to deny any sanctuary, safe haven or transit to terrorists. Every known terrorist camp must be shut down, its operators apprehended** and evidence of their arrest presented to the United Nations. We have a responsibility to **deny weapons to terrorists and to actively prevent private citizens from providing them.** These obligations are urgent, and they are binding on every nation with a place in this chamber. (emphasis added)

These are demanding expectations which apply to all UN members. Leaving large swaths of territory ungoverned would certainly violate “deny[ing] any sanctuary, safe haven or transit to terrorists,” as well as “crack[ing] down on terrorist financing” since many organizations profit from illicit trade in these areas (like antiques dealing in Syria and heroin production in Afghanistan). In short, these expectations demanded that countries take accountability of their ungoverned spaces.

As early as September 2001, President Bush implied that the US was considering action in many other countries as well, saying “[al-Qaeda] and its leader - a person named Osama bin Laden - are linked to many other organizations in different countries, including the Egyptian Islamic Jihad and the Islamic Movement of Uzbekistan. There are thousands of these terrorists in more than 60 countries.” The global scope of this plan was reiterated a month later when the President said, “We are at the beginning of our efforts in Afghanistan, but Afghanistan is only the beginning of our efforts in the world. This war will not end until terrorists with global reach have been found and stopped and defeated.”

This global campaign had two important elements. The first was the demand for action. In November, President Bush said “All nations, if they want to fight terror, must do something. Over time it’s going to be important for nations to know they will be held accountable for inactivity. You’re either with us or against us in the fight against terror.”

The second was the willingness to carry out preventative strikes. Secretary of Defense Donald Rumsfeld was most clear about this, saying:

If a terrorist can attack at any time, in any place, and using any technique, and it's physically impossible to defend in every place, at every time against every technique, then one needs to calibrate the definition of "defensive." ...[T]he only defense is to take the effort to find those global networks and to deal with them as the United States did in Afghanistan. Now is that defensive or is it offensive? I personally think of it as defensive... All one has to do is read the intelligence information to know that there are a good number of people who have been well trained. They are well financed. They are located in 40 or 50 countries. And they are determined to attack the values and the interests and the peace and the way of life of the people that are represented in the North Atlantic Treaty Organization nations... But, clearly, every nation has the right of self-defense and this is the only, only conceivable way for us to defend ourselves against those kinds of threats.

These two features (the willingness to pursue and combat terrorism anywhere and the openness to doing so preemptively before a terrorist attack occurs) came to be known as the "Bush doctrine."³ This doctrine was always focused on deterring terrorist sanctuaries and safe havens. In the June, 2003, words of Vice President Richard Cheney, "If there is anyone in the world today who doubts the seriousness of the Bush Doctrine, I would urge that person to consider the fate of the Taliban in Afghanistan, and of Saddam Hussein's regime in Iraq... Before 9-11, all too many nations tended to draw a distinction between the terrorist groups and the states that provided these groups with support... The distinction between the terrorists and their sponsors should no longer stand."

In conclusion, then, all senior administration officials have emphasized the three key themes. The first is the importance of action, and the willingness of the administration to punish governments for failing to act against terrorists. The second is the global scope of the war on terror. Finally, the administration emphasized its willingness to act *before* a terrorist attack had taken place. Together, these very explicit warnings were meant to raise the perceived costs of leaving swaths of open space where terrorists could train and flourish. In the rest of the paper, I test whether these warnings induced action.

³This term was used primarily by media commentators, but occasionally by the administration. It was never formally defined.

3 Measuring the Extent of Government Influence

In this section, I discuss the observable characteristics that help determine which provinces were more (and less) likely to be governed prior to 2001. As discussed in the methods section (Section 4), this facilitates a difference-in-difference design comparing subsequent violence between these two types of regions. Given evidence that violence rises as states try to capture space from rebels (Croft and Felter, 2016; Kalyvas, 2006), this violence increase signals an expansion of the state.

For simplicity, the proxies for government presence discussed here are divided into two sections. First, I discuss factors that reduce the costs or increase the benefits of establishing government presence. These, in large part, are drawn from the literature estimating the causal effects of these factors on the rational decision of states as to where to govern. Second, I discuss factors shown to be correlated with government presence (without causal claims or, in some cases, because they are reduced by government presence). Here, I discuss only the evidence behind the proxies. Data sources and measurement strategies are discussed in Section 4.

3.1 Determinants of the Decision to Govern

Terrain ruggedness. Rugged terrain (that with high-frequency changes in elevation) is indicative of expansive caves and mountains which afford rebels the ability to hide and build capacity without government monitoring. For instance, al-Qaeda famously operated out of a huge network of caves. Using a panel of 161 countries Fearon and Laitin (2003) show that rugged terrain raises the probability of civil war, and argue this is by affording insurgents opportunities to hide from the government.

At the same time, in addition to creating opportunities for rebels to hide, Nunn and Puga (2012) show that terrain ruggedness reduces agricultural productivity. Using the Pakistan government's explicit demarcation of spaces to be left ungoverned (the Frontier Crimes Regulation) and increased agricultural productivity from the Green Revolution, Callen et al. (2015) show causal evidence that agricultural productivity raises the government's willingness to establish formal governance and institutions. Theory and anecdotal evidence suggest this is due to the increased benefits of having a formal presence. Thus, one would expect more rugged terrain to be less likely to be governed, prior to the 2001 invasion.

Mineral resources. Mineral resources provide a source of wealth and, involving fairly centralized production, are relatively easy to tax. Thus, there is a strong incentive for governments to control mining regions. How do governments respond to the presence of minerals? Vanden Eynde (2015) uses a unique reform in India which increased state governments' legal ability to tax mineral revenue (differentially for different minerals). He finds that state governments,

which are responsible for the majority of counterinsurgency activity in India, are more likely to invest in controlling areas where mining revenues rise. This suggests that mining revenues are an effective incentive for government control. Interestingly, a larger literature has shown that large resource discoveries (especially oil) often induce conflict, particularly in the presence of weak institutions.⁴ Given the evidence that conflict and violence arise from the contest to establish control (Kalyvas, 2006; Crost and Felter, 2016), this supports an interpretation in which governments are more likely to seek control over areas with potential natural resource revenue.

Border provinces. States have an obvious incentive to avoid conflict, which is both costly and risky. Recently, Lee (2018) shows that this incentive manifests in states' decision to govern their border provinces, particularly those that border hostile neighboring states. She develops and validates a novel measure of state capacity based on implausible statistical anomalies in government-collected Census data, and shows within-country evidence of lower capacity in provinces bordering hostile neighbors. The mechanism appears to be the desire to avoid confusion over fluid national boundaries, the risk that the neighboring state will undermine government activities, and the challenge of battling insurgent groups when they can draw on support from a hostile neighbor.

3.2 Correlates of Government Presence

Remoteness. Broadly speaking, "remoteness" is a feature of geography that implies sparsely populated areas far from the national capital or other major cities.⁵ Condra (2015) shows that ethnic groups native to remote regions are more likely to fight for autonomy. Asher, Nagpal, and Novosad (2017) shows that providing public goods is more difficult in more remote districts, and, as a result, there is less government activity there. Generally, Tollefsen and Buhaug (2015) show that geographic variables like these are the most robust predictors of insurgency and civil war and they discuss a number of potential explanations.

Infant mortality. Most causes of infant mortality are preventable. Many of these are associated with state provision of public goods like health services (Rutstein, 2000). For this reason, infant mortality is a widely used and recommended measure of state capacity, as it correlates with other forms of state failure (Goldstone et al., 2000; Gurr et al., 1999; Lee, 2018).

Child malnutrition. Berman et al. (2016) study the Philippines government's attempt to expand governance through a targeted counterinsurgency program. They find that this expan-

⁴See Lei and Michaels (2014) for recent evidence and a summary of the literature. See Cotet and Tsui (2013) for a more critical review.

⁵One would expect this to be correlated with terrain ruggedness, which can be a reason why major population centers never formed, and border province status, although African capitals and major cities often lie along borders. I consider it a distinct, but closely related, concept.

sion of governance reduced child malnutrition, and provide evidence that this is a result of the security and institutions that come with government control, as the initiative displaced rebels and malnutrition increased in neighboring municipalities. Thus, there is some evidence that child malnutrition is lower where the government is better established. This is unsurprising, in light of the large range of health services that governments provide.

World bank projects. One reason why ungoverned spaces are concerning is because they are often too dangerous for the state or international organizations to provide services to their (overwhelmingly poor) populations. Crost, Felter, and Johnston (2014) use a discontinuity in the rule the World Bank used to award a community development block grant, and show that awarding these grants and initiating projects in poorly governed spaces induces an increase in violence. This violence response often leads the projects to be canceled. Thus, successful completion of a World Bank project is an indicator of some degree of government control.

4 Data and Methods

4.1 Sample

The main sample is based on a set of 50 African countries. For each, I use the Global Administrative Areas (GADM) database of administrative boundaries (Hijmans et al., 2015) for a time-invariant set of geographic boundaries. I focus on the first subnational administrative unit (e.g., “state” in the United States, “province” in Canada), which I refer to as provinces.

Population data is from the Oak Ridge National Library’s LandScan dataset, which estimates population at roughly a 1km resolution. From this, I calculate population of provinces using the shapefiles from GADM. The remainder of data sources are discussed below.

4.2 Violence data

My primary specification relies on violence data from the Armed Conflict Location and Event Data (ACLED) Project (Raleigh et al., 2010). ACLED seeks to create a comprehensive dataset of political violence in Africa and elsewhere. Information is drawn from news reports, and coders identify a range of event details, including the actors involved and the location (as precisely as possible), which I use to place events within provinces. ACLED data is available for all African countries from 1997 onward.

My primary specification uses the number of events, as is standard in the subnational conflict literature (Berman, Downey, and Felter, 2016; Berman, Shapiro, and Felter, 2011; Crost and Felter, 2016; Crost, Felter, and Johnston, 2014). To ease interpretability, I standardize this measure to have standard deviation 1. However, in robustness checks in Section 5.2, I consider

other measures (an indicator for any event, a count of deaths, and a per capitized count of events), none of which change the conclusions.

Because my interest is in government expansions of control, I focus on events involving the government. Specifically, my primary specification uses events in which the military engaged rebels, militias, or civilians.⁶ However, in Section 5.2 I show that using all events has no effect on the conclusions.

I also use data from the Uppsala Conflict Data Program (UCDP), one of the most historic collectors of conflict data. Specifically, I use the UCDP’s Georeferenced Event Dataset (Sundberg and Melander, 2013), which codes conflict events in a similar manner to ACLED and similarly makes it possible to include government-involved activity⁷ and calculate subnational conflict. The main differences are that the UCDP only codes events 1) in which at least one death occurred, and 2) involving a dyad that was engaged in substantial conflict during the given year or an adjacent year.⁸ This means that a large amount of government activity attempting to expand the state will go unmeasured. Thus, my preferred specification uses ACLED data, but Section 5.2 shows that the results are robust to UCDP data.⁹

4.3 Measures

I develop a total of eight measures to represent the proxies for “ungoverned” (or, more accurately, less governed) spaces that are described in Section 3.

Sparseness. I use the log of the inverse population density (area per population), which captures one dimension of remoteness. To avoid endogenous population changes, I use the year 2000 population.

Distance to the capital. Another dimension of remoteness is distance to the capital, and I use the log distance between the capital city and the province’s centroid.

Terrain Ruggedness Index. I use the Terrain Ruggedness Index (TRI) introduced by Riley, Degloria, and Elliot (1999) to study wildlife habits, and popularized in social science by Nunn and Puga (2012). The TRI essentially measures high-frequency changes in elevation (suggesting rocky, mountainous terrain). On his website, Diego Puga provides elevation data for a grid of 30 arc-second cells, and I use this data to create average TRI within each province.

Malnutrition. I use the World Bank’s Subnational Malnutrition Database to estimate

⁶ACLED Interaction codes 12-17. While the conceptual distinction between these groups is clear, in a given event, it can be difficult to precisely discern them.

⁷Specifically, I use events between the government (SideA includes government) and non-state actors (TypeOfViolence equal one, SideB does not include government) or civilians (TypeOfViolence equal 3).

⁸Specifically, if a dyad (pair of actors) engage in conflict causing 25 or more battle-deaths in a year, then the UCDP data includes all events between those two actors during that year, the prior year, and the next year.

⁹For a debate about the merits of the two datasets see Eck (2012) or Kishi (2016).

province-level malnutrition for the latest available pre-2001 year (which is overwhelmingly 2000). My primary specification uses the fraction of children who are underweight.

Infant Mortality. I use the subnational infant mortality data from Storeygard et al. (2008), which primarily pertains to the year 2000.

No minerals. I use an indicator for provinces which lack mines or oil or gas fields. For oil and gas fields, I use the American Association of Petroleum Geologists’ Giant Oil and Gas Fields of the World data.¹⁰ For mines, I use the US Geological Survey’s Mineral Resource Data System. Both provide longitudes and latitudes that allow me to place mines and fields within provinces.

Border province. Using GADM shapefiles, I code provinces as border provinces if they share a border with another country.

No World Bank Projects. Researchers at AidData at the College of William and Mary have geocoded every project approved by the World Bank’s IBRD and IDA programs from 1995 on. I map these projects to provinces, and use an indicator for whether any project began prior to September, 2001.

For all continuous measures (the first five of eight), I standardize the variable to have standard deviation of one, which facilitates interpretation.

4.4 Methods

I am interested in whether African governments expanded their control after the October, 2001, invasion of Afghanistan. Because violence rises when governments seek to control previously rebel-controlled space, it is equivalent to ask whether violence disproportionately increased in ungoverned territory following the invasion. For this question, a difference-in-difference approach is ideal. Specifically, my primary specification is:

$$GovVio_{p,t} = \alpha_p + \delta_{c(p),t} + \sum_{\tau \neq t_0} \beta_\tau (Ungoverned_i \times 1\{t = \tau\}) + \varepsilon_{p,t} \quad (1)$$

where p indexes provinces, $c(p)$ indexes the country of the province, t indexes time, and $1\{\cdot\}$ is the indicator function. $GovVio_{p,t}$ is one of the measures of government-involved violence described in 4.2 and $Ungoverned_i$ is one of the proxies for ungoverned (or less likely to be governed) space described in 4.3, all of which are either time invariant or are measured before September 2001 (pre-determined).

The province fixed effects (α_p) account for the possibility that violence is always higher in

¹⁰This data is created by former AAPG president Mike Horn and used in Arezki, Ramey, and Sheng (2017) and Lei and Michaels (2014).

ungoverned provinces. The country-by-time fixed effects ($\delta_{c(p),t}$) ensure that provinces are being compared within country, always to other provinces in the same country at the same time.¹¹ The β_τ coefficients trace out the level of violence in province p , relative to the same province at some baseline time period t_0 (first difference) and other provinces in the same country at the same time (second difference).

The β_τ coefficients for $\tau < t_0$ allow us to observe pre-trends in violence. If violence is stable in ungoverned provinces (relative to other provinces) during the time leading up to the invasion, then these β_τ coefficients should be near zero. If they are not near zero (for instance, β_τ is increasing over time during the lead up to t_0), then one should be concerned about whether differences *after* t_0 are really the result of the invasion or a continuation (or reversal) of a pre-existing trend. Assuming that the coefficients β_τ for $\tau < t_0$ are (roughly) zero, then the β_τ coefficients for $\tau > t_0$ trace out the disproportionate rise in violence after the invasion, which can be interpreted as the casual effect of the Afghan invasion on violence. If African governments attempted to expand their sphere of control in response to the invasion then we would expect these β_τ coefficients to be positive.

Below, I estimate this primary specification at both the quarterly level (where July-September, 2001, is the omitted pre-period t_0) and the yearly level (where all of 2001 is the omitted period). As expected, the yearly estimates have smaller standard errors, and the quarterly estimates show that there is no short-run, high frequency response that is obscured by the yearly estimates.

The results below, by and large, show little evidence of pre-trends. However, to further ensure comparability of “treatment” and “control” provinces, I implement a propensity score weighting scheme. I estimate propensity scores in which I use violence levels from the four years (or 18 quarters, when estimating quarterly regressions) *before* the invasion to predict $Ungoverned_i$. I then restrict to the common support of these scores, create weights from them, and use those weights in estimating my primary specification.¹² This method of identifying the

¹¹This is important because many of the proxies are inherently relative. For instance, larger countries will always have larger average distance to the capital, but the country-by-time fixed effects ensure that identification is always being driven by differences between provinces within the same country.

¹²Formally, this procedure is as follows. For continuous proxies for ungoverned, I create a binary indicator for whether each province is above the sample mean (for proxies that are already binary, this is unnecessary). I then estimate a conditional logistic regression, predicting this binary and time-invariant variable using country fixed effects and $GovVio_t$ for all $t < t_0$. That is, I use the pre-period violence to predict whether a province is likely to be ungoverned. Letting $\hat{p} \equiv \hat{Pr}(ungov \mid GovVio_{t < t_0})$ denote this predicted probability, I create weights as $1/\hat{p}$ for the “treatment” group (ungoverned equal to one) and $1/(1 - \hat{p})$ for the “control” group. I also restrict to the common support by calculating the 1st and 99th percentiles of the \hat{p} distribution separately for the treatment and control group, and only including observations for which \hat{p} is between both distributions’ 1st and both distributions’ 99th percentiles. Researchers commonly use the minimum and maximum (rather than these percentiles), but I find that this produces balance much more effectively. I use propensity score weighting instead of match because Busso, DiNardo, and McCrary (2014) show that it often performs better in

control group based on similar pre-trends is similar in spirit to the original implementation of synthetic control methods used to estimate the effects of the Basque conflict on GDP (Abadie and Gardeazabal, 2003), but is better suited for cases with a large number of “treated” units.

5 Results

5.1 Main results

The main results are best seen in Figure 1, which plots estimated coefficients from the quarterly and annual regressions, both with and without propensity weights to balance pre-trends. (For visual simplicity, confidence intervals are presented only for the unweighted regressions, but the standard errors are substantively similar for the propensity weighted regressions.) Because violence has been scaled to have standard deviation of one, the coefficients can be interpreted as the differential violence increase (in units of standard deviations) resulting from a 1-unit increase in the given “ungoverned” proxy. Because the continuous proxies (panels (a)-(e)) have also been scaled, 1-unit means a 1 standard deviation increase in those proxies.

The pre-trends are generally flat and not different from zero. Deviations are often small and usually (e.g., distance from the capital) though not always (e.g., malnutrition) fixed by the propensity weights.¹³ Regardless of the method, the estimated post-invasion coefficients are generally near zero. Particularly in the years immediately following 2001, non-zero estimates are typically negative, suggesting, if anything, less government involvement in ungoverned spaces. When the propensity weights improve the pre-trends, they usually bring the estimated effects closer to zero.

The standard errors rarely reject zero, and are often fairly precise. For instance, the specifications estimated with yearly data (and no weights) can often rule out effects larger than .1 standard deviations in the years immediately following, and can almost always rule out effects larger than .2 standard deviations. The only evidence of increased violence comes from select measures (e.g., terrain ruggedness) and begins only 5 years later. This can hardly be considered compelling evidence for Bush Doctrine effectiveness.

[Figure 1 about here.]

Table 1 presents coefficients for the yearly regressions without weights. In addition to the full set of estimated coefficients, the bottom of the Table presents a series of F -tests for the finite samples. Note to PacDev reviewers: Future iterations will use the Generalized Propensity Score (Hirano and Imbens, 2004) rather than dichotomizing continuous proxies.

¹³Malnutrition and infant mortality are both measured in 2000, and there is a corresponding spike during the previous year, because idiosyncratic shocks in violence raise next year’s malnutrition and infant mortality. This appears as a “pre-trend,” but is rather an artifact of the timing of measurement.

sums of various coefficients (all pre- coefficients, two-year pre-, two-years post-, and all post-coefficients).

Consistent with the Figure, the pre-2001 coefficients are almost never significant (individually or in total), suggesting that violence in less governed provinces was progressing fairly evenly with more governed during the years leading up to 2001. Likewise, the post-2001 coefficients usually are not statistically different from zero. When they are, the estimates are *negative*, inconsistent with the expectation of increased violence as governments expand control. The weighted regressions are not substantively different, with the exception of the malnutrition specification, which shows a marginally significant increase in violence immediately following (though note from Figure 1 that this specification does a poor job controlling for pre-trends).

[Table 1 about here.]

5.2 Robustness

I claim that there is little evidence to support the idea, central to the Bush Administration’s rhetoric following the Afghan invasion, that states would make an effort to control their ungoverned territories. I base this conclusion on a set of eight proxies drawn from the relevant literature. My primary results flexibly and transparently present evidence from four different specifications, for a total of 32 regressions. However, there are, of course, a large number of *other* regressions that could have been run, and in attempting to establish a null finding the burden is on the author to be exhaustive.

Table 1 presents a variety of additional robustness checks. For simplicity, I present only coefficients from yearly, unweighted regressions where I estimate a joint effect for the two immediately following years (2002 and 2003). In other words, I present only the single coefficient on the differential increase in violence during those two years. The full results are available upon request.

First, I consider different measures of government-involved violence using the ACLED data. Row 1 presents my baseline specification (the total count of events). In rows 2-4, I use a binary indicator for whether there was any event, the number of deaths, and the number of events per capita. In row 5, I use any ACLED event, rather than only those involving the government. None of these change the conclusions. In row 6, I relax the assumption that “ungoverned” is linear in the proxy, and present results using a dichotomous measure (for above vs. below average). In rows 7-11, I present parallel results using the UCDP data, and again find no effect. With the UCDP data it is also possible to include a longer time-frame, and so in row 12 I extend the sample back to 1992 in row 12.

Finally, I consider specific sub-samples. In row 13, I use the ACLED data and consider only conflict-prone countries (defined as those experiencing a conflict with 25+ battle deaths during at least one year in the 1990's). Even in this sub-sample (where instability is exacerbated), there is no effect.

Finally, the War on Terror was in response to an Islamic terrorist group, and much of the subsequent rhetoric centered on religion and radical Islam. Thus, in rows 14 and 15 I consider the set of countries with large Muslim populations (40% or more of the population, roughly the mean) and growing Muslim populations, and again find no results.

[Table 2 about here.]

Taken together, the results above suggest that my findings are not specific to one particular measure, specification, or sample, but hold broadly.

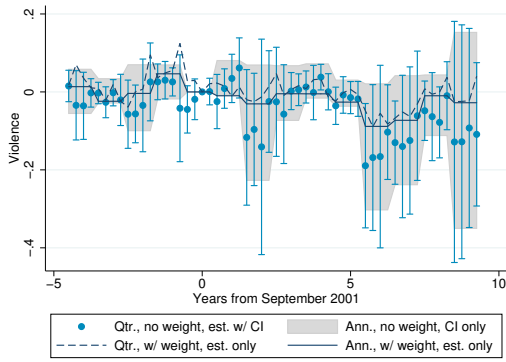
Note to PacDev reviewers: I have also checked variations on the ungoverned proxies (e.g., only large mines, only valuable minerals, etc.) and the results are robust. I have not yet had a chance to write these results up.

6 Conclusions

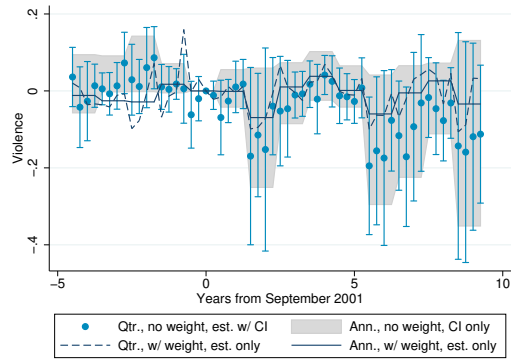
Deterrence remains one of the most hotly contested issues in international policy. It's role in shaping a wide range of behavior has been extensively discussed. Here, I consider whether deterrence was effective in incentivizing African governments to expand their states' control following the 2001 invasion of Afghanistan. Given the critical role of ungoverned spaces in both the development and international security landscapes today, I believe this is an important question.

I find no evidence that African governments pushed into their countries peripheries following the invasion. This result is robust to a host of different empirical specifications. This finding is not merely of academic interest, but given the role of deterrence in the Bush Administration's rhetoric (and therefore its likely role in their policy calculus), it has direct applications for future foreign policy.

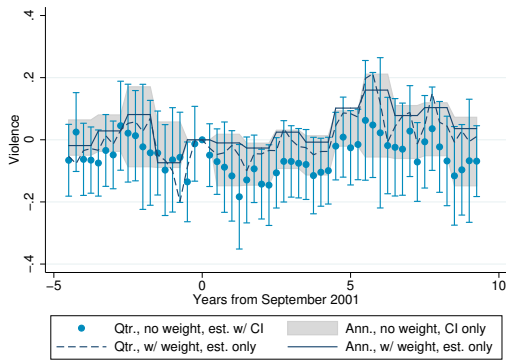
Figure 1: Main results



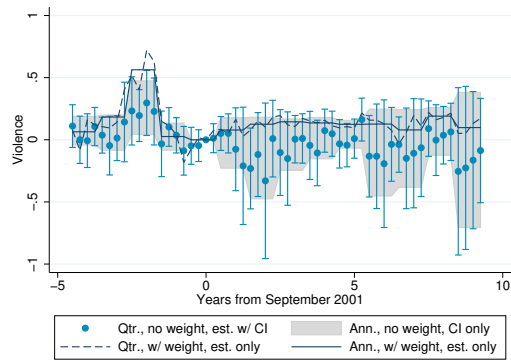
(a) Log(Sparseness)



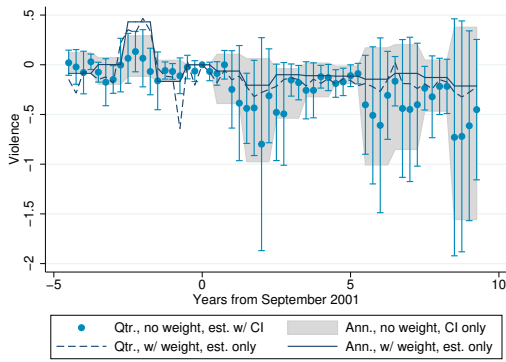
(b) Log(Dist. from Capital)



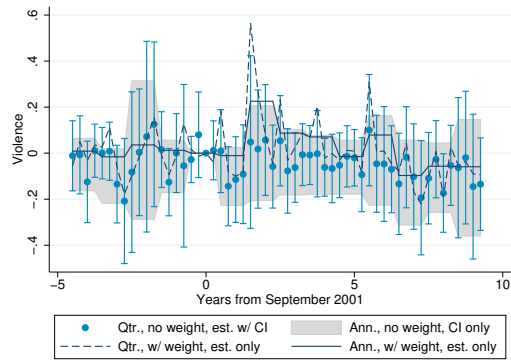
(c) Terrain Ruggedness Index



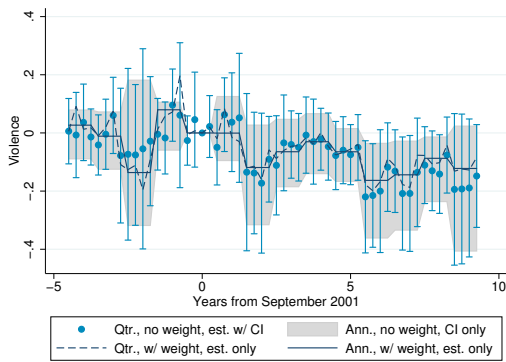
(d) Pre-2001 Child Malnutrition



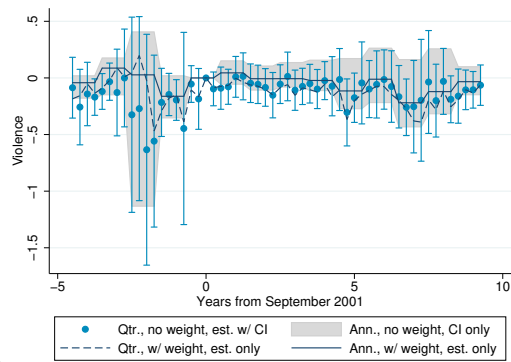
(e) Pre-2001 Infant Mortality



(f) No Mines or Oil Fields



(g) Border Province



(h) No pre-2001 World Bank Projects

Table 1: No evidence of increased government action in response to invasion

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
X_i	$\ln(\text{Sparse.})$	$\ln(\text{KM to Cap.})$	TRI	Malnut.	Inf. Mort.	No minerals	Border Prov.	No WB Proj.
$X_i \times d_{1997}$	0.001 (0.029)	0.019 (0.039)	0.007 (0.029)	0.073 (0.058)	0.026 (0.048)	-0.049 (0.058)	-0.005 (0.043)	-0.080 (0.050)
$X_i \times d_{1998}$	0.003 (0.016)	0.044* (0.024)	0.021 (0.030)	0.058 (0.072)	-0.062 (0.068)	-0.100* (0.060)	-0.026 (0.050)	0.014 (0.083)
$X_i \times d_{1999}$	-0.015 (0.043)	0.070* (0.037)	0.042 (0.066)	0.259** (0.110)	0.087 (0.104)	0.013 (0.153)	-0.068 (0.127)	-0.363 (0.393)
$X_i \times d_{2000}$	0.026 (0.022)	0.033* (0.019)	-0.016 (0.037)	0.025 (0.055)	-0.061 (0.054)	-0.057 (0.058)	0.023 (0.042)	-0.167* (0.095)
$X_i \times d_{2002}$	0.036 (0.023)	0.007 (0.025)	-0.065 (0.042)	-0.025 (0.104)	-0.142 (0.126)	-0.101 (0.064)	0.015 (0.056)	0.048 (0.053)
$X_i \times d_{2003}$	-0.079 (0.075)	-0.096 (0.079)	-0.078** (0.034)	-0.147 (0.167)	-0.457* (0.264)	0.000 (0.105)	-0.144* (0.087)	-0.001 (0.054)
$X_i \times d_{2004}$	-0.003 (0.036)	-0.006 (0.040)	-0.031 (0.031)	-0.039 (0.106)	-0.288** (0.127)	-0.039 (0.073)	-0.069 (0.059)	0.028 (0.055)
$X_i \times d_{2005}$	0.028 (0.022)	0.039 (0.032)	-0.050 (0.032)	0.013 (0.085)	-0.152* (0.086)	-0.051 (0.065)	-0.037 (0.053)	0.022 (0.068)
$X_i \times d_{2006}$	-0.003 (0.017)	0.012 (0.027)	0.036 (0.029)	0.045 (0.057)	-0.102** (0.045)	-0.061 (0.060)	-0.076 (0.046)	-0.050 (0.113)
$X_i \times d_{2007}$	-0.141* (0.082)	-0.127 (0.086)	0.078 (0.068)	-0.103 (0.178)	-0.419 (0.300)	-0.032 (0.099)	-0.199** (0.083)	0.022 (0.123)
$X_i \times d_{2008}$	-0.098 (0.071)	-0.080 (0.074)	0.025 (0.043)	-0.070 (0.160)	-0.325 (0.269)	-0.128 (0.094)	-0.181** (0.078)	-0.135 (0.152)
$X_i \times d_{2009}$	-0.034 (0.040)	-0.020 (0.047)	0.034 (0.043)	0.068 (0.098)	-0.210 (0.131)	-0.107 (0.077)	-0.125** (0.057)	-0.030 (0.146)
$X_i \times d_{2010}$	-0.099 (0.128)	-0.110 (0.123)	-0.038 (0.056)	-0.162 (0.277)	-0.590 (0.494)	-0.107 (0.129)	-0.191* (0.110)	-0.025 (0.063)
N	42784	42784	42784	42728	42728	42784	42784	42784
R^2	0.354	0.355	0.353	0.354	0.358	0.352	0.353	0.353
<i>F</i> -test <i>p</i> -values for cumulative effects:								
$p : \sum_{t=1997}^{2000} d_t = 0$	0.841	0.074	0.655	0.065	0.961	0.394	0.680	0.158
$p : \sum_{t=2000}^{1999} d_t = 0$	0.833	0.029	0.753	0.032	0.837	0.794	0.735	0.179
$p : \sum_{t=2003}^{2002} d_t = 0$	0.605	0.323	0.041	0.446	0.050	0.507	0.312	0.642
$p : \sum_{t=2010}^{2002} d_t = 0$	0.352	0.394	0.769	0.671	0.072	0.335	0.051	0.861

* $p < .10$, ** $p < .05$, *** $p < .01$. Unit of observation is a province-year. Dependent variable is average quarterly government-initiated combat events. All specifications include province and country-by-year fixed effects. Standard errors clustered at the province level. Sample includes 48 countries.

Table 2: Robustness to changes in sample and outcome variable

Change	(1) <i>ln</i> (Sparse.)	(2) <i>ln</i> (KM to Cap.)	(3) TRI	(4) Malnut.	(5) Inf. Mort.	(6) No minerals	(7) Border Prov.	(8) No WB Proj.
Baseline	-0.022 (0.042)	-0.045 (0.045)	-0.072** (0.035)	-0.086 (0.113)	-0.300** (0.152)	-0.051 (0.076)	-0.065 (0.064)	0.023 (0.050)
Any event	0.011 (0.007)	0.004 (0.007)	-0.038*** (0.013)	0.029* (0.017)	0.007 (0.020)	-0.023 (0.016)	-0.010 (0.016)	-0.000 (0.029)
Deaths	-0.005 (0.008)	-0.004 (0.007)	-0.002 (0.006)	0.016 (0.011)	-0.009 (0.013)	-0.029 (0.020)	-0.009 (0.015)	-0.037 (0.032)
Per capita	0.037 (0.037)	0.005 (0.018)	0.000 (0.024)	-0.020 (0.044)	-0.096* (0.055)	0.004 (0.039)	-0.038 (0.037)	-0.075 (0.099)
All vio.	-0.034 (0.048)	-0.049 (0.049)	-0.033 (0.030)	-0.137 (0.129)	-0.343* (0.190)	-0.065 (0.065)	-0.076 (0.063)	0.019 (0.056)
Binary indep. var.	-0.085 (0.083)	-0.017 (0.062)	-0.133* (0.074)	-0.081 (0.092)	-0.159 (0.115)	-0.051 (0.076)	-0.065 (0.064)	0.023 (0.050)
N	42784	42784	42784	42728	42728	42784	42784	42784
Baseline (UCDP)	-0.002 (0.025)	-0.017 (0.030)	0.028 (0.037)	-0.093 (0.064)	-0.111 (0.070)	-0.090 (0.063)	-0.007 (0.042)	0.038 (0.047)
Any event (UCDP)	-0.006 (0.010)	0.006 (0.011)	0.021 (0.015)	0.016 (0.021)	-0.003 (0.025)	-0.002 (0.026)	-0.015 (0.019)	0.008 (0.024)
Deaths (UCDP)	0.000 (0.027)	-0.014 (0.029)	0.003 (0.017)	-0.017 (0.032)	-0.069* (0.039)	0.032 (0.069)	-0.007 (0.041)	-0.013 (0.037)
Per capita (UCDP)	-0.013 (0.027)	-0.011 (0.015)	-0.011 (0.017)	-0.064** (0.031)	-0.052 (0.033)	-0.029 (0.038)	0.003 (0.028)	0.059 (0.060)
All vio. (UCDP)	0.001 (0.022)	-0.010 (0.028)	0.018 (0.033)	-0.102 (0.063)	-0.116 (0.072)	-0.104* (0.063)	-0.006 (0.043)	0.003 (0.068)
N	21300	21300	21300	21300	21300	21300	21300	21300
1992-2010	-0.001 (0.026)	-0.016 (0.032)	0.029 (0.039)	-0.098 (0.068)	-0.121 (0.074)	-0.094 (0.067)	-0.005 (0.045)	0.042 (0.050)
N	28844	28844	28844	28844	28844	28844	28844	28844
War in 1990s	-0.024 (0.046)	-0.054 (0.053)	-0.081** (0.039)	-0.095 (0.118)	-0.345** (0.170)	-0.054 (0.085)	-0.076 (0.073)	0.024 (0.055)
N	36456	36456	36456	36456	36456	36456	36456	36456
Muslim countries	-0.034 (0.060)	-0.039 (0.069)	-0.032 (0.023)	-0.151 (0.207)	-0.459 (0.316)	0.030 (0.052)	-0.049 (0.056)	-0.026 (0.032)
N	18200	18200	18200	18200	18200	18200	18200	18200
Islam growing	0.046* (0.026)	0.033 (0.026)	-0.111* (0.060)	-0.023 (0.121)	-0.212 (0.148)	-0.083 (0.074)	0.019 (0.062)	0.046 (0.069)
N	27160	27160	27160	27104	27104	27160	27160	27160

* $p < .10$, ** $p < .05$, *** $p < .01$. Unit of observation is a province-year. All specifications include province and country-by-year fixed effects. Standard errors clustered at the province level.

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