

Courting the Civilians during Conflict: Evidence from Taliban Judges in Afghanistan

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Abstract

Rebel organizations regularly provide public services, even as they primarily focus on fighting. Existing scholarship documents many predictors of insurgent services, but the theoretical mechanisms for, and downstream effects of, these activities remain unclear. This study examines Taliban courts in Afghanistan, theorizing that judicial services create a vested interest in Taliban rule and show governance capacity. We find that Taliban courts significantly reduced the frequency of major interpersonal disputes, especially around property, in districts where they operated. We find a corresponding reduction in citizen willingness to use government courts, and higher approval for Taliban rule. Lastly, the Taliban were able to conduct increased bombings and other attacks against government and foreign troops after they introduced local courts. The results indicate that competent rebel courts can significantly sway public opinion and enhance rebel fighting capacity. These findings also help to explain the Taliban's rapid takeover of Afghanistan in the wake of foreign withdrawal.

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1 Introduction

Rebels regularly provide public goods to civilians during conflict, with a goal of boosting productive capacity or winning supporters (Arjona, 2016; Stewart, 2018; Loyle, 2021). Although scholars have documented many predictors of and explanations for rebel service provision (Mampilly, 2012; Revkin, 2020; Stewart, 2020), the consequences of such programs remain largely unclear. Despite rich theoretical exploration of rebel services (Mampilly, 2012; Arjona, 2016; Huang, 2016; Stewart, 2018), microlevel evidence of their impacts and adjudication between causal mechanisms has been difficult to obtain yet remains important for understanding conflict more generally (Balcells and Justino, 2014).

We argue rebel courts can sway civilians to support insurgents through several mechanisms: legitimization of the rebellion, facilitating coercive control of the population, or by creating a vested interest in continued rebel presence. By promoting civilian collaboration with the insurgency, rebels can use courts as a means of advancing their position in the struggle over the flow of information: understanding where, when, and how the state operates in a given conflict. Winning the battle over information can enable armed opposition of the state to anticipate military operations, coordinate attacks with more precision, and engage in varied and intense violence.

We examine a common type of rebel service provision – judicial services – in the context of the War in Afghanistan. The Taliban implemented a system of mobile courts, whereby judges were rotated within districts to resolve major interpersonal, familial, and domestic legal issues that state courts and traditional dispute resolution were unable to resolve either due to cost, accessibility, or lack of enforcement capacity. The Taliban’s mobile court system was a “spot service” common among rebels who wish to provide services but lack territorial dominance (Loyle et al., 2021).

We use new data on the locations and timing of Taliban courts in Afghanistan in the early 2010s to evaluate the impact of judicial service provision. We exploit the time-series cross-sectional variation in district exposure to Taliban courts to estimate their effect on civilian attitudes and insurgent attacks. Our econometric design reweighs districts never exposed to courts to interpolate the counterfactual average using a trajectory balancing design (Hazlett and Xu, 2018; Hazlett, 2020).

We find Taliban courts impact public opinion in the short-run. We estimate a 15% decline in willingness to use state courts to resolve disputes after Taliban courts are introduced, and a 23% increase in support for the Taliban returning to power. Several studies have documented that exposure to rebel governance matters for civilian attitudes after conflict ends (Breslawski, 2023; Daly, 2016). We expand on these findings by showing how courting civilians impacts public opinion *during* conflict.

Further, we show changes in rebel services have effects on the battlefield. The Taliban increases direct fire and IED attacks significantly after courts are introduced, which leads to more coalition casualties. Consistent with the idea that shifts in public opinion facilitate more insurgent attacks by encouraging civilian collaboration with the rebels, we show a positive correlation between using government courts and reporting of IEDs to the coalition. Whereas we find civilian’s willingness to collaborate through information sharing is different conditional on exposure to insurgent courts, we do not find any changes in recruitment, suggesting that intelligence sharing is perhaps the key pathway connecting collaboration to conflict after courts are introduced.

Our paper makes a number of contributions, the first of which is empirical. As Ginsburg (2019) notes: “[t]he consequences of the use of law and courts are still rather obscure. Existing evidence does not suggest that rebels that use law are more likely to prevail against the state but does suggest that rebel legal institutions can compete rather favorably compared with state institutions.”

We provide evidence that rebel law and courts have a significant impact on civilian attitudes which translates into material changes on the battlefield. The limited existing empirical literature on the consequences of rebel governance activities suggests that rebel governance has ambiguous effects on rebel combat strength. Rebel services can signal high organizational capacity, increasing the odds of a peace deal (Heger and Jung, 2017), but governance may have a null or negative relationship with rebel strength (Stewart, 2020). Ginsburg (2019) finds no correlation between rebel law and courts with rebel victory. Our study provides microlevel evidence which leverages within-conflict variation, which can account for cross-country heterogeneity otherwise not accounted for in prior studies. In doing so, we provide some of the first concrete evidence that rebel justice shapes battlefield conditions.

Our second contribution is clarifying and testing causal mechanisms to explain our baseline findings. We elucidate different causal processes by which insurgent courts can secure civilian collaboration - legitimacy, coercion, and vested interests. Legitimacy emphasizes civilian ‘hearts and minds’ being bought by rebels, whereas coercion is a case where civilians remain staunchly opposed to rebel rule but collaborate due to the threat of force (Kalyvas, 2006). We highlight the under-discussed mechanism of vested interest. Vested interest differs from either legitimacy or coercion accounts, in that civilians may not have their hearts and minds bought by rebels, and may also not be entirely forced into supporting insurgents, but choose to side with rebels anyhow due to pragmatic concerns that arise from the type and quality of the service being provided.

We provide evidence consistent with all three mechanisms, suggesting they may work jointly, feeding into one another. First, we show courts lead to an increase in insurgent intimidation of civilians, which is consistent with increased targeting of government collaborators enabled by a legitimate process to try and prosecute opponents. With a fair judicial process, insurgents can credibly promise to not punish civilians who did not work with the state, alleviating a potential tradeoff between deterrence and backlash when rebels attempt to rule through fear alone. As such, courts facilitate more repression of political opponents by insurgents.

Next, we show that courts are not only enabling coercion of civilians - they also reduce major disputes between civilians. The evidence is consistent with vested interest: if the Taliban was attempting to resolve security issues which plagued the community where courts operated, one would detect a decline in large-scale communal conflicts, which is what we observe. The dispute results suggest courts are not just rubber stamping coercion which would have happened in the absence of judges: they are also meaningfully impacting civilian behavior.

Further, we connect our results on civilian attitudes to combat by exploring how civilian collaboration may facilitate insurgent attacks. We explore two possibilities: civilian informing behavior changes in response to courts, and civilians join the insurgency after courts. We find that usage of government courts is positively associated with informing on insurgents at the microlevel, which is consistent with the idea that insurgents can trade services for information (Berman, Shapiro and Felter, 2011). We find no evidence of changes in recruitment activity.

More broadly, our study joins many others which explore how war creates states and governing institutions (Hui, 2005; Sánchez De La Sierra, 2020; Tilly, 2017). Scholars have long recognized that the pressure to win wars spurs governance innovations, specifically fiscal instruments to secure tax revenue to finance war efforts.¹ We show that competitive state-building during civil wars can also incentivize a race to provide judicial services to the civilian population. We build on the ‘war made the state’ literature by focusing on how insurgent groups develop legal institutions to capture popular support in the context of ongoing conflict.

Although the Afghanistan case has unique characteristics and Taliban courts are not generalizable to all rebel governance institutions, the core strategic dynamics this study elucidates are highly relevant in a wide range of civil conflict contexts. In places as diverse as the Philippines, Colombia and Northern Ireland, rebels have employed courts and dispute resolution techniques in competition with government authorities. Worldwide data indicates that since World War II there have been more than 200 internal armed conflicts that have included informal justice processes (Loyle and Binningsbø, 2018). Our results suggest they may have shifted the political and battlefield conditions in favor of rebels courting civilians during conflict.

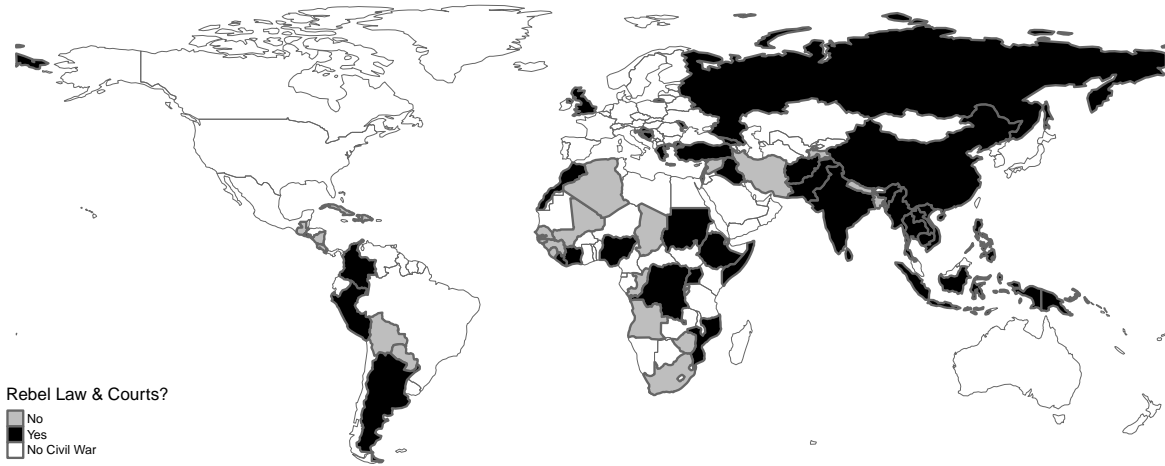
2 Theory

Courts are a key foundation of rebel governance since they “...allow the group to penetrate a community very effectively in relation to both important and mundane aspects of civilian life ” (Provost, 2021). Figure 1 shows the distribution of countries that have had a civil war during the 1945-2012 period (coded missing if not) and highlights those where at least one rebel group during the course of the conflict offered legal institutions. Slightly over half of civil wars during this period had rebel groups who provided law or court services (Albert, 2022). At the rebel level, Huang (2016) finds 28% of rebel groups in her data employed some form of court (Loyle, 2021).

Judicial services are institutions defined by three core functions: dispute resolution, social control, and lawmaking (Shapiro, 1981). Arbitration and mediation services are implied by courts, but a judiciary does something stronger: it connects rulings with coercive power, which allows

¹Scholars have shown how political crisis can drive legal development historically (Simpson, 2020); we focus on a contemporary case.

Figure 1: Rebel Judicial Service Provision During Civil Wars: 1945-2012



Data from Rebel Quasi-State Institutions dataset (Albert, 2022).

courts to modify behavior through social control and lawmaking rather than through creating self-enforcing agreements alone (Ginsburg, 2019). Judicial institutions support the rule of law - the concept that all are accountable to the same set of restrictions.

Courts rely on coercion, but their purpose is not to provide social order solely through violence. If insurgents wished to compel civilians to behave a particular way, they could secure compliance without making appeals to a formal legal process to justify their method of control. Investing in a court system is costly during an insurgency, since rulings need to be enforced and judges need to be protected, paid, and trained. Shifting resources to noncombat activities implies rebels find value in securing civilian approval not just through coercion, but also through legitimacy (Ginsburg, 2019).

2.1 How Courts Secure Civilian Collaboration

Courts can influence civilian collaboration with the insurgency in several ways. We stress these different pathways are not mutually exclusive. The existence of one pathway does not imply another mechanism is crowded out in its entirety.

2.1.1 Legitimacy

First, courts may persuade civilians that rebels are legitimate forces. Rebel governance can secure civilian cooperation; “by giving the community what it lacks, the group gives locals a reason to form positive beliefs about its involvement in local affairs” (Arjona, 2017). Further, courts can chip away at government legitimacy by demonstrating that plausible alternatives to the state exist (Ledwidge, 2017). To the extent moral outrage can drive civilian collaboration with insurgents (Wood, 2003), the presence of effective rebel courts can highlight the government’s failings while providing an outlet for civilians to act on their negative assessments of rule of law provided by the state. In much the same way as the state’s expansion into lawless areas can secure civilian support by facilitating social bonds (Karim, 2020), rebel judicial expansion may likewise increase the legitimacy of the insurgency.

An example of courts enhancing rebel legitimacy is the civil war in Nepal. Communist Party of Nepal-Maoist (CPN-M) gained significant support for the insurgency with their courts (Loyle, 2021; Sivakumaran, 2009). The so-called “people’s courts” provided a less corrupt judicial system, which was highly popular among the rural poor (Hutt, 2004, pg.18). Loyle (2021) suggests “Maoists themselves” attribute part of their success to the competence that their judicial system demonstrated, which focused on property crimes, domestic abuse, and war crimes.

2.1.2 Social Control

Second, courts may enable insurgent coercion of the population by creating a process that legitimizes violence against opponents. If rebels rule through fear alone, civilians may rationally choose to inform to the government when they would not want to otherwise: if a civilian can expect to be victimized with the same probability should they collaborate with the state or not under coercive rebel rule, they are better off working with the state to try to change their situation. Yet, with a criminal process and a court, a civilian may feel more secure in choosing not to collaborate with the state since they can prove their innocence to insurgents should they be accused. As such, with courts, insurgents can punish informers without the fear of creating backlash, leading to more net coercion.

An example of judicial institutions facilitating social control and coercion is the Irish Repub-

lican Army (IRA) in Ireland. Dudai (2022) documents how the IRA used a complex system - of which courts were a key component - to manage and coerce government informers to keep the insurgency alive. The constant fear that one could be accused of being an informer led to defections, since members thought they had a better chance of staying safe by working for government victory. To create the perception that one could contest a false accusation of informing, the IRA relied on a court martial procedure with due process to allow for adjudication of informing claims, rather than executions on the spot, which helped maintain cohesion (Dudai, 2022). Similarly, Islamic State (IS) declared violence was only permitted by the law, but also made “spying for nonbelievers” punishable by death under their penal code (Revkin, 2016). IS established a court system, but backed its governance with repression, which ruled civilians through fear (Revkin and Ahram, 2020).

2.1.3 Vested Interest

We elucidate a third mechanism - discussed often in the Afghanistan context - of vested interest. In this account, courts create a positive externality of social peace. Major disputes may remain unsettled by government courts for a variety of reasons - access to government buildings, administrative delays, or corruption may all block civilian access to justice. Local forms of dispute resolution - such as arbitration and mediation from elders or elites - may not be effective at resolving distributional conflicts between civilians over productive assets, because preserving a judgement requires some type of external enforcement. Whereas mediation and arbitration are efficacious when self-enforcing solutions can be found, judicial intervention with third party enforcement is necessary to resolve a conflict without self-enforcing solutions.

Rebel courts can settle conflicts which were otherwise intractable. This is especially the case for mobile courts: if justice can be delivered on the spot, rebels can make a good that was difficult for civilians to use due to distance or transportation costs suddenly accessible.

After settling conflicts between civilians that were otherwise intractable, communities exposed to insurgent courts enjoy new levels of order. The order created by rebel courts are intrinsic to continued rebel presence. Should insurgents lose a grip on a community, the cases they settled could reemerge because the legal and enforcement apparatus necessary for stability would dissipate.

The dynamic of vested interests has been used to explain judicial development historically.

During a period of civil conflict in medieval England, the king extended judicial services to politically vulnerable areas to secure support. Legal protection locked in support: “...there was no guarantee that rights extended by one ruler would be maintained by another. This gave subjects strong incentives to support a king under whom they enjoyed new protections” (Simpson, 2020). Since citizens knew the legal services provided by the crown might change under a new ruler, they increased support for the government relative to challengers.

The Taliban’s court system illustrates the vested interest mechanism. Vested interests in rebel rule created a cycle of dependence between civilians and continued rebel presence, which extended beyond the claimants of a particular case. Most obviously, winners of cases knew their preferred outcome was conditional on Taliban enforcement: “[o]ne could expect retaliation should the government reestablish its presence in any area that had been under Taliban influence for some time because the losers in disputes and criminal cases could turn to the government for support” (Giustozzi, 2012).

Since verdicts had community-wide impacts which created social peace, rebel courts can foster dependence on continued insurgent presence among civilians who were not claimants in a particular case. Since only the Taliban could enforce difficult cases: “...with each verdict the Taliban courts deliver, the insurgency’s presence increasingly becomes the condition for sustaining the social peace it has produced” (Baczko, 2013). For instance, a village in Ghaziabad district was subject to constant insecurity caused by an inter-family quarrel over a forest. The Taliban’s judges resolved the case, threatening arson should either family violate the judgement. Even villagers who had little sympathy for the Taliban approved of the ruling, and benefited from the end of the dispute (Baczko and Giustozzi, 2014).

Another example of vested interest can be found in rural Andar district of mixed-ethnicity Ghazni province, where there was a long-running dispute between two families about the ownership of a tract of land devoted to grape growing. All agreed that a neighboring tract had been sold in the 1950s from one family to a second, however the second family claimed that the sale also included the vineyard, which the original seller argued was not the case. The case caused significant tension in the community, with the former buyer reportedly viewed as stirring up trouble. After being approached by one of the claimants, the local Taliban judge issued a signed slip of paper requiring

all parties to come and produce their paperwork pertaining to ownership of the vineyard. After two days, judgment was rendered for the original seller, stating that the vineyard had not been sold and no further claims by the buyer family would be entertained.²

2.2 How Courts Increase Insurgent Attacks

Civilian collaboration augments insurgent attack capacity. Rebels rely on obedience, spontaneous support, or enlistment from civilians to provide them with resources and labor to fight the insurgency (Arjona, 2017). We highlight two potential pathways by which civilian collaboration could assist insurgents after courts: information sharing and recruitment.

2.2.1 Information Sharing

Information is critical to civil conflict dynamics; “counterinsurgents seek it, insurgents safeguard it, and civilians often trade it” (Lyall, Shiraito and Imai, 2015). Civilian informing to the government will spoil insurgent attacks, enable government ambushes, and will constrain rebel operations due to concerns about plans leaking to counterinsurgents. For instance, optimal bomb placement is along highly traveled paths; a group could help avoid civilian casualties if they inform civilians where they are planning on placing mines, but doing so risks the tip leaking to enemy forces (Giustozzi, 2019, 183). If insurgents have low political capital with the community, they may not be able to pursue their best military strategy due to the possibility of civilian collaboration with government forces. Conversely, with information from civilians and their support, insurgents can operate in secrecy to attack the government without fear of tips spoiling their attacks.

2.2.2 Recruitment

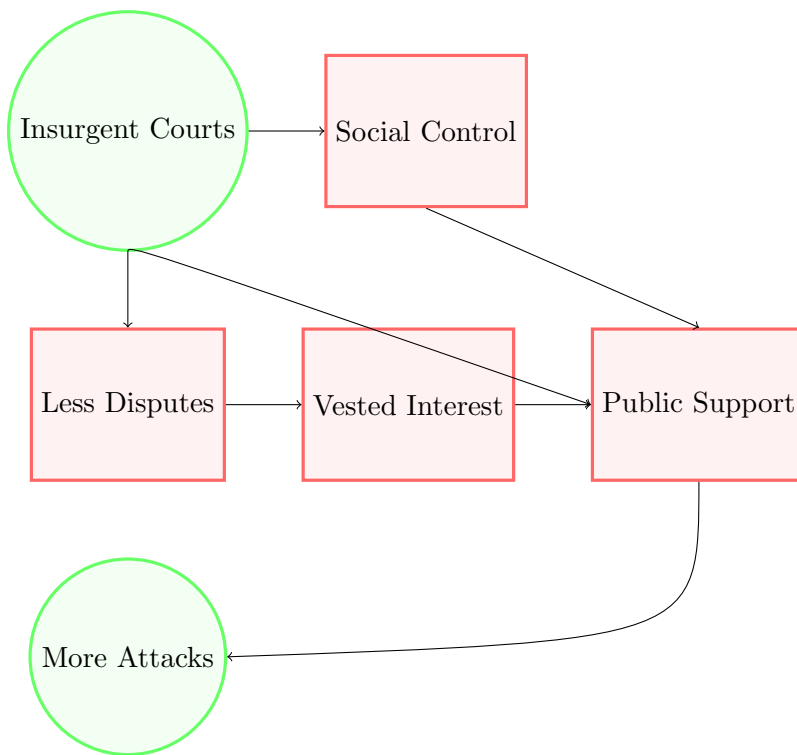
A second form of civilian cooperation with insurgents is enlistment (Arjona, 2017). Civilians can join rebel groups, providing labor for the insurgency and augmenting the capacity of the group to carry out attacks. The cost of joining an insurgency compared to sharing information is much higher, as such, we expect this mechanism to be inactive.

²Sahil Afghan 2020: <https://www.afghanistan-analysts.org/en/reports/war-and-peace/living-with-the-taleban-1-local-experiences-in-andar-district-ghazni-province/>

2.3 Summary of Causal Pathways

Figure 2 outlines the causal pathways mapping courts to our outcomes of interest as articulated in the theory section. Courts can boost public support directly through legitimacy, or indirectly by increasing social control or created vested interests. The upshot of increased public support (or collaboration) from civilians is more attacks.

Figure 2: Mechanisms Connecting Courts to Outcomes



Note: Causal mechanisms connecting insurgent courts to public support and additional attacks. Courts can have a direct effect on public support through legitimacy, or an indirect effect through creating a vested interest by resolving disputes or by increasing social control.

3 Context: Afghanistan 2008-2014

We study post-2001 Afghanistan to empirically test the effectiveness and mechanisms of rebel judicial services – in this case courts operated by the Afghan Taliban. In contrast with past work, we focus intensively on a single country and rebel group case, leveraging within-country variation, rather than looking for generalities across many rebel groups. This has both inferential and theoretical advantages, allowing us to hold fixed many of the potentially confounding factors across diverse country cases to isolate the specific effect of courts on citizen attitudes and conflict

outcomes.

3.1 The War in Afghanistan

After a devastating civil war (1992-1996) that followed the Soviet invasion of Afghanistan, the Taliban took control of the country in 1996, inaugurating a five-year regime that was notable for its uncompromising policies and close relationships with international Islamist organizations like Al Qaeda. Following the terrorist attacks of September 11, 2001, the United States and NATO allies invaded Afghanistan and expelled the Taliban from Kabul, installing an internationally-backed government.

The Taliban took several years to regroup, but then began mounting a large-scale insurgency against the new Kabul government (Giustozzi, 2008, 2019). In addition to violent operations against foreign troops and government security forces, the Taliban made “law and order” a core part of their appeal to the population. Dating back to their administration in the 1990s, the Taliban branded themselves as a movement that would provide social stability by aligning Afghan society with Islamic law – with some Pashtun traditional social rules included for good measure.

3.2 Taliban Courts

As the Taliban began to re-establish itself as a political contender and military force in the mid-2000s, with support from Pakistan, the group’s court system began to take shape. Providing courts was a mechanism for establishing and consolidating authority during a period of significant international military presence. Largely recruited from Deobandi madrassas, both in Afghanistan and Pakistan, incoming Taliban judges already were trained in the prevailing Islamic legal theories that the movement wished to enforce nationwide (Giustozzi, 2014). Taliban courts existed in earlier days of the insurgency, but became well-organized by 2011 (Baczko and Giustozzi, 2014, 208). The institutionalization of the court system included non-local judges and rotations across Afghanistan, to reduce the risk of corruption and co-optation (Giustozzi, 2012).

The case of the Taliban’s judiciary is of particular interest because of the evolution of the court system as it pertains to rebel territorial control. The Taliban’s judiciary was a core component of their governance framework, and it emerged prior to, rather than because of, territorial control.

As Jackson (2018) writes:

“The critical point, and one which is often missed in analyses of Taliban control, is that governance does not come after the capture of territory, but precedes it. Coercion, coupled with the more popular aspects of Taliban governance such as justice, softens the ground. Taliban governance does not supplant the Afghan government but co-opts and augments it, resulting in a hybrid service delivery arrangement.”

Taliban courts are an example of “on the spot” services that rebels deliver when they lack a monopoly on territorial control. The Taliban would leave contact information in villages and ride judges into town on demand to provide civilians with legal services should they have a dispute to settle. The Taliban’s mobile courts are similar to the courts set up by CPN-M in Nepal, which also dispensed justice from town-to-town (Loyle et al., 2021).

During the period we study, the Taliban used “...governance to keep the population at least marginally satisfied, and this, in combination with their coercive power, helps secure the population in areas under their influence or control” (Jackson, 2018). A part of the appeal of the Taliban’s court system is its reliance on an interpretation of Sharia that both allows the Taliban to use popular religious appeals to justify their rulings while incorporating local custom into decision making. These features mean the Taliban’s courts can base their decisions on criteria that local townspeople find agreeable and are more familiar in terms of procedure than Western-based rules and norms used by the Afghan government.

The rise of Taliban justice during the post-2001 era corresponded with a growing realization that the legal system of the Western-backed Kabul government was struggling to resolve a long backlog of civil and property disputes.³ Some dated back to the Soviet War era of the 1980s, with legal uncertainty and conflict interfering with final disposition (Giustozzi, 2014). By combining legal and religious training, as well as a careful understanding of the local cultural context, Taliban judges were able to render locally-legitimate judgments (Baczko, 2021). Perhaps most importantly, they were able to do so quickly.

Taliban courts, like other judiciaries, mainly resolved civil cases, such as land disputes, prop-

³Recognizing the weakness of the formal system, the Karzai administration outsourced some governance to warlords, with mixed results (Mukhopadhyay, 2014).

erty conflicts, or divorces (Jackson and Weigand, 2020). This is strategic: solving these problems provided social order for the whole village, which helped create a vested interest: “Taliban judges might even suddenly turn up in a village asking about a particular dispute or a criminal case, presumably following a report by the Taliban’s own intelligence system. This suggests that the Taliban did not merely conceive the judiciary as a service being provided, but as a strategy to penetrate rural communities” (Baczko and Giustozzi, 2014).

Judgments by Taliban courts were comparatively more effective than state courts or mediation for at least three reasons. First, Taliban courts had the power of religious legitimacy. As noted by one disputant, “[u]nfortunately, I lost the trial. I am not upset at the Taliban judges, they judge according to the Sharia, and I cannot oppose the Sharia” (Baczko, 2013). Although cases produce nominal “losers” in disputes, the religious legitimacy undergirding courts can shield the Taliban from backlash. One loser of a land dispute proclaimed “[t]he Taliban took my land from me, but to be honest I didn’t understand how Shariah worked...Now, logically looking at it, when they told me I needed to build a proper building, it makes sense to me.”⁴

Second, Taliban courts had greater enforcement capacity. Traditional mediation did not have a coercive backing which could compel a loser to abide by a decision; indeed, in the case of the forest dispute in Ghaziabad district described above, both families previously took the dispute to arbitration only for the loser to break the ruling without punishment (Baczko and Giustozzi, 2014).

Third, Taliban courts were more accessible than government courts. Sharia gave courts religious legitimacy, but also represented legal principles that were broadly familiar to the population. Relative to procedural rules largely transplanted by Western legal institutions for government courts, the Taliban system was far simpler to navigate. Additionally, government courts were hard for many rural villagers to access, with court fees, corruption and costs of transportation creating high barriers to entry. In contrast, Taliban judges often arrived in response to a single phone call. In a Congressional hearing in 2020, John Sopko, Special Inspector General for Afghanistan Reconstruction, remarked “...as much as you hate the Taliban, and I do, and I hate their brand of justice, to the average Afghan it is better than the justice provided by the National Unity Government”

⁴Azam Ahmed “Taliban Justice Gains Favor as Official Afghan Courts Fail” *New York Times*. Jan. 31, 2015.
[tps://www.nytimes.com/2015/02/01/world/asia/taliban-justice-gains-favor-as-official-afghan-courts-fail.html](https://www.nytimes.com/2015/02/01/world/asia/taliban-justice-gains-favor-as-official-afghan-courts-fail.html)

and he went on to detail how three separate Afghans he worked with had advised their families to use Taliban instead of government courts to resolve their disputes.⁵

4 Data and Design

4.1 Data

4.1.1 Courts

We use data on court locations and the years they were operational. The data were collected by a team of field researchers who survey two sources: Taliban officials and district elders and tribal leaders.⁶ Information from both sources was used for cross validation. The team collected baseline data in 2011 and updated information in 2012 and 2013. We use these field reports and harmonize the information available with district boundary information provided by the Empirical Studies of Conflict project. These boundaries are similarly used to merge the attitudinal outcomes and conflict event data we describe below. We map the spatial distribution of courts in Figure 3.

Although most courts continued indefinitely after being established, a small number of courts were withdrawn after a period of time (labeled “WD” in the map legend). These withdrawals were largely unrelated to local conditions, e.g. the types of cases, public opinion or conflict, but instead were driven by disagreements within and between the Quetta and Peshawar Shuras regarding turf (Baczko and Giustozzi, 2014). Our primary analysis focuses on the comparison of districts either exposed to courts once or never exposed. For robustness, we show our results remain unchanged when considering districts that received courts once as treated for the duration of the panel (See details in Appendix C.7). This design choice is minor in part due to the fact that the share of withdrawn court districts is relatively small.

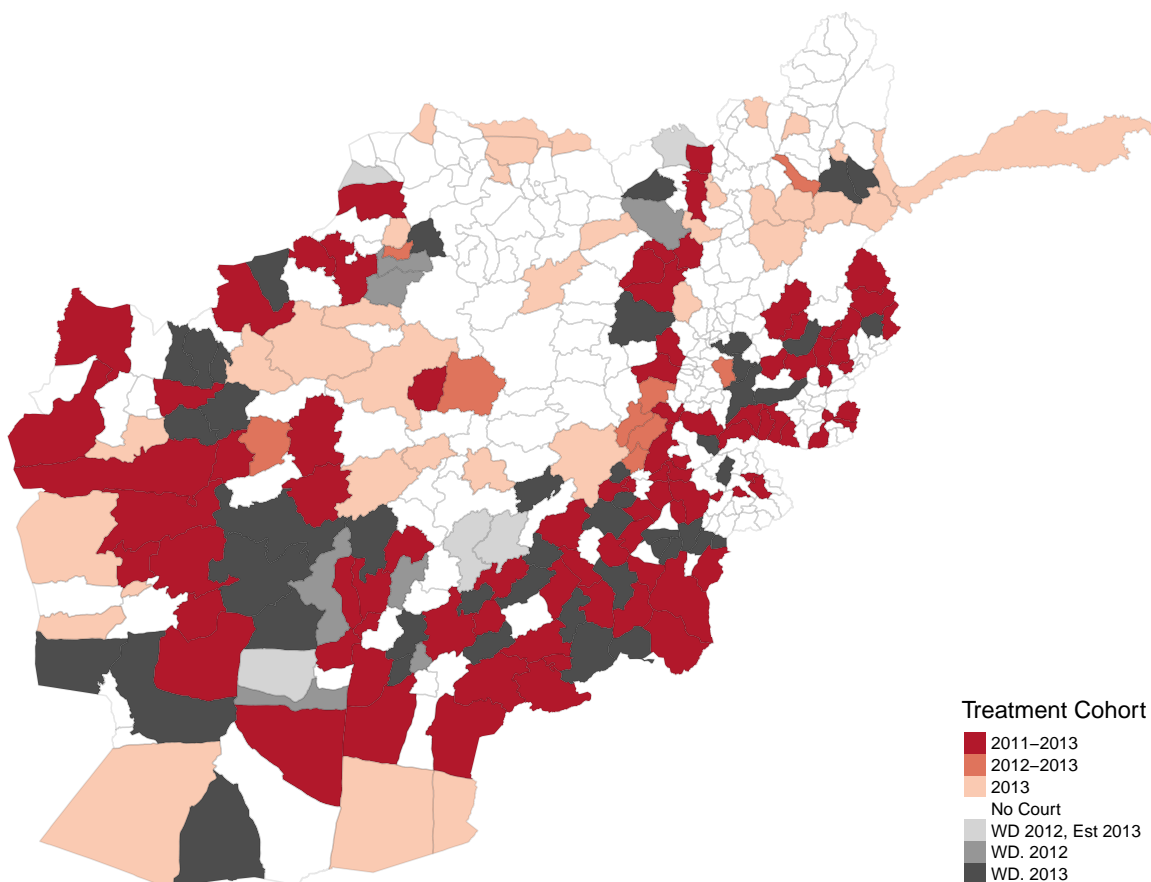
The presence of Taliban judicial services in 2011 does not necessarily mean that a court was established in 2011. Unfortunately, we lack precise information on start dates in districts that had courts in 2011 – some of these courts may have been first deployed in 2009 or 2010. In practice, though, during these early years (the heart of the Obama-era US surge) the Taliban judiciary was

⁵US Hearing Before the Committee on Foreign Affairs House of Representatives. 1-15-2020. Lessons Learned in Afghanistan <https://www.govinfo.gov/content/pkg/CHRG-116hhrg38915/html/CHRG-116hhrg38915.htm>

⁶We thank Antonio Giustozzi for generously sharing the original field reports used to produce our measures.

small and was not highly active, only becoming mature by 2011 (Baczko and Giustozzi, 2014, p. 208). From an empirical perspective, if some courts were operational and robust in a district before 2011, the error in the measurement of start dates is classical. There is no reason to think different start dates are correlated with potential outcomes, and by measuring the timing of courts with some error, bias will be away from a finding.

Figure 3: Taliban Courts Over Time and Space



Note: Map showing distribution of Taliban judicial activity. Boundaries are 398 Districts of Afghanistan.

4.1.2 Civilian Attitudes

We measure civilian attitudes with survey data that comes from the North Atlantic Treaty Organization, which contracted ACSOR, an Afghan subsidiary of the international firm D3, to design and field a recurring household-level survey. The data we rely on is drawn from the Afghanistan

Nationwide Quarterly Assessment Research (ANQAR) survey. ACSOR hired and trained local enumerators in household and respondent selection, data recording, culturally sensitive interview methods, and secure storage of contact information. ACSOR’s use of local-to-area enumerators increases comfort with survey interviews and decreases anxiety that external actors are monitoring and tracking respondents. We use Waves 1 through 24 of these quarterly surveys, which were collected between November/December 2008 and May 2014.

We construct four outcomes with our survey data which we aggregate to the district-year level, the level of granularity available for our court data.

State Court Usage: ANQAR asks respondents whether they would take a case to a government court if they hypothetically had a dispute. We use this question to measure whether civilians disengage with state institutions after being offered an alternative service from insurgents.

Taliban Approval: We use a question asked from Wave 6 to Wave 24 which inquired if the respondent thought a return of the Taliban as a governing body would be good for the country.

Government Influence: ANQAR asks “Between the two, the Anti- Government Elements (Mukhalafeen-e dawlat) and the Government, who has more influence in your mantaqah now?” We score a 1 if a respondent says the government and zero otherwise.

Support for Government Index (GOV Index): We follow Plumb et al. (2017) and build a index of support for the government using principal component analysis. The questions are highly correlated and ask respondents to assess how well different layers of government (district, provincial, national) perform on a variety of dimensions (economy, security, corruption, development and reconstruction, overall). Together, these questions represent civilian approval of the government’s performance, which we expect to decline when the Taliban offers a compelling alternative to the government’s services (e.g., courts).

4.1.3 Combat

We measure insurgent attacks, which we theorize will be a function of court presence due to changes in support from civilians. Our combat data is drawn from two sources.

4.1.4 Afghan NGO Safety Office (ANSO)

We use event logs from the Afghan NGO Safety Office (ANSO)⁷ from 2008-2013.⁸ During this period ANSO produced weekly spreadsheets of security-related incidents that recorded for each event the timing, location, participants and a description. The reports were submitted by a nationwide team of more than 100 enumerators working for ANSO, with nationwide coverage. Importantly, the Taliban during this period did not discourage or target NGOs or aid agencies; on the contrary, they encouraged aid agencies to operate, in an effort to skim resources and claim credit.

We use the text in event descriptions to code a range of event types, including disputes between civilians, property conflicts, improvised explosive device (IED) events and armed opposition group (AOG) events (excluding IEDs to avoid double-counting), and crime (e.g. robbery, non-AOG homicide, burglary or theft). AOG events reference armed opposition groups, usually typically Taliban forces. Disputes occur when non-AOGs engage in a violent clash over a disagreement. An example of a dispute from the data is as follows:

“An altercation occurred between two local civilians over a tribal dispute, injuring one person.”

4.1.5 US Military Significant Activities (SIGACTS)

We use declassified data collected by members of the International Security Assistance Force (ISAF) and their Afghan counterparts. During the conflict, these security forces documented the time and location of attacks, as well as attack type; there are more than 100,000 incidents between 2008 and 2013. We focus primarily on two types of attacks: direct fire, and improvised explosive devices (IEDs). Direct fire includes attacks perpetrated at close range (direct line-of-sight encounters), whereas IEDs are usually roadside bombs.

Our data also track casualties among Afghan and foreign forces. We use these measures to capture the intensity of insurgent missions that successfully harm or kill security forces. We also use information about instances of non-lethal attempted coercion or intimidation of the civilian population.⁹

⁷Now known as the International NGO Safety Organisation (INSO).

⁸We thank Renard Sexton for sharing data from ANSO beyond what was already published in Sexton (2016).

⁹As a validation exercise, we plot SIGACT IED explosion events as a function of ANSO IED events. We find the

4.2 Empirical Strategy

Our data contain $N = N_{tr} + N_{co}$ districts $i \in \{1, 2, \dots, N\}$, where tr and co denote treatment and control, across time periods $t \in \{2008, 2009, \dots, T\}$. Districts received Taliban courts (which we simply denote as ‘courts’) in a staggered fashion, placing them in cohorts $i \in \{2011, 2012, 2013, \infty\}$ where ∞ denotes that the district did not receive a court during the sample period. Further, let t_0 be the time where $t = \text{Cohort}_i$, meaning t_0 is the first time period where district i is treated, and t_{-1} is the time period *before* treatment. Define a treatment indicator $\text{Court}_{it} \in \{0, 1\}$.

$$\text{Court}_{it} := \begin{cases} 1 & \text{if } \text{Cohort}_i < \infty \text{ and } t \geq t_0 \\ 0 & \text{otherwise} \end{cases}$$

We are interested in estimating the the average treatment effect on the treated (ATT) for the outcomes of interest Y :

$$ATT_t = E[Y_{it}(1) - Y_{it}(0) | \text{Court}_{it} = 1]$$

where the potential outcomes are $Y(\text{Court}_{it})$ and $Y_{it}(1)$ denotes the outcome under treatment, $Y_{it}(0)$ what the outcome would have looked like under control, and Court_{it} is the binary indicator for whether a court is present.

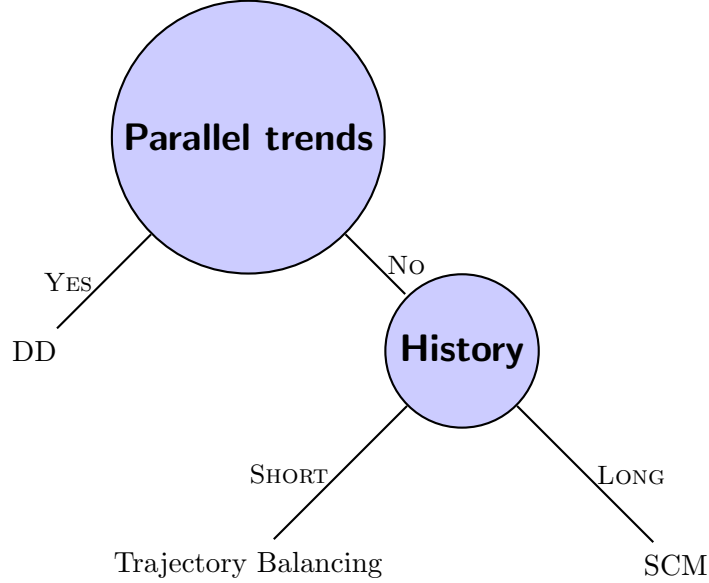
Our outcome of interest under control can be defined in four ways, the court group before courts, the court group after courts, the control group before courts, and the control group after courts. Going from left to right, those potential outcomes are defined as follows.

$$Y(0) = \begin{pmatrix} E[Y(0) | \text{Cohort} < \infty, t \geq t_0], & E[Y(0) | \text{Cohort} < \infty, t < t_0] \\ E[Y(0) | \text{Cohort} = \infty, t \geq t_0], & E[Y(0) | \text{Cohort} = \infty, t < t_0] \end{pmatrix},$$

However, we do not observe what the court group, those districts that received courts during the sample period, would have looked like had they never gotten courts. Replacing the above matrix with values we observe, the counterfactual world is missing.

reports are highly correlated between datasets (Appendix B.4.4).

Figure 4: Decision Tree Illustrating Choice of Research Design



Note: Decision Tree illustrating choice of research design. Difference-in-differences requires the parallel trends assumption, which do not hold either theoretically or empirically in our data. When the parallel trends assumption fails, researchers with a long pre-treatment history can use the synthetic control method (SCM), however, the approach does not work well when the history is short. Meanwhile, trajectory balancing is effective with a shorter pre-treatment history (Hazlett and Xu, 2018).

$$Y(0)^{observed} = \begin{pmatrix} Unobserved, & E[Y|Cohort < \infty, t < t_0] \\ E[Y|Cohort = \infty, t \geq t_0], & E[Y|Cohort = \infty, t < t_0] \end{pmatrix},$$

Our identification approach is to model the relationship between $E[Y_{it}(0)|Cohort = \infty, t < t_0]$ and $E[Y_{it}(0)|Cohort = \infty, t \geq t_0]$ (the second row in the $Y(0)^{observed}$ matrix) to interpolate $E[Y(0)|Cohort < \infty, t \geq t_0]$.

Popular approaches for modeling the counterfactual of treated units had they not been treated include difference-in-differences (DD) and the synthetic control method (SCM), neither of which are appropriate for our setting. SCM requires a small donor pool with a long pre-treatment history to arrive at an unbiased estimate of treatment effects. Our data has a short history prior to courts. DD requires parallel trends, which will be violated due to selective service provision based on trends in combat activity and civilian support.

We outline and explain our choice of design in Figure 4. Since DD and SCM are both inap-

propriate for our data, we use trajectory balancing to estimate the effect of courts on our outcomes of interest (Hazlett and Xu, 2018). Trajectory balancing is a general re-weighting approach for causal inference with panel data and binary treatment regimes where some units are exposed to an event in an absorbing fashion whereas other units are never exposed. The method is an extension of Hazlett (2020). The intuition for identification is as follows: if units that are exposed have a similar pre-trend in the outcome of interest as control units, then the control units serve as a valid counterfactual for what the evolution of the outcome would have looked like in the absence of the event. Following this idea, one may select a set of weights in order to create a weighted control group such that the trend of the actual treated units is mean-equal to the weighted control group. The weights from that control group may then be used to project out what the counterfactual evolution of the outcome would have been among the treated units. We estimate the average treatment effect on the treated.

$$\begin{aligned}
\widehat{ATT}_t &= \frac{1}{N_{\text{Court}_i < \infty}} \sum_{\text{Court}_i < \infty} y_{it} - \sum_{\text{Court}_i = \infty} w_i y_{it} \text{ with weights s.t.} \\
\frac{1}{N_{\text{Court}_i < \infty}} \sum_{\text{Court}_i < \infty} y_{it} &= \sum_{\text{Court}_i = \infty} w_i y_{it} \text{ for } t \leq \text{Year Before Treatment} \\
&\text{subject to } \sum_i w_i = 1 \text{ and } w_i > 0, \forall_i
\end{aligned} \tag{1}$$

Our approach is subject to three assumptions which we contextualize to our setting, test observable implications of, and relax when appropriate.

Assumption 1. *Conditional ignorability* $Y_{it}(0) \perp \text{Cohort}_i | \mathbf{Y}_{i,pre} \quad \forall t > T_0$

Assumption 1 states that the potential outcome for treated units had they not been treated is independent of their treatment status conditional on the trajectory of the outcome. The assumption is reasonable since the data generating process for outcome variables implies places that have similar values of an outcome are likely similar on fundamental baseline characteristics. For instance, if two places have the same level of support for the Taliban or same level of IED attacks, it would be very unusual if those districts were vastly different in terms of their history as a Taliban stronghold, assuming that past historical presence is a strong predictor of the outcome(s).

We assess a violation of this assumption as follows. Say $Y_{it}(0)$ depends on a transitory shock η_{it} and that $E[\eta_{it}|Cohort_i, \mathbf{Y}_{i,pre}] \neq 0$. Then our estimator will have the following bias.¹⁰

$$\underbrace{ATT}_{\text{Treatment Effect}} + \underbrace{E\left[\frac{1}{N_{Court_i < \infty}} \sum_{Court_i < \infty} \eta_{it} - \sum_{Court_i = \infty} w_i \eta_{it}\right]}_{\text{Bias}}$$

Transitory shocks η_{it} may include things like economic fluctuations (commodity price shocks, weather fluctuations) or changes in conditions on the ground (surges of troops). An example of this bias may be as follows: say a subset of control districts received an adverse economic shock which temporarily increased violence and reduced civilian support for the government. These districts may receive a high weight from our algorithm because they had higher levels of violence leading up to the treatment period. However, since transitory shocks are temporary, these control districts may have simply reverted back to their mean levels of support and conflict, making a normal cycle look like an *increase* among the treated units.

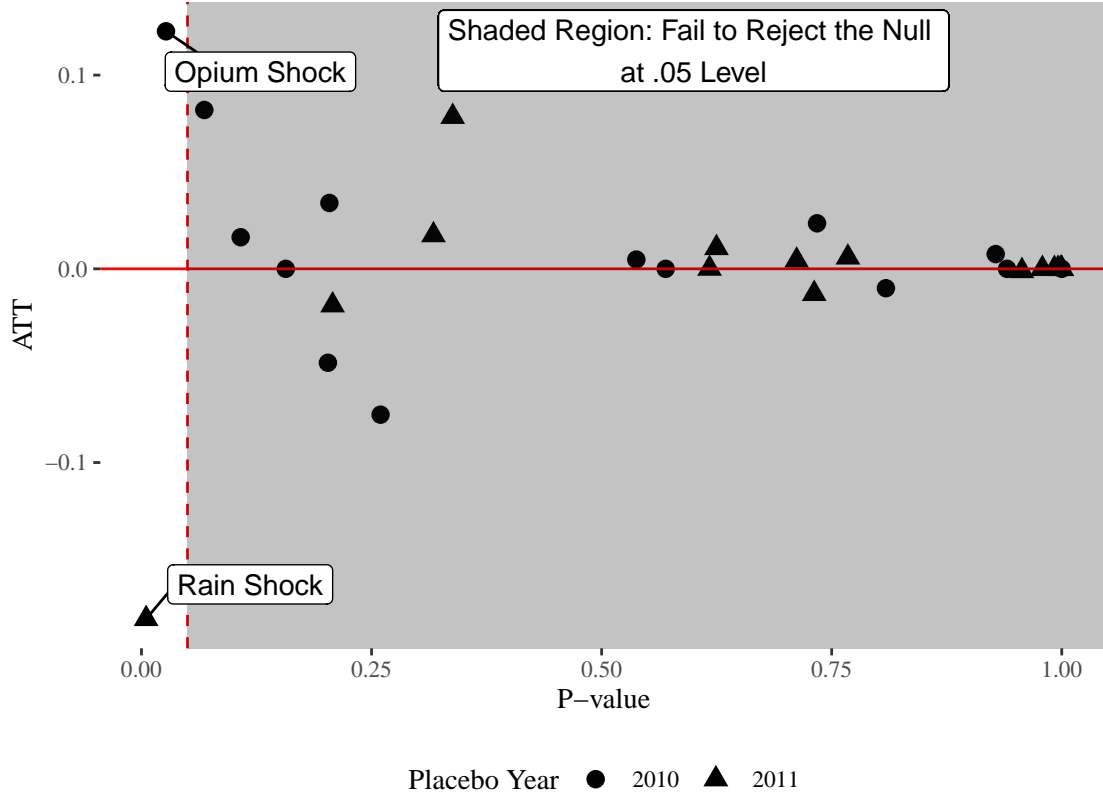
We assess a violation of this assumption by filtering our data to the pre-court period and checking for differences between court and (weighted) control districts. If future realizations of Y are independent of $Cohort_i$ conditional on Y_{pre} , then the difference between treated and control districts should be substantively small and statistically insignificant in the sample where no treatment effects are present. This should also be the case with time-varying covariates that courts ought to not impact as well. Intuitively, since the treatment effect is known to be zero in the preperiod, any differences in time-varying covariates is the result of bias rather than the impact of courts, which would suggest our estimates in the post period are contaminated.

We check for differences between the court group and control group by looking for differences in important time-varying variables: nighttime lights, opium, wheat price, rain, and temperature shocks, the number of forward operating bases in a district, and population.

Figure 5 plots the estimated ATT and p-value for our main outcomes of interest (attitudes and combat) along with auxiliary covariates (night lights, opium shocks, wheat shocks, rain shocks, temperature shocks, population, the count of US and NATO bases) using 2010 and 2011 as placebo

¹⁰See Appendix A.1 for a formalization of this result.

Figure 5: ATT and P-Value of Placebo Courts on Outcome(s)



Note: X-axis is the p-value for each test, and the y-axis is the estimated ATT on standardized outcomes using either 2010 (circles) or 2011 (triangles) as placebo court years. Vertical dashed line is the cutoff for statistical significance at the .05 level. Outcomes tested include government index, government control, Taliban approval, willingness to use state court, combat outcomes, nighttime lights, opium and wheat shocks, population, number of forward operating bases, temperature and rain shocks.

treatment time periods for all cohorts and the 2012/2013 cohort respectively with Equation (1). Results are substantively small (less than .2 standard deviations) and statistically indistinguishable from zero. The exceptions are opium shocks for the 2010 placebo year and rain shocks for 2011. Since we conducted 30 tests, finding two estimates that are statistically significant at the .05 level is approximately consistent with the null hypothesis. Because we do not find differences between the court group and non-court group in cases where we should not see treatment effects, we have provided evidence against the possibility that lurking differences between court and non-court districts explain the change in the outcomes, supporting Assumption 1.

Our next assumption is linearity in prior outcomes.

Assumption 2. *Linearity in Prior Outcomes (LPO)* $E[Y_{it}(0) | \mathbf{Y}_{i,pre}] = (1, \mathbf{Y}_{i,pre})^\top \theta_t + \eta_{it}$ for

$$E[\eta_{it} | \mathbf{Y}_{pre}] = 0$$

LPO states the potential outcome for the treated under control (conditional on prior outcomes) can be expressed as a linear function of past outcomes with a common intercept shift. LPO is reasonable given the aggregation level of our data. A violation would occur if our outcomes followed strong seasonal patterns: for instance, if we measured our outcomes at the monthly level, the cycles of the fighting season could create non-linearity in the potential outcome for the treated unit under control. However, since we aggregate our data to the yearly level, seasonal cycles would need to vary annually, and there is little evidence of this in the Afghan context.

Assumption 3. *Weight feasibility: there exists a set of weights w_i that are non-negative and sum to 1 such that:*

$$\frac{1}{N_{Cohort_i \neq \infty}} \sum_{Cohort_i \neq \infty} y_{it} = \sum_{Court_i = \infty} w_i y_{it} \text{ for } t \leq \text{Year Before Treatment}$$

We provide support for Assumption 3 by plotting the data for the treated cohorts versus the weighted average of the control cohorts. The plots illustrate that a set of weights that satisfy the constraints exist.

To conduct inference, we use a jackknife procedure which sequentially drops districts and re-estimates equation 1 to estimate variance and apply normal theory to obtain p-values (Hazlett and Xu, 2018). We explore other methods to obtain standard errors for robustness (Appendix C.9).

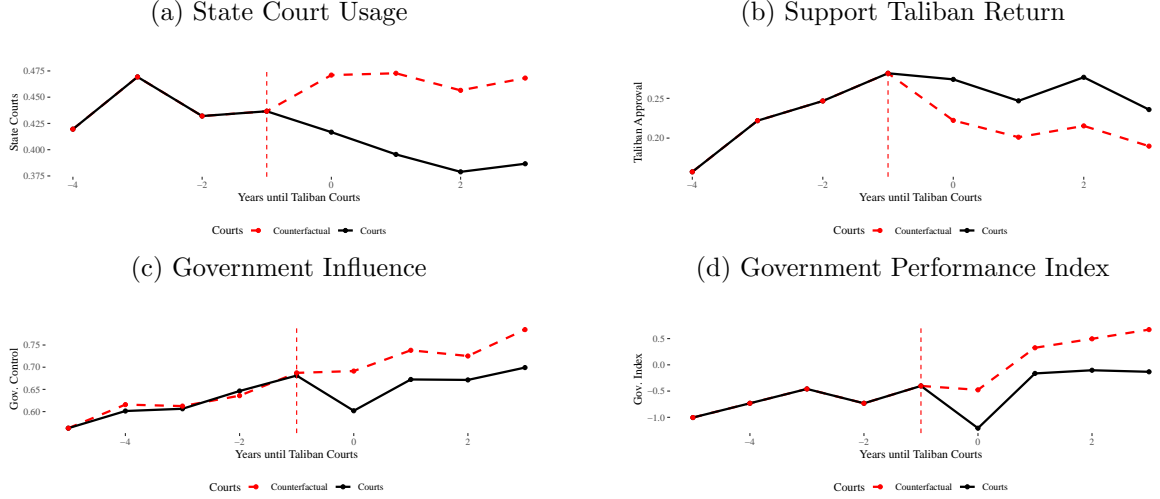
In Table 1, we summarize the key assumptions, whether they have directly observable implications that we can test, whether the assumptions can be relaxed, and how we relax them for robustness.

Table 1: Summary of Assumptions and Tests

Assumptions	Observable Implications	Tests	Relaxable	Relaxation
Conditional Ignorability	Yes	Placebo Test	Yes	Covariates
LPO	-	-	Yes	Kernel Balancing
Weights	Yes	Trend Plots	-	-

Note: Summary of three assumptions for the method. We list whether there are observable implications, how one can test for them, whether the assumption can be relaxed, and how we relax the assumption for robustness.

Figure 6: Public Opinion Trends: Trajectory Balanced



Note: Public Opinion Trends (ANQAR). Counterfactual trend constructed from the weighted average of districts that did not receive courts, with weights selected subject to the balancing constraint in equation 1 and explained in Section 4.2. Thick black line is the trend among districts that had courts (labeled ‘Courts’) and dashed, red line is trend among the counterfactual (labeled ‘Counterfactual’). The vertical axis is the average of each attitudinal outcome. The horizontal axis represents time, normalized to the time until courts are introduced (e.g., -1 is the year before courts, 0 is the first year courts are observed, 1 is one year after a court is introduced). The vertical dashed line is drawn at -1, the year before courts. The trend line to the left of the vertical dashed line is the pre-trend, before courts are introduced. The lines and points to the right of the line are the years after courts, the treatment period.

5 Results

5.1 Baseline Results

We first show the impact of courts on civilian attitudes and combat respectively. Afterwards, we explore causal mechanisms using other outcome data. We report our results graphically first, illustrating the trend of the court group versus the counterfactual as a function of years until courts are introduced. We then present the average difference between the groups in Tables.

5.1.1 Courts Shift Civilians Towards Rebels

Table 2 shows exposure to rebel courts shifts civilians attitudes towards rebels. Column (1) shows a 7% decline in respondents reporting that they would take their dispute to a state court should they have a dispute, nearly a $.4 \sigma$ decline. This provides evidence that Taliban courts crowd out government service provision. Column (2) shows that this reduction in usage of state courts translates into an increase in support for the Taliban: 5% more respondents indicate a return of the Taliban would be good for the country after courts.

Table 2: Civilian Attitude Results

	(1) State Court	(2) Taliban Approval	(3) Gov. Influence	(4) Gov. Index
Taliban Courts	-0.072*** (0.013)	0.051** (0.016)	-0.074*** (0.016)	-0.645*** (0.12)
N Districts	170	170	194	187
N. Years	6	6	7	7
SD DV	0.18	0.18	0.21	1.74
Mean DV	0.46	0.21	0.71	-0.05

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, $^{\dagger}p < 0.10$

Note: Outcomes are attitudes, measured with either ANQAR. Jackknife standard errors reported in parentheses.

Column 3 and 4 of Table 2 show courts also shift the government’s influence. Column (3) shows a 7% decrease in respondents reporting that the government has the most influence in their village. Finally, civilians score government performance lower; a $.37\sigma$ decline. Together, the evidence suggest civilians disengage from state institutions, change preferences regarding Taliban governance, rate government performance lower, and ultimately consider the government to be less influential in their district after courts are introduced.

One may expect subgroup effects along salient demographic traits if courts catered to particular interest groups. We find little systematic evidence of differences between Pashtun and non-Pashtun Afghans and between different economic strata. We find some weak but mixed evidence that men reduce uptake of state courts more than women, but we find no difference between genders on support for the Taliban returning (Appendix C.5).

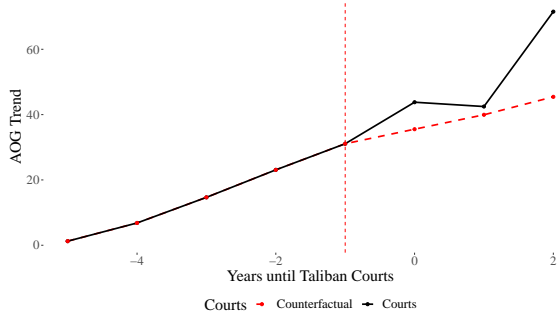
5.1.2 Courts Facilitate Insurgent Attacks

Table 3 shows rebels are able to convert increased civilian support into attacks. Column 1 and 2 use ANSO based outcomes. First, we observe 11 additional events involving armed opposition groups (AOG, the Taliban) and nearly 8 additional events involving improvised explosive devices (IEDs). These estimates are sizable relative to the mean of the control group (69% and 67% respectively).

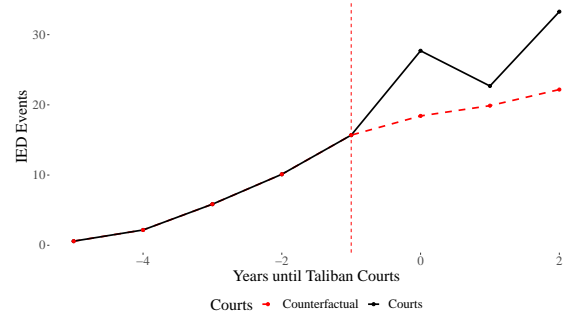
Columns 3-5 show combat increases using SIGACTs outcomes. We show insurgents execute 33 more direct fire attacks and nearly 8 more IED explosions. Note our estimate of IED events from ANSO and SIGACTs are nearly the same, which suggests the datasets which log events using

Figure 7: Armed Conflict Trends: Trajectory Balanced

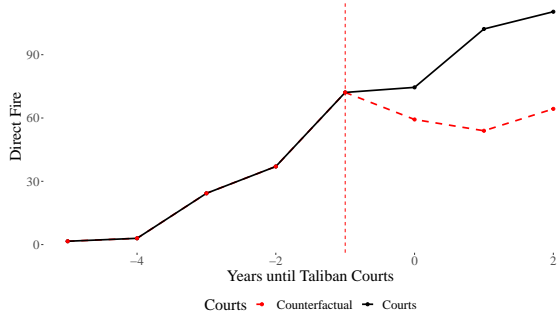
(a) Armed Opposition Group (AOG) Events (ANSO)



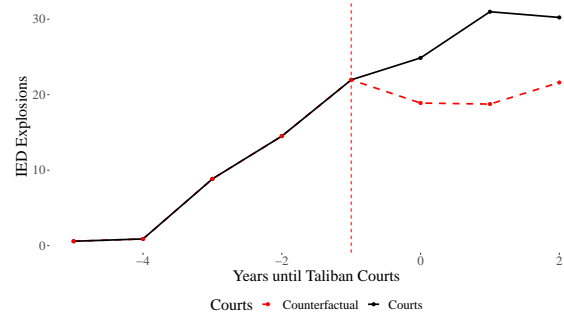
(b) IEDs (ANSO)



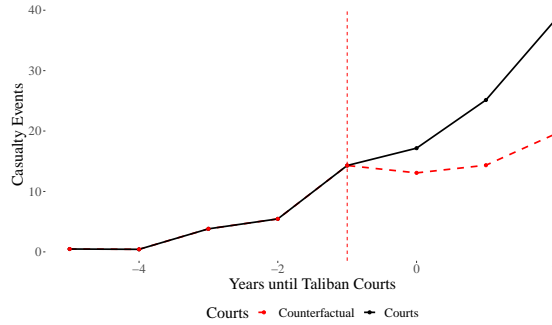
(c) Direct Fire (SIGACT)



(d) IED Explosions (SIGACT)



(e) Casualty Events (SIGACT)



Note: Average armed conflict trends (events involving armed opposition groups (AOG) from ANSO in Panel A, events involving improvised explosive devices (IEDs) from ANSO in Panel B, direct fire attacks from SIGACTs in Panel C, improvised explosive device explosions from SIGACTs in Panel D, and coalition and Afghan force casualties in Panel E) among districts in Afghanistan that received courts versus the counterfactual trend. Counterfactual trend constructed from the weighted average of districts that did not receive courts, with weights selected subject to the balancing constraint in equation 1 and explained in Section 4.2. Thick black line is the trend among districts that had courts (labeled ‘Courts’) and dashed, red line is trend among the counterfactual (labeled ‘Counterfactual’). The vertical axis is the average of each attitudinal outcome. The horizontal axis represents time, normalized to the time until courts are introduced (e.g., -1 is the year before courts, 0 is the first year courts are observed, 1 is one year after a court is introduced). The vertical dashed line is drawn at -1, the year before courts. The trend line to the left of the vertical dashed line is the pre-trend, before courts are introduced. The lines and points to the right of the line are the years after courts, the treatment period.

different methodologies are consistent with one another. Finally, these events increase casualties of coalition and Afghan forces, illustrating that the rebel attacks are not immaterial or without collateral consequences.

Table 3: Combat Results

Outcome	(1) AOG	(2) IED	(3) DF	(4) IED Explosions	(5) Casualty Events
Taliban Courts	11.46** (3.65)	7.81* (3.06)	33.87* (15.29)	8.62** (3.2)	10.25*** (2.98)
Dataset	ANSO	ANSO	SIGACT	SIGACT	SIGACT
N. Districts	339	339	339	339	339
N. Years	6	6	6	6	6
Standard Deviation DV	44.42	33.34	126.42	36.51	28.33
Mean DV (Control)	16.39	11.61	14.41	6.25	4.4

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, $^{\dagger}p < 0.10$

Note: Outcomes are combat, measured with either ANSO or SIGACT. Jackknife standard errors reported in parentheses.

5.2 Robustness of Baseline Results

We conduct the following supplemental analyses to ensure the robustness of our results.

Relaxing Conditional Ignorability Our first identification assumption is that $Y_{it}(0)$ is independent of $Court_{it}$ conditional on $\mathbf{Y}_{i,pre}$. Our placebo test introduced in Figure 5 increases our confidence in this assumption, but it may fail in practice if some other covariate is prognostic of both exposure and the potential outcome of our covariates of interest.

We relax the assumption by conditioning on district features which may predict courts and the outcome: rain and opium shocks, as those covariates were imbalanced in our placebo test, and Taliban control of the district (Appendix C.3). Further, we use three measures of control pre-courts to capture the possibility that our results are driven by pre-existing Taliban influence in a district. We use survey responses on government influence in a district, survey team access to districts, and the presence of Forward Operating Bases to capture control. Our results are consistent across measurements and specifications (Appendix C.10).

Relaxing LPO Our second key assumption is the potential outcome under no treatment is linear in the pre-treatment outcome history. Unlike either DD or SCM, our approach can relax LPO. We

relax this assumption by seeking balance on higher order dimensions of the pre-outcome history rather than just the mean through kernel balancing. For intuition, a mean balance for a flat trend could be achieved by collapsing very volatile control units with high and low values, however, the counterfactual that to be projected will likely be unrealistic since several units with different variance are unlikely to serve as a good counterfactual for a steady trend. Since kernel balancing accounts for volatility when creating weights, it avoids this pitfall. Further, by seeking balance on higher order features, kernel balancing performs better when there is a short pre-treatment history (Hazlett and Xu, 2018). We replicate our results with kernel balancing and covariates (Appendix C.4).

Inference We obtained standard errors from the jackknife in our main results, as described in the methods section. Inference is complicated in our setting because the weights we obtain are also subject to uncertainty. We calculate the variance of our estimates in two additional ways. First, we implement a block bootstrap treating districts as blocks, and obtain similar t-statistics (Appendix C.9.1). Next, We extend Hazlett and Xu (2018) and conduct inference through a placebo exercise that randomly assigns courts to the control group and reestimates equation (1).¹¹ We repeat the exercise many times to obtain a distribution of estimates which we use to estimate the uncertainty of the ATT. We include technical details in Appendix A.2.

Alternative Combat Models We measure combat as the count of events in our main models. We show that our results are consistent when we use the natural log of combat events per capita (Appendix C.1). ANSO or ANQAR only begin coverage in 2008, but SIGACT goes further back in time. We extend the panel with SIGACT outcomes from 2005-2014, documenting a similar pattern as found in the shorter panel (Appendix C.2).

6 Mechanisms

In this section, we provide evidence consistent with vested interest and social control. Further, we show that public support is connected to attacks by drawing on survey evidence that asks respondents about their use of court services and their willingness to inform on insurgents.

¹¹The same test is implemented in Arkhangelsky et al. (2019).

6.1 Do Courts Resolve Major Disputes?

The vested interest mechanism we propose suggests major interpersonal disputes should decline in response to the introduction of courts. We note that the reduction of disputes likely occurs through two channels: it is likely the case that that courts both reduce disputes directly, and indirectly deter disputes from occurring because the existence of an effective legal architecture makes illegitimate claims to ownership less likely to succeed. If it was the case that the Taliban’s judiciary prevented major social conflicts in a locality, creating stability and thus locking civilians into support for continued insurgent presence, one would observe especially large and disruptive conflicts decline in treated areas. Note courts have a direct effect on disputes by clearing existing cases, as well as an indirect effect, by deterring would-be disputants from disrespecting others legitimate claims by creating a legal infrastructure to enforce ownership.

We test the mechanism using ANSO event data. We parse text for violent events that did not involve armed opposition groups and included communal conflict or violence between neighbors. We further filter this data to include only disputes involving land. Since courts create vested interests when they solve disputes without self-enforcing solutions, the type of violent disputes measured in our data should be responsive to court introduction.

Further, as a falsification test, we include crime as an outcome: since the Taliban’s courts largely focused on resolving disputes that disrupted civilian day-to-day life and not on more trivial forms of criminal activity, we expect to see no difference in observed criminal violence. Our results could suffer from bias if reporting of events decreased after courts, which could lead us to uncover a negative effect when the only thing that actually changed was the flow of information. If this was the case, we would expect a differential decline in crime.

Table 4 shows disputes decline significantly after courts are introduced. The reduction can largely be attributed to changes in disputes over land specifically, which connects closely to the qualitative literature on the Taliban’s judicial services. Finally, we observe no difference in crime. The null result from crime guards against the possibility our findings are a modelling artifact: one may be concerned we only observe a decline in disputes because Taliban courts cut off access to reporting. However, if this was the case, one would find a decline in reporting across the board,

including crime. Yet, we see no change in criminal activity, increasing our confidence that the change in disputes is driven by behavior on the ground rather than the data reporting process.

Table 4: Trajectory Balanced Dispute Resolution and Crime Results

	Disputes		Property Disputes		Crime	
Outcome	(1) Log(+1)	(2) Binary	(3) Log(+1)	(4) Binary	(5) Log(+1)	(6) Binary
Taliban Courts	-0.12*	-0.09**	-0.04 [†]	-0.06*	-0.01	-0.01
	(0.05)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
Dataset	ANSO	ANSO	ANSO	ANSO	ANSO	ANSO
N. Districts	339	339	339	339	339	339
N. Years	6	6	6	6	6	6
N. Obs	2034	2034	2034	2034	2034	2034
Standard Deviation DV	0.76	0.5	0.32	0.34	0.65	0.48
Mean DV (Control)	0.59	0.47	0.13	0.15	0.44	0.38

Trajectory balancing results for courts on dispute resolution and crime. Columns 1-2 refer to disputes, Columns 3-4 refer to property (land) disputes, Columns 5-6 refer to crime. Odd columns are logged counts (+1) and even columns are binary $\mathbf{1(Event)}_0$. ‘Taliban Courts’ estimates derived from weighted difference in means where weights are obtained via equation 1. Standard errors nonparametrically computed through jackknife. *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, [†] $p < 0.10$

6.1.1 Do Courts Increase Coercion of Civilians?

After courts are introduced, the social control mechanism suggests the Taliban will be able to target opposition forces in districts more diligently and precisely. Indeed, the Taliban’s court system gathered information about crimes via villagers who reported facts to insurgents for the trial (Giustozzi and Baczko, 2012). The network of spies for court cases had a dual use; the Taliban could rely on these individuals to report on collaborators with the government, which the insurgency long considered a crime (Giustozzi, 2019). As such, the judiciary served to institutionalize insurgent’s coercive apparatus.

We use SIGACTs data on insurgent intimidation events, which logs threatened or realized violence directed at civilians - events where “...an individual or group of individuals are murdered by insurgent action due to their association with a particular group or organisation” where “killing of informers” is a key example.

In Table 5, we find Taliban intimidation incidents increase in frequency once courts are established. We note an increase of 15% in the probability of intimidation among treated units relative to the counterfactual. In terms of levels, our estimates suggest around 0.5 more intimidation events, which is 68% of the average number of intimidation events among control units.

Table 5: Insurgent Intimidation Results

Outcome	Intimidation (Count)	ln(Intimidation+1)	1(Intimidation)
Taliban Courts	0.45** 0.15	0.18*** 0.04	0.15*** 0.04
Database	SIGACTS	SIGACTS	SIGACTS
N. Districts	339	339	339
N. Years	6	6	6
N. Obs	2034	2034	2034
St. Dev. DV	2.75	0.59	0.46
Mean Control DV	0.66	0.26	0.25

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, $^{\dagger}p < 0.10$

Note: Table reports the effect of Taliban courts on insurgent intimidation - or coercion - of civilians. Outcomes measure intimidation as the count, log, or binary incidence of threats or use of lethal force against civilians by the Taliban. Estimates obtained per equation 1. Standard errors are jackknifed.

6.1.2 How Can Coercion Covary with Persuasion?

An increase in intimidation of civilians going hand-in-hand with an increase in support for the insurgency raises a question: why would civilians approve of an increased repressive force? The core reason insurgent intimidation of civilians can increase while civilians also grow more approving of the insurgency is that a segment of the civilian population prefers order to disorder, and may view coercion as a necessary intervention to achieve stability. The combination of increased services with more coercion aligns with the “varying combinations of persuasion and coercion” (Kalyvas, 2006, p. 101) insurgents use to consolidate control. Indeed, brutality against alleged criminals was met with public approval in Colombia, where insurgents used violence with the pretext of establishing local authority and social order (Arjona, 2016; Taussig, 2005).

Further, the finding is consistent with the state-building, courts, and civil war literature. Judicial institutions inherently combine both coercion and persuasion - the threat or use of force by the provider of a judicial service is an intrinsic and implied aspect of every court ruling (Shapiro, 1981). Further, the shift from interpersonal coercion in the form of violence over disputes towards

increased Taliban violence against civilians is strongly suggestive that force is being monopolized by the Taliban after the introduction of courts. Theoretical models of the transition from anarchy to state consolidation predict a decline in violence between civilians *because* of the increased capacity of the provider of security to use force (Hirshleifer, 1995).

We interpret the increase in coercion in tandem with public support as evidence of courts dual role, and the activation of two mechanisms occurring simultaneously. A rival account would be that coercion is the only active mechanism, and that coercion leads to preference falsification among respondents who feel socially pressured to express support for the Taliban on surveys.

We address social desirability bias in two steps.

First, if respondents became reluctant to express their true feelings after the introduction of courts, we would not only observe respondents lying about their true feelings, we likely would also see respondents refuse to answer questions. We use this insight to construct a new variable: the rate of missingness of our key attitudinal questions by district-year. We estimate equation (1) using missingness as the outcome(s), and find no evidence of divergence in response rates by group (Appendix C.6).

Next, we use an ANQAR question on respondent comfort during the survey to filter to respondents who are unlikely to feel social pressure. Respondents who express very high levels of comfort during the survey process are unlikely to be responding out of coercion or fear, since they did not find the survey process stressful. We estimate equation (1) using only comfortable respondents, and uncover the same results. (Appendix C.6)

6.2 Do Attacks Increase Because of Civilian Collaboration?

We study two different ways civilian collaboration may facilitate insurgent attacks after courts are introduced: information sharing and enlistment.

6.2.1 Information Sharing

ANQAR data allows us to test whether court preferences correlate with tipping preferences at the individual level. Waves 20-24 of ANQAR include the following question about providing tips about

insurgent attacks to government/coalition forces:

“If you knew that an IED had been planted, how likely would you be to report it to the local security forces? Very Likely, Somewhat Likely, Somewhat Unlikely, Very Unlikely.”

The likelihood citizens provide information about IEDs in particular is salient to our context. Unlike other insurgent attacks, effective IED placement relies to some degree on civilian support of rebel goals. Unfortunately, this survey question is not asked prior to the establishment of courts, therefore we cannot test our argument using trajectory balancing or other pre/post forms of analysis.

Since we cannot capture the change in tipping preferences, we use the ANQAR question about where a respondent would take a dispute if they had one - to a government court, a local shura/jirga, or elsewhere - to capture individual preferences for using state institutions and their likelihood of reporting to the government. We regress willingness to report IEDs on the measure of use of state courts, adjusting for a host of geographic, temporal, and individual covariates:

$$y_{i(d)(w)} = \alpha + \delta_d + \omega_w + \gamma \mathbb{1}(\text{Use Government Court})_{i(d)(w)} + \sum_{k=1}^k X_i^k + \eta_{i(d)(w)} \quad (2)$$

where $i \in \{1, 2, \dots, 62,199\}$ indexes respondents, $d \in \{1, \dots, 344\}$ indexes districts and $w \in \{20, \dots, 24\}$ indexes survey waves. The outcome of interest is civilians’ willingness to report an IED to local security forces. The outcome is measured on a 1-4 scale, with 4 representing the highest likelihood of informing and 1 capturing the very unlikely to report category. We also measure a binary measure $\mathbf{1}(\text{IED Report} \geq 3)$ which is unity when a respondent indicates a positive likelihood of reporting and 0 otherwise. Our regressor of interest is $\mathbb{1}(\text{Use Government Court})$, which is a binary variable scored 1 if a respondent indicates that they would take their dispute to a government court.

We include k individual covariates - education, age, age², ethnicity, gender, and perception of government control in the area, and whether a respondent would use a jirga/shura for a dispute - to capture observable traits that are correlated with support for the Taliban, which may codetermine willingness to report IEDs and the use of government judicial services. Further we adjust for district δ_d and survey wave ω_w fixed effects. We cluster errors at the district, since this is the administrative

level where government and Taliban courts were provided.

Table 6: IED Reporting on Court Usage

Likelihood of Reporting IED	Binary		Categories	
	(1)	(2)	(3)	(4)
$\mathbb{1}(\text{Use Government Court})$	0.19*** (0.01)	0.14*** (0.02)	0.32*** (0.04)	0.45*** (0.03)
N. Respondents	62199	62199	62199	62199
N. Districts/Clusters	344	344	344	344
District & Wave Fixed Effects	N	Y	N	Y
Individual Covariates	Y	Y	Y	Y

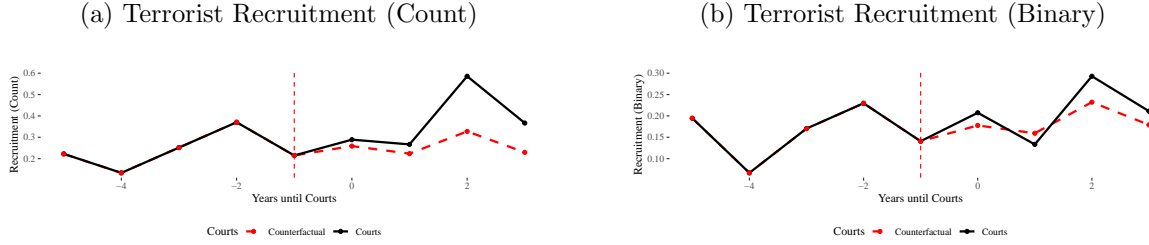
*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$

Note: Robust errors clustered at the district reported in parentheses. All models include age, age², education, ethnicity, gender, and perception of government control of the area. Regressor of interest is dummy for whether individuals would take their dispute to a government court. Outcome is civilian likelihood of reporting an IED either measured as binary (1 if likelihood is positive) or categories (likelihood is high, medium, low, very low). Question asked from Wave 20-24 which covered 344 districts.

Consistent with our expectation, results in Table 6 show the use of government public services, especially conflict resolution, is strongly correlated with willingness to collaborate with government forces in the neutralization of insurgent threats. More broadly, this positive correlation likely reflects a dynamic relevant to court provision in settings of contested authority: armed groups can use conflict resolution and other public services to thwart government access to vital information by undermining ties between the civilian population and government institutions.

Although these descriptive patterns are robust to a range of model specifications addressing the most pressing concerns about non-random selection into use of government courts, we cannot fully rule out other sources of bias. We therefore caution against interpreting these results in the same manner as our trajectory balanced estimates. However, these findings are consistent with a host of qualitative evidence from the Afghanistan context and beyond linking information sharing, civilian attitudes, and battlefield outcomes to provision of public services during conflict.

Figure 8: Public Opinion Trends: Trajectory Balanced



Note: Recruitment trend and counterfactual using mean balancing.

6.2.2 Recruitment

Another possibility is that the courts, by increasing sympathy, drive recruitment locally. We use SIGACTs data on terrorist recruitment. Recruitment events are rarely observed, unlike combat which is easy to record given its violent and public nature. Recruitment events are more difficult to log because they do not produce causalities. We measure levels and a binary specification. We do not find a consistent pattern in terrorist recruitment: although the ATT for the count outcome is positive and significant at the $p < .10$ level (Estimate: .104, SE .0585) we find no difference for the binary measurement, and the count increase is very small. We plot the counterfactual versus the observed data in Figure 8, which shows little evidence in changes in recruitment trends after courts. As such, it appears there is stronger evidence of civilian collaboration changing through information sharing rather than joining the insurgency.

7 Conclusion

Do rebel courts impact civilian attitudes and battlefield performance? We study the case of the Taliban courts in Afghanistan, one of the most intense counterinsurgency wars in the last twenty years. We leverage the timing and location of Taliban court expansion after the surge to explore the impact of rebel judicial institutions. Since the assumptions underlying common identification strategies will fail in this context, we adopt a novel design to reweigh control units to construct the counterfactual.

We show rebels can court public opinion by providing judicial services. Civilians become less likely to turn to government courts, become more approving of a Taliban return to power, consider government influence in their district to be lower, and government performance to be

less adequate after Taliban courts arrive in their district. Further, we document that the change in public opinion has material battlefield impacts: rebels increase their attacks and the coalition suffers more casualties after courts are introduced, ostensibly due to changes in civilian collaboration with the coalition instead of through recruitment.

Our study provides evidence that rebel judicial services can sway civilian attitudes during the conflict, with consequences for the war effort. The theoretical literature on rebel service provision suggests the impact of governing institutions should be strong and in the direction we expect, but identifying the impact of courts has remained an empirical challenge. We supply evidence that judicial services are not mere window dressing: they meaningfully impact the course of the war. Our study is one of the few that shows rebel public goods have spillover effects into combat.

Moreover, we show the causal processes by which this occurs: courts change patterns of interpersonal disputes and enable increased rebel discipline and punishment of civilians. As a consequence, civilian collaboration changes primarily through willingness to share information. We corroborate this channel by showing that using government courts correlates with willingness to inform on insurgents, and that patterns of recruitment do not budge after courts are introduced.

Our evidence on the theoretical mechanism should increase confidence that what we find in Afghanistan may very well occur elsewhere. Naturally each civil war has unique features, setting them apart from other historical struggles. Yet many intrastate conflicts emerge under political pressures relevant to the scope conditions of our arguments about vested interests and social control. These include a weak legal system being provided by the government, a demand for legal certainty from the civilian population, and an insurgent group that has an ideology which can be used to legitimize their rules. Weakened state capacity, especially in the provision of conflict resolution, creates opportunities for armed opposition groups to highlight and mobilize civilian support. Insurgents can amplify grievances created by unstable judicial institutions, including fractured systems of land tenure, corrupted public administration (where the likelihood of legal consequences is low), and varieties of illegal yet unregulated economic discrimination. These dynamics are reflected in mobilization tactics used by the LTTE in Sri Lanka, CPN-M in Nepal, and the IRA in Ireland and may well have had similar impacts on public support for these armed groups.

Rebel courts are a particular type of service, and our findings may not generalize to all types of public goods. For instance, a rebel-constructed road is unlikely to create a vested interest, because if rebels leave, the government may be able to seamlessly transition maintenance to ensure the good is provided uninterrupted. This would not be the case for something like social order produced through judicial rulings, since the state cannot credibly commit to honoring all holdings by another court. Additionally, roads may not serve the same information gathering or coercive functions as a court.

Yet vested interests may not be a mechanism unique to judicial institutions. Another example is indigenous, linguistically inclusive education systems. In cases where governments have engaged in historical discrimination with respect to languages spoken or subjects taught, rebels may be able to provide a credible alternative that generates a vested interest among affected communities. By devolving control over curricula to local authorities or taking a more active role in promoting identity-based education systems, rebels may enable an approach to education that the government cannot credibly commit to maintaining. Education is, like judicial systems, a platform for disciplining the local population, shaping knowledge formation and social behaviors through the subjects and languages taught (Paglayan, 2022). Importantly, any deep understanding of how these systems shape civilian's attitudes and behaviors will require future exploration.

The timing of our study also creates opportunities for future research in Afghanistan. This study focuses on how Taliban-led courts influenced civilian attitudes and combat outcomes in a relatively short-run period of several years in the middle of the twenty-year conflict in post September-2001 Afghanistan. Future research may explore how these early episodes of rebel justice provision shaped the broader fight to re/establish the political legitimacy of the Taliban. How did these courts, for example, influence Taliban attempts to consolidate authority after the majority of international forces left in 2014? Did these courts give the Taliban a refined view of local power dynamics, enabling them to negotiate with political elders and elites in the run-up to the final military withdrawal in 2021? How did these early judicial proceedings shape public expectations and demands after the Taliban took over local, provincial, and national seats of power? Did these courts buffer local populations for the various economic and political shocks caused by the Taliban's takeover of Kabul and the country's various institutions with it?

More broadly, our work contributes to debates concerning conflict during the process of state formation. The need to levy taxes during conflict has long been credited as an explanation for the formation of governing institutions. Beyond this literature, civil war has been called a state building process. We take a close look at how these institutions form and their impacts on civilian attitudes and actionable outcomes during an ongoing conflict. Future work might gainfully explore how public services provided by armed opponents of the state—in part or as a bundle of institutions—shape short- and long-run interactions with the state, whether or not rebels seize the seats of power.

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Online Appendix: Supporting Information

A Technical

A.1 Proof of Unbiasedness of Trajectory Balancing Estimator

The proof in this section follows very closely from Hazlett and Xu (2018), and is meant to illustrate the key findings in their paper for the purpose of our study.

First, we restate the three assumptions in the main text:

Assumption 1. Conditional ignorability $Y_{it}(0) \perp \text{Cohort}_i | \mathbf{Y}_{i,pre} \quad \forall t > T_0$

Assumption 2. Linearity in Prior Outcomes (LPO) $E[Y_{it}(0) | \mathbf{Y}_{i,pre}] = (1, Y_{i,pre})^\top \theta_t + \eta_{it}$ for $E[\eta_{it} | \mathbf{Y}_{i,pre}] = 0$

Assumption 3. Weight feasibility: there exists a set of weights w_i that are non-negative and sum to 1 such that:

$$\frac{1}{N_{\text{Cohort}_i \neq \infty}} \sum_{\text{Cohort}_i \neq \infty} y_{it} = \sum_{\text{Court}_i = \infty} w_i y_{it} \text{ for } t \leq \text{Year Before Treatment}$$

The argument we seek to prove is:

$$E[\widehat{ATT} | \mathbf{Y}_{pre}] = \frac{1}{N_{tr}} \sum_{\text{Cohort} \neq \infty} E[Y_{it} | \text{Cohort} \neq \infty, \mathbf{Y}_{pre}] - \sum_{\text{Cohort} = \infty} E[w_i Y_{it} | \text{Cohort} = \infty, \mathbf{Y}_{pre}]$$

We can rewrite Y_{it} as the sum of three components: the treatment effect of courts $\tau_{it} = Y_{it}(1) - Y_{it}(0)$, $Y_{it}(0)$, and an error term η_{it} . For simplicity, let $\text{Cohort}_{\text{cohort}}, \mathbf{Y}_{pre} = ..$. Then we can write the next line as

$$= \frac{1}{N_{tr}} \sum_{\text{Cohort} \neq \infty} E[\tau_{it} + Y_{it}(0) + \eta_{it} | .] - \sum_{\text{Cohort} = \infty} E[w_i Y_{it}(0) + w_i \eta_{it} | .]$$

which is a simple substitution of the term Y_{it} .

Next, we can rearrange terms and break up the expectations:

$$= \frac{1}{N_{tr}} \sum_{\text{Cohort} \neq \infty} E[\tau_{it} | .] + \frac{1}{N_{tr}} \sum_{\text{Cohort} \neq \infty} E[Y_{it}(0) | .] + \frac{1}{N_{tr}} \sum_{\text{Cohort} \neq \infty} E[\eta_{it} | .] - \sum_{\text{Cohort} = \infty} E[w_i Y_{it}(0) | .] + \sum_{\text{Cohort} = \infty} E[w_i \eta_{it} | .]$$

Under Assumption 1, we can drop the Cohort subscripts:

$$\begin{aligned}
&= ATT_t + \frac{1}{N_{tr}} \sum_{Cohort \neq \infty} E[Y_{it}(0)|\mathbf{Y}_{pre}] + \frac{1}{N_{tr}} \sum_{Cohort \neq \infty} E[\eta_{it}|\mathbf{Y}_{pre}] - \\
&\quad \sum_{Cohort=\infty} E[w_i Y_{it}(0)|\mathbf{Y}_{pre}] + \sum_{Cohort=\infty} E[w_i \eta_{it}|\mathbf{Y}_{pre}]
\end{aligned}$$

Then under Assumption 2:

$$\begin{aligned}
&= ATT_t + \frac{1}{N_{tr}} \sum_{Cohort \neq \infty} (1, Y_{i,pre})^\top \theta_t + \frac{1}{N_{tr}} \sum_{Cohort \neq \infty} E[\eta_{it}|\mathbf{Y}_{pre}] - \\
&\quad \sum_{Cohort=\infty} w_i (1, Y_{i,pre})^\top \theta_t + \sum_{Cohort=\infty} E[w_i \eta_{it}|\mathbf{Y}_{pre}]
\end{aligned}$$

Under Assumption 3:

$$\sum_{Cohort \neq \infty} (1, Y_{i,pre})^\top \theta_t = \sum_{Cohort=\infty} w_i (1, Y_{i,pre})^\top \theta_t$$

which allows us to write:

$$\underbrace{ATT_t}_{\text{Treatment Effect}} + \underbrace{E\left[\frac{1}{N_{Court_i < \infty}} \sum_{Court_i < \infty} \eta_{it} - \sum_{Court_i = \infty} w_i \eta_{it} | \mathbf{Y}_{pre}\right]}_{\text{Bias}}$$

Under our assumptions, $E[\eta_{it}|\mathbf{Y}_{pre}] = 0$, meaning shocks are mean zero conditional on the history of the outcome trajectory, so bias is zero. We test this assumption by estimating a version of equation (1) where ATT_t is zero by construction, meaning any difference detected between groups on average can be attributed to the bias term. However, we find that there is no effect in this subsample, which provides support for our assumption. Then, we have

$$E[\widehat{ATT}_t | \mathbf{Y}_{pre}] = \underbrace{ATT_t}_{\text{Treatment Effect}}$$

Under our assumptions, we have proven that

$$E[\widehat{ATT}_t | Y_{pre}] = ATT_t$$

A.2 Placebo Variance Estimator

We construct standard errors in the main text with the jackknife, which is the default method for computing standard errors with staggered treatment timing given by the authors of Trajectory Balancing. Standard errors with nonparametric estimation using reweighting is complicated by the fact that weights themselves are subject to uncertainty.

We conduct inference in a second way to ensure our results are not a function of the default selection of variance estimation. Our approach is as follows. We create a distribution of placebo estimates by assigning fake courts to control units and then estimating the ATT on the sample of

districts with no courts. We use three pieces of data, the outcome data for the controls \mathbf{Y}_{co} , the number of treated units per cohort $N_{\text{Cohort} \neq \infty}$, and select a number of repetitions K .

Our approach has three steps:

1. We use $N_{\text{Cohort} \neq \infty}$ to assign $N_{\text{Cohort} \neq \infty}$ control districts into a placebo cohort. The districts that are unassigned serve as the control group.
2. Next, we estimate the ATT of placebo courts on the outcome \mathbf{Y}_{co} . We name this estimate τ^k
3. We repeat steps (1) and (2) K times, leaving us with a $(k \times 1)$ matrix of placebo estimates where the elements are $(\tau^1, \tau^2, \dots, \tau^K)^\top$

We estimate the variance:

$$\hat{\mathbf{V}} = \sum_{k=1}^K \frac{1}{K} \left(\tau_k - \sum_{k=1}^K \frac{1}{K} \tau_k \right)^2$$

taking the square root to obtain the standard error.

B Descriptive

B.1 Survey Questions from ANQAR

B.1.1 Sampling Design of ANQAR

The ANQAR survey is stratified by province, covering all of the 34 first-level administrative units in Afghanistan. The administrative district is the primary sampling unit.¹² Sampled districts are selected via a probability proportional to size approach. After districts have been selected for sampling, secondary sampling units composed of villages and settlements are randomly selected.¹³ After the sampling set has been identified and before fielding a survey wave, ACSOR engages with local elders to secure permission for enumerators to enter sample villages. Once enumerators arrive at a village, a random walk method is used to identify target households. Once a household is selected, a Kish grid is used to randomize the respondent within each selected household. Prior work by Condra and Wright (2019) indicates that response rates in ANQAR are on par with other large surveys in more peaceful contexts.

B.2 Government Strength Questions

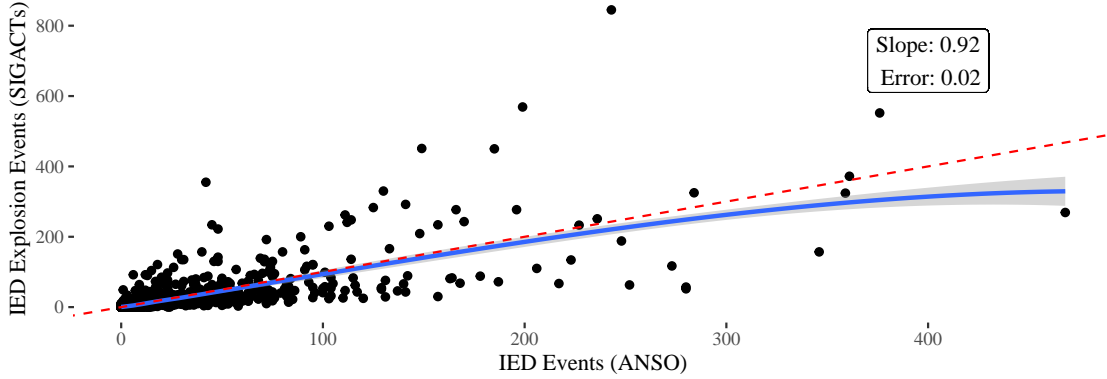
Table B.1: ANQAR Survey Questions

Question	Coverage	Concept
How well does the [Government of Afghanistan, Province Governor, District Governor] of Afghanistan do its job? Is it good, fair, or bad? (Overall, Security, Economy, Development, Corruption, Essential Services)	Wave 1-Wave32	Performance Index (15 Total Questions)
Between the two, the Anti- Government Elements (Mukhalafeen-e dawlat) and the Government, who has more influence in your mantaqa now?	Wave 1-Wave 32	Control (1 Total Question)

¹²One exception is Kabul district (the capital), which is subdivided into additional survey units due to the size of the city, which accounts for roughly 13% of the country’s population. For consistency, we utilize the administrative boundary designation for Kabul rather than these subdivisions.

¹³ACSOR maintains a list of villages and settlements, which is used as the sampling frame. It is the most comprehensive list currently available.

Figure B.1: Database Comparison: SIGACTs IED Events Against ANSO IED Events



Scatter plot of the IED Explosions against IED events from ANSO. Dashed line is 45 degree line (slope of 1, perfect correlation). Thick line is the LOESS fit which is highly linear. Linear regression coefficient reported in the top right corner with slope of 0.92.

B.3 Combat Data

ANSO data was collected from over 100 enumerators, who collected event information through “humanitarian and development NGOs, triangulated SMS and phone-in reports, and local news media” (Sexton, 2016). The effort was funded by a variety of nongovernmental organizations for the purpose of providing security incident information to keep aid workers safe.

We validate our measures by measuring the correlation between the same combat outcome - improvised explosive device explosion incidents (IEDs) - across datasets. We report the scatter plot and regression in Figure B.1. The slope coefficient is .9, and the linear and nonlinear fits of the data show a strong positive relationship between datasets. The fact that two different data collection methods are picking up on the same patterns is suggestive that different data collection methods are tracing the same pattern, increasing our confidence that ANSO is reliable.

B.4 Measuring Pre-Existing Taliban Influence (Control)

We rely on three measures of Taliban influence in the pre-2011 period to adjust our estimates.

B.4.1 Forward Operating Bases

We use the average number of forward operating bases in a district from 2008-2010.

B.4.2 Direct Survey Questions

ANQAR surveys directly ask respondents who has more control over the district: the government or the Taliban. We use the average responses to this question per district from the 2008-2010 period.

B.4.3 Background on ACSOR Control Data

Afghan Center for Socio-economic and Opinion Research (ACSOR) is a survey firm in Afghanistan responsible for fielding a wide variety of surveys on numerous topics across Afghanistan. They have a pool of over 1,000 interviewers, who come from diverse ethnic backgrounds and are both male and female.¹⁴

During the process of sampling villages from districts for surveys, ACSOR logged if a place was accessible, and if it was not, what the reason was for the accessibility. One of the reasons for inaccessibility was denial by the Taliban. ACSOR collected this data monthly at the district level for their internal purposes.

To measure control, we consider a district under Taliban control if the Taliban was able to deny access to a village in a district. The ability to make a place illegible to the state control par excellence in a counterinsurgency war, as it demonstrates the ability of an armed actor to deny external actors access to information.

To avoid posttreatment bias, we use the data from 2010 to measure control by the Taliban.

B.4.4 Relationship between ACSOR Control Data and ANQAR Survey Data

A natural concern from using survey data in a conflict zone is as follows: does the process of warfighting disrupt survey enumeration in a way that may confound one's results? We note any study that attempts to leverage public opinion data during wartime may fall prey to potential bias from inaccessibility due to the survey process.

Since our study has data on places that where access was disrupted due to Taliban influence, we can directly adjust our estimates for confounding from the data collection process, which is a feature unique to our study.

Importantly, in cases where survey teams are unable to visit a village, they do not simply give up on trying to assess opinion in a place. Instead, the teams use intercept surveys to collect information, which allows them to solicit information about the district even when the Taliban has influence. Again, if one is concerned that this difference in the collection process may influence our results since it could be correlated with control, we note that since we adjust for Taliban control through a direct measure of enumeration access, we account for this source of confounding.

¹⁴For more information on the survey teams, quality control, and general topics about the organization visit: ACSOR's website

C Empirical

C.1 Combat Per Capita

Table C.1: Combat Per Capita (Logged)

Outcome	(1) AOG	(2) IED	(3) DF	(4) IED Explosions	(5) Casualty Events
Taliban Courts	0.18** (0.06)	0.24*** (0.07)	0.22** (0.07)	0.23*** (0.06)	0.31*** (0.07)
Dataset	ANSO	ANSO	SIGACT	SIGACT	SIGACT
N. Districts	339	339	339	339	339
N. Years	6	6	6	6	6
Standard Deviation DV	1.86	1.68	1.96	1.65	1.57
Mean DV (Control)	2.14	1.69	1.8	1.26	1.13

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, $^{\dagger}p < 0.10$

Note: Combat outcomes transformed by per capita (attacks per 100,000 people) transformed with the natural log (plus one).

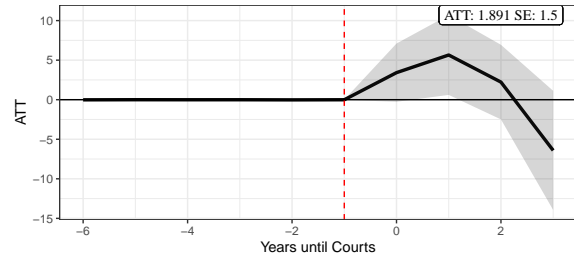
C.2 Combat Over Longer Time Period (2006-2014)

Figure C.1: Armed Conflict Trends: Trajectory Balanced 2006-2014

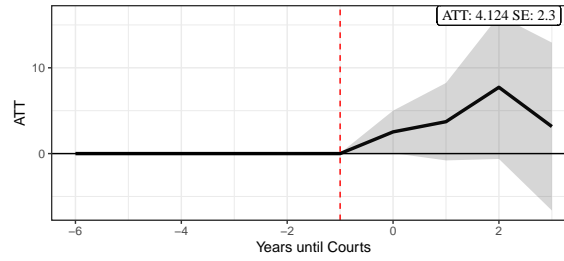
(a) Direct Fire (SIGACT)



(b) IED Explosions (SIGACT)



(c) Casualty Events (SIGACT)



Note: Mean balancing results using the levels of SIGACTs for the period of 2006-2014. ATT by period plotted. X-axis is the years until courts, the y-axis is the ATT, shading is the jackknifed standard error. Pooled ATT and standard error reported in the upper right corner.

C.3 Relaxing Conditional Ignorability

Table C.2: Civilian Attitude Results: Covariates

Outcome	(1) State Court	(2) Taliban Approval	(3) Gov. Influence	(4) Gov. Index
Taliban Courts	-0.07*** (0.02)	0.04** (0.01)	-0.07*** (0.02)	-0.72*** (0.12)
N Districts	170	170	194	187
N. Years	6	6	7	7
SD DV	0.18	0.18	0.21	1.74
Mean DV	0.46	0.21	0.71	-0.05

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, $^{\dagger}p < 0.10$

Table C.3: Combat Results: Covariates

Outcome	(1) AOG	(2) IED	(3) DF	(4) IED Explosions	(5) Casualty Events
Taliban Courts	11.6** (3.67)	5.86* (2.92)	32.46** (11.37)	7.68* (3.26)	9.82*** (2.88)
Dataset	ANSO	ANSO	SIGACT	SIGACT	SIGACT
N. Districts	339	339	339	339	339
N. Years	6	6	6	6	6
Standard Deviation DV	44.42	33.34	126.42	36.51	28.33
Mean DV (Control)	16.39	11.61	14.41	6.25	4.4

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, $^{\dagger}p < 0.10$

C.4 LPO: Kernel Balancing

Table C.4: Civilian Attitude Results: Kernel Balancing

	(1) State Court	(2) Taliban Approval	(3) Gov. Influence	(4) Gov. Index
Taliban Courts	-0.0699*** (0.018)	0.0313 (0.02)	-0.0807*** (0.02)	-0.8431 [†] (0.45)
N. Districts	170	170	194	187
N Years	6	6	7	7
SD DV	0.18	0.18	0.21	1.74
Mean DV	0.46	0.21	0.71	-0.05

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, [†] $p < 0.10$

Kernel balancing results with covariates. The outcomes are civilian attitudes measured by ANQAR.

Table C.5: Combat Results: Kernel Balancing

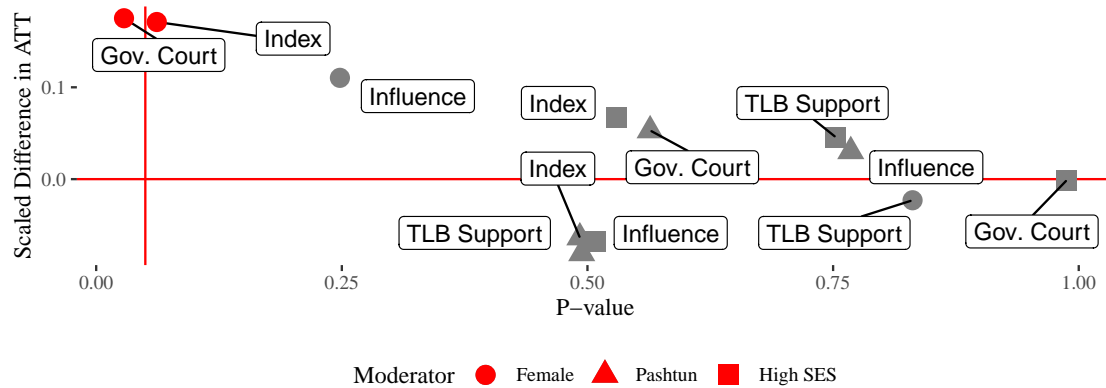
Outcome	(1) AOG	(2) IED	(3) DF	(4) IED Explosions	(5) Casualty Events
Taliban Courts	5.12 (3.3)	4.14 [†] (2.41)	35.29* (14.03)	5.38 (3.47)	6.71* (3.1)
Dataset	ANSO	ANSO	SIGACT	SIGACT	SIGACT
N. Districts	338	338	338	338	338
N. Years	6	6	6	6	6
Standard Deviation DV	42.9	31.34	92.23	29.23	22.26
Mean DV (Control)	16.39	11.61	14.41	6.25	4.4

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, [†] $p < 0.10$

Kernel balancing results with covariates. The outcomes are combat measured by ANSO and SIGACTs.

C.5 Attitude Heterogeneous Effects

Figure C.2: Difference in ATT by Subgroup



Each shape is a different moderator (gender, ethnicity, or SES). The vertical axis is the scaled difference in ATT between groups - for instance, the circle shape labeled female is the difference in ATT between female and male respondents. The horizontal axis is the p-value calculated from a t-test for difference in coefficients. Each label shows what the outcome is.

C.6 Social Desirability Bias

Table C.6: Civilian Attitude Results: Missingness Outcome

	State Court	Taliban Approval	Gov. Influence	Gov. Index
Taliban Courts	-0.0041	0.0049	-0.0026	-0.012
	0.0026	0.0065	0.0022	0.0068
N. Districts	167	167	192	187
N. Years	6	6	6	7

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, $^{\dagger}p < 0.10$

Outcome in each column is the share of total responses to the question that are missing. For instance, the state courts outcome is the number of respondents who did not respond to the question about state courts divided by the total number of responses, aggregated to the district year. All estimates are close to zero, and we do not reject the null that any estimate is distinct from zero.

Table C.7: Civilian Attitude Results

	State Court	Taliban Approval	Gov. Influence	Gov. Index
Taliban Courts	-0.0804*** (0.0156)	0.0572** (0.0181)	-0.0899*** (0.0205)	-0.6042*** (0.1442)
N. Districts	159	159	178	172
N Years	6	6	7	7
Mean DV	0.2	0.18	0.22	1.73
SD DV	0.46	0.19	0.73	-0.17

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, $^{\dagger}p < 0.10$

Outcome in each column is the response aggregated to the district year. For this analysis, we filter to only respondents who report they are comfortable with the interview. When restricting our analysis to only those respondents who are unlikely to be susceptible to coercion, we document the same pattern as in the main analysis, suggesting that respondent feelings about the interview process are unlikely to be driving our findings.

C.7 Sequencing: Withdrawn Courts

Table C.8: Civilian Attitude Results: Withdrawn Courts

Outcome	(1) State Court	(2) Taliban Approval	(3) Gov. Influence	(4) Gov. Index
Taliban Courts	-0.1** (0.04)	0.07* (0.03)	-0.11* (0.05)	-0.09 (0.25)
N. Districts	137	137	151	157
N. Years	3	3	4	4

Note: Outcomes are civilian attitudes measured by ANQAR. Taliban courts are using courts that are withdrawn in 2012 and 2013. We filter the data to 2011 and before to capture the effect of withdrawn courts before they are withdrawn.

Table C.9: Combat Results: Withdrawn Courts

Outcome	(1) AOG	(2) IED	(3) DF	(4) IED Explosions	(5) Casualty Events
Taliban Courts	2.95 (3.71)	7.81* (3.31)	21.8 (18.51)	9.56 (6.5)	2.96 (3.04)
Dataset	ANSO	ANSO	SIGACT	SIGACT	SIGACT
N. Districts	258	258	258	258	258
N. Years	4	4	4	4	4
Standard Deviation DV	33.08	32.12	74.42	26.92	15.95
Mean DV (Control)	14.29	10.42	11.03	5.57	3.16

Note: Outcomes are combat measured by ANSO and SIGACT. Taliban courts are using courts that are withdrawn in 2012 and 2013. We filter the data to 2011 and before to capture the effect of withdrawn courts before they are withdrawn.

C.8 Recode: Withdrawn Courts

Table C.10: Civilian Attitude Results: Recoding Withdrawn Courts

Outcome	(1) State Court	(2) Taliban Approval	(3) Gov. Influence	(4) Gov. Index
Taliban Courts	−0.07*** (0.01)	0.05*** (0.01)	−0.06*** (0.01)	−0.53*** (0.1)
N. Districts	198	199	228	221
N. Years	6	6	7	7

Note: Outcomes are combat measured by ANSO and SIGACT. Taliban courts are all courts, coding those as withdrawn as being exposed in 2011 and exposed continuously.

Table C.11: Combat Results: Recoding Withdrawn Courts

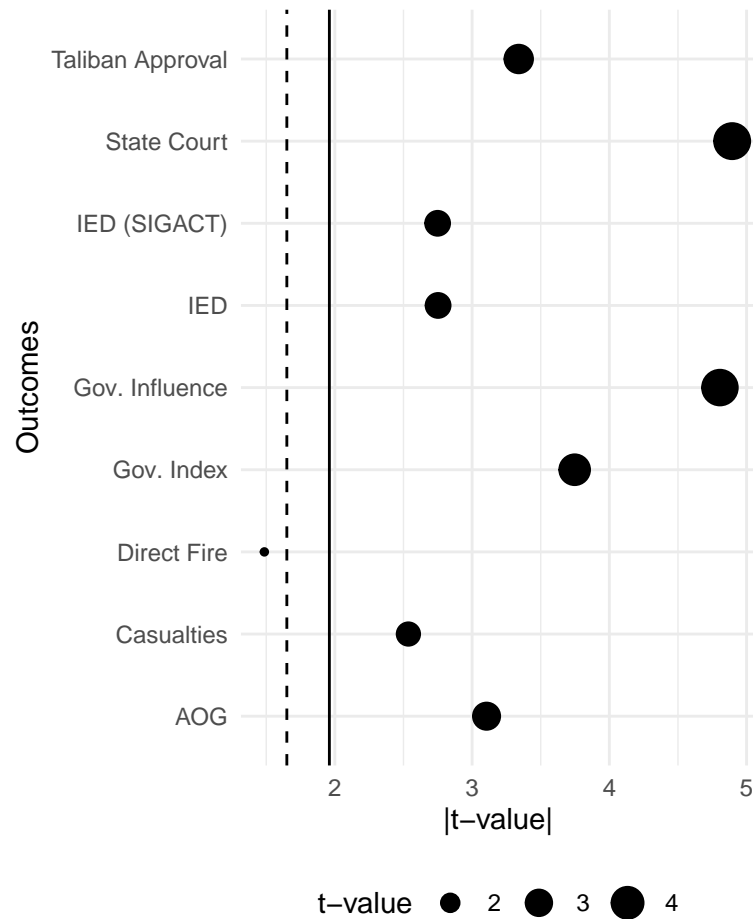
Outcome	(1) AOG	(2) IED	(3) DF	(4) IED Explosions	(5) Casualty Events
Taliban Courts	8.44*** (2.48)	6.74** (2.17)	33.51*** (10.08)	8.98*** (2.5)	8.44*** (2.05)
Dataset	ANSO	ANSO	SIGACT	SIGACT	SIGACT
N. Districts	398	398	398	398	398
N. Years	6	6	6	6	6
Standard Deviation DV	45.21	33.89	177.63	42.12	31.3
Mean DV (Control)	16.39	11.61	14.41	6.25	4.4

Note: Outcomes are combat measured by ANSO and SIGACT. Taliban courts are all courts, coding those as withdrawn as being exposed in 2011 and exposed continuously.

C.9 Standard Errors

C.9.1 Block Bootstrap

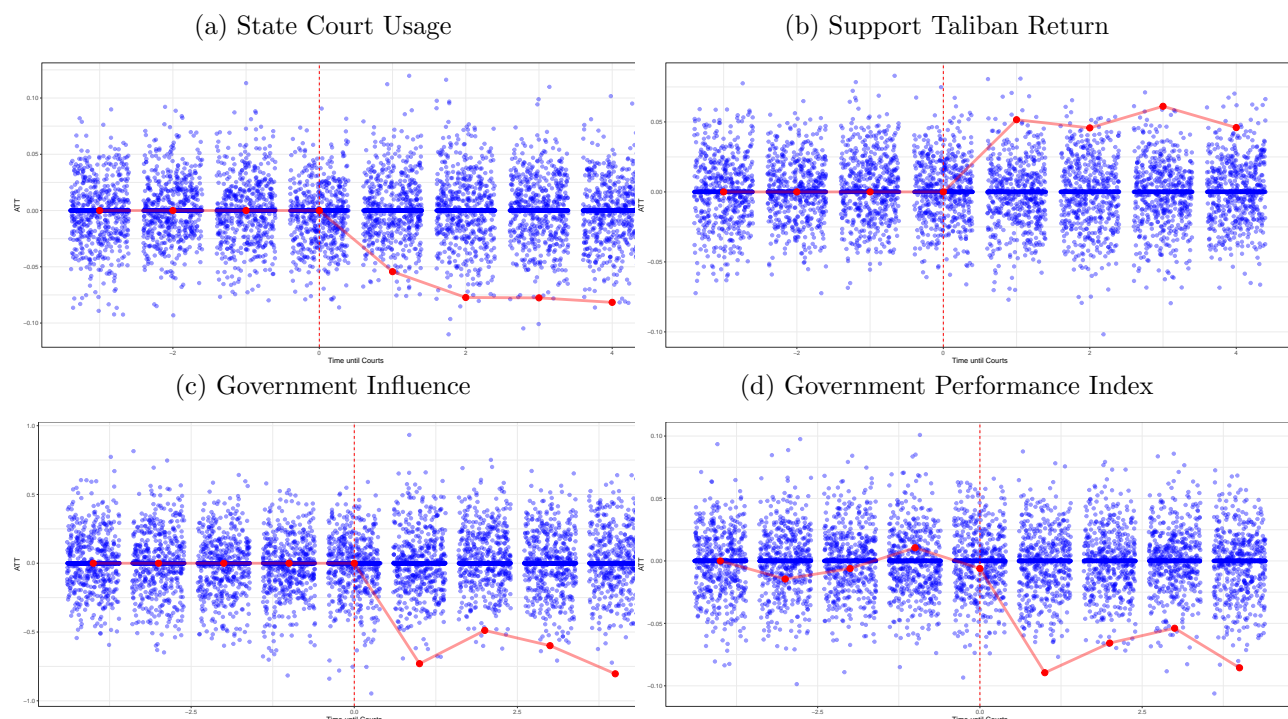
Figure C.3: Block Bootstrapped Standard Errors: Outcomes Against T-Statistics



Note: horizontal axis is the absolute value of the t-statistic (t-value) and the vertical axis is the outcomes. Points are sized proportional to the t-stat. The dashed line is the t-value for $p < .10$ and the solid line is the t-value for $p < .05$.

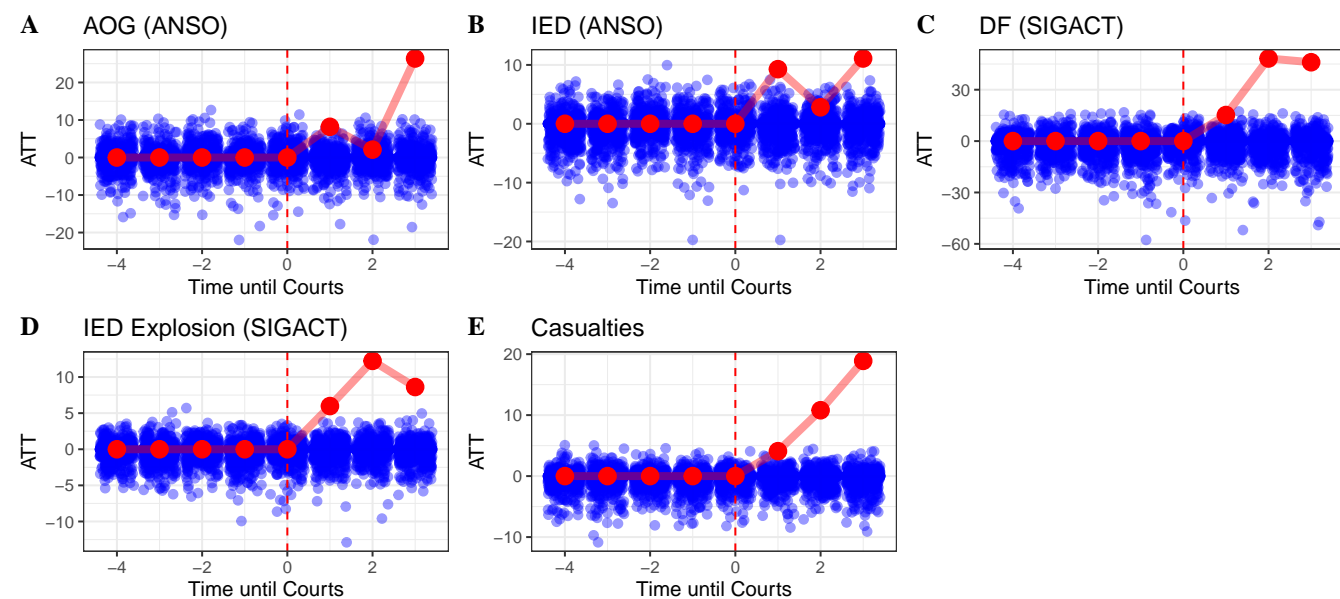
C.9.2 Placebo Variance

Figure C.4: Public Opinion Placebo Variance Estimates



Note: Each blue dot represents a period-wise ATT estimate of a version of equation (1) where the cohort vector is randomly shuffled to produce a placebo treatment variable. Red line shows the estimate using the true treatment vector.

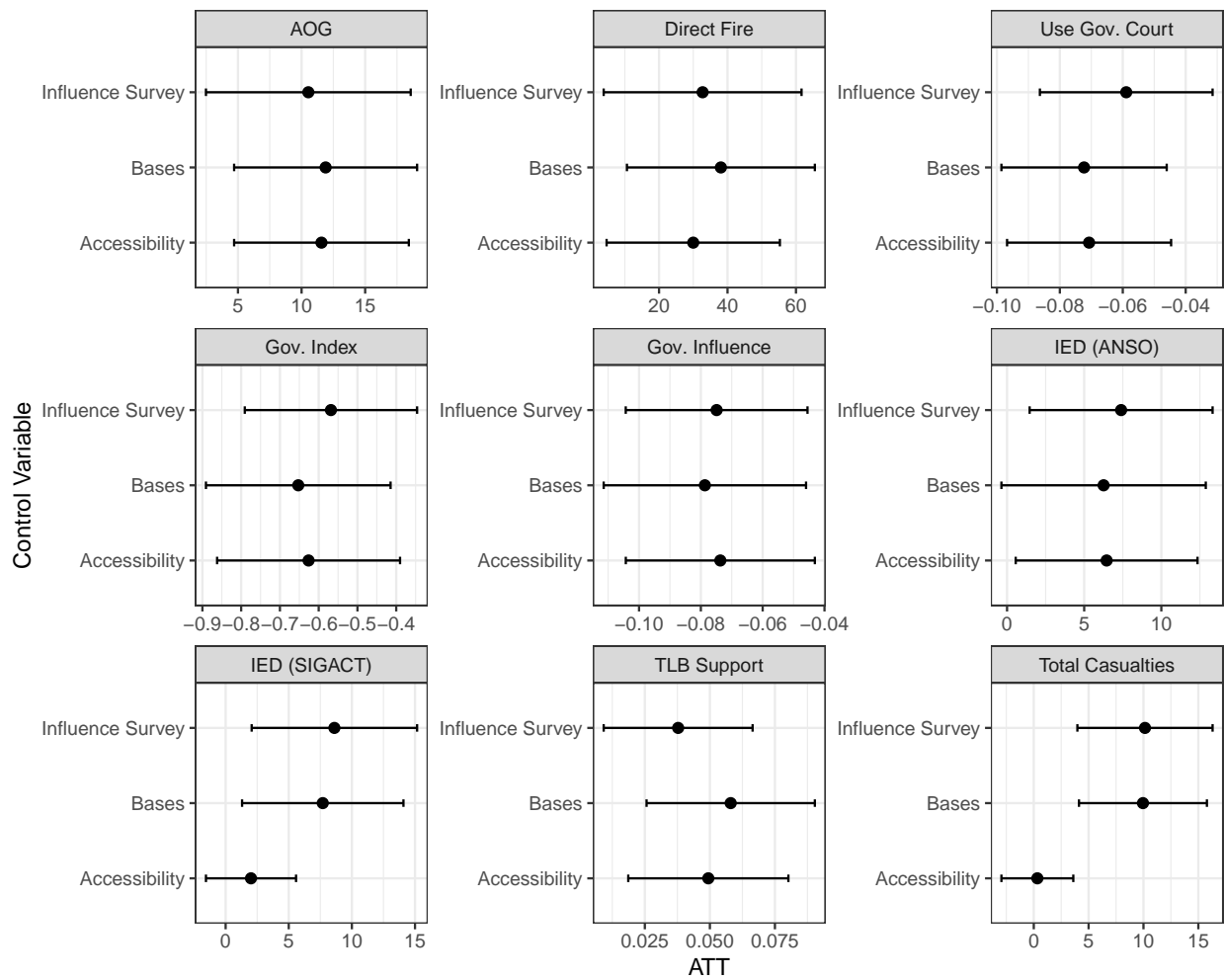
Figure C.5: Combat Placebo Variance Estimates



Note: Each blue dot represents a period-wise ATT estimate of a version of equation (1) where the cohort vector is randomly shuffled to produce a placebo treatment variable. Red line shows the estimate using the true treatment vector.

C.10 Adjusting for Control

Figure C.6: Adjusting for Control



Note: Each subfigure is a different outcome. Every estimate shows the ATT and confidence interval using a different pre-treatment measure of control.