The Logic and Impacts of Rebel Public Services Provision: Evidence from Taliban Courts in Afghanistan

Donald Grasse[†]   Renard Sexton[‡]   Austin Wright[§]

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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. Using a difference-in-differences design, 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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[†]Department of Political Science, Emory University (donald.grasse@emory.edu)

[‡]Department of Political Science, Emory University (renard.sexton@emory.edu).

[§]Harris School of Public Policy, University of Chicago (austinlw@uchicago.edu).

1 Introduction

Rebels regularly provide public goods in territories they control or seek to influence, with a goal of boosting productive capacity and revenues 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. At the same time, the theoretical mechanisms by which rebel service provision might meaningfully change attitudes and behavior remain underdeveloped.

In this study, we examine a common type of rebel service provision – judicial services, including dispute resolution – in the context of Afghanistan. Building on the growing literature on rebel governance, we develop a theory for why insurgent groups' courts could change citizens' opinions of the rebels and their behavior towards the group. We argue dispute resolution creates a public vested interest in rebel rule: if the government were to eject rebels and re-impose a state monopoly on governance, all the disputes resolved by insurgents would be re-opened. This means that even those ideologically ambivalent, or even opposed, to insurgents would be receiving a benefit to rebel rule, with formerly festering property disputes in the community now put to rest.

Using new data on the locations and timing of Taliban courts in Afghanistan in the early 2010s and employing a difference-in-differences approach, we find that these rebel courts significantly reduce the number of major interpersonal, and especially property, disputes in the districts where they operate. We identify a corresponding reduction in citizen willingness to bring disputes to government courts, and an increase in citizen approval of a return to Taliban rule. Finally, after courts are operational, we find that the Taliban are able to increase significantly the number of bombings and direct fire attacks that target government and foreign troops. Together, the results indicate that rebel judicial services, if carried out competently, can significantly sway public opinion and enhance the fighting capacity of rebels.

Might the establishment of courts by the Taliban simply be a proxy for latent insurgent control or popularity? This interpretation clashes with both the quantitative evidence and the qualitative historical record. As we detail below, leveraging an exhaustive review of administrative documents produced during the period of interest, the number of districts under Taliban control was dwarfed by

their governance footprint: courts were available in 84 districts beyond Taliban authority. We also implement several data-driven specifications that account for realized and potential rebel authority, finding evidence consistent with the main results. More broadly, we anticipate, as noted Taliban scholar Ashley Jackson has stated, during this period "governance does not come after the capture of territory, but precedes it."[1]

At the same time, could the increase in insurgent violence after courts are operational be a result of the Taliban coordinating their attacks with court roll-out, rather than the consequence of attitudinal change? We find evidence against this alternative interpretation: investigating a set of districts where courts were introduced but relatively quickly withdrawn, we find that disputes are somewhat reduced and public opinion changes slightly to favor the Taliban, but there is no difference in violence levels.

Examining mechanisms, we find survey evidence that citizens' court usage preferences are highly correlated with propensity to report improvised explosive devices (IEDs) to local government security forces. This bolsters the case that the effects on battlefield outcomes are mediated via citizen opinions rather than some other channel. We also find that the effects of the Taliban courts do not vary by socio-economic status, suggesting that it is not only wealthy property owners that are benefiting from dispute resolution. The effects are also consistent across ethnic groups, indicating that not only Pashtun co-ethnics of the Taliban are changing their attitudes and behavior in response to the courts. Furthermore, the public opinion effects are the same for both female and male respondents, indicating that gender (and possible gender-related judicial decisions by the Taliban) appears not to be driving the effects.

Nearly identical effects across these social strata support our argument that Taliban courts generated a *communal* dependence on insurgent rule, rather than catering to interest groups with ideological predispositions towards the rebels. Lastly, in contrast with civil and property disputes, we find only very small effects of courts on violent crime, suggesting that the Taliban focused especially on economically important property dispute cases.

These findings help to explain the Taliban's surprisingly rapid takeover of Afghanistan in

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[1] Jackson (2018)

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2021 in the wake of US staged withdrawal from the country. Many observers were puzzled how a fairly unpopular insurgent group with a history of indiscriminate violence and a brutal regime in the 1990s could sweep into power with relatively little opposition. Our study indicates that although the Taliban may not be widely revered, they have likely built up a reserve of goodwill and perceptions of some competence through local service provision, especially around judicial and related governance. Leveraging religious justifications and strong understanding of local norms, along with widespread dissatisfaction with the foreign-backed Kabul government, the Taliban were able to convince many Afghans that they were the 'less bad' option for governance going forward.

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.[2] 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). Where those processes successfully resolved disputes, our results suggest they may have shifted the political and battlefield conditions in favor of service-providing groups.

As in other contexts, Taliban courts are a 'bundled treatment' that includes soliciting cases, adjudicating cases and enforcing judgments. Some coercive capacity is a necessary condition for courts to resolve disputes, otherwise losers would simply ignore the rulings. Coercive strength on its own, however, could not generate a self-reinforcing interest for the public to cooperate with insurgents that we describe above. We anticipate insights about the role of coercion and vested interests illustrated by the Afghan case are relevant to a number of other cases of competitive governance (i.e., where multiple political actors vie for public support through the provision of governance).

Rebels may provide a range of public goods, not only dispute resolution and other judicial services. How well might these results travel to other types of governance activities? In contrast to court decisions, were rebels to improve road infrastructure or provide health visits, those services

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[2]For more on the distinction between courts and dispute resolution or mediation, see Ginsburg (2019).

could more easily be co-opted or replaced by a competing government agency. Although any change of de-facto governance implies costly uncertainly, those rebel services that tie public goods to the insurgent group actually being in power are more likely to generate the findings we uncover.

This study contributes broadly to political science, economics and security studies, as well as having important policy implications. In addition to the important scholarship on rebel governance (Arjona, 2016; Breslawski, 2021; Stewart, 2018; Liu, 2021), the findings speak to the literature on civilian collaboration in wartime (Shaver and Shapiro, 2021; Condra and Wright, 2019; Wright et al., 2017) and the relationship between war and state-building (Tilly, 1985; Olson, 1993; Arjona, 2014).

Our study introduces new data: in addition to the information on Taliban courts, we introduce re-coded data from an Afghan NGO consortium on all kinds of conflict, disputes and crime from 2008 to 2014. We also employ newly declassified public opinion survey data and military records. Lastly, our trajectory-balanced difference-in-differences approach enhances the credibility of our quantitative results.

Our paper also makes a conceptual contribution, highlighting the informational value of conflict resolution to insurgency. In addition to 'winning hearts and minds', this type of governance gives non-state actors a rich lens into the microscale power structures of communities. This deepens their understanding of ongoing interpersonal disputes, tribal feuds, and social cleavages within villages. In this respect, prior work has focused too narrowly on the flow operational information, where civilians play a pivotal role in monitoring and sharing information of the activities of combatants. Courts provide a mechanism for unveiling another, overlooked but essential form of intelligence gathering about civilian life under conflict.

The paper proceeds as follows. Section 2 lays out the main theoretical argument, followed by a description of the Afghanistan context in Section 3, and the data and research design in Section 4. Section 5 presents the results and mechanisms. Section 6 concludes.

2   Theory: Judicial Services in Unstable Environments

In conflict-affected regions, there is often considerable uncertainty about property rights, social rules and dispute resolution. Because authority is contested between multiple groups, citizens do not know what to expect. This uncertainty provides an opportunity for actors to make expansive new property claims, such as ownership of land, access to valuable natural resources, or to carry out protection rackets or impose informal taxes. Uncertainty about the relative strengths of the claimants can lead to escalation and, often, violence. Empirically, in conflict-affected areas we see many enduring property and family disputes (Blattman, Hartman and Blair, 2014; Murtazashvili and Murtazashvili, 2021).

Individuals and families involved in disputes directly suffer from escalations, but there are also significant negative externalities for the entire community. Violence can spill over and cause collateral damage, or become generalized into inter-group conflict that affects families far from the original dispute. Without clear property rights, people may be dissuaded from making productive investments in their land or business, depressing the economy.

Residents may turn to a variety of providers to resolve these issues. Typically, there are formal and informal institutions that offer dispute resolution, such as government courts or local councils. Where there is active rebellion, however, government courts are often limited in capacity and efficiency, and local councils may not be able to enforce their judgments. By the nature of civil conflict, existing judicial processes often fail to meet the needs of the population, leaving many disputes unresolved.

Traditional authority and local customary institutions are further limited in their ability to enforce decisions when the underlying cause of social conflict is rooted in commitment rather than information problems. Mediation and dispute resolution are most effective when there is a cooperative solution to conflict. However, when disputing parties are locked into a cycle of conflict because of issue indivisibility or commitment-based problems, judgments on the merits, with enforcement, can be the only durable solution.

Responding to this gap, rebels regularly conduct their own forms of dispute resolution. Insurgent justice ranges from ad hoc decisions made by armed commanders and enforced by their

fighters, to well-organized and institutionalized court systems. Dispute resolution with enforcement provides rebels with an opportunity to demonstrate competence to the local population, enhance the productive capacity of the local economy, and to cultivate collaborators and informants among the population. Building support with the population has long been identified as a critical goal for insurgents, both by observers and rebel theorists themselves (Guevara, 1964; Marighella, 1971; Kalyvas, 2006).

2.1   Rebel Courts

Rebel groups have offered judicial services in many contexts, including the IRA in Ireland, CPN-M in Nepal, LTTE in Sri Lanka, RUF in Sierra Leone, and FMLN in El Salvador (Sivakumaran, 2009). Courts are an attractive public good for rebels to provide since they do not require fixed infrastructure and insurgents already specialize in violence, meaning the marginal cost of enforcing rules and the start-up costs of providing the service are each relatively low compared to other types of public goods (Ginsburg, 2019).

Courts provide insurgents with the tools to win support from, and simultaneously coerce, the local population. Insurgent courts, unlike other services, provide rebels with both coercive and legitimate influence over the population (Loyle, 2021). If, on the other hand, rebels wanted to minimize their governance footprint and associated costs of governance, they might establish mediation and arbitration services to provide order for the population (Ginsburg, 2019; Shapiro, 1981). The decision to establish courts suggests the ability to enforce decisions over the medium to long term is of particular interest to rebels. As such, rebel-led courts represent a bundle of governance: conflict resolution and enforcement.

Legitimacy and justice are intimately related, which fuels insurgent attempts to portray the government's legal system as unjust as a means of delegitimizing state institutions (Ledwidge, 2017). Feelings of injustice drive civilians towards insurgents, so long as the insurgency can credibly promise a better deal than the status quo. Rebel courts are a concrete method by which rebels can increase the credibility of their promise of a better overall governance alternative (Ledwidge, 2017).

Crucially, rebel courts can create a vested interest in rebel political authority, since the

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durability of insurgent court rulings relies on continued rebel control of local political institutions. Unlike material public goods that remain fixed even if political control changes hands, the legal order provided by rebel courts would be interrupted, and could not be credibly replaced, if the government regains political influence. Importantly, this type of political authority and influence is distinct from geographic sovereignty, meaning that rebels can operate courts even in areas where they lack the ability to exclude rival actors militarily. This dynamic has been used to explain judicial development historically; in medieval England, citizens knew the legal services provided by the crown might change under a new ruler, increasing support for the government relative to challengers (Simpson, 2020).

This vested interest stands in contrast to other public goods, like health, education, and transportation services. Although there are sometimes context-specific comparative advantages, in many cases government can credibly commit to providing the same, if not superior, version of these services.

Other types of rebel services may also fit the scope condition for our theory. Consider a conflict where ethnicity is highly salient, and rebels provide education services where lessons are taught in a specific language frequently spoken by an ethnic minority group that the government has historically repressed. The government (or another ruler) may not be able to credibly commit to replicating such a service in the same way as insurgents, creating a vested interest in rebel rule.

While rebel courts create a vested civilian interest in continued insurgent territorial control, they also provide rebels with a tool to coerce the population and render them more legible (Ginsburg, 2019; Scott, 1998). By establishing courts, rebels showcase their ability to detect and punish wrong-doers. Demonstrating capacity to locate and reprimand criminals is a warning to potential informants who may wish to collaborate with the government. Unlike public goods like education and health, judicial services give rebels carrots and sticks to control populations. Importantly, to the extent rebels use judicial coercion, the violence is legitimized by judicial process (Shapiro, 1981).

2.2   Consequences of Rebel Courts

The first test of any dispute resolution institution is whether it in fact resolves disputes. None of the higher strategic goals that rebels (or governments, for that matter) have will be realized unless the courts are effective at settling outstanding cases. Court decisions must be enforced, both in the short and long run, requiring personnel that can intervene should there be resistance.

If rebel courts act as hypothesized, we would expect to see a significant effect on disputes, especially those related to property. At the same time, we might expect to see an increase in non-fatal coercive behavior by insurgents, conducted in the course of enforcing court decisions.

Second, conditional on courts being effective, we would expect that the operation of judicial services should change the way that citizens think about both the insurgency and the government. Members of the population, presuming that they observe the positive effects of reduced disputes within their community, ought to feel more positively towards the insurgents and be willing to utilize rebel services in the future, likely in lieu of government services. In line with past theories of public goods driving 'hearts and minds,' effective judicial services should make citizens shift their loyalties away from the government and towards rebels.

Lastly, to the extent that rebel courts successfully resolve disputes and shift citizen attitudes towards the insurgents away from government, we expect that they should enhance rebels' fighting capacity. If civilians (even weakly) prefer continued rebel rule, they will be more likely to provide battlefield-relevant information to rebels and less likely to provide information to government forces. The literature on civilian informing in civil war contexts indicates civilian support is essential to insurgent combat operations - in extreme cases, rebels may be unable to operate in areas where citizens are hostile to rebel aims (Berman, Shapiro and Felter, 2011; Guevara, 1964; Marighella, 1971).

At the same time, rebel services may build political capital with the community. Military effectiveness is often in tension with good relations with civilians. Guerrilla tactics put non-combatants at risk, which can breed resentment and backlash against rebels' political goals. For example, when staging an ambush, it may protect civilians if a town is evacuated beforehand, yet doing so would tip off enemy forces that something was afoot. Further, optimal bomb placement

is along highly traveled paths; a group could help avoid civilian causalities if they inform civilians where they are planing on placing mines, but doing so risks the tip leaking to enemy forces (Giustozzi, 2019, 183). In some instances, the trade-off between attacks and civilian support can lead to restraints that sacrifice useful tactics. During the War in Afghanistan, "Rahbari Shura ordered a suspension of the mine campaign, to prevent losing political capital among the communities" (Giustozzi, 2019, 143). In either (or both) case(s), effective rebel courts should be expected to increase insurgent attacks and general battlefield effectiveness.

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). In terms of legitimacy, some recent evidence suggests experience under rebel rule can shape civilian attitudes (Revkin, 2021), yet evidence on the dynamic links between the introduction of rebel institutions and public opinion is limited; scholars have recently called for more in-depth research leveraging survey data to understand how rebel governance shapes civilian attitudes in combat zones, and the degree to which legitimacy has increasing returns on rebel capacity to mount attacks (Loyle et al., 2021). Corresponding research on government providing services and aid has generated mixed results: although government public goods sometimes seem to reduce violence, there is growing evidence that they can fail to make meaningful change or backfire (Crost, Felter and Johnston, 2014; Berman, Felter and Shapiro, 2018; Lyall, Zhou and Imai, 2020; Sexton and Zürcher, 2021). Our study builds on this empirical work by providing a theoretical argument for why a particular governance strategy - dispute resolution - can shape attitudes and combat capacity, while providing tests with new data on the case of the Taliban.

3 Context: Afghanistan

We use the case of post-2001 Afghanistan to empirically test the effectiveness and mechanisms of rebel judicial services – in this case courts operated by the Afghan Taliban. Non-state armed groups have long been a feature of the Afghanistan context, and the Taliban are not the first group to provide governance services. 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.

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 9/11/2001, 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.

From the start, the Taliban sought to retake control of the country more broadly, rather than only certain regions. The development of a rival justice system was a key mechanism by which the Taliban sought to provide services to the civilian population (Baczko, 2021). In this case, rebel courts were not the only activities of the group's 'shadow government,' but they were among the most important.

3.1 Taliban Courts

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."

We approach Jackson's claims in several ways. First, using documents produced by the Special Inspector General for Afghanistan Reconstruction (SIGAR), we can track trends in Taliban control prior to the sample we study. Before the data collection teams went to the field, the Taliban only have substantial control over nine administrative districts. This contrasts sharply with the 93 districts where courts were present in 2011, suggesting that territorial control emerges after governance, rather than preceding it. Second, we leverage respondent classification of local political influence to reweight our trajectory balancing approach. We find that although there are small changes in the point estimate, the overall effect of Taliban courts on primary and secondary outcomes is strongly maintained. In line with this approach, we also show that there is no difference between districts with Taliban courts and without (in 2011) regarding missingness on sensitive political questions, another important indication that neither differential social desirability bias nor Taliban influence are driving the results we find (SI G).

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 1. allows the Taliban to use popular religious appeals to justify their rulings and 2. incorporates local custom into decision making. These features mean the Taliban's courts can both base their decisions on criteria that local townspeople find agreeable and are more familiar in terms of procedure than Western-based rules and norms which the Afghan government uses to adjudicate disputes.

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 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 reading 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.

For example, in rural Andar district of mixed-ethnicity Ghazni province, 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 neighborhood, 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.[4]

The Taliban's court system gathered information about crimes via villagers who report facts to insurgents for the trial (Giustozzi and Baczko, 2012). The network of spies for criminal cases had a dual use; the Taliban could rely on these individuals to report on collaborators with the government, which the court long considered a crime (Giustozzi, 2019). As such, the judiciary served to institutionalize insurgent's coercive apparatus. Courts also served as a means for civilians to lodge complaints against insurgents - cadres who violate the code of conduct could be tried by

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

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

the Taliban justices for their crimes against civilians (Giustozzi, 2014).

Taliban courts, like other judiciaries, mainly resolved civil cases, such as land disputes, property 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).

By introducing courts and resolving civilians' problems, the Taliban created a constituency for their continued dominance of a district: "[r]esolving cases created a relationship of dependence between a portion of the population and the authority that administered justice - the Taliban" (Baczko and Giustozzi, 2014). This is for two reasons - first, 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). Second, verdicts had community-wide impacts which created social peace, which only the Taliban could enforce in the short-term: "...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).

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 said 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). 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.

4 Research Design

In this section, we describe the data used in our study as well as the empirical strategy employed to investigate the impact of rebel-led courts on civilian attitudes and conflict intensity.

4.1 Data

4.1.1 Courts

To assess this argument, 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.[5] 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 1.

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). In the empirical analysis we treat the withdrawn courts in four ways:

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

excluded, as treated, as control, or as a separate category. These choices do not affect the results, in part because the withdrawn courts are a small proportion of the total.[6] We present the results comparing districts with withdrawn courts to control districts in the results section. Districts with withdrawn courts do not look systematically different from the average court-treated district in the country.

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 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, and should bias our results away from a finding.

4.1.2 Conflict and Insurgent Behavior

We rely on two data sources to measure disputes, insurgent coercion, and combat: ANSO and SIGACT.

Afghan NGO Safety Office (ANSO)

To track disputes, crime and insurgent violence, 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. Although inevitably the ANSO data collection missed incidents, likely from the most rural areas, we think that relative to the treatment of interest here – Taliban courts – there would be only classical measurement error. Importantly, the Taliban during this period did not discourage or target NGOs or aid agencies; on the contrary, they

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[6]See details in Appendix E.

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

[8]The authors are grateful to Renard Sexton for sharing data from ANSO beyond what was already published in Sexton (2016).

Figure 1: Taliban Courts Over Time and Space
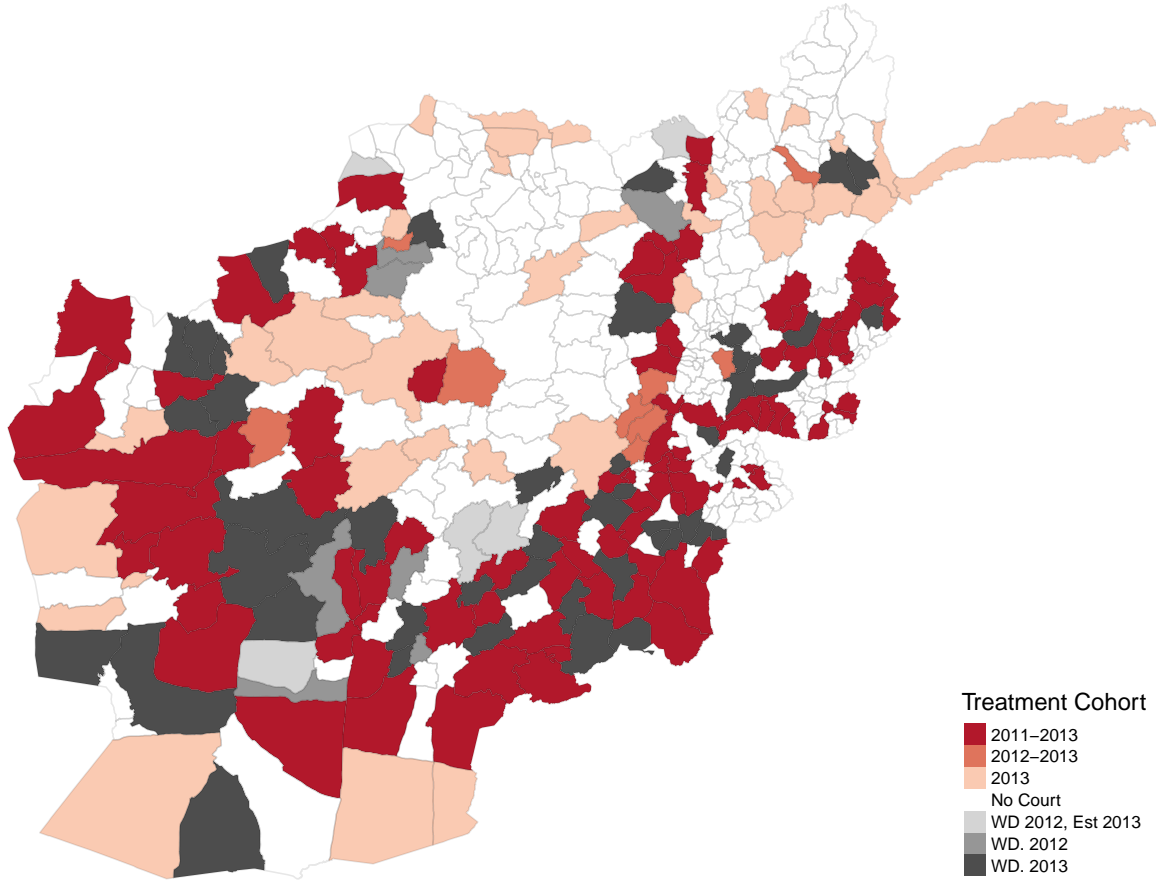
Note: Map showing distribution of Taliban judicial activity. Boundaries are 398 Districts of Afghanistan.

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."

US Military Significant Activities (SIGACTS)

We use declassified data collected by members of the International Security Assistance Force (ISAF) and their Afghan counterparts as published by Condra et al. (2018). 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.

4.1.3 Public Opinion: Afghanistan Nationwide Quarterly Assessment Research (AN-QAR)

Our survey data comes from the United States Central Command, 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.

The survey is stratified by province, covering all of the 34 first-level administrative units in Afghanistan. The administrative district is the primary sampling unit.[9] Sampled districts are selected via a probability proportional to size approach. After districts have been selected for

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[9]One 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.

sampling, secondary sampling units composed of villages and settlements are randomly selected.[10] 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.[11]

To measure respondents' support for the government, we use principal components analysis (PCA) to build an index of pro-government sentiment. Questions used for indices are asked consistently across waves from 2008-2014 (question wordings are in the SI A). The key advantage of PCA is we can collapse multiple questions into one information-rich variable, which hedges against false discovery from multiple hypothesis testing if we analyzed twenty questions separately. The index includes questions about government performance across security, the economy, development, corruption, and overall performance, general sentiment about whether or not the government is going in the "right direction," and whether the respondent believes the government is influential in the district. To hedge against the possibility that security-based sentiments drive our results, we show a version of the index that only includes non-security based questions is highly correlated with the broader index ($\rho = 0.98$) (SI A.1).

ANQAR surveys also ask Afghans if 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.

Third, to capture civilian approval of the Taliban, 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. Lists of questions and coverage across time are included in Appendix A

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

[11]Prior work by Condra and Wright (2019) indicates that response rates in ANQAR are on par with other large surveys in more peaceful contexts.

4.2 Empirical Strategy

Our panel covers $i \in \{1, 2, ...339\}$ districts over the years $t \in \{2008, 2009, ...2013\}$.[12] Districts belong to one of four treatment cohorts $\text{Courts}_i \in \{2011, 2012, 2013, 0\}$, meaning courts are either first observed in 2011, 2012, 2013, or never (0) in our sample. As described in Section 4.1.1, we restrict our attention to districts that either never received courts, or received courts in an absorbing way, excluding cases where Taliban justice switched on and off intermittently to avoid biased comparisons. In Appendix E, we show our main results are robust to including these districts as having been exposed to courts in 2011 or as being part of the control group.

We use variation in exposure to courts to estimate the impact of the Taliban's judicial services. The introduction of these courts is a bundle of governance dynamics, including the presence of Taliban judges, their decisions, and the use of coercion to enhance compliance with rulings. The quantity of interest is the average treatment effect on the treated (ATT), which is the difference between the outcomes among districts that received courts and counterfactual where the districts that received courts never had them introduced. Our focus on districts is theoretically appropriate, as the Taliban's governance system is administered at this level.

Our approach is a difference-in-differences design, where the change among court districts after courts are introduced is compared to the change among districts that never received courts. The core identifying assumption behind difference-in-differences is common trends, meaning the average change in the non-court potential outcomes is the same among districts that received courts and those that did not. Assuming a common (or parallel) trend, the change among the non-court districts is a valid counterfactual for the trend court districts would have followed, since the only time-varying difference between cohorts is the introduction of courts.

A key barrier to identification is selecting a valid comparison group which satisfies common trends. Standard panel fixed-effect approaches, which are common in the literature, likely yield biased estimates if it was the case that court allocation selected on trends. For instance, Arjona (2016) suggests rebel institutions are more likely when the time horizon of rule is longer, meaning

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[12]Our ANSO data begin in 2008 and do not cover the entirety of 2014, therefore we exclude this year from our analysis and measure only up to 2013 for conflict outcomes. To maximize comparability, we focus on this same time period with SIGACTS, although SIGACTS has broader temporal coverage. We show robustness to considering the longer panel with SIGACTS.

less violence should predict more rebel provision of goods. In this case, a two-way fixed-effects regression of conflict on court introduction would be downward biased (towards zero), since the increase in conflict after courts may be washed out by the pre-existing decline on conflict before courts. Further, fixed-effects difference-in-differences approaches require that past outcomes do not influence future treatments, a requirement which is violated if the Taliban set up courts on the basis of security or attitudinal trends (Imai and Kim, 2019). We think selection on trends in violence likely occurred in our case since, as Giustozzi has noted, court provision relies in part on the ability of Taliban forces to 'clear and hold' communities where judges are present.

To credibly estimate the impact of courts in light of diverging pre-trends, we use a trajectory balancing design, which reweights the comparison group that never received courts to match pre-intervention trends (Hazlett and Xu, 2018). First, a set of weights w_i is selected such that the weighted average of district outcomes which did not receive courts (control) is mean-equal to the average of districts that did receive courts (exposed) before the courts were introduced. The selection of weights is done by using the first P eigenvectors of the matrix containing pre-treatment outcome histories, where P is chosen automatically to bound worst case bias (Hazlett and Xu, 2018). This feature of our identification approach departs from other methods which require longer pre-treatment histories.[13] Second, the post-treatment average outcomes of districts exposed to courts are compared to the weighted average of control districts in the post-treatment time period, using the weights computed in step 1. Equation 1 displays our estimator.

$$\widehat{ATT}_t = \frac{1}{N_{\text{Court}_i \neq 0}} \sum_{\text{Court}_i \neq 0} y_{it} - \sum_{\text{Court}_i = 0} w_i y_{it} \text{ with weights s.t.}$$

$$\frac{1}{N_{\text{Court}_i \neq 0}} \sum_{\text{Court}_i \neq 0} y_{it} = \sum_{\text{Court}_i = 0} w_i y_{it} \text{ for } t \leq \text{Year Before Treatment} \quad (1)$$

$$\text{subject to} \sum_i w_i = 1 \text{ and } w_i > 0, \forall_i$$

Our core identifying assumption is a district's potential outcomes are independent of receiv-

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[13]For a longer description of the approach see Appendix B. In brief, mean balancing uses a linear kernel to weigh control units to match the trends of treated units, following Hazlett (2020).

ing a court, conditional on the pre-court outcome history (Hazlett and Xu, 2018).[14] After matching treated units to a weighted control group that has the same pre-court trajectory of conflict or attitudes, we assume changes in the outcome between court and non-court districts can be attributed to judicial services.

The advantage of our approach is our selection of control units is data-driven, which creates a formalized and transparent approach to creating our comparison group. Relative to regression-based approaches, our selection of weights for control units is guaranteed to be non-negative by design, and the counterfactual is constructed from the data rather than projected out.

Characterizing uncertainty is complicated by re-weighting, which is itself subject to uncertainty. We follow the procedure in Hazlett and Xu (2018) for constructing standard errors by using the jackknife, wherein each treated unit is systematically excluded, the treatment effect is re-estimated, and the standard deviation of effects is used for standard errors . We show our main results are robust to a block bootstrap allowing for overtime correlation of the errors (Appendix D). Although distinct from clustering, this procedure produces standard errors that account for the clustered nature of the data (i.e. effectively clustered at the district level). We also report p-values from a randomization inference exercise which reshuffles the cohort vector and calculates the share of absolute placebo effect sizes (Appendix D.2.)

5   Results

In this section we present our main findings, followed by an exploration of mechanisms. First, we examine how Taliban courts affect violent disputes, property disputes and criminal activity, a critical test of whether these courts indeed provide meaningful change to communities. Second, we trace the effects on public opinion: if courts and their effects on local quality of life are real, this should show up in public attitudes. Third, we check how courts change combat outcomes, expecting that winning support in the population and showing legitimacy should boost the ability of the insurgency to carry out anti-NATO and anti-government attacks. Lastly, we look at several potential mechanisms, helping to verify that the effects we find are indeed occurring through the channels our theory would predict.

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[14]Other assumptions include the feasibility of balancing weights and linearity in prior outcomes, see Appendix B.

Figure 2: Extensive Margin of Disputes and Land Disputes: Trajectory Balanced

(a) All Disputes
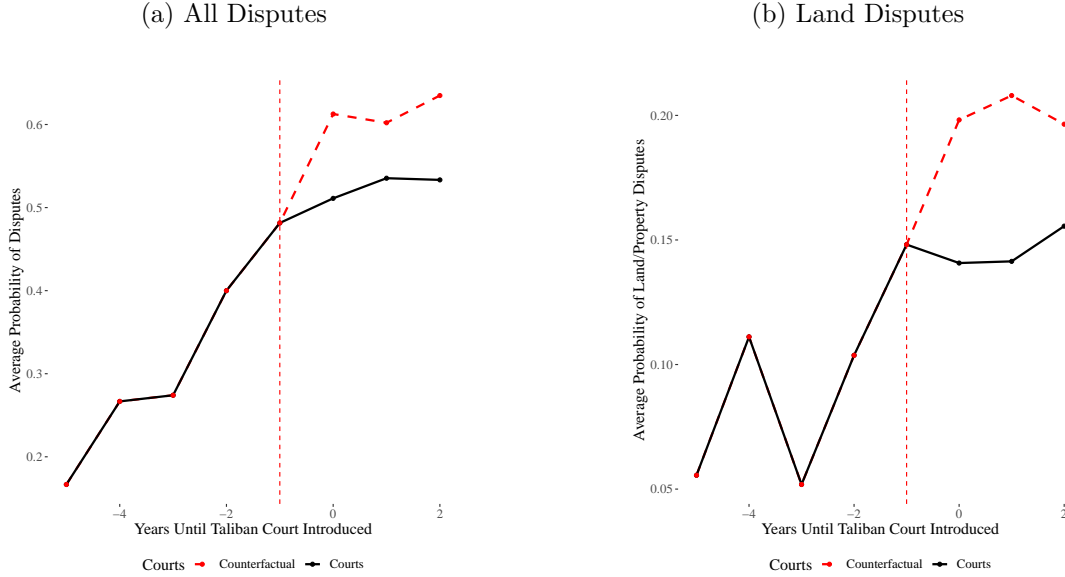
(b) Land Disputes

Note: Average probability of disputes and land disputes over time for districts 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. 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 in both figures is the average probability of an event. Panel A shows the average probability any dispute is observed in a district-year and Panel B shows the average probability a dispute is over land or property specifically. 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.

5.1 Dispute Resolution

To assess whether Taliban courts are effective at resolving disputes, we rely on detailed records of interpersonal disputes from our ANSO data. We look specifically at three categories of disputes that the organization collected, which we coded using the text of the incidents. Our first outcome is all violent disputes, that is, disputes that were significant enough to be recorded by ANSO's trackers. The second income is specifically property disputes, which is a subset of the first outcome that is typically related to land and other major assets. Third, we look at criminal activity, including burglaries, robbery, kidnapping, assault and homicide, as reported in the ANSO database.[15]

Figure 2 illustrates the overall relationship we find. In Panel A, we see that as compared to the counterfactual (no courts) districts, we find a significantly lower probability of a major active

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[15]We use ANSO data on crime and event descriptions about disputes to create a separate "land dispute" category.[16] Since the land dispute measure is infrequent, we prefer a binary measurement, whereas crime (as an umbrella category) is more common, leading to a measure of the natural log of crime events per district-year.

Table 1: Trajectory Balanced Dispute Resolution and Crime Results

Outcome	Disputes		Property Disputes		Crime	
	(1) Log(+1)	(2) Binary	(3) Log(+1)	(4) Binary	(5) Log(+1)	(6) Binary
Taliban Courts	-0.12^*	-0.09^{**}	$-0.04^\dagger$	-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. $^{***}p < 0.001$, $^{**}p < 0.01$, $^*p < 0.05$, $^\dagger p < 0.10$

dispute in districts with Taliban courts. Focusing on land disputes, Panel B shows that these property issues represent the bulk of the reduction achieved by Taliban justice.

In Table 1 we see that the effect is about 10 percentage points (20%) for total disputes and 6 percentage points (40%) for property disputes. In contrast, it appears that Taliban courts have no average effect on criminal activity like robberies. Consistent with our theoretical argument, we find that the introduction of Taliban-led courts reduces the likelihood and intensity of interpersonal disputes.

5.2 Public Opinion

We next turn our attention to the impact of Taliban-led courts on public opinion. In particular, we are interested in assessing three dynamics. How do rebel public services in the context of contested sovereignty impact perceptions of government strength? Relatedly, if Taliban courts undermine government strength, are they associated with a shift in the utilization of government services, especially conflict resolution? At the same time, do Taliban courts influence approval for their broader political agenda?

Figure 3: Public Opinion Towards State and Insurgents: Trajectory Balanced

(a) Gov. Index

(b) Use Gov. Courts

(c) Support Taliban Return
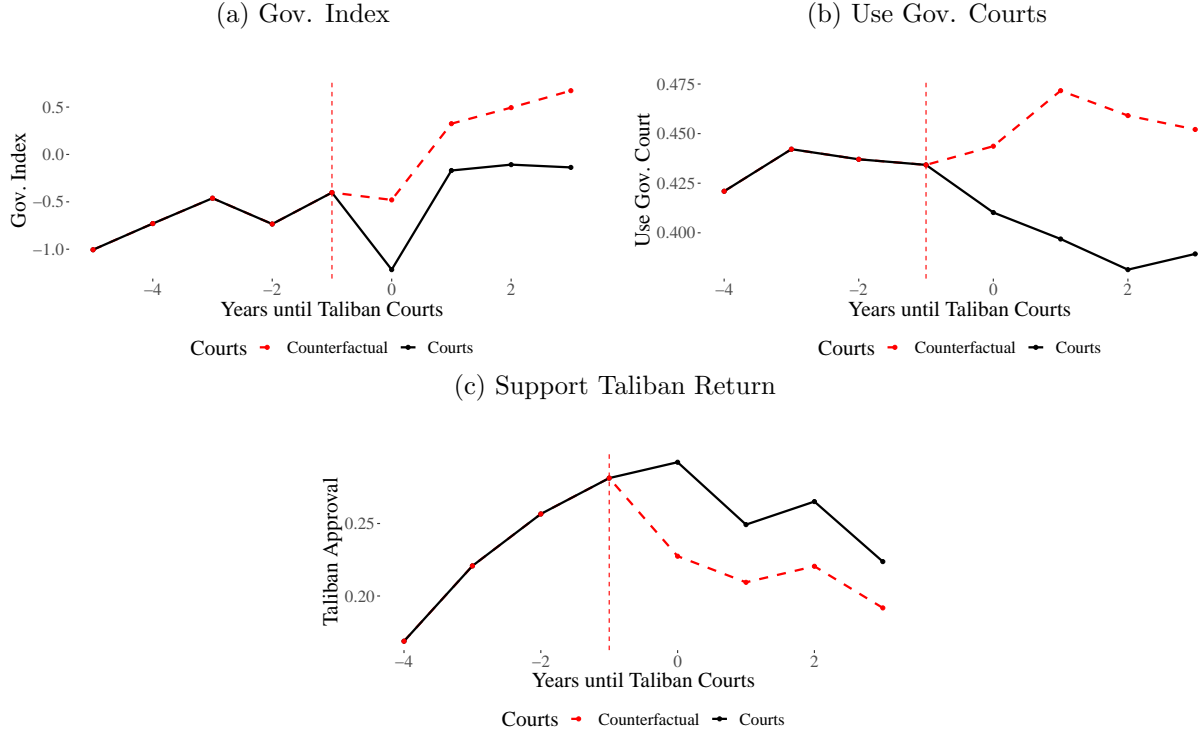
Note: Average public opinion trends (Government strength in Panel A, Reported willingness to use a government court in Panel B, and Approval of the Taliban in Panel C), among districts in Afghanistan that received courts versus the counterfactual trend. Counterfactual trend constructed from the weighted average of districts that did not receive courts, with weights selected subject to the balancing constraint in equation 1 and explained in Section 4. 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.

We study these questions using a battery of survey items. Trajectory balanced trends are illustrated in Figure 3. Solid lines are the average outcomes for Taliban court districts overtime (with years until the court was first observed serving as the time variable). Dashed lines are the estimated counterfactual trend, which is the weighted average of non-court districts using the weights from equation (1). Overall, civilians in districts with Taliban court provision (black, thick line) report significantly lower perceptions of government strength and are significantly less likely to use government courts. In the third sub-figure, notice that Taliban approval broadly increases with court provision relative the balanced counterfactual.

We investigate these trends further in Table 2, where the columns correspond to the sequence of sub-figures. Notice that the trends visualized in Figure 3 are robust and precise, suggesting that

Taliban court provision reduces the government strength index score by 0.3 standard deviations and government court utilization by 6%. Court provision also significantly improves approval for the Taliban more generally, increasing approval by 5%.

Table 2: Trajectory Balanced Public Opinion Results

	Gov. Strength Index	Use Government Courts	Taliban Approval
Taliban Courts	-0.65***	-0.06***	0.05**
	(0.12)	(0.02)	(0.02)
Dataset	ANQAR	ANQAR	ANQAR
N. Districts	187	149	149
N. Years	7	6	6
N. Obs	1309	894	894
Standard Deviation DV	1.80	0.19	0.19
Mean DV (Control)	0.13	0.46	0.18

Note: All outcomes are aggregated from the individual level to the district year. All outcome data is from ANQAR (see section 4.1.3). Government (Gov.) Strength Index is constructed through principal components analysis, combining twenty questions on government performance (see A). Use Government Courts is the share of respondents in a district year who report they would use government courts to resolve a dispute. Taliban Approval is the share of respondents indicating that a return to Taliban rule would be good for the country. Standard errors nonparametrically estimated with a jackknife procedure (Hazlett and Xu, 2018). ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$, †$p < 0.10$

5.3 Armed Conflict

Finally, we explore the impact of public service provision on Taliban fighting behavior. We anticipate several potential dynamics could emerge following the introduction of rebel-led courts. First, if, as we point out earlier, Taliban courts reduce government strength, pry civilians away from government institutions, and improve approval for their broader political agenda, courts may enable more effective recruitment. Courts may also trigger a shift in public material support for insurgents, leading to an increase in resources available to rebels as they compensate, train, and maintain their combat forces. These changes would reinforce the military capacity of insurgents. This change in rebel capacity might then increase the intensity of combat in areas where the Taliban have provided courts. Second, if the allegiance of civilians changes, this could reduce their willingness to engage in information sharing, lowering the operational effectiveness of government forces. This might lead to an increase in the effectiveness of attacks deployed by insurgents, causing government and coali-

tion casualties to increase. Violence might also increase through a third mechanism: attempts by government forces to undermine rebel institution building. If government forces were strategically deployed to combat insurgents in places where they have established court systems, violence would increase but so too would the scale of government forces and the tempo of their operations. The survey evidence presented earlier cuts against this potential mechanism since, as we have noted, civilians perceive a decline in government strength.

Figure 4 illustrates the relationship: we find that violence significantly increases after the introduction of government courts. We observe consistent patterns across various measures of combat activity and distinct data collection platforms. The consistent increases in combat across databases suggest our results are not an artifact of one particular collection methodology used by either ANSO or SIGACTS. We also observe an increase in the casualties associated with these attacks.

The trajectory balanced estimates are presented in Table 3. The uptick is robust and precise, with the increase in violence representing a 0.1 standard deviation increase in combat intensity at minimum. Panel A reports results when the outcomes are measured as levels, meaning the count of events per district year, and Panel B reports results where the outcome is logged. Irrespective of how armed conflict is parameterized, we find a consistent positive effect of courts on armed conflict across different measures of combat.

The short-run effects of courts on combat that we document may differ from the long-run or persistent effect of rebel dispute resolution across localities. The long-run effects could be negative: courts might enable rebels to consolidate control, reducing need to rely on violence to push out rivals (the state). During the 2015-2020 time period, we may find that the level of combat between court and non-court districts convergences or even begins to decline since the Taliban may face less contestation by coalition and Afghan forces after courts have created a strong bind between civilians and insurgents. Nonetheless, our results suggest that the legitimacy boost that rebels receive as a result of judicial services facilities a short-term increase in conflict that persists up to three years.

Figure 4: Armed Conflict Trends: Trajectory Balanced

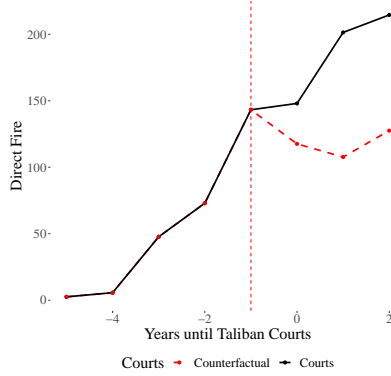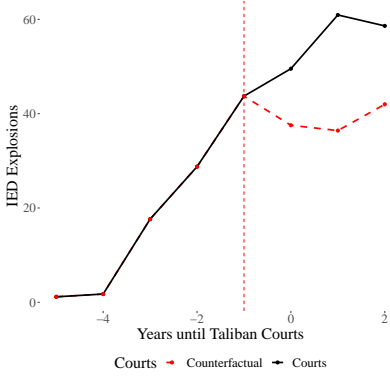(a) Armed Opposition Group (AOG) Events (ANSO)   (b) IEDs (ANSO)
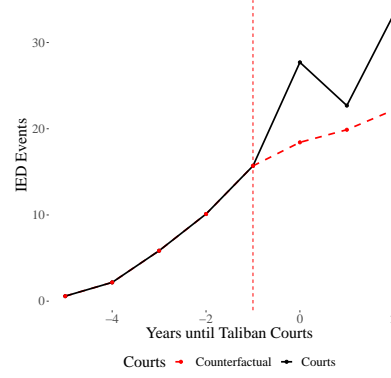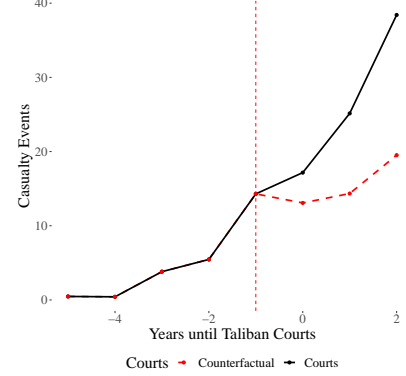

(c) Direct Fire (SIGACT)   (d) IED Explosions (SIGACT)   (e) Casualty Events (SIGACT)

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 causalities 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. 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.

5.4   Sequencing of Effects

Our theoretical construction and interpretation of the empirical findings is that Taliban courts reduce disputes, which causes a shift in public opinion, and in turn increases violence. An alternative interpretation of the findings would be that rising Taliban violence is not an effect of the courts but instead part of a coordinated strategy to roll-out both judicial activities and a military offensive in target districts. Could it be the case that fewer disputes and shifts in public opinion are the

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Table 3: Combat Outcomes

	AOG Events	IED Events	Direct Fire	IED Explosions	Total Casualties
Panel A: Count of Events					
			Count of Events		
Taliban Courts	11.4598**	7.8056*	65.4832*	17.1066**	10.2508***
	3.6492	3.0642	31.2802	6.3173	2.9585
Panel B: Natural Log of Events					
			Ln(Outcome+1)		
Taliban Courts	0.1742**	0.1741***	0.2214***	0.1739**	0.2604***
	0.0562	0.052	0.0611	0.0592	0.0572
Dataset	ANSO	ANSO	SIGACTS	SIGACTS	SIGACTS
N. Years	6	6	6	6	6
N. Districts	339	339	339	339	339
N. Obs	2034	2034	2034	2034	2034
Standard Deviation DV (Levels)	44.42	33.34	251.12	72.3	28.33
Mean Control DV (Levels)	16.39	11.61	28.1	12.29	4.4
Standard Deviation DV (Log)	1.57	1.43	1.9	1.64	1.27
Mean Control DV (Log)	1.5	1.39	1.7	1.47	1.1

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Note: Table reports the effect of Taliban courts on armed conflict using five outcomes: incidents involving armed opposition groups (AOGs) from ANSO (sans events involving IEDs), incidents involving improvised explosive devices (IEDs) from ANSO, direct fire attacks from SIGACTS, IED explosions from SIGACTS, and total causalities from Afghan and Coalition forces. Estimates are from equation 1. Panel A shows results when outcomes are counts of events, e.g., the number of IED explosions or the number of direct fire attacks. Panel B: All outcomes are log(+1) of counts at the district year, e.g. the natural logarithm of the count plus one. Standard errors nonparametrically estimated with a jackknife procedure (Hazlett and Xu, 2018).

consequence of this combined strategy, with violence as a cause rather than an effect?

To test this alternative, we look to the historical record and to a subset of districts where Taliban courts were briefly introduced, but later withdrawn. If the alternative narrative is correct, violence would have immediately increased in these districts corresponding with the initiation of the courts. However, we observe no such effects – in the withdrawn courts we see a small reduction in disputes and small movement in public opinion, but no significant change in violence (see SI F.3 for details).

In addition, qualitative evidence suggests that this sort of coordination was not occurring at the time. Because the Taliban was experiencing shortages of money and manpower, courts were in general a substitute rather than a complement for military commitments (Giustozzi, 2019). If indeed there is substitution going on, i.e. the Taliban either sent courts or additional troops to each district, then our estimates may underestimate the effectiveness of the courts in terms of violence generation because the 'control' areas would be artificially high from the injection of troops. Thus, our findings may in fact be a lower bound for the violence-enhancing effects of rebel courts.

5.5 Courts or Control?

A logical alternative story is that courts flow from Taliban territorial dominance, and that the authority of the Taliban in the area drives our results rather than judicial institutions themselves. This alternative account, however, ignores qualitative information about the case: during our period of study, the Taliban lacked territorial control, controlling just 9 districts around the time period were over 90 courts are reported to have existed. As explained by Jackson (2018), courts preceded territorial control, they were not a consequence of it.

To empirically test this alternative explanation, we use ANQAR data from 2008-2010 which tracks civilian perceptions of Taliban control in their village. We use the average perception of Taliban control pre-treatment as a covariate to adjust our trajectory balancing estimates. Substantively, the point and uncertainty estimates change very little, which validates the intuitions of experts in the field that courts helped the Taliban gain control, rather than arriving because of preexisting influence (Appendix I)

5.6 Mechanisms

5.6.1 Taliban Coercion

As the Taliban increase the number of judgments that they issue, how do they enforce compliance with their mandates? The main results on disputes indicate that Taliban courts indeed reduce the overall number of active land and property conflicts in districts where they operate, but is this just through force of will or legitimacy of their rulings? Especially in an active war zone, we think this is unlikely.

To examine how the Taliban enforce their rulings, we take advantage of reports logged by NATO troops regarding attempted intimidation by Taliban insurgents. Nearly all of these incidents are threats, rather than actual violence.[17] The data record non-combat related killing of civilians by insurgents for the purpose of intimidation. Anecdotal reports from these intimidation incidents suggest that in many cases Taliban fighters are obliging civilians to follow Taliban 'rule of law,' as they see it. Although many are enforcement of general Taliban rules on social issues like grooming, or stark reminders not to collaborate with foreign forces, we suspect some of these coercion episodes are specifically related to Taliban court decisions. Importantly, the use of coercion is a mechanism for establishing and consolidation of the judicial authority of rebel-led courts in the presence of a state alternative. As such, an increase in Taliban coercion that coincides with the introduction of rebel courts is an observable implication of our argument rather than a confounding factor.

In Table 4, we find that indeed, 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 4: Insurgent Intimidation Results

Outcome	Intimidation (Count)	ln(Intimidation+1)	1(Intimidation)
Taliban Courts	0.45**	0.18***	0.15***
	0.15	0.04	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

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

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

An increase in coercion of civilians along with increased public approval of the Taliban may

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[17]In our sample, there are 546 intimidation events compared to 1156 threat events. We pool both outcomes to capture the overall level of insurgent coercion of the civilian population.

seem to be contradictory. However, this finding is consistent with both the state-building and courts 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 (Bates, Greif and Singh, 2002; Hirshleifer, 1995). Our results are consistent with the idea that once the Taliban become the legitimate guarantor in a given space, they use their legitimacy to use force to increase their control over the population through coercion. 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.

Moreover, the increase in insurgent intimidation of the civilian population is consistent with the "dual use" nature of judicial institutions in a combat setting. Courts are also used to prosecute war crimes, and collaborating with the enemy is considered a crime by the Taliban. Since intimidation events include "the killing of informers who report to ISAF/Government of Islamic Republic of Afghanistan" according to SIGACTS, another potential explanation for our finding is that courts confer the legitimacy to use lethal force against civilians by providing a seemingly fair procedure for trying and punishing citizens accused of cooperating with the enemy. Courts may make the punishment of collaborators easier for the Taliban by signaling that their use of force is reasonable rather than capricious, reducing the risk of alienating the population when lethal force is used against the accused.

Whereas our data is not detailed enough to discriminate between the increase in intimidation coming from enforcement of decisions or persecution of collaborators through legal procedures, our findings suggest courts may help facilitate insurgent control by increasing their ability to use coercion against the civilian population.

5.6.2 State-Engagement and Civilian Reporting

Our argument suggests use of Taliban courts should reduce citizen willingness to collaborate with coalition forces because of vested interests in rebel rule conditional on receiving a verdict from an insurgent court. ANQAR data allows us to test our argument 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. When insurgents have tenuous support in an area, they must choose between keeping the location of IEDs secret, risking civilian causalities which both risk further blowback and result in an inefficient use of resources for terrorizing the enemy, or they may inform civilians about where IEDs are to minimize the causality risk while opening up the possibility that civilians may inform coalition forces.

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.

Consistent with our expectation, results in Table 5 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

Table 5: IED Reporting on Court Usage

Likelihood of Reporting IED	Binary		Categories	
	(1)	(2)	(3)	(4)
$\mathbb{1}$(Use Government Court)	0.19***	0.14***	0.32***	0.45***
	(0.01)	(0.02)	(0.04)	(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, age^2, 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. Table with controls included in SI F.2

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.

5.6.3 Heterogenous Effects on Public Opinion

Because the Taliban was established as a primarily Pashtun organization that includes traditional rules often associated with Pahstunwali in their interpretations of Sharia, we might expect the public opinion effects of Taliban courts to be more pronounced or driven primarily by Pashtun citizens. At the same time, the 'vested interest' mechanism might operate more strongly for high economic status families – who have more to lose in disputes, or among men rather than women, since Taliban rule has severe gender disparities. We find that, in fact, there are no differences in effects between ethnic groups, socio-economic groups, or reported sex of respondents. Details on this analysis are in SI F.1.

5.7 Are Respondents Persuaded or Fearful?

A potential concern is our survey results are driven by preference falsification – it may be the case that Afghans are not swayed into supporting the Taliban upon the introduction of courts, but are instead frightened of retaliation should they truthfully express their distaste for the insurgents and support for the government. This concern is muted for three reasons. First, courts signify an increase in rebel governance, which does not necessarily correspond in an increase in control. Second, if the result was driven by preference falsification, one may expect a very large number of respondents expressing support for the Taliban, as is typically the case when civilians are coerced into expressing support in public opinion polls in the case of dictatorship (Kuran, 1991). Yet, this is not the case, as we observe only 26% of respondents expressing support for a return to Taliban rule in treated districts. Third, we test for the possibility that our results are driven by fearful survey respondents. If it was the case that the Taliban intimidated civilians into answering a particular way, one would find that more respondents in treated districts would refuse to answer questions or simply say they do not know rather than engage in the survey. We find that survey non-response and disengagement is uncorrelated with courts across a range of survey items, which provides evidence against this interpretation. Finally, we filter the data to study responses only among those respondents who are highly comfortable with answering the survey questions. This subpopulation would be unlikely to report false beliefs based on intimidation, otherwise they would be recorded as uncomfortable. We obtain similar estimates within this group as we do in the baseline, providing further evidence that our results are from persuasion rather than pure intimidation (SI G).

6 Conclusion

"President Joe Biden and other top U.S. officials were stunned on Sunday by the pace of the Taliban's nearly complete takeover of Afghanistan" (Associated Press, August 15, 2021)

As the Taliban entered the Presidential palace in Kabul in August 2021 to take control of the country, journalists, diplomats and the global public gawked and asked "how did this happen?" Although the Taliban had a large, battle-hardened fighting force, they were dwarfed by the larger and better equipped US-backed Kabul government, which NATO expected to be able to hold out

for many months, if not years.

In this study, we argue that whether and how rebels govern during conflict plays a crucial role in shaping civilian attitudes, social conflict, and political violence. We focused specifically on how rebel-led courts can address conflicts between civilians, noting that conflict resolution mechanisms create institutional dependence. This institutional dependence reinforces the political authority of non-state actors, driving a wedge between the governed and the government.

Our research leverages a rich set of microdata on the location and operation of Taliban-led courts in Afghanistan during a critical phase of the international occupation. Combined with original measures of property and violent crime as well as civilian attitudes and combat activity, we explore how the timing of rebel governance influences civilian behaviors, preferences, and the intensity of political violence. We find that property crime declines following the introduction of Taliban courts and civilian attitudes towards state-led courts and institutions significantly worsen. We also find a significant worsening of combat activity in areas where the Taliban establish courts. This change is consistent with a long-theorized link between civilian collaboration – the willingness to provide information about insurgent threats to government forces – and government-led combat missions.

Taken together, these findings highlight the importance of insurgent service provision, especially those that build a vested interest for citizens in rebel rule, with important implications for both scholars and policymakers.

We attribute these changes in behavioral and attitudinal outcomes to the operation of Taliban courts rather than some other factor that could co-vary in time and location as the courts. We are able to largely rule out an alternative explanation that the violence effects were the result of an ex-ante coordinated push by the Taliban, rather than as a consequence of the courts.

Although we acknowledge that it is possible that there is another feature of the setting that explains both the presence of the courts and the outcomes we observe, a convincing alternative explanation would need to account for all of the outcomes holistically. For example, a competing account would need to simultaneously explain changes in public opinion, combat outcomes and explain why property disputes change but violent crime does not. Similarly, an alternative account

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would need to have similar effects across socio-economic strata and ethnic groups. We argue the most plausible explanation is that rebel-led courts shaped the character of local governance, offering opportunities to resolve intra-communal disputes, breaking the ties that bind civilians to state authority.

To what extent were Taliban courts popular with civilians because of valence considerations, versus people being ideologically aligned with the decisions the courts were rendering? Put another way, is the typical Afghan respondent in agreement with the Taliban's approach to justice or were people simply glad that disputes were resolved, regardless of the resolution? One interesting feature we identify is that Taliban courts appear to have had very similar public opinion effects for people from each of the major ethnic groups and among both high and low status individuals. This suggests that ideological comportment was less important than the valence consideration – people likely were just happy to have property disputes off the table. This evidence also cuts against the idea that the changes in public opinion we detect are not 'real' movement but instead shifts in response strategies among 'latent' supporters, who are less subject to social desirability bias once courts are operational. If this were true, we would expect to see much bigger effects among Pashtuns.

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. How did this play into the broader fight to establish political authority on by the Taliban and the former Government of the Islamic Republic of Afghanistan (and its foreign allies)?

The seeds of rebel governance likely shaped these dynamics, establishing and consolidating the Taliban's political authority nearly a decade later. For example, the earliest provincial capitals to fall to Taliban rule in non-Pashtun areas during the August 2021 offensive were all located in districts that hosted Taliban courts in 2011-2013.[18] Although we leave a more thorough treatment to future research, early efforts to resolve low-level conflicts may have enabled the Taliban to

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[18]Aybak, Samangan province, captured August 9, 2021; Puli Khumri, Baghlan province, captured August 10; Chaghcharan, Ghor province, captured August 13; Qala-E-Naw, Badghis province, captured August 13. Al Jazeera. "Afghanistan: Mapping the advance of the Taliban." 08-15-2021. https://www.aljazeera.com/news/2021/8/15/afghanistan-mapping-the-advance-of-the-taliban-interactive

effectively showcase an alternative model of governance and authority, easing the path to conquest on the road to Kabul.

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Online Appendix: Supporting Information

A Survey Questions from ANQAR

A.1 Government Strength Index

Table A.1: ANQAR Survey Questions: Government Strength Index

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 Assessment (18 Total Questions)
Generally speaking, do you believe the Government of Afghanistan is going in the right direction, the wrong direction, or is in the same place, not going anywhere?	Wave 1-Wave32	Progress Trend (1 Total Question)
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)

A.2 Other Survey Questions

Taliban Approval Question: "In your opinion, if the Taliban were to return to power and govern Afghanistan, would it be a good thing for the people and the country, or would it be a bad thing for the people and the country?" (Wave 6-32)

Court Question: " If you had a legal dispute, would you take it to an Afghanistan state court or a local Shura/Jirga?" (Wave 6-24)

Figure A.1: Total Governance Index versus Minimal Governance Index
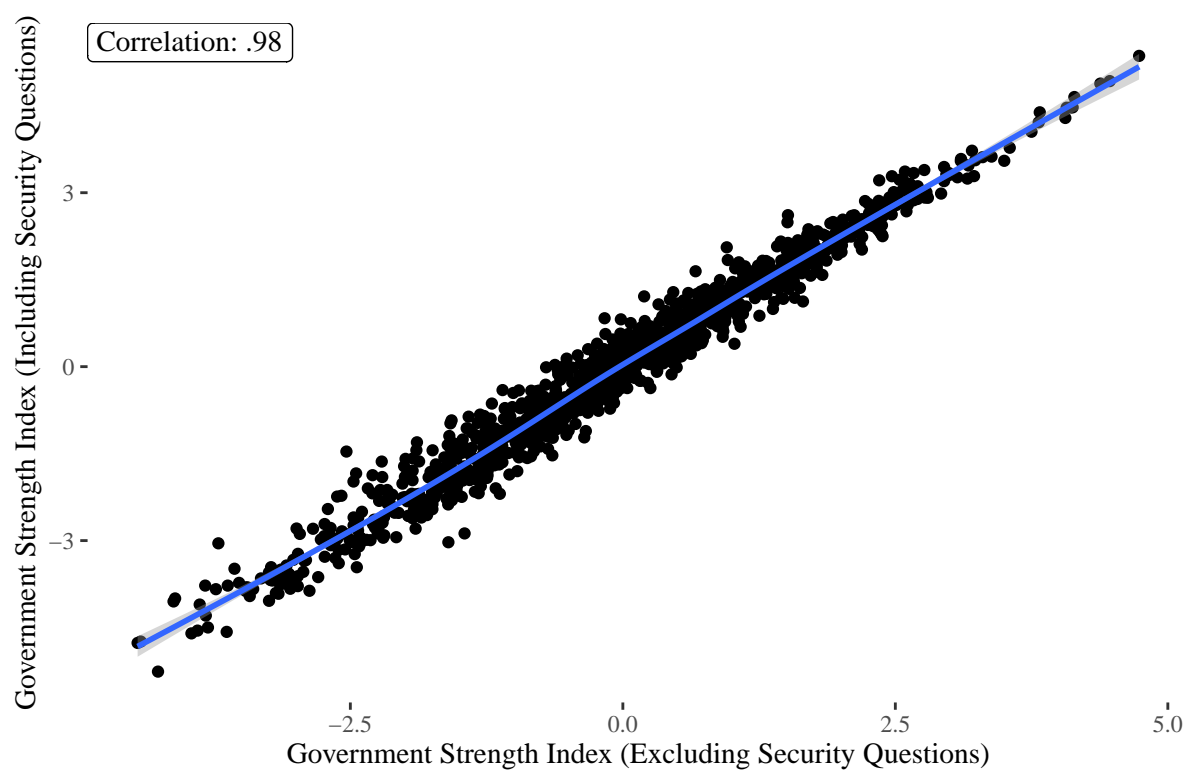
Note: Total governance index is the first principal component using the full list of questions presented in Table A.1. Minimal Governance Index omits any question about control or security.

B   Description of Trajectory Balancing

This section of the appendix briefly summarizes the key points in Hazlett and Xu (2018) to describe the trajectory balancing estimator in more detail.

Consider a time-series cross-sectional dataset containing N units over T periods. Some units over time receive a binary treatment, $\mathbf{1}(\text{Taliban Court}_{it})$. Units that receive treatment are denoted C_i for short. Denote T_0 as the last year units are in control, alternatively the year before treatment. The outcome of interest is Y_{it}, where Y^1 denotes Y observed among treated units under treatment and Y^0 denotes the unobserved counterfactual where treated units never undergo treatment.

The trajectory balancing approach assumes treatment status is sequentially ignorable, or 'as if' randomized after conditioning on pretreatment outcomes.

$$\mathbb{E}[Y_{it}^0|\mathbf{Y}_{i,pre}] = \mathbb{E}[Y_{it}^0|\mathbf{Y}_{i,pre}, C_i]$$

Expressed more generally, this term is

$$\mathbb{E}[Y_{it}^0|\mathbf{Y}_{i,pre}] = f(\mathbf{Y}_{i,pre}; \theta_t) = f(\mathbf{Y}_{i,pre}; \theta_t; C_i)$$

where $f(.)$ is a function taking pre-treatment outcome history, indexed by time θ_t, and group status C_i as arguments. Under mean balancing, we assume $f(.)$ is the expectation operator, but this assumption may be relaxed.

For mean balancing, we assume linearity in prior outcomes (LPO), an assumption researchers implicitly make when using two-way fixed-effects for difference-in-differences or with synthetic controls and latent factor models (see Hazlett and Xu (2018) for proofs).

$$\mathbb{E}[Y_{it}^0|\mathbf{Y}_{i,pre}] = (1, \mathbf{Y}_{i,pre}^\top \theta_t) \quad T_0 < t \leq T$$

θ_t is a $(T_0 +1)$ vector of coefficients. A time intercept is built into $1, \mathbf{Y}_{i,pre}^\top$ as the first term.

Prior to treatment, there exists an $(N \times T_0)$ matrix of pre-treatment outcomes $\mathbf{Y}_{pre} = (Y_{1,pre}^\top, Y_{2,pre}^\top ... Y_{N,pre}^\top)$, some of which will receive treatment at $T > T_0$. This matrix can be separated into two rectangular matrices, one for treatment and another for control. We use information in the pre-treatment period to estimate the counterfactual for treated units.

Balance is sought on the first P eigenvectors of the matrix of pre-treatment outcomes among treated and control units through a procedure that Hazlett and Xu (2018) show minimizes worst-case bias. For mean balancing, this is done with a linear kernel, and weights are found using entropy balancing.

The design is similar to a synthetic control method (SCM) for comparative case studies, but different since it seeks balance on the first P principal component of pre-treatment outcomes, where the choice of P is selected in a way that minimizes worst-case bias. This creates two critical distinctions between our approach and SCM. First, it does not require a long pre-treatment history, which is advantageous given the short panel for our data. Second, multiple treated units with different treating timing can be incorporated into estimating treatment effects without additional complications (Hazlett and Xu, 2018). Similar approaches in difference-in-differences have been employed to match treated units to controls with similar pre-treatment trends (Christensen, Nguyen

and Sexton, 2019; Truex, 2014) Our design applies a generalized identification-based justification of these approaches.

B.1 Illustration of Selection on Trends

Figure B.1 illustrates the utility of our modeling approach relative to parametric fixed effects. We display the raw trend, event study estimates, and trajectory balanced trends and estimates using the example of total events causing Afghan and Coalition causalities from SIGACT data from 2006-2014. Figure B.1a shows causalities were rising before the introduction of courts; Figure B.1b shows results from the Sun and Abraham (2020) interaction weighted event study estimator which illustrate a statistically significant pre-trend in casualty events, which may lead to upward bias on the DD estimate (reported in the top right corner of the plot with the district clustered standard error). Figure B.1c shows the trajectory balanced trends and the ATT and standard errors from equation 1. In line with upward bias from a negative pre-trend, the results from trajectory balancing are more conservative (4 rather than 6 additional events on average).

Figure B.1: Evidence of Selection of Courts on Pre-existing Trends: Events Causing Coalition and Afghan Casualties (SIGACT)

(a) Raw Trends

(b) Difference-in-Differences Event Study

(c) Trajectory Balanced Trend
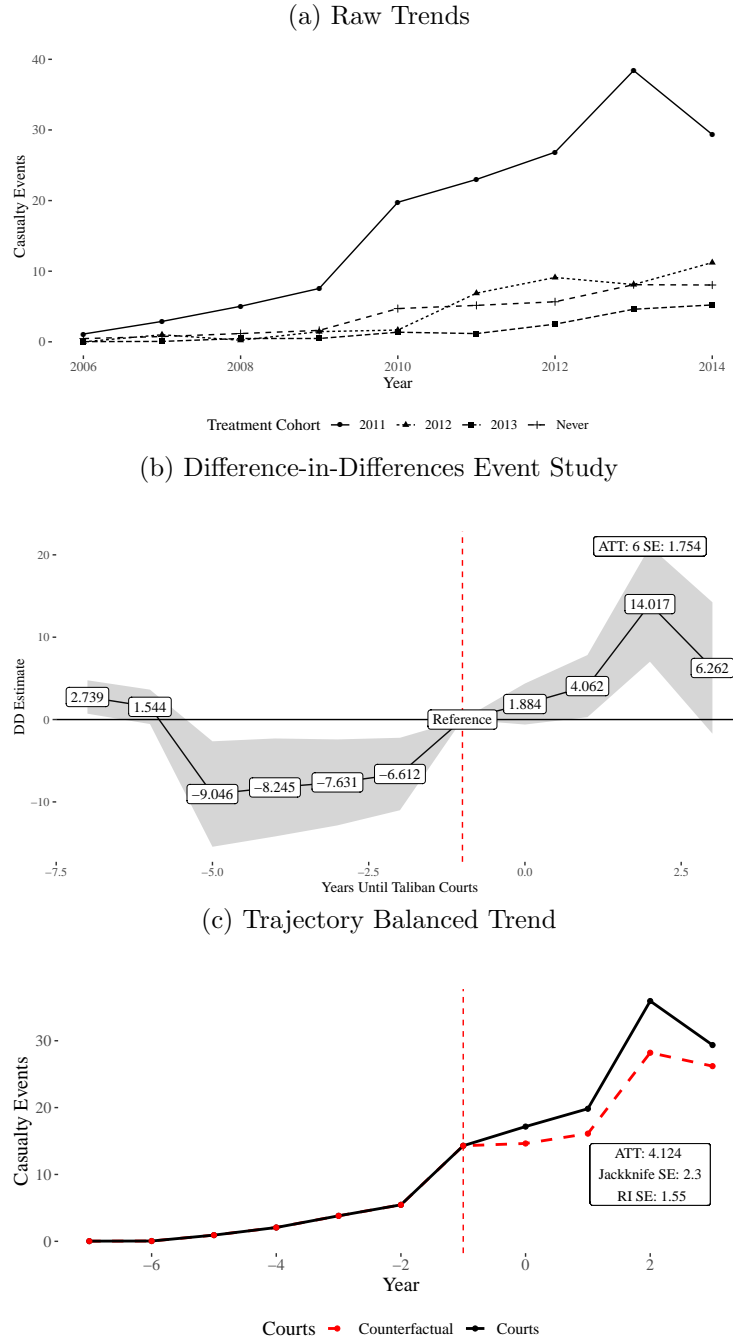
Notes: Total events resulting in coalition and Afghan casualties from SIGACT reported overtime by treatment cohort. Figure B.1a shows raw trends, Figure B.1b shows event study estimates using the Sun and Abraham (2020) estimator, and Figure B.1c shows trajectory balanced trends. Full results in Table 3.

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

(a) Direct Fire

(b) IED Explosions

(c) Events Causing Coalition and Afghan Casualties
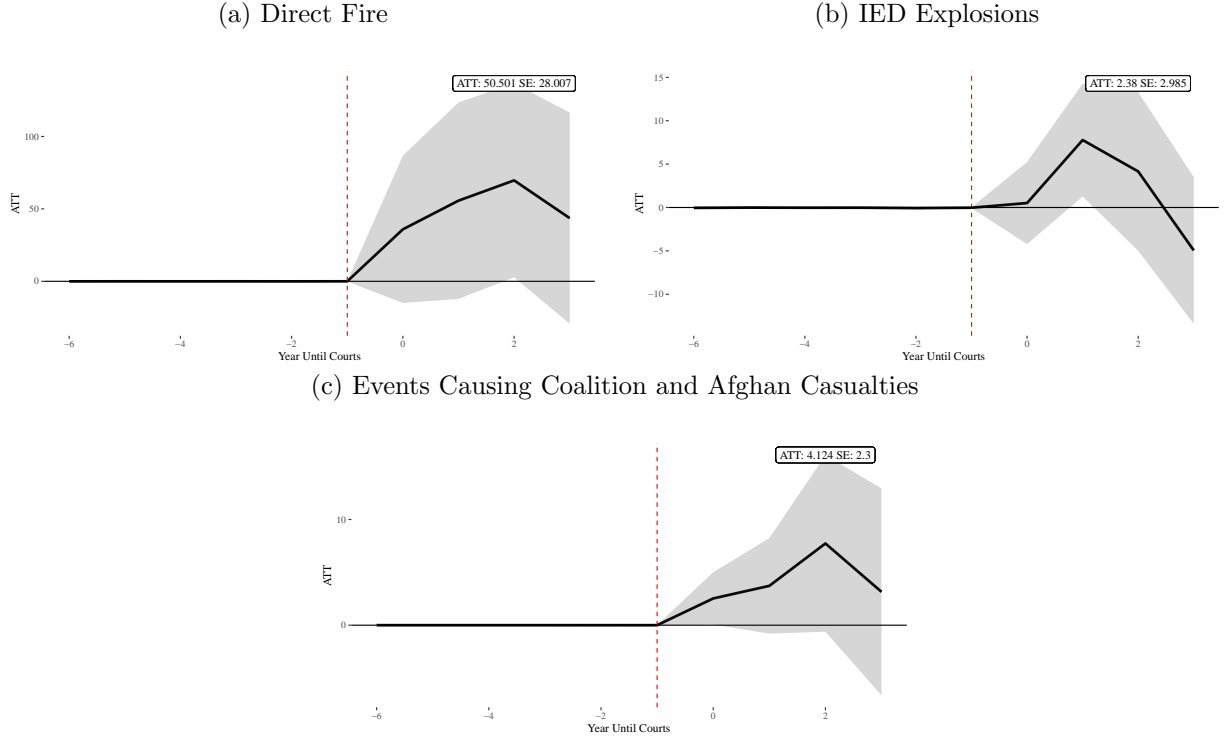
Note: SIGACTs combat outcomes (Direct Fire in Panel A, IED Explosions in Panel B B, Total Coalition and Afghan Casualties in Panel C), among districts in Afghanistan that received courts versus the counterfactual trend. Counterfactual trend constructed from the weighted average of districts that did not receive courts, with weights selected subject to the balancing constraint in equation 1 and explained in Section 4. 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.

C   Combat Outcomes: Alternative Measurement

C.1   SIGACT Outcomes: Longer Panel (2006-2014)

Our ANSO data begins in 2008 and ends in 2013, but SIGACTS covers 2001-2014. Here, we re-estimate our results using outcomes from SIGACTS on a longer panel (2006-2014). Results remain similar.

D   Alternative Standard Errors

D.1   Block Bootstrap

One may be concerned that the jackknife returns too liberal standard errors. We use the jackknife because the process of re-weighting and re-estimating with our procedure is computationally expensive, and becomes complicated with staggered treatment timing.

Therefore, we show results using only the 2011 cohort (the largest) where standard errors are computed through what amounts to a block bootstrap, allowing for serial correlation within units overtime. We obtain similar estimates and standard errors across models.

Table D.1: Dispute Outcomes: 2011 Cohort (Block Bootstrapped)

	(1)	(2)	(3)	(4)	(5)	(6)
	Disputes		Land Disputes		Crime	
Outcome	Ln(+1)	Binary	Ln(+1)	Binary	Ln(+1)	Binary
Taliban Courts	-0.14^{*}	-0.11^{**}	-0.05	$-0.06^{\dagger}$	-0.03	-0.02
	(0.07)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)
Dataset	ANSO	ANSO	ANSO	ANSO	ANSO	ANSO
N. Districts	294	294	294	294	294	294
N. Years	6	6	6	6	6	6
N. Obs	1764	1764	1764	1764	1764	1764
Standard Deviation DV	0.76	0.5	0.33	0.35	0.66	0.48
Mean DV (Control)	0.59	0.47	0.13	0.15	0.44	0.38

Block bootstrapped standard errors. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$.

Table D.2: Public Opinion Outcomes: 2011 Cohort (Block Bootstrapped)

Outcome	Government Support Index	Use Gov Courts	Taliban Approval
Taliban Courts	-0.66^{***}	-0.072^{***}	$0.042^{\dagger}$
	(0.16)	(0.021)	(0.023)
N. Districts	163	150	150
N. Years	7	6	6
N. Years	1141	900	900
Standard Deviation DV	1.72	0.176	0.184
Mean DV (Control)	0.232	0.482	0.185

Block bootstrapped standard errors. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$.

Table D.3: Combat Outcomes: 2011 Cohort (Block Bootstrapped)

	AOG Events	IED Events	Direct Fire	IED Explosions	Total Casualties
Panel A: Count of Events					
			Count of Events		
Taliban Courts	13.69**	9.46*	77.97	21.01*	12.27*
	(4.86)	(4.39)	(64.57)	(9.26)	(6.19)
Panel B: Natural Log of Events					
			Ln(Outcome+1)		
Taliban Courts	0.21*	0.22**	0.25*	0.23**	0.29***
	(0.08)	(0.07)	(0.11)	(0.09)	(0.08)
Dataset	ANSO	ANSO	SIGACTS	SIGACTS	SIGACTS
N. Districts	294	294	294	294	294
N. Years	6	6	6	6	6
N. Obs	1764	1764	1764	1764	1764
Standard Deviation DV (Levels)	46.94	35.47	268.53	77.19	30.24
Mean DV (Control) (Levels)	16.39	11.61	28.1	12.29	4.4
Standard Deviation DV (Logs)	1.59	1.47	1.93	1.68	1.3
Mean DV (Control) (Logs)	1.68	1.33	1.73	1.26	0.85

Block bootstrapped standard errors. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$.
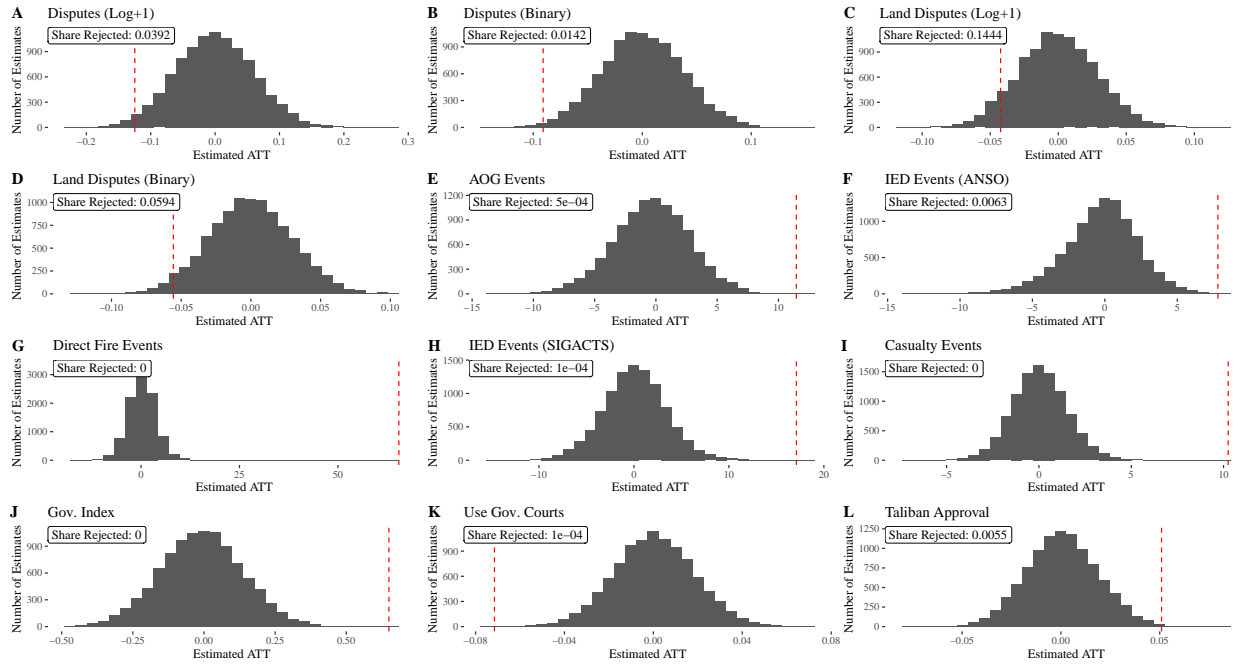
D.2 Randomization Inference

We calculate the probability that our estimates would be as extreme as observed under the null hypothesis of no effect (the p-value) within a randomization inference framework. We assume, for each outcome Y^j where j indexes the primary outcomes we study (disputes, armed conflict, attitudes), that assignment to treatment was randomized (conditionally ignorable) conditional on pre-court outcome trajectories. Assuming that this was the process by which assignment to treatment occurred, we compute the probability that findings of the magnitude we estimate occurred by chance through the following steps.

1. Randomly reassign the cohort vector (the district-invariant variable which codes the year district's first had a Taliban court recorded).

2. Construct a placebo indicator scored 1 for district i if year t is greater than or equal to the cohort variable, and zero otherwise (e.g., a district that has a placebo court in 2012 is considered 'treated' in 2012 and 2013, with a value of 0 for other years, and a value of 0 if the district is coded as "never treated").

3. Compute the ATT from trajectory balancing using the placebo treatment indicator, including the estimation of weights to satisfy balancing constraints. This step incorporates the uncertainty of the weights themselves into the calculation.

4. Repeat steps 1-3 k times (in our simulations, k=10,000, but we have found very small differences between k=5000 and k=10,000.).

5. Compute the share of placebo ATTs larger (in absolute terms) than the ATT computed using the actual timing and location of courts.

6. The share of placebo tests more extreme (in an absolute sense) than the estimated treatment effect is the probability a similar size treatment effect would have been observed under different hypothetical realizations of the timing and location of Taliban courts, conditional on matched pre-trends.

In line the theory underlying randomization inference, we keep our simulations as close to the theorized assignment mechanism as possible by estimating new mean balanced weights for each random distribution of courts.

We present our results below. The histograms are the distribution of estimated effects of placebo Taliban courts after trajectory balancing. The horizontal axis is the size of the estimate, and the vertical axis is the volume of estimates falling into each bin. The vertical dashed line is the estimated effect of Taliban courts using the actual timing and location of courts. The annotation in the upper left corner is the share of placebo estimates more extreme than the estimated effect of Taliban courts, which corresponds to the $p - value$. As each panel shows, the probability that estimates as extreme as the ones we observed arrived due to chance is very low; below conventional statistical significance thresholds.

Figure D.1: Randomization Inference Results

Results from randomization inference where the cohort vector is reshuffled 10,000 times. Vertical red line is the estimated ATT from the true data, and the histogram shows the density of placebo estimated effect sizes. Annotation in top left corner is the share of absolute placebo estimates larger than the true estimate.

E Alternative Treatment

The exclusion of districts that had treatment switch on and off since 2011 helps guard against attenuation of our main results due to endogenous service provision - one may expect that a district that had a court, and lost it, only to regain it again, was highly contested between insurgent and government forces, perhaps for reasons not driven by judicial services. As such, our preferred sample excludes courts that had treatment switch on an off.

However, qualitative evidence suggests even when courts may have been shut down, the influence of Taliban justice may have remained sticky. Under this assumption, we treat districts that had treatment switch on and off as "always treated" after the year 2011. We report results below. As one can see, the findings are largely consistent with our main results.

Table E.1: Coercion Outcomes: All Districts

Outcome	ln(Dispute)	1(Dispute)	ln(Land Dispute)	1(Land Dispute)
Taliban Courts	-0.1352***	-0.1042***	-0.0529**	-0.0638***
	0.0407	0.0253	0.018	0.0182
Database	ANSO	ANSO	ANSO	ANSO
N. Districts	398	398	398	398
N. Years	6	6	6	6
N. Obs.	2388	2388	2388	2388
Standard Deviation DV	0.74	0.5	0.31	0.34
Mean DV	0.78	0.5	0.33	0.36

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Districts that had courts withdrawn are coded as exposed in 2011 and thereafter. Jackknife standard errors. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$.

Table E.2: Survey Outcomes: All Districts

Outcome:	Gov. Strength Index	Use Government Courts	Taliban Approval
Taliban Courts	-0.54***	-0.06***	0.05**
	0.10	0.01	0.01
N. Districts	221	175	175
N. Years	7	6	6
N. Obs.	1547	1050	1050
Mean DV (Control)	0.23	0.48	0.19
SD DV	1.73	0.17	0.19

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Districts that had courts withdrawn are coded as exposed in 2011 and thereafter. Jackknife standard errors. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$.

Table E.3: Combat Outcomes: All Districts

	AOG	IED	Direct Fire	IED Explosions	Total Cas.
Panel A: Outcome			Levels		
Taliban Courts	8.4446***	6.7445**	65.1248**	17.5115***	8.4432***
	2.4828	2.1687	20.1006	4.9512	2.0354
Panel B: Outcome			Ln(y+1)		
Taliban Courts	0.1566***	0.2074***	0.196***	0.208***	0.2561***
	0.0473	0.0455	0.0525	0.0488	0.0451
Dataset	ANSO	ANSO	SIGACTS	SIGACTS	SIGACTS
N. Districts	398	398	398	398	398
N. Years	6	6	6	6	6
N. Obs	2034	2034	2034	2034	2034

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Districts that had courts withdrawn are coded as exposed in 2011 and thereafter. Jackknife standard errors. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$.

F Mechanism Tests

F.1 Heterogeneity by Subgroup

Below we present trajectory balanced results, where we estimate the effects of courts within subgroups (above average socioeconomic status versus low socioeconomic status, Pashtun versus non-Pashtun ethnicity, male versus female respondents).

Since trajectory balancing requires a balanced panel and produces ATT estimates, and has yet to be extended to study heterogeneous effects formally, we use the following procedure to test for heterogeneity by subgroups.

1. Estimate the trajectory balanced ATT within each subgroup of interest respectively

2. Z-test the difference between estimates within one subgroup relative to another to decipher if the estimates are statistically distinguishable from one another.

As results show, point estimates for each subgroup overlap with one another when accounting for the standard error, suggesting the null of an equivalent effect between subgroups cannot be rejected.

F.1.1 Heterogeneity by Socioeconomic Status (SES)

Table F.1: Attitude Effects By SES

Outcome	(1) High SES	(2) Gov. Index Low SES	(3) High SES	(4) Use Gov. Court Low SES	(5) High SES	(6) Taliban Approval Low SES
Taliban Courts	-0.5421	-0.515	-0.0932	-0.0586	0.0838	0.0422
	0.4782	0.1484	0.0509	0.0148	0.0386	0.0199
Z-statistic of Difference:	-0.054		-0.600		0.958	
N. Districts	43	191	52	147	53	147
N. Years	6	6	5	5	5	5
SD DV	2.45	1.673	0.299	0.169	0.273	0.183
Mean DV (Control)	0.875	-0.074	0.522	0.465	0.206	0.194

Note: Heterogeneous effects of courts by subgroup. Estimates obtained per equation 1 on each subpopulation separately. The row labeled Z-statistic of difference reports the the Z-statistic of a test of coefficient similarity. The odd columns report the Z-statistic of the different subgroups for each respective outcome (e.g., column (1) reports the test of whether the effect is different for the government strength outcome, column (3) reports the difference for using government courts, column (5) reports the difference for Taliban approval. Standard errors are jackknifed. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$.

F.1.2 Heterogeneity by Ethnicity

Table F.2: Attitude Effects By Ethnicity (Pashtun vs. Non-Pashtun)

Outcome	Gov. Index		Use Gov. Court		Taliban Approval	
	(1)	(2)	(3)	(4)	(5)	(6)
	Pashtun	Non-Pashtun	Pashtun	Non-Pashtun	Pashtun	Non-Pashtun
Taliban Courts	-0.4315	-0.4408	-0.0592	-0.0764	0.0303	0.0501
	0.1487	0.1754	0.0233	0.0274	0.0129	0.0234
Z-statistic of Difference:						
	0.04		0.478		-0.75	
N Districts	96	127	86	107	85	139
N Years	7	7	6	6	6	6
SD DV	1.88	1.765	0.191	0.187	0.193	0.2
Mean DV (Control)	-0.021	0.149	0.46	0.508	0.261	0.162

Note: Heterogeneous effects of courts by subgroup. Estimates obtained per equation 1 on each subpopulation separately. The row labeled Z-statistic of difference reports the the Z-statistic of a test of coefficient similarity. The odd columns report the Z-statistic of the different subgroups for each respective outcome (e.g., column (1) reports the test of whether the effect is different for the government strength outcome, column (3) reports the difference for using government courts, column (5) reports the difference for Taliban approval. Standard errors are jackknifed. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$.

F.1.3   Heterogeneity by Gender

Table F.3: Attitude Effects By Gender (Male vs. Female)

Outcome	Gov. Index		Use Gov. Court		Taliban Approval	
	(1)	(2)	(3)	(4)	(5)	(6)
Group	Male	Female	Male	Female	Male	Female
Taliban Courts	-0.7378	-0.3674	-0.0884	-0.0545	0.0537	0.0508
	0.1249	0.1597	0.0156	0.0151	0.0176	0.0193
Z-Statistic of Difference						
	1.83†		-1.56		0.03	
N. Districts	183	136	169	131	169	131
N. Years	7	7	6	6	6	6
SD DV	1.91	1.82	0.21	0.2	0.21	0.17
Mean DV (Control)	0.2	0.44	0.49	0.48	0.2	0.15

Note: Heterogeneous effects of courts by subgroup. Estimates obtained per equation 1 on each subpopulation separately. The row labeled Z-statistic of difference reports the the Z-statistic of a test of coefficient similarity. The odd columns report the Z-statistic of the different subgroups for each respective outcome (e.g., column (1) reports the test of whether the effect is different for the government strength outcome, column (3) reports the difference for using government courts, column (5) reports the difference for Taliban approval. Standard errors are jackknifed. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$.

F.2 ANQAR Attitudes Results

Here we describe the estimation strategy for the attitudes on reported state engagement regression reported in the main text.

We estimate the following with least squares among respondents in ANQAR in Wave 20-24 (2013-2014), the waves where the IED question was asked:

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

where $i \in \{1, 2...62, 199\}$ indexes respondents, $d \in \{1, ...344\}$ indexes districts and $w \in \{20, ...24\}$ 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, age^2, 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. Doing so allows us to compare individuals within the same district and wave that reported using courts or not. This approach also enables us to address any common shocks to court availability or utilization that coincide with the timing of each separate survey wave. This mitigates omitted variable bias from district invariant factors, such as institutional quality and service provision, and common shocks, such as the timing of the presidential elections in 2014. We cluster errors at the district, since this is the administrative level where government and Taliban courts were provided. Note that we include gender, ethnicity, survey wave, and district as fixed effects using the Method of Alternating projections, so there are no partial derivatives to report for these regressors.

The results - including the adjusting covariates - are reported below.

Table F.4: IED Reporting on Government Court Use - Partial Derivatives of Covariates

Likelihood of Reporting IED	Binary		Categories	
	(1)	(2)	(3)	(4)
$\mathbb{1}$(Use Government Court)	0.19***	0.14***	0.32***	0.45***
	(0.01)	(0.02)	(0.04)	(0.03)
$\mathbb{1}$(Use Jirga	0.12***	0.09***	0.20***	0.27***
	(0.02)	(0.02)	(0.03)	(0.04)
Age	−1.31*	−0.32	−1.03	−3.81*
	(0.59)	(0.47)	(1.11)	(1.63)
Age^2	1.15	−0.23	1.12	6.29***
	(0.70)	(0.47)	(1.05)	(1.70)
$\mathbb{1}$(Gov. Control)	0.16***	0.10***	0.20***	0.36***
	(0.01)	(0.01)	(0.03)	(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

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

F.3 Sequencing of Effects

Our argument suggests the causal sequence of events explaining an increase in combat after the introduction of courts is that (1) public opinion moves towards the Taliban which (2) facilitates increased insurgent activity.

To test our argument, we study the difference between control districts and districts that received courts which were withdrawn *before withdrawal occurred* (i.e, we focus on the trajectory balanced difference between court and control districts in 2011 if the court was withdrawn in 2012.)

If our sequencing argument is correct, we should not find consistently positive effects of courts on armed combat, since courts may not have created communal dependence on the community before withdrawal. However, courts that were withdrawn should have started shifting opinion while they were active if they were effective.

We provide evidence for both effects. Table F.3 shows courts reduced government strength, undermined citizen willingness to use government courts, and increased Taliban approval. Note that although the estimate for government strength is smaller than the baseline (which is sensible, given the courts were withdrawn later) the estimate for using government courts and Taliban approval are nearly identical to the benchmark specification.

Table F.5: Effect of Withdrawn Courts on Attitudes

Outcome	Government Support Index	Use Gov Courts	Taliban Approval
Taliban Courts	-0.0309	-0.0855^*	0.0588^*
	(0.2265)	(0.0349)	(0.0271)
N. Districts	155	141	141
N. Years	4	3	3
Standard Deviation DV	1.822	0.201	0.196
Mean DV (Control)	-0.068	0.489	0.205
Standard Deviation DV	1.681	0.178	0.18
Mean DV (Control)	0.232	0.482	0.185

Note: Attitude outcomes comparing districts with withdrawn Taliban courts to districts that never had Taliban courts pre-withdrawal.Estimates obtained per equation 1. Standard errors are jackknifed.
$^{***}p < 0.001$, $^{**}p < 0.01$, $^*p < 0.05$, $^\dagger p < 0.10$

Next, in Table F.3 we find no consistent evidence that withdrawn courts impacted armed combat. First, note the dramatic difference in estimated effects from our main armed combat results: AOG events are estimated to increase by 11 in the main results, along with 65 more direct fire events, but here we estimate only 2 more AOG events and 29 more direct fire events, and neither estimate is conventionally different from zero. Total casualty events are estimated to be near zero (.16).

We do find that IED events reported by ANSO increase - however, this does not invalidate our argument for three reasons. First, it is possible for courts to have shifted opinion in the short term facilitating more mine-based attacks. Second, we do not find an increase in IED explosions as recorded by SIGACTs, suggesting that the increase in IEDs is not systematic. Third, despite ANSO IED events increasing, the preponderance of evidence suggests that combat did not increase across the board in districts that had withdrawn courts in the same way that districts with courts saw increasing insurgent attacks. Therefore, on balance, the test is supportive of the causal sequencing

in our argument.

Table F.6: Effect of Withdrawn Courts on Armed Combat

Outcome	(1) AOG	(2) IED	(3) DF	(4) IED Explosions	(5) Causality Events
Taliban Courts	2.81	8.32**	29.09	12.84	0.16
	(3.61)	(3.2)	(35.85)	(14.69)	(3.93)
Dataset	ANSO	ANSO	SIGACT	SIGACT	SIGACT
N. Districts	263	263	263	263	263
N. Years	4	4	4	4	4
N. Obs	1052	1052	1052	1052	1052
Standard Deviation DV	36.6	34.47	378.04	77.02	24.14
Mean DV (Control)	14.29	10.42	21.55	11	3.16

Note: Armed conflict outcomes comparing districts with withdrawn Taliban courts to districts that never had Taliban courts pre-withdrawal.Estimates obtained per equation 1. Standard errors are jackknifed. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

G Survey Non-Response and Courts

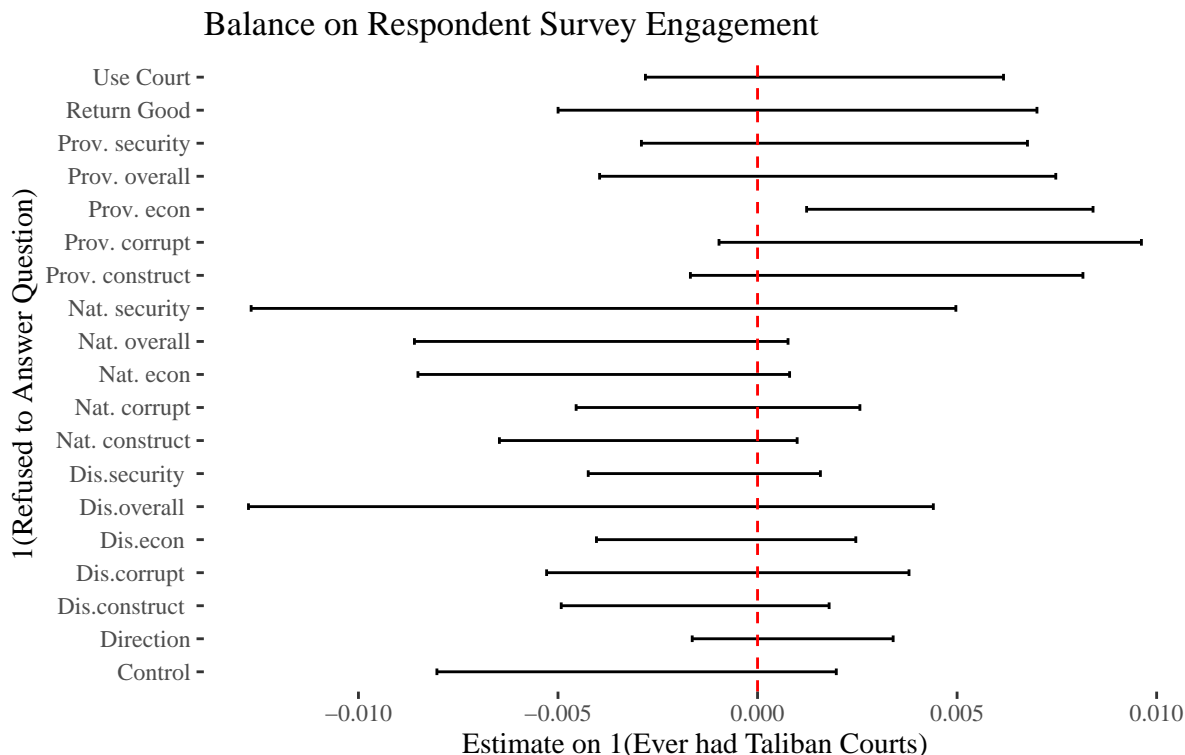## Balance on Respondent Survey Engagement

Figure G.1: Caption

H Ethical considerations

This study involves the quantitative analysis of several datasets. We describe the data, including how information was collected and by whom, in the manuscript in Section 4.1. Where relevant, military records that were previously classified or sensitive in nature have been formally declassified. Data on civilian collaboration has been aggregated to limit any potential unintended harm. Information on the location of Taliban-led courts, criminal activity, and combat operations can be circulated publicly. Our survey evidence is drawn from the ANQAR platform. Responses are collected anonymously and respondents granted their consent to be enumerated. No potentially identifying or otherwise sensitive information has been shared with the researchers or used in our analysis. Access to this data is governed by a legal agreement between the authors and the North Atlantic Treaty Organization (NATO) and cannot be circulated publicly at the individual level. To support replication of any survey-based results, the authors will guide and support interested researchers through the data access process.

I Adjusting for Taliban Influence Pre-Court

Table I.1: Trajectory Balanced Results Adjusting for Pre-Treatment Taliban Influence

Panel A: Disputes

	Dispute Log	Dispute Bin	Land Log	Land Bin
Taliban Courts	-0.11†	-0.09*	-0.04	-0.07*
	0.06	0.04	0.03	0.03
	0.06	0.02	0.16	0.03
N Districts	321	321	321	321
N Years	6	6	6	6
Balanced on Control?	Y	Y	Y	Y

Panel B: Public Opinion

	Gov. Strength Index	Use Gov Court	Taliban Approval
Taliban Courts	-0.58***	-0.05*	0.04*
	0.15	0.02	0.02
	0.00	0.01	0.04
N Districts	187	149	149
N Years	7	6	6
Balanced on Control?	Y	Y	Y

Panel C: Combat (Levels)

	AOG Events	IED Events	Direct Fire	IED Expl.	Total Cas.
Taliban Courts	7.59*	10.02***	58.34***	15.71***	9.54***
	2.63	3.21	18.00	3.52	1.64
	0.01	0.00	0.00	0.00	0.00
N Districts	321	321	321	321	321
N Years	6	6	6	6	6
Balanced on Control?	Y	Y	Y	Y	Y

Trajectory balanced results including pre-court Taliban influence as covariate. Standard errors and p-values obtained via randomization inference. ***$p < 0.001$, **$p < 0.01$, *$p < 0.05$, $^\dagger p < 0.10$