

Televising Justice during War*

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Abstract

Television is an overlooked tool of state building. We estimate the impact of televising criminal proceedings on public use of government courts to resolve disputes. We draw on survey data from Afghanistan, where the government used television as a mechanism for enhancing the legitimacy of formal legal institutions during an ongoing conflict. We find consistent evidence of enhanced support for government courts among survey respondents who trust television following the nation's first televised criminal trial. We find no evidence that public confidence in other government functions (e.g. economy, development, corruption) improved during this period. Our findings suggest that television may provide a means of building state legitimacy during war and other contexts of competition between political authorities.

*We thank Luke Condra, Oeindrila Dube, Kenneth Holland and Jake Shapiro for helpful comments. The authors are grateful to the North Atlantic Treaty Organization's Communications and Information Agency for granting access to the survey materials used in this study. A particular debt of gratitude is owed to Philip T. Eles, senior scientist at the Agency, for providing continued support for and feedback on this project. All errors remain with the authors.

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Civil war is, at its political core, a process of competitive state building. To establish claims of legitimate authority, states and insurgents compete both militarily and politically. Scholarship on civil war suggests these levels of competition are deeply intertwined: coercive success engenders political legitimacy, while effective popular governance fosters the civilian support necessary for military success in irregular war (Kalyvas, 2006). The latter mechanism creates a “market for governance,” where civilians choose a governance provider to reward with loyalty. To that end, both states and insurgents invest in state building, creating or improving governance institutions in a bid to win popular support (Berman and Matanock, 2015; Stewart, 2017).

Perhaps the most important of these institutions are mechanisms for dispute resolution. Managing disputes and enforcing property rights are fundamental functions of political authority. Disputes arise constantly in social life, where “every land boundary, business deal, will, or loan risks giving rise to a costly disagreement or dispute” (Blattman et al., 2014). Dispute resolution was vital to early processes of statebuilding, in which European monarchs created courts to enforce property rights and political order in exchange for taxation (Tilly, 1985; Bates, 2010). It remains vital to modern would-be political authorities: a wide range of rebel groups create justice institutions in areas they influence, from the Irish Republican Army in 1920 to Greek Communists in 1942 and Syrian militants in 2015 (Kotsonouris, 1994; Kalyvas, 2015; Arjona, 2016).

Yet fostering legitimacy during war requires not just building institutions but selling them. Television—and televised criminal proceedings in particular—may be an effective means of enhancing the legitimacy of dispute resolution institutions during conflict (Warren, 2014). Television news has been shown to shape public preferences for punitive justice (Gilliam Jr and Iyengar, 2000). Televised trials may also “ensure that no one could see the end result [of judicial proceedings] as arbitrary rather than reasonable and justifiable” (Mutz, 2007). Raising awareness of legal institutions has been shown to increase their perceived

legitimacy in a number of contexts (Gibson et al., 1998).

Our central argument is that televised legal proceedings can enhance the ability of the government to compete with other mechanisms for resolving disputes. Unlike canonical tools of statebuilding—militaries and tax bureaucracies capable of subjugating and extracting wealth from conquered subjects—dispute resolution institutions do not always impose themselves on citizens. If citizens do not use such institutions, they may not be aware of their existence, capabilities, or quality. This poses a challenge to would-be statebuilders: for successful trials to increase consumption of dispute resolution institutions, the signal they send requires amplification. In addition to successfully conducting the task of dispute resolution, then, states must be able to effectively publicize the proceedings and results of important trials. Mass media make this possible. Newspaper stories, radio and television reports, and live broadcasts of trials allow states to widely disseminate signals of capacity and intention to their populations. This information dissemination, we argue, is as essential for statebuilding as dispute resolution institutions themselves.

Using the unexpected timing of Afghanistan’s first televised criminal trial and survey data collected before and after the trial, we provide the first evidence that televised trials may enhance the legitimacy of judicial institutions during an ongoing insurgency. We find no evidence that public confidence in other government functions (economy, development, or corruption) improved after the trial, suggesting that legal institutions specifically gained from the high profile event. Though differences in public trust and engagement with the media may influence how generalizable our findings are to other settings, these results suggest that television can help state institutions compete for legitimacy during civil war.

This paper proceeds as follows. The next section outlines our argument about the role of trials and mass media in statebuilding and situates it in the literatures on state consolidation, information dissemination, and civil war. The following section traces the empirical context of post-2001 Afghanistan and introduces the 2014 trial that represents the focal point of

our analysis; section four articulates our empirical strategy and section five presents and discusses our results. The final section concludes and suggests implications for theories of statebuilding and governance.

Statebuilding through Public Trials

Trials play an important, underexplored role in statebuilding and political development. In contemporary states, trials are the principal manifestation of a state's capacity for dispute resolution. Dispute resolution, in turn, is one of the key functions required of political authority. Tilly (1985) argues that dispute resolution mechanisms were vital to the formation of European states. In exchange for the taxation needed to finance their wars, early European states offered their subjects protection against internal rivals and the institutional mechanisms—like courts—which would ensure that protection. Dispute resolution remains foundational to modern states. Formal and informal mechanisms for dispute resolution are necessary for maintaining social order, preventing violence, and promoting economic development (Blattman et al., 2014). Scholarship on groups that seek to emulate states places a similar emphasis on dispute resolution. Rebel groups frequently create dispute resolution services for local populations (Arjona, 2016, 69-70). Criminal organizations offer such services to their business partners and clients (Gambetta, 1993; Skarbek, 2011), and self-governing civil society groups may develop similar institutions for their members (Ostrom, 1990).

For modern states, nowhere are dispute resolution mechanisms more visible than in trial proceedings. Trials are where disputes are adjudicated and resolved, where a state's ability to effectively render verdicts and punish transgressions is tested. As such, they represent critical opportunities to signal state capacity and intention. States can signal their capacity to enforce order and maintain the rule of law by successfully trying powerful elites or those accused of grave offenses. Likewise, they can signal intentions by choosing how to prioritize

cases, when to seek more or less severe punishments, and whether to follow or disregard due process protocols.

We argue that these signals are a key element of statebuilding. In contexts of contested sovereignty—when states compete with insurgents, civil society groups, or external actors for political authority—citizens can often “forum shop,” deciding which governance services to use and which to ignore. In these settings, we argue, trials represent valuable tools for building state legitimacy. Citizens are more likely to use dispute resolution mechanisms if they are efficient, inexpensive, and impartial; they are less likely to use services they see as corrupt, costly, or biased against them. Trials present opportunities to send signals about these characteristics, and to encourage broader public utilization of government dispute resolution institutions.

One key intervening variable mediates the relationship between these signals and widespread use: information dissemination. Unlike canonical tools of statebuilding—militaries and tax bureaucracies capable of subjugating and extracting wealth from subjects—dispute resolution institutions do not necessarily impose themselves on citizens. Citizens may not decide to use such institutions because they are unaware of their existence, capabilities, or quality. This poses a challenge to would-be statebuilders: for trials to successfully increase public use of government dispute resolution, the signal they send requires amplification. In addition to successfully conducting the task of dispute resolution, then, states must also be able to effectively publicize the proceedings and results of important trials.

Mass media such as print, radio, television, and internet make this possible. Live broadcasts in particular enable states to widely disseminate signals of capacity and intention to their populations in real time, potentially avoiding interference or manipulation of trial proceedings by outside actors. As a mechanism of state building, televised trial proceedings can increase public awareness of how formal courts function and enhance judicial transparency. If televised trials fulfill these dual functions, we anticipate that these political events will

lead to an increase in popular use of government legal institutions.

In emphasizing the importance of media and information dissemination for statebuilding, we build upon a growing literature on the political consequences of television and radio. Empirical findings in political science and economics have demonstrated that broadcast media play a key role in shaping political attitudes and behavior. In some settings, radio programming increases political participation (Mvukiyehe, 2017). In others, radio and television can discourage participation and undermine social trust (Olken, 2009; Gentzkow et al., 2011). Mass media also represents a potent tool for electoral messaging: U.S. propaganda broadcast through Radio Liberty boosted Boris Yeltsin’s vote share in the 1991 Russian presidential election (Garcia-Arenas, 2016), while an anti-regime television advertising campaign in Chile increased votes against the Pinochet dictatorship in a referendum (Gonzalez and Prem, 2018).

Broadcast media also influence political behavior in contexts of civil war and political violence. Warren (2014, 2015) shows that radio and television access diminish the probability of large-scale collective violence by increasing the effectiveness of pro-regime and pro-social messaging. Yanagizawa-Drott (2014) demonstrates that radio messaging during the Rwandan genocide encouraged civilians to participate in anti-Tutsi violence, while Gagliarducci et al. (2017) find that anti-Nazi radio programming by the British Broadcasting Service increased the incidence of anti-regime resistance in World War II-era Italy. Other studies show that mass media can affect the political behavior of combatants themselves: large-scale radio and television information campaigns increased the demobilization of combatants in Colombia (Jones, 2006) and Iraq (Mallet, 1997; Lamb, 2005). Given these myriad effects of radio and television on political attitudes and behavior—both in electoral politics and settings of violent contestation—we argue that televising trial proceedings should shape citizens’ perceptions of justice institutions and decisions about which institutions to use and which to ignore.

Setting

The Market for Justice in Afghanistan

Two decades after the overthrow of the Taliban, the Afghan state continues to struggle with ineffective public goods provision, corruption, capture, and impunity. A fragile political stability is dependent upon the pragmatic acceptance of regional power structures that frequently bypass the justice system (Tondini, 2007). Consequently, weak judicial institutions and dispute resolution services have limited state legitimacy and hamstrung further state building attempts (Swenson, 2017). Despite substantial investment by the international and NGO community, Afghan government courts remain plagued by dysfunction, corruption, and inefficiency.

Several institutional factors serve to fray public trust in the formal justice system. Low pay, minimal court funding, and a non-adherence to meritocratic advancement incentivize a poorly-educated and disorganized judiciary to invite bribery – as well as criminal and political intrusion – into court decision-making (Singh, 2015). Political compromises to maintain national stability mean many regional warlord interests are cemented through key positions in the judiciary itself. Judges often serve exclusively the interests of these powerful actors, with effective democratic oversight further diminished at the provincial and district levels, which lack elected councils (Ruttig, 2013).

Once a case has been resolved, limited enforcement infrastructure leaves few paths for the prevailing party to seek restitution. Prisons and correctional institutions are rudimentary at best in urban areas, and nearly non-existent in rural and provincial centers (Wardak, 2004). Similarly, enforcement officers are sparse in rural Afghanistan, where over 80 percent of the population live. Where police do have sufficient capacity, close ties to militias reduce cooperation with the Afghan judiciary and lower local trust in the fair administration of

justice (Wardak, 2004). Perhaps more fundamentally, the Western inspiration of the new legal code and court system is often viewed by rural populations to be at odds with the tradition-based rulings common at the local level (Ahmed, 2015). The combination of these factors often leaves only the prevailing party truly invested in resolving court judgements, serving as a significant disincentive towards employing these services again. All told, a Gallup poll in October 2014 suggested only 25 percent of Afghans expressed any confidence in the formal judicial system (Ahmed, 2015).

Complicating matters, government courts are not the only game in town. The state faces sharp competition from two alternative providers of dispute resolution services: customary law, administered by local jirgas (councils), and Islamic courts established by the Taliban. Jirgas, traditional gatherings of elders and social leaders, have been used to resolve “tribal and national political, social, economic, cultural and even religious conflicts by making authoritative decisions” (Rafi, 2002). While primarily associated with Afghanistan’s Pashtun population, jirgas are also important social actors in rural areas where Uzbeks, Hazaras, and Tajiks are the predominant ethnic groups. One critical function of jirgas is dispute resolution. In contrast to the the national legal system, jirgas often enjoy a reputation for fairness and consistency. Elders draw on traditional and charismatic authority when rendering verdicts while appealing to established social norms and values (Wardak, 2004).

Jirgas have important procedural and representational downsides: their sentences may violate state laws and human rights standards, membership is limited to men, and councils are vulnerable to capture by warlords and political parties (Wardak, 2004; Faqir et al., 2013). Yet jirgas often exceed the formal justice system in legitimacy and popularity. They administer swift justice, preventing small disputes from escalating. In addition to smaller disputes, they also provide a mechanism for resolving community-level disputes (Faqir et al., 2013). As a result—and in light of the challenges to the formal justice system summarized above—Afghans in rural areas often turn to jirgas to resolve disputes (Wardak, 2003). In

2014, less than half of Afghans who sought dispute resolution went through the formal court system; most opted for informal jirgas (Farrell and Giustozzi, 2013).

The state justice system faces a second competitor: the Taliban. Since roughly 2006, the Taliban has offered dispute resolution services to local populations. Those services have grown in complexity and geographic coverage: by 2018, the Taliban had courts in nearly every Afghan province, nested in a tiered justice system encompassing district-level primary courts, provincial-level appeals courts, and a supreme court for appeals in Pakistan (Jackson, 2018). Judges are selected, salaried, and monitored by local Taliban commanders. They rule based on Islamic law, adjudicating personal disputes, conflicts over land, and other social and economic claims (Baczko, 2013). Taliban civil courts also hold jurisdiction over the insurgents' military forces, offering a forum for civilians to bring claims against combatants (Giustozzi, 2014).

Taliban courts are broadly popular. An Afghan deputy governor noted that, once the Taliban arrived in a community, “the people were consulting the government less. They went to the Taliban instead to sort out their issues” (Coghlan, 2008). Even Afghans who were opposed to the politics of the Taliban praised the effectiveness and honesty of its judges (Murphy, 2010; Baczko, 2013). “If I have a legal problem,” one civilian noted, “the Taliban will rule in an hour according to our customs and Islam. If I take it to the government, in six months nothing will have happened. Then whoever pays the most will win” (Farmer, 2010). In a 2012 study, 80 civilians who had applied to a Taliban court instead of a government one agreed, complaining that government courts could take years—and certainly took bribes (Azizi, 2012).

The Afghan justice system thus faces two major rivals. Rather than seeking redress in government courts, Afghans often turn to customary justice administered through local jirgas or insurgent justice meted out in Taliban tribunals, both of which enjoy substantial popularity and social support. In this environment of active competition between governance

providers, we expect that information dissemination through television can play a critical role in shaping the legitimacy of the state.

The Paghman Trial, 2014

On August 23, 2014, a car caravan of couples were returning from a wedding in the Paghman district of Kabul, a 15-minute drive from the capital. They were stopped by a group of men dressed in police uniforms, at which point four of the women were robbed, beaten, and raped. Within days, the details of the crime spread across social media, where a rapid online campaign galvanized police attention to the case (Najibullah, 2014).

Over the course of the next two weeks, thousands of Kabul residents demonstrated in the streets to demand swift justice and a death penalty for the perpetrators. Demonstrators warned officials not to take bribes to ignore the case (Nordland and Muzhary, 2014). These calls for quick and lethal action by police were echoed by a broad swath of Afghan society, making unexpected allies of local male elders and womens rights activists alike (Najibullah, 2014). Calls for the death penalty were even echoed by many civil rights advocates, with one quoted representative approvingly referencing prisoners who promised to kill the perpetrators in prison if they were not hanged (Nordland and Muzhary, 2014).

Facing intense public pressure, within days police detained seven suspects, and the office of the attorney general concluded its investigation. Before the case had gone to trial, President Karzai—a historically avid opponent of capital punishment—had already publicly assured a group of female politicians of his desire that the suspects be executed after conviction (Nordland and Muzhary, 2014). For the first time ever, the court proceedings were broadcast on live television—an unprecedented step for a system where trials are usually closed to the public (TOLONews, 2014). While the victims confronted their attackers in the courthouse, a group of protesters chanted “only hanging is acceptable” outside the Supreme Court (Nordland and Muzhary, 2014). After a case lasting only a couple of hours the sus-

pects were convicted, with Karzai signing the final approval for their execution on his last day in office (BBC, 2014). Five of the seven would be hanged October 8 despite domestic and international calls for the new President to stay the executions (France-Presse, 2014).

The broad public attention garnered by this case was unheard of in the Afghan justice system and was elevated by a robust national news environment. In the wake of the Taliban's removal from power, dozens of television channels and more than 100 radio stations had gained popularity as sources of national news across the country (CBSNews, 2014). These outlets, such as Tolo TV (with an estimated 10 million Afghan viewers), followed case developments closely, broadcasting the trial and newscaster analysis in its aftermath. In a country where three-fifths of households own a television and 58 percent report this being their primary source of news, this coverage served to amplify each turn in the case to national attention (Warren and Hopkins, 2014).

Widespread public awareness of this case in turn inspired a robust introspection as to why it had garnered such attention. While rape cases against women are rarely prosecuted so vigorously, the brutality of the crime and the particulars of the case may have activated more traditional parts of Afghan society. In particular, the fact that the women were raped by strangers in front of their husbands differed widely from more typical occurrences in the home, where women are often ruled to be at fault through zina, or adultery law (Owen, 2014). In the context of the foreign troop withdrawal and surge in Taliban activity, the gang rape in a popular picnic area so close to the capital may have "tapped into a vein of anxiety" and reinforced an image of public insecurity (Shalizi, 2014; Najibullah, 2014).

While public demonstrations before and during the trial suggest many citizens supported the trial and verdict, international organizations were quick to rebuke the process. Allegations of coerced confessions from the suspects, beatings in custody, inadequate time to prepare a defense, and biased defense attorneys plagued the trial. Advocacy groups such as Human Rights Watch and Amnesty International described the trial as being "wholly

undermined by numerous due process violations and political interference” (Human Rights Watch, 2014). We discuss the implications of these violations in the conclusion.

Data and Design

The broad national and international attention paid to the Paghman trial presents an opportunity to evaluate the impact of televised proceedings on citizen behavior. To estimate the effects of the trial, we study Waves 24 and 26 of the Afghanistan Nationwide Quarterly Research (ANQAR) survey collected May/June 2014 and November/December 2014. The firm contracted to design and implement the survey—ACSOR (an Afghan subsidiary of D3)—selects and trains local enumerators in household and respondent selection, recording of questions, appropriate interview techniques, and secure use of contact sheets. The administrative district is the primary sampling unit (PSU), and districts are selected by probability proportional to size (PPS) systematic sampling. From these districts, secondary sampling units (villages/settlements) are randomly selected via sampling frames based on administrative records. A random walk method is used to identify sampled households, with a Kish grid technique used to randomize respondents from each target household. Before administering each wave, ACSOR reaches out to local elders to secure access to sampled settlements. We describe refusal/non-contact rates in the Online Appendix.

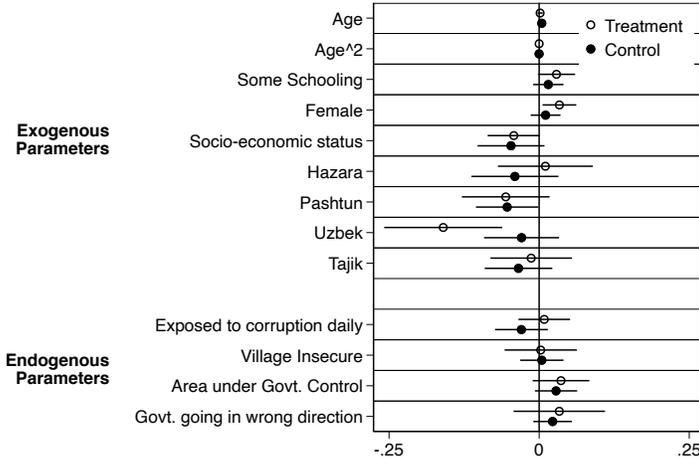
To causally identify the effect of the televised trial on attitudes toward justice institutions, we exploit the as-if random timing of the Paghman trial, following the approach taken in Balcells and Torrats-Espinosa (2018) and Mikulaschek et al. (2020). The August 23 assault that triggered the trial was unrelated to any national political and social developments that may have shaped public attitudes. It did not affect the timing of the ANQAR survey waves, the design of the survey, or the survey instrument, all of which were set *ex ante*. We therefore rely on the between-survey round timing of the Paghman trial to study how the televised

Table 1: Survey Instruments Overview

Variable	Question	Coding (= 1 if)
Legal Dispute to Govt Court	If you had a legal dispute, would you take it to an Afghanistan state court or a local Shura/Jirga?	State Court
Village Security Bad	How is the security situation in your mantaqa? Is it good, fair, or bad?	Bad
Govt in Wrong Direction	Generally speaking, do you believe the Government of Afghanistan is going in the right direction, the wrong direction, or is in the same place?	Wrong Direction
Govt Local Influence	Between the two, the Anti-Government Elements and Government, who has more influence in your mantaqa?	Government
Govt Corruption Affects Life	Do you believe that corruption in the Government affects your daily life?	Yes
Govt Improves Economy	How well does the Government of Afghanistan do its job in concern to improving the economy?	Well or Very Well
Govt Improve Development	How well does the Government of Afghanistan do its job in concern to development and reconstruction?	Well or Very Well
Govt Reducing Corruption	How well does the Government of Afghanistan do its job in concern to reducing corruption in the government?	Well or Very Well
Respondent Age	What is the age of the primary survey participant?	-
Education Level	What is the educational level?	≥Some Schooling
Household Count	How many people reside in your household	-

proceedings influenced attitudes towards government judicial institutions. Our empirical design follows the logic of a difference-in-difference estimator: we compare the preferences of respondents who state their most trusted source of news is television before and after the trial to those who trust other sources of information. Respondents who trust television may be systematically different than those who do not and thus have different preferences for conflict resolution mechanisms. Our design allows us to hold these general characteristics fixed as they are differenced out during estimation. However, because our survey is not longitudinal (sampled respondents vary by wave), some individual characteristics may vary among the surveyed populations.¹

Figure 1: Regression-Based Balance Tests across Waves and Study Groups



Notes: Outcome of interest is an indicator variable for post (equals 1 if a respondent is sampled in the survey wave after the televised trial). Ethnicity is split into four dummy variables for ethnic groups with at least 500 individuals sampled per wave. Samples are split into treatment and control groups following the main analysis (most trusted source of information is television = 1; 0 otherwise). Multivariate regressions are used to estimate coefficients and standard errors for samples by study group. Following the main regressions, standard errors are clustered by administrative district.

We address this concern in several ways. First, we investigate whether there were significant differences in respondent characteristics across the two groups by comparing exogenous

¹In the Online Appendix, we replicate the main analysis at the district-wave level to quantify any potential bias due to shifting individual-level preferences for media. We find no evidence that this type of bias is likely to influence our model estimates (see Table SI-5).

and potentially endogenous characteristics before and after the trial occurred, following Munoz et al. (2020). We use multivariate regressions to produce point estimates for our main and supplemental demographic and household characteristics, separately for our two groups, where the outcome is being sampled in the post period. We present the results of this exercise in Figure 1, which suggests significant balance across the two waves by group. The overall consistency holds for both exogenous characteristics as well as potentially endogenous factors, such as village security and exposure to corruption. The imbalances that do exist, for example the sampling of Uzbek subjects among those that trust television, can be accounted for in our baseline specification using control variables. Indeed, our baseline specification incorporates demographic characteristics, including age, education, gender, ethnicity, and socio-economic status. Other respondent attitudes, including general frustration with the government, security conditions, government control of the respondent’s area, and exposure to corruption might influence consumption of and confidence in media. We incorporate these control parameters as well.

We evaluate the impact of the Paghman trial on preferences for formal legal dispute resolution by studying equation (1):

$$y_i = \alpha + \beta_1 Post_i + \beta_2 Treatment_i + \beta_3 Post_i \times Treatment_i + \beta D_i + \beta X_i + \epsilon \quad (1)$$

Where y_i is the respondent’s choice to use government courts if they had a legal dispute. $Post_i$ takes the value of one if the respondent is surveyed after the trial (Wave 26). $Treatment_i$ indicates that the respondent’s most trusted source of news is television. To ease interpretation of the difference-in-differences estimator we are focused on, we use the standard descriptions of $Treatment_i$ and $Post_i$. This is not intended to imply that we are estimating an experimental effect. The context of our research is best characterized as a natural experiment. $Post_i \times Treatment_i$ captures the difference-in-differences estimator of the change in y_i of the treated (trust television) after treatment (the trial). D_i indicates

district level fixed effects and X_i is a vector of control variables, including a period-varying measure of television access and use. All models include age, age squared, gender, education, socio-economic status, and ethnicity as demographic controls. Robust standard errors are clustered by district. We hold the primary sample units fixed to ensure consistency in the sampled districts. All models are adjusted using population sampling weights.

Our survey contains no direct questions about awareness of and sentiments about the Paghman trial. That is, we cannot directly observe who watched the trial. Survey subjects enumerated in the wave prior to the trial would not have known about the unanticipated future trial or have been able to definitively state whether they would watch any hypothetical televised proceedings. Instead, we rely on trust in television as a mechanism for identifying individuals who are most likely to be exposed to and influenced by the criminal proceedings. In this respect, our identification strategy estimates an intention-to-treat effect. It is possible, however, that other political news might confound this estimate. For example, respondents trusting television might have also seen news stories about other government programs, including managing the economy, investing in development and reconstruction, or reducing corruption in public institutions, which might have led to a general increase in confidence in government, not just in legal institutions. Other nation-wide political shifts might have influenced preferences for government institutions as well. To disentangle these effects, we introduce several placebo checks which help us rule out such a broad growth in confidence in government functions. If we find that confidence in other government functions (economy, development, corruption) improved between periods, any change in preferences we observe for government courts will be difficult to attribute to the trial specifically and might be part of a larger upward trend in public trust in government. To address these concerns, we estimate equation (2):

$$gov_i = \alpha + \beta_1 Post_i + \beta_2 Treatment_i + \beta_3 Post_i \times Treatment_i + \beta D_i + \beta X_i + \epsilon \quad (2)$$

Where gov_i is a set of performance assessments of government management of the economy, development and reconstruction, and corruption. In equations (1) and (2), we parameterize instrument non-response using a set of indicator variables. All other components of the model remain the same.

Surveys relying on direct questions may yield biased estimates if respondents conceal their true preferences or beliefs. These concerns are difficult to address, but we consider several tests that should give us more confidence in the results we present if our results are unaffected. First, respondents uncomfortable with or who do not understand the survey might not give reliable answers. Enumerators were asked to classify interviews on both dimensions. Second, subjects from large households or who were interviewed in the presence of a large number of people might be more likely to give desirable answers. Data was collected on each of these measures as well. Finally, the quality of interviews (and subsequent responses) might vary within each survey as a function of the time enumerators have spent in the field collecting data. We tackle this concern in two ways. We can account for the survey wave day within each primary sampling unit (district) an interview took place, allowing us to control for any subtle changes that may have occurred from the beginning to end of each survey. Respondents might also be more more willing to reveal their true preferences if they have observed their enumerator (or survey team more generally) walking around their village and conducting interviews throughout the day. This would suggest that data collected later in the day is more reliable. Our survey data includes information that allows us to reconstruct the within-day interview sequence for each enumerator. We reproduce equation (1) incorporating these diagnostics.

Quantitative Evidence

Descriptive Statistics

Descriptive statistics reveal a large differential shift in preferences for government courts among subjects that trust television. Among the control population, the pre-trial court use rate was 43% while the post-trial rate was 42%. These two rates are statistically indistinguishable ($p = .437$). This suggests that the attitudes of the control population—those subjects that trust other sources of information (like radio, family and friends, and local religious leaders) more—are not impacted by the televised proceedings. If information about the trial spread through other sources (and not just via television), we would expect a shift in public support in this group. We observe no such change. Among the treated population, on the other hand, we observe a pre-trial rate of 53% and a post-trial rate of 61%, a statistically meaningful 8% increase ($p < .001$). This suggests that the attitudes towards the judiciary of subjects trusting television, on average, shifted substantially upward, consistent with our theoretical argument. It is important to recall that any systematic pre-trial differences in the treated and control groups are partialled out as the first difference in the difference-in-differences research design. That said, these descriptive difference-in-means tests do not allow us to address potentially confounding demographic factors. To do this, we turn to our regression estimates.

Regression-based Evidence

Table 2 reports the results from our main specifications following equation (1). Column 1 includes only district fixed effects and demographic controls. Our point estimate reveals a highly significant 7.1% increase in the likelihood that subjects trusting television reported a willingness to use government courts to resolve legal disputes *after* the Paghman trial.

Preferences for formal dispute resolution might be correlated with other respondent attitudes including their frustration with the direction of government, perceived insecurity of their village, and government control of their area. We sequentially add these covariates to our baseline specification in Columns 2-4. In Column 5, we incorporate a measure of subject exposure to corruption. The estimated treatment effect of the televised proceedings increases slightly when we add these covariates.

For ease of interpretation and feasibility of estimation with high dimensional fixed effects, we estimate the main results using linear probability models (LPM). Although homoskedasticity will be violated by construction, we present heteroskedasticity robust standard errors clustered by administrative district throughout the analysis. It is still possible that these LPM estimates are biased in magnitude since we are imposing a linearity condition. We address this potential concern in the Online Appendix using both logistic and probit regression specifications. These results are presented in Tables SI-1 and SI-2, where we calculate and present average partial effects derived from the analogous model specifications presented in Table 2.² The corresponding non-linear results suggest an estimated increase in preferences for government courts by 6.47% and 6.48% respectively. These marginal effects are statistically indistinguishable from the LPM specification presented in Table 2, suggesting any meaningful potential bias in coefficient magnitude is unlikely.

In Table 3, we conduct several placebo tests, outlined earlier. If the Paghman trial coincided with some other major political or economic reforms, we may be overestimating the effect of the televised trial. To assess this concern, we consider whether there are any changes in public confidence in other government functions, including managing the economy, investing in reconstruction and development, and cracking down on public corruption, after the trial among the television trusting subjects (relative to non-trusting subjects). Column

²For details on the calculation of the appropriate marginal effects under non-linear difference-in-differences, see Puhani (2012). We thank Patrick Puhani for additional guidance on this point.

Table 2: Estimates of televising trial proceedings on use of government courts for legal disputes

	(1)	(2)	(3)	(4)	(5)
Post	-0.000207 (0.0158)	0.00209 (0.0159)	0.00265 (0.0161)	0.00212 (0.0163)	0.00259 (0.0163)
Treatment	0.0149 (0.0153)	0.0135 (0.0152)	0.0120 (0.0151)	0.0112 (0.0152)	0.0105 (0.0152)
Post × Treatment	0.0715*** (0.0236)	0.0738*** (0.0232)	0.0742*** (0.0232)	0.0746*** (0.0233)	0.0746*** (0.0234)
SUMMARY STATISTICS					
Outcome Mean	0.499	0.499	0.499	0.499	0.499
Outcome SD	0.500	0.500	0.500	0.500	0.500
BASELINE PARAMETERS					
District FE	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes
ADDITIONAL PARAMETERS					
Attitude towards Govt.	No	Yes	Yes	Yes	Yes
Local Security	No	No	Yes	Yes	Yes
Govt. Control	No	No	No	Yes	Yes
Exposure to Corruption	No	No	No	No	Yes
MODEL STATISTICS					
N	24167	24167	24167	24167	24167
Clusters	293	293	293	293	293

Notes: Outcome is whether the respondent would take a legal dispute to a government court. Unit of analysis is individual survey respondent. All models include administrative district fixed effects (using ESOC boundaries), as well as demographic controls (age, education, gender, ethnicity, socio-economic status). Standard errors clustered at the district level and are presented in parentheses, stars indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

1 replicates our preferred specification from Table 2 (Column 5) for comparison. Columns 2-4 correspond to the three placebo conditions. We find no evidence of an upward trend in confidence of government performance along non-judicial dimensions. If anything, these assessments are consistently negative (not positive). In Table SI-3, we replicate these results using continuous measures of the placebo condition rather than a binary condition, which we used in the main analysis to ease interpretation. The results are substantively unchanged, with no meaningful shifts in public sentiment detected.³ In Column 5 of Table 3, we add these performance assessments as regressors in our main model specification. Our main

³Naturally the coefficient magnitudes differ due to the continuous scale of the outcome variable.

results are unaffected. The same is true in Table SI-3 as well. These tests give us more confidence in the estimated impact of the trial by helping to rule out a differential trend in public confidence in government within the treatment group.

Table 3: Estimates of televising trial proceedings on attitudes towards unrelated government functions (placebo tests)

	(1)	(2)	(3)	(4)	(5)
	Court Use	Economy	Dev/Reconstr.	Corruption	Court Use
Post	0.00259	-0.0328**	-0.0370**	0.00711	0.00309
	(0.0163)	(0.0158)	(0.0167)	(0.0128)	(0.0162)
Treatment	0.0105	0.0113	0.000536	-0.0210	0.0106
	(0.0152)	(0.0158)	(0.0143)	(0.0163)	(0.0151)
Post × Treatment	0.0746***	-0.00308	0.0211	-0.00576	0.0745***
	(0.0234)	(0.0226)	(0.0203)	(0.0184)	(0.0235)
SUMMARY STATISTICS					
Outcome Mean	0.499	0.332	0.406	0.211	0.499
Outcome SD	0.500	0.471	0.491	0.408	0.500
BASELINE PARAMETERS					
District FE	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes
ADDITIONAL PARAMETERS					
Attitude towards Govt.	Yes	Yes	Yes	Yes	Yes
Local Security	Yes	Yes	Yes	Yes	Yes
Govt. Control	Yes	Yes	Yes	Yes	Yes
Exposure to Corruption	Yes	Yes	Yes	Yes	Yes
Placebo Outcome	No	Yes	Yes	Yes	No
Additional Attitudes towards Govt.	No	No	No	No	Yes
MODEL STATISTICS					
N	24167	24213	24297	24301	24167
Clusters	293	293	293	293	293

Notes: Outcomes vary: column 1/5 are legal dispute resolution, column 2/3/4 are whether the government handling of the economy (2), reconstruction (3), and corruption (4) well. Baseline model specification is most saturated specification in Table 2 Column (5). Unit of analysis is individual survey respondent. All models include administrative district fixed effects (using ESOC boundaries), as well as demographic controls (age, education, gender, ethnicity, socio-economic status). Standard errors clustered at the district level and are presented in parentheses, stars indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

In Table 4, we produce several diagnostic tests to address concerns about social desirability bias. In Column 1, we reproduce our main specification from Table 2 (Column 5). In Column 2, we account for whether the survey respondent was comfortable with the survey and understood most of the survey instruments (separate parameters). In Column 3, we ad-

dress potential concerns about household size and the number of people present during the interview, both of which may increase the likelihood the subject does not answer truthfully. Column 4 accounts for the date (within each district-wave) an interview was conducted and column 5 incorporates a measure of the within-day sequence of enumeration. Our estimated treatment effects are large, stable (within .3% of the main estimate), and precise, suggesting that our main results are unlikely to be substantially influenced by biased responses.

Table 4: Estimates of televising trial proceedings on use of government courts for legal disputes, accounting for potential sources of survey bias

	(1)	(2)	(3)	(4)	(5)
Post	-0.000207 (0.0158)	0.00209 (0.0159)	0.00265 (0.0161)	0.00212 (0.0163)	0.00259 (0.0163)
Treatment	0.0149 (0.0153)	0.0135 (0.0152)	0.0120 (0.0151)	0.0112 (0.0152)	0.0105 (0.0152)
Post \times Treatment	0.0715*** (0.0236)	0.0738*** (0.0232)	0.0742*** (0.0232)	0.0746*** (0.0233)	0.0746*** (0.0234)
SUMMARY STATISTICS					
Outcome Mean	0.499	0.499	0.499	0.499	0.499
Outcome SD	0.500	0.500	0.500	0.500	0.500
BASELINE PARAMETERS					
District FE	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes
ADDITIONAL PARAMETERS					
Attitude towards Govt.	No	Yes	Yes	Yes	Yes
Local Security	No	No	Yes	Yes	Yes
Govt. Control	No	No	No	Yes	Yes
Exposure to Corruption	No	No	No	No	Yes
MODEL STATISTICS					
N	24167	24167	24167	24167	24167
Clusters	293	293	293	293	293

Notes: Outcome is whether the respondent would take a legal dispute to a government court. Outcomes in Panel B vary: column 1/5 are legal dispute resolution, column 2/3/4 are whether the government handling of the economy (2), reconstruction (3), and corruption (4) well. Baseline model specification in Panels B and C is most saturated specification in Panel A (5). Unit of analysis is individual survey respondent. All models include administrative district fixed effects (using ESOC boundaries), as well as demographic controls (age, education, gender, ethnicity, socio-economic status). Standard errors clustered at the district level and are presented in parentheses, stars indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Discussion

We find strong evidence that a high profile televised trial in Afghanistan increased the public's willingness to use government courts. In a context of high contestation between potential dispute resolution forums (state judiciary, jirgas, and Taliban courts), these results show that increasing exposure to trial proceedings may bolster demand for formal legal institutions. This suggests that televising trials may offer a potent tool for statebuilders seeking to increase popular buy-in to their political projects.

In considering the implications of these results, two caveats bear mentioning. First, the generalizability of these findings may vary by context. In societies where television is less consumed or less trusted than in Afghanistan, or where coverage of the justice system is sharply critical, televised trials may not have the same effect on support for state institutions. Similarly, the effect of televised trials may vary depending on public opinion about the defendants, plaintiffs, and verdict of the proceedings.

Second, the effect of televised proceedings may be fleeting. It is difficult to assess whether the Paghman trial had long-run effects on public use of government courts. As the first televised trial in Afghan history, we may be estimating the upper bound on how much citizen preferences can shift in the wake of a high profile court case. Such attitudinal changes might not persist or be observed in later televised proceedings or in other legal contexts, where exposure to judicial institutions is already high.

In the Online Appendix, we present preliminary evidence of the longer-run impact of the Paghman trial. Recalling that our identification strategy is most valid when comparing political attitudes right before and after the televised trial, we take advantage of survey data collected six months after the trial. We find evidence that the overall effect attenuates. Once we separately estimate the short and longer-run effects, the downstream effect is no longer statistically significant at conventional levels. Though these results should be interpreted

with due caution, they suggest the effects of the trial may not persist long after the trial occurred.

Conclusion

How do states generate popular support for their political projects? We have argued that a crucial way to build legitimacy during civil war is by developing popular dispute resolution institutions. Importantly, simply creating effective dispute resolution services is not sufficient to foster legitimacy. Rather, states must be able to publicize those services through mass media to induce meaningful shifts in public opinion and behavior.

We evaluate this claim against evidence from contemporary Afghanistan, where the state justice system competes for popular support with Taliban tribunals and traditional jirgas. We find that exposure to media coverage of a high-profile trial increased the probability that respondents would take their disputes to government courts. This result has implications for theories of competitive statebuilding, governance, and the rule of law. We highlight two main implications below.

Procedural Fairness and the “Messaging Curse”

The Paghman trial and its consequences point to an important, normatively troubling implication of our findings. Popular trials may not be just. As discussed above, the Paghman trial was deeply deficient in its respect for due process and human rights. Human rights and advocacy organizations catalogued these flaws: Karzai’s public statements in the lead-up to the trial were prejudicial, the proceedings were cursory, the defendants’ confessions may have been coerced, and their sentences were draconian.

These serious violations reveal a dark irony in the effects of the Paghman trial. In a bid to demonstrate the legitimacy of state courts at the expense of their insurgent rival, the

Karzai government and prosecutors crafted a trial that fundamentally resembled a Taliban court—a swift, decisive verdict with little regard for due process or the civil liberties of the accused. The Paghman trial thus highlights a common dilemma for would-be statebuilders. How can states balance public demand for speedy and decisive verdicts with adherence to legal norms? More broadly, how should legitimacy-seeking authorities manage the trade-offs between popular demands and respect for the law and civil liberties?

Equally troubling, the success of televised proceedings in the Paghman trial suggests that effective information dissemination can substitute for good governance. Like the “resource curse” (e.g. Ross, 2015), in which natural resource endowments relieve states of the necessity to develop extractive and administrative institutions, the effectiveness of information dissemination may create a “messaging curse.” If political authorities can generate popular support through messaging alone, they need not invest in improving the quality and accountability of their institutions. Propaganda offers an inexpensive—if temporary—alternative to the substantial political and material costs of institution-building. Rulers may be tempted to forgo good governance for good marketing.

Non-Violent Markets for Governance

Civil wars are not the only contexts in which markets for governance emerge. In many cases, the existence of the modern bureaucratic state does not imply a monopoly on governance. In a large majority of contemporary countries, (some) citizens are governed by traditional political institutions—“institutions whose legitimacy is based in part on their association with customary modes of governing a community” (Baldwin and Holzinger, 2019). These traditional or customary governance structures coexist with autocratic, anocratic, and democratic states. Traditional authorities provide public goods, interface between citizens and formal institutions, and adjudicate disputes (Diaz-Cayeros et al., 2014; Casey et al., 2018; Baldwin

and Holzinger, 2019).⁴ Beyond traditional authorities, some criminal organizations provide dispute resolution services and ensure contracts for their members, business partners, and local civilians (Arias, 2006; Skarbek, 2011). Civil society becomes involved in governance, as well: in cases of material inequality or low state legitimacy, groups of citizens sometimes take it upon themselves to adjudicate disputes through vigilante justice (Nivette, 2016; Phillips, 2017).

As in civil war, we expect that information dissemination offers a powerful tool for state-building in these settings of competing governance institutions. Formal and informal dispute resolution institutions almost always overlap in their jurisdiction: state courts accept cases in the same areas where traditional authorities, civil society, or criminal actors govern. As a result, citizens often have the choice between formal justice institutions and traditional authorities, private sector dispute adjudication, gang rule, or vigilante justice. Like in the case we study, that choice is often driven by popular perceptions of the comparative quality and efficacy of these institutions (e.g. Sage, 2005; Macfarlane, 2007; Bowd, 2009; Flomoku and Reeves, 2012; Opasina, 2017). Signals about the relative quality of these institutions should consequently affect which of these options citizens choose—and, as a result, which political and social projects prosper and which wither.

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⁴Dispute resolution is where traditional governance structures often hold the most sway. Many states, including Ethiopia, South Africa, and Sierra Leone, recognize the legitimacy of customary law in their constitutions. Customary legal institutions often undergird large portions of the economy—in Ghana and Mozambique, around 90% of transactions involving land tenure are governed by traditional authorities (Chirayath et al., 2005).

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Online Appendix to “Televising Justice during War”

Supplemental Figures

SI-1 ANQAR diagnostics during waves conducted by firm collecting Wave 8 survey data (ACSOR) SI-3 (Online Only)

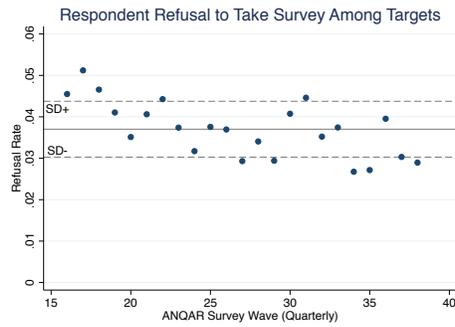
Supplemental Tables

SI-1 Logisitic regression estimates of televising trial proceedings on use of government courts for legal disputes (average partial effects) SI-4 (Online Only)
SI-2 Probit regression estimates of televising trial proceedings on use of government courts for legal disputes (average partial effects) SI-5 (Online Only)
SI-3 Estimates of televising trial proceedings on attitudes towards unrelated government functions (placebo tests) with continuous outcomes . SI-6 (Online Only)
SI-4 Longer run estimates of televising trial proceedings on use of government courts for legal disputes SI-10 (Online Only)
SI-5 District-level estimates of televising trial proceedings on use of government courts for legal disputes SI-12 (Online Only)

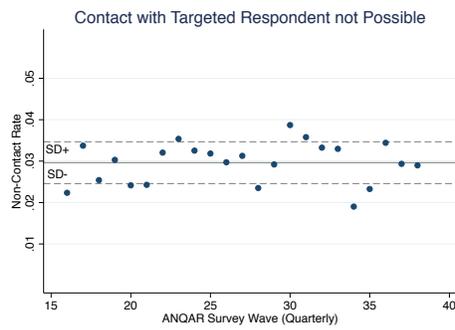
A Survey Diagnostics

Previous work points out several potential issues with the survey administration of the ANQAR platform. Blair et al. (2014), for example, note high refusal and non-contact rates (nearly 50%) observed during Wave 13, conducted in November to December 2011. It is important to note that a different firm (not ACSOR) administered Waves 11 through 15. This firm did not employ local enumerators and deviated from other ACSOR standards when collecting data. Fortunately, NATO kept records of participation and non-contact rates during Waves 24 and 26 (used in this study). In Figure SI-1, we plot these rates for waves 16 through 38, all administered by the firm collecting our data. We reproduce these figures from Condra and Wright (2019) for accessibility. Across these waves, ACSOR's cooperation rate exceeds 94%, with an average of 96%. The refusal rate during this set of survey waves never exceeds 5% (mean = 3.5%). The non-contact rate similarly ranges from 1.9% to 3.9% (mean = 3%). These are consistent with or better than national surveys conducted in the United States (such as ANES) and other developed countries (BHPS in the UK and HILDA in Australia). These differences are, in part, due to the fact that ACSOR conducts door-to-door interviews, rather than telephone-based enumeration. Overall, these diagnostic trends give us confidence in the overall design and implementation of the survey by ACSOR.

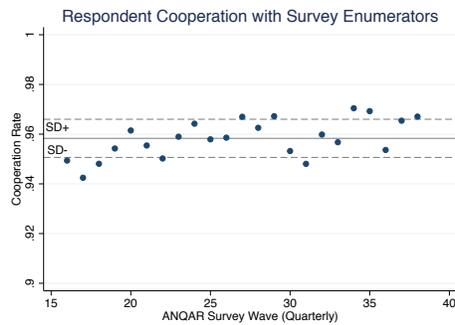
Figure SI-1: ANQAR diagnostics during waves conducted by firm collecting Wave 8 survey data (ACSOR)



(a) Refusal rate



(b) Non-contact rate



(c) Cooperation rate

Notes: data on refusal, non-contact, and overall cooperation were shared with the authors by NATO.

Table SI-1: Logistic regression estimates of televising trial proceedings on use of government courts for legal disputes (average partial effects)

	(1)	(2)	(3)	(4)	(5)
Post	-0.00318 (0.0150)	-0.000913 (0.0151)	-0.000822 (0.0154)	-0.00151 (0.0156)	-0.000935 (0.0156)
Treatment	0.0218 (0.0159)	0.0206 (0.0160)	0.0187 (0.0159)	0.0177 (0.0159)	0.0171 (0.0158)
Post × Treatment	0.0647*** (0.0229)	0.0655*** (0.0225)	0.0665*** (0.0224)	0.0670*** (0.0224)	0.0671*** (0.0224)
SUMMARY STATISTICS					
Outcome Mean	0.479	0.479	0.479	0.479	0.479
Outcome SD	0.500	0.500	0.500	0.500	0.500
BASELINE PARAMETERS					
District FE	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes
ADDITIONAL PARAMETERS					
Attitude towards Govt.	No	Yes	Yes	Yes	Yes
Local Security	No	No	Yes	Yes	Yes
Govt. Control	No	No	No	Yes	Yes
Exposure to Corruption	No	No	No	No	Yes
MODEL STATISTICS					
N	24122	24122	24122	24122	24122
Clusters	291	291	291	291	291

Notes: Outcome is whether the respondent would take a legal dispute to a government court. Unit of analysis is individual survey respondent. All models include administrative district fixed effects (using ESOC boundaries), as well as demographic controls (age, education, gender, ethnicity, socio-economic status). Average partial effects of logistic regressions reported. Standard errors clustered at the district level and are presented in parentheses, stars indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table SI-2: Probit regression estimates of televising trial proceedings on use of government courts for legal disputes (average partial effects)

	(1)	(2)	(3)	(4)	(5)
Post	-0.00282 (0.0150)	-0.000857 (0.0151)	-0.00105 (0.0154)	-0.00185 (0.0156)	-0.00131 (0.0156)
Treatment	0.0219 (0.0162)	0.0205 (0.0162)	0.0188 (0.0160)	0.0177 (0.0160)	0.0172 (0.0160)
Post × Treatment	0.0648*** (0.0233)	0.0663*** (0.0229)	0.0669*** (0.0228)	0.0676*** (0.0228)	0.0676*** (0.0228)
SUMMARY STATISTICS					
Outcome Mean	0.479	0.479	0.479	0.479	0.479
Outcome SD	0.500	0.500	0.500	0.500	0.500
BASELINE PARAMETERS					
District FE	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes
ADDITIONAL PARAMETERS					
Attitude towards Govt.	No	Yes	Yes	Yes	Yes
Local Security	No	No	Yes	Yes	Yes
Govt. Control	No	No	No	Yes	Yes
Exposure to Corruption	No	No	No	No	Yes
MODEL STATISTICS					
N	24122	24122	24122	24122	24122
Clusters	291	291	291	291	291

Notes: Outcome is whether the respondent would take a legal dispute to a government court. Unit of analysis is individual survey respondent. All models include administrative district fixed effects (using ESOC boundaries), as well as demographic controls (age, education, gender, ethnicity, socio-economic status). Average partial effects of probit regressions reported. Standard errors clustered at the district level and are presented in parentheses, stars indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table SI-3: Estimates of televising trial proceedings on attitudes towards unrelated government functions (placebo tests) with continuous outcomes

	(1)	(2)	(3)	(4)	(5)
	Court Use	Economy (c)	Dev/Reconstr. (c)	Corruption (c)	Court Use
Post	0.00259 (0.0163)	-0.0596 (0.0412)	-0.0876** (0.0389)	0.0586 (0.0373)	0.00265 (0.0163)
Treatment	0.0105 (0.0152)	0.0413 (0.0365)	-0.00619 (0.0314)	-0.0366 (0.0635)	0.00778 (0.0154)
Post × Treatment	0.0746*** (0.0234)	-0.0581 (0.0590)	0.0703 (0.0486)	0.0110 (0.0624)	0.0777*** (0.0231)
SUMMARY STATISTICS					
Outcome Mean	0.499	2.926	3.131	2.504	0.498
Outcome SD	0.500	1.114	1.095	1.150	0.500
BASELINE PARAMETERS					
District FE	Yes	Yes	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes	Yes	Yes
ADDITIONAL PARAMETERS					
Attitude towards Govt.	Yes	Yes	Yes	Yes	Yes
Local Security	Yes	Yes	Yes	Yes	Yes
Govt. Control	Yes	Yes	Yes	Yes	Yes
Exposure to Corruption	Yes	Yes	Yes	Yes	Yes
Continuous Placebo Outcome	No	Yes	Yes	Yes	No
Additional Attitudes towards Govt.	No	No	No	No	Yes
MODEL STATISTICS					
N	24167	24213	24297	24301	23866
Clusters	293	293	293	293	293

Notes: Outcomes vary: column 1/5 are legal dispute resolution, column 2/3/4 are how the government handling of the economy (2), reconstruction (3), and corruption (4) along a continuous five point scale (with higher values indicating more positive respondent evaluations). Baseline model specification is most saturated specification in Table 2 Column (5). Unit of analysis is individual survey respondent. All models include administrative district fixed effects (using ESOC boundaries), as well as demographic controls (age, education, gender, ethnicity, socio-economic status). The size of the sample varies in Column (5) due to item non-response being excluded from the construction of the continuous scale. Standard errors clustered at the district level and are presented in parentheses, stars indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B Long run effects of televised trial proceedings

A natural extension of this research asks: if these televised proceedings impacted public support for government judicial institutions, did this shift in public attitudes last? It is difficult to credibly answer this question with our identification strategy. Our research design exploits the fact that some survey respondents were more likely to be exposed to and influenced by the Paghman trial than others. For our design, television is the medium of exposure to treatment. Because of this, it is possible that our estimates may be confounded by subsequent political or economic events that differentially impact survey respondents that do and do not trust television as we move further in time away from the trial itself. Our inferences about the impact of the trial most valid in the window around the trial.

B.1 Baseline Design

With that caveat in mind, as a first pass to address this question we process data from the subsequent survey wave (Wave 27) following our post period wave. Fortunately, Wave 27 also included the main outcome of interest, this being willingness to use government courts to resolve legal disputes. We introduce two supplemental difference-in-differences specifications. First, we treat waves 26 and 27 as a single post period. $Post_i$ takes the value of one if the respondent is surveyed after the trial (Wave 26 *or* 27). If the effect is attenuated as we move further away from the information shock, we would expect the merged difference-in-differences effect to be smaller than the baseline specification with only Wave 26. Second, to further probe potential attenuation of the main effect, we separately estimate the effects of the first (Wave 26) and second (Wave 27) post periods. For the first specification, we study:

$$y_i = \alpha + \beta_1 Post_i^{combined} + \beta_2 Treatment_i + \beta_3 Post_i^{combined} \times Treatment_i + \beta D_i + \beta X_i + \epsilon \quad (3)$$

Where y_i is the respondent's choice to use government courts if they had a legal dispute. $Post_i^{combined}$ takes the value of one if the respondent is surveyed after the trial (Wave 26 or 27). $Treatment_i$ indicates that the respondent's most trusted source of news is television. Therefore, $Post_i^{combined} \times Treatment_i$ captures the difference-in-difference estimator of the change in y_i of the treated (trust television) after treatment (the trial). All other elements and controls of the model are held constant from the main analysis.

To uncouple the two post period effects, we study:

$$y_i = \alpha + \beta_1 Post_i^{first} + \beta_2 Post_i^{second} + \beta_3 Treatment_i + \beta_4 Post_i^{first} \times Treatment_i + \beta_5 Post_i^{second} \times Treatment_i + \beta D_i + \beta X_i + \epsilon \quad (4)$$

Where y_i is the respondent's choice to use government courts if they had a legal dispute. $Post_i^{first}$ takes the value of one if the respondent is surveyed in the first period after the trial (Wave 26). $Post_i^{second}$ takes the value of one if the respondent is surveyed in the second period after the trial (Wave 26). $Treatment_i$ indicates that the respondent's most trusted source of news is television. $Post_i^{first} \times Treatment_i$ captures the difference-in-difference estimator of the change in y_i of the treated (trust television) in the wave immediately after the trial (Wave 26). $Post_i^{second} \times Treatment_i$ captures the difference-in-difference estimator of the change in y_i of the treated (trust television) in the wave roughly six months after the trial (Wave 27). Due to potentially confounding factors that may differentially impact control and treatment groups, we believe the most valid estimate of the Paghman trial's impact on support for judicial institutions is estimated by β_4 with respect to $Post_i^{first} \times Treatment_i$. Again, all other elements of the model are held constant from the main analysis.

B.2 Results

We introduce supplemental results in Table SI-4. Column 1 reproduces the benchmark estimate from the most saturated model in Table 2 (Column 5). We compare this to Column 2, which introduces the combined post period specification in equation 3. Notice that the difference-in-differences estimate is smaller, although it remains statistically precise. This suggests that the main effect is attenuated by adding respondents enumerated further away from the timing of trial proceedings. To confirm this, we consider equation 4, and compare β_4 and β_5 in Column 3. Indeed, notice that the large upward swing in willingness to utilize government judicial institutions just after the trial is substantially reduced in magnitude and precision six months after the Paghman trial.

These results suggest the effect of the trial that our research design allows us to estimate did not persist. This could be for at least two reasons. One, the trial did not have a lasting impact on public attitudes. The initial information shock might have dissipated and citizens no longer put weight on the signal sent by the televised trial. Or, second, other salient political or economic factors could have passed through our mechanism of treatment (television) and confounded our estimate of the longer-run effect. As we note above, we cannot credibly differentiate these two potential explanations for the attenuation we observe. We hope these results motivate future research on the long run effects of trial proceedings. To advance this goal, we introduce a list of other televised criminal and political trials above, along with relevant citations.

Table SI-4: Longer run estimates of televising trial proceedings on use of government courts for legal disputes

	(1)	(2)	(3)
	Robust baseline	Pooled post period model	Split post period model
Post (Baseline)	0.00259 (0.0163)		
Post (Combined Post Periods)		-0.0107 (0.0134)	
Post (first)			0.000968 (0.0164)
Post (second)			-0.0227 (0.0152)
Treatment	0.0105 (0.0152)	0.0121 (0.0147)	0.0124 (0.0147)
Post (Baseline) × Treatment	0.0746*** (0.0234)		
Post (Combined) × Treatment		0.0430** (0.0187)	
Post (first) × Treatment			0.0768*** (0.0244)
Post (second) × Treatment			0.0115 (0.0200)
SUMMARY STATISTICS			
Outcome Mean	0.499	0.497	0.497
Outcome SD	0.500	0.500	0.500
BASELINE PARAMETERS			
District FE	Yes	Yes	Yes
Demographic Controls	Yes	Yes	Yes
ADDITIONAL PARAMETERS			
Attitude towards Govt.	Yes	Yes	Yes
Local Security	Yes	Yes	Yes
Govt. Control	Yes	Yes	Yes
Exposure to Corruption	Yes	Yes	Yes
MODEL STATISTICS			
N	24167	36588	36588
Clusters	293	302	302

Notes: Outcome is whether the respondent would take a legal dispute to a government court. Baseline model specification is most saturated specification in Column (5) of Table 2 in main text. Unit of analysis is individual survey respondent. All models include administrative district fixed effects (using ESOC boundaries), as well as demographic controls (age, education, gender, ethnicity, socio-economic status). The size of the sample varies across Columns (1) and (2), reflecting the inclusion of an additional wave of survey data. Standard errors clustered at the district level and are presented in parentheses, stars indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C District-level difference-in-difference estimates

In the main analysis, we rely on individual-level survey data to compute the change in support for government courts after the televised trial, comparing people who do and do not trust television as their primary source of information. Our survey is not longitudinal, which means that survey respondents vary by wave. Although we can account for a battery of demographic characteristics as well as political and economic characteristics, it is still possible that airing the trial may have changed any given subject's preferences for media sources. We anticipate this is unlikely, but it would suggest our main estimate is potentially biased upward. To quantify if these changing preferences meaningfully bias the main results, we collapse our individual data to the district-level (by wave). This allows us to replicate the main result in two ways. First, we can repeat the main analysis where each observation is now district-wave and the outcome is the percentage of district respondents that would use a government court if they had a legal dispute and the treatment (exposure) measure is the percentage of district respondents that trust television as their primary source of information. We allow the district-level treatment to vary between waves. We can then, second, hold the treatment fixed using the pre-trial mean. We present these results in Table SI-5. We find that the point estimates are not statistically significantly different from one another, though the model that holds preferences fixed produces a larger effect in magnitude. This suggests our main effects are potentially lower bounds (i.e., not upwardly biased) with respect to bias from time-shifting preferences for alternative media sources.

Table SI-5: District-level estimates of televising trial proceedings on use of government courts for legal disputes

	(1)	(2)
	Wave varying district treatment	Wave invariant district treatment
Post × Treatment	0.216*	
	(0.115)	
Post × Treatment (Wave Invariant)		0.246**
		(0.103)
SUMMARY STATISTICS		
Outcome Mean	0.427	0.427
Outcome SD	0.216	0.216
BASELINE PARAMETERS		
District FE	Yes	Yes
Wave FE	Yes	Yes
MODEL STATISTICS		
N	586	586
Clusters	293	293

Notes: Outcome in percentage of district respondents that would take a legal dispute to a government court by wave. Treatment varies by column. In Column 1, it is the percentage of district respondents that trust television as their primary source of information. This varies by wave. In Column 2, we use the same measure, restricted to the pre-trial level. Unit of analysis is district by wave. All models include administrative district fixed effects (using ESOC boundaries) as well as wave fixed effects. Standard errors clustered at the district level and are presented in parentheses, stars indicate *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.