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 court 'uptake' 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.

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Civil war is fundamentally 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 that these two levels of competition are deeply intertwined: coercive success engenders political legitimacy, while effective and 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 which provider of governance to reward with their loyalty. To that end, both states and insurgents invest resources in state building, creating or enhancing governance institutions to win civilian 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). The importance of dispute resolution systems is reflected by their prevalence among aspiring 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).

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 legal institutions during conflict. Television news shapes public preferences for punitive justice (Gilliam Jr and Iyengar, 2000). Televised trials might 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). Historically, televised trials have played this role during periods of political instability. The trials of Saddam Hussein, Hosni Mubarak, and war criminals in Uganda are poignant exam-

ples of how televised trials can enhance awareness of and confidence in judicial institutions.

Our central argument is that televised legal proceedings enhance the ability of the government to compete with other mechanisms for resolving grievances. Televised proceedings increase public awareness of how formal courts function and enhance trial transparency. As such, we anticipate that public 'uptake' of government legal institutions will increase following televised proceedings. 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. These results suggest television may enable state institutions to compete for legitimacy during civil war.

Background

Nearly two decades after the Western intervention to overthrow the Taliban, the Afghan state continues to struggle with ineffective public goods provision, corruption, capture, and impunity. In particular, weak judicial institutions and inept dispute resolution services have hamstrung state building attempts (Swenson, 2017). Government judicial systems must compete with customary law, administered by local jirgas (councils), and Islamic courts established by the Taliban. Despite substantial investment by the international and NGO community, Afghan government courts remain plagued by dysfunction, corruption, and inefficiency. In 2014, less than half of Afghans sought resolution of a dispute went through the formal court system; most opted for informal jirgas. In contrast, the Taliban system of informal courts has proved popular, dispensing quick, inexpensive, and relatively unbiased—if harsh—justice (Farrell and Giustozzi, 2013).

In a competitive environment, how can states signal the value of their legal institutions?

One mechanism is through televising proceedings. This allows the public to observe legal institutions at work, altering their information set, and perhaps shifting prior beliefs about the legitimacy of the trial and state courts more broadly. We study this in Afghanistan by leveraging the timing of the country's first televised trial.[1] On September 6, 2014, seven men were tried in a Kabul court for the August 23 assault and rape of four women in the district of Paghman, just outside the capital city. The alleged assault triggered a public outcry for justice. President Hamid Karzai publicly called for the men to receive the harshest sentences possible. In an unexpected move, the proceedings were nationally televised. Confessions of the accused were read aloud to the cameras. Victims were allowed to physically confront the defendants. Statements by the prosecution were also broadcast, as well as the court proceedings. All seven men were found guilty and sentenced to death, and five of the seven were executed on October 8, 2014.

Data and Design

To estimate the effects of this trial, we study Wave 24 and 26 of the Afghanistan Nationwide Quarterly Research (ANQAR) survey collected from May/June 2014 and November/December 2014. The firm contracted to design and implement the survey is ACSOR (an Afghan subsidiary of D3). Local (to survey region) enumerators are selected by ACSOR and trained in proper 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 via probability proportional to size (PPS) systematic sampling. Among sampled districts, secondary sampling units (villages/settlements) are randomly selected from a sampling frame based on administrative records. A random walk method is used to identify sampled households and a Kish grid technique is used to randomize the respondent within each target household. Before

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[1]For additional details, see `https://tinyurl.com/y9q2yxef`.

administering each survey wave, ACSOR reaches out to local elders in order to secure access to sampled settlements. We describe refusal and non-contact rates in the Supporting Information section.

We rely on the between-survey round timing of the Paghman trial to study how the televised proceedings influenced public 'uptake' of 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. We address this concern in our baseline specification by incorporating 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 parameters as well.

We evaluate the impact of the Paghman trial on public 'uptake' of judicial institutions. We begin by studying equation (1):

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

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. $Post_i \times Treatment_i$ captures the difference-in-difference estimator of the change in y_i of the

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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. 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. But other political news might confound the relationship we are trying to 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):

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

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.

Results

Descriptive statistics reveal a substantial increase in court 'uptake' 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 ($p =.495$). Among the treated population, however, we observe a pre-trial rate of 53% and a post-trial rate of 61%, a statistically meaningful 8% increase ($p < .001$). These difference-in-means

tests do not allow us to address potentially confounding demographic factors. To do this, we turn to our regression estimates.

Table 1 Panel A 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.

In Panel B, 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 1 replicates our preferred specification from Panel A (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 Column 5, we add these performance assessments as regressors in our main model specification. Our main results are unaffected. These tests give us more confidence in the estimated impact of the trial by helping to rule out a secular trend in public confidence in government within the treatment group.

In Panel C, we produce several diagnostic tests to address concerns about social desir-

ability bias. In Column 1, we reproduce our main specification from Panel A (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 address 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.

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 (judiciary, jirgas, and Taliban courts), these results suggest that increasing exposure to trial proceedings may enhance demand for formal legal institutions.

Yet it is difficult to assess whether this 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 be matched in later televised proceedings or in other legal contexts, where exposure to judicial institutions is already high.

The event we study also illustrates that popular trials may not be just. After the Paghman trial, numerous human rights organizations harshly criticized the court's conduct: Karzai's public statements were prejudicial, the defendants' confessions may have been coerced, and their punishments were severe. This highlights a common dilemma–how can states balance speedy and decisive verdicts with adherence to legal norms? Emerging democracies must find ways to signal the value of their institutions during war without compromising the

core values of impartiality before the law and due process.

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Table 1: Estimates of televising trial proceedings on use of government courts for legal disputes

	Panel A: Baseline specifications				
	Column 1	Column 2	Column 3	Column 4	Column 5
Post	-0.000217	0.00208	0.00264	0.00211	0.00258
	(0.0158)	(0.0159)	(0.0161)	(0.0163)	(0.0163)
Treatment	0.0149	0.0135	0.0120	0.0112	0.0105
	(0.0153)	(0.0152)	(0.0151)	(0.0152)	(0.0152)
Post × Treatment	0.0715***	0.0738***	0.0742***	0.0746***	0.0746***
	(0.0236)	(0.0232)	(0.0232)	(0.0233)	(0.0234)
N	24167	24167	24167	24167	24167
Clusters	293	293	293	293	293

	Panel B: Placebo tests (other government functions)				
	Column 1	Column 2	Column 3	Column 4	Column 5
Post	0.00258	-0.0209**	-0.0198**	-0.00362	0.00352
	(0.0163)	(0.00885)	(0.00895)	(0.00642)	(0.0162)
Treatment	0.0105	0.00337	0.00770	0.00484	0.0107
	(0.0152)	(0.00784)	(0.00793)	(0.00701)	(0.0152)
Post × Treatment	0.0746***	-0.0128	-0.00705	-0.0145	0.0742***
	(0.0234)	(0.0113)	(0.0119)	(0.00997)	(0.0235)
N	24167	24213	24297	24301	24167
Clusters	293	293	293	293	293

	Panel C: Baseline specifications with survey diagnostic tests				
	Column 1	Column 2	Column 3	Column 4	Column 5
Post	0.00258	0.00292	0.00286	0.0639***	0.00251
	(0.0163)	(0.0161)	(0.0162)	(0.0172)	(0.0163)
Treatment	0.0105	0.0112	0.0105	0.0121	0.00996
	(0.0152)	(0.0151)	(0.0152)	(0.0157)	(0.0152)
Post × Treatment	0.0746***	0.0736***	0.0740***	0.0718***	0.0760***
	(0.0234)	(0.0232)	(0.0234)	(0.0221)	(0.0231)
N	24167	24167	24162	24167	24167
Clusters	293	293	293	293	293

Notes: Outcome in Panels A and C 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. 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 *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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. 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 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)
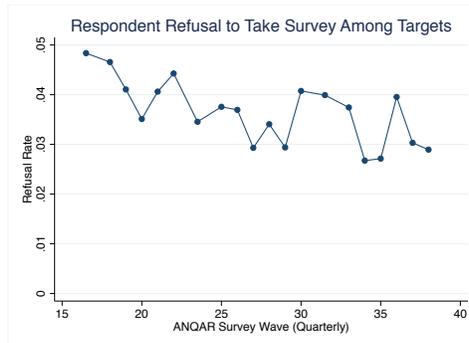
(a) Refusal rate
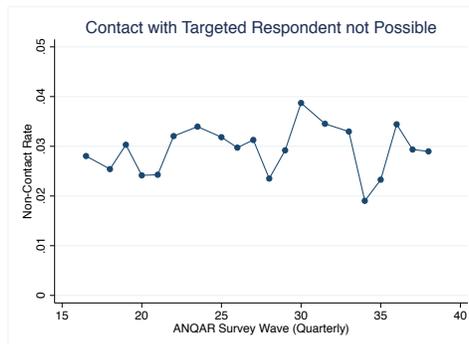
(b) Non-contact rate
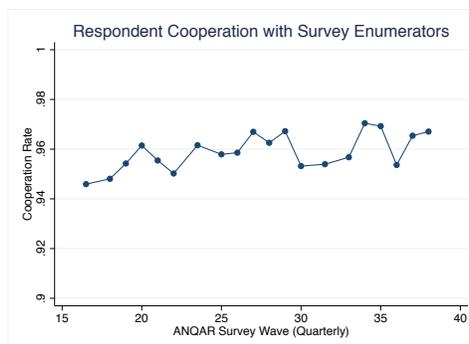
(c) Cooperation rate

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