Trust in State and Nonstate Actors: Evidence from Dispute Resolution in Pakistan

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This paper investigates whether information about improved public services can help build trust in state institutions and move people away from nonstate actors. We find that (truthful) information about reduced delays in state courts in rural Pakistan leads to citizens reporting higher likelihood of using them and to greater allocations to the state in high-stakes lab games. We also find negative indirect effects on nonstate actors and show that these changes are a response to improved beliefs about state actors, which make individuals interact less with nonstate actors and, we argue, induce them to downgrade their beliefs about these actors.

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I. Introduction

Endemic lack of state capacity is one of the most challenging problems facing many less developed societies around the world.[1] Though the ineffectiveness of state institutions has complex historical and contemporary causes ranging from institutional deficiencies to corruption and lack of adequate resources, it becomes exacerbated as it undermines trust in state institutions and belief in their ability to provide basic services. This problem is further intensified as powerful nonstate actors step in to fill the void, providing competing services such as protection, conflict resolution, and broader public goods (for case studies in the context of various Middle Eastern countries, see, e.g., Clark 2004; Harmsen 2010; Roy 2013). The shifting balance between state and nonstate actors may even create a feedback cycle where state weakness leads to more interactions with and greater trust in nonstate actors, which then fuels even closer association with them and less engagement with the state.

Although this interplay between state and nonstate actors is plausible, there is little direct evidence that the strength of nonstate actors derives from the weakness of state institutions. Similarly, to the best of our knowledge, there has been no investigation of whether deep-rooted mistrust in state institutions can be redressed. In this paper, we study these issues using relatively high-stakes lab-in-the-field experiments in rural Punjab, Pakistan. Our study aims to shed light on two related questions. First, we investigate whether providing (truthful) media-reported information about improved service delivery—in particular, reductions in the number of pending cases in state courts—can change people's beliefs and behavior. We ask whether this information makes our respondents more willing to use, interact with, and trust state courts. Second, and more pertinent to the issue of potential feedback between state and nonstate institutions, we investigate whether trust in state and nonstate actors is tightly linked such that positive information about state courts makes our respondents less willing to interact with competing nonstate actors and less likely to have positive views about them—even though this information has no direct relevance to the nonstate actor's effectiveness and trustworthiness.

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[1] A growing literature in political science, sociology, and economics emphasizes the central role of state capacity for economic development (see, inter alia, Johnson 1982; Wade 1990; Amsden 1992), while weak and ineffective ("low-capacity") states are often argued to cause poverty, instability, and even civil war (e.g., Migdal 1988; Centeno 2002; Besley and Persson 2009; Acemoglu, Ticchi, and Vindigni 2011; Herbst 2014).

Pakistan is an ideal setting for such an investigation because of the well-recognized weakness of state institutions and the associated low levels of access to and trust in the state (Jackson et al. 2014; Cheema, Hameed, and Shapiro 2017), as well as the critical role that various nonstate actors have come to play in parts of the country, especially in dispute resolution (see Chaudhary 1999; Siddique 2013, 2015; Gayer 2014; Shinwari 2015). Ineffectiveness of state courts in Pakistan is one of the key dimensions of state weakness and has spawned widespread discontent.[2] We focus on the role of a major nonstate actor involved in the process of dispute resolution: the panchayat. Panchayats, comprised of groups of village elders and other influential locals, are the primary alternative to state courts in rural Pakistan. Notably, panchayats are outside of the formal judicial system, base their rulings on cultural norms, and do not typically follow laws promulgated by the Pakistani state.[3]

We use two approaches to measure behavior and attitudes toward state and nonstate institutions. First, we collect survey information on expected usage and assessment of state courts and nonstate dispute-resolution forums such as panchayats. Second, we design lab games meant to address concerns arising from using self-reported data and elucidate different aspects of behavior toward these forums. Our respondents can earn as much as PKR 550 (approximately USD 5.30 during the first round of our study) in these games, equivalent to one and a half times the average daily earnings in our sample. The first game, which we call the *fund dictator game*, is a version of the well-known dictator game in experimental economics. It gives our respondents a choice between allocating a pot of money between themselves and a fund that helps others access state courts. They then make a similar decision for a fund that improves access to panchayats using a separate pot of money. Thus, the fund dictator game measures how much our respondents are willing to contribute to others' access to these two forums. Our second game, referred to as the

[2] This ineffectiveness has also generated support for nonstate actors, including the Taliban. For example, in 2009, Taliban militants took control of parts of Pakistan's frontier province of Khyber Pakhtunkhwa (KP) and instituted parallel justice and administrative systems based on sharia and funded through taxes they imposed on the population (Rana 2009; Rehman, Haider, and Zahid 2014). After the army retook control of the province, the president of Pakistan established sharia courts as part of the settlement to end the conflict. This was an acknowledgment of the discontent surrounding the state courts that had fueled the Taliban's rise (Walsh 2009; Siddique 2013).

[3] In contrast to panchayats in India, which are part of the local government structure, the panchayat system in Pakistan is entirely outside of the control of the state and often competes with it (Chaudhary 1999; Shinwari 2015; Siddique 2015). Like India, Pakistan's local governments have introduced provisions to enact local mediation bodies called Musalihat-i-Anjuman. However, these bodies have not been institutionalized because local governments have been periodically disbanded (Cheema, Khan, and Myerson 2010; Shinwari 2015).

investment game, measures our respondents' beliefs about the effectiveness of the two forums. More specifically, this game asks our respondents to invest money in a complainant's case being addressed by either state courts or panchayats, with the understanding that this investment will be repaid depending on the effectiveness of the relevant actor. The game is framed to evoke coinvestments that are familiar to rural households.

Our within-subject experimental design relies on first capturing baseline behavior and beliefs and subsequently providing respondents with information about reduced delays in state courts. We then measure how this information changes their game behavior and responses. At baseline, people report relatively low expected usage of state courts. Once people receive this "state positive" (informational) treatment, we see a notable improvement in expected usage of these courts, indicating that the information we provided is indeed believed. We also estimate fairly large direct effects on their allocations to the state in both the fund dictator and the investment games—approximately a 15% increase over the baseline. These direct impacts are not driven by social experimenter effects whereby our respondents change their behavior because they think that this is what we would like them to do. We verify this through two checks. First, we provide a randomly selected sample with a statement that does not contain any information about improved performance of state courts but is a clearly stated opinion favorable to the state. There are no significant changes in the allocations in the two games following this "social experimenter treatment," and there is a much smaller effect on self-reported expected usage. We then explicitly net out any such social experimenter effects and still find large and robust impacts of the state positive treatment. Second, we run a fully anonymized version of the fund dictator game where our respondents understand that their individual allocations cannot be seen by us or our surveyors. We show that our results are the same in this game, thereby indicating that individual respondents' behavior is not affected by whether it is observed by the surveyors or the research team. Finally, we further confirm the robustness of our results to a series of checks concerning specification, data, and respondent comprehension. Overall, our results suggest that there is nothing hardwired about the lack of trust in the state, as credible new information can trigger changes in beliefs and behavior.

Our second set of results is more striking and novel. Consistent with the notion that attitudes toward state and nonstate actors are tightly linked, we find large and robust indirect effects. Following the positive information about state courts (which provides no information on any nonstate actor), expected usage of panchayats declines and our respondents choose significantly fewer (by about 10%) allocations toward panchayats in both the fund dictator and the investment games. As before, these results hold after netting out any social experimenter effects, are present in the

anonymized version of the game, and are robust to a range of additional data and specification checks.

Our preferred interpretation of these findings is based on "motivated reasoning," whereby individuals choose their behavior (such as usage decisions and allocations) simultaneously with their beliefs.[4] Motivated reasoning enables us to capture the notion that individuals internalize the norms and values of the institutions they are working with. In our conceptual framework, belief choice affects the perceived expected utility of the agent, but deviations from the full Bayesian benchmark are costly because they distort behavior. This results in a complementarity between behavior and beliefs: when an individual interacts (or expects to interact) more with one type of actor, he has an incentive to distort his beliefs to be more favorable to this type of actor. In this case, information about improved performance of the state induces agents to use and contribute more to state courts instead of panchayats, and as a result agents change their beliefs in favor of state courts (because these are now used more intensively) and against panchayats (because they are used less intensively). Motivated reasoning also encapsulates the feedback cycle mentioned above: the more the state is used, the more negative views about nonstate actors become, and this further encourages the use of state institutions.

The most distinctive implication of motivated reasoning models is that beliefs about the effectiveness of and general trust in nonstate actors should deteriorate after positive information about state courts, even though these have no direct relevance to the performance and effectiveness of nonstate actors. To investigate this implication, we examine three specific dimensions of beliefs about the effectiveness of state and nonstate forums—*service effectiveness*, which concerns the quality of the service; *enforcement effectiveness*, which measures how well the judgment in the relevant forum will be enforced; and *access*, which captures ease and costs of accessing the forum. In addition, we measure our respondents' (general) trust in the two forums. As a first-stage check, we first verify that the state positive treatment improves our respondents' (self-reported) beliefs concerning service and enforcement effectiveness, access, and trust

[4] Motivated reasoning refers to the possibility that individuals manipulate their own beliefs either for direct benefit or for strategic purposes. The theory of motivated reasoning in psychology goes back at least to Festinger's (1962) theory of cognitive dissonance and Bem (1967). See also Kunda (1990), Edwards and Smith (1996), Jost et al. (2003), Kahan (2013), Gilovich and Ross (2015), and Epley and Gilovich (2016) for more recent discussions, and see Trivers (2011) and von Hippel and Trivers (2011) for an approach emphasizing the benefits of motivated reasoning from an evolutionary viewpoint. One of the first applications of motivated reasoning in economics is Akerlof and Dickens's (1982) use of ideas from cognitive dissonance in occupation choice. For more recent contributions in economics, see Loewenstein (1987), Rabin (1994), Caplin and Leahy (2001), Carrillo and Mariotti (2001), Bénabou and Tirole (2002, 2004, 2016), Van den Steen (2004), Brunnermeier and Parker (2005), and Ortoleva and Snowberg (2015).

for state courts. We then turn to indirect effects and document that beliefs regarding effectiveness and trust concerning the nonstate forum deteriorate significantly following the positive information about state courts. These results illustrate a powerful shift in our respondents' views against panchayats once they expect to interact less with this actor. They thus provide evidence consistent with the feedback mechanisms mentioned above: positive views about nonstate actors are fed by negative beliefs regarding the effectiveness of state institutions, and vice versa.

We present additional evidence bolstering the interpretation that the results are mediated by belief updating. We first show that the impact of the state positive treatment is greater if respondents find the information we provide to be more credible. We then go a step further by exogenously varying the source (and hence the credibility) of the information we provide. We randomized respondents between sealed envelopes containing information from one of two sources: private news channels or Pakistan Television (PTV). In our surveys, the former is reported as less credible relative to the latter. Our results show significantly greater responses to the more credible source of information. These findings thus lend additional support to our interpretation that the direct and indirect effects we are documenting are working through an informational channel.

An alternative interpretation of the indirect impact on panchayats is that they reflect mechanical "contrast effects" (Pepitone and DiNubile 1976; Kamenica 2008; Bhargava and Fisman 2014), whereby perceived improvements about state courts automatically lead to a deterioration in beliefs about the only other option, panchayats. We present two types of evidence against such contrast effects. First, in our initial games we include additional survey questions about a third actor—state hospitals—and do not detect any self-reported negative effects on this third actor. Second, and more importantly, we test for contrast effects directly by introducing an additional set of high-stakes games where we substitute sports clubs for panchayats in the comparison. We again do not find any negative indirect effects on the second actor.[5] These findings support our interpretation that the negative indirect effects we estimate reflect a genuine deterioration of beliefs about panchayats.

Another alternative explanation for our motivated reasoning interpretation is that our respondents are fully Bayesian (without any motivated reasoning considerations), but their priors about the effectiveness of the state and the nonstate actors are negatively affiliated (correlated). In this case, any news about the state being more effective becomes relevant for them to update their beliefs about nonstate actors. We discuss this issue

[5] If anything, for both hospitals and sports clubs there are some small positive effects, which appear to be driven by our respondents' belief that when state institutions function better, both other state institutions and even nonstate actors such as local sports clubs (that may nevertheless depend on state institutions) become more effective.

further below. Here we note that for our purposes such negatively affiliated priors have implications very similar to motivated reasoning but make additional predictions, which we test in our analysis of heterogeneous effects and do not find uniform support for.

Though our main results focus on an informational treatment that provides positive information about state courts, in our pilots we also tried the three remaining combinations and provided (truthful, media-reported) information about less successful dimensions of the state court's performance as well as negative and positive information about panchayats. Despite the smaller sample sizes in these cases, we once again find similar direct and indirect effects. This suggests that the feedback between perceptions of state and nonstate forums holds more generally than only for the state positive informational treatment that we primarily focus on.

Our paper contributes to a number of literatures. While there is an extensive literature on the implications of low state capacity in the development process and a similarly large literature on the origins of state capacity, there is little work about how state and nonstate institutions interact and compete. The role of trust and political culture in the functioning of state institutions goes back to the classic works by Banfield (1958), Almond and Verba (1963), and Coleman (1990) and that have been elaborated by Putnam (1993) in the context of the contrasting institutional trajectories of the north and the south of Italy. The importance of cooperation of citizens, which itself depends on their trust in institutions, has been emphasized by Peter Evans's notion of "embedded autonomy" (Evans 2012) and in a few works in political science (e.g., Mishler and Rose 2001; Rothstein and Stolle 2008; Zmerli and Newton 2008). It has also been modeled in the context of "consensually strong states," defined as states that derive authority from citizens who have the capability to rein them back (Acemoglu 2005; see also Acemoglu and Robinson 2019). Recent work by Dell, Lane, and Querubin (2015) argues that the greater capacity of the north Vietnamese state (relative to areas in the south that were under the historical influence of the Khmer Empire) is related to the cooperation of villagers. As mentioned above, the rise of extremist religious organizations such as Hamas, the Muslim Brotherhood, and various Salafist groups has been linked to the weakness of the state in the qualitative literature on Middle Eastern politics (e.g., Clark 2004; Kepel 2009; Roy 2013). However, to the best of our knowledge, this linkage has not been systematically investigated.

Our approach is related to and builds on several different strands in the experimental economics literature as well. There is a growing line of work using experimental methods to measure trust, beliefs, and norms in different settings (see, e.g., Berg, Dickhaut, and McCabe 1995; Dufwenberg and Gneezy 2000; Burks, Carpenter, and Verhoogen 2003; Bohnet and Zeckhauser 2004; Camerer and Fehr 2004; Ashraf, Bohnet,

and Piankov 2006; Johnson and Mislin 2011). Most of this work does not focus on attitudes toward state institutions, with the notable exception being Cox et al. (2009). Another branch of the literature related to our work investigates various dimensions of extremism. For example, Bullock, Imai, and Shapiro (2011) and Blair et al. (2013) look at support for militant groups in Pakistan, while Delavande and Zafar (2012) and Bursztyn et al. (2016) focus on anti-American attitudes. There is also a literature using lab-in-the-field games in development economics (see the survey in Cardenas and Carpenter 2008) and a number of papers investigating the effects of providing information to voters or citizens (e.g., Andrabi, Das, and Khwaja 2017; Grossman and Michelitch 2018).

Finally, some works in the sociology and social psychology literatures are related to our paper as well. For instance, Sullivan and Transue (1999), Anderson (2010), and Schoon and Cheng (2011) emphasize the role of individual experiences in shaping political trust, while a number of other works develop similar ideas in the context of organizations (e.g., Kramer 1999).

The remainder of this paper is organized as follows. Section II describes the context of dispute resolution in Pakistan. Section III provides the details of our experimental design and empirical strategy. Section IV presents our main empirical results. Section V discusses potential mechanisms that may account for our results on direct and indirect effects and then presents additional evidence relevant for these mechanisms. Section VI concludes. Appendix A presents a formal model elucidating various mechanisms via which direct and indirect effects may be working, while appendixes B and C (available online) provide additional robustness checks and details on study design.

II. Background and Context

In this section, we provide a brief overview of dispute resolution in Pakistan and citizens' engagement with state courts and panchayats.

A. Dispute Resolution in Pakistan

Dispute resolution is one of the most important services demanded by Pakistani citizens and one of the Pakistani state's core responsibilities. Disputes are a particularly common occurrence in our setting, Pakistani Punjab, as manifested by high litigation rates—about three times as high as the rates of litigation in Indian parts of colonial Punjab.[6] In our survey,

[6] The partition of British India split the former province of colonial Punjab into the Punjab province in Pakistan and the states of Punjab, Haryana, and Himachal Pradesh in India. The officially reported litigation rates in these Indian states ranged between

one in every five households reports that they have accessed the legal system in the last 3 months.[7] Becoming embroiled in a dispute imposes significant costs. Estimates suggest that cases take many years to resolve and involve sizable financial costs (Chemin 2009; Siddique 2016). Illustrating the centrality of dispute resolution to ordinary Pakistanis, the rallying slogan used by the Taliban insurgency in KP province, mentioned in the introduction, was the provision of cheap and swift justice (Kapoor 2000).

Dispute resolution therefore offers an ideal setting for our study. We further narrow our focus to rural areas, where there are clearly defined state and nonstate actors providing competing dispute-resolution services.

On the state's side, the judicial system operated by the Pakistani state consists of state courts backed by the police.[8] It is an adversarial and retributive judicial system that is divided into courts of first instance (both civil and criminal) and appellate courts, which have the power to review the decisions of the lower courts. The legal system works primarily through three key actors—police, judges, and lawyers. The police are responsible for the maintenance of law and order and for the administration of criminal justice, making them the typical first point of contact for citizens in criminal matters. Judges adjudicate on the basis of codified procedures and consistent application of state law. Lawyers are meant to assist state courts in reaching just decisions.

Nonstate actors have historically run parallel dispute-resolution forums in rural areas of Pakistan that are distinct from the state judicial system. These nonstate forums are typically ad hoc local councils of village elders (panchayats) and are usually given the authority to resolve disputes on behalf of residents of the community (Chaudhary 1999; Ayaz and Fleschenberg 2009; Soomro and Chandio 2013).[9] They ignore the formal

5.3 and 9.2 per 1,000 persons between 2005 and 2010 (Eisenberg, Kalantry, and Robinson 2013) compared with 17 per 1,000 in Pakistani Punjab during the same period (authors' estimates based on the Lahore High Court Annual Reports).

[7] Recent studies show that a majority of cases that end up in state courts involve disputes around land, property, inheritance, and contract. Siddique (2013), e.g., finds that approximately 57.5% of court cases in Lahore involved land, property, and inheritance disputes; 18% involved marital or guardianship cases; and around 8% were contract disputes.

[8] In colonial India, officers of the executive (as opposed to the judicial) branch were invested with specific judicial powers under the criminal procedure and penal codes. This system has continued in postindependence India but was abolished in Pakistan as a result of the Devolution Reforms of 2001 to achieve separation of powers between the judiciary branch and the executive branch (article 175(3) of the Constitution of Pakistan). A consequence has been a significant expansion of the remit of the state courts in Pakistan.

[9] In other areas of Pakistan, panchayats are also called *kath*, *paryah*, *faislo*, or *jirga* (Chaudhary 1999; Shinwari 2015). The panchayat system is not new to the Indian subcontinent, and it remains fairly prevalent in both India and Pakistan. There are references to it in the Sanskrit epic of the eighth and ninth centuries BCE, the *Mahabharata*, and it also appears to have continued through the period of Muslim rule. This is in contrast to the state judicial system, which is a product of British colonial rule (Siddique 2015). Hoebel (1965), quoted in Chaudhary (1999, 23), observed more than five decades ago that "the legal system of Pakistan does not constitute a neatly integrated whole; it is made up of

law and compete with state institutions. Eighty percent of villages in our sample report the presence of such a system in their community, dealing with a wide array of cases including theft, robbery, family feuds, small social complaints, and land disputes. While the state judicial system is relatively punitive, panchayat decisions tend to be restorative. They use a combination of mediation, compromise, and penalties, including social ostracism, boycott, and sometimes even physical retaliation. Enforcement of panchayat decisions is typically underpinned by the threat of sanctions by the community or its powerful members (Chaudhary 1999; Shinwari 2015; Siddique 2015).

Since independence in 1947, the Pakistani state has been highly suspicious of such parallel nonstate forums, viewing them as antithetical to its legal system. This is in sharp contrast to India, which has tried to incorporate panchayats into the formal state apparatus. In fact, the report of the Pakistan government's Law Reform Commission of 1967–70 argued that "it will be a retrograde step to revert to the primitive method of administration of justice by taking our disputes to a group of ordinary laymen ignorant of modern complexities of life and not conversant with legal concepts and procedures" (Chaudhary 1999, 3). In 2004, the Sindh High Court banned trials under the nonstate system and declared these forums illegal (Cowasjee 2004; Brohi 2016). Similarly, the Supreme Court of Pakistan has made a series of rulings during the past decade decreeing many panchayat decisions to be unconstitutional (Brohi 2016). It has specifically targeted panchayat decisions that sanction direct vengeance for murders and forced marriages of young girls as punishment for crimes committed by their male relatives (Shinwari 2015; Siddique 2015).

B. Access to and Views toward State Courts and Panchayats

Our primary informational treatment is to provide positive information about state courts and study how this impacts behavior and views toward both state courts and panchayats. It is therefore instructive to understand what the baseline situation is in terms of access to and effectiveness of these forums and the prevailing information about them.

Access and costs.—Respondents in our baseline surveys report relatively low access to the state—on a scale of zero to 10, they report their likely usage of state courts as four while panchayats have a reported usage of 6.5. This is driven in part by the relatively higher costs faced in accessing state courts. Since resolving disputes through state courts is a lengthy process, an individual needs to consider the loss of daily wages, the cost of

an undetermined multiplicity of subsystems. Deeply embedded in the village and tribal areas of Pakistan is a vast array of local folk systems of law varying from village to village."

transportation, and the legal fees necessary to enter and remain in the system.[10] In contrast, panchayats offer quick resolution by gathering the disputing parties directly in the village. Their proximity allows each party to bring their supporters to the meetings with limited expenses. Moreover, unlike panchayats, which are located within most villages, state courts are fewer in number and are usually situated in the main city of the district. Our baseline surveys confirm this cost differential: respondents rated court costs as 7.5 and panchayat costs as 3.3 (on a scale from zero to 10, where 10 means extremely expensive). Similarly, respondents report that a theft case that costs PKR 1,000 to settle in a panchayat would cost about PKR 23,000 to settle in a court. Difficulties in accessing state courts are compounded by a lack of knowledge about how to navigate these institutions.

Quality.—In addition to access issues, state courts are generally rated as unreliable and unfair. Popular media is full of accounts of the miscarriage of justice (see Shinwari 2015; Nekokara 2016; Javed 2017). Siddique (2013) finds that 47% of the respondents in Lahore felt that the laws are either biased against them or unjust, and three-quarters of respondents in a survey of litigants were dissatisfied or deeply dissatisfied with the pace at which their case was proceeding; about the same proportion could not predict when a verdict would arrive. Respondents report, for example, "For 20 years have I been waiting for justice. Judges and lawyers ensure that case does not come to a conclusion" and "My family has withered away while pursuing this matter," and they bemoan, "This legal system is a complete failure." Nearly 90% of the respondents who had accessed the police or the judicial system in the 3 months preceding our survey thought that the police cannot be trusted, and another 65.7% viewed the courts as not trustworthy. In contrast, rural Pakistanis better understand how panchayats work (Shinwari 2015), and our respondents rate panchayats to be not just more accessible but also more effective in delivering services (their service effectiveness score is 5.4 compared with 3.9 for state courts). This is despite the panchayats' lack of legal training, their systematic deviations from prevailing laws, and their failure to incorporate disenfranchised members of society, such as women and low-income groups.

Both access difficulties and perceptions of low effectiveness of state courts are rooted in endemic delays. Over 80% of respondents in

[10] From interviews with lawyers at the sessions courts, we found that different types of cases vary in length and cost. For example, an inheritance case could last more than 2 years on average, with anywhere between PKR 5,000 and 200,000 in costs. The resolution of business-related cases may be faster, though even those can take upwards of 6 months with potentially significant costs for the parties (Siddique 2013). Shinwari (2015) finds that low-income households and women in particular face high costs of accessing formal justice institutions, in part because of the process being time consuming, the high lawyer fees, and the long distances to courts.

Siddique's (2013) sample felt that there were significant delays when going through state courts, and 27% of litigants had their case stuck in the court system for more than 5 years. The majority did not know when a verdict was expected. In contrast, panchayats typically offer faster decisions. Recall that a major factor in the rise of the Taliban was their promise of speedy dispute resolution; decisions would be made (almost) on the spot in sharia courts. This desire for faster decisions is unsurprising, as Chemin (2009) and Siddique (2016) report that cases in state courts take on average 2 or 3 years. Shinwari (2015) finds that lack of speedy justice is one of the biggest complaints made against the state courts by over three-quarters of the respondents in his nationally representative survey. Consistent with this, Chemin (2009) estimated that more than 1.2 million cases were pending in 2001, and recent estimates suggest that this number may have climbed to 2 million (Siddiqi 2016).

Informational context and recent changes.—Villagers operate in an environment of incomplete and unreliable information. They may have heard of judicial reforms but are often unaware of specific changes that could affect them directly, such as reductions in the number of pending cases in their area. Slow knowledge diffusion about state courts implies that rural Pakistanis are probably not well informed about recent changes. Indeed, in our surveys 98% of respondents acknowledged that the specific piece of information we provided them regarding delay reduction was not something they had heard before. Therefore, credible information provision concerning recent developments regarding improved access to and effectiveness of state courts is likely to have an impact on behavior and perceptions, as we see later in the paper.

Since our study sample includes two distinct rounds with an almost 2-year gap between rounds (more on this below), we conclude this section by noting some relevant changes in state courts over our study period. This period has seen the emergence of an activist Supreme Court initiating a series of high-profile cases related to administrative and political corruption, bureaucratic sinecure, public service delivery failures, and misconduct by private businesses. The period of judicial activism began with the appointment of Chief Justice Saqib Nisar on December 31, 2016. His tenure (2016–19) is marked by the Supreme Court's frequent use of its *suo motu* powers (the court taking action on its own account) in high-profile cases.[11] These cases were extensively covered by electronic

[11] In a significant deviation from previous norms, the chief justice initiated approximately three high-profile *suo motu* cases every month (Haroon 2018). These included cases against high-profile bankers for money laundering, powerful private developers for encroachment on state land, public sector hospitals and water authorities for poor performance (*Express Tribune* 2019), and the police for slow action in rape and murder cases. The Supreme Court ordered private schools to reduce their fees by 20% and imposed taxes on private bottled water companies for overexploitation of groundwater (Samaa 2019). The chief justice also took *suo motu* action against high-salary appointments in public sector

and print media and resulted in the Supreme Court's activism gaining tremendous salience and fairly broad support from the public.[12]

III. Experimental Design

Understanding the determinants of individual decisions to access state or nonstate actors for dispute resolution is challenging given the myriad factors impacting such behavior. To overcome these challenges, we utilize a within-subject experimental study design and examine how the behavior of individuals changes in response to information they receive. We expose individuals to information on state effectiveness and then see how their views and behavior change toward state and nonstate actors. To help address concerns that survey responses may not reflect real-world behavior, we focus on relatively high-stakes experimental games designed to reveal respondents' behavior toward both the state and the nonstate actors. In this section, we detail the informational treatments, experimental protocols, and data and sampling methods.

A. Informational Treatments

We are interested in whether perceptions of state effectiveness can change attitudes and behavior toward the state and the nonstate actors. Given the generally poor views of state courts and the likelihood that the average citizen may not be fully informed, our primary informational treatment provides true and favorable evidence on the performance of the formal judicial system. We refer to this as the state positive treatment. On the basis of our field discussions where a variety of information primes were discussed, and since many of our respondents felt that they would not get effective justice because of the pervasive delays in the court system, we chose to focus on reduced delays. This is also desirable because, as we noted in section II, regardless of the legitimacy or beliefs concerning judicial biases, a reduction in delays is likely to be widely attractive. This

companies (*Dawn* 2018), instituted a fund to help raise money for the construction of new dams in Pakistan (Ijaz 2019), and most notably disqualified the prime minister at the time, Nawaz Sharif, from holding public office on grounds of dishonesty in not disclosing his complete assets in his nomination papers (*Dawn* 2017).

[12] *The Herald*, Pakistan's leading monthly, analyzed public support for an activist Supreme Court through a nationally representative public attitudes survey conducted in June 2018. The survey asked respondents whether they agreed with the statements that the Supreme Court should directly exercise executive authority and in particular (1) set the prices of essential commodities and (2) have the power to dismiss government officials for poor performance. The survey data reveal significant public support for an activist Supreme Court, with approximately 72% of the respondents agreeing with both statements.

treatment provides respondents with the following truthful information about a district (Multan) in a nearby region to our study sample district (Sargodha): "The legal system and judges have formed a new judicial policy. This policy was introduced in Multan and has resolved 6,000 pending cases in 2 months. For this reason, Multan's number of pending cases has decreased by 20%. This policy has now been implemented in Sargodha, and it is estimated that most pending cases could potentially be resolved within a year."

Our initial design included other variations—in particular, state negative, nonstate positive, and nonstate negative treatments (see app. C). These treatments were also based on truthful media reports. Our pilots revealed that the impact of these different treatments was fairly symmetric (see sec. IV.E), so for the sake of statistical power we decided to scale up only the state positive version. We should note that the state negative treatment primed on decision-making delays as well, while the nonstate positive (negative) primes additionally included information about the (in)effectiveness of the decisions made by the panchayats. This suggests that the informational impacts we observed are not only about changes in delays, but respondents react analogously when informed about more or less effective dispute resolution.

One potential drawback of designs based on informational treatments is that respondents may change their views and behaviors after the informational treatment for other reasons. For instance, they may feel obligated to do so given what the experimenter has just said, despite no real change in their view. A direct way to deal with this social experimenter effect is by using a treatment that directly primes it. To do so, we provided the following social experimenter treatment to a randomly selected group of respondents (again, after the baseline surveys and games): "So I've been thinking about the current state of affairs and how the state's been dealing with everything, and while I don't really know how great a job state institutions are doing, in my personal opinion, I really like the state system." We then repeated the surveys and games after this treatment. Using this sample, we can net out any potential social experimenter effects. The social experimenter treatment further enables us to use a pure cross-subject design as an alternative strategy, as described below.[13]

[13] While both the state positive and the social experimenter treatments are randomly assigned across individuals, the fraction assigned to either treatment varies across the sampling strata, because after the first few surveys we recalibrated the sample sizes of these two treatments. To avoid any concerns related to "*p*-hacking," sample sizes were adjusted using information only on standard errors of the outcomes of interest and not on estimated effect sizes, *p*-values, or *t*-statistics. Our within-subject design is unaffected by this recalibration, and in any specification that involves cross-subject comparisons we include strata fixed effects interacted with a posttreatment dummy to capture any differential responses that may arise due to baseline differences across strata.

B. Experimental Procedures and Games

Our primary protocol is to approach a randomly selected household and seek their consent to have a discussion around issues regarding dispute resolution. We introduce ourselves as researchers interested in understanding the different forums for dispute resolution in their community, solicit their views, and play experimental games. Participants are informed that they will receive a token PKR 50 payment for agreeing to participate, and they also have the potential of earning significant payoffs from the games (see app. C).

After receiving consent, respondents are asked a series of questions regarding dispute resolution and their views on the effectiveness of both state courts and panchayats. The survey instrument was designed to understand the actions of respondents with regard to effectiveness of state and nonstate actors. The survey includes a question on the expected usage of the state and nonstate forums, which we use throughout the paper. In later stages of the study, we also included questions on individual beliefs regarding the effectiveness of the relevant forum as well as general trust in the forum.[14] We additionally gathered information about their expectations of others' usage (all of these variables are on a scale from zero to 10).[15] By comparing responses to these questions before and after treatment, we can measure the change in a subject's own expected usage and their perceptions of others' usage of state and nonstate forums resulting from our state positive and social experimenter treatments.

In addition to the baseline surveys, respondents play two different games—the fund dictator game and the investment game—before and after the state positive and social experimenter treatments. The two games are designed to capture different aspects of citizens' views of the state and the nonstate actors. The fund dictator game is meant to measure changes in beliefs and behavior concerning how beneficial the state forum is to the general population. It is set up along the lines of a standard dictator game, where we seek to understand respondents' proclivity to assist those in their communities in accessing the state or the nonstate dispute-resolution forums. We do so by asking individuals to contribute to two potential

[14] There was initially a concern that including a detailed set of questions regarding effectiveness of state and nonstate actors at baseline could generate its own priming effect and confound our interpretation of state positive and social experimenter treatments. We included this richer set of questions in subsequent samples to shed further light on the mechanisms at play. Moreover, given our budget and power calculations, we could provide only the state positive treatment—and not the social experimenter treatment—to the sample where we asked these additional belief questions.

[15] Our expected usage question is, "What is the likelihood of you going to the state or the panchayat, zero meaning not at all, and 10 meaning completely?" Regarding perceptions of other villagers' engagement, we asked, "What is the likelihood of others in your area going to the state or the panchayat, zero meaning not at all, and 10 meaning completely?"

funds, one (state fund) to assist those individuals in the community who prefer to go through the state system to resolve disputes and the other (nonstate fund) for those who would rather go to the nonstate alternative. Respondents can confidentially make a choice to give all, some, or none of a specified amount to the fund in question on the basis of their beliefs about the relevant actor's benefits to citizens and their own level of trust in the chosen actor. The surveyor also explains that the research organization is considering setting up such funds so the money that the respondent allocates to the funds will actually be donated.[16] We therefore expect the respondent to allocate more money to the fund he believes will be more useful and/or to the one toward which he feels more altruistic. To avoid any mechanical spillover effects, participants receive two separate endowments of PKR 250, which they can allocate to the fund in consideration or keep for themselves. Thus, in the baseline play, for example, an individual may decide to donate PKR 100 to the state fund and keep PKR 150 for himself out of the first endowment while donating PKR 150 to the nonstate fund and keeping PKR 100 for himself out of the second endowment.

The investment game aims to measure changes in beliefs and behavior concerning how effective the two forums are in resolving disputes. Respondents are told to consider two hypothetical members of their community, each of whom is experiencing a dispute, but one member has chosen to take his case through state institutions and the other has taken their case to the panchayat. Both members are rightfully owed remunerations from a defendant, and the respondent is given a chance to invest an amount X out of PKR 250 in the plaintiff's case in return for a share of the remuneration. They are told that the share they receive will be ηX, and thus their total take-home amount will be $(250 - X) + \eta X$, where $\eta \in [0, 2]$ measures the effectiveness of the forum. Hence, if a forum is completely ineffective, then $\eta = 0$ and the respondent will receive zero returns on his investment. If a forum is fully effective, then $\eta = 2$ and the respondent will double his investment. Participants are informed that η has been calibrated for each forum to reflect reality.[17] Consequently, the

[16] From all the games played so far, we have a total of PKR 203,480 and 226,830 contributed by our respondents in state and nonstate funds, respectively. We are currently working with two organizations to which we will allocate these funds. The state fund is being allocated to the Punjab Police's 8787 Police Complaint Hotline, which provides citizens with the ability to lodge a complaint or a grievance against police or members of the judicial system who are not fulfilling their obligations. The nonstate fund is being donated to the Legal Aid Society, a nongovernmental organization that provides advice to respondents free of charge.

[17] After piloting different options, we chose to keep the return on investment from both the state and the nonstate forums at $\eta = 1$. This implies that each respondent receives a payment of $(250 - X) + \eta X = 250$. While one could have varied the return, our pilots revealed significant variation in success rates between state courts and panchayats in general

more effective the respondent believes a dispute-resolution forum to be, the more he will invest in that forum. While the specifics of the game may have been somewhat unusual, the context is not, since villagers are familiar with investing in each others' projects or lending money to help each other out, with the return/likelihood of repayment being contingent on the success of the endeavor.

We arrived at the game designs described above through multiple iterations of pilots with alternative framings. The goal was to ensure that the games were well understood and tailored to the context so that they would appear familiar to our respondents, especially since they were unlikely to have ever experienced such lab-in-the-field games before.

Once the basic design of the games had been finalized, we ran additional pilots to ensure that the language and format details were easily comprehended by our respondents. Our original survey was created in Urdu (Pakistan's national language), but initial piloting revealed that the nuances of the games were best understood in Punjabi (the local vernacular), prompting us to present the information in Punjabi. We varied the sequence in which respondents played the games to see whether their understanding differed depending on which game was played first, but we found no such effects.[18] We nevertheless decided to keep the order randomization to account for any potential level or treatment effects induced by order. The game payoff amounts were also piloted to arrive at an amount that was large enough to create credible stakes without being too costly. Finally, we ensured that the wording was such that respondents' game allocations followed their own beliefs rather than other potentially salient factors, such as what they may have thought the surveyor/research team wanted. For some (randomly selected) respondents, we directly asked about their thought process in making their allocations and found little evidence of any such concerns (see n. 17).

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and across regions, so we could not obtain reliable/region-specific estimates of η. Given this, it would have been misleading to encode through our choice of η that one forum was more effective than the other. Setting $\eta = 1$ in all villages was therefore a natural benchmark. This choice has the added advantage that all respondents' take-home pay, $(250-X) + \eta X$, is independent of the amount they invested. This minimizes the concerns around the negative externalities of deception for future research (Gunnthorsdottir, Houser, and McCabe 2007). Indeed, our respondents never gave any indication of loss of experimental control arising from a perception of deception (Jamison, Karlan, and Schechter 2008) or expressed any opinion suggesting reduced trust in future research (Friedman and Sunder 1994). The same considerations and constraints made us set $\eta = 1$ in the context of the investment game with local sports clubs (discussed below). A related concern is whether our respondents are trying to guess our views of η rather than using their own beliefs and information. We asked them at the end of the second round of surveys about their decisions and found no evidence supporting this concern: 96% reported prioritizing their own priors or the information we provided over guessing our views of what the return is.

[18] For example, the average (self-reported) understanding of the respondents in the fund dictator game is 7.9 (on a scale from zero to 10) when playing the investment game first and is 8.0 when playing the fund dictator game first.

We further took several steps to ensure high-quality responses in the games. Our first strategy was to give each respondent PKR 50 at the start as a participation fee, building credibility with respect to our intention to pay out their winnings in cash. Respondents then played both the fund dictator and the investment games three times. First, they played a practice game of each, followed by a discussion to ensure that they had understood the game procedures. A customized board with subdivisions for the pre- and posttreatment allocations for each experiment was used as a visual tool during the explanations. The benefit of such a tool is threefold: (i) it provides a visual aid for respondents; (ii) it creates a sense of privacy, as each section has a cover that hides the allocations of the respondent from the surveyor; and (iii) it provides demarcation between the two pretreatment and the two posttreatment games for each experiment. Each section is further subdivided to depict the respondent's allocations for state/self and nonstate/self. The board is shown in figure B1 (available online). After the surveyor was sure that the respondent had understood the game, pretreatment games were played. Next, the respondents were provided with one of our (state positive or social experimenter) treatments. Finally, after confidentially reviewing their pretreatment allocations, they repeated the same games. Participants were paid for only one of the four allocation decisions (pretreatment state and nonstate and posttreatment state and nonstate allocations) for each type of game. For the fund dictator game, the payoff was simply one of the four amounts that the participant selected to keep for himself. In the investment game, the respondent received both the money allocated to himself and a return on their investment in the chosen game—that is, $(250 - X) + \eta X$ (=250 PKR).

In addition to the basic games, in the second round of our study we used three additional designs. The first was an anonymized version of our fund dictator game. This design helps further address any social experimenter concerns arising from lack of anonymity (e.g., respondents altering their allocations because they know these will be observed by the surveyors or researchers). The second variant replaced the nonstate actor—the panchayat—with a more neutral actor—local sports clubs—to test against any mechanical contrast effects. Finally, the third variant randomly varied the source of the information to test whether respondents update differentially on the basis of how credible they think a source is. We describe these games in more detail when we present the results.

C. Sample and Data

Our study sample consists of rural male household heads (or their close relatives) between the ages of 20 and 64 and spans four districts in Punjab, Pakistan's most populous state. The results from our pilots indicated that respondents from this gender and age bracket are most likely to be decision

makers within a household and are best suited to understand and relate to the contextual framework of the survey and the behavioral experiments. We conducted close to 100 pilots with women but found that they did not have as much direct experience with the state or the nonstate forums as men had, making them less suitable as respondents for our purposes. Moreover, no obvious patterns of heterogeneity emerged, and this, combined with statistical sampling/power and logistical constraints, led us to limit our sample to males. After pilots within urban, peri-urban, and rural settings, we decided to conduct the study in rural areas only, because engagement with nonstate actors was more easily identifiable within this setting. While urban areas also have nonstate actors, the specific actor varies considerably from place to place. In contrast, in rural areas, the panchayat was invariably identified as the primary nonstate actor. Moreover, according to the 2017 population census, a majority (63.6%) of Pakistan's population still resides in rural areas.

We randomly drew rural households in one district (Sargodha) of Punjab. The district is fairly representative of Punjab and enables us to use a preexisting sampling frame (the 2007 Sargodha Village and Household Survey [SVHS]) from a primary survey designed to be representative at the district level and conducted by Cheema and Naseer (2013). We carried out our study in two rounds. A first round from January to October 2015 consisted of a total sample of 2,986 male respondents from 37 villages.[19] Following reviewer and editorial feedback, we then conducted a second round from April to May 2019 where we introduced design modifications for robustness checks and further investigations of potential channels. This second-round sample consists of another 872 male respondents. Whenever we can, we pool observations from the two rounds (and then, where appropriate, report separate regressions for the two rounds in app. B).

Survey participants were recruited through door-to-door household visits in a presurvey location round. Available households were then randomly assigned to primary and replacement lists to meet a prespecified target for each village. In each village (or in each settlement if a village consisted of more than one settlement), we conducted a limited number (45–50) of surveys and surveyed for only a couple of days. This was to minimize the risk that information about how our games are conducted would spread in the village, contaminating our sample. Since the second

[19] To measure potential heterogeneous treatment effects by caste, we stratified part of the first round by identifying neighborhoods with high-, low-, and middle-tier households through a preexisting definition of caste from SVHS. Caste, or *quom*, as it is referred to in Cheema and Naseer (2013), is defined as a social group based on patrilineal descent. Castes are further distinguished into high or low depending on colonial assignment of ownership of land. We found little evidence of heterogeneous impact of the informational provision by caste.

round was conducted in a subset of the first-round villages, we added additional screener questions to exclude potential respondents who had either played or heard of our games and survey activity before. In any case, our preventative measures of limiting surveying days in each village appear to have been successful; no respondents had met our criteria for exclusion as defined above.[20] Table B1 presents summary statistics for our sample as well as the baseline values for our main outcome variables (observation counts vary because not all questions were asked in the two rounds). Recall that our primary sample includes only men, and from the first panel we see that they have an average age of 38, 76% have some formal education, 28% own land, and their average monthly household income is PKR 17,001. There is also considerable heterogeneity in land ownership and income. Though there is reasonable usage of state courts, as mentioned previously, our respondents have a significantly higher expected usage of the panchayat, somewhat higher trust and perceived service effectiveness, and similar views on enforcement effectiveness of the two forums. Interestingly, their baseline allocations to the two forums are quite similar in both the fund dictator and the investment games.[21]

IV. Results

In this section, we present our main results. As detailed in the previous section, our main treatment is to provide our respondents with (truthful, media-reported) information about the improved performance of state courts. We then measure how this affects their (self-reported) expected

[20] Because we used the same sample frame of households for the second round of sampling, our households are on average about 2 years older and have more education and assets than in the first round. We confirmed that these compositional differences have no effect on our results. In particular, we show in table B3 (tables B1–B32 are available online) that these differences typically have no predictive power for the pre- and posttreatment changes in expected usage and game allocations, and in no specification do they have a statistically significant impact on our treatment effects—the estimates of β in eq. (2) below.

[21] Recall from the previous section that our respondents (correctly) believe that accessing state courts is substantially more expensive than using panchayats. This may explain why, even though they expect lower usage of state courts, allocations to the panchayat in the fund dictator game are only slightly higher than allocations to state courts. Thus, despite our instructions to ignore cost factors in their game allocations, the respondents may have incorporated them into their decisions. For the investment game, our conjecture is that this similarity in baseline allocations reflects the fact that our respondents do not perceive any difference in enforcement effectiveness and only a small difference in service effectiveness between state courts and panchayats. Since the investment game was designed such that the rate of return is determined by enforcement and service effectiveness and not expected usage (or cost of accessing the forums), it is plausible that their baseline allocations should not be very different. Even though baseline allocations in these two games are not different between state courts and panchayats, they are meaningful and informative. The correlation between the difference between baseline state and nonstate allocations and the difference between state and nonstate usage is positive: 0.39 for the fund dictator game and 0.42 for the investment game.

usage of state courts and their behavior in the two games described above. We first present our results concerning the effects of our state positive treatment on self-reported and high-stakes behavior toward state courts. We then turn to indirect effects on panchayats and also present a range of robustness checks. The next section investigates the mechanisms behind these effects.

A. *Direct Effects*

Our primary informational treatment, detailed above, provides positive information about reduced delays in state courts. Our first results are from the within-subject design described in the previous section and are presented in panel A of table 1. We estimate

$$Y_{it} = \alpha + \beta \text{Post}_t + \delta_i + \varepsilon_{it}, \quad (1)$$

where Y_{it} is our outcome variable of interest, which is either expected usage or allocations in the two games, and Post_t is a dummy for observations after the state positive treatment. The parameter β is the coefficient of interest and measures the within-subject effect of the informational treatment—how much a given person changes their behavior (and later perceptions) following the new information. In addition, ε_{it} is an error term capturing all omitted influences, and δ_i is a person fixed effect. The inclusion of these fixed effects has no impact on the estimates of β but improves precision.

Panel A of table 1 reports estimates of equation (1) for our three main outcome variables—expected usage, allocation to the state in the fund dictator game, and allocation to the state in the investment game. We see uniformly positive and precisely estimated effects on these three variables. The table additionally includes the estimate of the constant, α, which represents the pretreatment average. For example, in column 1 the state positive treatment increases expected usage by about 20%—by 0.8 (SE = 0.05) starting from a base of about 4.1.

In contrast to the expected usage variable, our two other main variables are not based on self-reports and come from our respondents' behavior in fairly high-stakes experiments. The picture they paint is very similar to the expected usage variable. Column 2 shows a large impact on the amount allocated to state courts in the fund dictator game. Starting from a base of 104.8, this allocation increases on average by 15.4 (SE = 1.30). This corresponds to an average posttreatment allocation of 120.2 or roughly one-quarter of a day's wages in our sample villages and represents a 15% increase from the pretreatment base. The pattern for the investment game (col. 3) is similar. In this case, we see an increase of 14.6 (SE = 1.34) from a base of 115.4.

TABLE 1
DIRECT EFFECTS

	Expected Usage (1)	Allocation in Fund Dictator Game (2)	Allocation in Investment Game (3)
A. Effects of state positive treatment on state courts:			
Posttreatment	.80	15.41	14.59
	(.05)	(1.30)	(1.34)
Constant	4.06	104.82	115.39
	(.03)	(.92)	(.95)
Observations (respondents)	3,812 (1,906)	3,918 (1,959)	3,938 (1,969)
B. Effects of social experimenter treatment on state courts:			
Posttreatment	.28	1.72	2.23
	(.05)	(1.66)	(1.81)
Constant	3.23	100.32	103.26
	(.04)	(1.18)	(1.28)
Observations (respondents)	1,702 (851)	1,822 (911)	1,806 (903)
C. Netting out social experimenter effects:			
Post × state positive	.69	12.54	16.05
	(.10)	(2.78)	(2.90)
Observations (respondents)	5,514 (2,757)	5,740 (2,870)	5,744 (2,872)

NOTE.—This table estimates the effects of the state positive treatment on expected usage and game allocations for state courts. "Posttreatment" is a dummy for posttreatment observations. "Expected Usage" is the likelihood of using state courts, with values between zero and 10. Allocations in the fund dictator and investment games are the amounts that the respondent allocated to state courts in the two games. Panel A includes the respondents who received the state positive treatment, panel B includes the respondents who received the social experimenter treatment, and panel C is for all respondents. All regressions include individual fixed effects, and panel C additionally includes a posttreatment dummy and strata fixed effects interacted with the posttreatment dummy to control for varying sampling probabilities across strata. The constants in panels A and B represent the pretreatment values of the relevant variables. Observation counts vary because of differences in response rates and small changes in survey questions. Standard errors are reported in parentheses.

Panel B turns to the social experimenter effect. One may be concerned that the estimates in panel A reflect our respondents' desire to act in a way that they think the experimenter would like to see. Since the informational treatment is providing positive news about the state, respondents may infer that we would like them to become more positive toward the state and change their responses accordingly. We use our social experimenter treatment to gauge the extent of these effects. As described in the previous section, we provide a statement that contains only an opinion about state courts, without any clear, objective information. The experimenter effect should be, if anything, stronger in this case because there is a clearly stated opinion. But in the absence of objective information, there should be no, or only very limited, updating of beliefs—there can be some updating if the respondent thinks that the experimenter's opinion was in turn informed by some relevant metric.

We see in panel B of table 1 that changes in our main outcome variables following the social experimenter treatment are very different than in panel A.[22] Although there is a small response for expected usage, which increases by about one-third of our estimate in panel A, 0.28 (SE = 0.05), there are no statistically significant effects from the social experimenter treatment in the higher-stakes decisions in the two games.

This question is more formally investigated in panel C, where we pool our state positive treatment and social experimenter treatment samples and estimate the differential impact of the state positive treatment relative to the social experimenter treatment. More specifically, we estimate the following model:

$$Y_{it} = \alpha + \gamma \text{Post}_i + \beta \text{StatePositive}_i \cdot \text{Post}_t + \delta_i + \text{Post}_t \cdot S_i + \varepsilon_{it}, \quad (2)$$

where the parameter of interest, again denoted by β, is now the coefficient on the interaction between the posttreatment dummy, Post_t, and a dummy for the group receiving the state positive treatment, StatePositive_i. The coefficient on the dummy Post_t, denoted by γ, captures the impact on respondents who receive the social experimenter treatment. We additionally include a full set of interactions between Post_t and strata fixed effects, denoted by S_i, to account for the strata-level differences in sampling probabilities for the two treatments. The results from the estimation of equation (2) are consistent with what can be seen by comparing panels A and B: there is a large impact of the state positive treatment even once the social experimenter effect is netted out.[23]

Overall, we conclude from table 1 that there are robust and fairly large effects from our state positive treatment, which provide accurate and favorable information about delays in state courts, on self-reported expected usage, and on allocations in high-stakes experimental games. These results suggest that, despite the pervasive and deep-rooted inefficiencies of state institutions in Pakistan, citizens are willing to update their views and behavior in response to credible information about improved service

[22] Table B4 shows that individual characteristics and baseline responses are broadly balanced between our state positive and social experimenter treatment samples. In particular, out of the 19 variables on individual characteristics and baseline responses available for our entire sample, one of them (the allocation made to the state in the fund dictator game) shows differences that are significant at 10%, which is not surprising given what we would expect due to sampling variation.

[23] The coefficients in panel C should not be the same as the difference between the estimates in panels A and B, because our sample is stratified (where each strata has varying fractions of the state positive and social experimenter treatments). These strata do not matter in panels A and B, where the estimation is entirely within subject. They do in panel C, because there is a cross-subject element. For this reason, we include strata fixed effects interacted with the Post_t dummy in this panel, making the estimates in panel C deviate somewhat from the simple difference between the coefficients reported in panels A and B.

quality. We next turn to the spillover effects of this informational treatment on behavior concerning the nonstate actor.

B. Indirect Effects

As we emphasized in the introduction, ineffectiveness of state institutions may prompt citizens to turn to nonstate actors for public services, and conversely, when state institutions improve, the same mechanism may trigger a withdrawal away from nonstate actors. We now investigate this question, focusing on the nonstate actor competing with state courts in the area of dispute resolution: panchayats. Our main results are summarized in table 2, which has a structure identical to table 1, except that the dependent variables are expected usage and the allocations in the two games for the panchayat. The informational treatment continues to be the same—providing positive information about the effectiveness

TABLE 2
INDIRECT EFFECTS

	Expected Usage (1)	Allocation in Fund Dictator Game (2)	Allocation in Investment Game (3)
A. Effects of state positive treatment on panchayats:			
Posttreatment	−.55	−10.42	−12.13
	(.04)	(1.30)	(1.35)
Constant	6.64	103.99	116.68
	(.03)	(.92)	(.96)
Observations (respondents)	3,810 (1,905)	3,918 (1,959)	3,938 (1,969)
B. Effects of social experimenter treatment on panchayats:			
Posttreatment	−.24	−2.84	−3.23
	(.06)	(1.87)	(1.82)
Constant	7.01	109.31	116.15
	(.04)	(1.32)	(1.29)
Observations (respondents)	1,700 (850)	1,822 (911)	1,806 (903)
C. Netting out social experimenter effects:			
Post × state positive	−.38	−4.82	−6.81
	(.09)	(2.87)	(2.93)
Observations (respondents)	5,510 (2,755)	5,740 (2,870)	5,744 (2,872)

NOTE.—This table estimates the (indirect) effects of the state positive treatment on expected usage and game allocations for panchayats. "Posttreatment" is a dummy for posttreatment observations. "Expected Usage" is the likelihood of using panchayats, with values between zero and 10. Allocations in the fund dictator and investment games are the amounts that the respondent allocated to panchayats in the two games. Panel A includes the respondents who received the state positive treatment, panel B includes the respondents who received the social experimenter treatment, and panel C is for all respondents. All regressions include individual fixed effects, and panel C additionally includes a posttreatment dummy and strata fixed effects interacted with the posttreatment dummy to control for varying sampling probabilities across strata. The constants in panels A and B represent the pretreatment values of the relevant variables. Standard errors are reported in parentheses.

of state courts—so that our focus is now indirect effects on the nonstate actor.

The results are very clear-cut. There are large, statistically significant, and robust impacts on panchayats from our state positive treatment about improved delays in state courts.

In panel A of table 2, we start with our within-subject design (for the sample that has received the state positive treatment). The estimates in the three columns are directly comparable to the estimates in panel A of table 1. In column 1, we see a negative indirect effect: −0.55 (SE = 0.04). This is about 30% smaller than the positive estimate in table 1 but still very precisely estimated and significant.

In columns 2 and 3, we see similar indirect effects for the allocations in the fund dictator and investment games. The estimates are again precise and statistically significant: −10.4 (SE = 1.30) in column 2 and −12.1 (SE = 1.35) in column 3. The estimate in column 2 is about 30% smaller than the direct positive effect in table 1, while the estimate in column 3 is about 15% smaller.

In panel B, we estimate the social experimenter effects in the same fashion as in panel B of table 1, and we find similar results. There is a negative impact on self-reported expected usage, but this is much smaller than the impact of the state positive treatment in panel A. The effects on the allocations in the two games are small and imprecisely estimated, insignificant for the fund dictator game, and marginally significant at 10% for the investment game. When we net out the social experiment effects in panel C using the specification in (A4), we continue to find significant and sizable negative effects from the state positive treatment on the nonstate actor.

In summary, our results show substantial negative spillovers on nonstate institutions once individuals update their beliefs about improvements in state institutions. These results suggest that information about the effectiveness of state institutions may not only convince people to engage more with these institutions but also induce them to disengage from nonstate actors providing competing services. We next investigate the robustness of these results before turning to a more detailed examination of beliefs and trust variables to shed light on the mechanisms for these indirect effects.

C. Anonymity

The social experimenter concerns arise in part because the lack of anonymity perceived by our respondents may affect their responses. The relatively high stakes in our games and the lack of comparable results from our social experimenter treatment supports the interpretation that our direct and indirect estimates could not be explained by social experimenter effects. Nevertheless, to provide additional evidence against social

experimenter concerns, in the second round we designed a fully anonymous version of the fund dictator game, where allocations are completely private and cannot be identified by surveyors or researchers.

Our anonymous fund dictator game is designed as follows. Surveyors prepared packets of game materials containing a randomized participation fee, varying between PKR 30 and 70, and one empty white envelope for each allocation of a respondent. Each envelope was labeled with a unique ID on the inside of the envelope, such that all envelopes in one packet contain the same ID, and which envelopes had what ID was neither known to the surveyors nor recorded by us. Before the game, each respondent was presented with 10 such game packets and picked one. Their choice of packet was not observed by surveyors and cannot be recovered later. The games were played as before, with the major exception that the respondents put their (baseline and posttreatment) allocations privately into their envelopes and sealed and placed them in the appropriate game board. This procedure ensured that no one except the respondents themselves could observe the allocations they made. In the final step, each respondent was paid the amount he decided to keep for himself in a randomly chosen game out of the four he played. Since directly observing this amount in the process of working out their payments would have revealed their allocation for one of the four games, we implemented an additional procedure to ensure that this amount was paid in an anonymous manner as well. This was done by the respondent combining their allocation in the envelope chosen with the random participation fee, without showing either of the two amounts to the surveyor. Since the latter, determined by the packet picked by the respondent, is not known to us, the total paid to each respondent does not reveal his allocation even in the randomly chosen game. Finally, respondents took their sealed envelopes and placed them in a single container combining the envelopes from all participants, thus ensuring that their identity could never become known thereafter. Since envelopes from the same individual had the same ID marked inside the envelope, we could still construct an individual (but anonymized) panel for econometric analysis.

Our pilots revealed that this procedure was well understood by the respondents, and this was confirmed by their postgame survey responses. All of the respondents we asked reported that they thought their allocations were fully anonymous.

Table 3 shows that the results in this anonymized game are qualitatively similar to those we obtained with the original fund dictator game. These results suggest that lack of anonymity and hence any related social experimenter concerns are not a major contributor to the direct and indirect effects we are estimating.

We should note that the estimates in these games are almost twice as large as our original estimates in tables 1 and 2. This, however, is not related

TABLE 3
EFFECTS OF STATE POSITIVE TREATMENT IN ANONYMOUS GAMES

	Expected Usage (1)	Allocation in Fund Dictator Game (2)
A. Direct effects (on state courts):		
Posttreatment	1.63	34.46
	(.10)	(2.84)
Constant	3.86	74.30
	(.07)	(2.01)
Observations (respondents)	498 (249)	498 (249)
B. Indirect effects (on panchayats):		
Posttreatment	−.66	−21.29
	(.08)	(3.64)
Constant	6.47	103.21
	(.06)	(2.57)
Observations (respondents)	498 (249)	498 (249)

NOTE.—This table estimates the direct and indirect effects of the state positive treatment on expected usage and game allocations in an anonymous version of the fund dictator game. "Posttreatment" is a dummy for posttreatment observations. For the definitions of "Expected Usage" and "Allocation in Fund Dictator Game," see the notes to tables 1 and 2. Panel A reports effects on state courts, while panel B reports (indirect) effects on panchayats. All regressions include individual fixed effects. The constants in panels A and B represent the pretreatment values of the relevant variables. Standard errors are reported in parentheses.

to anonymity, since it is a pattern that is present in all of the games in the second round. This can be seen in tables 5 and 7, and tests for differential effects do not reject the hypothesis that the magnitudes of the direct and the indirect effects in anonymous and nonanonymous second-round games are the same (see table B5). Rather, this difference is likely a consequence of the much greater activism of the Supreme Court we outlined in section II, which appears to have made our state positive informational treatment more powerful. In particular, though in the baseline our respondents did not exhibit significant changes in their beliefs or behavior concerning the effectiveness of state courts, they appear (and report in survey questions) to find the state positive treatment more believable. Consistent with this interpretation, the responsiveness of the belief questions is significantly more pronounced in the second round (table B23). We show in the next subsection that increased credibility of information indeed leads to greater responses among our subjects.[24]

[24] The constants in the first- and second-round games are generally fairly similar; see, e.g., panel A of tables 1 and 7. The one difference is for the anonymous games, where baseline court allocations in the fund dictator game are somewhat smaller (see panel A of table 3). We conjecture that this is because anonymity may have encouraged less generous giving in the baseline.

D. *Credibility of Information*

In this subsection, we report two exercises that bolster the case that respondents are indeed reacting to the specific informational content of our treatment. First, our survey asked respondents about how much they trusted the information we provided. In table 4, we interact this measure with our treatment variable and estimate the following model:

$$Y_{it} = \alpha + \beta \text{Post}_i + \kappa \text{Credibility}_i \cdot \text{Post}_t + \delta_i + \varepsilon_{it}, \quad (3)$$

where Credibility_i is a dummy for individual i reporting a high level of trust in the accuracy of the information. Our subjects reported their trust in this information on a scale of zero to 10, and we code those reporting trust greater than or equal to six as having high credibility. The informational channel suggests that these individuals should respond more. Estimates of this equation reported in table 4 support this

TABLE 4
HETEROGENEITY BY TRUST IN THE INFORMATION

	Expected Usage (1)	Allocation in Fund Dictator Game (2)	Allocation in Investment Game (3)
A. Direct effects (on state courts):			
Posttreatment	.74	13.45	11.86
	(.09)	(2.36)	(2.63)
Post × high trust	.68	13.97	12.84
	(.11)	(2.79)	(3.11)
Constant	3.75	101.06	109.51
	(.03)	(.89)	(.99)
Observations (respondents)	3,902 (1,951)	4,008 (2,004)	3,532 (1,766)
B. Indirect effects (on panchayats):			
Posttreatment	−.41	−8.01	−6.53
	(.08)	(2.54)	(2.70)
Post × high trust	−.37	−8.58	−10.03
	(.10)	(3.06)	(3.26)
Constant	6.48	106.80	116.21
	(.03)	(1.00)	(1.07)
Observations (respondents)	3,404 (1,702)	3,510 (1,755)	3,034 (1,517)

NOTE.—This table estimates heterogeneous direct and indirect effects by trust in the information provided in the state positive treatment. "High trust" is a dummy for respondents who reported the level of trust in the information provided to be greater than or equal to six (on a scale from zero to 10). "Posttreatment" is a dummy for posttreatment observations. For the definitions of "Expected Usage," "Allocation in Fund Dictator Game," and "Allocation in Investment Game," see the notes to tables 1 and 2. Panel A reports effects on state courts, while panel B reports (indirect) effects on panchayats. All regressions include individual fixed effects. The constants in panels A and B represent the pretreatment values of the relevant variables. Standard errors are reported in parentheses.

prediction (see also table B26). We robustly estimate a significant and quantitatively large κ. For example, an individual who reports a high level of trust (Credibility$_i$ = 1) has on average twice as large a response as an individual who attaches a low level of credibility to the same information (Credibility$_i$ = 0).

Our second strategy is more ambitious. In the second round, we devised a new set of games where we varied the source of the information provided in our treatment. Pilots revealed that the respondents trust national media more than local (private) television channels. Building on this information, our design provides two sealed envelopes to our respondents, who are told that the information in one envelope has been reported from the national PTV while the other has been reported in private television channels. The information in both envelopes is identical (this information was indeed reported in both national and local media). Individuals were randomly assigned to one of the two envelopes, which they opened and read (the randomization was done by individuals picking chits from a ballot box). This design has the added advantage that the information provided was further disassociated from the surveyors/researchers.

The results are reported in table 5. We have a strong first-stage relationship in column 1, confirming that respondents' trust in the information is greater when it is reported by PTV as opposed to coming only from private television channels. Our main results in this table, reported in columns 2–4 (panels A–C), indicate that there is a statistically significant and quantitatively large additional direct impact (about one-third of our main effects) from the more credible information. Inevitably, the impacts of this additional differential treatment on indirect effects, shown in columns 5–7, are less precise, and our estimates are statistically insignificant.[25] Nevertheless, the differential direct effects provide a powerful confirmation of the informational channel hypothesized in this paper.

E. Robustness

In this subsection, we report several additional robustness checks on our direct and indirect effects. Our main specifications utilize a within-subject design, whereby the effects of interest are estimated by using information on how our respondents change their expectations and behavior after being provided with relevant information. We present estimates based on a pure cross-subject strategy in table B8. These results, particularly in columns 4–6 where we control for baseline (pretreatment) values, are

[25] Even though our randomization procedure was implemented correctly, because of the relatively small size of the sample here there are some baseline differences between the two credibility treatments; see table B6. Reassuringly, once we control for age differences, this imbalance is largely eliminated. Moreover, all of our results are robust to allowing differential trends by age as shown in table B7.

TABLE 5
VARYING CREDIBILITY OF INFORMATION

	First Stage	Direct Effects (on State Courts)			Indirect Effects (on Panchayats)		
	Trust in Information (1)	Expected Usage (2)	Allocation in Fund Dictator Game (3)	Allocation in Investment Game (4)	Expected Usage (5)	Allocation in Fund Dictator Game (6)	Allocation in Investment Game (7)
A. Effects of high-credibility treatment:							
Posttreatment		1.55	40.00	42.53	−.89	−25.54	−16.94
		(.11)	(3.83)	(3.22)	(.11)	(3.24)	(2.94)
Constant	7.33	4.47	96.83	101.61	5.65	105.32	108.49
	(.11)	(.08)	(2.71)	(2.28)	(.08)	(2.29)	(2.08)
Observations (respondents)	374 (187)	374 (187)	374 (187)	374 (187)	374 (187)	374 (187)	374 (187)
B. Effects of low-credibility treatment:							
Posttreatment		1.07	30.48	25.83	−.65	−24.06	−19.89
		(.10)	(3.00)	(2.63)	(.10)	(3.25)	(2.64)
Constant	6.01	4.01	90.53	95.88	6.28	105.67	120.05
	(.13)	(.07)	(2.12)	(1.86)	(.07)	(2.30)	(1.87)
Observations (respondents)	372 (186)	372 (186)	372 (186)	372 (186)	372 (186)	372 (186)	372 (186)

C. Differential effects of high-credibility treatment:							
Posttreatment		1.07	30.48	25.83	−.65	−24.06	−19.89
		(.11)	(3.43)	(2.93)	(.11)	(3.24)	(2.79)
Post × credibility	1.32	.48	9.52	16.70	−.25	−1.47	2.96
	(.17)	(.15)	(4.86)	(4.16)	(.15)	(4.59)	(3.95)
Constant	6.01	4.24	93.67	98.74	5.97	105.50	114.29
	(.12)	(.05)	(1.72)	(1.47)	(.05)	(1.62)	(1.40)
Observations (respondents)	746 (373)	746 (373)	746 (373)	746 (373)	746 (373)	746 (373)	746 (373)

NOTE.—This table estimates heterogeneous direct and indirect effects by the credibility of the information provided in the state positive treatment. Panel A includes respondents who received the high-credibility treatment (from national PTV), panel B includes respondents who received the low-credibility treatment (from private television channels), and panel C includes all respondents. "Credibility" is a dummy for receiving the high-credibility treatment. "Posttreatment" is a dummy for posttreatment observations. For the definitions of "Expected Usage," "Allocation in Fund Dictator Game," and "Allocation in Investment Game," see the notes to tables 1 and 2. All regressions include individual fixed effects. The constants in panels A and B represent the pretreatment values of the relevant variables. Standard errors are reported in parentheses.

close to those in tables 1 and 2. Although designed for within-subject comparisons, we find it reassuring that the data generated from our experiment yield results similar to these cross-subject specifications.

Table B9 investigates whether respondents correctly forecast how others' expected usage and allocation choices in the two games change in response to the same information. Columns 1–3 are analogues of the specifications in panel A of tables 1 and 2, while columns 4–6 are analogues of the specifications in panel C (which net out the social experimenter effect). The results are in line with those in tables 1 and 2 and show that our respondents not only respond to the positive information about state courts but also predict correctly and with considerable precision that others will do likewise, and with very similar magnitudes. We find these patterns to be a reassuring reality check on our main results.

In addition to carefully explaining the procedures and details of each game to our respondents at each stage, we collected information on the surveyors' and the respondents' own assessments of how well they understood the game to check the implications of any remaining confusion about the game. In table B10, we report estimates after removing any respondents who received a score below five (out of 10) from either their own reports or the surveyors'. The results are very similar to our baseline estimates. In table B11, we show estimates from specifications that downweight outliers (following the procedure of Li 1985), which are also very similar to our baseline results. In table B12, we report estimates after excluding respondents who have allocations in any of the games that are at the "boundary"—meaning that before the state positive treatment they were allocating the minimum or the maximum amount to one of the two actors. The results are again very similar to the baseline results. We also randomized the order in which the respondents played different games and answered different questions, and the results in table B13 show that there are no robust order effects.

We additionally investigated whether (positive or negative) "social multiplier" effects, related to expectations of our respondents concerning others' information or behavior, complicate the interpretation of our results. To check for such social multiplier effects, we emphasized to a random subset of our respondents that others in their village were also given the same information. Using this variation, we estimated social multiplier effects (interacting our Post_i variable with a dummy for having received this information). As table B14 shows, we find no evidence of any such effects.

In our pilots, in addition to our main, state positive, informational treatment that provides positive information about state institutions, we experimented with treatments that provide (truthful) negative information about state institutions as well as negative or positive information about nonstate actors. Though naturally underpowered, the results in these smaller pilot samples are in line with our expectations. In table B15, with

the state negative treatment, we estimate statistically significant negative direct effects for usage and the investment game (the fund dictator game was not played at the time this pilot was conducted) and a positive and statistically significant indirect effect for expected usage but imprecise estimates for allocations in the investment game. For the nonstate negative treatment, table B16 shows negative and statistically significant direct effects for the nonstate actor and positive and generally statistically significant positive indirect effects for state institutions. With the nonstate positive treatment in table B17, where we have the smallest sample, the effects have the expected signs, but they are mostly imprecise. We find it reassuring that, even with the much smaller samples, we are able to detect effects consistent with our expectations and that when the information is negative, our estimates show negative direct and positive indirect effects. In summary, our main results appear quite robust to a range of variations and are not unduly affected by outliers or respondents who may not have fully understood the context or the details of the game.

V. Mechanisms

In this section, we investigate the potential channels underlying our main results—the positive direct effects on behavior toward state courts and the negative indirect effects on nonstate panchayats. We start with a discussion about the different potential channels that may be at play and then present additional empirical evidence relevant for these channels.

A. Conceptual Framework

The results presented in the previous section show robust direct and indirect effects from the state positive treatment about improved delay times in state courts. The direct effects on expected usage and game allocations toward the state actor indicate that our respondents found this information novel and believable and changed their beliefs and behavior in response. Their survey responses and our estimates of heterogeneous effects depending on the credibility of information bolster this interpretation (see sec. IV.D). This type of reaction is consistent with several models of Bayesian and non-Bayesian belief formation.[26] In appendix A, we present a simple Bayesian model of belief updating

[26] One interesting question is whether our respondents changing their beliefs and behavior in response to one piece of information is an "overreaction." We are not able to address this question because it is difficult to estimate how precise our respondents' priors are. The fact that rural households do not have very reliable information about the functioning of state courts makes it plausible that they had imprecise priors. On the other hand, the experience of several households with state courts in various contexts (discussed in sec. II) suggests that they may have relatively precise priors.

combined with allocation decisions, which clearly illustrates these direct effects. Though in practice our respondents may not be fully Bayesian, Bayesian and non-Bayesian models do not have different implications so far as our experimental results on direct effects are concerned.

The more intriguing and novel results of our study concern the indirect effects on nonstate panchayats. Recall that our experiment is designed such that the information we provide is not directly relevant to the effectiveness and trustworthiness of panchayats. Nevertheless, as we anticipated in the introduction, our respondents changed not only their expected usage of panchayats but also their allocations in the fund dictator and investment games. Changes in expected usage are relatively easy to understand: if our respondents expect to use state courts more frequently, they may make less use of panchayats. But it is not immediately clear why this should change their allocations. As explained above, the investment game is designed such that their allocations to panchayats should depend only on their belief concerning the effectiveness of this actor's rulings. It is also not clear whether they should change their allocations in the fund dictator game (as we explain in app. A). We next discuss three sets of reasons why there may be such changes and derive additional predictions from these approaches, which we then investigate empirically.

Motivated reasoning.—In models of motivated reasoning, individuals choose not only actions but also their beliefs. The motivation for the choice of beliefs is that the overall utility of an individual may be higher when there is greater congruence between actions and beliefs. One of the most celebrated versions of this idea is Festinger's (1962) theory of cognitive dissonance in social psychology, where individuals feel dissonance if their behavior and beliefs are not congruent and try to reduce this dissonance by changing either behavior or beliefs (see other references in n. 4). One of the first applications of this idea in economics was in Akerlof and Dickens's (1982) work on the behavior of coal miners. If coal miners accept that mines are dangerous and hazardous, they will feel a high degree of cognitive dissonance. Since redressing this dissonance by changing their occupation is costly, they may instead decide to convince themselves that coal mines are not dangerous and do not lead to worse health outcomes. Distorting beliefs is beneficial to the individual because of cognitive dissonance considerations but comes with costs because it distorts behavior as well. For example, a coal miner who receives a manufacturing job offer with a slightly lower wage would be objectively better off if he were to take this new job, but, believing that coal mining is not dangerous, he would not do so.

Motivated reasoning provides a natural conceptual framework for the linkage between the attitudes toward state and nonstate actors, since individuals typically need to accept some of the values, ideology, and norms of the actor they interact with (see Acemoglu and Robinson 2019). Given

this premise, in appendix A we develop a model of motivated reasoning, which illustrates how information about state courts affects expected usage patterns, allocations in our games, and beliefs concerning both state courts and nonstate panchayats. The main conclusion from this model is that positive information about state courts will make our agents become more negative about the competing nonstate actor, because they expect to use the nonstate actor less and thus dial back their initially inflated positive views of this alternative forum while becoming more positive toward state courts (see app. A for why beliefs in this model are always more positive than objectively justified). This is despite the fact that the information we provide has no relevance about the effectiveness or trustworthiness of the nonstate actor. The motivated reasoning model additionally suggests a potential feedback cycle: when state institutions are ineffective, people start turning to nonstate actors, and this motivates them to change their beliefs to become more positive toward nonstate actors and less positive about state institutions. But this then discourages interactions with state institutions further, paving the way to a vicious cycle.

Bayesian updating.—In appendix A, we additionally analyze the same setting when agents are pure Bayesians, without any motivated reasoning considerations. We show that under the most plausible assumptions, Bayesian agents should not adjust their behavior or beliefs toward the nonstate actor in response to the new information about the effectiveness of the state actor. Under alternative assumptions—for example, because there are powerful income effects—their behavior toward the nonstate actor may change, but they should not alter their beliefs about and their trust in the nonstate actor. This result, together with our empirical results documenting such changes, justifies our focus on non-Bayesian behavior based on motivated reasoning.

There is, however, one case in which Bayesian agents also change their views about the nonstate actor in response to our state positive treatment. This happens when the priors about the state and the nonstate actors are negatively affiliated—meaning that our respondents believe that if the state is effective, then panchayats must be ineffective/nontrustworthy. None of the respondents in the focus groups we conducted in four villages as part of our pilots expressed views consistent with such negatively affiliated priors. For completeness, we will return to a discussion of this type of Bayesian updating in the context of heterogeneous effects in section V.D. We also note that this type of negative affiliation in priors, if present, can be interpreted as a type of linkage between views of the state and nonstate actors and thus would be consistent with our overall message.

Mechanical contrast effects.—A final possibility is that in our experiments there is an indirect effect on the nonstate actor, but this reflects a nongeneralizable feature of the experimental setting, not a type of response that would occur in real-world environments. The idea is that when presented with two options in an experiment, respondents may naturally be tempted

to engage in relative comparisons—for example, reporting that an option is less attractive because it is paired with a particularly attractive alternative. Such mechanical contrast effects could be responsible for the negative indirect effects on panchayats. Contrast effects have been detected in various social psychology experiments (e.g., Pepitone and DiNubile 1976; Kenrick and Gutierres 1980; Wedell, Parducci, and Geiselman 1987). They are also present in field experiments and quasi experiments. For instance, Bhargava and Fisman (2014) provide a clear example in the context of speed dating evaluations, where naturally occurring exogenous variation in the attractiveness of prior matches affects the scores individuals give to their subsequent matches. Contrast effects could also arise from rational decision-making under uncertainty (e.g., Kamenica 2008).

Though such contrast effects are a theoretical possibility, we show below that they cannot explain our results since negative indirect effects are present only when the comparison is between two competing forums (see below). We next turn to an empirical investigation of some of the implications and mechanisms implied by these different approaches, starting with effects on beliefs and trust.

B. Effects on Beliefs and Trust

The key prediction of the motivated reasoning model is that information about reduced delays in state courts should adversely affect beliefs about and trust in panchayats. In this subsection, we investigate these issues, focusing on four variables designed to measure beliefs and trust. Our first three variables are "service effectiveness," which proxies our respondents' beliefs about how effectively a forum provides services to an ordinary person; "enforcement effectiveness," which corresponds to how effectively a forum enforces its verdicts; and "access," which measures how easy it is for an ordinary person to access the forum. Our fourth variable, "trust," is a general measure of trust in the forum.

The impacts of our state positive treatment on the belief variables are investigated in table 6. Panel A looks at the direct effects on state courts, while panel B focuses on indirect effects on panchayats. Panel A shows fairly uniform improvements in beliefs, usage, and trust for state courts, consistent with the notion that the respondents believe our state positive treatment and update their views about state courts positively. For example, we estimate a direct effect of 0.87 (SE = 0.04) for service effectiveness, 0.54 (SE = 0.03) for enforcement effectiveness, 0.60 (SE = 0.04) for access, and 0.87 (SE = 0.04) for trust. These effects are sizable, ranging from 10% to 20% of the baseline values (represented by the constants in the table).[27]

[27] As noted in n. 14, these belief questions were introduced at later stages of our fieldwork (one-third of the first round and all of the second round). Our budget, coupled with concerns

TABLE 6
BELIEFS

	Service Effectiveness (1)	Enforcement Effectiveness (2)	Access (3)	Trust (4)
A. Direct effects (on state courts):				
Posttreatment	.87	.54	.60	.87
	(.04)	(.03)	(.04)	(.04)
Constant	3.95	6.55	2.96	4.86
	(.03)	(.02)	(.03)	(.03)
Observations (respondents)	3,363 (1,682)	3,385 (1,693)	3,394 (1,697)	3,371 (1,686)
B. Indirect effects (on panchayats):				
Posttreatment	−.17	−.16	.07	−.24
	(.03)	(.02)	(.03)	(.03)
Constant	5.41	6.26	7.93	6.53
	(.02)	(.02)	(.02)	(.02)
Observations (respondents)	2,888 (1,444)	2,885 (1,443)	2,888 (1,445)	2,886 (1,443)

NOTE.—This table estimates the direct and indirect effects of the state positive treatment on belief and trust questions. "Posttreatment" is a dummy for posttreatment observations. "Service Effectiveness" measures how effective respondents believe the forum to be at providing services to ordinary people. "Enforcement Effectiveness" measures how effective respondents believe the forum to be at enforcing its verdicts. "Access" measures how easy respondents believe it is for ordinary people to access the forum. "Trust" measures how much respondents trust the forum. Panel A reports effects on state courts, while panel B reports (indirect) effects on panchayats. All regressions include individual fixed effects. Standard errors are reported in parentheses.

More interestingly, and consistent with the predictions of our motivated reasoning model, we also estimate fairly precise, though quantitatively smaller, indirect effects on panchayats in panel B. For example, in column 1 of panel B, the estimate for the indirect effect on service effectiveness for panchayats is negative and significant: -0.17 (SE $= 0.03$). In column 2,

of not having a large enough sample for these questions, meant that we chose not to provide the social experimenter treatment to a subsample of the respondents answering these questions. Because our initial sample results do not show any sizable social experimenter effects, we believe that the impacts for these belief questions are unlikely to be because of social experimenter effects. Moreover, while we did not ask these detailed questions in the initial sample, we did ask two questions about the relative effectiveness of the state and nonstate actors in delivering justice and enforcing verdicts at the end of our survey. Since we have the social experimenter treatment for this sample, we tested whether this treatment had a similar impact on beliefs. Table B18 presents the results using a cross-subject design (for we have answers to these questions only at the end of the survey) and shows statistically significant and sizable impacts from our state positive treatment relative to the social experimenter treatment. It is also relevant in this context that the belief results hold in the anonymous version of the fund dictator game (see table B19). Both of these results allay concerns related to social experimenter effects.

for enforcement effectiveness, the estimate is −0.16 (SE = 0.02). There are similarly significant and quite precise negative effects on overall trust. The only exception to this pattern is for access in column 3, where we have a positive (albeit small) indirect effect: 0.07 (SE = 0.03). This effect is not present in our second-round data, as shown in table B23.[28]

We checked the robustness of the results reported in table 6 in a number of ways. Tables B21 and B22 show that these results are robust to dropping respondents who may not have fully understood the context or the details of some of the games and to dropping outliers and observations on the boundaries. Table B23 confirms that our main results hold separately by survey rounds.

Overall, the results in table 6 show fairly consistent positive effects on beliefs concerning state courts and negative indirect effects on beliefs for the competing nonstate actor, the panchayats. These patterns are in line with the motivated reasoning interpretation (as well as with the Bayesian approach with negatively affiliated priors, which we discuss further below). As we emphasized in the introduction, they are also in line with the interpretation that part of the reason why rural Pakistanis turn to and start trusting nonstate actors may be the pervasive inefficiencies of state institutions, possibly generating a vicious cycle.

C. Evidence against Mechanical Contrast Effects

In this subsection, we provide two pieces of evidence against contrast effects accounting for our results. First, in a subset of our first-round sample, comprising about 800 individuals, we included pre- and postgame usage and belief questions for an additional actor: state hospitals. While we did not play the two experimental games with this actor, our reported usage and service effectiveness measures, reported in table B24, show small positive effects on hospitals, with no discernible impact on the access or trust measures. Thus, rather than negative contrast effects, our evidence points to some of the positive effects on state courts spilling over to state hospitals.

[28] One possibility is that our respondents expect others to switch away from panchayats to state courts, as our results in table B9 demonstrate, and thus also surmise that accessing panchayats would become even easier, counterbalancing any other negative effects for panchayat access. We additionally asked questions about "allegiance"—specifically, about the importance of building a relationship with the relevant forum. Results reported in table B20 indicate that after receiving the state positive treatment, respondents feel less need to build a relationship with the state (presumably because the state is working more effectively and so personal connections are no longer as necessary), and therefore they exert less effort in building a relationship with state courts. Interestingly, while respondents generally believe that relationships are important for panchayats, there is little change in this perceived importance after our informational treatment. Once again consistent with motivated reasoning considerations, we estimate negative and significant indirect effects on the need to exert effort to build allegiance toward panchayats.

Our hospital results are not from high-stakes game allocations, however. In addition, because hospitals are added as a third actor, the nature of contrast effects may be different. To rectify these problems and more directly test for contrast effects, in the second round we went back to the field and identified a context-relevant and relatively neutral actor: local sports clubs. We then ran a modified game that was identical to our original game setup with the one exception that we replaced panchayats with local sports clubs as the second actor. The script for the local sports clubs analogously asks our respondents to consider contributing to or investing in local sports clubs. These games were played with 250 randomly chosen respondents.

If our negative indirect estimates were driven by contrast effects, we should find similarly sized negative impacts on local sports clubs as we did for panchayats. The results presented in table 7 show no evidence of such contrast effects. We continue to find statistically significant and precise direct effects on the state actor and much smaller and sometimes significant positive effects on local sports clubs. For example, the coefficient estimate for the fund dictator game is 3.60 (SE = 2.25), while the estimate for the investment game is 5.20 (SE = 2.11). Because these positive indirect estimates are much smaller than (about 10%–14% of the magnitude of) the direct estimates, in panel C we find large and robust differential positive effects on expected usage and game allocations for the state courts relative to local sports clubs.

In hindsight, the small positive indirect effects on local sports clubs should not be surprising. A more effective state may enable even unaffiliated local sports clubs to work more efficiently. Indeed, while all of the respondents in our focus groups in eight villages confirmed that they viewed local sports clubs as neutral nonstate actors, a few noted that these sports clubs would be more effective if state-controlled sports boards, such as the Pakistan Cricket Board, were run better. Consistent with this interpretation, our belief questions show some increases in perceived effectiveness of and trust in local sports clubs following our informational treatment on reduced delays at state courts (see cols. 1 and 2 in table B25). Overall, these results suggest that negative indirect effects are confined to nonstate actors that are competing with state courts and cannot be explained by mechanical contrast effects.

D. Heterogeneous Effects

Bayesian models make additional predictions that can be investigated by studying heterogeneous effects.[29] We focus on two predictions (for details,

[29] We were also interested in other types of heterogeneous effects—e.g., depending on caste status and socioeconomic characteristics. Even though these characteristics predict initial expected usage and game allocations, they are not associated with any heterogeneous responses to the informational treatment.

TABLE 7
EFFECT OF STATE POSITIVE TREATMENT WITH LOCAL SPORTS CLUBS

	Expected Usage (1)	Allocation in Fund Dictator Game (2)	Allocation in Investment Game (3)
A. Direct effects (on state courts):			
Posttreatment	1.94	35.88	30.16
	(.12)	(2.86)	(2.96)
Constant	3.87	101.16	110.52
	(.09)	(2.02)	(2.09)
Observations (respondents)	500 (250)	500 (250)	500 (250)
B. Indirect effects (on sports clubs):			
Posttreatment	.21	3.60	5.20
	(.06)	(2.25)	(2.11)
Constant	6.56	133.16	134.92
	(.04)	(1.59)	(1.49)
Observations (respondents)	500 (250)	500 (250)	500 (250)
C. Netting out indirect effects:			
Posttreatment	.21	3.60	5.20
	(.18)	(3.93)	(3.60)
State	−2.70	−32.00	−24.40
	(.18)	(3.93)	(3.60)
Post × state	1.73	32.28	24.96
	(.25)	(5.55)	(5.09)
Constant	6.56	133.16	134.92
	(.12)	(2.78)	(2.54)
Observations (respondents)	1,000 (250)	1,000 (250)	1,000 (250)

NOTE.—This table estimates the direct and indirect effects of the state positive treatment on expected usage and game allocations in a design that replaces panchayats with local sports clubs. "Posttreatment" is a dummy for posttreatment observations. For the definitions of "Expected Usage," "Allocation in Fund Dictator Game," and "Allocation in Investment Game," see the notes to tables 1 and 2. Panel A reports effects on state courts, panel B reports (indirect) effects on sports clubs, and panel C nets out the indirect effects (and thus includes four observations per respondent, two from their responses for state courts and two from their responses for local sports clubs). All regressions include individual fixed effects. The constants in panels A and B represent the pretreatment values of the relevant variables. Standard errors are reported in parentheses.

see app. A). First, under Bayesian updating, agents who are very sure about the quality of the state or the nonstate actors should not update their beliefs much (whatever the source of the new information). This implies a nonmonotonic relationship between initial beliefs and updates and changes in behavior. Second, when priors are negatively affiliated, the effects of initial beliefs about one of the actors should impact the direct and indirect effects symmetrically (e.g., if more favorable initial beliefs about the state lead to smaller direct effects, then they should have also led to smaller negative

indirect effects). We seek to investigate both of these predictions by estimating the following generalization of (1):

$$\begin{aligned} Y_{it} = {} & \alpha + \beta \text{Post}_i + \psi_1^{\text{state}} \text{InitialBelief}_i^{\text{state}} \cdot \text{Post}_t + \psi_2^{\text{state}} (\text{InitialBelief}_i^{\text{state}})^2 \cdot \text{Post}_t \\ & + \psi_1^{\text{nonstate}} \text{InitialBelief}_i^{\text{nonstate}} \cdot \text{Post}_t + \psi_1^{\text{nonstate}} (\text{InitialBelief}_i^{\text{nonstate}})^2 \\ & \cdot \text{Post}_t + \delta_i + \varepsilon_{it}, \end{aligned} \tag{4}$$

where $\text{InitialBelief}_i^{\text{state}}$ and $\text{InitialBelief}_i^{\text{nonstate}}$ measure individual i's initial beliefs about the state and the nonstate actors. This equation therefore allows the effects of the state positive treatment to be different by the baseline beliefs of individuals concerning the state and the nonstate actor. Moreover, the square terms in initial beliefs introduce the possibility that these effects are nonmonotonic. We estimate this equation using baseline expected usage to proxy for initial beliefs. Appendix A provides a formal justification of using initial usage to proxy for initial belief. The results are reported in table 8 (where we normalize the proxy for initial beliefs on the right-hand side to lie between zero and one for ease of inspection). As usual, panel A is for direct effects and panel B presents indirect effects. Tables B29–B32 show that when we use service effectiveness, enforcement effectiveness, access, and trust variables to proxy initial beliefs, the results are similar but somewhat less precise given the smaller sample sizes.

Columns 1–3 do not include the quadratic terms and thus focus primarily on the second prediction mentioned above—about symmetric effects of initial beliefs for the state and the nonstate actors. This in particular implies that if $\psi_1^{\text{state}} < 0$, then $\psi_1^{\text{nonstate}} > 0$, so that smaller positive direct effects should go together with smaller negative indirect effects. The results in these three columns show some interesting heterogeneities: higher (more favorable) initial beliefs for state courts translate into smaller positive direct effects on expected usage and the allocations in the two games following our informational treatment. Similarly, initial beliefs about panchayats impact the magnitude of the negative indirect effects for this actor. Bayesian models do not make tight predictions about the direction of these effects. Rather, they suggest that these impacts should be nonmonotonic (as we explore next) and that, when priors are negatively affiliated, the interactions between initial beliefs and the post dummy should have opposite signs for the state and the nonstate actors. The results in table 8 do not support this second prediction. For example, initial beliefs about both state courts and panchayats have negative, albeit not always significant, effects in both panels.

Columns 4–6 investigate the first prediction mentioned above, related to nonmonotonic effects. The results show some evidence for nonmonotonicities but are not uniformly consistent with fully Bayesian models. In

TABLE 8
HETEROGENEITY BY PRIORS

	Model 1			Model 2		
	Expected Usage (1)	Allocation in Fund Dictator Game (2)	Allocation in Investment Game (3)	Expected Usage (4)	Allocation in Fund Dictator Game (5)	Allocation in Investment Game (6)
A. Direct effects (on state courts):						
Posttreatment	1.79	26.46	21.45	1.45	16.23	13.72
	(.13)	(3.78)	(4.12)	(.14)	(4.14)	(4.47)
Post × initial state	−1.79	−11.56	−4.82	−1.36	15.28	27.91
	(.13)	(3.80)	(4.10)	(.43)	(12.38)	(13.54)
Post × initial state^2				−.49	−29.43	−35.40
				(.43)	(12.42)	(13.50)
Post × initial nonstate	−.15	−2.16	−3.52	1.87	49.26	29.42
	(.13)	(3.84)	(4.17)	(.50)	(14.38)	(15.60)
Post × initial nonstate^2				−1.86	−47.30	−30.37
				(.44)	(12.78)	(13.97)
Constant	4.07	100.41	113.04	4.07	100.41	113.04
	(.03)	(.77)	(.84)	(.03)	(.76)	(.84)
Observations (respondents)	5,056 (2,528)	5,056 (2,528)	4,558 (2,279)	5,056 (2,528)	5,056 (2,528)	4,558 (2,279)

B. Indirect effects (on panchayats):						
Posttreatment	.27	−11.49	−6.41	.43	−5.51	−2.90
	(.12)	(3.86)	(4.06)	(.13)	(4.24)	(4.42)
Post × initial state	−.13	−.00	−2.61	−.13	13.40	14.73
	(.12)	(3.88)	(4.04)	(.39)	(12.68)	(13.39)
Post × initial state^2				.01	−13.39	−17.50
				(.39)	(12.72)	(13.35)
Post × initial nonstate	−1.24	−2.95	−8.69	−2.34	−53.50	−47.11
	(.12)	(3.92)	(4.11)	(.46)	(14.73)	(15.42)
Post × initial nonstate^2				1.01	46.57	35.69
				(.40)	(13.09)	(13.81)
Constant	6.53	103.99	115.29	6.53	103.99	115.29
	(.02)	(.78)	(.83)	(.02)	(.78)	(.83)
Observations (respondents)	5,054 (2,527)	5,056 (2,528)	4,558 (2,279)	5,054 (2,527)	5,056 (2,528)	4,558 (2,279)

NOTE.—This table estimates heterogeneous direct and indirect effects by priors. We proxy priors by pretreatment expected usage for the relevant forum. "Posttreatment" is a dummy for posttreatment observations. For the definitions of "Expected Usage," "Allocation in Fund Dictator Game," and "Allocation in Investment Game," see the notes to tables 1 and 2. Model 1 in cols. 1–3 includes only linear interactions, while model 2 in cols. 4–6 additionally includes quadratic interactions. Panel A reports effects on state courts, while panel B reports (indirect) effects on panchayats. All regressions include individual fixed effects. Standard errors are reported in parentheses.

particular, the square of initial beliefs about state courts is negative and significant in the allocations to the state actor in both the fund dictator and the investment games but not for expected usage of state courts. The estimated pattern is monotonic for expected usage and the fund dictator game but nonmonotonic for the investment game. The nonmonotonicity is consistent with Bayesian predictions—when initial beliefs are close to their minimum value, the impact of the informational treatment is positive, and when they are close to their maximum value, the impact is negative. In contrast, the impact on expected usage and game allocations to panchayats is monotonic as a function of initial beliefs about panchayats; even though the quadratic is significant, it simply makes the relationship convex, without introducing any nonmonotonicity. In fact, the negative effects are stronger when initial beliefs are close to their minimum value, which again contrasts with the predictions of the Bayesian model.

Overall, we find interesting heterogeneities depending on initial beliefs (or our proxies thereof). These heterogeneous effects are suggestive about some type of belief updating—for example, indicating that individuals who already trust state courts do not update and change their behavior much following additional positive information about them. Nevertheless, our results are inconsistent with two of the major predictions of a fully Bayesian model. As such, they shed doubt on a purely Bayesian explanation for the findings we have reported so far.[30]

VI. Conclusion and Future Directions

Lack of trust in state institutions, which discourages citizens from seeking services from and interacting with these institutions, is a pervasive problem in many developing countries. Though this lack of trust is often well grounded in evidence of weakness, ineffectiveness, and corruption of state institutions, it exacerbates these problems by encouraging citizens to disengage with the state. It also drives them to seek protection, conflict resolution, and public services from nonstate actors, further undermining trust in the state. This feedback process might even be argued to create a vicious circle of weakness and illegitimacy: low effectiveness of state institutions reduces people's interactions with and trust in them while simultaneously increasing their use and trust in competing nonstate actors, and the more they turn to these nonstate actors, the more people trust them and the less they work with and trust state institutions. Despite the ubiquity of these issues in many developing societies, there is relatively little evidence about whether this lack of trust is real, what its implications

[30] Though our motivated reasoning model also has a Bayesian component, because beliefs are choice variables, it does not place the same structure on initial beliefs and subsequent responses.

are, and how the interplay between state and nonstate actors contributes to it.

Our paper provides a first investigation of these issues. We focus on rural Punjab in Pakistan, where there is endemic lack of trust in state institutions and especially in state courts. Nevertheless, truthful information favorable to state courts makes our respondents more willing to trust in and work with these courts. This translates into fairly large and very robust positive direct effects for state institutions both in our respondents' self-reported expected usage and in allocations in our high-stakes lab-in-the-field games. These results indicate that, despite the deep-rooted mistrust of the Pakistani state, truthful (credible) information can change people's beliefs and behavior.

More interestingly for our hypothesis of feedbacks between state ineffectiveness and success of nonstate actors, we estimate large and robust indirect effects on competing nonstate actors, in this case panchayats. Namely, after the same state positive informational treatment, our respondents report that they are less likely to use nonstate institutions for dispute resolution and allocate fewer resources to them in our two games. We verify that these results are not driven by mechanical contrast effects and cannot be explained by social experimenter concerns or lack of anonymity.

Our survey questions and additional games enable us to dig deeper into the mechanisms for these direct and indirect effects. We find that the state positive treatment improves beliefs about state courts. More importantly, it makes them more pessimistic about the effectiveness of nonstate institutions and reduces their trust in these competitors of the state.

We argue that our evidence can be best understood from the viewpoint of motivated reasoning, where reduced usage of nonstate institutions makes people less likely to hold positive views about them. In terms of the larger motivation of our project, these patterns, and especially the indirect effects on panchayats, provide some evidence for one aspect of the negative feedback cycle—whereby positive views of nonstate institutions are fed by the ineffectiveness of state institutions and vice versa—but also suggest that this negative feedback can be reversed if and when state institutions become more effective and when credible information about their improved performance is provided to citizens.

In ongoing work, we are pursuing this agenda further by focusing on populations that have recently experienced a dispute and are at the initial stages of considering which (state or nonstate) forum to utilize. In addition to providing such individuals with analogous informational treatments about improvements in state services, we are exploring ways to better provide such services to them. The goal is not only to measure self-reported and game-based outcomes but also to follow these individuals over time and examine which forum they choose to engage with and in what manner. This will allow us to study whether the results documented in this paper

continue to operate over extended periods of time by impacting decisions people make when dealing with the actual disputes they are facing.

We view our paper as a first step in a systematic investigation of the interplay between state and nonstate institutions and citizens' trust in these institutions. Improving public services and building state capacity, badly lacking in many parts of the world, often necessitates cultivating trust in state institutions. Our work suggests that the competition between state and nonstate actors is critical in this process, and there may be feedback effects both between the quality of public services and trust in state institutions and between trust in state and nonstate actors that may be leveraged in reconstructing state capacity.

Appendix A

Theory

Here we provide formal models of Bayesian updating and motivated reasoning and derive their implications for behaviors, game allocations, and beliefs.

A1. Setup

We consider a setting in which two actors—one state and one nonstate—offer services to an individual. Crucially, the individual is uncertain about the quality and effectiveness of these two actors. For simplicity, we collapse this uncertainty into two binary variables representing the qualities of the two actors, $\theta_S \in \{0, 1\}$ and $\theta_N \in \{0, 1\}$. We denote the prior beliefs about these two quality variables by $\pi_S^0 = \mathbb{E}[\theta_S|\text{PriorInformation}]$ and $\pi_N^0 = \mathbb{E}[\theta_N|\text{PriorInformation}]$. Our experimental treatment is to provide (some of) the respondents with relevant information (signal σ_S) about the quality of the state actor.[31] We denote the beliefs of the individual after he receives this information by $\pi_S = \mathbb{E}[\theta_S|\text{PriorInformation}, \sigma_S]$ and $\pi_N = \mathbb{E}[\theta_N|\text{PriorInformation}, \sigma_S]$.

We also assume that each individual has the following expected utility function:

$$u(c) + \pi_S g_S + \pi_N g_N, \tag{A1}$$

where c denotes cash in hand ("consumption") and g_S and g_N denote services provided by the state and the nonstate actor, respectively. These terms are multiplied by π_S and π_N to capture the fact that the quality of the relevant actor matters for utility (or effectiveness of services). The function $u(\cdot)$ is an increasing and weakly concave utility function measuring benefits from cash in hand (which the individual can consume or put to other uses). We will consider this utility function to be either linear, in which case we have overall "quasi-linear" utility without income effects, or strictly concave, in which case there will be income effects.

[31] As we mentioned in the main text, in our pilot experiments we also gave information about the nonstate actor, but here we focus on our main treatment and sample for which the information provided concerned the effectiveness of the state actor.

In mapping this expected utility function to our experimental setting, we interpret the terms with g_S and g_N as incorporating not only what the individual himself is receiving but also the services to the entire community that the individual may care about (this is relevant in the context of our fund dictator game, where our respondents make contributions for other people's benefit). Relatedly, in the context of the investment game, these terms may be interpreted as additional income resulting from the individual's investment in the court case of another plaintiff.

In our baseline, $u(\cdot)$ is linear. We view this as the most appropriate specification for our setting for at least two reasons. First, our experimental design clarifies that our respondents will be paid for only one of the games they are playing, so we expect only limited across-game effects working through the marginal utility of consumption. In particular, conditional on one of the games being chosen for payment, allocations in the other games have no impact on the subject's take-home amount and thus should not generate any income effects on their choices. Second, one of our games—the investment game—focuses on investing resources with potential returns, so income effects should not be present in this context. These justifications notwithstanding, we allow for strict concavity of this utility function for generality and for elucidating how our main results and interpretation apply in this case.

The individual starts with income y and has to decide how much to contribute to the state and the nonstate actor, denoted by T_S and T_N, respectively. In reality, these terms include taxes (which individuals can try to evade by taking costly actions) and voluntary contributions. In our games, individuals have an explicit decision to make about these contributions. We assume that these contributions affect the amount of services that the individual receives via the following relationships:

$$g_S = h_S(T_S) \text{ and } g_N = h_N(T_N),$$

where $h_S(\cdot)$ and $h_N(\cdot)$ are continuously differentiable, increasing, and concave functions.

Because, as mentioned above, our respondents will be paid for only one of the games, we do not impose a budget constraint linking state and nonstate allocations in our setting and simply write the cash in hand (consumption) of the individual as

$$c = y - T_N - T_S \tag{A2}$$

(without a nonnegativity constraint on c).

Putting all these together and taking his beliefs as given, the individual's maximization problem is

$$U(\pi_S, \pi_N) = \max_{T_S, T_N \geq 0} u(y - T_N - T_S) + \pi_S h_S(T_S) + \pi_N h_N(T_N). \tag{A3}$$

For future reference, this equation also defines $U(\pi_S, \pi_N)$ as the (maximized) expected utility level of the individual as a function of his beliefs.[32]

[32] We also note at this point that the solution of this problem links allocations to beliefs in a simple manner. For example, the first-order condition for T_S immediately yields $T_S = (h_S')^{-1}(u'(c)/\pi_S)$, and $(h_s')^{-1}(\cdot)$ is the inverse function of the derivative of h_S and is decreasing in view of h_S being concave. Hence, there is a one-to-one relationship between beliefs and initial allocations, which we use in our analysis in sec. V.D.

Before analyzing the solution to this maximization problem and how it changes with information about the effectiveness of the state actor, we discuss the individual's belief update. We start with the benchmark Bayesian updating model.

A2. Bayesian Updating

Consider how the provision of signal σ_S affects the individual's beliefs under Bayesian updating. Following this signal, the Bayesian posterior about the state actor, denoted by π_S^B, can be computed as

$$\pi_S^B = \frac{\Pr[\sigma_S|\theta_S = 1]\pi_S^0}{\Pr[\sigma_S|\theta_S = 1]\pi_S^0 + \Pr[\sigma_S|\theta_S = 0](1 - \pi_S^0)}, \tag{A4}$$

where $\Pr[\sigma_S|\theta_S]$ denotes the probability of receiving signal σ_S conditional on the value of θ_S. Since the signal in our experimental treatment is designed to be good news about the quality of the state actor, we assume that $\Pr[\sigma_S|\theta_S = 1] > \Pr[\sigma_S|\theta_S = 0]$. The Bayesian updating formula, (A4), then yields

$$\pi_S^B > \pi_S^0.$$

What about π_N? This depends on whether the signal σ_S is directly informative about the nonstate actor (our experimental setup is specifically designed to rule this out) and whether the priors about the state and the nonstate actors are independent. When the two priors are independent and the signal is not informative about the nonstate actor, we have $\Pr[\sigma_S|\theta_N = 1] = \Pr[\sigma_S|\theta_N = 0]$ (see the next section for details). If this is indeed the case, the Bayesian posterior, π_N^B, about the nonstate actor satisfies

$$\pi_N^B = \frac{\Pr[\sigma_S|\theta_N = 1]\pi_N^0}{\Pr[\sigma_S|\theta_N = 1]\pi_N^0 + \Pr[\sigma_S|\theta_N = 0](1 - \pi_N^0)} = \pi_N^0. \tag{A5}$$

We next discuss the case in which π_S^0 and π_N^0 are jointly distributed and are negatively affiliated (e.g., they could be jointly normally distributed with a negative covariance term). In this case, we will see that Bayesian updating implies that $\pi_N^B < \pi_N^0$. We first introduce the notion of negative affiliation between priors in our context and in the process clarify the conditions for the signal not to be directly informative about the nonstate actor. Let $p_{11} = \Pr[\sigma_S|\theta_S = 1, \theta_N = 1]$, $p_{10} = \Pr[\sigma_S|\theta_S = 1, \theta_N = 0]$, $p_{01} = \Pr[\sigma_S|\theta_S = 0, \theta_N = 1]$, and $p_{00} = \Pr[\sigma_S|\theta_S = 0, \theta_N = 0]$. The scenario we are interested in is one in which σ_S is directly informative only about the quality of the state actor, and hence we assume that

$$p_{11} = p_{10} = p_1 > p_{01} = p_{00} = p_0. \tag{A6}$$

When this is not the case, the informational treatment will directly impact beliefs about the nonstate actor.[33] For reasons explained in the main text, throughout we focus on the case where (A6) holds. In addition, the inequality in (A6) simply repeats our assumption that σ_S is good news about the state actor's quality.

[33] For example, if $p_{11} < p_{10}$ and $p_{00} < p_{01}$, then we immediately have $\pi_N^B < \pi_N^0$.

The question is about the association between the priors concerning the state and nonstate actors. Let $\pi_{11} = \Pr[\theta_S = 1, \theta_N = 1]$, $\pi_{10} = \Pr[\theta_S = 1, \theta_N = 0]$, $\pi_{01} = \Pr[\theta_S = 0, \theta_N = 1]$, and $\pi_{00} = \Pr[\theta_S = 0, \theta_N = 0]$. Recall as well that $\pi_S^0 = \Pr[\theta_S = 1] = \pi_{11} + \pi_{10}$ and $\pi_N^0 = \Pr[\theta_N = 1] = \pi_{11} + \pi_{01}$. We say that priors about the state and nonstate actors are independent if $\pi_{11} = \pi_S^0 \cdot \pi_N^0$, $\pi_{10} = \pi_S^0 \cdot (1 - \pi_N^0)$, $\pi_{01} = (1 - \pi_S^0) \cdot \pi_N^0$, and $\pi_{00} = (1 - \pi_S^0) \cdot (1 - \pi_N^0)$. Independence, together with (A6), implies that

$$\begin{aligned}\pi_N^B &= \frac{p_{11}\pi_{11} + p_{01}\pi_{01}}{p_{11}\pi_{11} + p_{01}\pi_{01} + p_{10}\pi_{10} + p_{00}\pi_{00}} \\ &= \frac{p_1\pi_S^0\pi_N^0 + p_0(1 - \pi_S^0)\pi_N^0}{p_1\pi_S^0 + p_0(1 - \pi_S^0)} = \pi_N^0,\end{aligned}$$

confirming (A5).

Alternatively, we say that priors about the state and nonstate actors are negatively affiliated if $\pi_{11} \leq \pi_S^0 \cdot \pi_N^0$, $\pi_{10} \geq \pi_S^0 \cdot (1 - \pi_N^0)$, $\pi_{01} \geq (1 - \pi_S^0) \cdot \pi_N^0$, and $\pi_{00} \leq (1 - \pi_S^0) \cdot (1 - \pi_N^0)$. In other words, priors are such that when the state is high quality, the nonstate actor is less likely to be high quality, and vice versa.

A3. Basic Implications of Bayesian Updating

We now study the implications of the signal σ_S on beliefs and behavior under Bayesian updating.

Proposition A1. Suppose that the individual is Bayesian and that signal σ_S is directly informative only about the state actor (i.e., [A6] holds).

i. Suppose in addition that preferences are quasi linear ($u(\cdot)$ is linear) and priors for the state and nonstate actors are independent. Then the provision of signal σ_S (which is good news for θ_S and not directly relevant for θ_N) weakly increases π_S and T_S and has no effect on π_N and T_N.
ii. Suppose instead that preferences are not quasi linear ($u(\cdot)$ is strictly concave) but priors for the state and nonstate actors are still independent. Then the provision of signal σ_S weakly increases π_S and T_S and weakly reduces T_N but has no effect on π_N.
iii. If priors for the state and the nonstate actors are negatively affiliated, then the provision of signal σ_S weakly increases π_S and T_S and weakly reduces π_N and T_N.

Proof. (i) Recall that in this case, $\pi_S^B > \pi_S^0$ and $\pi_N^B = \pi_N^0$ as a result of Bayesian updating as in (A5). In this case, (A3) can be written as

$$\max_{T_S, T_N} y - T_S - T_N + \pi_S h_S(T_S) + \pi_N h_N(T_N).$$

Now it is straightforward to see that this problem is supermodular in (T_S, π_S) and also separable between this vector and (T_N, π_N). This implies that the optimal values of the control variables for the individual can be written as $T_S^*(\pi_S)$ and $T_N^*(\pi_N)$, where each one of these functions is (weakly) increasing. Since π_S increases and π_N remains constant, the claims in the proposition follow immediately. In particular, T_S (weakly) increases, and T_N does not change. Moreover, if T_S is interior, the change is strict.

(ii) This proof is very similar to part i, except that a strictly concave $u(\cdot)$ implies that the problem is no longer separable and optimal choices are functions of beliefs regarding both the state and the nonstate actors: $T_S^*(\pi_S, \pi_N)$ and $T_N^*(\pi_S, \pi_N)$, and because the utility cost of transfers is greater when more is contributed to the state actor, $T_N^*(\pi_S, \pi_N)$ is (weakly) decreasing in π_S. However, because priors are independent, (A5) applies.

(iii) In this case, because priors are negatively affiliated, (A5) does not apply and instead we have

$$\begin{aligned}
\pi_N^B - \pi_N^0 &= \frac{p_{11}\pi_{11} + p_{01}\pi_{01}}{p_{11}\pi_{11} + p_{01}\pi_{01} + p_{10}\pi_{10} + p_{00}\pi_{00}} - \pi_N^0 \\
&= \frac{p_1\pi_{11} + p_0\pi_{01}}{p_1\pi_S^0 + p_0(1 - \pi_S^0)} - \pi_N^0 \\
&= \frac{p_1(\pi_{11} - \pi_S^0\pi_N^0) + p_0(\pi_{01} - (1 - \pi_S^0)\pi_N^0)}{p_1\pi_S^0 + p_0(1 - \pi_S^0)} \\
&= \frac{(p_1 - p_0)(\pi_{11} - \pi_S^0\pi_N^0)}{p_1\pi_S^0 + p_0(1 - \pi_S^0)} \\
&< 0,
\end{aligned}$$

where the second line follows from (A6), the third line puts the terms in the second line to a common denominator, the fourth line uses the fact that $\pi_{01} = \pi_N^0 - \pi_{11}$, and finally, the last line is a consequence of negative affiliation and the fact that $p_1 - p_0 > 0$. As a consequence, with Bayesian updating with negatively affiliated priors, we have $\pi_N^B < \pi_N^0$. Given this, regardless of whether we have quasi-linear preferences, $T_N^*(\pi_S, \pi_N)$ is a (weakly) decreasing function of π_N. This completes the proof of the proposition. QED

Let us start with part i. Note that the positive signal σ_S naturally increases π_S. These improved beliefs about the quality of state institutions make the state a better forum for the individual, who is then induced to increase T_S, unless this was at the corner solution of zero. In this Bayesian baseline, given the nature of the signal σ_S, there is no impact on the beliefs concerning the nonstate actor, π_N. In addition, given the quasi-linear preferences, allocations to the nonstate actor are unaffected.

To understand part ii, first suppose that the utility function $u(\cdot)$ is strictly concave. Then increased contributions to the state would raise the marginal utility of consumption, encouraging a decline in contributions to the nonstate actor, T_N. However, there would continue to be no impact on π_N (recall that priors are independent in this case). Moreover, as noted above, this mechanism should not apply to the allocations in the investment game because the amount allocated represents an investment made by the respondent based on what he believes the expected effectiveness of the relevant actor is.

Finally, part iii is intuitive as well. In this case, the Bayesian agents are convinced that the qualities of the state and nonstate institutions are negatively affiliated—when one is bad, the other one is likely to be good, and vice versa. If so, even though our state positive treatment is not directly informative about panchayats, it will be perceived as being informative about them. In this case, our state positive

treatment reduces π_N and as a result will lead to lower allocations toward the now less favorably viewed panchayats.

We also note two additional properties of Bayesian updating, which are relevant for our discussion in section V.D. First, equations (A4) and (A5) make it clear that if priors are close to zero or one, there will not be much updating, and hence changes in beliefs and economic responses should be nonmonotonic in initial beliefs.[34] Second, since priors are negatively affiliated, the impacts of the priors on the absolute value of the direct effects should be in the same direction as their impacts on the absolute value of the indirect effects. For example, if a higher π_S^0 leads to a smaller (positive) direct effect, it should also lead to a smaller (negative) indirect effect.[35]

In summary, the Bayesian model suggests that in the most plausible scenario, where utility is quasi linear (because, as explained above, the amounts are small and our respondents understand that they will receive only one payment), there should be no indirect spillover effects on panchayats. If there are income effects (strictly concave utility), allocations to panchayats may be affected, but there should still be no impact on beliefs about panchayats unless our agents are convinced that the quality and effectiveness of panchayats and state courts are negatively affiliated. As the main text has documented, we find very robust negative indirect effects on panchayats. Our preferred interpretation will be based on a model of motivated reasoning, which we present in the next section.

Before presenting this model, we explain why we do not find the model of Bayesian updating with negatively affiliated priors to be plausible. First, our fieldwork revealed no apparent reasons to suspect that this was important in practice. During our pilots, we ensured that the state positive treatment was worded carefully to prevent such a misunderstanding. Subsequent interviews with respondents indicated that they understood that the two forums were separate and that they did not equate improvements in one with changes in the other. Second, to the extent that such negative correlation can be microfounded in our setting, it would be based on an assumption related to motivated reasoning—somehow, agents would convince themselves that only one of the two competing actors could be good, so they could trust one and only one of these actors. Finally,

[34] For example, under (A6), we can write $\pi_S^B - \pi_S^0 = \{(p_1 - p_0)[(\pi_{11} + \pi_{10}) - (\pi_{11} + \pi_{10})^2]\}/[p_1(\pi_{11} + \pi_{10}) + p_0(1 - \pi_{11} + \pi_{10})]$, which is zero when $\pi_S^0 = \pi_{11} + \pi_{10}$ is equal to zero or one.

[35] To see this, take the case where a higher π_S^0 leads to a smaller direct effect. Then, from n. 34, this implies that $\pi_S^0 = \pi_{11} + \pi_{10} > 1/2$. Note the following: $\Delta = (p_1\pi_{11} + p_0\pi_{01})/[p_1(\pi_{11} + \pi_{10}) + p_0(1 - \pi_{11} - \pi_{10})] - (\pi_{11} + \pi_{01}) = \{(p_1 - p_0)[\pi_{11} - (\pi_{11} + \pi_{01})(\pi_{11} + \pi_{00})]\}/[p_1(\pi_{11} + \pi_{10}) + p_0(1 - \pi_{11} - \pi_{10})]$. Next, using the fact that $\pi_{11} + \pi_{01} = 1 - \pi_{00} - \pi_{10}$, this expression can be written as $\Delta = \{(p_1 - p_0)[-\pi_{10} + (\pi_{11} + \pi_{10})(\pi_{10} + \pi_{00})]\}/[p_1(\pi_{11} + \pi_{10}) + p_0(1 - \pi_{11} - \pi_{10})]$. Taking an equally weighted average of these two expressions for Δ and adding and subtracting from the numerator $2\pi_{10}(1 - \pi_{11} - \pi_{10})$ and $2\pi_{01}(\pi_{11} + \pi_{10})$, we have

$$\frac{(p_1 - p_0)\left[\begin{array}{c}(\pi_{11} + \pi_{10})(1 - \pi_{11} - \pi_{10}) + (\pi_{11} + \pi_{10})(1 - \pi_{11} - \pi_{10}) \\ -\pi_{10}(1 - \pi_{11} - \pi_{10}) - \pi_{01})(\pi_{11} + \pi_{10})\end{array}\right]}{p_1(\pi_{11} + \pi_{10}) + p_0(1 - \pi_{11} - \pi_{10})}.$$

Now, holding $\pi_{01} = \pi_{10}$, consider an increase in π_S. We can then conclude that a higher π_S^0 (when it is greater than 1/2) should lead to a smaller (more positive) indirect effect.

as just noted, the Bayesian model makes additional predictions related to the magnitudes of responses of direct and indirect effects depending on priors. We show in section V.D that these predictions do not receive support in our data.

Instead of these possibilities, we argue that a model of motivated reasoning, whereby individuals manipulate their own beliefs to be in line with their actions or expected actions, provides a better match to the ideas discussed in the introduction and to our setting, and it also crucially implies a change in beliefs concerning the nonstate actor—a pattern we document in the data. We next present such a model.

A4. Motivated Reasoning

We now present a simple variation on the utility function in (A1) that incorporates motivated reasoning.[36] To isolate the effects of motivated reasoning, we develop this model when preferences are quasi linear and there is no negative affiliation in the priors concerning the quality of the state and nonstate actors—so that in the absence of motivated reasoning, there will be no indirect effect.

In our setting, motivated reasoning is relevant in part because when choosing a particular actor as their service provider, individuals may have greater reason to convince themselves that this actor is providing high-quality services (and perhaps that it is honest, well-meaning, and effective). More formally, under motivated reasoning, individuals choose not only their behavior but also their beliefs and will do so to make their beliefs more congruent with their behavior. A simple way of modeling motivated reasoning is therefore to allow a simultaneous choice over behavior and beliefs with a penalty for deviations of these beliefs from the Bayesian benchmark, as in the next optimization problem:

$$\max_{c,\{T_i,\pi_i\}_{i\in\{S,N\}}} c + \pi_S h_S(T_S) - d[U(\pi_S, \pi_N) - U(\pi_S^B, \pi_N^B)], \tag{A7}$$

again subject to (A2). Note that, compared to (A3), there are now two important differences. First, there is an explicit choice over beliefs, π_S and π_N. Because these beliefs multiply $h_S(T_S)$ and $h_N(T_N)$, this choice creates a force toward beliefs that are more favorable toward the actor that the individual is using and investing in. Second, the second line introduces a penalty for the deviation of these beliefs from their Bayesian counterparts, π_S^B and π_N^B. In particular, here $d[\cdot]$ is a convex function that is increasing when its argument is positive and decreasing when it is negative, and we also assume that it is differentiable with $d'[0] = 0$. This penalty is in terms of the difference between the (actual) utility level $U(\pi_S, \pi_N)$ as defined in (A3) when the individual's beliefs are π_S and π_N, and his utility under Bayesian beliefs, $U(\pi_S^B, \pi_N^B)$, which is the maximum utility he can achieve. Convexity implies that the penalty for further deviations is greater when $U(\pi_S, \pi_N)$ is farther apart from $U(\pi_S^B, \pi_N^B)$.[37] Overall, this objective function captures in a simple way the trade-off between the direct utility of congruence of beliefs

[36] This model builds on the works cited in n. 4 but develops a somewhat more general (even if reduced-form) model of motivated reasoning, which elucidates the forces at work in our environment.

[37] Without this penalty term, the individual would have an incentive to choose the highest possible values of π_S and π_N.

and actions versus the cost of manipulating beliefs in terms of departures from the best ex post decisions (which would give utility $U(\pi_S^B, \pi_N^B)$). Note also that in this maximization problem beliefs and behavior are simultaneously chosen, so it is the expectation of decisions that the individual will make in the future—not necessarily past behaviors—that are influencing his beliefs.

The following proposition describes the results from our simple conceptual framework in the presence of motivated reasoning.

PROPOSITION A2. Suppose that the individual engages in motivated reasoning. Suppose also that there is a unique solution to the individual's maximization problem. Then the provision of information σ_S (which is good news for θ_S and not directly relevant for θ_N) (weakly) increases π_S and T_S and (weakly) decreases π_N and T_N.

Proof. Consider the same maximization problem as in the proof of proposition A1, which defines

$$U(\pi_S, \pi_N) = y - T_S^*(\pi_S) - T_N^*(\pi_N) - h_S(T_S^*(\pi_S)) - h_N(T_N^*(\pi_N)).$$

Now, substituting this into (A7), we obtain an objective function that is supermodular in $(T_S, \pi_S, \pi_S^B, -T_N, -\pi_N)$. Consequently, the signal σ_S that increases π_S^B (and does not affect π_N^B) will lead to a (weak) increase in T_S and π_S and a (weak) decrease in T_N and π_N, establishing the desired result. All changes are strict when these variables are interior. QED

As before, the positive information σ_S about the state actor increases the Bayesian benchmark beliefs about this actor, π_S^B. Because of the convex penalty term, σ_S also induces an increase in the beliefs that the individual holds about this actor, π_S. Given these improved beliefs, the individual raises his contribution to the state actor, T_S. In turn, these higher levels of T_S induce a further increase in beliefs concerning the quality of the state actor, π_S, amplifying these qualitative effects.

In addition, and crucially for our purposes, a higher π_S for a given π_S^B and π_N^B increases the deviation between $U(\pi_S, \pi_N)$ and $U(\pi_S^B, \pi_N^B)$, because the deviation of actual payoff from its maximum value (realized under Bayesian beliefs) increases. From the convexity of the penalty function $d[\cdot]$, this increases the marginal cost of motivated reasoning, which also applies to beliefs concerning the other actor. Consequently, π_S and π_N become substitutes, and any change that induces an individual to increase π_S triggers a decline in π_N, which leads to lower contributions to the nonstate actor, T_N. Intuitively, the convex penalty for the deviation of beliefs from their Bayesian counterparts implies that incentives to improve beliefs about the state also create incentives to make beliefs about the nonstate actor more realistic and thus less positive.[38]

Finally, we also remark that because of the complementarity between behavior and beliefs, the maximization problem of the individual is potentially nonconvex and may have multiple solutions (this is the reason why we focus attention on the case in which there is a unique solution to the individual's maximization problem). The presence of multiple solutions does not change the substantive comparative

[38] Note, in particular, that this formulation implies that the individual will always hold more positive beliefs about the actors he is interacting with, so the downgrading of his beliefs about the nonstate actor will make them closer to their Bayesian counterpart.

statics emphasized in proposition A2, and in this case we would state the comparative statics for the greatest and least solutions, which exist because supermodularity ensures that the set of solutions forms a complete lattice. The possible existence of multiple solutions is interesting independently since it is one facet of the feedback cycle emphasized in the introduction. Specifically, small changes in the underlying environment or new information may destroy some of these solutions and create large shifts in belief and behavior, propelled by a logic similar to the one discussed in the main text: as the individual interacts more with the state actor, he becomes more positive about this actor and less positive about the nonstate actor, and this motivates further increases in interactions with the state actor and additional reductions of relations with the nonstate actor, and so on.

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