Trust in State and Non-State Actors: Evidence from Dispute Resolution in Pakistan

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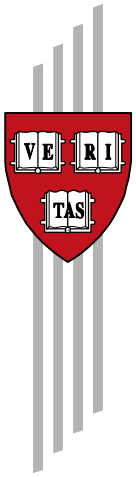

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Abstract

Lack of trust in state institutions, often due to poor service provision, is a pervasive problem in many developing countries. It may also be one of the reasons citizens turn to non-state actors for services. This paper investigates whether information about improved public services can help build trust in state institutions and move people away from non-state actors. We focus on dispute resolution in rural Pakistan. We find that (truthful) information about reduced delays in state courts leads to citizens reporting higher likelihood of using them and to greater allocations to the state in two high-stakes lab-in-the-field games designed to measure belief in the effectiveness of state courts and willingness to contribute resources for others to access them. More interestingly, we find indirect negative effects on non-state actors in the same high-stakes settings. We show that the positive direct and negative indirect effects are both mediated by changes in beliefs about the effectiveness of these actors. Our preferred interpretation explains these behaviors as a response to improved beliefs about state actors which then motivate individuals to interact less with non-state actors and as a result downgrade their beliefs about them. We provide additional checks bolstering this interpretation and alleviating concerns about potential social experimenter effects or mechanical contrasts between the two actors. These results indicate that, despite distrust of the state in Pakistan, credible new information can change beliefs and behavior.

JEL Classification: D02, D73, D74, D83, C91, C93, K40, O17, P16.

Keywords: dispute resolution, lab-in-the-field games, legitimacy, motivated reasoning, non-state actors, state capacity, trust.

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1 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 non-state actors step in to fill the void, providing competing services such as protection, conflict resolution, and broader public goods (see, for example, Clark 2004, Harmsen 2010, and Roy 2013 for case studies in the context of various Middle Eastern countries). The shifting balance between state and non-state actors may even create a feedback cycle where state weakness leads to not only more interactions with but also greater trust in non-state actors, which then fuels even closer association with them and less engagement with the state.

Although this interplay between state and non-state actors is plausible, there is little direct evidence that the strength of non-state actors derives from the weakness of state institutions. Similarly, there has been no investigation, to the best of our knowledge, of whether the deep-rooted lack of trust in state institutions in many societies can be redressed. In this paper, we study these issues by 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 if this information makes our respondents more willing to use, interact with, and trust state courts. Second, more pertinent to the issue of potential feedback between state and non-state institutions, we investigate whether trust in state and non-state actors are tightly linked such that positive information about state courts makes our respondents less willing to interact with competing non-state actors and less likely to have positive views about them — even though this information has no direct relevance to the non-state actor's effectiveness and trustworthiness.

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 et al. 2017), as well as the critical role that various non-state actors have come to play in parts of the country, especially in dispute resolution (see Chaudhary 1999, Gayer 2014, Shinwari 2015, Siddique 2013, 2015). Ineffectiveness of state courts in Pakistan is one of the key dimensions of state weakness that has both spawned general discontent and generated support for non-state actors such as the Taliban. For example, in 2009 Taliban militants took control of parts of Pakistan's

[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, Amsden 1992, Wade 1990), while weak and ineffective ("low capacity") states are often argued to cause poverty, instability and even civil war (e.g., Migdal 1988, Herbst 2014, Centeno 2002, Acemoglu et al. 2011, Besley and Persson 2009).

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 et al. 2014).[2] While the Taliban and other associated extremist Islamist groups are the most visible threat to the Pakistani state, the involvement of non-state actors in the provision of dispute resolution and public services is much more widespread, and our study focuses on the role of *panchayats* in dispute resolution.[3] Panchayats, comprised of groups of village elders and other influential locals, are a mainstay of conflict resolution in rural Pakistan and the primary alternative to state courts. 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.[4]

We use two approaches to measure behavior and attitudes towards state and non-state institutions. First, we collect survey information on expected usage and assessment of state courts and non-state dispute resolution forums like panchayats. Second, we design lab games meant to both address concerns arising from using self-reported data and elucidate different aspects of behavior toward these forums. Our respondents can earn as much as 550 PKR (approximately 5.3 USD at the time of the 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 *investment game*, is related to the trust game in experimental economics and 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 co-investments 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 improved court efficiency. We then measure how this information changes their game behavior and responses. At baseline, people report fairly low expected usage of state courts. Once people receive this "state positive" (informational)

[2]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]A study directly focusing on the Taliban would have been unsafe in the Pakistani context at the time of our study. It was also not possible to obtain official permission for research in areas undergoing violent insurgency.

[4]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*. But these bodies have not been institutionalized because local governments have been periodically disbanded (Shinwari 2015, Cheema et al. 2010).

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 investment games. These direct impacts are not driven by social experimenter effects whereby our respondents change their behavior because they think 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 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 towards state and non-state actors are tightly linked, we find large and robust *indirect effects*. Following the positive information about state courts (which provides no information on any non-state actor), expected usage of panchayats declines, and our respondents choose significantly lower allocations towards panchayats in both the fund dictator and 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.[5] 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 rational expectations/Bayesian benchmark are costly because they distort behavior. This results in a *complemen-*

[5]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 to Bem (1967). See also Kunda (1990), Gilovich and Ross (2015), Epley and Gilovich (2016), Edwards and Smith (1996), Jost et al. (2003) and Kahan (2013) for more recent discussions, and 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), Carrillo and Mariotti (2001), Caplin and Leahy (2001), Bénabou and Tirole (2002, 2004, 2016), Brunnermeier and Parker (2005), Van den Steen (2004), and Ortoleva and Snowberg (2015).

tarity between behavior and beliefs: when an individual interacts more with one type of actor, he has an incentive to distort his beliefs to be more favorable to this 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 non-state 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, non-state actors should deteriorate after the positive information about state courts (even though these have no direct relevance to the performance and effectiveness of non-state actors). To investigate this implication, we examine three specific dimensions of beliefs about the effectiveness of state and non-state 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 for state courts. We then turn to indirect effects and document that beliefs regarding effectiveness and trust concerning the non-state 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 non-state 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 more credible. We then go a step further by exogenously varying the source (and hence the credibility) of the information we provide. We present our respondents with (sealed) envelopes containing information from one of two sources: private news channels or Pakistan Television. In our surveys, the former is reported as less credible relative to the latter. Respondents are then randomly assigned one of the two sources (envelopes) and the two games played (post-treatment). Our findings from this exercise confirm that our respondents respond more strongly to the more credible source of information. These findings provide additional evidence for our interpretation that the direct and indirect effects we are documenting work through an informational channel.

An alternative interpretation of the indirect impact on panchayats is that they reflect mechan-

ical "contrast effects" (Pepitone and DiNubile 1976, Bhargava and Fisman 2014, Kamenica 2008) whereby perceived improvements about state courts automatically lead to a deterioration in beliefs about the only other option, panchayats.[6] 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, to test for such contrast concerns directly, we introduce 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 including both usage and game allocations.[7] These findings support our interpretation that the negative indirect effects we estimate reflect a genuine deterioration of beliefs about panchayats.

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 non-state forums holds more generally than just for the particular (state positive) informational treatment 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 non-state 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 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 et al. (2015) argues that the greater capacity of the north Vietnamese state (relative to areas in the

[6]Yet another interpretation is that our respondents are fully Bayesian (without any motivated reasoning considerations), but their priors about the effectiveness of the state and the non-state actors are negatively affiliated (correlated), and thus any news about the state being more effective becomes relevant for them to update their beliefs about non-state actors. We discuss this issue further below. Here we note that for our purposes such negatively-affiliated priors have very similar implications to motivated reasoning, but make additional predictions, which we test in our analysis of heterogeneous effects and do not find uniform support for.

[7]If anything, for both hospitals and sports clubs we find 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 non-state actors like local sports clubs (that nevertheless rely on the state for maintaining underlying order etc.) become more effective.

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 various different settings (see, for example, Berg et al. 1995, Burks et al. 2003, Dufwenberg and Gneezy 2000, Bohnet and Zeckhauser 2004, Camerer and Fehr 2004, Ashraf et al. 2006, Johnson and Mislin 2011). Most of this work does not focus on attitudes towards state institutions, 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 et al. (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 by Cardenas and Carpenter 2008).

Finally, some works in the sociology and social psychology literatures are related to our paper as well. For instance, Anderson (2010), Sullivan and Transue (1999), 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 2 describes the context of dispute resolution in Pakistan. Section 3 provides the details of our experimental design and empirical strategy. Section 4 presents our main empirical results, Section 5 discusses potential mechanisms that may account for our results on direct and indirect effects, and then presents additional evidence relevant for these mechanisms. Section 6 concludes. Appendix A presents a formal model elucidating various mechanisms via which direct and indirect effects may be working, while the online appendices provide additional robustness checks and details on study design.

2 Background and Context

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

2.1 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.[8] In our survey one in every five households reports that they have accessed the court system in the last three months.[9] 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 (Khan et al., 2000). The current party in power, the Pakistan Tehreek-e-Insaaf, or Movement for Justice Party, went from being a small third party to wining the 2018 elections outright, in large part based on its platform of improving access to justice.

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 non-state 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.[10] 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 primarily works through three key actors — police, judges, and lawyers. The police are responsible for the maintenance of law 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.

Non-state actors have historically run parallel dispute resolution forums in rural areas of Pakistan that are distinct from the state judicial system. These non-state forums are (typically) ad hoc local councils of village elders called panchayats, and are usually given the authority to resolve disputes on behalf of residents of the community (Chaudhary 1999, Soomro and Chandio 2013, Ayaz and Fleschenberg 2009).[11] They ignore the formal law and compete with state institutions.

[8]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 5.3 and 9.2 per 1000 persons between 2005 and 2010 (Eisenberg et al. 2013) compared to 17 per 1000 in Pakistani Punjab during the same period (authors' estimates based on the Lahore High Court Annual Reports).

[9]Recent studies show that a majority of cases that end up in state courts involve disputes around land, property, inheritance, and contract. Siddique (2013), for example, 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.

[10]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. Whereas this system has continued in post-independence India, it was abolished in Pakistan as a result of the Devolution Reforms of 2001 to achieve separation of powers between the judiciary and the executive (Article 175(3) of the Constitution of Pakistan). A consequence has been a significant expansion of the remit of the state courts in Pakistan's judicial system.

[11]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 8th and 9th centuries B.C.E., the *Mahabharata*, and it also appears to have continued through the period of Muslim rule. This is in contrast to the state judicial system,

80% 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 (Shinwari 2015, Siddique 2015, Chaudhary 1999).

Since independence in 1947, the Pakistani state has been highly suspicious of such parallel non-state 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 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). In 2004, the Sindh High Court banned trials under the non-state 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).

2.2 Access to and Views towards State Courts and Panchayats

Our primary informational treatment is to provide positive information about state courts and study how this impacts behavior and views towards both state courts and panchayats. It is therefore instructive to understand what the baseline situation is in terms of access 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 1 to 10, they report their likely usage of state courts as 4 while panchayats have a reported usage of 6.5. This is in part driven 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, cost of transportation, and the legal fees necessary to enter and remain in the system.[12] In contrast, panchayats offer quick resolution by gathering the disputing

which is a product of British colonial rule (Siddique, 2015).

Hoebel (1965) observed more than five decades ago that "the legal system of Pakistan does not constitute a neatly integrated whole; it is made up 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" (Chaudhary 1999).

[12]Based on 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 two years on average with anywhere between 5,000 PKR and 200,000 PKR in costs. The resolution of business related cases may be faster, though even those can take upwards of six months with potentially significant costs for the parties (Siddique 2013). Shinwari (2015) finds that

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 district city. Our baseline surveys also bear out this cost differential: respondents rated court costs as 7.5 and panchayat costs as 3.3 (where 10 means extremely expensive). Similarly, respondents report that a theft case that costs 1,000 PKR to settle in a panchayat would cost about 23,000 PKR 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 Javed 2017, Nekokara 2016, Shinwari 2015). Siddique (2013) finds that 47% of the respondents in Lahore felt that the laws are either biased against them or unjust, and that three quarters of respondents in a survey of litigants were dissatisfied or deeply dissatisfied with the pace at which their case was proceeding, and 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 "(M)y family has withered away while pursuing this matter," and they bemoan: "(T)his legal system is a complete failure." Nearly 90% of the respondents who had accessed the police or the judicial system in the three 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 understand better 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 to 3.9 for state courts). This is despite the panchayats' lack of legal training, their systematic tendency to go against the writ of the state, and their failure to incorporate disenfranchised members of society such as women and low-income groups.

Both access difficulties and low effectiveness of state courts are rooted in endemic delays. Over 80% of respondents in 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 five years. The majority did not know when a verdict was expected. In contrast, panchayats typically offer faster decisions. Recall, 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 Siddique (2016) and Chemin (2009) report that cases in state courts take on average two or three 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

low income households and women particularly face high costs of accessing formal justice institutions, in part, due to the process being time consuming, the high lawyer fees, and the long distances to courts.

than 1.2 million cases were pending in 2001, and recent estimates suggest that this number may have climbed up 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 fully informed about recent changes. Indeed, in our surveys an overwhelming 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 will see later in the paper.

Since our study sample includes two distinct rounds with an almost two 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 the exercise of market power by the private sector. The period of judicial activism began with the appointment of Chief Justice Saqib Nisar on the 31st of December 2016. His tenure (2016-2019) 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.[13] These cases were extensively covered by electronic and print media and resulted in the Supreme Court's activism gaining tremendous salience and fairly broad support with the public.[14]

3 Experimental Design

Understanding the determinants of individual decisions to access state or non-state actors for dispute resolution is challenging given the myriad factors impacting such behavior. In order to overcome these challenges, we utilize a within-subject experimental study design and examine how the be-

[13]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 (Newsdesk, 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 over-exploitation of groundwater (Samaa, 2019). The Chief Justice also took Suo Motu action against high salary appointments in public sector companies (Reporter, 2019), 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.com, 2017).

[14]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 or disagreed 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.

havior 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 towards state and non-state actors. To help address concerns that survey responses may not reflect real-world behavior, we focus on relatively high-stake experimental games designed to reveal respondents' behavior towards both the state and the non-state actors. In this section we detail the informational treatments, experimental protocols, and data and sampling methods.

3.1 Informational Treatments

We are interested in whether perceptions of state effectiveness can change attitudes and behavior towards the state and the non-state actors. Given the generally poor views of state courts and the likelihood that the average citizen may not be fully informed, our primary information treatment is one that provides true and favorable evidence on the performance of the formal judicial system. We refer to this as the **"state positive (SP)** treatment." Based on 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 2, regardless of the legitimacy or beliefs concerning judicial biases, a reduction in delays is likely to be widely attractive. This treatment provides respondents 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 6000 pending cases in two 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, non-state positive and non-state negative treatments (see Appendix C). These treatments were also based on truthful media reports. Our pilots revealed that the impact of these different treatments was fairly symmetric (see Section 4.5), so for the sake of statistical power we decided to only scale up the state positive version. We should note that while the state negative treatment also primed on decision-making delays, the non-state positive (negative) primes included information about the (in)effectiveness of the decisions made by the panchayats as well. This suggests that the informational impacts we observed are not just about changes in delays, but respondents react analogously when informed about more or less effective resolution of disputes as well.

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. In order to do so we provided the following **social experimenter (SE)** 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.[15]

3.2 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 50 PKR payment for agreeing to participate, plus they have the potential of earning significant payoffs from the games (see online Appendix 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 non-state actors. The survey includes a question on the expected usage of the state and non-state forums, which we use throughout the paper. In later stages of study we also included questions on individual beliefs regarding the effectiveness of the relevant forum as well as general trust in the forum.[16] We additionally gathered information about their expectations of others' usage (all of these variables are on a scale from 0 to 10).[17] By comparing responses to these questions pre- and post-treatment, we can measure the change in a subject's own expected usage and their perceptions of others' usage

[15]While both the state positive and 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 re-calibrated 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 outcomes of interest, and not based on estimated effect sizes, p-values or t-statistics. Our within-subject design is unaffected by this re-calibration, and in any specification that involves cross-subject comparisons we include strata fixed effects interacted with a post-treatment dummy to capture any differential responses that may arise due to baseline differences across strata.

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

[17]In particular, we ask about expected usage: "*How likely are you to use the state or panchayat, 0 meaning you would not go to the forum if you had a dispute and 10 meaning you would definitely go to the forum if you had a dispute?*" Regarding perceptions of other villagers' engagement, we ask: "*If 10 people have different types of dispute (of the kind listed above), how many of them are likely to go to the state courts to resolve their disputes, 0 meaning no one would go to them and 10 meaning, all 10 people will go to them?*"

of state and non-state forums resulting from our state positive and social experimenter treatments.

In addition to the baseline surveys, respondents play two different games — the fund dictator and the investment game — before and after the state positive and social experimenter treatments. The two games are designed in order to capture different aspects of citizens' views of the state and the non-state 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 non-state dispute resolution forums. We do so by asking individuals to contribute to two potential funds; one (state fund) to assist those individuals in the community who prefer to go through the state system in order to resolve disputes, and the other (non-state fund) for those who would rather go to the non-state alternative. Respondents can confidentially make a choice to give all, some, or none of a specified amount to the fund in question based on their beliefs about the relevant actor's benefit to citizens and the respondent's 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.[18] We therefore expect the respondent to allocate more money to the fund he believes will be more useful and/or to the one he feels more altruistic towards. In order to avoid any mechanical spillover effects, participants receive two separate endowments of 250 PKR, 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 100 PKR to the state fund and keep 150 PKR for himself out of the first endowment, while donating 150 PKR to the non-state fund and keeping 100 PKR 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 to the panchayat. Both members are rightfully owed remunerations from a defendant, and the respondent is given a chance to invest an amount $X \not\in$ out of 250 PKR in the plaintiff's case in return for a share of the remuneration. They are told the share they receive will be ηX, and thus their total take-home amount will be $(250 - X) + \eta X$, where $\eta \not\in [0, 2]$ measures the effectiveness of the forum. If a forum is completely ineffective, then $\eta \not= 0$ and the plaintiff will receive no remunerations; if a forum is fully effective, then $\eta \not= 2$ and the plaintiff will receive full remuneration. The respondents are informed that $\eta \not$ has been calibrated

[18] Based on all the games played so far we have a total of 203,480 PKR and 226,830 PKR contributed by our respondents in state and non-state 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 the ability to lodge a complaint or a grievance against police or members of the judicial system who are not fulfilling their obligations. The non-state fund is being donated to the Legal Aid Society, a non-governmental organization, which provides advice to respondents free of charge.

for each forum to reflect reality.[19] Hence, the 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 all 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. Similarly, we varied the sequence in which respondents played the games to see if their understanding differed depending on which game was played first. We found no such effects, but 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 in order 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 footnote 19).

We also took several steps to ensure high quality responses in the games. Our first strategy was to

[19] After piloting different options, we chose to keep the return on investment from both the state and the non-state forums at $\eta = 1$. This implies that each respondent receives a payment of $(250 - X) + \eta X = 250$. We do not view this choice as involving deception, since this decision was made in good faith and on the basis of the information available to us. In particular, while one could have varied the return, our pilots revealed significant variation in success rates between state courts and panchayats across regions, so we did not have the ability to have reliable region-specific estimates of η. This choice was the simplest and also minimized any potential harm/loss to the respondents (an important factor given we wanted to ensure there were no human subject concerns arising from the research). Furthermore, this choice avoids the incorrect inference that our respondents might have drawn from differential values of η about one forum being significantly better than the other at the end of our game. Indeed, 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 et al. 2007). Moreover, our respondents never gave any indication of loss of experimental control arising from a perception of deception (Jamison and Schechter 2008) or expressed any opinion suggesting reduced trust in future research (Friedman and Shyam 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% of respondents reported prioritizing their own priors or the information we provided over "guessing" our views of what the return is.

give each respondent 50 PKR 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 game three times. First, they played a practice game of each, followed by a discussion to ensure they had understood the game procedures. A customized board with subdivisions for the pre- and post-treatment allocations for each experiment was used as a visual tool during the explanations. The benefit of such a tool is three-fold: i) it provides a visual aid for respondents; ii) it creates a sense of privacy (each section has a cover that hides the allocations of the respondent from the surveyor); and iii) it provides demarcation between the two pre-treatment and the two post-treatment games for each experiment. Each section is further subdivided to depict the respondent's allocations for state/self and non-state/self. The board is shown in Figure B1 in Appendix B. After the surveyor was sure that the respondent had understood the game, pre-treatment games were played. Next, the respondents were provided with one of our (state positive or social experimenter) treatments. Finally, after confidentially reviewing their pre-treatment allocations, they repeated the same games. Participants were paid for only one of the four allocation decisions (pre-treatment state and non-state and post-treatment state and non-state allocations) for each type of game. For the fund dictator game, the payoff was simply one of the four amounts 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 one 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 non-state (panchayat) actor 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 based on how credible they think a source is. We describe these games in more detail when we present the results.

3.3 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 non-state 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 non-state actors was more easily identifiable within this setting. While urban areas also have non-state actors, the specific actor varies considerably from place to place. In contrast, in rural areas, the panchayat was invariably identified as the primary non-state actor. Moreover, based on the 1998 Population Census, a majority (68.7%) of Pakistan's population still resides in rural villages (Pakistan Bureau of Statistics).

We randomly draw rural households in one district (Sargodha) of Punjab. The district is fairly representative of Punjab and was drawn according to a pre-existing 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 2015 to October 2015 that consisted of a total sample of 2,986 male respondents from 37 villages.[20] Based on reviewer and editorial feedback, we then conducted a second round from April 2019 to May 2019 that introduced design modifications that added to the robustness of our results and provided further insights into potential channels behind the observed impacts. This second 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 Appendix B).

Survey participants were recruited through door-to-door household visits in a pre-survey location round. Available households were then randomly assigned to primary and replacement lists to meet a pre-specified target for each village. In each village (or in each settlement if a village consisted of more than one settlement) we only conducted a limited number (45-50) of surveys and only surveyed for 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 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/or survey activity before. However, our preventative measures of limiting surveying days in each village were highly successful; no respondents had met our criteria for exclusion as defined above.[21] Table B1 in Appendix B presents demographic summary statistics for our sample, as well as the baseline values for our main

[20] In order 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 pre-existing 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.

[21] Because we used the same sample frame of households for the second round of sampling, our households are, on average, about two 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 in Appendix B that these differences typically have no predictive power for the pre- and post-treatment 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 equation (2) below.

outcome variables (observation counts vary because not all questions were asked in both rounds). Recall that our primary sample includes only men, and from Panel A we see that they have an average age of 38, 78% have some formal education, 29% own land, and their average monthly household income is 16,654 PKR. 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 non-state actor (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 investment games.[22]

4 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 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 towards 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.

4.1 Direct Effects

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

$$Y_{it} = \alpha + \beta \text{Post}_t + \delta_i + \varepsilon_{i,\psi} \tag{1}$$

[22] 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 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 (where additional information on reduced delays should make them more favorable to state courts). 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 non-state allocations and the difference between state and non-state usage is positive: 0.39 for the fund dictator game and 0.42 for the investment game.

where Y_{it} is our outcome variable of interest (expected usage or allocations in the two games) and Post_t is a dummy for observations after the state positive treatment. The parameter $\beta\psi$is our 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 pre-treatment average. For example, in column 1 the state positive treatment increases expected usage by about 20% — by 0.8 (s.e. = 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 relatively 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 (s.e. = 1.30). This corresponds to an average post-treatment allocation of 116 or roughly a quarter of a day's wages in our sample villages. The impact represents about a 15% increase from the pre-treatment base. The pattern for the investment game (column 3) is similar; in this case we see an increase of 14.6 (s.e. = 1.34) from a base of 115.4.

Panel B of the table 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 towards 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 only contains 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, for example, 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: though there is a small response for expected usage, which increases by about a third of our estimate in Panel A, 0.28 (s.e. = 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.[23] 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 post-treatment dummy, Post_i, and a dummy for the group receiving the state positive treatment, StatePositive_i. The coefficient on the dummy Post_i denoted by γ, captures the impact on respondents that receive the social experimenter treatment. We also 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.[24]

Overall, we conclude from Table 1 that there are robust and fairly large effects of 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 widespread 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 quality. We next turn to the spillover effects of this informational treatment on behavior concerning the non-state actor.

4.2 Indirect Effects

As we emphasized in the Introduction, ineffectiveness of state institutions may prompt citizens to turn to non-state actors for public services, and conversely, when state institutions improve, the same mechanism may trigger a withdrawal away from non-state actors. We now investigate this question,

[23] Table B4 in Appendix B 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.

[24] Note that 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.

focusing on the non-state actor competing with state courts in the area of dispute resolution, panchayats. Our main results are summarized in Table 2, which has an identical structure 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 of *state courts* — so that our focus is now *indirect effects* on the *non-state 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 3 we see a negative indirect effect, -0.55 (s.e. = 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 (s.e. = 1.30) in column 2, and -12.13 (s.e. = 1.35) in column 3. The estimate in column 2 is about 30% smaller than the direct positive effect in Table 2, 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 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 (8), we continue to find significant and sizable negative effects from the state positive treatment on the non-state actor.

Overall, our results show substantial negative spillovers on non-state 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 to induce them to disengage from non-state 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.

4.3 Anonymity

The social experimenter concern arises 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 are reassuring that our direct and indirect effects cannot be explained by social experimenter effects. Nevertheless, to provide additional

evidence against observability and 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 30 and 70 PKR) and one empty white envelope for each allocation of a respondent. Each envelope is labeled with a unique ID on the inside of the envelope, such that all envelopes in one packet contain the same ID (which envelopes had what ID was neither known to the surveyors nor recorded by us). Before the game, each respondent is presented with 10 such game packets and pick one. Their choice of packet was not observed by surveyors and cannot be recovered later. The games are played as before, with the major exception that the respondents put their (baseline and post-treatment) allocations privately into their envelopes and seal and place them in the appropriate game board. This procedure ensures that no one except the respondents themselves could observe the allocations they made. In the final step, each respondent is 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 this amount was paid in an anonymous manner as well. This is 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 take their sealed envelopes and place 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 have the same ID marked inside the envelope, we can 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 post-game survey responses. All (100%) of the respondents we asked reported that they thought their allocations were fully anonymous.

Reassuringly, Table 3 shows that the results in this anonymized fund dictator game are similar to those we obtained with the original fund dictator game.[25] Therefore, our results in these games

[25]Notice that the estimates are almost twice as large as our original estimates in Tables 1 and 2. This, however, is not related 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 8, and tests for differential effects do not reject the hypothesis that the magnitudes of the direct and the indirect effects in anonymous and non-anonymous 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 2, which appears to have made our state positive informational treatment more powerful. In particular, though in the baseline our respondents did not show 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. We explicitly test and show in Table 5 that increased credibility of information leads to respondents changing their

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.

4.4 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 1 to 10, and we code those reporting trust greater than or equal to 6 as having high credibility. The informational channel suggests that these individuals should respond more. Estimates of this equation reported in Table 4 support this prediction (see also Table B25 in Appendix B). We robustly estimate a significant and quantitatively large κ. For example, an individual who reports a high level of trust ($\text{Credibility}_i = 1$) has, on average, twice as large a response as an individual who attaches a low level of credibility to the same information ($\text{Credibility}_i = 0$).

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. We conducted pilots that revealed that our respondents trust national media more than local (private) TV 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 Pakistan Television (PTV) while the other has been reported in private TV channels. The information in both envelopes is identical (since our information has indeed been reported both in 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 showing that respondents' trust in the information is greater when it is reported by PTV as opposed to coming only from private TV channels. Our main results in this table, reported in columns 2-4 (Panels A to 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

behavior by more. Consistent with this interpretation, Table B22 shows that the response of belief questions is significantly more pronounced in the second round as well.

less precise, and our estimates are statistically insignificant.[26] Nevertheless, the differential direct effects provide a powerful confirmation of the informational channel hypothesized in this paper.

4.5 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. Although our experiments were designed for within-subject comparisons, our data still enable a cross-subject strategy (albeit with lower statistical precision) where we simply compare the post-treatment expected usage and allocations in the two games between respondents who received the informational and the social experimenter treatments. These results are presented in columns 1-3 of Table 6. Panel A of this table focuses on direct effects, while Panel B is for indirect effects. The results are quite similar to those presented in Tables 2 and 3, even if less precise, because heterogeneity in baseline allocations create additional variability. For example, we estimate a direct effect of 0.82 (s.e.= 0.19) for expected usage, of 19.71 (s.e. = 3.93) for allocation in the fund dictator game, and 21.31 (s.e. = 3.91) in the investment game. The indirect effects reported in Panel B are similar as well, though again less precisely estimated and thus statistically significant at 1% just for expected usage. In columns 4-6, we estimate cross-subject regressions directly controlling for these baseline (pre-treatment) values. These estimates are now highly significant and very close to those in Tables 1 and 2, highlighting the importance of accounting for the (substantial) cross-subject heterogeneity at baseline. Overall, although designed for within-subject comparisons, we find it reassuring that the data generated from our experiment yield very similar results with these cross-subject specifications.

Table B8 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.

Even though we carefully explained the procedures and details of each game to our respondents at each stage, we also wanted to ensure that any remaining imperfect understanding of the game

[26] Even though our randomization procedure was implemented correctly, due to relatively small sample there are some baseline differences between the two credibility treatments; see Table B6 in Appendix B. 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 in Appendix B.

would not influence our results. For this reason, we collected information on the surveyors' and the respondents' own assessments of how well they understood the game. In Table B9, we report estimates after removing any respondents who received a score below 5 (out of 10) from either their own reports or from the surveyors. The results are very similar to our baseline estimates. In Table B10, we show estimates from specifications that down-weight outliers (following the procedure of Li, 1985), which are also very similar to our baseline results. In Table B11, we report estimates after excluding respondents that 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 B12 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 B13 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 non-state actors. Though naturally under-powered, the results in these smaller pilot samples are in line with our expectations. In Table B14, with the "state negative treatment", we estimate statistically significant negative direct effects for usage and 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 "non-state negative treatment," Table B15 shows negative and statistically significant direct effects for the non-state actor, and positive and generally statistically significant positive indirect effects for state institutions. With the "non-state positive treatment" in Table B16, where we have the smallest sample, the effects have the expected signs, but they are mostly imprecise. Overall, we find it reassuring that, even with the 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 that may not have fully understood the context or the details of the game.

5 Mechanisms

In this section, we investigate the potential channels underlying our main results — the positive direct effects on behavior towards state courts and the negative indirect effects on non-state 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.

5.1 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 towards 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 Section 4.4). This type of reaction is consistent with several models of Bayesian and non-Bayesian belief formation.[27] In Appendix A we present a simple Bayesian model of belief updating 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 non-state 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 only depend on their belief on 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 Appendix 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

[27] 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, as discussed in Section 2, suggests that they may have relatively precise priors.

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 either by changing behavior or beliefs (see other references in footnote 5 in the Introduction). 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 believe (correctly) 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 for them (and for their families living nearby). Distorting beliefs is beneficial to the individual because of cognitive dissonance considerations, but comes with costs because it distorts behavior too. 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 towards state and non-state 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). Based on 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 non-state panchayats. The main conclusion from this model is that positive information about state courts will make our agents become more negative about the competing non-state actor, because they expect to use the non-state actor less and thus dial back their initially-inflated positive views of this alternative forum while becoming more positive towards state courts (see Appendix 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 non-state actor. The motivated reasoning model additionally suggests a potential feedback cycle: when state institutions are ineffective, people start turning to non-state actors, and this motivates them to change their beliefs to become more positive towards non-state 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 towards the non-state actor on the basis of the new information about the effectiveness of the state actor. Under alternative assumptions, for example because there are powerful income effects, their behavior towards the non-state actor may change, but they should not alter their beliefs about and their trust

in the non-state 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 non-state actor on the basis of our state positive treatment. This happens when the priors about the state and the non-state actors are negatively affiliated — meaning that our respondents believe that if the state is effective, then panchayats must be ineffective/non-trustworthy. We do not find any evidence from our interviews and surveys supporting such negatively affiliated priors. Nevertheless, we will return to a discussion of this type of Bayesian updating in the context of heterogeneous effects in Section 5.4. 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 the non-state 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 non-state actor, but this reflects a non-generalizable 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 in theory 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 et al. 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 reduces the scores of next 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 only present 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.

5.2 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 7. 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 (s.e. = 0.04) for service effectiveness, 0.54 (s.e. = 0.03) for enforcement effectiveness, 0.60 (s.e. = 0.04) for access, and 0.87 (s.e. = 0.04) for trust. These effects are sizable, ranging from 10% to 20% of the baseline values (represented by the constants in the table).[28]

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 2 of Panel B, the estimate for the indirect effect on service effectiveness for panchayats is negative and significant, -0.17 (s.e. = 0.03). In column 4, for enforcement effectiveness, the estimate is -0.16 (s.e. = 0.02). There are similarly statistically 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, s.e. = 0.03). This effect is not present in our second-round data as shown in Table B22 in Appendix B.[29]

[28] As noted in footnote 16, these belief questions were asked at both baseline and after the state positive treatment only at later stages of our fieldwork (a third of first round and all of the second round). Our budget, coupled with concerns of not having a large enough sample for these questions, meant that we chose not to provide the social experimenter treatment to a sub-sample of our respondents answering these questions. Since our initial sample results do not show any sizable social experimenter effects, we believe that the impacts for these belief questions are highly unlikely to be due to 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 the non-state actors in delivering justice and enforcing verdicts at the end of our survey. Since we have the social experimenter treatment for this sample, we can directly test whether social experimenter effects could have a similar impact on these beliefs. Table B17 in Appendix B presents the results using a cross-subject design (since 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 B18). Both of these results allay concerns related to social experimenter effects.

[29] One possibility is that our respondents expect others to switch away from panchayats to state courts (as our results in Table B8 in Appendix B demonstrate) and thus also surmise that accessing panchayats would get 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 B19 in Appendix B 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 towards panchayats.

We checked the robustness of the results reported in Table 7 in a number of ways. Tables B20 and B21 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 B22 confirms that our main results hold separately by survey rounds.

Overall, the results in Table 7 show fairly consistent positive effects on beliefs concerning state courts and negative indirect effects on beliefs for the competing non-state 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 non-state actors may be the pervasive inefficiencies of state institutions, possibly generating a vicious cycle.

5.3 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 post- 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 B23 in Appendix B, 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.

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 which was identical to our original game setup with the once 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 main results were driven by contrast effects, following the positive information on state courts we should find similarly-sized negative spillover effects on local sports clubs as we did for panchayats. The results presented in Table 8 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 (s.e. = 2.25), while the one for the investment game is 5.20 (s.e. = 2.11). Because these positive indirect effects are much smaller than the direct effects (about 10 to 14% of the magnitude of the direct

effects), 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. Focus group interviews revealed that our respondents (justifiably) believe that these types of activities are not independent of the effectiveness of the state: improvements in the state likely complement even neutral actors. For example, a more effective state means that even unaffiliated local sports clubs may operate 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 Table B24 columns 1 and 2).

Overall, these results suggest that negative indirect effects are confined to non-state actors that are competing with state courts and cannot be explained by mechanical contrast effects.

5.4 Heterogeneous Effects

Bayesian models make additional predictions which can be investigated by studying heterogeneous effects.[30] We focus on two predictions (see Appendix A for details). First, under Bayesian updating, agents who are very sure about the quality of the state or the non-state actors should not update their beliefs much (whatever the source of new information). This implies a non-monotonic 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 lead to smaller negative indirect effects). We seek to investigate both of these predictions by estimating the following generalization of (1):

$$\begin{aligned} Y_{it} &= \alpha + \beta \text{Post}_i + {}^{\text{state}}_{1}\text{InitialBelief}_i^{\text{state}} \cdot \text{Post}_t + {}^{\text{state}}_{2}(\text{InitialBelief}_i^{\text{state}})^2 \cdot \text{Post}_t \\ &+ {}^{\text{non-state}}_{1}\text{InitialBelief}_i^{\text{non-state}} \cdot \text{Post}_t + {}^{\text{non-state}}_{1}(\text{InitialBelief}_i^{\text{non-state}})^2 \cdot \text{Post}_t + \delta_i + \varepsilon_{it}^{\psi,\psi} \end{aligned} \tag{4}$$

where $\text{InitialBelief}_i^{\text{state}}$ and $\text{InitialBelief}_i^{\text{non-state}}$ measure individual i's initial beliefs about the state and the non-state 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/or the non-state actor. Moreover, the square terms in initial beliefs introduce the possibility that these effects are non-monotonic. 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 9 (where we normalize the proxy for initial beliefs on the right-hand side to lie between 0 and 1 for ease of inspection). As usual, Panel A is for direct effects and Panel B

[30] We were also interested in other types of heterogeneous effects, for example, depending on caste status and social economic characteristics. Even though these characteristics predict initial expected usage and game allocations, we did not detect any heterogeneities depending on these characteristics.

presents indirect effects. Tables B28-B31 in Appendix B 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.

The first three columns 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 non-state actors. This, in particular, implies that if $\beta_1^{\text{state}} < 0$, then $\beta_1^{\text{non-state}} > 0$, so that smaller positive direct effects should go together with smaller negative indirect effects. The results in the 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 affect 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 non-monotonic (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 non-state actors. The results in Table 9 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.

The next three columns investigate the first prediction mentioned above, related to non-monotonic effects. The results show some evidence for non-monotonicities, but are not uniformly consistent with fully Bayesian models. In particular, the square of initial beliefs about state courts is negative and significant in the allocations to the state actor in both the funds dictator and investment games, but not for expected usage of state courts. The estimated pattern is monotonic for expected usage and the fund dictator game, but non-monotonic for the investment game. Notably, the non-monotonicity is consistent with Bayesian predictions: when initial beliefs are close to their minimum value, their impact is positive, and when they are close to their maximum value, their impact is negative. In contrast, however, the impact of initial beliefs about panchayats on expected usage and game allocations to panchayats is monotonic; even though the quadratic is significant, it simply makes the relationship convex, without introducing any non-monotonicity, and in fact, the negative effects of these beliefs are stronger when they 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 that 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.[31]

6 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 non-state 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 them and trust in them, while simultaneously increasing their use and trust in competing non-state actors; and the more they turn to these non-state 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 are, and how the interplay between state and non-state 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 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 non-state actors, we estimate large and robust *indirect effects* on competing non-state actors, in this case panchayats. Namely, after the same state positive informational treatment, our respondents report that they are less likely to use non-state 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 experimenting 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 non-state 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"

[31] 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.

where reduced usage of non-state 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 non-state 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 credible information about their 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 non-state) 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 to measure not only 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 non-state 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 non-state 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 non-state actors that may be leveraged in reconstructing state capacity.

Appendix A: Theory

In this part of the Appendix, we provide formal models of Bayesian updating and motivated reasoning, and derive their implications for behaviors, game allocations and beliefs.

6.1 Setup

We consider a setting in which two actors, one state and one non-state, 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 $\pi_S^0 = \mathbb{E}[\theta_S|\text{Prior information}]$ and $\pi_N^0 = \mathbb{E}[\theta_N|\text{Prior information}]$. Our experimental treatment is to provide (some of) the respondents with relevant information (signal σ_S) about the quality of the state actor.[32] We denote the beliefs of the individual after he receives this information by $\pi_S = \mathbb{E}[\theta_S|\text{Prior information, } \sigma_S]$ and $\pi_N = \mathbb{E}[\theta_N|\text{Prior information, } \sigma_S]$.

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

$$u(c) + \pi_S g_S + \pi_N g_N, \psi \tag{5}$$

where c denotes cash-in-hand ("consumption"), and g_S and g_N are services provided respectively by the state and the non-state actor. 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.

In mapping this expected utility function to our experimental setting, we interpret the terms with g_S and g_N as incorporating not just what the individual himself is receiving but also the services to the entire community which 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

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

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 non-state actor, denoted by T_S and T_N. 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

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

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 non-state allocations in our setting and simply write the cash-in-hand (consumption) of the individual as

$$c \not= y - T_N - T_S \tag{6}$$

(without a non-negativity constrained on c).

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

$$U(\pi_S, \pi_N) = \max_{T_S, T_N \geq 0} u(y - T_N - T_S) + \pi_S h_S(T_S) + \pi_N h_N(T_N).\psi \tag{7}$$

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.[33]

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.

6.2 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

[33] We also note at this point that the solution of this problem links allocations to beliefs in it 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 Section 5.4.

as

$$\pi_S^B = \frac{\Pr[\sigma_S|\theta_S = 1]\pi_S^0}{\Pr[\sigma_S|\theta_S = 1]\pi_S^0 + \Pr[\sigma_S|\theta_S = 0](1 - \pi_S^0)}, \psi \tag{8}$$

where $\Pr[\sigma_S|\theta_S]$ is 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, (8), then yields

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

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

$$\pi_N^B = \frac{\Pr[\sigma_S|\theta_N = 1]\pi_N^0}{\Pr[\sigma_S|\theta_N = 1]\pi_N^0 + \Pr[\sigma_S|\theta_N = 0](1 - \pi_N^0)} = \pi_N^0. \tag{9}$$

We next discuss the case in which π_S^0 and π_N^0 are jointly distributed and are negatively affiliated (for example, they could be jointly normally distributed with a negative covariance term). In this case, we will see that Bayesian updating implies $\pi_N^B < \pi_N^0$. 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 non-state 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

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

When this is not the case, the informational treatment will directly impact beliefs about the non-state actor.[34] For reasons explained in the text, throughout we focus on the case where (10) holds. In addition, the inequality in (10) simply repeats our assumption that σ_S is good news about the state actor's quality.

The question is about the association between the priors concerning the state and the non-state 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 the non-state 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

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

(together with (10)) implies that

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

confirming (9).

Alternatively, we say that priors about the state and the non-state 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 non-state actor is less likely to be high-quality, and vice versa.

6.3 Basic Implications of Bayesian Updating

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

Proposition 1 *Suppose that the individual is Bayesian and that signal σ_S is directly informative only about the state actor (that is, (10) holds).*

1. *Suppose in addition that preferences are quasi-linear ($u(\cdot)$ is linear) and priors for the state and non-state 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.*

2. *Suppose instead that preferences are not not quasi-linear ($u(\cdot)$ is strictly concave), but priors for the state and non-state 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.*

3. *If priors for the state and the non-state 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. Part 1. 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 (9). In this case (7) can be written as

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

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.

Part 2. The proof is very similar to Part 1, 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 non-state 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, (9) applies.

Part 3. In this case, because priors are negatively affiliated, (9) does not apply and instead we have

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

where the second line follows from (10); the third line puts the terms in the second line to 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 $p_1 - p_0 >_\psi 0$. 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 (weakly) decreasing functions of π_N. This completes the proof of the proposition. ■

Let us start with Part 1. 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 non-state actor, π_N. In addition, given the quasi-linear preferences and allocations to the non-state actor are unaffected.

To understand Part 2, 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 non-state 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 is the expected effectiveness of

the relevant actor.

Finally, Part 3 is intuitive too. In this case, the Bayesian agents are convinced that the qualities of the state and non-state 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 in informative about them. In this case, our state positive treatment reduces π_N, and as a result will lead to lower allocations towards the now less favorably-viewed panchayats.

We also note two additional properties of Bayesian updating (relevant for our discussion in Section 5.4). First, equations (8) and (9) make it clear that if priors are close to 0 or 1, there will not be much updating and hence changes in beliefs and economic responses should be non-monotonic in initial beliefs.[35] 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 direct effect, it should also lead to a smaller (negative) indirect effect.[36]

In summary, the Bayesian model suggests that in the most plausible scenario, where utility quasi-linear (because, as explained above, the amounts are small and our respondents understand that they will only receive one payment), there should be no indirect spillover effects on panchayats. If there are income effect (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 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 as 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

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

[36] To see this, take the case where a higher π_S^0 leads to a smaller direct effect. Then from footnote 35, this implies $\pi_S^0 = \pi_{11} + \pi_{10} > 1/2$. Note the following: $\Delta = \frac{p_1\pi_{11}+p_0\pi_{01}}{p_1(\pi_{11}+\pi_{10})+p_0(1-\pi_{11}-\pi_{10})} - (\pi_{11} + \pi_{01}) = \frac{(p_1-p_0)[\pi_{11}-(\pi_{11}+\pi_{01})(\pi_{11}+\pi_{00})]}{p_1(\pi_{11}+\pi_{10})+p_0(1-\pi_{11}-\pi_{10})}$. Next using the fact that $\pi_{11} + \pi_{01} = 1 - \pi_{00} - \pi_{10}$, this expression can be written as $\Delta = \frac{(p_1-p_0)[-\pi_{10}+(\pi_{11}+\pi_{10})(\pi_{10}+\pi_{00})]}{p_1(\pi_{11}+\pi_{10})+p_0(1-\pi_{11}-\pi_{10})}$. Now 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^0. 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.

improvements in one with changes in the other. Second, to the extent that such negative correlation can be micro-founded 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, the Bayesian model makes additional predictions, in particular, related to the magnitudes of responses of direct and indirect effects depending on priors. We show in Section 5.4 that these predictions are not supported by our empirical work.

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 provides a better match to the ideas discussed in the Introduction and to our setting, and also crucially implies a change in beliefs concerning the non-state actor — a pattern we document in the data. We next present such a model.

6.4 Motivated Reasoning

We now present a simple variation on the utility function in (5) that incorporates motivated reasoning.[37] To isolate the effects of motivated reasoning, we develop this model when preferences are quasi-linear and there is no negative correlation in the priors concerning the quality of the state and non-state 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 conditional on 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:

$$\max_{c,\{T_i,\pi_i\}_{i\in\{S,N\}}} c\psi + \pi_S h_S(T_S) - d[U(\pi_S,\pi_N) - U(\pi_S^B,\pi_N^B)],\psi \tag{11}$$

again subject to (6). Note that, compared to (7), 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 towards beliefs that are more favorable towards 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,

[37] This model builds on the works cited in footnote 5, but develops a somewhat more general, even if reduced-form, model motivated reasoning, which elucidates the forces at work.

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 (7) when the individual's beliefs are π_S and π_N, and his utility under Bayesian beliefs is $U(\pi_S^B, \pi_N^B)$, 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 further apart from $U(\pi_S^B, \pi_N^B)$.[38] Overall, this objective function captures in a simple way the trade-off between the direct utility of congruence between beliefs and actions versus the cost of manipulating beliefs in terms of departures from the best ex-post decisions (which would give utility $U(\pi_S^B, \pi_N^B)$).

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

Proposition 2 *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 1, which defines

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

Now substituting this into (11), 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. ■

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 given π_S^B and π_N^B now 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 in turn leads to lower contributions to the non-state actor, T_N. Intuitively, the convex penalty for the deviation of beliefs from their Bayesian

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

counterparts implies that incentives to improve beliefs about the state also create incentives to make beliefs about the non-state actor more realistic (and less positive).[39]

Finally, we also note that because of the complementarity between behavior and beliefs, the maximization problem of the individual is potentially non-convex and may have multiple solutions (this is the reason why we focus attention to 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 statics emphasized in Proposition 2, 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 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 text: as the individual interacts more with the state actor, he becomes more positive about this actor and less positive about the non-state actor, and this motivates further increases in interactions with the state actor and additional reductions of relations with the non-state actor, and so on.

[39]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 non-state actor will make them closer to their Bayesian counterpart.

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Main Figures and Tables

Table 1: **Direct Effects**

	(1) Expected Usage	(2) Allocation in Fund Dictator Game	(3) Allocation in Investment Game
Panel A: Effects of State Positive Treatment on State Courts			
Post-Treatment	0.80	15.41	14.59
	(0.05)	(1.30)	(1.34)
Constant	4.06	104.82	115.39
	(0.03)	(0.92)	(0.95)
Panel B: Effects of Social Experimenter Treatment on State Courts			
Post-Treatment	0.28	1.72	2.23
	(0.05)	(1.66)	(1.81)
Constant	3.23	100.32	103.26
	(0.04)	(1.18)	(1.28)
Panel C: Netting out Social Experimenter Effects			
Post × State Positive	0.69	12.54	16.05
	(0.10)	(2.78)	(2.90)
Observations (Respondents):			
Panel A:	3812 (1906)	3918 (1959)	3938 (1969)
Panel B:	1702 (851)	1822 (911)	1806 (903)
Panel C:	5514 (2757)	5740 (2870)	5744 (2872)

Notes: This table estimates the effects of the state positive treatment on expected usage and game allocations for state courts. Post-Treatment is a dummy for post-treatment observations. Expected Usage is the likelihood of using state courts, with values between 0 and 10. Allocations in the Fund Dictator and Investment Games are the amounts the respondent allocated to state courts in the two games. Panel A includes the respondents who received the state positive treatment, Panel B include 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 post-treatment dummy and strata fixed effects interacted with the post-treatment dummy to control for varying sampling probabilities across strata. The constants in Panels A and B represent the pre-treatment values of the relevant variables. Observation counts vary because of differences in response rates and small changes in survey questions. Standard errors reported in parentheses.

Table 2: **Indirect Effects**

	(1) Expected Usage	(2) Allocation in Fund Dictator Game	(3) Allocation in Investment Game
Panel A: Effects of State Positive Treatment on Panchayats			
Post-Treatment	-0.55	-10.42	-12.13
	(0.04)	(1.30)	(1.35)
Constant	6.64	103.99	116.68
	(0.03)	(0.92)	(0.96)
Panel B: Effects of Social Experimenter Treatment on Panchayats			
Post-Treatment	-0.24	-2.84	-3.23
	(0.06)	(1.87)	(1.82)
Constant	7.01	109.31	116.15
	(0.04)	(1.32)	(1.29)
Panel C: Netting out Social Experimenter Effects			
Post × State Positive	-0.38	-4.82	-6.81
	(0.09)	(2.87)	(2.93)
Observations (Respondents):			
Panel A:	3810 (1905)	3918 (1959)	3938 (1969)
Panel B:	1700 (850)	1822 (911)	1806 (903)
Panel C:	5510 (2755)	5740 (2870)	5744 (2872)

Notes: This table estimates the (indirect) effects of the state positive treatment on expected usage and game allocations for panchayats. Post-Treatment is a dummy for post-treatment observations. Expected Usage is the likelihood of using panchayats, with values between 0 and 10. Allocations in the Fund Dictator and Investment Games are the amounts the respondent allocated to panchayats in the two games. Panel A includes the respondents who received the state positive treatment, Panel B include 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 post-treatment dummy and strata fixed effects interacted with the post-treatment of dummy to control for varying sampling probabilities across strata. The constants in Panels A and B represent the pre-treatment values of the relevant variables. Standard errors reported in parentheses.

Table 3: **Effects of State Positive Treatment in Anonymous Games**

	(1) Expected Usage	(2) Allocation in Fund Dictator Game
Panel A: Direct Effects (on State Courts)		
Post-Treatment	1.63	34.46
	(0.10)	(2.84)
Constant	3.86	74.30
	(0.07)	(2.01)
Panel B: Indirect Effects (on Panchayats)		
Post-Treatment	-0.66	-21.29
	(0.08)	(3.64)
Constant	6.47	103.21
	(0.06)	(2.57)
Observations (Respondents):		
Panel A:	498 (249)	498 (249)
Panel B:	498 (249)	498 (249)

Notes: 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. Post-Treatment is a dummy for post-treatment observations. For the definitions of Expected usage and Allocation in the Fund Dictator Game see 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 pre-treatment values of the relevant variables. Standard errors reported in parentheses.

Table 4: **Heterogeneity by Trust in the Information**

	(1) Expected Usage	(2) Allocation in Fund Dictator Game	(3) Allocation in Investment Game
Panel A: Direct Effects (on State Courts)			
Post-Treatment	0.74	13.45	11.86
	(0.09)	(2.36)	(2.63)
Post × High Trust	0.68	13.97	12.84
	(0.11)	(2.79)	(3.11)
Constant	3.75	101.06	109.51
	(0.03)	(0.89)	(0.99)
Panel B: Indirect Effects (on Panchayats)			
Post-Treatment	-0.41	-8.01	-6.53
	(0.08)	(2.54)	(2.70)
Post × High Trust	-0.37	-8.58	-10.03
	(0.10)	(3.06)	(3.26)
Constant	6.48	106.80	116.21
	(0.03)	(1.00)	(1.07)
Observations (Respondents):			
Panel A:	3902 (1951)	4008 (2004)	3532 (1766)
Panel B:	3404 (1702)	3510 (1755)	3034 (1517)

Notes: 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 greater than or equal to 6 (on a scale from 1 to 10). Post-Treatment is a dummy for post-treatment observations. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see 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 pre-treatment values of the relevant variables. Standard errors reported in parentheses.

Table 5: **Varying Credibility of Information**

	First Stage	Direct Effects (on State Courts)			Indirect Effects (on Panchayats)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Trust in Information	Expected Usage	Allocation in Fund Dictator Game	Allocation in Investment Game	Expected Usage	Allocation in Fund Dictator Game	Allocation in Investment Game
Panel A: Effects of High Credibility Treatment							
Post-Treatment		1.55	40.00	42.53	-0.89	-25.54	-16.94
		(0.11)	(3.83)	(3.22)	(0.11)	(3.24)	(2.94)
Constant	7.33	4.47	96.83	101.61	5.65	105.32	108.49
	(0.11)	(0.08)	(2.71)	(2.28)	(0.08)	(2.29)	(2.08)
Panel B: Effects of Low Credibility Treatment							
Post-Treatment		1.07	30.48	25.83	-0.65	-24.06	-19.89
		(0.10)	(3.00)	(2.63)	(0.10)	(3.25)	(2.64)
Constant	6.01	4.01	90.53	95.88	6.28	105.67	120.05
	(0.13)	(0.07)	(2.12)	(1.86)	(0.07)	(2.30)	(1.87)
Panel C: Differential Effects of High Credibility Treatment							
Post-Treatment		1.07	30.48	25.83	-0.65	-24.06	-19.89
		(0.11)	(3.43)	(2.93)	(0.11)	(3.24)	(2.79)
Post × Credibility	1.32	0.48	9.52	16.70	-0.25	-1.47	2.96
	(0.17)	(0.15)	(4.86)	(4.16)	(0.15)	(4.59)	(3.95)
Constant	6.01	4.24	93.67	98.74	5.97	105.50	114.29
	(0.12)	(0.05)	(1.72)	(1.47)	(0.05)	(1.62)	(1.40)
Observations (Respondents):							
Panel A:	374 (187)	374 (187)	374 (187)	374 (187)	374 (187)	374 (187)	374 (187)
Panel B:	372 (186)	372 (186)	372 (186)	372 (186)	372 (186)	372 (186)	372 (186)
Panel C:	746 (373)	746 (373)	746 (373)	746 (373)	746 (373)	746 (373)	746 (373)

Notes: 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 multiple sources), Panel B includes respondents who received the low credibility treatment (from private TV channels), and Panel C includes all respondents. Credibility is a dummy for receiving the high credibility treatment. Post-Treatment is a dummy for post-treatment observations. For the definitions of Expected Usage and Allocations in the Fund Dictator and Investment Games, see notes to Tables 1 and 2. All regressions include individual fixed effects. The constants in Panels A and B represent the pre-treatment values of the relevant variables. Standard errors reported in parentheses.

Table 6: **Effect of State Positive Treatment (Cross-Subject Design)**

	Model 1			Model 2		
	(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
Panel A: Direct Effects (on State Courts)						
State Positive	0.82	19.71	21.31	0.70	14.33	17.53
	(0.19)	(3.93)	(3.91)	(0.10)	(2.60)	(2.68)
Pre-Treatment Value				0.86	0.75	0.72
				(0.01)	(0.01)	(0.01)
Constant	3.88	101.00	107.67	0.67	27.13	30.17
	(0.15)	(3.05)	(3.05)	(0.08)	(2.36)	(2.49)
Panel B: Indirect Effects (on Panchayats)						
State Positive	-0.65	-3.41	-3.10	-0.41	-4.41	-5.76
	(0.19)	(3.88)	(3.94)	(0.09)	(2.61)	(2.69)
Pre-Treatment Value				0.89	0.71	0.71
				(0.01)	(0.01)	(0.01)
Constant	6.75	99.99	109.31	0.59	25.72	27.87
	(0.15)	(3.02)	(3.08)	(0.10)	(2.39)	(2.54)
Observations (Respondents):						
Panel A:	2757 (2757)	2870 (2870)	2872 (2872)	2757 (2757)	2870 (2870)	2872 (2872)
Panel B:	2755 (2755)	2870 (2870)	2872 (2872)	2755 (2755)	2870 (2870)	2872 (2872)

Notes: This table estimates the direct and indirect effects of the state positive treatment using a cross-subject design. The sample includes only post-treatment observations. State Positive is a dummy for the respondents who received the state positive treatment. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see notes to Tables 1 and 2. Model 1 in columns 1-3 reports pure cross-sectional regressions. Model 2 in columns 4-6 additionally controls for the pre-treatment value of the dependent variable on the right-hand side. Panel A reports effects on state courts, while Panel B reports (indirect) effects on panchayats. All regressions include individual fixed effects. Standard errors reported in parentheses.

Table 7: **Beliefs**

	(1) Service Effectiveness	(2) Enforcement Effectiveness	(3) Access	(4) Trust
Panel A: Direct Effects (on State Courts)				
Post-Treatment	0.87	0.54	0.60	0.87
	(0.04)	(0.03)	(0.04)	(0.04)
Constant	3.95	6.55	2.96	4.86
	(0.03)	(0.02)	(0.03)	(0.03)
Panel B: Indirect Effects (on Panchayats)				
Post-Treatment	-0.17	-0.16	0.07	-0.24
	(0.03)	(0.02)	(0.03)	(0.03)
Constant	5.41	6.26	7.93	6.53
	(0.02)	(0.02)	(0.02)	(0.02)
Observations (Respondents):				
Panel A:	3371 (1686)	3371 (1686)	3371 (1686)	3371 (1686)
Panel B:	2888 (1444)	2885 (1443)	2888 (1445)	2886 (1443)

Notes: This table estimates the direct and indirect effects of the state positive treatment on belief and trust questions. Post-Treatment is a dummy for post-treatment 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 difficult 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 reported in parentheses.

Table 8: **Effect of State Positive Treatment with Local Sports Clubs**

	(1) Expected Usage	(2) Allocation in Fund Dictator Game	(3) Allocation in Investment Game
Panel A: Direct Effects (on State Courts)			
Post-Treatment	1.94	35.88	30.16
	(0.12)	(2.86)	(2.96)
Constant	3.87	101.16	110.52
	(0.09)	(2.02)	(2.09)
Panel B: Indirect Effects (on Sports Club)			
Post-Treatment	0.21	3.60	5.20
	(0.06)	(2.25)	(2.11)
Constant	6.56	133.16	134.92
	(0.04)	(1.59)	(1.49)
Panel C: Netting Out Indirect Effects			
Post-Treatment	0.21	3.60	5.20
	(0.18)	(3.93)	(3.60)
State	-2.70	-32.00	-24.40
	(0.18)	(3.93)	(3.60)
Post × State	1.73	32.28	24.96
	(0.25)	(5.55)	(5.09)
Constant	6.56	133.16	134.92
	(0.12)	(2.78)	(2.54)
Observations (Respondents):			
Panel A:	500 (250)	500 (250)	500(250)
Panel B:	500 (250)	500 (250)	500(250)
Panel C:	1000 (250)	1000 (250)	1000(250)

Notes: This table estimates the direct and indirect effects of the state positive treatment on expected usage and game allocations in a design that replaces panchayat with local sports clubs. Post-Treatment is a dummy for post-treatment observations. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see 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 to be 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 pre-treatment values of the relevant variables. Standard errors reported in parentheses.

Table 9: **Heterogeneity by Priors**

	Model 1			Model 2		
	(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
Panel A: Direct Effects (on State Courts)						
Post-Treatment	1.79	26.46	21.45	1.45	16.23	13.72
	(0.13)	(3.78)	(4.12)	(0.14)	(4.14)	(4.47)
Post × Initial State	-1.79	-11.56	-4.82	-1.36	15.28	27.91
	(0.13)	(3.80)	(4.10)	(0.43)	(12.38)	(13.54)
Post × Initial State Squared				-0.49	-29.43	-35.40
				(0.43)	(12.42)	(13.50)
Post × Initial Non-State	-0.15	-2.16	-3.52	1.87	49.26	29.42
	(0.13)	(3.84)	(4.17)	(0.50)	(14.38)	(15.60)
Post × Initial Non-State Squared				-1.86	-47.30	-30.37
				(0.44)	(12.78)	(13.97)
Constant	4.07	100.41	113.04	4.07	100.41	113.04
	(0.03)	(0.77)	(0.84)	(0.03)	(0.76)	(0.84)
Panel B: Indirect Effects (on Panchayats)						
Post-Treatment	0.27	-11.49	-6.41	0.43	-5.51	-2.90
	(0.12)	(3.86)	(4.06)	(0.13)	(4.24)	(4.42)
Post × Initial State	-0.13	-0.00	-2.61	-0.13	13.40	14.73
	(0.12)	(3.88)	(4.04)	(0.39)	(12.68)	(13.39)
Post × Initial State Squared				0.01	-13.39	-17.50
				(0.39)	(12.72)	(13.35)
Post × Initial Non-State	-1.24	-2.95	-8.69	-2.34	-53.50	-47.11
	(0.12)	(3.92)	(4.11)	(0.46)	(14.73)	(15.42)
Post × Initial Non-State Squared				1.01	46.57	35.69
				(0.40)	(13.09)	(13.81)
Constant	6.53	103.99	115.29	6.53	103.99	115.29
	(0.02)	(0.78)	(0.83)	(0.02)	(0.78)	(0.83)
Observations (Respondents):						
Panel A:	5056 (2528)	5056 (2528)	4558 (2279)	5056 (2528)	5056 (2528)	4558 (2279)
Panel B	5054 (2527)	5056 (2528)	4558 (2279)	5054 (2527)	5056 (2528)	4558 (2279)

Notes: This table estimates heterogeneous direct and indirect effects by priors. We proxy priors by pre-treatment expected usage for the relevant forum. Post-Treatment is a dummy for post-treatment observations. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see notes to Tables 1 and 2. Model 1 in columns 1-3 only includes linear interactions, while Model 2 in columns 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 reported in parentheses.

Appendix B: Additional Figures and Tables

Figure B1: **Boards Used for Field Games**

(a) Board with game allocation slots closed.

(b) Board with game allocation slots open.

Table B1: **Summary of Key Variables**

	Mean	Std. Dev.	N	Round
Demographic Variables:				
Age	37.88	12.03	3,856	1 & 2
Income	17,001.42	68,015.62	1,629	1a
Formal Education	0.76	0.43	3,858	1 & 2
Asset Index	-0.01	0.80	2,151	1a
Landlord	0.28	0.45	2,986	1
Service Usage:				
Gov. Health Center	0.39	0.49	2,159	1a
Gov. Education	0.44	0.50	2,159	1a
Police	0.20	0.40	2,159	1a
Courts	0.21	0.41	2,159	1a
Civic Values:				
Helped Neighbor	0.50	0.50	2,156	1a
Resolved Dispute	0.40	0.49	2,155	1a
Civic Donation	0.06	0.24	2,155	1a
NGO Member	0.06	0.23	2,153	1a
State Outcomes:				
Self:				
Expected Usage	3.86	3.53	3,629	1 & 2
Investment Game Allocation	110.13	77.84	3,495	1 & 2*
Fund Dictator Game Allocation	100.34	76.00	3,742	1 & 2
Perceptions:				
Service Effectiveness	3.95	3.05	1,681	1b & 2
Enforcement Effectiveness	6.55	2.86	1,692	1b & 2
Access	2.96	3.01	1,697	1b & 2
Trust	4.86	3.26	1,685	1b & 2
Non-State Outcomes:				
Self:				
Expected Usage	6.65	3.52	3,379	1 & 2*
Investment Game Allocation	116.26	79.45	3,245	1 & 2*
Fund Dictator Game Allocation	105.48	78.89	3,492	1 & 2*
Perceptions:				
Service Effectiveness	5.41	3.34	1,444	1b & 2*
Enforcement Effectiveness	6.26	2.96	1,442	1b & 2*
Access	7.93	3.12	1,445	1b & 2*
Trust	6.53	3.26	1,443	1b & 2*

Notes: This table presents summary statistics on respondent demographics, as well as baseline values for our primary outcome variables. Income given as household monthly income (and was only for part of the first round since it raised sensitivities). Asset index is calculated using factor analysis on number of chairs, beds, motorcycles, bicycles, buffaloes, goats, radios, televisions, and heaters the respondent's household owned. Landlord is an indicator for if the respondent owns land. Service usage questions are indicators for if the respondent had used the given service in the three months prior to surveying. Similarly, civic values questions ask respondents if they have, in the three months prior to surveying, helped their neighbor (e.g. harvesting, building, or home repairs), helped resolve a community dispute, donated (money or time) to a non-governmental social welfare/civic organization, or held membership in an NGO. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see notes to Tables 1 and 2. For definitions of Service Effectiveness, Enforcement Effectiveness, Access, and Trust see notes to Table 7. Column 4 shows which survey round each variable was asked in. 1 refers to all of the first round, 2 refers to all of the second round. 1A refers to the first part of the first round where only the state positive treatment was administered. 1B refers to the subsequent part of the first round where the social experimenter, social multiplier, and state positive treatments were all administered, as well as some survey questions added or removed. 2* refers to only those games in round two which measured the listed outcome (recall, not all actors and games were present in all of the second round, e.g. there was no investment game in anonymous games). Observation counts vary mildly within groups due to some non-responsiveness.

Table B2: **Summary of Key Variables by Survey Round**

	First Round			Second Round		
	Mean	**Std. Dev.**	**N**	**Mean**	**Std. Dev.**	**N**
Demographic Variables:						
Age	37.57	12.38	2,984	38.94	10.66	872
Formal Education	0.73	0.44	2,986	0.85	0.36	872
State Outcomes:						
Self:						
Expected Usage	3.81	3.76	2,757	4.02	2.66	872
Investment Game Allocation	111.57	79.63	2,872	103.47	68.65	623
Fund Dictator Game Allocation	103.39	78.32	2,870	90.29	66.86	872
Perceptions:						
Service Effectiveness	4.12	3.39	810	3.79	2.69	871
Enforcement Effectiveness	6.73	3.19	820	6.39	2.50	872
Access	3.06	3.48	825	2.85	2.48	872
Trust	5.16	3.71	817	4.58	2.75	868
Non-State Outcomes:						
Self:						
Expected Usage	6.76	3.65	2,757	6.17	2.86	622
Investment Game Allocation	116.51	80.94	2,872	114.29	66.98	373
Fund Dictator Game Allocation	105.68	80.79	2,870	104.58	69.51	622
Perceptions:						
Service Effectiveness	5.37	3.56	822	5.45	3.03	622
Enforcement Effectiveness	6.23	3.10	820	6.30	2.76	622
Access	7.74	3.51	823	8.17	2.51	622
Trust	6.51	3.49	822	6.56	2.91	621

Notes: This table presents summary statistics separately by the two survey rounds for respondents' demographics (for variables present in both rounds), as well as baseline summary statistics for our primary outcome variables. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see notes to Tables 1 and 2. For definitions of Service Effectiveness, Enforcement Effectiveness, Access, and Trust see notes to Table 7. Observation counts change due to some non-responsiveness and changes in survey instrument over time.

Table B3: **Main Results with Differential Demographic Trends**

	Direct Effect (on State Courts)			Indirect Effect (on Panchayats)		
	(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
Panel C: Netting Out Social Experimenter Effect						
Post × State Positive	0.63	13.18	16.24	-0.36	-4.41	-7.40
	(0.13)	(3.50)	(3.73)	(0.11)	(3.54)	(3.63)
Observations (Respondents):	2798 (1399)	3020 (1510)	3026 (1513)	2798 (1399)	3020 (1510)	3026 (1513)
R-Squared	0.018	0.009	0.010	0.009	0.003	0.006
p-value for F-test of all demographics	0.341	0.655	0.300	0.760	0.365	0.058
p-value for F-test of first round demographics	0.471	0.867	0.379	0.574	0.372	0.107
p-value for equality of treatment effects with Tables 1 and 2	0.697	0.889	0.968	0.871	0.933	0.898

Notes: This table examines robustness of our results from Tables 1 and 2 to differential demographic trends. This table uses all first round respondents to net out the social experimenter effect from the state positive effect. All regressions include individual fixed effects, strata fixed effects interacted with a post indicator, and demographic fixed effects interacted with a post indicator. The demographics included in these regressions are: age, income, whether the respondent has any formal education, an asset index, and whether the respondent owns any land. For definitions of these variables see notes for Table B1. P-value for F-test of all demographics shows the p-value of the test of joint significance on all demographic variables. P-value for F-test of first round demographics shows the p-value of the test of joint significance of only those demographic questions that were dropped from the second round. These are income, the asset index, and landlord status. Finally, p-value for equality of treatment effects with Tables 1&2 shows the p-value of the test of equality with Panel C of table 1 for columns 1-3 and with Panel C of tables 2 for columns 4-6. Standard errors reported in parentheses.

Table B4: **Baseline Balance on Outcomes and Covariates, First Round**

	State Positive	Social Experimenter	Balance	
	(1) Mean	(2) Mean	(3) Difference	(4) p-value
Demographics:				
Age	37.84	36.99	0.85	0.16
Income	16023.06	18258.34	-2235.28	0.55
Formal Education	0.73	0.71	0.02	0.44
Asset Index	-0.03	0.02	-0.04	0.26
Landlord	0.29	0.26	0.03	0.13
Service Usage:				
Gov. Health Center	0.39	0.39	0.01	0.79
Gov. Education	0.44	0.44	-0.00	0.99
Police	0.21	0.20	0.01	0.74
Courts	0.20	0.22	-0.02	0.28
Civic Values:				
Helped Neighbor	0.51	0.49	0.01	0.58
Resolved Dispute	0.39	0.41	-0.02	0.36
Civic Donation	0.06	0.07	-0.01	0.41
NGO Member	0.06	0.05	0.01	0.44
State Outcomes:				
Self:				
Expected Usage	3.85	3.71	0.14	0.48
Investment Game Allocation	113.23	107.97	5.26	0.19
Fund Dictator Game Allocation	105.67	98.50	7.17	0.07
Non-State Outcomes:				
Self:				
Expected Usage	6.68	6.94	-0.26	0.16
Investment Game Allocation	117.68	113.97	3.71	0.36
Fund Dictator Game Allocation	106.13	104.72	1.41	0.73

Notes: This table presents balance checks on the respondent-level treatment randomization between the state positive and social experimenter treatments. Strata fixed effects are included due to treatment assignment probability varying by strata. For definitions of the variables please see notes for Table B1. All questions available in this round from Table B1 are included. Column 1 reports the mean for the respondents who received the state positive treatment, column 2 reports the mean and observations for the respondents who received the social experimenter treatment, column 3 shows the difference between the means of these two groups, and column 4 shows the p-value for a t-test measuring if that difference is different from zero.

Table B5: **Anonymous Game Differential Effects (Second Round Games Only)**

	Direct Effects (on State Courts)		Indirect Effects (on Panchayats)	
	(1) Expected Usage	(2) Allocation in Fund Dictator Game	(3) Expected Usage	(4) Allocation in Fund Dictator Game
Panel A: Effects of Anonymous Games				
Post-Treatment	1.63	34.46	-0.66	-21.29
	(0.10)	(2.84)	(0.08)	(3.64)
Constant	3.86	74.30	6.47	103.21
	(0.07)	(2.01)	(0.06)	(2.57)
Panel B: Effects of All Other Games in Second Round				
Post-Treatment	1.56	35.49	-0.77	-24.80
	(0.07)	(1.86)	(0.08)	(2.29)
Constant	4.09	96.68	5.97	105.50
	(0.05)	(1.31)	(0.05)	(1.62)
Panel C: Differential Effects				
Post-Treatment	1.56	35.49	-0.77	-24.80
	(0.07)	(1.84)	(0.07)	(2.58)
Post × Anonymous	0.06	-1.03	0.11	3.51
	(0.13)	(3.44)	(0.12)	(4.09)
Constant	4.02	90.29	6.17	104.58
	(0.04)	(1.10)	(0.04)	(1.42)
Observations (Respondents):				
Panel A:	498 (249)	498 (249)	498 (249)	498 (249)
Panel B:	1246 (623)	1246 (623)	746 (373)	746 (373)
Panel C:	1744 (872)	1744 (872)	1244 (622)	1244 (622)

Notes: This table examines the effect of the state positive treatment on our main outcomes for respondents playing the anonymized game, compared to those playing the sports and credibility games, in the second round. Columns 1-2 show the treatment effect on state courts and columns 3-5 show the treatment effect on panchayats. Post-Treatment is a dummy for post-treatment observations. For the definitions of Expected usage and Allocation in the Fund Dictator Game see notes to Tables 1 and 2. Panel A reports our main outcomes for those respondents who played the anonymized game, and Panel B reports the same outcomes for respondents who played the credibility and sports games. Panel C uses both set of respondents to show the differential effect of anonymized games. Regressions in all panels include individual fixed effects. Standard errors reported in parentheses.

Table B6: **Baseline Balance on Outcomes and Covariates by Credibility Games Treatment Arm**

	High Credible	Low Credible	Balance		Balance with Age Controls	
	(1) Mean	(2) Mean	(3) Difference	(4) p-value	(5) Difference	(6) p-value
Demographic Variables:						
Age	41.09	38.95	2.14	0.04	0.00	.
Formal Education	0.88	0.89	-0.01	0.76	-0.01	0.84
State Outcomes:						
Self:						
Expected Usage	4.01	4.47	-0.46	0.10	-0.31	0.30
Investment Game Allocation	95.88	101.61	-5.73	0.41	-3.60	0.63
Fund Dictator Game Allocation	90.53	96.83	-6.29	0.38	-2.25	0.77
Perceptions:						
Service Effectiveness	3.67	3.58	0.09	0.74	0.14	0.64
Enforcement Effectiveness	6.21	6.35	-0.14	0.60	0.00	1.00
Access	2.95	3.03	-0.08	0.75	-0.11	0.69
Trust	4.32	4.58	-0.26	0.36	-0.09	0.76
Non-State Outcomes:						
Self:						
Expected Usage	6.28	5.65	0.63	0.03	0.52	0.10
Investment Game Allocation	120.05	108.49	11.56	0.10	6.60	0.36
Fund Dictator Game Allocation	105.67	105.32	0.35	0.96	-7.24	0.34
Perceptions:						
Service Effectiveness	5.95	5.17	0.78	0.01	0.61	0.06
Enforcement Effectiveness	6.50	6.22	0.28	0.34	0.32	0.31
Access	8.32	7.99	0.33	0.21	0.08	0.76
Trust	6.72	6.06	0.66	0.03	0.55	0.09

Notes: This table presents balance checks on the respondent-level variables between the low and high credible treatments in the second round. Column 1 reports the mean for the respondents who received the high credible treatment, column 2 reports the mean for respondents who received the low credible treatment, column 3 shows the difference between the means of these two groups, column 4 shows the p-value for a t-test measuring if that difference is different from zero, column 5 again shows the differences between the two groups after controlling for differences in age, and column 6 shows the p-value for a t-test measuring if that difference is different from zero when including age controls.

Table B7: **Varying Credibility of Information (With Age Controls)**

	Direct Effects (on State Courts)			Indirect Effects (on Panchayats)		
	Expected Usage	Allocation in Fund Dictator Game	Allocation in Investment Game	Expected Usage	Allocation in Fund Dictator Game	Allocation in Investment Game
Post-Treatment	1.00	50.00	50.00	1.00	-50.00	-50.00
	(1.50)	(48.16)	(41.02)	(1.42)	(44.62)	(38.13)
Post × Credibility	0.45	10.92	17.10	-0.18	-1.54	2.17
	(0.17)	(5.32)	(4.53)	(0.16)	(4.93)	(4.21)
Constant	4.24	93.67	98.74	5.97	105.50	114.29
	(0.06)	(1.76)	(1.50)	(0.05)	(1.63)	(1.40)
Observations (Respondents):	746 (373)	746 (373)	746 (373)	746 (373)	746 (373)	746 (373)

Notes: This table is analogous to Panel C of Table 5, with additional controls for age effects. Columns 1-3 show the treatment effects on state courts and columns 4-6 show the (indirect) treatment effects on panchayats. Credibility is a dummy for receiving the high credibility treatment. Post-Treatment is a dummy for post-treatment observations. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see notes to Tables 1 and 2. Regressions in all panels include individual fixed effects. Standard errors reported in parentheses.

Table B8: **Views about Others**

	State Positive			Netting Social Experimenter		
	(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
Panel A: Direct Effects (on State Courts)						
Post-Treatment	0.66	13.60	12.65			
	(0.04)	(1.09)	(1.10)			
Post-Treatment × State Positive				0.59	8.22	13.20
				(0.08)	(2.68)	(2.64)
Constant	5.12	113.12	122.04			
	(0.03)	(0.77)	(0.78)			
Panel B: Indirect Effects (on Panchayats)						
Post-Treatment	-0.46	-7.73	-10.33			
	(0.04)	(1.13)	(1.07)			
Post-Treatment × State Positive				-1.32	-7.73	-10.05
				(0.59)	(2.77)	(2.64)
Constant	6.01	112.46	119.88			
	(0.03)	(0.80)	(0.76)			
Observations (Respondents):						
Panel A:	4028 (2014)	3496 (1748)	3492 (1746)	5946 (2973)	4852 (2426)	4842 (2421)
Panel B:	4024 (2012)	3500 (1750)	3496 (1748)	5944 (2972)	4856 (2428)	4850 (2425)

Notes: This table examines the effect of the state positive treatment on alternative outcomes concerning our respondents' views about others. Columns 1-3 report results of regression analogous to those from Panel A of Table 1, while columns 4-6 report results from regressions analogous to those from Panel C of Table 1. Post-Treatment is a dummy for post-treatment observations. Panel A reports effects on state courts, while Panel B reports (indirect) effects on panchayats. Expected Usage asks respondents how likely (0-10) they believe others are to use a forum, Fund Dictator Game Allocation is the amount the respondent believes others would allocate to the forum in the Fund Game, and Investment Allocation is the amount the respondent believes others would allocate in the Investment Game. Observation counts vary slightly due to response rate differences and small changes in survey questions. All regressions include individual fixed effects. Regressions in columns 4-6 also includes strata fixed effects interacted with a post indicator to account for strata level differences in sampling probabilities. Standard errors reported in parentheses.

Table B9: **Dropping Observations with Poor Understanding**

	(1) Expected Usage	(2) Allocation in Fund Dictator Game	(3) Allocation in Investment Game
Panel A: Direct Effects (on State Courts)			
Post-Treatment	0.80	15.27	14.72
	(0.05)	(1.33)	(1.38)
Constant	4.13	105.74	116.00
	(0.03)	(0.94)	(0.97)
Panel B: Indirect Effects (on Panchayats)			
Post-Treatment	-0.55	-10.05	-12.04
	(0.04)	(1.33)	(1.40)
Constant	6.64	103.06	116.73
	(0.03)	(0.94)	(0.99)
Observations (Respondents):			
Panel A:	3632 (1816)	3716 (1858)	3752 (1876)
Panel B:	3630 (1815)	3716 (1858)	3752 (1876)

Notes: This table examines the effect of the state positive treatment on our main outcomes, dropping observations that could potentially be low-quality. After playing each game we had both the surveyor and respondent rank the respondent's understanding of the game on a scale of 1-10. These regressions drop respondents who received below a 5 from either source. Post-Treatment is a dummy for post-treatment observations. Panel A reports effects on state courts, while Panel B reports (indirect) effects on panchayats. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see notes to Tables 1 and 2. Observation counts vary slightly due to response rate differences across the different dependent variables. Sample is restricted to those who received the state positive treatment. All regressions include individual fixed effects. Standard errors reported in parentheses.

Table B10: **Robust to Outliers**

	(1) Expected Usage	(2) Allocation in Fund Dictator Game	(3) Allocation in Investment Game
Panel A: Direct Effects (on State Courts)			
Post-Treatment	0.85	17.53	16.89
	(0.13)	(2.79)	(2.85)
Constant	4.01	100.73	112.83
	(0.09)	(1.97)	(2.01)
Panel B: Indirect Effects (on Panchayats)			
Post-Treatment	-0.64	-10.73	-13.35
	(0.13)	(2.72)	(2.73)
Constant	6.86	99.21	115.00
	(0.09)	(1.92)	(1.93)
Observations (Respondents):			
Panel A:	3812 (1906)	3918 (1959)	3938 (1969)
Panel B:	3810 (1905)	3918 (1959)	3938 (1969)

Notes: This table examines whether the effect of the state positive treatment on our main outcomes is robust to outliers using Stata's rreg command. Post-Treatment is a dummy for post-treatment observations. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see notes to Tables 1 and 2. Panel A reports effects on state courts, while Panel B reports (indirect) effects on panchayats. Observation counts vary slightly due to response rate differences and small changes in survey questions.

Stata's rreg command first eliminates gross outliers by performing an initial screening based on Cook's distance > 1. Then, an iteration process begins in which weights are calculated based on absolute residuals (both Huber iterations and biweight iterations are used, as suggested by Li(1985)). The iterating stops when the maximum change between the weights from one iteration to the next is below tolerance. With Stata defaults, robust regression is about 95 percent as effcient as OLS (Hamilton, 1991). Source: http://www.ats.ucla.edu/stat/stata/dae/rreg.htm.

Table B11: **Dropping Observations on the Boundary**

	(1) Expected Usage	(2) Allocation in Fund Dictator Game	(3) Allocation in Investment Game
Panel A: Direct Effects (on State Courts)			
Post-Treatment	0.72	13.72	14.56
	(0.05)	(1.20)	(1.22)
Constant	4.78	104.64	110.81
	(0.03)	(0.85)	(0.86)
Panel B: Indirect Effects (on Panchayats)			
Post-Treatment	-0.49	-8.91	-10.78
	(0.05)	(1.19)	(1.15)
Constant	5.49	102.84	111.96
	(0.03)	(0.84)	(0.82)
Observations (Respondents):			
Panel A:	2040 (1020)	2926 (1463)	2914 (1457)
Panel B	1816 (908)	2902 (1451)	2934 (1467)

Notes: This table re-examines the effect of the state positive treatment on our main outcomes, dropping observations that were potentially constrained from changing their initial outcome due to the bounded nature of our outcomes. These regressions omit observations where the respondent allocated the maximum (e.g. 250 PKR in either game) or minimum (e.g. 0 PKR in either game) to the relevant forum in the baseline. Post-Treatment is a dummy for post-treatment observations. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see 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. Standard errors reported in parentheses.

Table B12: **Effect of Forum Allocation Order**

	(1) Expected Usage	(2) Allocation in Fund Dictator Game	(3) Allocation in Investment Game
Panel A: Direct Effects (on State Courts)			
Post-Treatment	0.63	14.16	15.79
	(0.11)	(3.16)	(3.17)
Courts First in Pre × Post	0.02	5.85	-1.88
	(0.12)	(3.50)	(3.51)
Courts First in Post × Post	-0.01	-0.36	-0.84
	(0.12)	(3.50)	(3.51)
Constant	4.77	98.78	118.99
	(0.04)	(1.23)	(1.24)
Panel B: Indirect Effects (on Panchayats)			
Post-Treatment	-0.44	-12.53	-11.80
	(0.10)	(3.05)	(3.38)
Courts First in Pre × Post	-0.04	-2.98	-5.58
	(0.11)	(3.37)	(3.74)
Courts First Post × Post	0.02	2.42	3.91
	(0.11)	(3.37)	(3.74)
Constant	6.63	98.28	116.46
	(0.04)	(1.19)	(1.32)
Observations (Respondents):			
Panel A:	1650 (825)	1650 (825)	1650 (825)
Panel B:	1648 (824)	1650 (825)	1650 (825)

Notes: This table examines whether the state positive treatment has a differential impact based on whether respondents allocated to the court first. For a subset of our respondents we randomize the order in which they allocated to each forum, both pre-/post-treatment. Post-Treatment is a dummy for post-treatment observations. Pre-Court first is an indicator that in the pre-treatment games, the first game played was with the courts. Post-Court first is an indicator that in the post-treatment games, the first game played was with the courts. Panel A reports effects on state courts, while Panel B reports (indirect) effects on panchayats. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see notes to Tables 1 and 2. All regressions include individual fixed effects. Standard errors reported in parentheses.

Table B13: **Effect of the Social Multiplier Treatment**

	(1) Expected Usage	(2) Allocation in Fund Dictator Game	(3) Allocation in Investment Game
Panel A: Direct Effects (on State Courts)			
Post × Social	0.07	4.91	-2.17
	(0.12)	(3.52)	(3.53)
Post-Treatment	-0.03	1.58	24.08
	(0.33)	(9.95)	(10.00)
Constant	4.77	98.78	118.99
	(0.04)	(1.23)	(1.24)
Panel B: Indirect Effects (on Panchayats)			
Post × Social	0.04	-2.06	0.01
	(0.12)	(3.40)	(3.77)
Post-Treatment	-0.75	2.49	-4.62
	(0.33)	(9.61)	(10.65)
Constant	6.63	98.28	116.46
	(0.04)	(1.19)	(1.32)
Observations (Respondents):			
Panel A:	1650 (825)	1650 (825)	1650 (825)
Panel B:	1648 (824)	1650 (825)	1650 (825)

Notes: This table examines whether the state positive treatment has a differential impact depending on whether respondents also received the social multiplier treatment. Post-Treatment is a dummy for post-treatment observations. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see notes to Tables 1 and 2. Panel A reports effects on state courts, while Panel B reports (indirect) effects on panchayats. The observation numbers are lower as this social multiplier randomization was only introduced in a later part of only first round of surveys. All regressions include individual fixed effects. Standard errors reported in parentheses.

Table B14: **Effect of State Negative Treatment (Pilot)**

	(1) Other Usage	(2) Allocation in Investment Game
Panel A: Direct Effects (on State Courts)		
Post-Treatment	-0.68	-26.49
	(0.17)	(11.84)
Constant	3.16	100.54
	(0.12)	(8.37)
Panel B: Indirect Effects (on Panchayats)		
Post-Treatment	0.54	-3.24
	(0.18)	(13.86)
Constant	7.51	151.62
	(0.13)	(9.80)
Observations (Respondents):		
Panel A:	74 (37)	74 (37)
Panel B	74 (37)	74 (37)

Notes: This table examines the effect of the pilot state negative treatment on main outcomes. Post-Treatment is a dummy for post-treatment observations. For the definition of Allocation in the Investment Game see notes to Tables 1 and 2. For the definition of other's usage see notes to Table B8. Panel A reports effects on state courts, while Panel B reports (indirect) effects on panchayats. This treatment was only administered during the pilot phase, hence the lower observation count. All regressions include individual fixed effects. Standard errors reported in parentheses.

Table B15: **Effect of Non-State Negative Treatment (Pilot)**

	(1) Expected Usage	(2) Allocation in Fund Dictator Game	(3) Allocation in Investment Game
Panel A: Direct Effects (on Panchayats)			
Post-Treatment	0.65	11.97	6.83
	(0.15)	(4.62)	(4.37)
Constant	2.81	99.52	110.24
	(0.11)	(3.27)	(3.09)
Panel B: Indirect Effects (on State Courts)			
Post-Treatment	-0.69	-13.17	-12.30
	(0.15)	(4.67)	(4.54)
Constant	6.97	124.66	130.28
	(0.11)	(3.30)	(3.21)
Observations (Respondents):			
Panel A:	312 (156)	498 (249)	504 (252)
Panel B:	312 (156)	498 (249)	504 (252)

Notes: This table examines the effect of the pilot non-state negative treatment on our main outcomes. Post-Treatment is a dummy for post-treatment observations. For definitions of Expected Usage and Allocations in the Fund Dictator and Investment Games see notes to Tables 1 and 2. Panel A reports effects on state courts, while Panel B reports (indirect) effects on panchayats. This treatment was only administered during the pilot phase and the earliest round of the main surveys, hence the lower observation count. All regressions include individual fixed effects. Standard errors reported in parentheses.

Table B16: **Effect of Non-State Positive Treatment (Pilot)**

	(1) Other Usage	(2) Allocation in Investment Game
Panel A: Indirect Effects (on State Courts)		
Post-Treatment	-0.33	-10.00
	(0.21)	(11.13)
Constant	3.53	83.33
	(0.15)	(7.87)
Panel B: Direct Effects (on Panchayats)		
Post-Treatment	0.20	30.00
	(0.37)	(11.75)
Constant	7.40	143.33
	(0.26)	(8.31)
Observations (Respondents):		
Panel A:	30 (15)	30 (15)
Panel B	30 (15)	30 (15)

Notes: This table examines the effect of the pilot non-state positive treatment on our main outcomes. Post-Treatment is a dummy for post-treatment observations. For the definition of Allocation in the Investment Game see notes to Tables 1 and 2. For the definition of other's usage see notes to Table B8. Panel A reports effects on state courts, while Panel B reports (indirect) effects on panchayats. This treatment was only administered during the pilot phase, hence the lower observation count. All regressions include individual fixed effects. Standard errors reported in parentheses.

Table B17: **Alternative Belief Questions (Cross-Subject)**

	(1) Deliver Justice	(2) Enforce Verdict
Panel A: Direct Effects (on State Courts)		
State Positive	0.07	0.06
	(0.02)	(0.02)
Constant	0.41	0.57
	(0.02)	(0.02)
Panel B: Indirect Effects (on Panchayats)		
State Positive	-0.06	-0.03
	(0.02)	(0.02)
Constant	0.49	0.37
	(0.02)	(0.02)
Observations (Respondents):		
Panel A:	2160 (1080)	2160 (1080)
Panel B:	2160 (1080)	2160 (1080)

Notes: This table estimates the direct and indirect effects of the state positive treatment on alternative belief questions using a cross-subject design. The sample includes only post-treatment observations. State Positive is a dummy for the respondents who received the state positive treatment. Deliver Justice asked respondents "*Between the Panchayat and the court, which is most effective in delivering justice? *" and Enforce Verdict asked "*Between the Panchayat and the court, which is more able to enforce the verdict?*"Respondents were able to answer Court, Panchayat, both, or neither. Panel A reports effects on the share of respondents who answers only court, while Panel B shows effect on share who answered only Panchayat. Strata fixed effects included in all regressions. Standard errors reported in parentheses.

Table B18: **Belief Questions (Anonymous Games)**

	(1) Service Effectiveness	(2) Enforcement Effectiveness	(3) Access	(4) Trust
Panel A: Direct Effects (on State Courts)				
Post-Treatment	1.43	1.00	0.72	1.36
	(0.11)	(0.08)	(0.10)	(0.11)
Constant	3.72	6.22	2.63	4.40
	(0.08)	(0.06)	(0.07)	(0.08)
Panel B: Indirect Effects (on Panchayats)				
Post-Treatment	-0.33	-0.24	-0.13	-0.50
	(0.07)	(0.05)	(0.06)	(0.08)
Constant	5.30	6.22	8.19	6.80
	(0.05)	(0.04)	(0.04)	(0.06)
Observations (Respondents):				
Panel A:	494 (247)	494 (247)	494 (247)	494 (247)
Panel B:	498 (249)	498 (249)	498 (249)	496 (248)

Notes: This table estimates the direct and indirect effects of the state positive treatment on belief and trust questions for the anonymized games (in the second round) only. Post-Treatment is a dummy for post-treatment observations. For definitions of Service Effectiveness, Enforcement Effectiveness, Access, and Trust, see notes to Tables 7. Panel A reports effects on state courts, while Panel B reports (indirect) effects on panchayats. All regressions include individual fixed effects. Standard errors reported in parentheses.

Table B19: **Allegiance Belief Questions**

	(1) Allegiance Importance	(2) Allegiance Effort
Panel A: Direct Effects (on State Courts)		
Post-Treatment	-0.29	-0.38
	(0.03)	(0.04)
Constant	6.41	6.99
	(0.02)	(0.03)
Panel B: Indirect Effects (on Panchayats)		
Post-Treatment	0.01	-0.09
	(0.02)	(0.03)
Constant	5.11	6.38
	(0.01)	(0.02)
Observations (Respondents):		
Panel A:	3394 (1697)	3394 (1697)
Panel B:	2892 (1446)	2892 (1446)

Notes: This table estimates the direct and indirect effects of the state positive treatment on alternative belief questions. Post-Treatment is a dummy for post-treatment observations. Panel A reports effects on state courts, while Panel B reports (indirect) effects on panchayats. Observation counts are about 250 lower in Panel B as we did not include the panchayat non-state actor in the sports games. All outcomes are measured on a scale of 0-10. Allegiance Importance measures how important respondents think having a connection to the forum is to get fair treatment, and Allegiance Effort measures how much effort they would make in order to strengthen their connection to the forum. All regressions include individual fixed effects. Standard errors reported in parentheses.

Table B20: **Dropping Observations with Poor Understanding (Beliefs)**

	(1) Service Effectiveness	(2) Enforcement Effectiveness	(3) Access	(4) Trust
Panel A: Direct Effects (on State Courts)				
Post-Treatment	0.87	0.54	0.60	0.87
	(0.04)	(0.03)	(0.04)	(0.04)
Constant	3.95	6.55	2.96	4.86
	(0.03)	(0.02)	(0.03)	(0.03)
Panel B: Indirect Effects (on Panchayats)				
Post-Treatment	-0.17	-0.16	0.07	-0.24
	(0.03)	(0.02)	(0.03)	(0.03)
Constant	5.41	6.26	7.93	6.53
	(0.02)	(0.02)	(0.02)	(0.02)
Observations (Respondents):				
Panel A:	3362 (1681)	3384 (1692)	3394 (1697)	3370 (1685)
Panel B:	2888 (1444)	2884 (1442)	2886 (1443)	2886 (1443)

Notes: This table examines the effect of the state positive treatment on the outcomes in Table 7, dropping observations that could potentially be low-quality. After playing each game we had both the surveyor and respondent rank the respondent's understanding of the game on a scale of 1-10. These regressions drop respondents who received below a 5 from either source. Post-Treatment is a dummy for post-treatment observations. Panel A reports effects on state courts, while Panel B reports (indirect) effects on panchayats. For definitions of Service Effectiveness, Enforcement Effectiveness, Access, and Trust, see notes to Tables 7. Observation counts vary slightly due to response rate differences across the different dependent variables. Sample is restricted to those who received the state positive treatment. All regressions include individual fixed effects. Standard errors reported in parentheses.

Table B21: **Dropping Observations on the Boundary (Beliefs)**

	(1) Service Effectiveness	(2) Enforcement Effectiveness	(3) Access	(4) Trust
Panel A: Direct Effects (on State Courts)				
Post-Treatment	0.80	0.53	0.50	0.89
	(0.04)	(0.03)	(0.04)	(0.04)
Constant	4.63	6.85	4.37	5.02
	(0.03)	(0.02)	(0.03)	(0.03)
Panel B: Indirect Effects (on Panchayats)				
Post-Treatment	-0.19	-0.18	-0.02	-0.28
	(0.03)	(0.02)	(0.02)	(0.03)
Constant	5.46	6.65	8.56	5.78
	(0.02)	(0.02)	(0.01)	(0.02)
Observations (Respondents):				
Panel A:	2450 (1225)	3236 (1618)	2298 (1149)	2412 (1206)
Panel B	1982 (991)	2714 (1357)	2672 (1336)	1794 (897)

Notes: This table examines the effect of the state positive treatment on the outcomes in Table 7, dropping observations that we're potentially constrained from changing their initial outcome due to the bounded nature of our outcomes. These regressions omit observations where the respondent allocated the maximum (e.g. 250 PKR in either game) or minimum (e.g. 0 PKR in either game) to the relevant forum in the baseline. Post-Treatment is a dummy for post-treatment observations. For definitions of Service Effectiveness, Enforcement Effectiveness, Access, and Trust, see notes to Tables 7. Panel A reports effects on state courts, while Panel B reports (indirect) effects on panchayats. All regressions include individual fixed effects. Standard errors reported in parentheses.

Table B22: **Beliefs, By Round**

	First Round				Second Round			
	(1) Service Effectiveness	(2) Enforcement Effectiveness	(3) Access	(4) Trust	(5) Service Effectiveness	(6) Enforcement Effectiveness	(7) Access	(8) Trust
Panel A: Direct Effects (on State Courts)								
Post-Treatment	0.52	0.26	0.41	0.58	1.19	0.80	0.79	1.15
	(0.06)	(0.04)	(0.06)	(0.05)	(0.06)	(0.04)	(0.06)	(0.06)
Constant	4.12	6.73	3.06	5.16	3.79	6.39	2.85	4.58
	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
Panel B: Indirect Effects (on Panchayats)								
Post-Treatment	-0.07	-0.09	0.13	-0.11	-0.30	-0.25	-0.02	-0.41
	(0.03)	(0.03)	(0.05)	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)
Constant	5.37	6.23	7.74	6.51	5.45	6.30	8.17	6.56
	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)
Observations (Respondents):								
Panel A:	1621 (811)	1641 (821)	1650 (825)	1635 (818)	1742 (871)	1744 (872)	1744 (872)	1736 (868)
Panel B:	1644 (822)	1641 (821)	1644 (823)	1644 (822)	1244 (622)	1244 (622)	1244 (622)	1242 (621)

Notes: This table estimates the direct and indirect effects of the state positive treatment on belief and trust questions, split by round. Post-Treatment is a dummy for post-treatment observations. For definitions of Service Effectiveness, Enforcement Effectiveness, Access, and Trust, see notes to Tables 7. Panel A reports effects on state courts, while Panel B reports (indirect) effects on panchayats. All regressions include individual fixed effects. Standard errors reported in parentheses.

Table B23: **Hospital Usage and Perception Results**

	(1)	(2)	(3)	(4)	(5)
	Self-Usage	Others' Usage	Service Effectiveness	Access	Trust
Post-Treatment	0.05	0.05	0.03	-0.03	0.03
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
Constant	7.19	7.15	6.06	7.17	6.54
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Observations (Respondents):	1648 (824)	1641 (821)	1646 (823)	1650 (825)	1650 (825)

Notes: This table estimates the indirect effects of the state positive treatment on belief and trust questions about about a different forum, state hospitals. Post-Treatment is a dummy for post-treatment observations. For definitions of Service Effectiveness, Enforcement Effectiveness, Access, and Trust, see notes to Tables 7. All regressions include individual fixed effects. Standard errors reported in parentheses.

Table B24: **Beliefs (Sports Actor Games)**

	(1) Service Effectiveness	(2) Access	(3) Trust
Panel A: Direct Effects (on State Courts)			
Post-Treatment	1.27	0.80	1.07
	(0.11)	(0.12)	(0.11)
Constant	4.12	2.87	4.96
	(0.08)	(0.08)	(0.08)
Panel B: Indirect Effects (on Sports Club)			
Post-Treatment	0.29	0.30	0.10
	(0.06)	(0.09)	(0.06)
Constant	6.22	3.78	6.74
	(0.04)	(0.06)	(0.04)
Panel C: Netting Out Indirect Effects			
Post-Treatment	0.29	0.30	0.09
	(0.17)	(0.18)	(0.17)
State Actor	-2.10	-0.91	-1.81
	(0.17)	(0.18)	(0.17)
Post × State Actor	0.98	0.50	0.98
	(0.24)	(0.25)	(0.25)
Constant	6.22	3.78	6.76
	(0.12)	(0.13)	(0.12)
Observations (Respondents):			
Panel A:	500 (250)	500 (250)	500 (250)
Panel B:	497 (249)	497 (249)	497 (249)
Panel C:	998 (250)	1000 (250)	993 (249)

Notes: This table estimates the direct and indirect effects of the state positive treatment on belief and trust questions. Post-Treatment is a dummy for post-treatment observations. For definitions of Service Effectiveness, Enforcement Effectiveness, Access, and Trust, see notes to Tables 7. Panel A reports effects on state courts, while Panel B reports (indirect) effects on local sports clubs. All regressions include individual fixed effects. Standard errors reported in parentheses.

Table B25: **Heterogeneity by Trust in the Information, by Round**

	First Round			Second Round		
	(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
Panel A: Direct Effects (on State Courts)						
Post-Treatment	0.60	9.23	8.97	0.95	20.71	18.91
	(0.13)	(3.27)	(3.46)	(0.12)	(3.10)	(3.53)
Post × High Trust	0.49	7.45	8.40	0.83	19.26	17.93
	(0.15)	(3.98)	(4.18)	(0.13)	(3.57)	(4.04)
Constant	3.53	109.22	112.79	4.03	90.44	103.47
	(0.05)	(1.31)	(1.37)	(0.04)	(1.08)	(1.21)
Panel B: Indirect Effects (on Panchayats)						
Post-Treatment	-0.41	-6.36	-4.58	-0.41	-11.32	-12.48
	(0.11)	(3.32)	(3.39)	(0.10)	(3.66)	(3.51)
Post × High Trust	-0.32	-3.46	-10.48	-0.44	-17.13	-8.65
	(0.13)	(4.03)	(4.09)	(0.12)	(4.35)	(4.24)
Constant	6.65	108.15	116.84	6.16	104.35	114.29
	(0.04)	(1.33)	(1.34)	(0.04)	(1.40)	(1.39)
Observations (Respondents):						
Panel A:	2162 (1081)	2268 (1134)	2288 (1144)	1740 (870)	1740 (870)	1244 (622)
Panel B:	2162 (1081)	2268 (1134)	2288 (1144)	1242 (621)	1242 (621)	746 (373)

Notes: This table estimates heterogeneous direct and indirect effects by trust in the information provided in the state positive treatment, split by round. High Trust is a dummy for respondents who reported the level of trust in the information provided greater than or equal to 6 (on a scale from 1 to 10). Post-Treatment is a dummy for post-treatment observations. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see 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 pre-treatment values of the relevant variables. Standard errors reported in parentheses.

Table B26: **Heterogeneity by Priors (First Round Only)**

	Model 1			Model 2		
	(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
Panel A: Direct Effects (on State Courts)						
Post-Treatment	1.54	20.99	16.35	1.32	15.87	11.97
	(0.16)	(4.43)	(4.62)	(0.17)	(4.73)	(4.93)
Post × Initial State	-1.69	-11.80	-4.31	-1.94	7.17	18.50
	(0.15)	(4.33)	(4.51)	(0.51)	(14.54)	(15.17)
Post × Initial State Squared				0.20	-21.10	-24.91
				(0.51)	(14.51)	(15.15)
Post × Initial Non-State	-0.08	-0.93	-1.12	1.93	28.81	19.77
	(0.16)	(4.44)	(4.63)	(0.59)	(16.82)	(17.56)
Post × Initial Non-State Squared				-1.88	-27.82	-19.55
				(0.54)	(15.16)	(15.82)
Constant	4.06	105.14	115.83	4.06	105.14	115.83
	(0.03)	(0.91)	(0.95)	(0.03)	(0.91)	(0.95)
Panel B: Indirect Effects (on Panchayats)						
Post-Treatment	0.33	-4.53	-1.96	0.44	-1.49	0.37
	(0.14)	(4.49)	(4.61)	(0.15)	(4.79)	(4.93)
Post × Initial State	-0.09	-2.32	-5.30	-0.16	29.83	14.46
	(0.14)	(4.38)	(4.50)	(0.47)	(14.74)	(15.16)
Post × Initial State Squared				0.10	-32.71	-19.99
				(0.47)	(14.71)	(15.13)
Post × Initial Non-State	-1.28	-7.05	-12.06	-2.13	-54.74	-45.25
	(0.14)	(4.50)	(4.62)	(0.54)	(17.05)	(17.54)
Post × Initial Non-State Squared				0.80	44.54	31.00
				(0.49)	(15.37)	(15.81)
Constant	6.64	103.79	115.48	6.64	103.79	115.48
	(0.03)	(0.93)	(0.95)	(0.03)	(0.93)	(0.95)
Observations (Respondents):						
Panel A:	3812 (1906)	3812 (1906)	3812 (1906)	3812 (1906)	3812 (1906)	3812 (1906)
Panel B	3810 (1905)	3812 (1906)	3812 (1906)	3810 (1905)	3812 (1906)	3812 (1906)

Notes: This table estimates heterogeneous direct and indirect effects by priors for the first round only. We proxy priors by pre-treatment expected usage for relevant forum. Post-Treatment is a dummy for post-treatment observations. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see notes to Tables 1 and 2. Model 1 in columns 1-3 only includes linear interactions, while Model 2 in columns 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 reported in parentheses.

Table B27: **Heterogeneity by Priors (Second Round Only)**

	Model 1			Model 2		
	(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
Panel A: Direct Effects (on State Courts)						
Post-Treatment	2.34	34.78	37.49	2.24	19.34	26.44
	(0.22)	(6.86)	(8.03)	(0.28)	(8.61)	(10.07)
Post × Initial State	-2.09	-3.86	-1.00	-1.40	-14.61	22.57
	(0.25)	(7.83)	(9.06)	(0.79)	(24.52)	(29.01)
Post × Initial State Squared				-0.75	13.69	-23.61
				(0.82)	(25.51)	(29.07)
Post × Initial Non-State	-0.08	2.78	-4.87	-0.08	83.93	31.58
	(0.23)	(7.30)	(8.51)	(0.89)	(27.54)	(30.93)
Post × Initial Non-State Squared				-0.01	-72.45	-33.54
				(0.76)	(23.62)	(27.16)
Constant	4.09	85.92	98.74	4.09	85.92	98.74
	(0.04)	(1.31)	(1.50)	(0.04)	(1.30)	(1.50)
Panel B: Indirect Effects (on Panchayats)						
Post-Treatment	0.22	-29.12	-30.23	0.33	-25.16	-27.61
	(0.21)	(7.42)	(7.44)	(0.26)	(9.38)	(9.36)
Post × Initial State	-0.50	4.48	15.32	0.89	-6.68	43.23
	(0.23)	(8.46)	(8.40)	(0.74)	(26.71)	(26.95)
Post × Initial State Squared				-1.54	11.84	-30.11
				(0.77)	(27.79)	(27.00)
Post × Initial Non-State	-1.20	6.32	8.91	-2.66	-5.10	-25.70
	(0.22)	(7.89)	(7.89)	(0.83)	(29.99)	(28.73)
Post × Initial Non-State Squared				1.28	10.46	31.46
				(0.71)	(25.72)	(25.23)
Constant	6.17	104.58	114.29	6.17	104.58	114.29
	(0.04)	(1.42)	(1.39)	(0.04)	(1.42)	(1.39)
Observations (Respondents):						
Panel A:	1244 (622)	1244 (622)	746 (373)	1244 (622)	1244 (622)	746 (373)
Panel B	1244 (622)	1244 (622)	746 (373)	1244 (622)	1244 (622)	746 (373)

Notes: This table estimates heterogeneous direct and indirect effects by priors for the second round only. We proxy priors by pre-treatment expected usage for relevant forum. Post-Treatment is a dummy for post-treatment observations. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see notes to Tables 1 and 2. Model 1 in columns 1-3 only includes linear interactions, while Model 2 in columns 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 reported in parentheses.

Table B28: **Heterogeneity by Priors - Service Effectiveness**

	Model 1			Model 2		
	(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
Panel A: Direct Effect (on State Courts)						
Post-Treatment	1.05	25.38	19.03	0.90	19.53	9.44
	(0.10)	(2.86)	(3.02)	(0.11)	(3.44)	(3.58)
Post × Initial State	-0.64	-3.41	-3.08	0.00	24.82	35.76
	(0.14)	(4.16)	(4.38)	(0.44)	(13.29)	(14.01)
Post × Initial State Squared				-0.75	-32.69	-44.82
				(0.48)	(14.41)	(15.11)
Post × Initial Non-State	0.33	1.88	4.51	0.98	22.79	45.94
	(0.13)	(3.90)	(4.10)	(0.46)	(13.85)	(14.63)
Post × Initial Non-State Squared				-0.64	-20.56	-40.72
				(0.44)	(13.25)	(13.97)
Constant	4.49	92.57	112.37	4.49	92.57	112.37
	(0.03)	(0.92)	(0.98)	(0.03)	(0.91)	(0.97)
Panel B: Indirect Effect (on Panchayats)						
Post-Treatment	-0.59	-13.74	-9.03	-0.49	-10.33	-7.54
	(0.09)	(2.85)	(3.03)	(0.11)	(3.43)	(3.63)
Post × Initial State	0.19	3.42	3.28	-0.46	-2.36	-6.68
	(0.13)	(4.14)	(4.40)	(0.42)	(13.27)	(14.21)
Post × Initial State Squared				0.75	6.96	11.39
				(0.46)	(14.39)	(15.32)
Post × Initial Non-State	-0.13	-10.05	-11.88	-0.30	-30.65	-15.21
	(0.12)	(3.89)	(4.11)	(0.44)	(13.84)	(14.83)
Post × Initial Non-State Squared				0.17	20.44	3.21
				(0.42)	(13.24)	(14.16)
Constant	6.42	100.69	114.86	6.42	100.69	114.86
	(0.03)	(0.91)	(0.98)	(0.03)	(0.91)	(0.98)
Observations (Respondents):						
Panel A:	2856 (1428)	2856 (1428)	2360 (1180)	2856 (1428)	2856 (1428)	2360 (1180)
Panel B	2854 (1427)	2856 (1428)	2360 (1180)	2854 (1427)	2856 (1428)	2360 (1180)

Notes: This table estimates heterogeneous direct and indirect effects by priors. We proxy priors by pre-treatment service effectiveness for relevant forum. Post-Treatment is a dummy for post-treatment observations. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see notes to Tables 1 and 2. Model 1 in columns 1-3 only includes linear interactions, while Model 2 in columns 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 reported in parentheses.

Table B29: **Heterogeneity by Priors - Enforcement Effectiveness**

	Model 1			Model 2		
	(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
Panel A: Direct Effect (on State Courts)						
Post-Treatment	1.09	32.67	25.40	0.69	32.46	12.08
	(0.14)	(4.16)	(4.35)	(0.20)	(5.93)	(6.03)
Post × Initial State	-0.30	-12.64	-9.12	0.65	-23.62	17.05
	(0.15)	(4.40)	(4.59)	(0.59)	(17.62)	(18.28)
Post × Initial State Squared				-0.83	9.37	-23.56
				(0.49)	(14.78)	(15.40)
Post × Initial Non-State	0.13	0.56	1.57	1.21	13.59	44.84
	(0.15)	(4.39)	(4.60)	(0.57)	(16.96)	(17.66)
Post × Initial Non-State Squared				-0.94	-11.65	-38.49
				(0.49)	(14.56)	(15.23)
Constant	4.47	92.96	112.71	4.47	92.96	112.71
	(0.03)	(0.92)	(0.98)	(0.03)	(0.92)	(0.98)
Panel B: Indirect Effect (on Panchayats)						
Post-Treatment	-0.94	-25.75	-19.36	-0.86	-18.93	-12.56
	(0.13)	(4.16)	(4.46)	(0.19)	(5.93)	(6.19)
Post × Initial State	0.24	9.27	6.84	0.43	-6.56	5.97
	(0.14)	(4.41)	(4.70)	(0.56)	(17.62)	(18.77)
Post × Initial State Squared				-0.15	14.00	1.33
				(0.47)	(14.77)	(15.82)
Post × Initial Non-State	0.33	3.76	0.91	-0.28	-14.25	-34.64
	(0.14)	(4.39)	(4.72)	(0.53)	(16.95)	(18.14)
Post × Initial Non-State Squared				0.54	15.86	31.71
				(0.46)	(14.56)	(15.64)
Constant	6.43	101.09	115.75	6.43	101.09	115.75
	(0.03)	(0.92)	(1.01)	(0.03)	(0.92)	(1.01)
Observations (Respondents):						
Panel A:	2874 (1437)	2874 (1437)	2376 (1188)	2874 (1437)	2874 (1437)	2376 (1188)
Panel B	2872 (1436)	2874 (1437)	2376 (1188)	2872 (1436)	2874 (1437)	2376 (1188)

Notes: This table estimates heterogeneous direct and indirect effects by priors. We proxy priors by pre-treatment enforcement effectiveness for relevant forum. Post-Treatment is a dummy for post-treatment observations. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see notes to Tables 1 and 2. Model 1 in columns 1-3 only includes linear interactions, while Model 2 in columns 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 reported in parentheses.

Table B30: **Heterogeneity by Priors - Access**

	Model 1			Model 2		
	(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
Panel A: Direct Effect (on State Courts)						
Post-Treatment	0.91	23.70	12.97	0.67	17.61	6.21
	(0.12)	(3.67)	(3.83)	(0.15)	(4.48)	(4.51)
Post × Initial State	-0.51	-7.63	-5.38	0.25	24.58	37.02
	(0.14)	(4.19)	(4.40)	(0.45)	(13.20)	(14.22)
Post × Initial State Squared				-0.93	-38.77	-50.46
				(0.49)	(14.56)	(15.51)
Post × Initial Non-State	0.28	4.13	11.74	1.50	28.17	39.69
	(0.14)	(4.16)	(4.33)	(0.63)	(18.52)	(19.40)
Post × Initial Non-State Squared				-1.08	-21.20	-25.35
				(0.54)	(15.82)	(16.83)
Constant	4.47	93.17	112.54	4.47	93.17	112.54
	(0.03)	(0.92)	(0.99)	(0.03)	(0.91)	(0.98)
Panel B: Indirect Effect (on Panchayats)						
Post-Treatment	-0.53	-14.59	-12.00	-0.39	-7.64	-4.66
	(0.12)	(3.68)	(3.93)	(0.14)	(4.49)	(4.63)
Post × Initial State	0.08	0.07	0.31	-0.17	-20.15	6.56
	(0.13)	(4.20)	(4.51)	(0.42)	(13.23)	(14.63)
Post × Initial State Squared				0.31	25.07	-4.83
				(0.46)	(14.59)	(15.95)
Post × Initial Non-State	-0.09	-3.53	-3.22	-0.89	-39.55	-61.52
	(0.13)	(4.17)	(4.44)	(0.59)	(18.55)	(19.96)
Post × Initial Non-State Squared				0.71	31.67	51.92
				(0.50)	(15.85)	(17.31)
Constant	6.44	101.13	115.98	6.44	101.13	115.98
	(0.03)	(0.92)	(1.01)	(0.03)	(0.92)	(1.01)
Observations (Respondents):						
Panel A:	2890 (1445)	2890 (1445)	2392 (1196)	2890 (1445)	2890 (1445)	2392 (1196)
Panel B	2888 (1444)	2890 (1445)	2392 (1196)	2888 (1444)	2890 (1445)	2392 (1196)

Notes: This table estimates heterogeneous direct and indirect effects by priors. We proxy priors by pre-treatment access to relevant forum. Post-Treatment is a dummy for post-treatment observations. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see notes to Tables 1 and 2. Model 1 in columns 1-3 only includes linear interactions, while Model 2 in columns 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 reported in parentheses.

Table B31: **Heterogeneity by Priors - Trust**

	Model 1			Model 2		
	(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
Panel A: Direct Effect (on State Courts)						
Post-Treatment	0.89	24.85	22.28	0.52	13.12	10.06
	(0.12)	(3.56)	(3.72)	(0.15)	(4.45)	(4.60)
Post × Initial State	-0.38	-3.23	-5.77	0.65	39.81	52.76
	(0.13)	(3.91)	(4.05)	(0.45)	(13.41)	(14.21)
Post × Initial State Squared				-1.06	-44.37	-59.66
				(0.44)	(13.18)	(13.83)
Post × Initial Non-State	0.40	2.22	1.60	1.77	36.97	28.77
	(0.14)	(4.02)	(4.18)	(0.53)	(15.78)	(16.44)
Post × Initial Non-State Squared				-1.23	-30.88	-23.80
				(0.47)	(13.87)	(14.55)
Constant	4.48	92.96	112.59	4.48	92.96	112.59
	(0.03)	(0.92)	(0.99)	(0.03)	(0.92)	(0.98)
Panel B: Indirect Effect (on Panchayats)						
Post-Treatment	-0.61	-19.72	-14.71	-0.26	-13.52	-11.03
	(0.11)	(3.52)	(3.77)	(0.14)	(4.42)	(4.70)
Post × Initial State	0.13	6.10	8.60	-0.65	-16.65	10.27
	(0.12)	(3.86)	(4.10)	(0.42)	(13.33)	(14.54)
Post × Initial State Squared				0.82	23.45	-1.55
				(0.42)	(13.09)	(14.16)
Post × Initial Non-State	-0.05	-1.19	-6.20	-1.50	-19.53	-30.23
	(0.13)	(3.97)	(4.23)	(0.50)	(15.68)	(16.83)
Post × Initial Non-State Squared				1.30	16.30	21.92
				(0.44)	(13.78)	(14.89)
Constant	6.43	100.46	115.64	6.43	100.46	115.64
	(0.03)	(0.91)	(1.00)	(0.03)	(0.91)	(1.00)
Observations (Respondents):						
Panel A:	2866 (1433)	2866 (1433)	2374 (1187)	2866 (1433)	2866 (1433)	2374 (1187)
Panel B	2864 (1432)	2866 (1433)	2374 (1187)	2864 (1432)	2866 (1433)	2374 (1187)

Notes: This table estimates heterogeneous direct and indirect effects by priors. We proxy priors by pre-treatment trust in relevant forum. Post-Treatment is a dummy for post-treatment observations. For the definitions of Expected usage and Allocation in the Fund Dictator and Investment Games see notes to Tables 1 and 2. Model 1 in columns 1-3 only includes linear interactions, while Model 2 in columns 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 reported in parentheses.

Appendix C: Full Text of Informational Treatments & Selected Questions and Study Protocols

Main Treatment Text:

- **State Positive Treatment:** The legal system and judges have formed a new judicial policy. This policy was introduced in Multan and has resolved 6000 pending cases in 2 months. For this reason, Multan's number of pending cases have 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.
- **Social Experimenter Treatment:** 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.

Piloted Treatment Text:

- **Non-State Positive Treatment:** Panchayats have been doing a great job of resolving people's issues at the local level. From our research we have found that panchayats manage to resolve 80% of cases where most cases are resolved in a week. We also found that a third of all cases in panchayats are resolved without any cost while in the remaining cases the costs are also very low, mostly around Rs. 1,000. 80% of people also believe that panchayats are very good at bringing justice to people and these people are very satisfied with their panchayats.
- **Non-State Negative Treatment:** Through our research we have found that in Kasur district, the number of disputes arising across baradaris and across villages has increased significantly. These disputes primarily relate to land, family or water theft issues. Our research has shown that where in the past the panchayat was 70% effective in resolving disputes; now that percentage has dropped to 35%. The reason for this is that disputes across baradaris and villages are not so easily resolved through panchayats. Most often, these disputes are not resolved, and even those cases where a resolution is reached, it is near impossible to implement/enforce the decision of the panchayat. Due to these reasons the number of disputes are increasing, it is taking longer, and no action has been taken to tackle this problem.
- **State Negative Treatment:** At, the moment, the district courts of Okara there are 16,241 pending cases that have been lying dormant for a long time. Even if every judge in the district were to drop all new cases, it would take them 13 years to resolve these pending cases at the current rate.

Perception Question Text:

- **Self-Usage:** What is the likelihood of you going to [STATE/NON-STATE]? 0 meaning not at all, and 10 meaning completely.
- **Other-Usage:** What do you think is the likelihood of others in your area going [STATE/NON-STATE]? 0 meaning not at all, and 10 meaning completely.
- **Effectiveness:** Generally speaking, how effective do you find [STATE/NON-STATE] in providing services to the ordinary man? 0 meaning not at all and 10 meaning completely?
- **Enforce:** Generally speaking, how effective do you find the [STATE/NON-STATE] in enforcing verdicts? 0 meaning not at all, and 10 meaning completely.
- **Access:** In your opinion, how difficult is it for an ordinary man to get access to the [STATE/NON-STATE]? 0 meaning not at all difficult, and 10 meaning completely.
- **Trust:** Generally speaking, how much do you trust [STATE/NON-STATE] officials? 0 meaning not at all and 10 meaning completely.
- **Allegiance Importance:** How important is it for the ordinary man to have strong connections to the [STATE/NON-STATE] in order to receive justice? 0 meaning not important at all, and 10 meaning extremely important.
- **Allegiance Effort:** How much effort would you like to make in order to strengthen these connections? 0 meaning no effort at all, and 10 meaning completely.

Study and Experimental Protocols:

This section provides the study and experimental protocols used by out surveyors.

Surveyors start by obtaining consent from respondent. They inform the respondent that they are from the Center for Economic Research in Pakistan and are working in conjunction with professors from the Lahore University of Management Sciences in a study that seeks to understand dispute resolution and the relevant forums (state courts and panchayats) that are in their area. If respondents have participated in the study before, or are familiar with it, they are skipped. Respondents are provided with a participation fee of PKR 50 and told they may receiving some more money during the study as we will be playing some games.

The baseline survey is then conducted. After completion of the survey, the two game types are explained as determined by the randomization order. If respondents do not understand the games, the instructions are repeated to ensure they have understood. They are shown game boards (Figure B1) and told the will play each type of game twice (hence the 8 slots in each game i.e. pre- and post-treatment allocations to self and state courts, and to self and panchayats). We provide translated instructions that the surveyors followed for both types of games below:

Fund dictator: *As aforementioned, our organization CERP, has teamed with LUMS. We are considering the possibility of establishing 2 funds in your area. There's one adalti fund. The purpose of this fund is to help those individuals in your area who want to go to courts to resolve their disputes. The second is the panchayati fund which is for individuals in your area who would want to go to panchayats to resolve their disputes. We are taking your valuable opinions because even though we are the ones thinking of making these funds, we do not yet know what the common man in the rural areas needs and how much money should be allocated to each fund. In order to ascertain your opinion, we will conduct the fund exercise. Now I will show you how we will conduct this exercise. Each time, we will provide you with Rs.250 and you will think of it as your own money. The green box here is for the court fund. However much people in your area prefer going to the court to resolve their disputes, you can accordingly allocate money to the adalti fund. If you think people prefer to go more/less to courts in your area, you can choose to allocate more/less money with the fund. The rest you will keep in the blue box for yourself - in both exercises, the blue box will always be your own box.*

Now in the same manner here is Rs.250. The yellow box here is for the panchayati fund. However much people in your area prefer going to the panchayat, you can allocate money to the panchayati fund. If you think people prefer more/less to go to panchayat in your area, you can choose to allocate more/less money with that fund. The rest you will keep in the blue box for yourself - in both exercises, the blue box will always be your own box.

Remember, do not allocate according to the expenses one anticipates for these institutions. We know cost of going to the court is high and going to the panchayat is low – simply allocate according

to how useful/beneficial you think each dispute resolution body is and allocate the rest (or all) to yourself in the blue box.

The same boards are used for the investment game except the money they invest is put in the court/panchayat box.

Investment: *Before starting the exercise, imagine a friend from your village whom you trust completely. This person has a dispute related to land and he has decided to go to the court in order to fight his case and to retrieve the money that is owed to him. Keep in mind that he has a just cause to fight his case and he is in the right. He offers to make you a stakeholder in the amount received from winning the case. This is obviously not a real business, but think of the court as a business, and consider the benefit to yourself if your friend actually wins his case.*

If he wins the case, then he will return the amount you invest; it is possible that amount he returns can be twice the invested amount, equal to the invested amount or a little less than the invested amount. If he loses the case then your investment will also sink and you will not get anything. This means that the money you may get back is dependent upon how effective the court is in dispensing justice. Please keep in mind that our institute will give you money for this experiment. If you think that an honest man can be proven guilty in courts then feel free to not invest anything. If you think that courts are only somewhat effective in ruling and dispensing justice then invest accordingly. And, if you trust the courts 100% and you think that they are fully effective in providing justice, then you can invest all the money in the outcome of the case of the person going to court.

Consider the money you are giving to this person as an opportunity to become a stakeholder. The money invested can be returned to you by a factor ranging from 0-2. For example, if you invest Rs.1000, your return can be anywhere between Rs.0-2000 <Give more examples with different amounts invested in order to explain this concept>. Before making your investment, you also have to keep in mind that the person you are helping is in the right and you believe that he should receive compensation. So now you will invest based upon how effective you think the court system is in ruling and dispensing justice. You will not invest from your own pocket; our institute will give you money for this exercise.

Now in the same way, imagine there is another friend from your village whom you trust completely and he also has a similar dispute related to land but the only difference is that he decides to take his case to a local panchayat in your area. He also offers to make you a partner by offering you a part of the money recovered from the money received based on the panchayat's ruling. This person is also in the right and is hopeful that the panchayat will rule in his favor. This is obviously not a real business, but think of the court as a business, and consider the benefit to yourself if your friend actually wins his case.

If he wins the case, then he will return the amount you invest; it is possible that amount he returns can be twice the invested amount, equal to the invested amount or a little less than the

invested amount. If he loses the case then your investment will also sink and you will not get anything. This means that the money you get back will depend on how effective the panchayat is in dispensing justice. If you think that panchayat will not decide in his favour then feel free to not invest anything. If you think that panchayat will do somewhat justice then invest accordingly. If you trust panchayat 100% and you think that it is fully effective in providing justice then you can invest all the money in the outcome of the case of the person going to the panchayat. You shouldn't think about the expense and you also shouldn't think about helping your friend. You should only think about investing in a business opportunity.

After each game is explained, a practice game is played. Once this is done respondents are told they will play the game for real next, and that their play will determine their payoff at the end, since they will be randomly paid from one of the four (court or panchayat, pre- or post-treatment) games that contains their own allocation/investment. Once the baseline games are played, we provide the information, state positive or social experimenter, and then both types of games are played again. The survey is repeated along with any additional questions. Final payments are made and the respondents thanked.