

Updating People’s Beliefs in the State’s Rule Adherence*

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Abstract

This PAP builds on our prior RCT in Mali in which we tested the effects of an information campaign in the context of a country-wide decentralization program—the Programme d’Appui aux Collectivités Territoriales (PACT)—on measures of horizontal and vertical social cohesion including state-society interactions, intergroup relations, and civic engagement. Our main analysis have generated mixed results on the effects of the provision of aid information on people’s social and political perceptions. Auxiliary analyses indicate that people’s pessimistic pre-existing attitudes on local institutions’ adherence to aid-related rules and regulations may be one reason for the limited effect of information. However, it is unclear (1) if these auxiliary findings represent (spurious) correlations between survey measures or actual causal effects and (2) how perceptions of aid-related rule adherence may be improved. In this follow-up intervention, we provide respondents in the treatment groups with information on local government’s adherence to rules of PACT project selection. Treatment 1 is based on the *real* formal assessments of technical experts mandated by the Government of Mali and treatment 2 is based on data from our previous surveys on citizens’ own reported assessment visits to the PACT projects in their communities. Both, the expert assessment and citizens’ assessments have reached positive conclusions on adherence to rules of project selection in 7 out of 10 PACT projects. We hypothesize that both treatments increase people’s willingness to contribute to maintaining the project infrastructure and possibly also their perceptions towards local state institutions and other social groups. We expect stronger effects of the citizen monitoring treatment than for the expert assessment treatment.

Keywords— Development aid, state-building, state-society relations, fragile states

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1 Background

This is follow-up data collection in Mali was initially not planned. The original PAP (De Juan and Koos, 2023) foresaw three waves of data collection in Mali and Niger to assess the impact of an information campaign on vertical and horizontal social cohesion (Chan, To and Chan, 2006; Fiedler, 2023).¹ Due to the coup d'état in Niger on 26 July 2023, we had to stop our research and data collection in Niger after having implemented one baseline survey experiment. We therefore decided to use the remaining resources to continue the research in Mali.

Our preliminary analyses of the public information campaign in Mali yielded relatively weak average treatment results on our main outcomes of social cohesion. The impact on (i) state-society interactions and (ii) intergroup relations was mostly insignificant. The effects on (iii) civic engagement—i.e., to what extent respondents would donate time and money to maintain the financed projects—yielded more robust results.

One reason for the limited effect of pure information could be people's pessimistic pre-existing attitudes on local institutions' adherence to aid-related rules and regulations. Around 80 percent of our baseline respondents agree a lot or somewhat that the rules on how aid projects should be distributed and on how people should be involved are “meaningless because no one sticks to these rules anyway”. Similarly, 74 percent of the respondents believe that the most influential individuals and families select specific PACT projects in their respective communes.

Subjective perceptions of state institutions' aid-related rule adherence correlate with a series of important social and political attitudes in our baseline survey—namely, several of our main outcome and mediator variable. While the strength and direction of these correlations vary, they indicate a potentially important role of subjective perceptions of state institutions' in shaping civic engagement, state-society relations, intergroup-relations or feelings of efficacy.

However, people's perceptions of the state's rule adherence in weak state contexts such as Mali are not necessarily grounded in evidence but are often influenced by rumours, hear-say, elite manipulation, and a general dissatisfaction with the state's ability to provide quality public services.

In fact, in the case of the PACT project in Mali, peoples' subjective perceptions of rule

¹We define vertical social cohesion as the extent of trust and participation between citizens and the state, and horizontal social cohesion as relations between citizens across (intergroup) and within (intragroup) identity groups. Social cohesion manifests in attitudes and behavior including trust, an inclusive identity, and cooperation (e.g., Chan, To and Chan, 2006).

adherence seem more negative than external controls of aid activities suggest. While recent external controls of 360 PACT projects warn that a vast majority of projects deviate from procurement and financial management rules, they also conclude that around 70 percent adhere to the formal rules of project selection. While this number is still unsatisfactory from a governance and aid-delivery perspective, it paints a substantially more favorable picture of local rule adherence than the subjective perceptions of survey respondents.

Taken together, these patterns indicate an important role of subjective interpretations of rule adherence and a potentially important field for information and awareness campaigns: while the provision of factual information on project properties and processes of project selection may not affect citizen’s attitudes towards development interventions and local state institutions, information on rule adherence may have a more pronounced effect.

However, thus far, it is unclear (1) if observable associations between perceptions of rule adherence and citizens’ perceptions represent (spurious) correlations between survey measures or actual causal effects and (2) how perceptions of aid-related rule adherence may be improved.

Therefore, we implement a fourth survey wave and an embedded survey experiment to investigate the effects of perceptions of (a) local government rule adherence and (b) state vs. non-state monitoring and control mechanisms ensuring this adherence. This PAP describes our research questions, hypotheses, treatment design, and measurement.

2 Theory, research questions & hypotheses

The main research questions we aim to answer in this follow-up project build on the theoretical framework and findings from the first three survey waves of our RCT in Mali. In our main PAP ([De Juan and Koos, 2023](#)), we described the content of the information campaign and how we theorized it to influence vertical and horizontal social cohesion, i.e., the relationships between citizens and the local state (vertical), relationships across communities, and within communities (horizontal).

The information campaign in the original project informed people about a real, planned development project in the vicinity of their home. The content of the information treatment consisted of locally customized factual information about the type of project (education or health), a summary of the precise investment (e.g., two new classrooms, furniture, and a water tower), the planned duration and project costs, and the selection and allocation mechanism

(treatment 1). In treatment 2, respondents were additionally given the opportunity to share their feedback on the project selection and information provided.

Our main hypothesis back then was that providing people with systematic and localized information on planned development projects and public services and the procedural rules should strengthen people's political efficacy and sense of distributional fairness which in turn should empower citizens to act more confidently with local state institutions (e.g., inquire to receive information, report irregularities), have more favorable perceptions of inter-group relations, and engage more in the upkeep and maintenance of financed public services.

As discussed above, the average treatment effects were mostly insignificant for the two main outcome indexes state-society relation and intergroup relations, but reasonably strong for civic engagement.

Our planned survey builds on this research. Our research questions build on auxiliary analyses which showed that (1) a large share of respondents did not believe that local governments abide by rules of aid project selection and (2) perceptions of rule adherence correlate strongly with our main initial outcomes and mediators of interest.

Given that people's perception of the state's rule adherence and legitimacy more generally is not necessarily informed by credible information and evidence but through rumours and hearsay in social networks, and is also often manipulated by political and social elites, our interest is whether the provision of actual information on rule adherence can update people's beliefs and then also positively affect vertical and horizontal social cohesion.

In the previous surveys, people could also prioritize which information would be most useful for them in the context of local development projects. Information on project selection turned out to be in high demand.

We build on these initial insights. We provide respondents in the treatment groups with information on actual (positive) results of different types of evaluations of local adherence to rules of PACT project selection in the Sikasso region. In particular, we aim to assess the following research questions:

- *Q1*: Does the provision of information on positive evaluation results of the PACT program result in more positive perceptions of state institutions' rule adherence?
- *Q2*: Does the provision of information on positive evaluation results of the PACT program affect perceptions of state institutions, inter-group relations and civic engagement?

- *Q3*: Do different sources of the evaluation results—government-mandated technical experts vs. citizens—influence people’s attitudes differently?
- *Q4*: Do effects vary between respondents’ pre-treatment (i) perceptions of state institutions’ rule adherence (ii) preferences of information sources (experts vs. citizens), and their (iii) preferences of information types (factual vs. procedural information).

In answering these questions, we seek to advance research on the social and political impacts of foreign aid-funded development in conflict-affected states, particularly the extent to which customized information campaigns can empower citizens to engage with local state institutions, help reduce perceptions of distributional injustice, and motivate citizens to engage in the upkeep and maintenance of local public services. From a policy perspective, we aim to assess the potential of embedding customized (mobile phone) information campaigns into development programs to increase intended positive and avert unintended negative aid effects.

Figure 1: Conceptual model

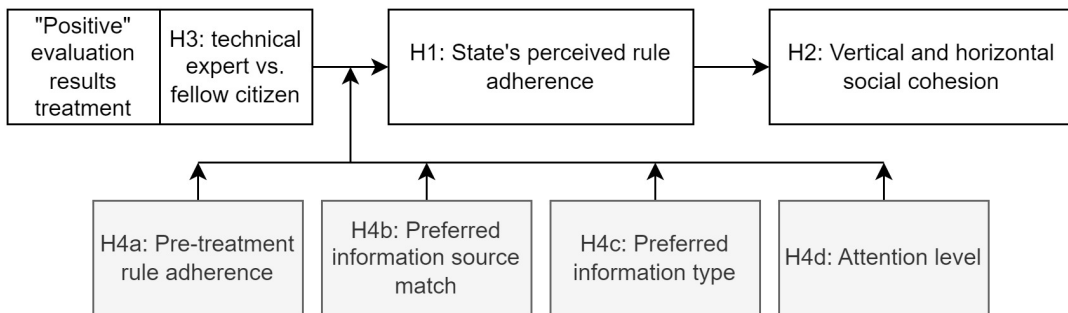


Figure 1 illustrates how the treatment provision of real “positive” evaluation results may influence mediators and outcomes downstream, and how a set of pre-treatment preferences and beliefs may alter this effect. Our average expectations are that citizens positively update their beliefs about the state’s rule adherence (H1), and that this in turn contributes positively to vertical and horizontal social cohesion, conceptualized as interactions with the local state, improved intergroup perceptions and intragroup civic engagement (H2).

In our design, we randomly vary the source of the evaluation results—technical experts vs. citizens—but do not have strong a priori expectations as to whether one source is more effective. If anything, given the relatively low levels of political trust in Mali and the recent coup d’état, we expect that citizens’ evaluation results would have stronger effects as respondents may have

higher levels of trust in fellow citizens than in government-mandated technical experts who may be more skilled in evaluation technical processes (H3).

It is likely that the effects of providing the evaluation results depend on a set of pre-treatment beliefs. First, we expect that people’s pre-treatment perceptions of the state’s adherence to formal rules conditions the effect of the evaluation results information. People who have very low trust in the state, are harder to convince of the opposite even if the information is true. Vice versa, for people with high trust in the state’s rule adherence, the marginal effect of the treatment may smaller than for people with intermediate trust. Thus, we believe that the treatment will have stronger effects for the middle category, approximately one standard deviation above and below the average (H4a).

Further, when the preference of information sources (in terms of their level of trust in different sources’ assessments) matches the treatment condition (expert vs. citizen) we expect the effects to be stronger (H4b). Likewise, when respondents are more interested in procedural information about distribution and allocation as opposed to factual information on project type, duration, and costs, we expect the treatments to be stronger (H4c).

3 Sampling, interview mode, and ethics

We draw our random sample from our baseline survey sample of 5,200 respondents. All of these respondents have been treated, i.e. they have all received information on PACT projects in their vicinity (we have provided the information treatment to the original control group in the final wave of the first part of the study to avert any unintended negative effects of unequal information provision). Our sample for this follow-up study draws a new random sample of 3,500 from the sampling frame of 5,200. Our expectation is that respondent characteristics including previous treatment status will be balanced across the three conditions to which they will be assigned in this project.

This data collection will be implemented via phone. One particular challenge with phone interviews is that the response rates may be lower than in actual face-to-face interviews. When respondents receive calls from unknown numbers, they may prefer not to pick up the phone. For this reason, sampled respondents will receive a text message a few days before the call reminding them of their previous participation and this follow-up study. Like our previous survey waves, the interviews will be conducted by GeoPoll, a US-based survey company specialized in surveys

in fragile and conflict-affected states. The phone enumerators are Malians and the survey is available in French and Bambara.

The roll-out of the survey takes place in three steps. First, we have implemented a small pre-test with a sample of 50 individuals not included in our main sample. We have implemented minor changes in the survey instrument following enumerator debriefing. Second, we have implemented a “soft-launch” of the main survey. We have asked our survey partner to deliver a first tranche of around 500 interview forms and to halt the data collection for several days. Based on this first data batch, we have implemented minor changes in individual survey items that generated inconsistent responses (e.g., that did not display any variation in the distribution across response options). Finally, we moved on to the roll-out of the remaining sample of 3000 respondents. For our main analysis, we will rely on this final *clean* sample.

Considering that all respondents have given their informed consent to the participation in our baseline survey as well as in follow-up survey waves, our current instrument includes a shortened informed consent that provides information on responsible/implementing institutions, duration of the interview, anonymity of the data and incentives provided. The full survey project investigating the effects of information campaigns on attitudes in Mali and Niger has been approved by the University of Osnabrück’s institutional review board.

4 Treatment design

The set-up of this survey is that of a simple survey experiment in which the treatment information is delivered in the form of vignettes. An important feature of the survey experiment is that the information which is provided to the respondents is real and not fictional. As noted above, our main interest is whether respondents update their beliefs about the state’s rule adherence and if this translates into perceptions of state institutions, inter-group relations and civic engagement.

We provide respondents with information on an aspect of rule adherence that respondents had highlighted in open question on their information needs and their assessments of PACT projects in the previous surveys: a substantial share of respondents has voiced (a) interest in receiving information on project selection and (b) concerns related the transparency of selection procedures.

To provide factual and meaningful information, we base the vignettes on two sources: (i)

a formal control by technical experts mandated by the Government of Mali, and (ii) data from respondents' assessments in the previous surveys we conducted in Mali. According to the technical control report, around 70 percent of the evaluated PACT projects in the region of Sikasso have met requirements of project selection and community involvement. For the second vignette, we draw on survey data from our previous waves. We have hand-coded feedback on PACT projects provided by survey respondents that have indicated that they have visited the PACT projects in their respective communities—around 2000 respondents indicated that they have visited the projects. Among those respondents that have provided feedback on processes of project selection, around 70 percent have indicated that they were satisfied with the way that PACT projects were selected. This allows us to vary the sources of information (i.e., the type of evaluator) without having to vary the results of assessments—without deception of respondents.

The information that a share of 70 percent of projects have been evaluated positively, paints a substantially better picture of local rule adherence than respondents' pre-treatment beliefs expressed in our first survey wave (see information above). Thus, we expect that the treatment induces a process of positive believe updating. Below, we describe the protocol for the information module.

Treatment introduction (all respondents): “In our previous interview, we have also talked about the formal rules on how PACT projects should be selected. The PDSEC, the Social, Economic, and Cultural Development Plan, is the basis for all PACT projects. The PDSEC is supposed to be developed on the basis of public consultation. This consultation serves to allow the population to participate in the identification of development needs in the municipality. The local council and the mayor are responsible for selecting urgent and feasible projects from the PDSEC. These are the formal rules. Some people think that the majority of all PACT projects in Sikasso have been selected according to these rules, others think that only a minority of projects were implemented according to the formal rules.”

Treatment arms

- *Control:* No further information.
- *Treatment 1:* “On that note, last year technical experts have visited more than 20 PACT projects in the Sikasso region on behalf of the government. These technical experts found that the vast majority of these PACT projects in Sikasso were implemented according to

the formal rules of project selection and citizen involvement.”

- *Treatment 2*: “On that note, last year more than 2000 people like you have visited more than 20 PACT projects in the Sikasso region. These fellow citizens found that the vast majority of these PACT projects in Sikasso were implemented according to the formal rules of project selection and citizen involvement.”

5 Measurement

5.1 Outcomes

State-society relations (index): Rather than explicitly asking respondents about their “trust” or “confidence” in institutions, we gauge their willingness to engage (or not) with state institutions. Two considerations inform this decision: first, we expect that concrete activities are easier to assess for respondents than abstract notions like “trust”. Second, from a policy perspective (intended) behavior seems more relevant than general attitudes. Thus, we rely on two survey items related to inquiring information and filing a complaint. Respondents can answer to each item on a ten-point Likert scale ranging from 1 to 10. Responses to each of item will be standardized, combined into an index and standardized again:

- On a scale from 1 to 10 where 1 means not very likely and 10 means very likely, how likely are you to personally contact the mayor of your commune to inquire about development needs and projects?
- On a scale from 1 to 10 where 1 means not very likely and 10 means very likely, how likely are you to file complaints if you notice that development projects in your commune have not been selected according to the formal rules?

Intergroup relations (index): To measure our second main outcome, we rely on vignettes that confront respondents with hypothetical distributional decisions in zero-sum situations. Again, our main motive for relying on vignettes rather than standard questions related to “trust in others” is to make survey items as concrete as possible. Specifically, we rely on a scenario of inter-village distribution of a resource surplus. We ask for respondents’ own distributional preferences as well as their expectations of other villages’ preferences. Responses to each of the four items will be standardized, combined into an index and standardized again:

- Imagine an aid agency would provide you with 5 million FCFA. You can decide to split this amount for the benefit of your village or that of another village in your commune. How many millions, if any, would you contribute to a project in another village in your commune?
- Imagine an aid agency would provide someone in another village in your commune with 5 million FCFA. This person can decide to split this amount for the benefit of her village or that of your village. How many millions if anything at all would that person contribute to a project in your village?

Civic engagement (index): We measure civic engagement through two self-reported items in which respondents need to indicate that likelihood of contributing money or unpaid time to maintain and upkeep the development project which was financed in their community. Small user fees and unpaid labor are common expectations to create and maintain public goods in wide parts of Africa.

- On a scale from 1 to 10 where 1 means not very likely and 10 means very likely, how likely is it that you would contribute unpaid time to help maintain the PACT project in the future?
- On a scale from 1 to 10 where 1 means not very likely and 10 means very likely, how likely is it that you would donate money to help maintaining the PACT project in the future?

Civic engagement (public goods game): As an alternative to the self-reported civic engagement measure we include a public goods game (PGG) in the survey.² We created a shortened version of the PGG given that the survey is conducted via telephone.³

²We focus the behavioral outcome on civic engagement and not the other outcomes since the results of the prior surveys suggested this to be the outcome that is most likely to be affected by the treatments.

³We are aware that our protocol for the PGG departs from standard PGG—for instance, we do not use continuous contributions but either 0 or 600 CFA. This limitation derives from the limitation that phone service providers allow different minimum transferable amounts. Six-hundred CFA or multiples thereof are amounts that work across phone providers. To that end, we prefer this version in order to avoid deception among respondents. That would be the case if we tell people that we round individual profits of a shared pot to 600.

1. Motivation: Before we finish the interview, let's talk about how development projects such as PACT are taken care of in your commune. When the local government does not have enough money to maintain these projects, citizens like you might pitch in to help. If everyone in the commune makes a small contribution to support the maintenance, everyone can benefit from a well-maintained project.

2. Explanation: Now, we would like to invite you to participate in a small exercise. Many people from your commune will also participate. Here is it how it works: For this exercise, each participant will receive 600 CFA. With these 600 CFA you can make a decision. You can keep it for yourself. Then you have these 600 CFA for sure, no matter what other commune members do. Or, you can contribute it a shared commune pot. Only people from your commune can contribute to your commune's pot. If you decide to contribute to the commune pot, two things can happen: First, when more than half of all participants in your commune decide to contribute, we will give everyone in your commune 1200 CFA. Second, if less than half of the participants in your commune decide to contribute to the commune pot, no one in your commune will receive any top-up from this exercise.

3. Clarification: Note that you will not know if other persons in your commune contributed. The only thing you can control is how much you decide to contribute to the shared commune pot. Also, keep in mind that nobody will know if you contributed apart from the research team. Do you have any questions so far?

4. Amount: Would you like to contribute 600 CFA to the communal fund, or would you prefer not to contribute at this time?

5. Confirmation: Let me confirm: you want to put <PGG amount> CFA into the pot?

6. Transfer information: Ok, thank you. After we have finished the surveys, you will receive the amount you kept and did not put in the pot plus your share of the commune pot. We will increase your profit to the closest transferable amount.

Overall, we are interested whether the treatments affect the likelihood that respondents contribute to the PGG. Moreover, we will explore the extent to which any treatment effect is driven by the expectation that other people in the commune will contribute, compared to changes in individual preferences to contribute. Put differently, we are interested in differentiating between behavioral changes due to changes in individual beliefs or due to changes in how individuals perceive others to act. To assess that, we add the following question at the end of the PGG protocol:

Allow me a follow-up question on your contribution. Out of ten people in your commune, how many do you think will contribute 600 CFA?

5.2 Manipulation checks

To assess whether the information in the two treatment versions has been taken up by respondents, we include the following manipulation checks.

- Please tell me what you think about the following question: "Does someone control if the mayors and the councils adhere to formal rules of PACT project selection?"
 - 1)Yes
 - 2)No
- In the last year, who controlled the formal rules of PACT project selection?
 - 1)Technical experts
 - 2)The citizens
 - 3)No one

5.3 Mediators

We expect that the information on evaluation results of the PACT project in the *region* of Sikasso (ADM1 level) would alter respondents view of their local government's rule adherence in their *commune* (ADM2 level) which is located in the region of Sikasso, unless people have strong convictions that the information is incredible in their commune.

- Out of the 10 PACT projects implemented in your commune, in how many do you believe that the local council and mayor of your commune followed the formal rules of PACT project selection?
- Which of the following statements on how the PACT project has been selected is closer to your own view?
 - 1)It has been selected to respond to the needs of all citizens of my commune
 - 2)It has been selected to respond to the needs of some individuals/families or groups

In addition to these main mediators, our survey also includes a series of indicators of potential alternative mechanisms linking the treatment to the outcomes of interest. In particular, we assess whether information on external controls of PACT projects influences (1) people’s perceived necessity to make own contributions to PACT projects, (2) their assessment of the extent of social capital in their communities, (3) their understanding of the willingness of the state to cater for the needs of the population or (4) their perceptions of community members’ expectations that everyone should contribute to PACT projects. We investigate these alternative variables in the same way as indicators of our main mediator variables (see section 7.2).

5.4 Moderators (interaction)

As described above, we expect that the magnitude of the treatment effects depend on certain pre-treatment beliefs (see section 2).

Pre-treatment rule adherence: One of the most important moderator that the treatment aims to alter is people’s pre-treatment perception of the state’s adherence to formal rules and principles overall and in public service provision projects in particular. To capture people’s pre-treatment beliefs, we rely on the following item.

Out of 10 PACT projects in your commune, how many do you think have been selected according to the formal PACT rules of project selection?

Preferred information source: In order to assess the moderating role of respondents’ preferred information source—technical expert versus citizen—we proceed as follows. First, we use the following pre-treatment item to elicit the preferred information source:

Let’s say someone tells you that he or she has visited a development project and found that it has been selected and implemented strictly according to formal rules. Whose assessment would you trust more:

- 1) Fellow citizens
- 2) Technical experts sent by the government

Then we create a variable `Source_Match` which takes the value of 1 or 0. The variable

takes the value of 1 for the following matches: Treatment 1 (technical experts) is a match if respondents chose option 2 and treatment 1 (citizen) is a match if respondents chose option 1. Otherwise the variable takes the value 0.

Preferred information type: To estimate how the preferred information type conditions the effect of the treatment we proceed as follows. We use the following pre-treatment item to elicit the preferred information type.

Imagine a development project is planned in your village. Please tell me which information on the project is most important for you to receive.

- 1)Information on the project type/ location/ costs and duration
- 2)Information on the how the project was selected
- 3)Information on whom to contact to learn more about the project
- 4)Other [specify]

We create the variable `Type_Match` which takes the value of 1 if respondents chose options 2 or 3. Both options reflect that their preferred information aligns with the nature of the information treatment on the allocation and decision-making mechanism. If respondents chose option 1 or 4, we consider their preferred information type to not precisely match the offered information and thus the variable takes the value 0.

5.5 Pre-treatment covariates

In our preferred model specification we include baseline pre-treatment individual level covariates to reduce noise in the outcome measures. Specifically, we will include the following socio-demographic variables: sex, age, education, and formal employment (measured as civil servants or professional occupations).

5.6 Attention level

Telephone interviews are challenging for both the respondent and the enumerator. Poor network connection, background noise, lack of face mimic, etc. undermine attention and create distractions and can amplify acquiescence effects and yea-saying. We integrated two inverse items before the treatment section to assess respondents' attention levels.

- How much do you agree with the following statement? “I find it **very easy** to find information about planned development projects in my commune.”

- 1)Strongly agree
- 2)Agree
- 3)Disagree
- 4)Strongly disagree

- How much do you agree with the following statement? “I find it **very hard** to find out what development projects are planned in my commune.”

- 1)Strongly agree
- 2)Agree
- 3)Disagree
- 4)Strongly disagree

Respondents receive the following attention reminder if both their responses to these inverse questions are either agree and strongly agree, or disagree and strongly disagree: “We have noticed that you have given contradictory responses to two of our previous questions. Maybe our question was not fully clear to you or you have been distracted when we asked the question. We kindly ask you to try to be attentive to the survey and to let us know if you have difficulties understanding a question. Are you ready to continue?”.

Put differently respondents need to respond consistently to both questions. While we hope that respondents improve their response behavior upon receiving the attention reminder, we will use the attention variable to estimate our models described below in the full sample as well as in a “high attention” subsample. While, we expect similar treatment effects in both samples, we also assume that effects will be more pronounced in the subsample.

6 Setting of the study

This study focuses on the *Programme d’Appui aux Collectivités Territoriales (PACT)* in Mali which is funded both by the German KfW Development Bank on behalf of the German Federal Government. They aim at supporting local public service delivery and local governance through financial contributions to communal investment funds as well as accompanying investment-related measures (i.e., training and technical/financial control). We provide detailed information

on this project setting in our main PAP (De Juan and Koos, 2023).

7 Empirical specifications

7.1 Main effects

We use a linear model (OLS) with the following specification to estimate the intention-to-treat (ITT) effect of our treatments:

$$Y_{iv} = \alpha + \beta_1 T_i + \mu_x \mathbb{X}_i + \epsilon_{it} \quad (1)$$

where Y_{iv} is one of our outcomes of interest (see section 5.1) of individual i in village v . T_i is the individual-level treatment status, \mathbb{X}_i is a vector of individual-level pre-treatment covariates (see section 5.5), commune fixed effects, and ϵ_{it} is the normally distributed error term. Standard errors will be clustered at the village-level. In addition, we will run the same model without pre-treatment covariates \mathbb{X}_i . While we expect similar point estimates due to random assignment, standard errors may be larger and may result in insignificant results in case the ITT effect is relatively small. As explained above, we estimate models in the full sample as well as in a sub-sample excluding respondents that failed the attention test (expecting stronger treatment effects in the subsample).

7.2 Mediators

To assess whether the main effects plausibly operates through one of our mediators, we specify the following ITT model:

$$M_{iv} = \alpha + \beta_1 T_i + \mu_x \mathbb{X}_i + \epsilon_{it} \quad (2)$$

where M_{iv} is one of our mediators of interest (see section 7.2) of individual i in village v . T_i is the individual-level treatment status, \mathbb{X}_i is a vector of individual-level pre-treatment covariates (see section 5.5), commune fixed effects, and ϵ_{it} is the normally distributed error term. Standard errors will be clustered at the village-level. In addition, we will run the same model without pre-treatment covariates \mathbb{X}_i . We are aware of current methodologies (e.g., causal mediation analyses) to estimate the direct and mediator effects of the treatment on outcomes (Imai, Keele, Tingley and Yamamoto, 2011), but refrain from that in our analyses given the criticism

of strong assumption, e.g., regarding sequential ignorability, that could potential bias mediation estimates (Green, Ha and Bullock, 2010). As in our main analyses, we estimate models in the full sample as well as a “high-attention subsample” (expecting stronger treatment effects in the subsample).

7.3 Heterogeneous treatment effects

To evaluate whether the effects of our treatments varies with several pre-treatment conditions, we employ the following specification in a linear model:

$$Y_{iv} = \alpha + \beta_1 T_i + \gamma Z_i + \omega T_i * Z_i + \mu_x \mathbb{X}_i + \epsilon_{it} \quad (3)$$

where Y_{iv} is our outcome measure of individual i in village v . T_i is the individual-level treatment status, Z_i is one of our individual-level pre-treatment moderators (see section 5.4), and $T_i * Z_i$ is the treatment effect conditional on pre-treatment moderators. In addition, we will run the same model without pre-treatment covariates \mathbb{X}_i . We estimate models in the full sample and in the “high-attention subsample”.

7.4 Multiple comparisons

Given the number of hypotheses (2) and outcome variable indexes (2) in the study, multiple comparison testing is a concern. We will report both uncorrected p-values and results from the Romano and Wolf (2016) familywise error rate (FWER) correction. This procedure is more conservative than false discovery rate (FDR) such as for instance the one proposed by Benjamini and Hochberg (1995). We consider the following families for each group of analyses.

- Analysis of main outcomes: 2 hypotheses (joint and source), 4 outcome indexes = 8 families
- Mediation analysis: 2 hypotheses, 2 mediation variables = 4 families
- Heterogeneous treatment effects: 3 hypotheses, 4 outcome indexes = 12 families

7.5 Power calculations

Our power calculations need to take into account multiple treatment arms and the comparisons we aim to assess. In our case, we have one control group and two treatments (T1: information

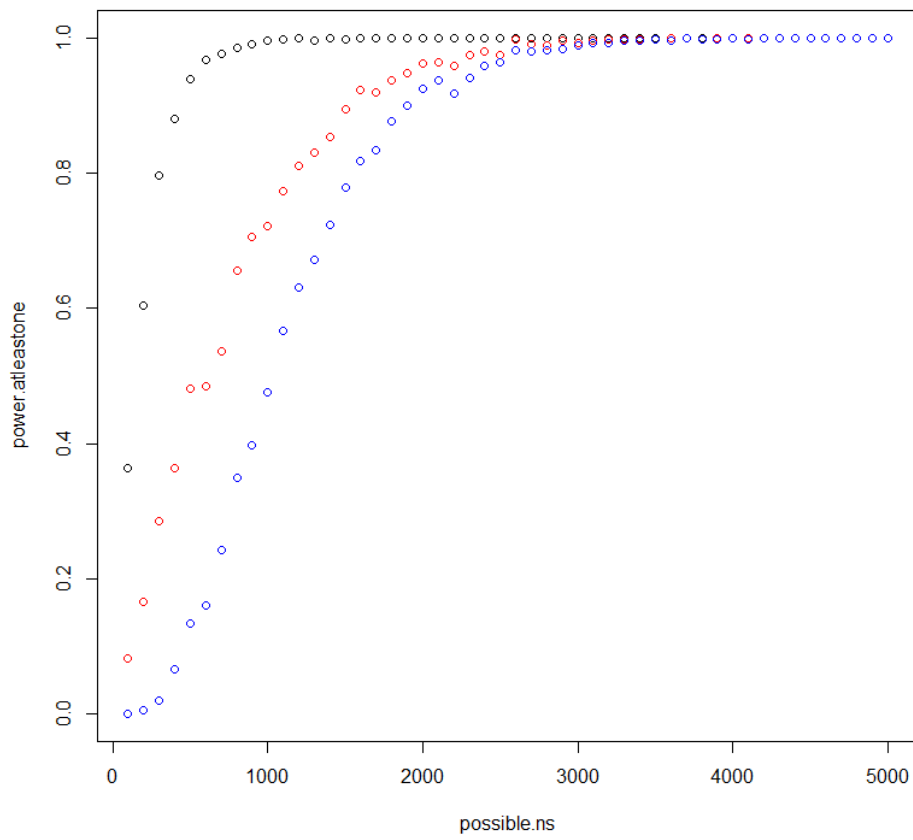
only, T2: information and feedback). We aim to conduct the following three comparisons: i) Control vs. Treatment 1, ii) Control vs. Treatment 2, and iii) Treatment 1 vs Treatment 2. Note that the randomization occurs at the respondent-level, i.e. we do not take into account a clustered design.

We employ EGAP's power analysis for multiple treatments to our expectations.⁴ We make the following assumptions: We assume a standard distribution in our outcome variables with a mean of 0 and a standard deviation of 1. For the average treatment effect of treatment 1, we assume a 0.2 standard deviation change (two-tailed) and for treatment 2, we assume a 0.4 standard deviation change in the outcome, compared to the control group.

Figure 2 shows the relationship between power (y-axis) and sample size (x-axis). The black dots indicate the power for recovering at least one significant hypotheses. The red dots shows the power curve for identifying two significant hypotheses and suggests that due to the Bonferroni correction the sample size needed is more demanding. Our goal, identifying all three hypotheses is identified with the blue dots and requires around 1,600 respondents to achieve 80% power. Our study is thereby well powered with a sample size of around 3,000 respondents.

⁴<https://egap.org/resource/script-power-analysis-simulations-in-r/>

Figure 2: Statistical power for three treatment arms



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