Sacred Land for Sale: Taboo Trade-offs and the Effects of Land-Rights Reform on Prosocial Behavior

Fabbri, M.

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Sacred Land for Sale: Taboo Trade-offs and the Effects of Land-Rights Reform on Prosocial Behavior

Marco Fabbri[*]

Abstract

In this study, I make use of a unique case of large-scale land-rights reform implemented as a randomized control trial to study the effects of institutions on prosocial behavior. The reform transformed informal and collective land tenure by registering individual land rights and making it possible to sell these rights, collateralize them, and defend them in court. Comparing the results of two lab-in-the field experiments designed to elicit cooperativeness, trust, and trustworthiness, I find that receiving well-defined land rights increased prosociality, but only among participants in villages with a relatively high level of market integration. For villagers in isolated communities less exposed to markets, the reform significantly reduced prosocial behavior. These findings resonate with recent models predicting a response

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of outrage and social distancing when land tenure reforms are perceived as an attempt to commodify sacred values. Consistent with this hypothesis, I find that, in communities with low market integration, the land-rights reform strengthened social norms against establishing land markets and using the formal judiciary. This result is not observed in communities more exposed to markets.

JEL-Classification: D02; O17; Z13 ;

Keywords: Lab-in-the-field Experiment; Land Tenure; Public Goods; Randomized Control Trial; Trust Game; Values

1 Introduction

Research has shown that trust and willingness to cooperate are important determinants of economic development (Fernandez and Fogli, 2009, Gorodnichenko and Roland, 2016, Granovetter, 2005, Tabellini, 2010, Zak and Knack, 2001). Therefore, understanding which factors influence the formation and development of these prosocial cultural traits is a key goal for a society's prosperity.[1] This article contributes to a recent wave of studies investigating how the structure and organization of formal institutions impact prosocial behavior.[2] Within this literature, the ongoing debate focuses on the consequences that interventions aimed at strengthening property rights over land in developing countries have on the values and beliefs of individuals. Contrasting hypotheses have been advanced.

One hypothesis states that strengthening property rights over land reinforces prosociality and crowd-in civic goods (Bisin and Verdier, 2001). The results of several empirical studies align with this hypothesis. First, scholars have shown that the enforcement of the rule of law in a country is associated with a higher level of generalized morality (Tabellini, 2008). Second, DiPasquale and Glaeser (1999) started a wave of studies on household behavior that

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[1]When referring to "culture" or "cultural traits", I follow this definition of Guiso et al. (2006): "those customary beliefs and values that ethnic, religious, and social groups transmit fairly unchanged from generation to generation." Moreover, in what follows, for the term "institutions" I use a restrictive definition that excludes informal constraints such as social norms. (For a broader definition of the term "institutions" that would include informal constraints, see, for instance, North and Thomas, 1973). Therefore, I use the terms "institutions" and "formal institutions" interchangeably.

[2]Examples of events and institutional shocks that have been shown to have important and long-lasting cultural effects are political events (Alesina and Fuchs-Schündeln, 2007, Botticini and Eckstein, 2007), wars (Voors et al., 2012), regulations (Gruber and Hungerman, 2008), the experience of economic crisis (Fisman et al., 2015), and the modification of states borders (Becker et al., 2016). For an extensive survey of the empirical literature on institutions and culture, see Alesina and Giuliano (2015).

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found evidence of home ownership increasing social capital accumulation. Third, Di Tella et al. (2007) exploited a natural experiment to show that the assignment of formal land ownership to peri-urban Argentinian squatters increased generalized trust in and beliefs supporting the establishment of a market economy.

In contrast with this positive view of strengthening property rights over land, other scholars have warned that replacing collective and informal land-rights institutions might produce negative social consequences. Some of these researchers argue that assigning formal and individual land rights could negatively affect prosociality by increasing wealth inequality and eliminating the informal insurance provided by collectively owned property (Baland and Francois, 2005, Deininger and Feder, 2009, Ostrom and Hess, 2010), displacing the traditional land-governance institutions trusted by local populations (Arruñada and Garoupa, 2005, Platteau, 1996, 2000), and sparking latent land-related conflicts (André and Platteau, 1998, Arruñada, 2018).

Others have warned that, in traditional societies where land is associated with people's identity and religious or supra-natural beliefs, the concept of holding individual exclusive rights or owning a land parcel is often considered "unthinkable" and establishing a land market remains taboo (Durkheim, 1965, Radcliffe-Brown, 1952, Raz, 1986, Tetlock, 2000).[3] Research contributions from the fields of economics, psychology, and neuroscience have shown that, if individuals do not see land and land-related rights as a standard commodity, then assigning individual rights over land considered as sacred and allowing land to be traded in markets would create resistance to potentially beneficial structural changes, elicit negative emotional reactions, and encourage social distancing (Bénabou and Tirole, 2011, Berns et al., 2012, Fiske and Tetlock, 1997, Roth, 2007).[4] However, research has also shown that what is considered taboo varies considerably in different cultures or across individuals within the same society and that the

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[3]As explained by Fiske and Tetlock (1997, p.274), "In traditional West Africa [...] land is a commons; West Africans can no more rent or sell the right to plant it than Americans can rent or sell the right to swim in the ocean."

[4]Bénabou and Tirole (2011) propose a model to explain the resistance to potentially beneficial structural changes, which is a negative reaction to social innovation often displayed by individuals. In the model, an agent chooses to invest in a "modern identity" or in a "traditional identity." The authors show that, even when the agent expects the transition from the traditional to the modern identity to generate a payoff increase, the agent will underinvest in the modern identity if he or she perceives this investment as an action that devalues the traditional identity being replaced. In a series of experiments, Fiske and Tetlock (1997) and Tetlock (2000) show that, when a society faces the invention or diffusion of new institutions, the tension generated by the "taboo trade-off" produces the anger, disgust, and aversive emotional reactions that engender social distancing. Berns et al. (2012) study the neural correlates that characterize decision-making processes that involve sacred values. Using functional magnetic resonance imaging, the authors show that requests to attach a market price to sacred values activate neural regions responsible for retrieving deontic rules.

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strength of negative reactions dwindles when the secular violations of a sacred value become routine in a society (Tetlock et al., 2000, Tetlock, 2003, Zelizer, 1978). Based on this evidence, Fershtman et al. (2011) propose a model predicting that, if the social heterogeneity with respect to attitudes toward norms and taboos is sufficiently large, a society can be characterized by "multiple identities" concerning what constitutes a violation of sacred values. Thus, while assigning formal and individual property rights over land can be considered a taboo trade-off by the populations of traditional and isolated rural villages, individuals from communities more exposed to market interactions might not perceive the reform as a violation of sacred values.

This paper sheds additional light on how changes in property-rights institutions affect culture. Specifically, I study how a land tenure reform implemented in rural West Africa, one that transforms collective informal land rights into a system akin to private ownership, affected trust, trustworthiness, and willingness to cooperate of the individuals subject to the reform. My research design combines a unique randomized evaluation of the land rights reform with two lab-in-the-field experiments. My main results show that strengthening land rights increases the prosocial behaviors of the participants in the sample who live in communities characterized by relatively high levels of market integration but has the opposite effect on the subjects who are less exposed to market interactions. An analysis of the possible mechanisms underlying the observed behavior suggests that the decrease in prosociality registered in subjects from communities with low market integration stems from a negative reaction to perceived violations of sacred values. That reaction is absent in the participants from communities characterized by higher market integration.

Between 2009 and 2011, the Beninese government, supported by the Millennium Change Corporation and the World Bank, implemented a land-tenure reform called *Plans Foncier Rural* (PFR). The reform, whose details are reported in the next section, consisted of formalizing customary tenure over land characterized by collective property and informal possession. This formalization was achieved by recording the set of rights each individual holds over land parcels, mapping the parcels, and creating a precise boundary demarcation. With the reform, the rights-holders acquired formal, legally recognized use rights that can be traded, used as collateral, and defended in court against contenders. Thus, the reform produced an important shift toward a system of well-defined and individually assigned land-property rights. The key feature of the Benin PFR is that it is a unique case of large-scale land-tenure reform to be implemented as a

randomized control-trial. The implementation was done through a public lottery of hundreds of eligible villages and by the selection of a "treatment group" of villages in which the reform was implemented and a "control group" where no change of the existing customary system took place. The participant pool used in my experiments comprises villagers from the PFR treatment and control groups. Because the lottery produced a random selection of villages in which the reform was implemented, the comparison of cooperativeness, trust, and trustworthiness levels between the two groups allows to identify the causal impact of the land rights formalization on these social preferences.

I measure the effects of the land-tenure reform using an experimental laboratory setting created in each of the 32 villages where the fieldwork was conducted. The lab setting's controlled environment guarantees the anonymity of the participants and allows to silence the effects of social sanctions and other possible strategic considerations. To measure the participants' cooperativeness, I employ a standard linear public goods game (Zelmer, 2003). I collect data on trust and trustworthiness via a standard trust game (Berg et al., 1995).

Throughout the analysis, I additionally check whether, as hypothesized by the theoretical model of Fershtman et al. (2011), societies characterized by unequal levels of market integration and economic development might react differently to the reform. To do so, I estimate the effects of the land-tenure reform conditional to the level of participants' market integration. As a proxy for the level of market integration, I use the distance of a village from the closest paved road. Indeed, the distance from paved roads has been proven to be strongly associated with exposure to markets (Fafchamps, 1992, Goletti et al., 1995), and this proxy had already been used by scholars in economics (Jakiela, 2011, 2015).

Results from the experiments show that, for individuals in the sample that belong to communities with direct access to paved roads and thus the highest market integration, experiencing the formalization of land rights significantly increased contribution to the common account in the public goods game, as well as trustors' transfers in the trust game. The trustees' transfers were also higher on average for the treatment-group subjects compared to the control group, albeit the estimated coefficient is not statistically significant. Point estimates suggest that the land-tenure reform produced an increase of roughly 40% in contributions to public goods and 30% in trustors' transfers. However, these effects progressively vanish and even reverse when the participants' level of market exposure is reduced. A standard deviation increase in the dis-

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tance from paved roads produces a 25% drop in public goods contributions and a 18% and 10% decrease in trustor and trustee's transfers, respectively, for subjects experiencing the reform. This implies that, for participants living in communities characterized by a level of market integration lower than the sample median, the reform significantly reduced the three experimental measures of prosociality.

I verify that these results were not driven by confounding factors like migration across villages or changes in the level of conflicts experienced as a consequence of the reform. I then explore the possible mechanisms determining the observed behavior. Survey data suggest that differences in income, access to credit, and investments in education do not explain the observed effects. Using data from a Dictator game experiment and from a post-experimental survey, I also verify that the results are not driven by changes in altruism or by beliefs regarding generalized morality. Finally, using a set of incentivized coordination games, I show that, in villages characterized by low market integration, the reform significantly strengthened social norms against trading land and against replacing traditional customary authorities with formal institutions for conflict resolution. These results are not observed for participants belonging to communities characterized by higher market integration. These findings support a hypothesis that villagers less exposed to markets perceived the land-tenure reform as an attempt to commodify sacred land and reacted by displaying moral outrage and social distancing in order to reaffirm their traditional identity. Conversely, the reform was not perceived as a sacred value violation by participants who were more used to interacting in markets. In societal contexts where markets are the norm, and in line with existing evidence on the effects of increasing property rights protection in urban and per-urban settings, the reform strengthened prosocial behavior.

These findings are related to the predictions put forth by Hirschman (1982) and tested empirically by Chen et al. (2016)[5] that institutional reforms aimed at facilitating the establishment of a market economy will trigger asymmetric changes in participants' moral values depending on a community's stage of economic development. More generally, this project is related to

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[5]Chen et al. (2016) tests Hirschman's prediction in an online incentivized experiment in which the author manipulates the pay-rate structure in the labor market. The results showed that exposing participants to higher levels of competition led to more utilitarian choices in a subsequent moral dilemma for participants recruited from Western countries, while participants belonging to developing countries reacted by increasing the frequency of deontological choices. My results complement and expand these findings by studying the medium-term effects of an institutional reform on prosocial cultural traits, by looking at an institutional shock that determined real-world consequences, and by testing how different levels of economic development *within* a single society influenced individuals' reactions to an institutional reform.

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studies that attempt to isolate the drivers of behavioral change by comparing groups of individuals within a society that are exposed to different cultural or institutional factors (Bigoni et al., 2016, Gneezy et al., 2016, Jakiela et al., 2015, Lowes et al., 2017).

Finally, from a methodological perspective, this paper contributes to a growing body of research that uses lab-in-the-field experiments to to measure the values and beliefs of subjects living in different societies (Buchan et al., 2009, Henrich et al., 2001, Herrmann et al., 2008). By studying a unique large-scale, randomized control-trial implementation of a major institutional reform, I am analyzing an institutional shock especially suitable for investigating the univariate causal effects of institutions on culture. Indeed, the property-rights reform analyzed in this study had important real-world consequences, unlike laboratory experiments that can only study short-term reactions to the manipulation of artifactual institutions. (For examples of laboratory experiments that study the effects of institutions on preferences, see Bó et al., 2010, Rodriguez-Sickert et al., 2008, Sutter et al., 2010; for a methodological discussion regarding the external validity of the experimental findings, see Loewenstein, 1999 and Henrich et al., 2010.) At the same time, in the randomized evaluation I study, the allocation to the treatment groups was unquestionably exogenous to the agents' culture. In contrast, in many cases the modifications of existing institutions considered by the natural experiment approach might reflect the preferences of the institutions' builders (see, for instance, the arguments proposed by Fischer, 1991, Greif, 1994, Todd, 1990 and the discussion reported in Alesina and Giuliano, 2015). Therefore, this paper contributes to the existing literature focusing on culture and institutions by proposing an innovative approach that balances between the results' internal and external validity.

The remainder of the paper is structured as follows. In the next section, I describe the institutional framework in which the study takes place. Section 3 discusses the experimental design and procedures. Section 4 reports the results. Section 5 discusses possible confounding factors and section 6 examines possible mechanisms underlying the observed behavior. Section 7 concludes.

2 Customary Land Rights and the *Plans Foncier Rural* in Benin

In recent years, systems of formal land ownership registration have been introduced in nearly every African state. Nonetheless, customary land rights still represent the predominant land-tenure arrangement in most rural areas of the African continent. Customary land rights are characterized by a complex set of tenure principles and regulatory mechanisms, usually defined at the village or local level. While a variety of diverse customary arrangements exists, it is possible to identify a set of common features (Delville et al., 2000). Customary rights consist of socially determined land-use rules, where access to land is an integral part of the social structure and tenure is determined by sociopolitical relationships. Governance and enforcement of principles characterizing this system are implemented by customary authorities, who are entities legitimated by previous occupancy or religious customs. The distribution of land rights is based on the sociopolitical local structure and on family relationships (Delville, 2006). This system implies that rights held by individuals are the result of a social and political process of negotiations arbitrated by customary local authorities. This enforcement process has an inherently procedural nature. Rules governing customary arrangements do not provide a precise codification of each landholder's rights. Instead they only state procedures by which an individual obtains access to the land (Chauveau et al., 1998). Therefore, the informal nature of customary rules might be an obstacle to the establishment of well-defined land-property rights. Population growth and the consequent increasing pressure on natural resources create serious concerns for the functioning of informal customary arrangements. Scholars have noticed that the absence of written documentation regarding land use gave rise to increasing conflicts over inheritance and disputes over land use (Deininger and Castagnini, 2006, Goldstein et al., 2016). In Benin, the policy response to problems due to tenure insecurity has been a land-tenure reform known as the *Plans Foncier Rural*. The reform consists of socio-land surveys at the village level to identify rights-holders, their rights, and parcel boundaries. Rights and associated rights-holders are then recorded in public registries, and a process of land demarcation takes place. The process allows for public objection to the proposed registration of rights and requires that rights-holders and neighbors publicly sign survey records (Hounkpodote, 2007). According to the PFR roadmap, following the processes of land demarcation and public registry recording,

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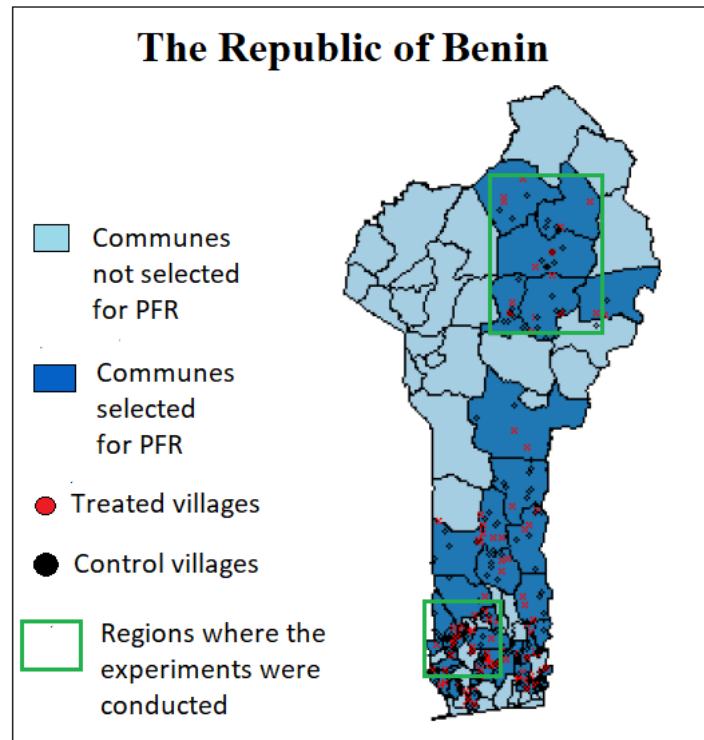
Figure 1: Villages in the *Plan Foncier Rura* included in the lottery pool.

each local administration will create a land registry and issue certificates for each parcel iden-
tified.[6] Remarkably, once the land-demarcation intervention and the recording of rights in a
public registry have taken place, the subsequent process of releasing land certificates is purely
administrative and does not require further action by landholders. Moreover, as clarified also
by Benin State Law 2017-15, even in cases when the local administration has not yet released
the certificates, the recorded rights that constitute the basis for the land-demarcation process
assign to right-holders the use of rights recognized by courts. Given these characteristics, the
PFR reform in Benin instituted a major modification of the institution of property rights over
land by creating a system akin to private ownership.

The PFR pilot program began in Benin in 1993. However, due to a lack of resources,
the reform involved only a small number of villages until 2006, when the Millennium Change
Corporation subsidized a five-year PFR implementation program. The unique aspect of the

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[6]According to the original formulation as stated in the Rural Land Act 2007-003, the local administration
would issue the "Certificat Foncier Rural," that is, land certificates that required registration to assign land
ownership titles ("Titre Foncier"). The new Rural Land Law 2013-01 creates a unique ownership document, the
"Certificat de Propriete Foncier," that reunifies land certificates and ownership titles.

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PFR in Benin is that the implementation followed a randomized control-trial process involving hundreds of rural villages. In fact, this is the first case of a large-scale land-tenure reform implemented in this manner. The objective of the five-year PFR program was to deliver land certificates in 300 rural villages across 40 communes.[7] In the preliminary phase of the PFR project, rural villages were informed of the PFR reform and invited to apply for the lottery. As a second step, each application received was examined to verify whether the village fit certain eligibility criteria.[8] Among the 1,235 villages that applied for the PFR lottery, 576 were judged to be eligible. Once this lottery pool was identified, a subsample of 300 villages was selected via public lottery, and in these villages PFR was implemented.[9] The villages that were not selected for the PFR did not receive any intervention and, as of today, continue to have customary land rights. Figure 1 shows a map of the communes and villages where the PFR reforms took place.

3 Research Design

3.1 Experimental Games

The games played during the experiment were one-shot, and no feedback regarding the games' outcomes was provided until the end of the session. To assess the subjects' cooperativeness, trust, and trustworthiness, a public goods game (PGG) and a trust game (TG) were employed. In the PGG, subjects were divided into groups of three, and the identity of the other group members remained unknown. Each subject received an initial amount of 1,000 XOF (approximately $ 1.5) in the form of 10 coins worth 100 XOF each. The subject could then divide the 10 coins between a "private envelope" and a "common envelope." The coins placed in the private envelope became part of the subject's endowment. Coins placed in the common envelope were increased by 50% by the experimenter and then equally divided among the three group members. Given these parameters, contributing nothing to the public good would be the dominant strategy but such contributions would increase the group earnings.

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[7]Communes are institutional units similar to counties. Benin has 77 communes. The communes that were excluded from participating in the PFR lottery were those where NGOs and other organizations were engaging in other programs of land governance at the time of the PFR design.

[8]The criteria for eligibility were poverty index, potential for commercial activities, regional market integration, local interest in promoting gender equality, infrastructure for economic activities, adherence to the PFR application procedure, the incidence of land conflicts, and the production of main crops.

[9]Since four selected villages refused to complete the program implementation, the sample that implemented the reform numbered 296 villages.

In the TG, players were divided into pairs and randomly assigned the role of trustor or trustee.[10] Each player was initially endowed with five coins worth 100 XOF each. In the first stage of the game, the trustor could send some or all of her coins to the trustee. For each coin sent, the experimenters added two coins. In the second stage of the game, the trustee would decide how many coins to send back to the trustor. The trustee's decision was assessed using the strategy method.[11] Unlike the trustees' choices, the decisions made by the trustors and the decisions made in the PGG were unknown to the experimenter on site. Specifically, the participants privately divided their coins into two envelopes of different colors marked by a code in a separate room and then placed the envelopes in a box. Data on the participants' risk preferences were collected following a lottery-choice task similar to the one used by Voors et al. (2012).[12]

3.2   Procedures

The data collection for this study took place between December 2017 and February 2018. Participants were residents of a sample of villages randomly selected within the PFR lottery pool for the provinces of Coffou, Alibori, and Borgou (highlighted in the rectangular boxes in figure 1). Each experimental session was run in a different village, and 32 experimental sessions were conducted. Approximately 18 subjects took part in each session.[13] In total, 515 subjects (292 from treatment-group villages and 223 from control-group villages) participated in the experiment.

The selection of participants proceeded as follows. The day before the experiment, a member

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[10]In one session, each of the 12 participants, after being informed that the only payoff-relevant decisions would be those taken in the role randomly assigned at a later stage, took decisions as a trustor and subsequently as a trustee. The exclusion of data from this session leaves the results qualitatively unchanged.

[11]The experimenter asked each trustee to state her decision for each of the six possible transfer levels received from the trustor.

[12]Each subject had to make six choices between participating in a lottery or gaining/losing a certain amount. In the initial three choices, participants could choose a lottery with a 3/10 probability of them winning 500 XOF, a 7/10 probability of them winning nothing, or an equivalent gain of 100, 150, or 200 XOF with certainty. In their last three choices, the participants could decide whether to play a lottery in which they had a 3/10 probability of losing 500 XOF, a 7/10 probability of losing nothing, or a certainty of losing of 100, 150, or 200 XOF. I note there that the maximum loss of 500 XOF equaled the show-up fee received and that, by design, none of the games would result in the participants losing money). In order to facilitate the participants' comprehension of the choice alternatives, the experimenter used colored balls representing the probability of gains and losses would be drawn from a bag to determine the probabilistic outcome. A single die was then thrown to determine which of the six lotteries was paid .

[13]Most of the sessions were completed by 18 participants. However, there was some variation in the number of participants – the minimum number was 12 subjects and the maximum 22.

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of the research team informed the local authority (village chief) that the a team of researchers would come to the village to perform research and recruit participants from the village members. None of the villages refused to take part to the study. The day of the experiment, researcher randomly selected nine male and nine female to participate in the study among the individuals who convened at the established time. The selected participants had to be older than 18 years, and a maximum of one member per household was allowed to take part in the experiment. None of the recruited participants had taken part in an economic experiment before.

The experiment sessions took place in a public space (usually a school or a religious building) that included a large common room and a separate room where subjects could make decisions in private. Upon arrival, the participants were randomly assigned a number identifier and completed a brief sociodemographic questionnaire. They were then informed that they had earned a participation fee equal to 500 XOF (roughly $0.75) and that they had the opportunity to earn additional money by participating in a series of tasks. In each session, the participants initially made the decisions in the PGG and TG games described above. They then participated in five additional incentivized experimental games and the sociodemographic survey described in section 6 below.[14] To avoid potential income effects, the participants did not receive feedback regarding the game outcomes until the end of the experimental session. Moreover, the participants were told at the outset that only the income generated in four of the seven games played during the session would be paid and that these four games would be randomly determined at the end of the session by lottery. Since the majority of the participants were illiterate, the instructions for the experimental games were given orally in public by the experimenter.[15] Before they were allowed to enter the decision room, each participant had to correctly answer a set of control questions posed in private by the experimenter. If a participant failed to provide the correct answers, the experimenter repeated the game explanation until the participant could answer all control questions correctly. Each experimental session lasted approximately 3 hours. The participants received an average of $7 as final payment, roughly the equivalent of three days' wages for the subjects in our sample.

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[14]The incentivized games, which were played in the same order during each session, are the following: PGG, TG, coordination games, a risk elicitation in both the losses and gains domains, and a donation game. After completing the experimental games described in this paper, the participants also took part in a modified dictator game conducted in the context of a different research project.

[15]A English translation of the instructions is included in the appendix.

4 Results
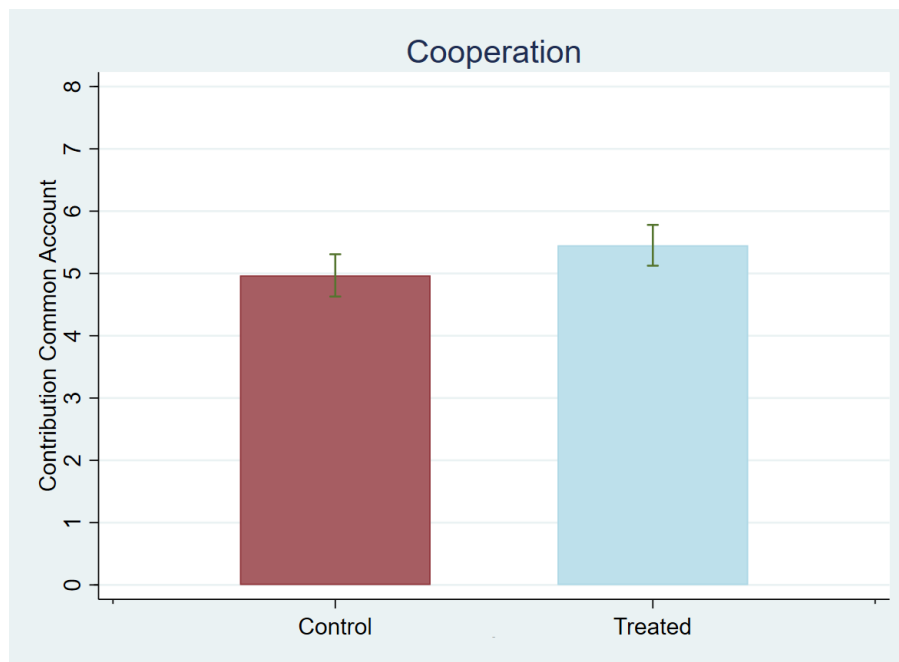
(a) Contribution to common account
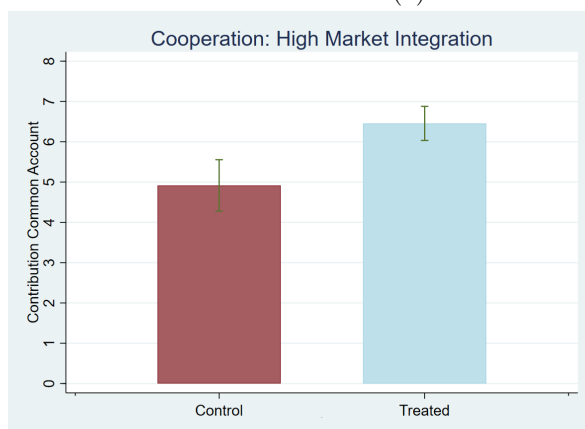
(b) Contribution to common account in high market integration
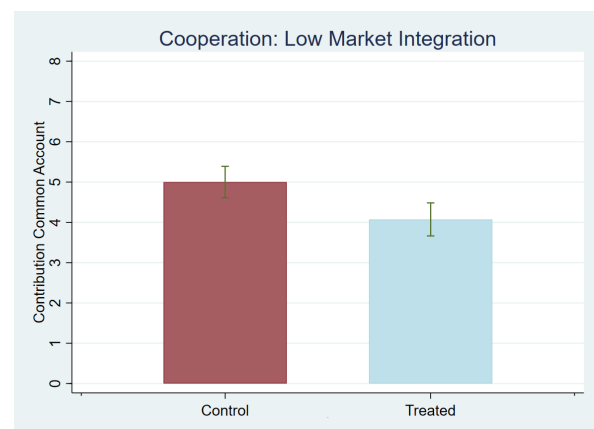
(c) Contribution to common account in low market integration

Figure 2: Contribution to public good

I begin the analysis by focusing on the PGG results. Figure 2a plots the average number of coins that participants contribute to the public good across treatment groups. In our sample, participants who experienced the reform contribute on average more to the common account compared to the control sample. However, a Wilcoxon rank-sum test suggests that the difference is only marginally significant (p-value=.09). With the model of Fershtman et al. (2011) in mind,

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Table 1: Contribution to public good

	Model 1	Model 2	Model 3
Treated	1.880**	-0.905**	2.492***
	(0.946)	(0.392)	(0.928)
distanceroad	0.046		
	(0.042)		
Treated × distanceroad	-0.141**		
	(0.068)		
Controls	Yes	Yes	Yes
Constant	4.705***	4.879***	4.022***
	(1.320)	(1.250)	(1.483)
N.obs.	515	262	253

Notes: Dependent variable: coins contributed to the public good. Censored Tobit regressions. Standard errors robust for clustering at the village level. Model 1 includes the full sample. Model 2 considers the restricted sample of villages with distance to paved roads higher than the median (low market integration). Model 3 considers the restricted sample of villages with distance to paved roads lower than the median (low market integration). Controls include age, gender, estimated measure of risk preferences, religion, whether participant is married, whether participant is monogamous, and a dummy for villages in the South. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% levels, respectively.

I then investigate whether market-integration levels influence the differences in the results. To do so, I divide the participants into two samples of roughly equal numbers using as a threshold the median distance from paved roads of the village of origin (3.75 miles). In figure 2b, the left panel shows the sample of participants living in villages closer than the median (henceforth, the "high market integration" sample). In figure 2c, the right panel shows the sample of participants from villages whose distance from paved roads is larger than the median (the "low market integration" sample). For participants living in high-market-integration villages, the PFR reform's implementation results in an increase in the average contribution to the public account compared to the contribution of the control-group villagers. Formal tests for comparison of the sample means and distributions confirms the visual impression (p-value < 1%, t-test two sided; p-value < 1%, Wilcoxon rank-sum test). The opposite effect occurs regarding those living in low-market-integration villages, that is, where the reform results in a reduction of the participants' contribution to the common account compared to control subjects. Also in this case, formal statistical tests reject the hypothesis that the sample means and distributions are the same for the treatment and control groups (p-value < 1%, t-test two sided; p-value < 1%, Wilcoxon rank-sum test)

I then estimate the effects of the reform on participants' cooperation in a regression frame-

work. Table 1 displays the results of a censored Tobit regression. In Model 1, the amount of coins contributed to the public good is regressed on the dummy variable *treated*, the variable *distanceroad* indicating the village distance from paved roads that is used as a proxy for market integration, the interaction among these variables, and a set of sociodemographic controls.[16] The coefficient of *treated* is positive and statistically significant at the conventional level. The point estimate suggests that the reform caused a roughly 40% increase in the public good contribution for villagers having direct access to paved roads and so the highest market integration in our sample. However, the interaction term *treated*distanceroad* is negative and statistically significant, showing that the positive effect of the reform on the public good contribution progressively vanishes as the distance from paved roads of the participants' village increases.

To better investigate these effects, I consider separately the samples of participants with low and high market integrations. In Model 2, I consider the sample of participants with lower market integration. I regress the number of coins contributed to the public good to the dummy *treated* and the controls specified above. The coefficient of *treated* is negative and statistically significant at the 5% level, suggesting that the reform induced a reduction in the number of coins contributed to the public account for participants living in low-market-integration villages. In contrast, in Model 3, in which the same regression is run on the sub-sample of participants coming from villages closer to paved roads, the coefficient of *treated* is positive and statistically significant at the 1% level, confirming that the reform induced a significant increase in the public good contribution for subjects from villages characterized by comparatively high market integration.

I continue the analysis by considering the participants' choices in the TG. Figure 3a displays the average number of coins sent by the trustee to the trustor in the first stage of the TG. The participants who experienced the reform send on average a larger number of coins. However, a Wilcoxon rank-sum test shows that the difference is only statistically significant at the 10% level. I then consider the villagers in the high and low-market-integration villages separately. Figure 3b and 3c display the average number of coins sent by the trustor in the first stage of the TG for participants living in the sub-samples of villages characterized by high and low market integration, respectively. Villagers who experienced the PFR reform and who live in

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[16]The controls include age, gender, religion, marital status, a dummy equal to one if the subject is monogamous, an incentivized measure of risk preferences, and a dummy equal to one for villages in communes in the South.
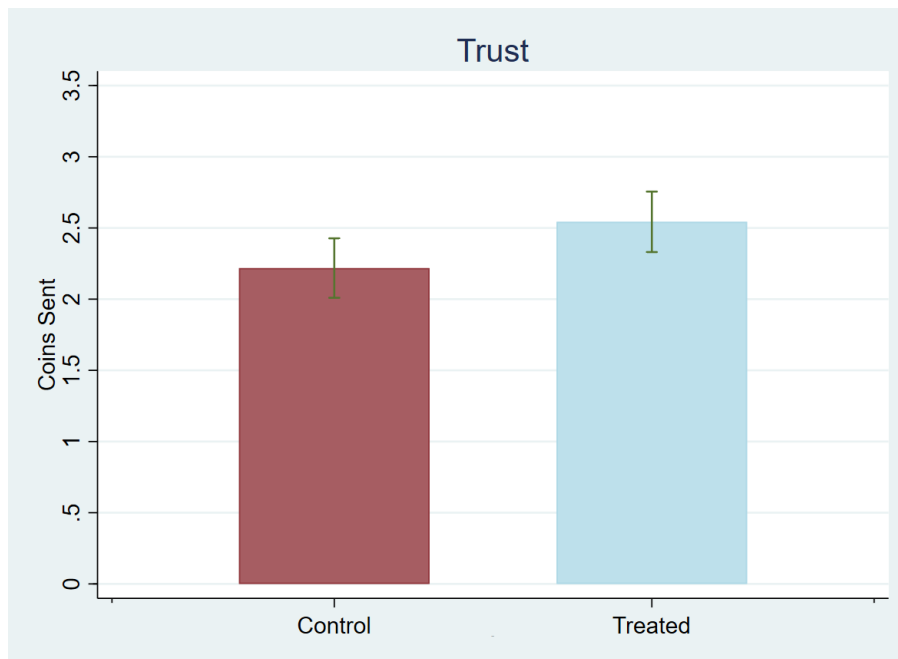
(a) Coins sent to trustee
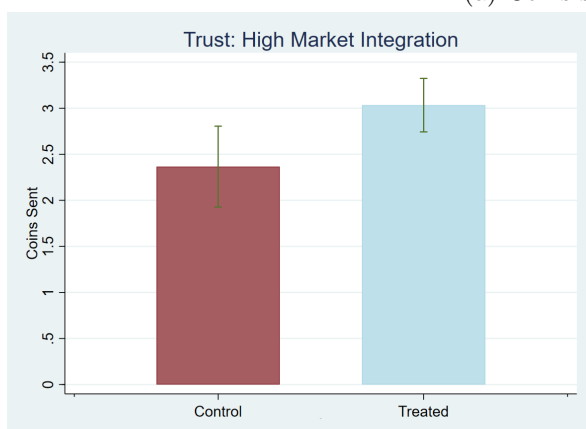
(b) Coins sent to trustee in high market integration
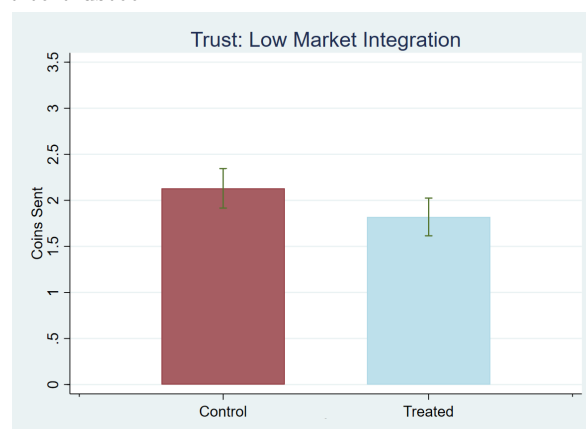
(c) Coins sent to trustee in low market integration

Figure 3: Trustor transfer to trustees

villages with comparatively high market integration on average send more coins than the control-group villagers. This result is confirmed by a Wilcoxon rank-sum test (p-value<1%) and by a two-sided t-test (p-value<5%). The reform produces the opposite effect for trustors living in villages with comparatively low market integration. Indeed, experiencing the PFR for these participants significantly decreases the number of coins sent to the trustee (p-value<5% for both the Wilcoxon rank-sum and t-test two-sided tests).

The results from a Tobit regression, reported in table 2, confirm these findings. In Model

Table 2: Coins sent by trustor

	Model 1	Model 2	Model 3
Treated	0.897**	-0.313*	1.046**
	(0.434)	(0.174)	(0.483)
distanceroad	-0.001		
	(0.017)		
Treated × distanceroad	-0.062**		
	(0.027)		
Controls	Yes	Yes	Yes
Constant	2.408***	2.274***	1.369
	(0.619)	(0.343)	(0.974)
N.obs.	261	130	131

Notes: Dependent variable: coins sent by trustor. Censored Tobit regressions. Standard errors robust for clustering at the village level. Model 1 includes the full sample. Model 2 considers the restricted sample of villages with distance to paved roads higher than the median (low market integration). Model 3 considers the restricted sample of villages with distance to paved roads [higher] than the median ([low] market integration). Controls include age, gender, estimated measure of risk preferences, religion, whether the participant is married, whether the participant is monogamous, and a dummy for villages in the South. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% levels, respectively.

1, the number of coins sent by the trustor is regressed on the dummy *treated*, the variable *distanceroad*, their interaction, and the set of controls described above. The coefficient of *treated* shows a statistically significant increase in the transfers by trustors living in villages with direct access to paved roads. The point estimate suggests a roughly 37% increase in average transfer for these subjects. However, as was the case for contributions to the public good, the interaction term *treated*distanceroad* is negative and statistically significant. In Models 2 and 3, I split the sample and consider separately participants living in villages with comparatively higher or lower market integration by using, as before, the median distance from paved roads as a threshold. For participants coming from villages more distant from paved roads, the coefficient of the variable *treated* in Model 2 is negative and marginally significant (p-value = .07), suggesting that the reform reduces trust as measured in the experiment. Conversely, the positive and statistically significant coefficient of the dummy *treated* in model 3 shows that experiencing the PFR produces an increase in the average trustor's transfers for participants in villages with comparatively high market exposure.

Finally, I look at the average number of coins returned by the trustee in our experiment as a measure of trustworthiness. Trustworthiness choices are assessed using the strategy method. Therefore, each participant decides how many coins to send back for each possible level of
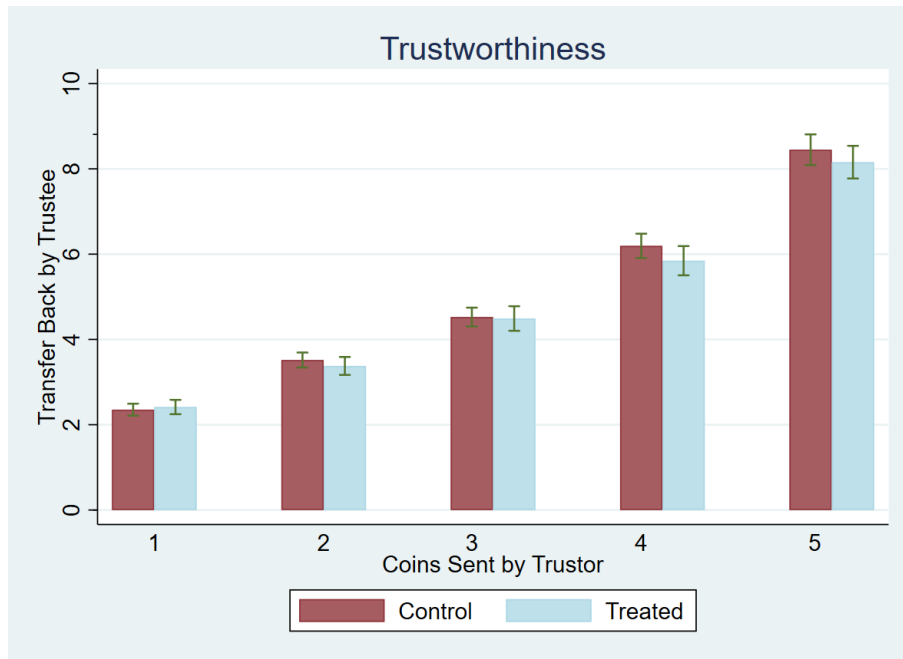
(a) Trustworthiness – whole sample
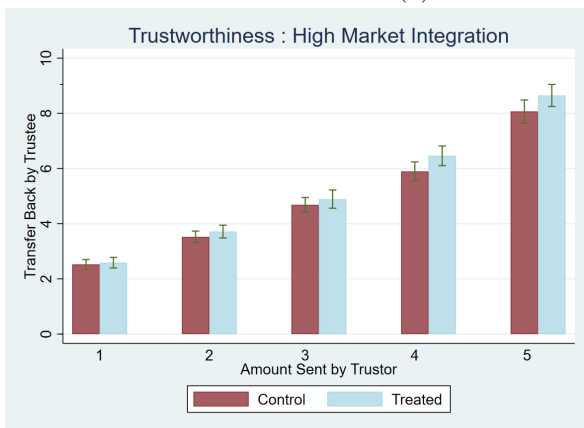
(b) Trustworthiness in high market integration
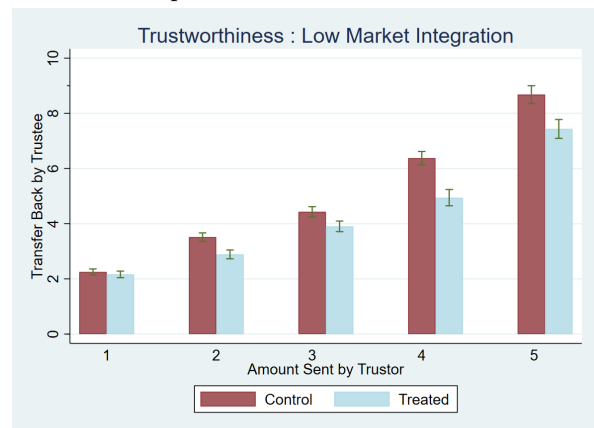
(c) Trustworthiness in low market integration

Figure 4: Trustee transfer back to trustors

the trustor's initial transfer. Figure 4a shows that, in our sample of participants, the levels of trustworthiness are on average left substantially unaffected by the reform. A Hotelling T-squared test confirms that there is no statistically significant difference in the number of coins sent back to the trustee between the treatment-group and control-group participants. Figures 4b and 4c plot the average trustee transfer as a function of the initial amount received by the trustor for participants in villages with high and low market integration, respectively. Trustees on average transfer back more coins in treated villages with higher market integration. However,

Table 3: Coins sent back by trustee

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Treated	0.208	0.333	-0.911***	-1.129***	0.317	0.458
	(0.399)	(0.474)	(0.255)	(0.314)	(0.459)	(0.529)
distanceroad	-0.004	0.002				
	(0.025)	(0.028)				
Treated x distanceroad	-0.056*	-0.076**				
	(0.031)	(0.035)				
Controls	YES	YES	YES	YES	YES	YES
Constant	4.975***	5.609***	4.564***	5.292***	5.784***	6.643***
	(0.664)	(0.765)	(0.701)	(0.826)	(1.036)	(1.257)
N.obs.	1345	1076	670	536	675	540

Notes: Dependent variable: trustee transfer back in the TG. Generalized Linear Latent and Mixed Model. Standard errors robust for clustering at the subject and village levels. Model 1 includes the full sample. Model 2 considers the restricted sample of trustor transfers of more than one coin. Model 3 considers the restricted sample of villages with distance from paved roads higher than the median (low market integration). Model 4 considers the restricted sample of trustor transfers of more than one coin for villages with distance from paved roads higher than the median (low market integration). Model 5 considers the restricted sample of villages with distance from paved roads smaller than the median (high market integration). Model 6 considers the restricted sample of trustor transfers of more than one coin for villages with distance from paved roads smaller than the median (high market integration). Controls include age, gender, estimated measure of risk preferences, religion, whether the participant is married, whether the participant is monogamous, and a dummy for villages in the South. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% levels, respectively.

a Hotelling T-squared test shows no statistically significant difference between the two samples. Conversely, for the low-market-integration condition, trustees in the treatment-group villages transfer back significantly less coins to the trustors compared to trustees in the control-group villages (Hotelling T-squared test two-sided, p-value < 1%).

I then conduct a regression analysis. Given the strategy method used to assess trustworthiness, the data are in the form of a panel of individual choices clustered within villages. Therefore, a hierarchy can be detected in the data structure, and different sources of heterogeneity might arise at different levels of the hierarchy. To capture the correlation between repeated individual measurements and the village-specific effect, I implement a hierarchical mixed-effects regression model.[17] As before, I regress the trustee number of coins sent back to the dummies *treated*, *distanceroad*, their interaction, and the set of controls specified above.

Table 3 displays the results. Model 1 includes the whole sample of observations. The co-

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[17]I specified a three-level model by introducing random effects for the set of trustee choices for each possible level of trustor transfer received within a village and individual trustees nested within villages. Therefore, the trustor-specific transfer comprises the first level of the model, the individual trustee comprises the second level, and the villages comprise the third level.

efficient *treated* is not statistically different from zero, which shows that the reform has no significant effects on the trustworthiness choices of participants living in villages with direct access to paved roads. The coefficient of the interaction term is instead negative and marginally statistically significant, suggesting that being exposed to the reform causes a progressive reduction of participants' trustworthiness when the level of market integration decreases. Model 2 restricts the attention to the sample of trustee choices when the trustor's initial transfer is more than one coin. The results are confirmed, and the coefficient of the interaction term *treated*distanceroad* becomes significant at the conventional level.

Models 3 and 4 repeat the analysis but focus only on the participants in low-market-integration villages. The coefficient of the dummy *treated* is negative and strongly statistically significant in both models. Point estimates suggest that, for participants in communities characterized by lower market integration, experiencing the reform causes a roughly 20% decrease in the average number of coins returned to the trustee. Models 5 and 6 repeat the analysis with a focus on the sample of participants living in high-market-integration villages. The coefficient of the dummy *treated* is positive albeit not statistically different from zero in both cases, confirming the finding that for these subjects on average the reform does not have significant effects.

Taken together, the evidence suggests that in Benin the formalization of property rights over land brought about by the PFR reform has opposing effects on cooperativeness, trust, and trustworthiness according to the level of market integration that characterized the communities experiencing the reform. Specifically, the results suggest that the reform triggers an increase in prosocial behavior for the participants in the sample belonging to communities characterized by relatively high market integration, while for those living in relatively low-market-integrated villages, experiencing the formalization of land rights reduces the level of prosocial behavior. Before turning to the investigation of the channels that determined the observed behavior I first address possible issues that might have affected the interpretation of the findings just presented.

5 Examining Potential Confounders: Migration and Conflicts

Since my objective in this research is to isolate the impact of the PFR on cooperation, trust, and trustworthiness approximately seven years after the reform was implemented, I need to

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verify that the effects estimated did not occur due to individuals with systematically different characteristics self-selecting into the treatment group by migrating across villages within this time period. To do so, I look at data on migration across villages collected in a post-experimental survey. The vast majority of the participants live in the village where they were born. In the seven years preceding my experiment, only 4% of the participants had moved to a different village, and the reason for the few migrations registered was almost exclusively connected to marriage. The results of a Chi-square test reject the hypothesis that the likelihood of migrating out of a treated village differs from the likelihood of migrating out of a control village. The result is the same whether I use the whole sample of participants or focus on the high-market-integration or low-market-integration villages, separately.

Another element that could have affected the observed changes in behavior is the level of conflicts experienced by participants. The PFR reform introduced a process of systematic land demarcation and rights recording that included the resolution of existing land disputes as a preliminary step in the procedure. Therefore, the intervention might have prevented new disputes from arising due to unclear land boundaries and their associated land-user rights. Had this happened, the changes in cooperativeness, trust, and trustworthiness registered might have been a consequence of a reduction in the conflicts experienced by the participants rather than a change in their values or beliefs. I verify this possibility by comparing the self-reported data on conflicts experienced by the participants in the seven years before the experiment. A Chi-square test cannot reject the hypothesis that the likelihood of experiencing conflicts is the same across treatment groups whether I consider the whole sample of participants or divide them according to their villages' high and low market integration. These results are confirmed by a Logit regression analysis that include the set of controls specified above (see table A1 in appendix A).

6   Examining Causal Channels

I now turn to examining potential causal channels for the findings of increased cooperativeness, trust, and trustworthiness displayed by participants in the high-market-integration condition and a concurrent opposite effect for those living in the low-market-integrated condition as a consequence of experiencing the PFR reform.

6.1 Income and access to credit

A potential mechanism underlying the observed changes in behavior is the possibility that the land-rights reform affected villagers' prosperity. The PFR reform might have improved a villager's wealth and financial situation – for instance, because clear, verifiable use-rights over a parcel of land made it easier to access credit. I examine this possibility by first checking for income differences among participants in the treatment-group and control-group samples. A comparison of self-reported income data suggests that the average and the distribution of income are not statistically different between the samples of participants in treatment-group and control-group villages, whether I consider the whole sample of participants or the high or low-market-integration conditions separately (t-test two sided, p-value>10%; Wilconxon rank-sum test, p-value>10%). The results of a regression analysis (see table A2 in appendix A) confirm these findings.

Additionally, I check whether the PFR facilitates the participants' borrowing of resources through the formal or informal credit markets. To do so, I compare self-reported data on the participants' borrowing choices in the seven years preceding the experiment. The difference in the likelihood of borrowing resources or accessing credit is not statistically significant for the participants in treatment-group and control-group villages in each of the three samples considered above (Chi-square test, P-value>10%).

Finally, I re-estimate the equations presented in section 4 by controlling for income (see tables A4, A5, and A6 in the appendix). The results are qualitatively the same as those presented above.

6.2 Education

A second channel through which the PFR reform might have affected behavior is the accumulation of human capital. Indeed, Galiani and Schargrodsky (2010) report that Argentinian peri-urban squatters who received formalized land rights significantly increase investments in their offspring's education. Had the PFR reform produced a similar effect for the participants in our sample, it would be possible to explain the observed behavior through a change in education levels.

I first test whether the fraction of illiterate people differs between treatment groups. A Chi-

square test shows that there are no statistically significant differences (p-value>10%) across the treatment and control groups whether I consider the whole sample or the high and low-market-integration conditions separately. Similarly, there are no differences in the average number or in the distribution of the education years of the participants across treatment groups in any of the three samples (t-test two sided and Wilcoxon rank-sum test in all cases > 10%). The results of a regression analysis (see table A3 in appendix A) confirm these findings. Finally,tables A4, A5, and A6 in appendix A show that controlling for education does not change the results of the estimations presented in section 4.

6.3 Changes in beliefs

I test whether the observed changes in the cooperativeness and trust measures captured in my experiments could be explained by a change in beliefs concerning generalized trust. To perform the test, I rely on the answers to a post-experimental survey question, taken from the standard World Values Survey formulation, that asks whether people can be trusted.[18] A Chi-square test cannot reject the hypothesis that the fraction of respondents answering that people can be trusted is the same across treatment groups at the 10% level for the whole sample of participants or for the high or low-market-integration conditions considered separately. I also test for a change in beliefs concerning generalized trust by employing a series of Logistic regressions, where I control for the usual set of observables. The results (see table A7 in appendix A) confirm the findings described above. The only difference is that in Model 3, in which I focus on the sample of participants in the high-market-integration condition, the coefficient of the dummy *treated* is positive and marginally significant.

6.4 Altruism

I also verify whether the observed changes in cooperativeness, trust, and trustworthiness could be explained by a modification of altruism, a social preference that could affect partici-pants' choices in the experimental games. To test this possibility, I had the participants play a standard dictator game framed as a donation. Specifically, each participant received 10 coins worth 100 XOF each. The participant was then asked to allocate as many of these coins as

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[18]Specifically, I ask, "In general, in your country, would you say that you can trust people (other people) or that people can be trusted?" The possible answers are "One can (cannot) trust people."

he wanted to his own endowment or donate them to a Beninese orphanage located outside the village.[19] As specified for the other games described above, the procedure that was followed – in which the unsupervised participant made the choice by dividing the coins between two envelopes in the decision room – guaranteed anonymity and was not observed by the experimenter on site. Figures A1a and A1b in appendix A plot the average number of coins donated across treatments in the high and low-market-integration conditions, respectively. The distribution and average number of coins donated is not statistically different between the treatment and control groups in both samples (Wilcoxon rank-sum test $> 10\%$; t-test two-sided $> 10\%$). These results are confirmed by regression analysis (see table A8 in appendix A).

6.5 Land market, sacred values, and reactions to taboo trade-offs

From the perspective of the promoters of the PFR in Benin, the land demarcation and formalization of associated rights would increase the protections accorded to individual right-holders by granting them access to the formal legal system. By doing so, the PFR would also facilitate the trading of the now-secured rights and the creation of a land market. However, this logic might be challenged if villagers do not view land and land-related rights as a standard commodity that can be freely traded and so react to the reform with moral outrage and social distancing. To verify how villagers perceived the reform, I investigate how the PFR affected the social norms related to using the formal judiciary and trading village land. The participants took part in two incentivized coordination games similar to Krupka and Weber (2013) in which they were presented with the action of a hypothetical agent.[20] The participants were asked to choose among four possible characterizations of the social appropriateness of the agent's action that ranged from "Very Socially Inappropriate" to "Very Socially Appropriate." In each game, the participants who correctly guessed which characterization would be the modal choice within the village received monetary compensation equal to two experimental coins, each worth 100 XOF. No coins were awarded to the participants who chose any of the other three characterizations. In the first game, the situation involves a conflict over a land parcel between two agents and a judgment by the customary conflict resolution authority that is unfavorable to one party. The participants had to rate the decision of the disfavored party to legitimize the judgment of the

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[19]We donated the amount collected in the experiment to an orphanage in Cotonou.

[20]The complete text of the instructions and situational descriptions used in each game are reported in appendix B.

Electronic copy available at: https://ssrn.com/abstract=3054535

customary authority and to abstain from challenging the unfavorable decision by through the formal judiciary. In the second game, the participants were told of a situation in which a villager turns down a profitable opportunity to sell a parcel of land because the villager believes village land cannot be sold.

Table 4: Social Legitimacy of Traditional Conflict Resolution Authorities

	Model 1	Model 2	Model 3
Treated	0.061	1.336***	-0.292
	(0.287)	(0.278)	(0.315)
Controls	Yes	Yes	Yes
cut1	-2.472***	-3.736***	-1.308
	(0.760)	(0.950)	(1.006)
cut2	-2.366***	-3.696***	-1.155
	(0.741)	(0.955)	(0.972)
cut3	-2.112***	-3.503***	-0.841
	(0.726)	(0.998)	(0.936)
N.obs.	515	262	253

Notes: Dependent variable: Perceived social appropriateness of respecting the judgment of traditional authorities for conflict resolutions (assuming values 1 to 4). Ordinal Logit regressions. Standard errors robust for clustering at the village level. Model 1 includes the whole sample. Model 2 considers the restricted sample of villages with distance from paved roads higher than the median (low market integration). Model 3 considers the restricted sample of villages with distance from paved roads [higher] than the median ([low] market integration). Controls include age, gender, estimated measure of risk preferences, religion, whether the participant is married, whether the participant is monogamous, and a dummy for villages in the South. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% levels, respectively.

Table 4 and 5 display the results of an Ordinary Logistic regression relative to the participants' choices in the first and second game, respectively. The dependent variables – the perceived appropriateness of legitimizing the judgment of the customary authority and of refusing to trade land, respectively – range from 1 (the least appropriate) to 4 (the most appropriate). Regressions include the set of controls specified above. Standard errors are clustered at the village level. Model 1 of table 4 considers the whole sample of participants. The coefficient of the treatment dummy shows that there is no significant difference between the treatment groups and control groups overall. In Model 2, I consider the restricted sample of participants living in the low-market-integrated condition. The coefficient of the treatment dummy is positive and significant at the 1% level. The result suggests that, for participants in this subsample, experiencing the reform strengthened the social norms supporting the legitimacy of the customary authority for dispute resolution. On the other hand, in the sample of participants living in the high-market-integration condition, the effect of experiencing the reform on legitimization of the

Table 5: Social Support for Banning Land Markets

	Model 1	Model 2	Model 3
Treated	0.262	0.845***	0.121
	(0.213)	(0.260)	(0.173)
Controls	Yes	Yes	Yes
cut1	-2.425***	-2.488***	-1.444
	(0.638)	(0.838)	(0.959)
cut2	-2.207***	-2.363***	-1.149
	(0.628)	(0.848)	(0.950)
cut3	-1.901***	-2.031**	-0.839
	(0.614)	(0.815)	(0.948)
N.obs.	515	262	253

Notes: Dependent variable: Perceived social appropriateness of avoiding the trading of land (assuming values 1 to 4). Ordinal Logit regressions. Standard errors robust for clustering at the village level. Model 1 includes the whole sample. Model 2 considers the restricted sample of villages with distance from paved roads higher than the median (low-market-integration condition). Model 3 considers the restricted sample of villages with distance from paved roads [higher] than the median ([low] market integration). Controls include age, gender, estimated measure of risk preferences, religion, whether the participant is married, whether the participant is monogamous, and a dummy for villages in the South. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% levels, respectively.

customary authority is not statistically significant. In table 5, the analysis just described is repeated using as a dependent variable the appropriateness of refusing to trade land. Once again, the coefficient of the treatment dummy is statistically different from zero (at the 1% level) only in Model 2, in which I consider the participants in the low-market-integrated condition. This result suggests that, while in the sample the PFR reform had no significant effect on the social norm regarding selling land for participants with comparatively high exposure to markets, it strengthened the resentment against trading land for participants living in villages with lower market integration.

The evidence presented in tables 4 and 5 supports the hypothesis that two key elements of the PFR reform – using the formal judiciary and trading land – are perceived as taboo trade-offs by participants in villages characterized by low market integration. This result suggests that, in line with the predictions put forth by Fershtman et al. (2011) and the empirical evidence summarized above, the decline in the experimental measures of prosociality registered in this sample of participants reflects feelings of moral outrage and social distancing following the perceived violations of sacred values. On the other hand, for agents used to interacting in a market environment, the possibility of trading land and resorting to the formal judicial system is more easily accepted as a routine activity, one not perceived as a moral violation of sacred

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values. For these subjects, receiving formalized land rights had the effect of increasing prosocial behavior, a result consistent with previous empirical studies on the effects of ownership in urban settings and industrialized societies (DiPasquale and Glaeser, 1999, Di Tella et al., 2007, Field, 2007).

7   Conclusion

This paper studies the impact of a major land-tenure reform implemented in West Africa on prosocial behavior. The reform transformed collective and informal land rights in individual and formally registered use rights that could be defended in court, sold, or used as collateral. The identification strategy makes use of the peculiar implementation process of the reform as a large-scale randomized control trial. From a sample of hundreds of villages, half were selected by means of public lottery for the reform's implementation. As of today, the villages not selected for the reform continue to follow the traditional system of customary land rights. The participants pool in my experiment comprises individuals belonging to 32 villages randomly selected from the PFR lottery pool. Three experimental measures of prosociality – willingness to cooperate, trust, and trustworthiness – are assessed using a public goods game and a trust game.

I find that the PFR reform has heterogeneous effects on prosociality and that the direction of these effects depends on the level of market integration characterizing the community where the subjects live. The reform significantly increases prosociality in communities in the sample with relatively high market integration, where distance from paved roads is used as proxy for market integration. However, the increase in prosociality falls progressively as the level of market integration diminishes. As a result, when I split the sample villages in half according to their distance from a paved road, the participants in the sample of villages with lower market integration exhibit significantly reduced prosocial behavior due to the reform.

These findings resonate with previous evidence showing that receiving well-defined property rights increases prosociality for subjects living in urban and peri-urban contexts. The results are also in line with recent models where the attempt to commodify what is perceived as a sacred value – land in the setting studied here – produced moral outrage and social distancing. Consistent with such a mechanism, I find that, after experiencing the reform, the communities characterized by low market integration developed social norms favoring the ban of a land

market and the predominance of customary dispute resolution mechanisms over the formal state judiciary. These effects did *not* occur in communities with relatively high market integration.

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Appendix A: Supplementary Material and Data Analysis

Table A1: Probability of experiencing conflicts

	Model 1	Model 2	Model 3
Treated	-0.410	-0.516	-0.197
	(0.309)	(0.338)	(0.588)
Controls	Yes	Yes	Yes
Constant	-1.169**	-0.750	-1.659
	(0.471)	(0.584)	(1.066)
N.obs.	515	262	253

Notes: Dependent variable: dummy *conflict* (equal to one if the respondent experienced conflicts in the previous eight years). Logistic regression. Standard errors robust for clustering at the village level. Model 1 includes the whole sample. Model 2 considers the restricted sample of villages with distance from paved roads higher than the median (low market integration). Model 3 considers the restricted sample of villages with distance from paved roads [higher] than the median ([low] market integration). Controls include age, gender, estimated measure of risk preferences, religion, whether the participant is married, whether the participant is monogamous, and a dummy for villages in the South. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A2: Income

	Model 1	Model 2	Model 3
treated	-582.494	682.486	-2922.948
	(2113.787)	(1826.312)	(4460.075)
Controls	Yes	Yes	Yes
Constant	2664.128	1284.418	2605.041
	(3834.331)	(3164.148)	(8025.243)
N.obs.	515	262	253

Notes: Dependent variable: Income. Ordinary Least Square regressions. Standard errors robust for clustering at the village level. Model 1 includes the whole sample. Model 2 considers the restricted sample of villages with distance from paved roads higher than the median (low market integration). Model 3 considers the restricted sample of villages with distance from paved roads [higher] than the median ([low] market integration). Controls include age, gender, estimated measure of risk preferences, religion, whether the participant is married, whether the participant is monogamous, and a dummy for villages in the South. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A3: Education

	Model 1	Model 2	Model 3
treated	0.036	0.153	-0.130
	(0.180)	(0.219)	(0.214)
Controls	Yes	Yes	Yes
Constant	3.043***	2.599***	3.302***
	(0.384)	(0.715)	(0.352)
N.obs.	515	262	253

Notes: Dependent variable: years of education. Ordinary Least Square regressions. Standard errors robust for clustering at the village level. Model 1 includes the whole sample. Model 2 considers the restricted sample of villages with distance from paved roads higher than the median (low market integration). Model 3 considers the restricted sample of villages with distance from paved roads [higher] than the median ([low] market integration). Controls include age, gender, estimated measure of risk preferences, religion, whether the participant is married, whether the participant is monogamous, and a dummy for villages in the South. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A4: Contribution to the public good controlling for income and education

	Model 1	Model 2	Model 3
Treated	1.877**	-0.918**	2.526***
	(0.942)	(0.417)	(0.914)
distanceroad	0.047		
	(0.042)		
Treated $\times$ distanceroad	-0.141**		
	(0.069)		
Controls	Yes	Yes	Yes
Constant	4.214***	4.865***	3.289**
	(1.367)	(1.268)	(1.516)
N.obs.	515	262	253

Notes: Dependent variable: coins contributed to public good. Censored Tobit regressions. Standard errors robust for clustering at the village level. Model 1 includes the full sample. Model 2 considers the restricted sample of villages with distance from paved roads higher than the median (low market integration). Model 3 considers the restricted sample of villages with distance from paved roads [higher] than the median ([low] market integration). Controls include income, years of education, age, gender, estimated measure of risk preferences, religion, whether the participant is married, whether the participant is monogamous, and a dummy for villages in the South. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A5: Coins sent by trustor controlling for income and education

	Model 1	Model 2	Model 3
Treated	0.866**	-0.322*	0.981**
	(0.414)	(0.182)	(0.458)
distanceroad	-0.002		
	(0.017)		
Treated $\times$ distanceroad	-0.061**		
	(0.026)		
Controls	Yes	Yes	Yes
Constant	2.483***	2.209***	1.595
	(0.639)	(0.344)	(1.046)
N.obs.	261	130	131

Notes: Dependent variable: coins sent by trustor. Censored Tobit regressions. Standard errors robust for clustering at the village level. Model 1 includes the full sample. Model 2 considers the restricted sample of villages with distance from paved roads higher than the median (low market integration). Model 3 considers the restricted sample of villages with distance from paved roads [higher] than the median ([low] market integration). Controls include income, education, age, gender, estimated measure of risk preferences, religion, whether the participant is married, whether the participant is monogamous, and a dummy for villages in the South. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A6: Coins sent back by trustee controlling for income and education

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Treated	0.223	0.327	-0.878***	-1.106***	0.297	0.373
	(0.403)	(0.478)	(0.260)	(0.328)	(0.467)	(0.577)
distanceroad	-0.003	0.002				
	(0.024)	(0.027)				
Treated X- -distanceroad	-0.058*	-0.077**				
	(0.031)	(0.035)				
Controls	YES	YES	YES	YES	YES	YES
Constant	5.453***	6.139***	5.365***	6.171***	6.200***	7.150***
	(0.709)	(0.819)	(0.785)	(0.915)	(1.161)	(1.436)
N.obs.	1345	1076	670	536	675	540

Notes: Dependent variable: trustee transfer back in TG. Generalized Linear Latent and Mixed Model. Standard errors robust for clustering at the subject and village levels. Model 1 includes the full sample. Model 2 considers the restricted sample trustor transfers of more than one coin. Model 3 considers the restricted sample of villages with distance from paved roads higher than the median (low market integration). Model 4 considers the restricted sample of trustor transfers of more than one coin for villages with distance from paved roads higher than the median (low market integration). Model 5 considers the restricted sample of villages with distance from paved roads smaller than the median (high market integration). Model 6 considers the restricted sample of trustor transfers of more than one coin for villages with distance from paved roads smaller than the median (high market integration). Controls include income, education, age, gender, estimated measure of risk preferences, religion, whether the participant is married, whether the participant is monogamous, and a dummy for villages in the South. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A7: Self-reported Beliefs on Generalized Trust

	Model 1	Model 2	Model 3
treated	0.146	-0.027	0.650*
	(0.229)	(0.280)	(0.379)
Controls	Yes	Yes	Yes
Constant	1.310***	1.719**	1.249
	(0.425)	(0.726)	(0.780)
N.obs.	515	262	253

Notes: Dependent variable: self-reported trust in other people (dummy equal to one for those reporting other people can be trusted). Logit regressions. Standard errors robust for clustering at the village level. Model 1 includes the whole sample. Model 2 considers the restricted sample of villages with distance from paved roads higher than the median (low market integration). Model 3 considers the restricted sample of villages with distance from paved roads [higher] than the median ([low] market integration). Controls include age, gender, estimated measure of risk preferences, religion, whether the participant is married, whether the participant is monogamous, and a dummy for villages in the South. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% levels, respectively.

(a) Altruism in high market integration

(b) Altruism in low market integration
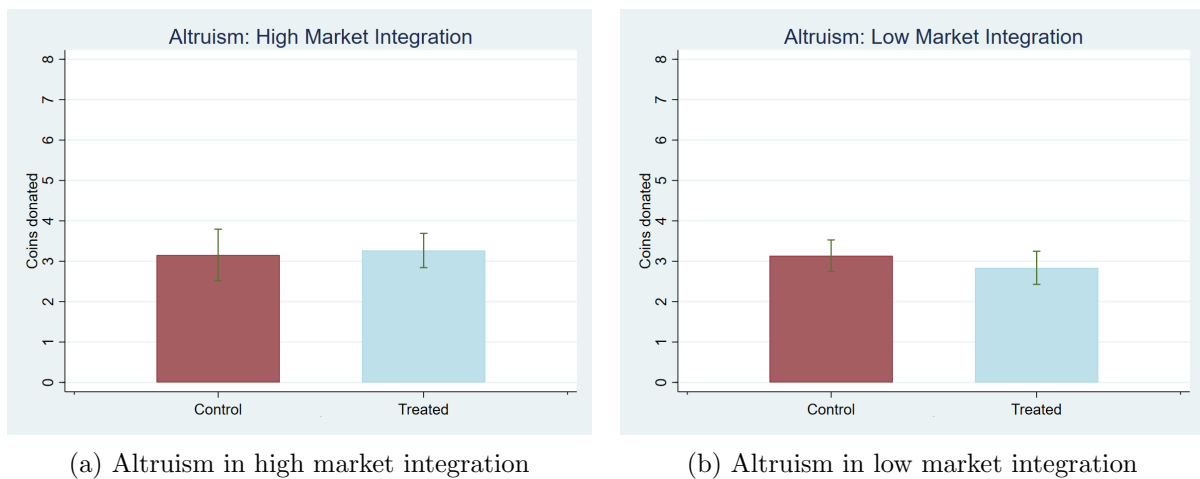
Figure A1: Donation in dictator game

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Table A8: Coins donated in dictator game

	Model 1	Model 2	Model 3
Treated	-0.061	-0.195	-0.001
	(0.273)	(0.334)	(0.412)
Controls	Yes	Yes	Yes
Constant	4.009***	4.547***	4.153***
	(0.612)	(0.666)	(0.823)
N.obs.	261	130	131

Notes: Dependent variable: coins donated. Ordinary Least Square regressions. Standard errors robust for clustering at the village level. Model 1 includes the whole sample. Model 2 considers the restricted sample of villages with distance from paved roads higher than the median (low market integration). Model 3 considers the restricted sample of villages with distance from paved roads [higher] than the median ([low] market integration). Controls include age, gender, estimated measure of risk preferences, religion, whether the participant is married, whether the participant is monogamous, and a dummy for villages in the South. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5%, and 10% levels, respectively.

Appendix B: Instructions for Experimental Games

Thank you for coming to today's meeting. Please note that, if you do not feel comfortable, you are free to leave this meeting at any point of time. Today's meeting starts with 7 games in which you have to make some choices. During the games, you will have the chance to earn a substantial amount of money. The money you earn, together with the 500 XOF for showing up today, will be paid out at the end of the meeting. Specifically, you will be paid:

- The 500 XOF for showing up today

- The money you earn in 4 games. To determine which 4 games will be selected for payouts among all the games you will play today, we will draw 4 numbers from this bag, and the game corresponding to the number extracted will be the one paid. This means that you should take your decisions in all 7 games seriously because there is a very high chance that any one game will become relevant to your payment!

The meeting will last for some hours, and, to receive payment, it is necessary that you attend the meeting until the end. No one other than me will know what you earn today. The payment will be private. You should know that the money comes from research funds and not from our own pockets or from the pocket of politicians. Please note that there is no right or wrong in making the decisions. This is not a test. During today's session you will receive a code. This ensures that everything you do – your decisions and your answers in questionnaires – will remain anonymous. During the 7 games, we will speak of coins. One coin is worth 100 XOF in the 4 games that will be chosen for payment. In the other 3 games, the coins will be not converted to money.

Public goods game (NEVER CALL IT THIS IN FRONT OF THE PARTIC-IPANTS!)

The outcome in this game depends on your decisions and the decisions of two others in this meeting. Note that you will never know who these two others are and these two others will never know that they played with you. You and the two others will have to make the same decision. Here are two envelopes. In one envelope, which is denoted as your envelope, are 10

coins worth XOF 100 each. These coins are yours. The other, which is denoted as your group envelope, is empty. You decide how many of the 10 coins you transfer to your group envelope.

What happens if you transfer [points] to your group envelope? First, of course, you will have fewer points in your envelope. Second, for every point you transfer to the group envelope, we will add 0.5 coin. Thus, if you transfer (e.g.) 10 coins, we will add 5 coins and there will be 15 coins in the group envelope. If you transfer nothing, we will not add points to the group envelope.

What happens to the points in the group envelope? They will be equally distributed among all participants in your group, including you. So, if there are 15 coins in the group envelope, you and the other two in your group get 5 coins. You do not know how many coins the others transfer to the group envelope. The other two participants in your group will also have to decide how many points they transfer to the group envelope before knowing the decisions of their group members.

Example: Imagine all three participants (including you) decide to transfer no points to the group envelope. Thus, there are no coins in the group envelopes and all three participants stay with their 10 coins in their private envelope. Imagine now all three participants including you decide to transfer all 10 coins to the group account, that is, there are $30 + (0.5*30) = 45$ coins in the group envelopes. We will then divide the 45 coins equally and each of you will receive 15 coins.

Example: Imagine Participant 1 gives 10 points to the group envelope, Participant 2 gives 0 points to the group envelope, and you give 4 points to the group envelope. We will then add 0.5 points for each point in the group envelopes, that is, there are $14+ (10 + 0 + 4) * 0.5 = 21$ coins. Then we divide these coins equally among the three participants so that all get 7 coins in addition to the coins they kept in their individual envelopes. So, Participant 1 gets $0 + 7 = 7$ coins, Participant 2 gets $10 + 7 = 17$ coins, and you get $5 + 7 = 12$ coins. Note that Participant 2 received more points than you and Participant 1 because he did not transfer any coins to the group envelope. In contrast, Participant 1 received less because he transferred all 10 coins to the group envelope.

Do you understand? While you make your decision, I will turn my back. Please do not tell me what you plan to do. Please decide now and transfer the amount of points you want from this envelope to the other and then put the two envelopes in the box in front of you. Tell me

when you are ready!

Trust game (NEVER CALL IT THIS IN FRONT OF THE PARTICIPANTS!)

In this game, participants are matched in couples. You will never know with whom you are playing and the other will not know that s/he is playing with you. There are two roles: Participant 1 and Participant 2. You will be randomly assigned to one of the two roles. Both participants receive 5 coins initially. Participant 1 can send some of the 5 coins to the other participant. All coins that you send will be tripled by us before being passed to the other participant. After receiving coins from Participant 1, Participant 2 will decide how many of the tripled coins he sends back to Participant 1. Then this game is over. The outcome in this game will be the coins each participant has after Participant 2's decision.

Example: Imagine Participant 1 sends 0 coins to Participant 2. This means no coins are sent, and Participant 2 cannot send any coins back. Therefore, the game ends with the initial coins for both participants: Participant 1 keeps 5 coins and Participant 2 keeps 5 coins.

Example: Imagine Participant 1 sends 3 coins to the other participant. As mentioned before, we will triple these coins, which means that Participant 2 gets 9 coins. Therefore, Participant 2 has now 14 coins: 9 coins received plus the 5 coins from the start. Then Participant 2 decides how many of his/her 14 coins s/he sends back. Imagine Participant 2 sends back 0 coins. Then, Participant 2 will still have 14 coins and Participant 1 will have 5-3 = 2 coins. Imagine Participant 2 sends back 5 coins. Then Participant 2 will have 14-5 coins = 9 coins and Participant 1 will have 5-3+5 coins = 7 coins.

Is this clear? Shall I repeat?

Risk elicitation (NEVER CALL IT THIS IN FRONT OF THE PARTICI-PANTS!)

I will now present you two options. One option gives you a certain outcome: either you gain for sure or lose for sure some coins. The other option consists of a lottery. The lottery is the following: in this bag, there are 3 orange balls and 7 white balls. We withdraw a ball. If orange, you gain/lose 5 coins; if white, you gain/lose zero. You have to decide if you prefer to gain/lose the coins for sure or to play the lottery. You have to make 6 decisions, where the number of coins that you gain/lose if you choose the "certain" option varies. Only one out of

the 6 decisions will be paid. Once you have told us whether you prefer the certain option or the lottery for the 6 cases, we roll this 6-faced die. The number resulting tells which of the 6 decisions will be paid. If for that decision you chose the lottery, we then extract the ball.

Gain

Question	Certain Gain	Gamble	
		P	gain
1	100	0.3	500
2	150	0.3	500
3	200	0.3	500

Loss

Question	Certain Loss	Gamble	
		P	loss
4	- 100	0.3	- 500
5	- 150	0.3	- 500
6	- 200	0.3	- 500

Instruction for coordination game (NEVER CALL IT THIS IN FRONT OF THE PARTICIPANTS!)

We will describe to you a series of situations. In each situation, a person must make a decision. You will be asked whether taking the action that the person chose in the situation described is "socially appropriate" and "consistent with moral or proper social behavior" or "socially inappropriate" and "inconsistent with moral or proper social behavior." For socially appropriate, we mean the behavior that most people think is the "correct" or "ethical" thing to do. Another way to think what we mean by socially appropriate is that if the person were to select a socially inappropriate choice, then someone else might be angry at the person for doing so. We ask you to indicate whether you think the action chosen by the person is "Very socially inappropriate," "Somewhat socially inappropriate," "Somewhat socially appropriate," or "Very socially appropriate." When all the participants to today's meeting have given an answer for a situation, for each possible choice we determine which response was selected most often. If you give the same response as the one selected most often by other participants, then you receive an additional 10 coins.

Description of the situation 1: *A farmer living in village A is asked by a farmer living in another village to sell him a lot of his land. The price offered for the land is very good and*

higher than the money the farmer living in village A can make with that land.

Action undertaken by the farmer in village A: *The farmer in village A refuses to sell the land to the farmer living in the other village since the land of the village cannot be sold.*

Description of the situation 2: *Person A and Person B enter into a conflict regarding the use of a parcel of land. Person B has been cultivating that land for a few years. Person A claims that that land belongs to him and that Person B in the past could use it because he did not need it. But Person A now wants to sell the property to someone else, so Person B has to leave the land. The chief of the village decides that Person B could keep a part of the land and that Person A can try selling the other part. Person A is not satisfied with the decision, since the buyer of the land wants either all of the land (including the part allocated by the chief to Person B) or nothing. Person A has the right to ask the formal judicial authority of the Republic of Benin to recognize his right over the land and to kick out Person B.*

Action undertaken by Person A: *Person A leaves the situation as it is and forgoes the possibility of selling the land.*