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Property Rights and Prosocial Behavior: Evidence from a Land Tenure Reform Implemented as Randomized Control-Trial

Marco Fabbri*

Abstract

I study the first case of a large-scale land tenure reform implemented as a randomized control-trial in rural Benin to isolate the effects of formalizing property rights on trust and cooperation. The reform transformed informal and collective land tenure by registering individual rights over land and making it possible to sell, collateralize, and defend these rights in court. Seven years after the intervention, results of a public goods game and a trust game show that cooperation and trust substantially increase but only for participants in villages served by paved roads who can benefit from access to institutions and government services introduced by the reform. Conversely, in more isolated communities characterized by larger costs to access institutions, the reform significantly reduced prosocial behavior. An analysis of possible mechanisms suggests that subjects in isolated villages perceived the reform as facilitating institutional shopping for wealthy individuals, thus sparking resentment against the replacement of the customary conflict resolution system and increasing the support for banning the land market.

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1 Introduction

Research has shown that trust and willingness to cooperate are important determinants of economic development (Fernandez and Fogli, 2009, 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 which attempt to isolate the causal effects of different land rights institutions on the values and beliefs of individuals. Within the literature studying the social impact of formalized property rights, contrasting hypotheses have been advanced.

One hypothesis states that stronger property rights reinforce prosociality and crowd-in civic goods (Bisin and Verdier, 2001, Tabellini, 2008). Scholars have shown the enforcement of the rule of law in a country is associated with a higher level of generalized morality and that property titles favors social capital accumulation (DiPasquale and Glaeser, 1999, Galiani and Schargrodsky, 2010, Tabellini, 2010). In contrast with this view, other scholars observe that the introduction of formal ownership rights may erode moral values and increase social conflicts (Hoffman et al., 1996, Ross and Ward, 1995). Evidence from land rights formalization programs show that replacing collective and informal land-rights institutions could hinder prosociality by increasing wealth inequalities 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), and sparking latent land-related conflicts (André and Platteau, 1998, Arruñada, 2018).

A common problem faced by empirical research investigating the social effects of land tenure reforms is that titling decisions and formalization policies are often endogenous, and hidden causal variables may influence both the titling of land and its supposed consequences. For instance, the implementation of titling projects often starts with the regions that have the best economic outlook (Arruñada, 2012). Moreover, observed modifications of the existing institutional environment that are interpreted as producing a quasi-random allocation of titles across the sample of households could in reality just be the consequence of a change in preferences of the institution builders (Alesina and Giuliano, 2015).

¹When referring to “culture” or “cultural traits”, I follow the definition provided by 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.

In this paper, I contribute to the debate regarding the effects of land rights formalization on prosocial behavior by studying the first case of a large-scale land tenure reform that was implemented as a randomized control-trial (RCT) in rural Benin. The reform transformed collective informal land rights into a system akin to private ownership. I use lab-in-the-field incentivized experiments to isolate the effects of formalizing land rights on trust and willingness to cooperate. Results show that land rights formalization significantly and substantially increases the prosocial behavior of those participants from communities served by paved roads who enjoy a comparative better access to institutions and government services introduced by the reform. However, the reform simultaneously reduces trust and cooperation of subjects belonging to isolated villages for whom there are higher costs to access markets and the formal system of land-disputes resolution introduced by the land rights registration.

In 2010-2011, the Beninese government, supported by the Millennium Challenge Corporation, carried on the implementation of a land-tenure reform called *Plan 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, creating precise boundaries demarcation, and generating official land maps stored in a public repository. With the reform, the right-holders acquired formal, legally recognized use rights that can be traded, used as collateral, and defended in court against contenders. Thus, the reform produced a substantial shift toward a system of well-defined and individually assigned land rights.

The Benin PFR is the first case of land tenure reform to be implemented as a randomized control-trial on a large scale. The implementation was done through a public lottery involving 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 – and that, as of today, maintained the same informal rights. The participant pool used in the experiments described below 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 cooperation and trust levels between the two groups allows to identify the causal impact of the land rights formalization on these prosocial traits.

I measure the effects of the land-tenure reform on prosociality using an experimental laboratory setting recreated 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 rule out that the elicitation of participant’s choices in the experimental games are influenced by social sanctions and other possible strategic considerations. To measure the participants’ cooperation, I employ a linear public goods game (Zelmer, 2003). I collect data on trust via a standard trust game (Berg et al., 1995).

In the analysis, I account for the fact that, in the context of rural villages in a low-income developing country, variations in the socio-demographic, ecological, and institutional character-

istics of the environment in which individuals operate are likely to affect the possibility and costs of accessing markets and state services. This implies that the Beninese PFR intervention, which introduced the opportunity to enforce registered land rights in state courts or to trade them in markets, *de facto* affected villagers with different levels of intensity according to the costs faced for accessing legal or market institutions. Considering that several factors can influence the cost of accessing institutions (i.e. social networks, land value, levels of market integration), here I follow an established literature that links the proximity of roads to economic outcomes and access to government services in the rural contexts of low-income developing countries (Asher et al., 2018, Banerjee et al., 2020, Casaburi et al., 2013, Howe and Richards, 2019, Jakiela, 2015, Porter, 2002, Redding and Turner, 2015). As explained in Section 2.3, first I show that villagers in my sample who live close to paved road enjoy better access, face substantial lower costs, and make a more frequent use of the state services and legal innovations introduced by the reform. I then account for road distance in the main analysis.

Results from the experiments show that, for individuals in the sample that belong to communities with direct access to paved roads, 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. Point estimates suggest that the land-tenure reform produced an increase of roughly 40% in contributions to public goods and 30% in trustors' transfers for those participants. However, these effects progressively vanish and even reverse when the participants' distance to roads grows. A standard deviation increase in the distance from the closest paved road produces a 25% drop in the treatment effect of land reform on public goods contributions, and a 18% decrease of this effect on trustor's transfers, respectively, for subjects experiencing the reform. This implies that, for participants living in communities characterized by distance from paved roads larger than the sample median, the reform significantly reduced the levels of cooperation and trust.

After having verified that these results were not driven by confounding factors like migration or selection of participants across villages, I then explore the possible mechanisms determining the observed behavior. Data suggest that land-related conflicts do not explain the observed changes in behavior. Moreover, levels of income or changes in access to the credit market do not explain the findings – albeit, as I discuss in Section 6, I cannot rule out that the heterogeneous effects on pro-social behavior in my experiments reflect that land titling produced a larger increase in land value for villagers with a comparatively better access to markets. Finally, I present data collected in a follow-up survey indicating that differences in the possibility to access and use government institutions are associated with villagers' use of PFR registries and normative beliefs concerning the effectiveness of state-led mechanisms for conflict resolution. Using an incentivized coordination experiment, I then show that in villages with more difficult access to institutions the implementation of PFR significantly reinforced the social support for customary conflict resolution mechanisms and for banning the land market. Survey responses

126 suggest that, by de facto restricting access to the legal innovations introduced by the reform
127 only to individuals who can afford the costs of the formal judiciary, the behavior of villagers
128 in more isolated communities is driven by their perception of PFR as facilitating institutional
129 shopping and increasing inequality in access to justice.

130 The article is related to the literature investigating the effects of different types of land
131 rights institutions on a vast range of social and economic variables, such as investment, credit,
132 employment, market beliefs, and health (Aragón et al., 2020, Field, 2007, Galiani and Schar-
133 grodsky, 2010, Jacoby and Minten, 2007, Lanjouw and Levy, 2002; for extensive surveys that
134 focuses on investments and agricultural production, see Lawry et al., 2017). My paper con-
135 tributes to the branch of this literature that studies the social effects of land rights reforms by
136 proposing a research design that tackles endogeneity issues common to titling decisions and,
137 at the same time, by using a lab-in-the-field approach that mitigates the concerns for external
138 validity characterizing standard laboratory experiments.²

139 The article is closely related to the contribution of Di Tella et al. (2007). The authors study
140 the consequences of a legal change that determined the allocation of land titles to some, but not
141 all, of a community of Argentinian squatters illegally occupying plots of peri-urban land. Results
142 show that individuals who become legally entitled property owners soon develop beliefs favoring
143 individualism and market support. My paper complements and expands the work of Di Tella
144 et al. (2007) by investigating how the reform affected subjects' willingness to cooperate in
145 addition to generalized trust, and by eliciting participants' choices using high-stake incentivized
146 experiments instead of self-reported survey questions. Moreover, my article collects data from
147 a larger sample of subjects distributed across several villages located in various rural provinces
148 of an entire country, thus providing evidence from a different and less geographically-limited
149 context compared to the case study investigated by Di Tella et al. (2007).

150 The article presented here is also closely related to the work of Fabbri and Dari-Mattiacci
151 (2020) who conduct an experimental study making use of the same tenure reform described here.
152 The authors use a taking dictator game to show that participants who have received individual
153 property rights reduce the amount of resources subtracted from a passive player. Compared to
154 Fabbri and Dari-Mattiacci (2020) who elicit participants' decisions in a non-strategic setting
155 where the final allocations are unilaterally determined by the decision-maker, this paper looks at
156 participants' pro-social choices in strategic games in which a player's final payoff is determined

²The identification strategy I propose does not rely on assumptions regarding the exogeneity of the institu-
tional shock, since being selected to receive land rights via a lottery is unquestionably orthogonal to villagers'
culture. Moreover, participants in my experiments are adults villagers who are making high-stake incentive com-
patible decisions and who are exposed to the real-world consequences of the reform. The incentive-compatible
approach reduces concerns related to the vagueness of non-incentivized survey questions for the elicitation of
social preferences. The combination of a large-scale RCT intervention with lab-in-the-field experiments also
improves on standard laboratory experiments that can only study short-term reactions to the manipulation of
artifactual property 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).

by the combination of her own and the matched players' choices. The strategic settings of the public goods and trust games reproduce in the laboratory stylized dilemmas typical of real-world market interactions. The games outcomes thus reveal the effects of land titling on dimensions of behavior which, compared to the unilateral allocation decisions studied by Fabbri and Dari-Mattiacci (2020), are more informative on the establishment of a market culture and directly linked to economic development (related to this point, see also in Section 5 the results on altruistic preferences that I measured for participants in my study and the discussion that follows).

Finally, the paper is related to the recent contributions of Goldstein et al. (2018) and Huntington and Shenoy (2021) who also study the results of land rights reform implemented as RCT. Compared to these studies that investigate the effects of formalizing land rights on investments, my article focus on the effects of pro-social cultural traits.

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

2 Institutional Framework

2.1 Customary Land Rights and the *Plan 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 (Lavigne-Delville, 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 local customary authorities. The distribution of land rights is based on the sociopolitical local structure and on family relationships (Lavigne-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 secure and 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). In Benin, the policy response to problems due to tenure insecurity has been a land-tenure reform known as the *Plan 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. In the following paragraphs, I summarize the main points of the Beninese PFR, focusing on the aspect most relevant for this study. For a thorough description of the reform characteristics and implementation process, including details on the selection of the villages to be included in the RCT pool and the evaluation of the effects on investments see Goldstein et al. (2018) and Omondi (2019).

According to the PFR roadmap, following the processes of land demarcation and public registry recording, each local administration will create a land registry and issue certificates for each parcel identified.³ The registered 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 formalized ownership.

The implementation of the reform, that was subsidized by the Millennium Challenge Corporation, was carried on by the Beninese government in 2010-2011. The peculiar aspect of the PFR in Benin is that the implementation followed a randomized control-trial process involving hundreds of rural villages. The objective of the PFR program was to formalize land rights in 300 rural villages across 40 communes.⁴ In the preliminary phase of the PFR project, 2062 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.⁵ This process lead to the identification of 575 rural villages that composed the RCT pool. From this pool, a subsample of 300 villages was selected via public lottery, and in these

³According 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. Benin State Law 2017-15 further clarifies that rights registered in the public registries are protected by state laws irrespectively of whether the rightholders has obtained a property title.

⁴Communes 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.

⁵The criteria for eligibility were: whether the village is located in a rural area, 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.

villages PFR was implemented. The villages that were not selected for the PFR did not receive any intervention and, as of today, continue to have customary land rights.⁶ Figure A1 in Appendix A shows a map of the communes and villages where the PFR reforms took place.

2.2 The Reform and Villagers' Perception of Tenure Security

Studies on the effects of land rights formalization programs have shown that in some circumstances the titling efforts were not followed by changes in the existing systems of property rights and that, if not perceived useful by the local populations, formalized rights tend to revert to informality (Ali et al., 2019, Bubb, 2013). For the Beninese PFR, Goldstein et al. (2016, 2018) report results of early impact evaluations and a study of the reform suggesting that it determined important changes in the institutional environment and the perception of tenure security of the involved population. The authors show that women- and minorities-headed households, who under the customary regime enjoyed a comparative low level of tenure security, substantially increased investments in long-term crops in registered land parcels.

Confirming these findings, in a follow-up survey that I administered in 2020 to 594 individuals across 43 villages in the same Beninese communes where the data collection relative to this paper took place, 93% of respondents consider impossible for customary authorities to expropriate the land from an household who has registered PFR rights⁷, and 89% of the sample think that PFR rights are secured even if the rightholder engages in a dispute against a wealthier and more powerful contender. Indeed, 97% of respondents reported that, before purchasing a land parcel, they have requested or would try to obtain from the seller proof of official PFR registration.

⁶As suggested by one Referee, this context can be prone to “John Henry” experimental effect, namely a reactive behavior displayed by subjects in the control group for not having received the land tenure intervention. While I cannot completely exclude this hypothesis for participants in my experiments, the results from a survey administered in 2020 to a sample of respondent from villages included in the PFR RCT pool suggest no marked differences in the reported level of appreciation for government institutions (notice that the Beninese government, and in particular the Ministry of Urban Planning, was officially in charge of the reform implementation). Specifically, participants were requested to report their appreciation for the level of support provided by the government in a Likert scale (from 1-7, with one representing the lowest level of satisfaction). The question stated: “Do you think the central state is helping the villagers enough relative to what it asks them to contribute?”. 566 participants answered the question, with an average of 3.17 in control and 3.22 in treated – the difference is not statistically significant (p-value 0.65, t-test two-sided).

⁷The questions that were asked stated, respectively: “Imagine that a person in the village becomes wealthy and has more land than he and his family need. The village committee / customary authority decides that the wealthy should donate some of their land to poor families in need. The rich have an official title to the property or a certificate of the Rural Land Plan issued by the Republic of Benin which declares that they have the right to use the land. He refuses to give up the land.” and the possible answers were: “1 = Village authorities will force him; 2 = He has the official title, so can keep the land”.

2.3 Heterogeneity in the Costs of Access to Institutions

The Beninese PFR intervention of 2009-2011 produced a substantial effort to demarcate parcels' boundaries and formally register associated customary rights. However, this is only the first step of the reform of land rights institutions. In fact, the maintenance of the registries, including the handling of transfers and the actual enforcement of the certificates, and the use by the villagers of the institutional innovations and state services introduced by the reform is where property rights actually happen. An important finding from the survey is that the accessibility of those institutional facilities which make possible to enforce the rights registered through the PFR – such as formal state courts – is strongly associated with a village's proximity to paved roads. In the article, I refer to a village's "distance to paved roads" as the travel distance by motor vehicle between the village administrative headquarters and the closest paved road.⁸

If we split the sample of participants between those living closer than the median distance to paved roads and the others, in the latter subsample only 9% of the respondents report to know somebody who solved a land-related conflict in a state tribunal, compared to the 41% of respondents living closer to paved roads (the difference is strongly statistically significant, two-sided χ^2 test, $p < 1\%$). These proportions roughly match the share of subjects in our sample who actually experienced a conflict and solved the dispute in a formal court (40% of those living closer than the sample median to paved roads versus 16% of those living more distant).

The finding is easily understood in light of the costs associated to accessing the formal judiciary for these two categories of respondents. Among the respondents who had first-hand experience of a land-related conflict and who solved it in an formal court, those in the sample more distant from paved roads reported to have born total costs more than three times larger on average compared to those participants living in proximity of paved roads (CFA–thousands 1,233 vs. 382; a two-sided t-test shows that the difference is statistically significant at the 1% level).

As a consequence of these large differences in costs to access the formal system, data from the same survey show also that villagers in communities more distant from paved roads rely comparatively less on the formal judiciary. First, proximity to paved road is positively associated with consulting PFR registries. Among the 288 respondents in treated villages, 38% of those living close to paved roads reported to know where official PFR registries are stored and which procedure should be followed to consult them, a statistically significant larger share compared to the 26% of villagers stating so in villages more distant to paved roads (χ^2 test, $p = 2\%$). Similarly, 39% of respondents living in proximity of paved roads confirmed to have consulted or to know somebody who have consulted PFR registries, against 24% in villages far away from paved road (χ^2 test, $p < 1\%$).

⁸For all the 32 villages where the data collection took place the construction of the closest paved road pre-dated the PFR intervention.

3 Research Design

3.1 Identification

The research design is based on comparing participants' choices in villages that had been randomly selected to have the land tenure reform implemented against non-selected villages which maintain customary land rights. In order for this identification strategy to hold, two caveats are in order. First, I need to verify that the random allocation to different property institutions characterizing the original PFR lottery was successful in eliminating pre-reform differences across treatment branches. Following this, I also need to show that the selection of the subsample of villages where the data collection for this study took place resulted in a balanced sample. Second, I need to verify that, after the reform implementation, migration patterns have not generated an imbalance across the treatment branches.

With respect to the RCT implementation of the reform across Beninese villages, a thorough impact evaluations of the reform carried out by the World Bank's Gender Innovation Lab reports evidence that the randomization determined by the lottery was successful (Omondi, 2019). In particular, the World Bank team made use of both a rich set of pre- and post-treatment survey data collected by a national agency, as well as of administrative monitoring and evaluation data independently collected by the MCC-Benin. The impact evaluation, resulting from a cross-evaluation performed by using these independently collected data sources, show pre-intervention balance on outcome variables between treatment groups and dispels residual concerns regarding the randomization resulting from the lottery selection (Goldstein et al., 2016, Omondi, 2019; on the success of the PFR randomization, see also the discussion and additional evidence reported by Goldstein et al., 2018).

Concerning the data collected for this study, participants were residents of 32 villages randomly selected among those in the RCT pool. Table A1 in Appendix A reports descriptive statistics relative to socio-demographic characteristics of these subjects. The sample is well balanced, with only a weakly statistically difference in age across treatments ($p=9\%$) out of the 22 variables reported. When comparing separately the subsamples of participants living closer or more distant to paved roads across treatments, in both cases the comparisons return balanced across subsamples for most of the variables. The most prominent difference consists of participants reporting Islam to be their primary religion being over-represented (25% vs. 5%, $p=.01$), and those believing in Animist religions slightly under-represented (39% vs. 52%, $p=.06$), in the sample of treated villages close to paved roads.⁹ To account for the imbalance, in the analysis I control for these characteristics.

⁹When asked about their religious beliefs, virtually all participants in the sample reported to practice some form of Voodoo, an animist religion traditional in the country, while some additionally reported that they combine traditional Voodoo with Christian or Islamic practices. With this caveat in mind, in the analysis I classified subjects' religion by using an additional survey question that asked participants to choose which of these religions they consider their main/primary religion.

Concerning migration, as a first step 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 villages in proximity of, or more distant from, paved roads separately. Moreover, Tables A6 and A7 in Appendix A replicate the regression models estimated in Tables 1 and 2 in the main text additionally controlling for whether the participant has migrated and the number of years she has lived in the village. Results remain qualitatively unaffected.

As a second step, I need to verify whether, in the seven years following the reform, different patterns of out-migration from treated and control villages that cannot be captured by survey responses collected from my study participants (for instance, because of migration from the village to cities) are registered. To address this, I first rely on pre- and post-intervention survey data reported in the MCC impact evaluation (Omondi, 2019). Out of 3,338 households belonging to villages in the RCT pool who were surveyed before the intervention, only 43 (1,2%) have migrated by the second round of survey in 2015. While the Authors do not report the proportion of migrating subjects who belonged to control or treated villages, these numbers confirms a very low propensity to migrate in this context. Moreover, I show that data from 2017 relative to the population of the 32 villages in my sample display no significant differences in average village size across treatment branches ($p=.26$, t-test two-sided), in line with the above-mentioned absence of imbalances in out-migration in treated and control villages reported by the study participants. Finally, in a survey that I run in 2020 in a sample of 43 Beninese villages included in the PFR, no significant population differences was found between treated and control villages ($p=.85$, t-test two-sided). These pieces of evidence mitigate concerns that migration or selection effects could have compromised the identification strategy.

3.2 Experimental Games

To assess the subjects' cooperation and trust, 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.

In the TG, players were divided into pairs and randomly assigned the role of trustor or trustee.¹⁰ Each trustor was initially endowed with five coins worth 100 XOF each. The trustor could send some or all of her coins to the trustee. For each coin sent, the experimenters added two coins. After having received the trustor’s transfer, the trustee has the possibility to send back coins to the trustor before the game is over.

The cooperation and trust decisions were one-shot, and no feedback regarding the games’ outcomes was provided until the end of the session. To prevent experimenter effects, we implemented a procedure for which the trust and cooperation decisions made by participants 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. In addition to the elicitation of cooperation and trust, in the second stage of the trust game we also collected trustees’ choices regarding how many coins to send back to the trustor. However, due to logistical constraints, the trustees’ decisions were assessed using the strategy method, with the experimenter asking each trustee to state her decision for each of the six possible transfer levels received from the trustor. Thus, unlike trust and cooperation choices, trustworthiness decisions were elicited using the strategy methods and were not blind to the experimenter on site. We report the analysis of data on trustworthiness in Figure A2 and Table A2 in Appendix A. Despite the methodological differences in the elicitation method and experimental procedure, the pattern of results remains similar to those of cooperation and trust. Data on the participants’ risk preferences were collected following a lottery-choice task similar to the one used by Voors et al. (2012).¹¹

3.3 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 A1 in Appendix A). Each experimental session was run in a different village, and 32

¹⁰In 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.

¹¹Each 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. Note 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 .

experimental sessions were conducted. Approximately 18 subjects took part in each session.¹² 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 of the research team informed the local authority (village chief) that 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, researchers 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 5 below.¹³ 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.¹⁴ 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 \$6 as final payment, roughly the equivalent of two days' wages for the subjects in our sample.

¹²Most 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.

¹³The 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.

¹⁴A English translation of the instructions is included in Appendix B.

3.4 Empirical Specifications

In the analysis, I compare the choices of participants resident in treated villages against those in control ones. The main variables of interest c_i and f_i are the number of coins contributed to the common account in the public goods game and the choice sent by the trustor in the trust game, respectively. The main empirical specifications used in the analysis are the following:

$$c_i = \alpha + \alpha_D D_i + \delta_T T_i + \delta_D D_i T_i + X_i + \epsilon_i \quad (1)$$

$$f_i = \alpha + \alpha_D D_i + \delta_T T_i + \delta_D D_i T_i + X_i + \epsilon_i \quad (2)$$

where D_i is the participant's village distance to the closest paved road, T_i is a dummy equal to 1 for subjects in treated villages, and X_i is the vector of individual characteristics collected in the post-experimental survey.

4 Results

I begin the analysis by focusing on the PGG results. Panel 1a of Figure 1 plots the average number of coins that participants contribute to the public good across treatment groups. Participants who experienced the reform contribute slightly more to the common account compared to the control sample (t-test two-sided, $p=4\%$), even if the difference among the samples averages is small. However, the picture changes when separating the villages who are in close proximity to paved road to the others. 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 Panel 1c of Figure 1, it is plotted the amount of coins contributed to the common account in treated and control villages by the sample of participants living in villages closer to paved roads (left bars) or more distant (right bars). For participants living in villages close to paved roads, the PFR reform results in a substantial and significant increase in the average contribution to the public account compared to the contribution of the control-group villagers (t-test two sided; p-value $< 1\%$). The opposite effect occurs regarding those living in villages more distant from roads, that is, where the reform results in a reduction of the participants' contribution to the common account compared to control subjects (t-test two sided; p-value $< 1\%$).

These results are confirmed when participants' cooperation decisions are investigated in a regression framework. Table 1 displays the results of a censored Tobit regression with standard errors clustered at the village level. In Model 1, the amount of coins contributed to the public good is regressed on the dummy variable *treated* and a set of socio-demographic controls.¹⁵

¹⁵The 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

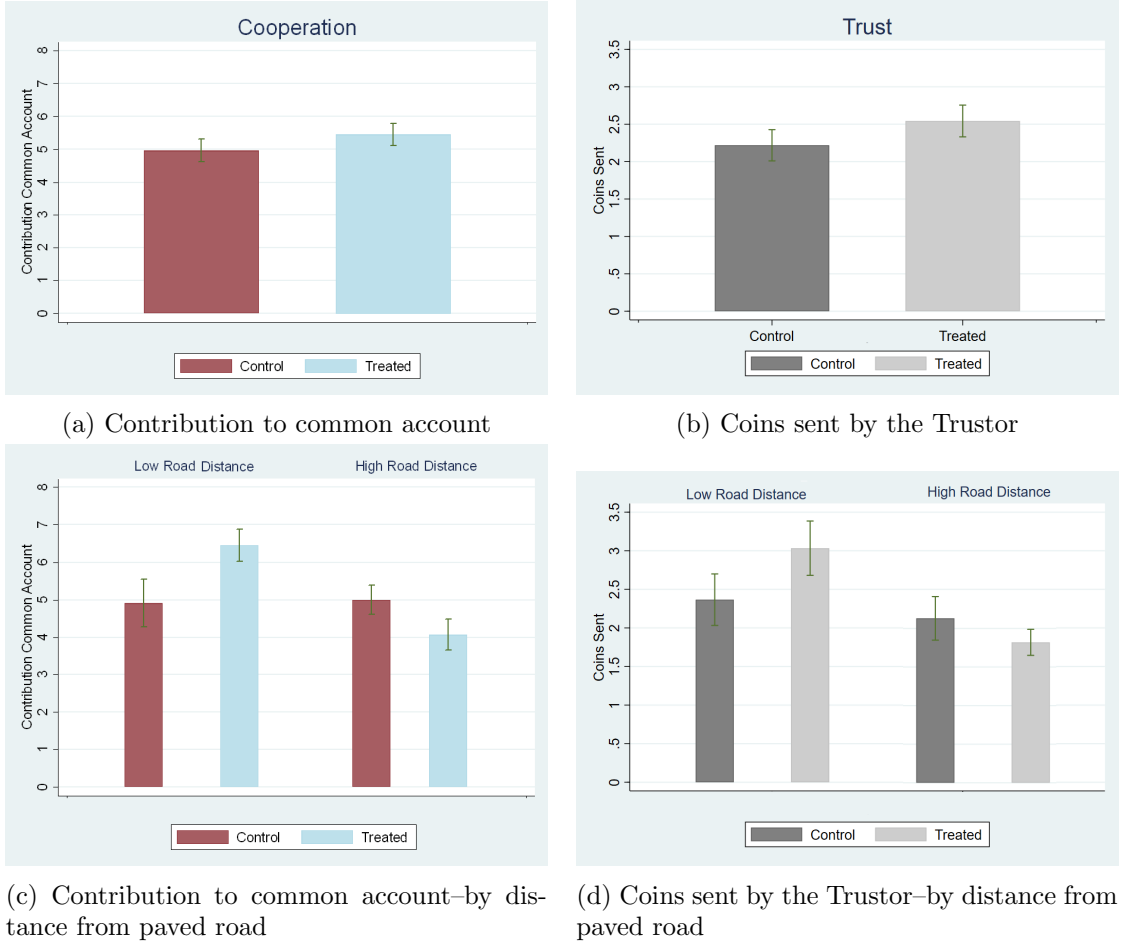


Figure 1: Cooperation and Trust by treatment

The coefficient is positive but small and not statistically different from zero, indicating that the reform had on average no effect on cooperation for participants in our sample. In Model 2, the dummy *treated* is interacted with the variable *road_distance* indicating the village distance from paved roads. The coefficient of *treated* becomes 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 in our sample. The interaction term *treated*road_distance* is instead 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 living in villages whose distance from paved road is smaller or larger than the sample median. In Model 3, I focus on the latter group. 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 dummy for residents in treated villages who took part to the experiments but do not own PFR land.

Table 1: Contribution to the public good

	Model 1	Model 2	Model 3	Model 4
	Whole Sample		High-Dist	Low-Dist
Treated	0.657	1.958**	-0.865**	2.545***
	(0.564)	(0.945)	(0.415)	(0.964)
road_distance		0.049		
		(0.042)		
Treated \times road_distance		-0.143**		
		(0.068)		
Constant	4.488***	4.204***	4.840***	3.297**
	(0.996)	(1.355)	(1.243)	(1.517)
N.obs.	515	515	262	253

Notes: coins contributed to the public good. Censored Tobit regressions. Standard errors robust for clustering at the village level. Models 1 and 2 include the whole sample, Models 3 considers the restricted sample of villages with distance to paved roads higher than the median (“High-Dist”). Model 4 considers the restricted sample of villages with distance to paved roads lower than the median (“Low-Dist”). Controls include: age, gender, household weekly income, education, estimated measure of risk preferences, religion, whether participant is married, whether participant is monogamous, a dummy for villages in the South, a dummy for participants in treated villages who do not own PFR land. Symbols * * *, **, and * indicate significance at the 1%, 5% and 10% level, respectively.

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 the villages more distant to paved roads. In contrast, in Model 4, 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 this category of villagers.

I continue the analysis by considering the participants’ choices in the TG. Panel 1b in Figure 1 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 0.25 coins more than those in control, a small but significant difference (t-test two-sided, $p=3\%$). In Panel 1d of Figure 1b I then consider trust choices in villages close or distant from paved roads separately. The two left bars display the average number of coins sent by the trustors living in the sub-samples of villages characterized by proximity to paved roads. Villagers who experienced the reform in this sub-sample on average send significantly more coins than the control-group villagers (two-sided t-test, $p=1\%$). However, the reform produces the opposite effect for trustors living in villages more distant to paved roads, as shown by the two bars on the right hand-side of the panel. For those participants, experiencing the PFR significantly decreases the number of coins sent to the trustee (t-test two-sided, $p=4\%$).

The results from a Tobit regression with standard errors clustered at the village level, reported in table 2, confirm these findings. In Model 1, the number of coins sent by the trustor

Table 2: Coins sent by the Trustor

	Model 1	Model 2	Model 3	Model 4
	Whole Sample		High-Dist	Low-Dist
Treated	0.394	0.844**	-0.306*	0.927**
	(0.263)	(0.415)	(0.180)	(0.459)
road_distance		-0.002		
		(0.017)		
Treated \times road_distance		-0.060**		
		(0.026)		
Constant	2.107***	2.472***	2.217***	1.514
	(0.585)	(0.644)	(0.347)	(1.078)
N.obs.	261	261	130	131

Notes: coins sent by the Trustor. Censored Tobit regressions. Standard errors robust for clustering at the village level. Models 1 and 2 include the whole sample, Models 3 considers the restricted sample of villages with distance to paved roads higher than the median (“High-Dist”). Model 4 considers the restricted sample of villages with distance to paved roads lower than the median (“Low-Dist”). Controls include: age, gender, household weekly income, education, estimated measure of risk preferences, religion, whether participant is married, whether participant is monogamous, a dummy for villages in the South, a dummy for participants in treated villages who do not own PFR land. Symbols * * *, **, and * indicate significance at the 1%, 5% and 10% level, respectively.

is regressed on the dummy *treated* and the set of controls described above. The coefficient of the treatment dummy is not statistically different from zero. In Model 2, where the treatment dummy is interacted with the variable *road_distance*, 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. As it was the case for contributions to the public good, the interaction term *treated*road_distance* is negative and statistically significant. In Models 3 and 4, I again split the sample and consider separately participants living in villages closer or more distant than the median distance from paved roads in the sample. For participants coming from villages more distant from paved roads, the coefficient of the variable *treated* in Model 3 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 4 shows that experiencing the PFR produces an increase in the average trustor’s transfers for participants in villages with comparatively high market exposure.

Some of the controls used in the regression analysis just presented could be caused by the treatment. As a robustness check, I replicate the model specifications presented in Tables 1 and 2 in the main text by excluding those controls which could have been influenced by the reform (income, education, risk preferences, marital status, whether polygamous). Tables A8 and A9 in Appendix A report the results. The qualitative results remain unaffected and point estimates are very similar, thus mitigating concerns regarding the effects of possibly selecting

“bad controls”. Finally, in Tables A3 and A4 in Appendix A, I show that the results reported in the main text are robust to correcting for multiple hypothesis testing.

Taken together, the evidence suggests that in Benin the formalization of property rights over land brought about by the PFR reform has opposite effects on cooperation and trust depending on the cost of accessing the new formal institutions and legal innovations for the communities involved. Specifically, the results suggest that the reform triggers an increase in prosocial choices in the experiments for those participants with relatively easy access to roads – a proxy indicating the comparatively low cost of accessing institutions – while for those living in villages more distant to infrastructure connections, experiencing the formalization of land rights reduces prosocial behavior.

5 Mechanisms

I now turn to examining potential causal channels for the findings of increased cooperation and trust displayed by participants living in villages close to paved roads and an opposite effect for those living in communities less connected to infrastructures as a consequence of experiencing the PFR reform.

Conflicts

I verify whether the observed changes in cooperation and trust could be explained by a change of the land-related conflict rate 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 cleared existing disputes due to unclear land boundaries and their associated land-user rights. Had this happened, the observed changes in cooperation and trust 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 land-related conflicts experienced by the participants. In rural Benin most disputes are solved without resorting to formal courts and no reliable administrative data source on land-related conflicts is available. Here I rely on self-reported data on conflict episodes that happened in the seven years before the experiment. However, this data might suffer of recalling bias. Therefore, the results of this subsection are valid on the assumption that participants correctly reported the disputes eventually experienced or that, if misreporting happened, that the recalling bias was similar between treated and control villages. 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 or low distance from roads. These results are confirmed by regression analysis reported in Tables A10 and A11 in Appendix A, in which I re-estimated

the main model specifications additionally controlling for the land-related conflicts experienced by participants in the previous seven years. The results remain qualitatively the same.

Changes in Altruistic Preferences

I verify whether the observed changes in trust and cooperation reflect a general modification of social preferences. I test for changes in altruism by letting participants play a standard dictator game framed as a donation.¹⁶ Figures A3a and A3b in Appendix A plot the average number of coins donated across treatments in villages next to or more distant to paved roads, respectively.

The distribution and average number of coins donated are not statistically different between the treatment and control groups in both samples (t-test two-sided, $p > 10\%$; notice that this result is not driven by participants adopting a 50-50 coins split as a rule of thumb, since donating half of the endowment is not the modal choice). The results are confirmed by regression analysis reported in Table A12 in Appendix A. This evidence suggests that the changes in behavior observed in the strategic settings of the games used to elicit cooperation and trust – arguably more similar to market-alike interactions compared to a dictator game – do not reflect a general modification of social preferences.

Income and Access to Credit

A potential mechanism underlying the observed changes in behavior is that the land-rights reform increases the value of the land parcels own by (some of the) participants in treated villages and makes possible for them to use titled land as collateral, thus increasing access to credit. In Section 3.1, I showed that average income levels are similar for treated and control participants, both when considering the whole sample or when comparing subsamples according to the villages' distance to paved roads. Moreover, all the regression models presented in the main text control for income levels, thus further suggesting that income levels do not play a role in determining the observed variations in prosocial choices.

A similar conclusion can be drawn when considering participation in the credit market. Previous research in rural areas of low-income developing countries suggests that land titling tends to have small effects on improving credit access (Besley and Ghatak, 2010, Deininger and Feder, 2009). I check whether the Beninese PFR increased 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 (please

¹⁶Specifically, each participant received 10 coins worth 100 XOF each. The participant was then asked to allocate as many of these coins as he wanted to his own endowment or donate them to a Beninese orphanage located outside the village. We donated the amount collected in the experiment to an orphanage in Cotonou. 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.

notice that the caveat concerning possible recall bias discussed in the previous section applies as well here). 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, neither when looking at the whole sample nor when we split it in sub-samples according to the villages distance to paved roads (Chi-square test, $p > 10\%$ in all cases). I also estimate the likelihood that a participant reports to have used financial instruments in the previous seven years. Results from Logit specifications reported in Table A13 show that experiencing the reform does not determine significant differences across treatment groups, neither on average nor when we focus on sub-samples of villages with small or large distance to paved roads.

Individual and Social Approval of Formal Institutions

I then check whether the reform affected participants' individual perception of the legitimacy of formal institutions and the social approval for using them. The different frequency of using PFR documents for villagers characterized by unequal costs for accessing government services reflects heterogeneous normative beliefs regarding the legitimacy of state courts as conflict resolution mechanism. The 53% of villagers living in communities closer to paved roads than the median sample respond that the formal judiciary (instead of customary authorities) should resolve land-related disputes, against only 15% of the remaining respondents (the difference is strongly statistically significant, χ^2 test $p < 1\%$). Moreover, while 80% of villagers in the former sample think that the decisions of formal state courts overrule decisions of customary courts, only 66% of those respondents living more distant from paved roads than the sample median report so.

I then move to the social legitimacy of the new institutions and investigate how the PFR affected the social norms related to use the formal judiciary in order to overrule an unfavorable decision of the customary authority. To do so, the sample of participants to the PGG and TG took part in an incentivized coordination game similar to Krupka and Weber (2013). The participants were presented with the action of a hypothetical agent and 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". Those 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. 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 customary authority and to abstain from challenging the unfavorable decision through the formal judiciary.¹⁷

Models 1-3 of Table 3 display the results of an Ordinal Logistic regression relative to the

¹⁷The complete text of the instructions, including the description of the situation that was read to participants, are reported in Appendix B.

Table 3: Social support for customary conflict resolution authorities and banning the land market

Dep. Var.	Legitimacy Customary Authorities			Banning Land Market		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:	Whole	High-Dist	Low-Dist	Whole	High-Dist	Low-Dist
Treated	-0.524	1.218***	-0.167	-0.161	0.782***	0.122
	(0.354)	(0.271)	(0.345)	(0.256)	(0.266)	(0.171)
road_distance	0.001			0.007		
	(0.022)			(0.016)		
Treated×	0.118***			0.068***		
road_distance	(0.039)			(0.025)		
N.obs.	515	262	253	515	262	253

Notes: Dependent variable: Models 1-3 perceived legitimacy of traditional authorities for conflict resolutions within the community (assuming values 1 to 4); Models 4-6 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. Models 1 and 4 include the whole sample. Models 2 and 5 consider the restricted sample of villages with distance from paved roads higher than the median (“High-Dist”). Models 3 and 6 consider the restricted sample of trustor transfers of more than one coin for villages with distance from paved roads smaller than the median (“Low-Dist”). 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, a dummy for participants in treated villages who do not own PFR land. Symbols ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

participants’ rating of the situation. The dependent variable—the perceived appropriateness of legitimizing the judgment of the customary authority—ranges 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 considers the whole sample of participants and interacts the treatment dummy with the village distance from paved roads. While the coefficient of the treatment dummy is not significant, suggesting no effects on villagers with direct access to paved roads, the coefficient of the interaction term is positive and significant at the 1% level. The result shows that, for villagers experiencing the PFR, the legitimacy of the customary authority increases with distance to paved roads. This result is confirmed by Models 2 and 3, in which I consider the restricted sample of participants living more distant and closer to paved roads than the sample median, respectively. The coefficient of the treatment dummy is positive and significant at the 1% level for villagers distant from paved roads in Model 2, showing an increased legitimization of the customary authorities, while small and insignificant in Model 3.

But why in villages with more difficult access to government services and state courts the reform has triggered these negative reactions against the formal judiciary and increased support for the customary authorities? A possible explanation is suggested by data from the follow-up survey relative to the strategic use of the formal judiciary. Over 84% of respondents think that wealthy people who can afford the cost to access the formal system has the possibility to engage

in “institutional shopping” by using the formal judiciary as an appeal court in case of a dispute solved by the customary authorities which resulted in an unfavorable verdict.¹⁸ Moreover, 80% of villagers in this category consider formal courts and state judges more corrupt than the customary authorities (this percentage drops to 54% for respondents close to paved roads). This evidence suggest that the formal mechanism of dispute resolution introduced with PFR was perceived as favoring wealthier individuals and increasing disparities in access to justice by villagers in isolated communities who cannot afford the costs to access to the formal judiciary.

A finding that further supports this interpretation comes from an incentivized coordination game to elicit social norms concerning the trade of land that was performed by participants in the PGG and TG experiments. Participants were described a situation in which a villager refuses an advantageous offer to sell a land parcel because “land belongs to the community and cannot be sold”.¹⁹ As before, participants were requested to rate the social appropriateness of this action and rewarded if their answer matched the modal response. Results from an Ordinal Logit are reported in Models 4-6 of Table 3. While for in villages close to paved roads experiencing the PFR produces no effect, in communities distant from paved roads the reform significantly and substantially increases social support for banning the land market.

6 Discussion and Conclusions

This paper studies the impact of a major reform of property rights over land on trust and cooperation. The reform was implemented in rural Benin and transformed collective and informal land rights in individual and formally registered property rights that can be defended in court, sold, or used as collateral. The identification strategy makes use of the peculiar implementation process of the reform, the first case of large-scale land tenure reform implemented as randomized control trial. From a sample of hundreds of villages, half were selected by means of a 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. Experimental measures of willingness to cooperate and trust are assessed using a public goods game and a trust game.

I find that the reform has heterogeneous effects on prosociality and that the direction of these effects is associated to the accessibility and costs of the institutions introduced by the reform, as proxied by the distance from paved roads characterizing the community where the participants live. The reform significantly increases prosocial behavior in villages with direct access to paved roads in the sample. Conversely, the gains in prosociality fall progressively, and

¹⁸The question asked was: “Do you think wealthy/powerful people can use the formal court if they don’t like the decision of the traditional/customary court?”.

¹⁹The complete instructions of the coordination game and the description of the situation that was read to participants are reported in Appendix B.

even reverse, as a village distance from roads increases. As a result, the reform significantly reduces cooperation and trust choices of participants in villages characterized by distance from paved roads larger than the sample median.

An investigation of possible mechanisms excludes that changes in the frequency of land-related conflicts, income, access to credit, or changes in altruistic preferences are driving the observed behavior. Results from a follow-up survey and two coordination games designed to elicit social norms related to using the formal judiciary and trading land – the two key features of the PFR – show that in villages distant to road infrastructures the reform significantly increased villagers’ ostracism against these practices. The analysis of further survey responses suggests that, in these more isolated communities, villagers perceived the legal innovations introduced by PFR as favoring those wealthy individuals who can afford the cost of the formal judiciary and engage in institutional shopping.

These findings resonate with previous empirical studies on the cultural effects of ownership in urban settings and industrialized societies (DiPasquale and Glaeser, 1999, Di Tella et al., 2007, Field, 2007). At the same time, they reconcile the contrasting evidence reporting little impact or even negative results produced by case studies of land tenure reforms in rural sub-Saharan Africa and other low-income developing countries (Goulding and Friedman, 2018, Lund et al., 2006, Platteau, 2000). The results shed light on the key role played by the specific institutional and legal framework in which a land rights reform takes place, emphasizing that institutional constraints might offset potential social and cultural gains of formalization.

It is worth emphasizing that the observed effects of land titling do not apply to a random sample of Beninese villages, since those included in the RCT pool have volunteered to receive the intervention. For instance, as shown by Omondi (2019), if compared to the remaining 1487 Beninese villages that were reached by the 2006-2007 awareness campaign run by the Beninese government regarding the possibility to apply for the PFR lottery, the 575 villages that actually applied and were included in the RCT pool are on average more ethnically diverse, more likely to be located in rural areas, and reported a larger frequency of land-related conflicts. This feature of the PFR is not a problem for the internal validity of the findings reported here, since the randomization took place within the group of self-selected villages. This conditionality tells us is that the introduction of formal institutions increases trust and cooperation in situations where the population demands institutional change. Future research should study what would be the effects of a top-down institutional reform on prosociality if the intervention takes place in the absence of local demand for formalization.

In this sense, these findings are also related to the literature discussing the costs and benefit of a selective and voluntary vis-à-vis universal and systematic approaches to property rights formalization (Arruñada and Garoupa, 2005, Deininger and Feder, 2009). While I show that the results do not depend on participants’ income or access to credit, it should be emphasized that controlling for these factors in the analysis might not capture variations in wealth and

land value determined by the reform. It is possible that the titling effort increased the value of land parcels comparatively more in villages close to roads that have higher baseline land value, enjoy better access to markets, and so benefited more from upholding titling in formal courts. In turn, this might have contributed to the observed differential effects on prosociality according to road distance. For instance, this may happen because in villages with better access to formal institutions the wealth gains were sufficiently large to compensate for the discontent provoked by the institutional innovations and so to prompt trust and cooperation, while these “baseline” negative effects on prosociality remain uncompensated in villages where the reform did not produce substantial wealth effects. Future studies that have access to data on how titling affects land value as a function of access to institutions and government services will be able to further clarify how these mechanisms interact.

A limitation of the paper is that it investigates the effects of the reform on trust and cooperation displayed by individuals involved in interactions with their own village members. While prosociality within small-scale rural communities might be an important factor for supporting activities and organizations at an early stage of economic development, the flourishing of a mature market economy requires the establishment of informal norms capable of complementing and supporting formal institutions in sustaining impersonal trade (Arruñada, 2012, Buchan et al., 2009, North, 1991). Future research will have to investigate the effects that property rights institutions have on trust and cooperation in interactions involving unknown strangers.

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839 Appendix A Supplementary Material and Data Analysis

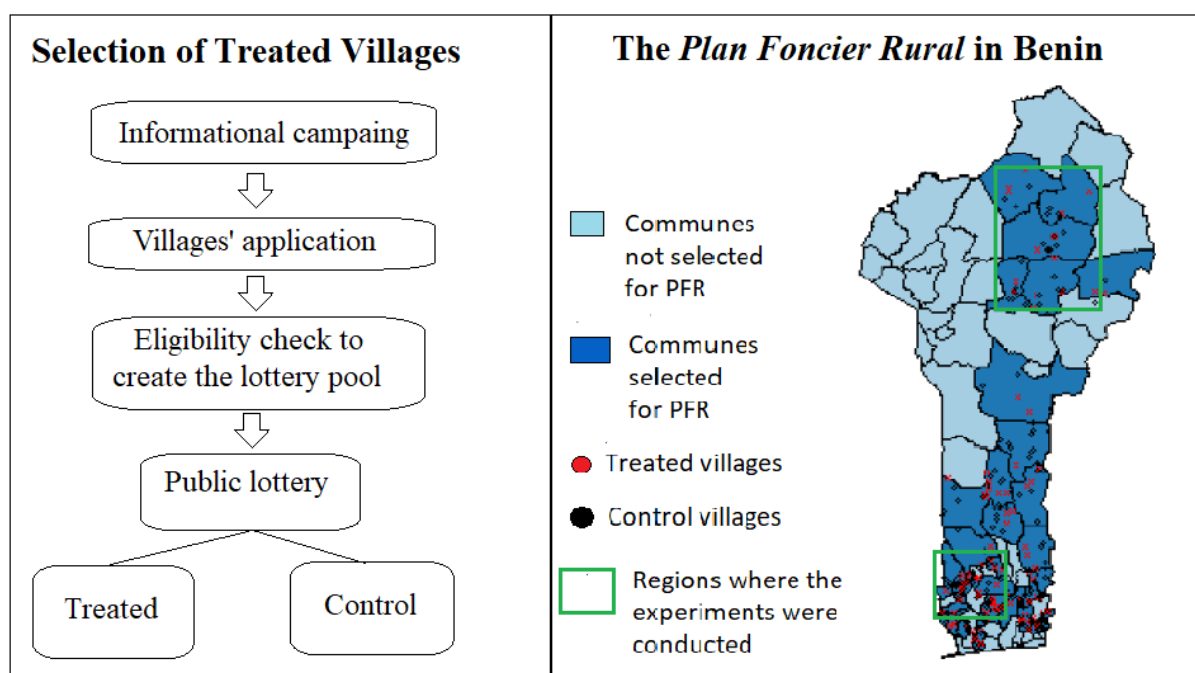


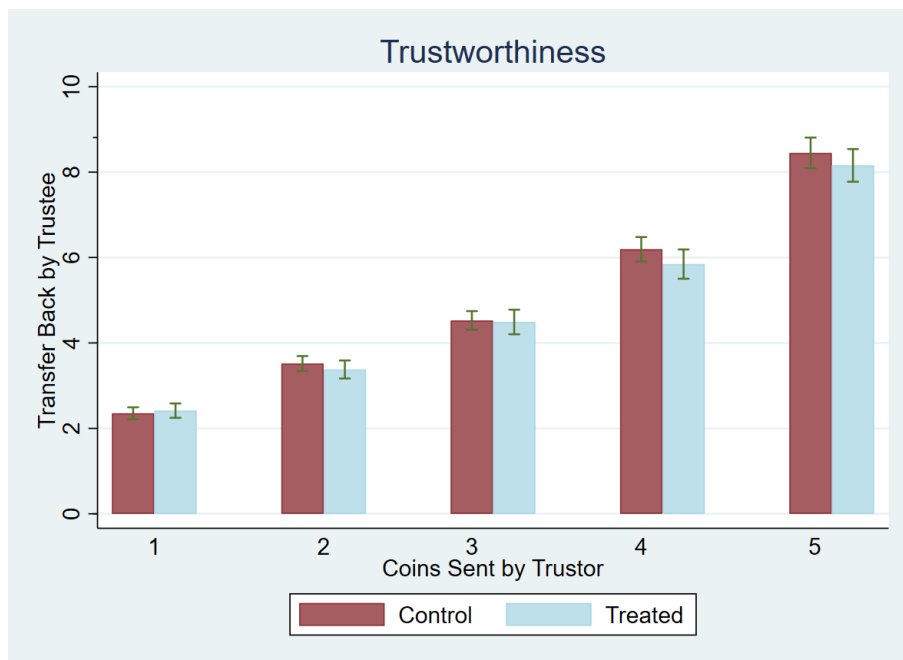
Figure A1: *Left Panel:* The mechanism for selecting treated villages. *Right Panel:* Distribution of treated and control villages after the RCT implementation. The green square identifies the provinces where the data collection took place. The communes that were excluded from participating in the PFR lottery (in light blue) were those where NGOs and other organizations were engaging in other programs of land governance at the time of the PFR design. The other criteria for eligibility were: whether the village is located in a rural area, 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.

Table A1: Balance

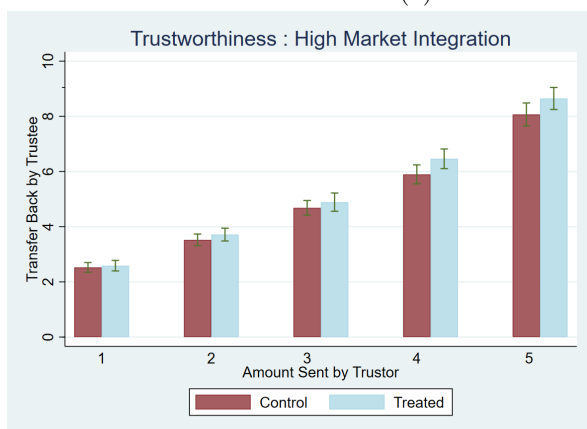
Sample:	Whole			High-dist			Low-dist		
	Treated	Control	diff(p)	Treated	Control	p	Treated	Control	p
male	.59	.57	.64	.49	.56	.24	.65	.57	.19
age	38.8	37.0	.09	38.4	35.3	.03	39.1	39.6	.75
age-village(%)	85.0	83.2	.24	87.9	83.9	.22	84.5	82.1	.50
literacy	.39	.40	.84	.32	.36	.46	.44	.46	.76
edu-years	1.22	1.17	.76	.90	.98	.67	1.45	1.48	.92
alphabet	.27	.20	.16	.19	.20	.90	.33	.22	.18
christian	.29	.29	.94	.22	.21	.83	.35	.43	.22
animist	.35	.34	.76	.28	.22	.25	.39	.52	.06
muslim	.36	.37	.71	.56	.50	.24	.25	.05	.01
income(k)	11.3	11.2	.97	10.2	8.65	.34	12.1	15.5	.44
work-7d	.92	.92	.97	.96	.99	.27	.88	.81	.09
money-sat	2.18	2.26	.29	2.21	2.32	.37	2.15	2.18	.84
food-sat	1.71	1.77	.45	1.72	1.69	.78	1.70	1.90	.07
health-sat	2.14	2.14	.99	2.37	2.22	.21	1.97	2.01	.72
household-nr	13.4	13.6	.89	16.2	16.0	.87	11.4	9.6	.06
eth-adj	.35	.32	.55	.01	.10	.01	.70	.60	.13
eth-bariba	.43	.48	.27	.72	.66	.35	.22	.16	.33
eth-other	.21	.19	.55	.28	.24	.47	.18	.13	.35
married	.88	.91	.42	.91	.91	.90	.86	.90	.36
polygamous	.39	.38	.69	.53	.42	.09	.30	.30	.97
politic-part	.60	.59	.79	.53	.56	.60	.65	.63	.76
politic-freq	1.72	1.75	.86	1.8	2.0	.47	1.62	1.60	.93

Notes: The p-value columns report results of a two-sided t test for continuous variables and of a Chi-squared test for binary variables. The dummy “alphabet” is equal to one if the subject is taking part to an on-going alphabetization program; “age-village(%)” reports the share of a subject’s life spent in the village where the data collection takes place; “income(k)” reports weekly household’s income in thousands; “work-7d” is a dummy equal to 1 if the subject worked in the last 7 days; “money-sat” takes values {0;3} and indicates increasing self-reporting levels of satisfaction for the household’s education; “food-sat” takes values {0;3} and indicates increasing self-reporting levels of satisfaction for the household’s amount of food available; “health-sat” takes values {0;3} and indicates increasing self-reporting levels of satisfaction for the household’s health conditions; “household-nr” report the number of members in the household; “politic-part” is a dummy equal to 1 if the subject reported to be involved in political decisions within the community; “politic-freq” is the number of community meetings attended in the last month.

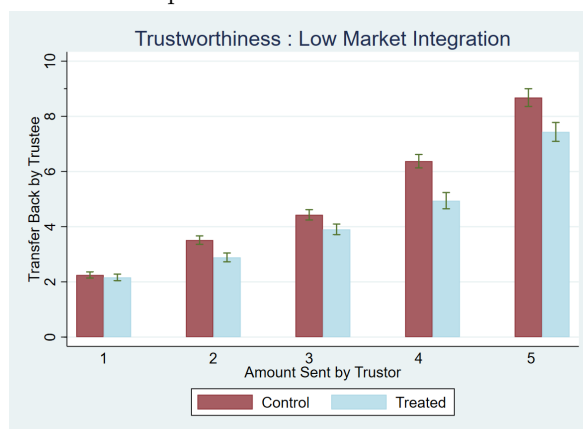
840 Analysis of Trustworthiness Choices



(a) Trustworthiness – whole sample



(b) Trustworthiness in high market integration



(c) Trustworthiness in low market integration

Figure A2: Trustee transfer back to trustors

841 In the second stage of the trust game I collected trustees' choices regarding how many coins
 842 to send back to the trustor. However, due to logistical constraints, the trustees' decisions were
 843 assessed using the strategy method, with the experimenter asking each trustee to state her
 844 decision for each of the six possible transfer levels received from the trustor. Thus, unlike trust
 845 and cooperation choices, trustworthiness decisions were elicited using the strategy methods and
 846 were not blind to the experimenter on site. I start the analysis by looking at the average
 847 number of coins returned by the trustee in the TG as a measure of trustworthiness. Each

participant decides how many coins to send back for each possible level of the trustor's initial transfer. Figure A2a 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 A2b and A2c plot the average trustee transfer as a function of the initial amount received by the trustor for participants in villages with distance from paved roads smaller and larger than the median, respectively. Trustees on average transfer back more coins in treated villages with distance from paved roads lower than the sample median. However, a Hotelling T-squared test shows no statistically significant difference between the two samples. Conversely, for villages with high distance from paved road, 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, $p\text{-value} < 1\%$).

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.²⁰ As before, I regress the trustee number of coins sent back to the dummies *treated*, *road_distance*, their interaction, and the set of controls specified above.

Table A2 displays the results. Model 1 includes the whole sample of observations. The coefficient *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 distance from paved roads increases. 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*road_distance* becomes significant at the conventional level.

Models 3 and 4 repeat the analysis but focus only on the participants in villages whose distance from paved roads is larger than the median. The coefficient of the dummy *treated* is negative and strongly statistically significant in both models. Point estimates suggest that, for participants in these villages, 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 villages closer to paved roads. The coefficient of the

²⁰I 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.

883 dummy *treated* is positive albeit not statistically different from zero in both cases, confirming
884 the finding that for these subjects on average the reform does not have significant effects.

Table A2: Coins sent back by the Trustee

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
	Whole Sample		High-Dist		Low-Dist	
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)
road_distance	-0.003	0.002				
	(0.024)	(0.027)				
Treated×	-0.058*	-0.077**				
road_distance	(0.031)	(0.035)				
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.

Robustness Checks and Analysis of Possible Channels

Table A3: Contribution to the public good – Replicating Table 1 Correcting for Multiple Hypothesis Testing, Sharpened False Discovery Rate Q-values (Benjamini et al., 2006)

Sample:	Whole	High-Dist	Low-Dist
Model:	(2)	(3)	(4)
<i>treated</i>			
p-value	.039	.038	.009
q-value	.041	.02	.019
<i>treated</i> × <i>road_distance</i>			
p-value	.036		
q-value	.041		

Notes: Sharpened False Discovery Rate (FDR) q-values calculated following the procedure specified by Anderson (2008). The FDR is the expected proportion of rejections that are type I errors (false rejections). Note that sharpened FDR q-values can be less than unadjusted p-values when many hypotheses are rejected.

Table A4: Coins sent by the Trustor – Replicating Table 2 Correcting for Multiple Hypothesis Testing, Sharpened False Discovery Rate Q-values (Benjamini et al., 2006)

Sample:	Whole	High-Dist	Low-Dist
Model:	(2)	(3)	(4)
<i>treated</i>			
p-value	.043	.093	.046
q-value	.045	.102	.066
<i>treated</i> × <i>road_distance</i>			
p-value	.022		
q-value	.045		

Notes: Sharpened False Discovery Rate (FDR) q-values calculated following the procedure specified by Anderson (2008). The FDR is the expected proportion of rejections that are type I errors (false rejections). Note that sharpened FDR q-values can be less than unadjusted p-values when many hypotheses are rejected.

Table A5: Likelihood to migrate

	Model 1	Model 2	Model 3	Model 4
	Whole Sample		High-Dist	Low-Dist
Treated	-0.182	0.324	-0.762	-0.351
	(0.562)	(0.877)	(1.012)	(0.679)
road_distance		0.057		
		(0.046)		
Treated \times road_distance		-0.035		
		(0.060)		
Constant	1.862	0.925	3.423	2.655
	(1.350)	(1.422)	(2.737)	(2.322)
N.obs.	515	515	262	253

Notes: Dependent variable: Dummy equal to one if the participant migrated to a different village in the previous seven years. Logistic regression. Standard errors robust for clustering at the village level. Models 1 and 2 include the whole sample, Models 3 considers the restricted sample of villages with distance to paved roads higher than the median. Model 4 considers the restricted sample of villages with distance to paved roads lower than the median. Controls include: age, gender, household weekly income, education, estimated measure of risk preferences, religion, whether participant is married, whether participant is monogamous, a dummy for villages in the South, a dummy for participants in treated villages who do not own PFR land. Symbols ***, **, and * indicate significance at the 1%, 5% and 10% level, respectively.

Table A6: Contribution to the public good – controlling for years spent in village

	Model 1	Model 2	Model 3	Model 4
	Whole Sample		High-Dist	Low-Dist
Treated	0.663	1.959**	-0.848**	2.536***
	(0.562)	(0.946)	(0.409)	(0.966)
road_distance		0.050		
		(0.042)		
Treated \times road_distance		-0.143**		
		(0.068)		
yearsinvillage	-0.011	-0.003	-0.001	-0.001
	(0.016)	(0.014)	(0.018)	(0.018)
dmoving	-0.567	-0.450	0.738	-1.952**
	(0.700)	(0.759)	(0.621)	(0.862)
Constant	4.453***	4.210***	4.785***	3.390**
	(0.982)	(1.333)	(1.239)	(1.499)
N.obs.	515	515	262	253

Notes: coins contributed to the public good. Censored Tobit regressions. Standard errors robust for clustering at the village level. Models 1 and 2 include the whole sample, Models 3 considers the restricted sample of villages with distance to paved roads higher than the median. Model 4 considers the restricted sample of villages with distance to paved roads lower than the median. Controls include: age, gender, household weekly income, education, estimated measure of risk preferences, religion, whether participant is married, whether participant is monogamous, a dummy for villages in the South, a dummy for participants in treated villages who do not own PFR land. Symbols ***, **, and * indicate significance at the 1%, 5% and 10% level, respectively.

Table A7: Coins sent by the Trustor – controlling for years spent in village

	Model 1	Model 2	Model 3	Model 4
	Whole Sample		High-Dist	Low-Dist
Treated	0.409	0.834**	-0.294*	0.965**
	(0.260)	(0.416)	(0.167)	(0.441)
road_distance		-0.002		
		(0.017)		
Treated \times road_distance		-0.057**		
		(0.026)		
	(0.011)	(0.011)	(0.008)	(0.019)
yearsinvillage	-0.021	-0.016	-0.007	-0.031
	(0.013)	(0.013)	(0.008)	(0.020)
dmoving	-0.161	-0.040	0.608	-1.208*
	(0.463)	(0.454)	(0.403)	(0.636)
Constant	2.002***	2.375***	2.096***	1.396
	(0.598)	(0.640)	(0.367)	(1.059)
N.obs.	261	261	130	131

Notes: coins sent by the Trustor. Censored Tobit regressions. Standard errors robust for clustering at the village level. Models 1 and 2 include the whole sample, Models 3 considers the restricted sample of villages with distance to paved roads higher than the median. Model 4 considers the restricted sample of villages with distance to paved roads lower than the median. Controls include: age, gender, household weekly income, education, estimated measure of risk preferences, religion, whether participant is married, whether participant is monogamous, a dummy for villages in the South, a dummy for participants in treated villages who do not own PFR land. Symbols ***, **, and * indicate significance at the 1%, 5% and 10% level, respectively.

Table A8: Contribution to the public good - exclude controls potentially endogenous to treatment

	Model 1	Model 2	Model 3	Model 4
	Whole Sample		High-Dist	Low-Dist
treated	0.585	1.890**	-0.898**	2.545***
	(0.565)	(0.947)	(0.397)	(0.950)
road_distance		0.048		
		(0.043)		
Treated \times road_distance		-0.146**		
		(0.067)		
Constant	4.967***	4.813***	5.589***	3.708***
	(0.887)	(1.085)	(0.974)	(1.161)
N.obs.	515	515	262	253

Notes: coins contributed to the public good. Censored Tobit regression. Standard errors robust for clustering at the village level. Models 1 and 2 include the whole sample, Models 3 considers the restricted sample of villages with distance to paved roads higher than the median (“High-Dist”). Model 4 considers the restricted sample of villages with distance to paved roads lower than the median (“Low-Dist”). Controls included: age, gender, religion, a dummy for villages in the South. Symbols ***, **, and * indicate significance at the 1%, 5% and 10% level, respectively.

Table A9: Coins sent by the trustor - excluding controls potentially endogenous to treatment

	Model 1	Model 2	Model 3	Model 4
	Whole Sample		High-Dist	Low-Dist
treated	0.418 (0.278)	0.879** (0.439)	-0.288* (0.170)	1.034** (0.503)
road_distance		-0.000 (0.017)		
Treated \times road_distance		-0.062** (0.027)		
Constant	2.382*** (0.475)	2.719*** (0.583)	2.464*** (0.256)	1.791* (0.947)
N.obs.	515	515	262	253

Notes: coins sent by the trustor. Censored Tobit regression. Standard errors robust for clustering at the village level. Models 1 and 2 include the whole sample, Models 3 considers the restricted sample of villages with distance to paved roads higher than the median (“High-Dist”). Model 4 considers the restricted sample of villages with distance to paved roads lower than the median (“Low-Dist”). Controls included: age, gender, religion, a dummy for villages in the South. Symbols ***, **, and * indicate significance at the 1%, 5% and 10% level, respectively.

Table A10: Contribution to the public good – controlling land-related conflicts experienced

	Model 1	Model 2	Model 3	Model 4
	Whole Sample		High-Dist	Low-Dist
Treated	0.627 (0.567)	1.930** (0.948)	-0.906** (0.420)	2.530*** (0.970)
road_distance		0.050 (0.041)		
Treated \times road_distance		-0.142** (0.068)		
conflict	-0.324 (0.347)	-0.273 (0.339)	-0.423 (0.262)	-0.275 (0.521)
Constant	4.551*** (1.000)	4.240*** (1.347)	4.965*** (1.267)	3.315** (1.507)
N.obs.	515	515	262	253

Notes: coins contributed to the public good. Censored Tobit regressions. Standard errors robust for clustering at the village level. Models 1 and 2 include the whole sample, Models 3 considers the restricted sample of villages with distance to paved roads higher than the median. Model 4 considers the restricted sample of villages with distance to paved roads lower than the median. Controls include: age, gender, household weekly income, education, estimated measure of risk preferences, religion, whether participant is married, whether participant is monogamous, a dummy for villages in the South, a dummy for participants in treated villages who do not own PFR land. Symbols ***, **, and * indicate significance at the 1%, 5% and 10% level, respectively.

Table A11: Coins sent by the Trustor – controlling land-related conflicts experienced

	Model 1	Model 2	Model 3	Model 4
	Whole Sample		High-Dist	Low-Dist
Treated	0.413	0.841**	-0.297	0.914**
	(0.263)	(0.402)	(0.188)	(0.431)
road_distance		-0.004		
		(0.017)		
Treated \times road_distance		-0.058**		
		(0.025)		
conflict	0.302	0.305	0.085	0.437
	(0.216)	(0.217)	(0.198)	(0.310)
Constant	2.115***	2.515***	2.203***	1.691
	(0.576)	(0.621)	(0.349)	(1.049)
N.obs.	261	261	130	131

Notes: coins sent by the Trustor. Censored Tobit regressions. Standard errors robust for clustering at the village level. Models 1 and 2 include the whole sample, Models 3 considers the restricted sample of villages with distance to paved roads higher than the median. Model 4 considers the restricted sample of villages with distance to paved roads lower than the median. Controls include: age, gender, household weekly income, education, estimated measure of risk preferences, religion, whether participant is married, whether participant is monogamous, a dummy for villages in the South, a dummy for participants in treated villages who do not own PFR land. Symbols ***, **, and * indicate significance at the 1%, 5% and 10% level, respectively.



(a) Altruism in villages closer to paved roads than the sample median



(b) Altruism in villages more distant to paved roads than the sample median

Figure A3: Coins donated in a standard Dictator game

Table A12: Coins donated in a standard Dictator game

	Model 1	Model 2	Model 3	Model 4
	Whole Sample		High-Dist	Low-Dist
Treated	-0.143	0.021	-0.258	-0.099
	(0.264)	(0.396)	(0.329)	(0.392)
road_distance		-0.012		
		(0.029)		
Treated \times road_distance		-0.026		
		(0.033)		
Constant	3.805***	4.155***	4.525***	3.850***
	(0.601)	(0.594)	(0.681)	(0.726)
N.obs.	515	515	262	253

Notes: Dependent variable: coins donated in a standard Dictator game. Ordinal Least Square regressions. Standard errors robust for clustering at the village level. Models 1 and 2 include the whole sample, Models 3 considers the restricted sample of villages with distance to paved roads higher than the median. Model 4 considers the restricted sample of villages with distance to paved roads lower than the median. Controls include: age, gender, household weekly income, education, estimated measure of risk preferences, religion, whether participant is married, whether participant is monogamous, a dummy for villages in the South, a dummy for participants in treated villages who do not own PFR land. Symbols ***, **, and * indicate significance at the 1%, 5% and 10% level, respectively.

Table A13: Access to credit

	Model 1	Model 2	Model 3	Model 4
	Whole Sample		High-Dist	Low-Dist
Treated	-0.273	-0.325	-0.409	-0.165
	(0.246)	(0.352)	(0.398)	(0.288)
road_distance		-0.007		
		(0.026)		
Treated \times road_distance		0.004		
		(0.030)		
Constant	-1.586***	-1.476**	-2.309***	-0.890
	(0.519)	(0.691)	(0.770)	(0.827)
N.obs.	515	515	262	253

Notes: Dependent variable: dummy equal to one if the respondent have had use financial instruments in the previous seven years. Logistic regressions. Standard errors robust for clustering at the village level. Models 1 and 2 include the whole sample, Models 3 considers the restricted sample of villages with distance to paved roads higher than the median (low market integration condition). Model 4 considers the restricted sample of villages with distance to paved roads higher than the median (low market integration condition). Controls include: age, gender, income, education, estimated measure of risk preferences, religion, whether participant is married, whether participant is monogamous, a dummy for villages in the South. Symbols ***, **, and * indicate significance at the 1%, 5% and 10% level, 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 PARTICIPANTS!)

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 $30 + (0.5 \cdot 30) = 45$ 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 $14 + (10 + 0 + 4) \cdot 0.5 = 21$ 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.

957 *Example:* Imagine Participant 1 sends 3 coins to the other participant. As mentioned before,
 958 we will triple these coins, which means that Participant 2 gets 9 coins. Therefore, Participant
 959 2 has now 14 coins: 9 coins received plus the 5 coins from the start. Then Participant 2
 960 decides how many of his/her 14 coins s/he sends back. Imagine Participant 2 sends back 0
 961 coins. Then, Participant 2 will still have 14 coins and Participant 1 will have $5-3 = 2$ coins.
 962 Imagine Participant 2 sends back 5 coins. Then Participant 2 will have $14-5$ coins = 9 coins
 963 and Participant 1 will have $5-3+5$ coins = 7 coins.

964 Is this clear? Shall I repeat?

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

967 I will now present you two options. One option gives you a certain outcome: either you
 968 gain for sure or lose for sure some coins. The other option consists of a lottery. The lottery
 969 is the following: in this bag, there are 3 orange balls and 7 white balls. We withdraw a ball.
 970 If orange, you gain/lose 5 coins; if white, you gain/lose zero. You have to decide if you prefer
 971 to gain/lose the coins for sure or to play the lottery. You have to make 6 decisions, where the
 972 number of coins that you gain/lose if you choose the “certain” option varies. Only one out of
 973 the 6 decisions will be paid. Once you have told us whether you prefer the certain option or
 974 the lottery for the 6 cases, we roll this 6-faced die. The number resulting tells which of the 6
 975 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

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

978 We will describe to you a series of situations. In each situation, a person must make a
 979 decision. You will be asked whether taking the action that the person chose in the situation
 980 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.*