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Property rights and prosocial behavior: Evidence from a land tenure reform implemented as randomized control-trial

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

Marco Fabbri\*

4 **Abstract**

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

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18 **JEL-Classification: K1; Z1**

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

## 21 **1 Introduction**

22 Research has shown that trust and willingness to cooperate are important determinants  
23 of economic development (Fernandez and Fogli, 2009, Tabellini, 2010, Zak and Knack, 2001).  
24 Therefore, understanding which factors influence the formation and development of these proso-  
25 cial cultural traits is a key goal for a society’s prosperity.<sup>1</sup> This article contributes to a recent  
26 wave of studies which attempt to isolate the causal effects of different land rights institutions  
27 on the values and beliefs of individuals. Within the literature studying the social impact of  
28 formalized property rights, contrasting hypotheses have been advanced.

29 One hypothesis states that stronger property rights reinforce prosociality and crowd-in civic  
30 goods (Bisin and Verdier, 2001, Tabellini, 2008). Scholars have shown the enforcement of the  
31 rule of law in a country is associated with a higher level of generalized morality and that  
32 property titles favors social capital accumulation (DiPasquale and Glaeser, 1999, Galiani and  
33 Schargrodsky, 2010, Tabellini, 2010). In contrast with this view, other scholars observe that  
34 the introduction of formal ownership rights may erode moral values and increase social conflicts  
35 (Hoffman et al., 1996, Ross and Ward, 1995). Evidence from land rights formalization programs  
36 show that replacing collective and informal land-rights institutions could hinder prosociality by  
37 increasing wealth inequalities and eliminating the informal insurance provided by collectively  
38 owned property (Baland and Francois, 2005, Deininger and Feder, 2009, Ostrom and Hess, 2010),  
39 displacing the traditional land-governance institutions trusted by local populations (Arruñada  
40 and Garoupa, 2005, Platteau, 1996), and sparking latent land-related conflicts (André and  
41 Platteau, 1998, Arruñada, 2018).

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

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<sup>1</sup>When 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.

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

61 In 2010-2011, the Beninese government, supported by the Millennium Challenge Corpora-  
62 tion, carried on the implementation of a land-tenure reform called *Plan Foncier Rural* (PFR).  
63 The reform, whose details are reported in the next section, consisted of formalizing customary  
64 tenure over land characterized by collective property and informal possession. This formaliza-  
65 tion was achieved by recording the set of rights each individual holds over land parcels, creating  
66 precise boundaries demarcation, and generating official land maps stored in a public repository.  
67 With the reform, the right-holders acquired formal, legally recognized use rights that can be  
68 traded, used as collateral, and defended in court against contenders. Thus, the reform produced  
69 a substantial shift toward a system of well-defined and individually assigned land rights.

70 The Benin PFR is the first case of land tenure reform to be implemented as a randomized  
71 control-trial on a large scale. The implementation was done through a public lottery involving  
72 hundreds of eligible villages and by the selection of a “treatment group” of villages in which  
73 the reform was implemented and a “control group” where no change of the existing customary  
74 system took place – and that, as of today, maintained the same informal rights. The participant  
75 pool used in the experiments described below comprises villagers from the PFR treatment and  
76 control groups. Because the lottery produced a random selection of villages in which the reform  
77 was implemented, the comparison of cooperation and trust levels between the two groups allows  
78 to identify the causal impact of the land rights formalization on these prosocial traits.

79 I measure the effects of the land-tenure reform on prosociality using an experimental lab-  
80 oratory setting recreated in each of the 32 villages where the fieldwork was conducted. The  
81 lab setting’s controlled environment guarantees the anonymity of the participants and allows  
82 to rule out that the elicitation of participant’s choices in the experimental games are influenced  
83 by social sanctions and other possible strategic considerations. To measure the participants’  
84 cooperation, I employ a linear public goods game (Zelmer, 2003). I collect data on trust via a  
85 standard trust game (Berg et al., 1995).

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

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

101 Results from the experiments show that, for individuals in the sample that belong to com-  
102 munities with direct access to paved roads, experiencing the formalization of land rights sig-  
103 nificantly increased contribution to the common account in the public goods game, as well as  
104 trustors' transfers in the trust game. Point estimates suggest that the land-tenure reform pro-  
105 duced an increase of roughly 40% in contributions to public goods and 30% in trustors' transfers  
106 for those participants. However, these effects progressively vanish and even reverse when the  
107 participants' distance to roads grows. A standard deviation increase in the distance from the  
108 closest paved road produces a 25% drop in the treatment effect of land reform on public goods  
109 contributions, and a 18% decrease of this effect on trustor's transfers, respectively, for subjects  
110 experiencing the reform. This implies that, for participants living in communities characterized  
111 by distance from paved roads larger than the sample median, the reform significantly reduced  
112 the levels of cooperation and trust.

113 After having verified that these results were not driven by confounding factors like migration  
114 or selection of participants across villages, I then explore the possible mechanisms determining  
115 the observed behavior. Data suggest that land-related conflicts do not explain the observed  
116 changes in behavior. Moreover, levels of income or changes in access to the credit market do not  
117 explain the findings – albeit, as I discuss in Section 6, I cannot rule out that the heterogeneous  
118 effects on pro-social behavior in my experiments reflect that land titling produced a larger  
119 increase in land value for villagers with a comparatively better access to markets. Finally,  
120 I present data collected in a follow-up survey indicating that differences in the possibility to  
121 access and use government institutions are associated with villagers' use of PFR registries and  
122 normative beliefs concerning the effectiveness of state-led mechanisms for conflict resolution.  
123 Using an incentivized coordination experiment, I then show that in villages with more difficult  
124 access to institutions the implementation of PFR significantly reinforced the social support for  
125 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.<sup>2</sup>

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

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<sup>2</sup>The 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  
artificial 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).

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

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

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

## 173 **2 Institutional Framework**

### 174 **2.1 Customary Land Rights and the *Plan Foncier Rural* in Benin**

175 In recent years, systems of formal land ownership registration have been introduced in nearly  
176 every African state. Nonetheless, customary land rights still represent the predominant land-  
177 tenure arrangement in most rural areas of the African continent. Customary land rights are  
178 characterized by a complex set of tenure principles and regulatory mechanisms, usually defined  
179 at the village or local level. While a variety of diverse customary arrangements exists, it is  
180 possible to identify a set of common features (Lavigne-Delville, 2000). Customary rights consist  
181 of socially determined land-use rules, where access to land is an integral part of the social struc-  
182 ture and tenure is determined by sociopolitical relationships. Governance and enforcement of  
183 principles characterizing this system are implemented by local customary authorities. The dis-  
184 tribution of land rights is based on the sociopolitical local structure and on family relationships  
185 (Lavigne-Delville, 2006).

186 This system implies that rights held by individuals are the result of a social and political  
187 process of negotiations arbitrated by customary local authorities. This enforcement process has  
188 an inherently procedural nature. Rules governing customary arrangements do not provide a  
189 precise codification of each landholder's rights. Instead they only state procedures by which an  
190 individual obtains access to the land (Chauveau et al., 1998). Therefore, the informal nature  
191 of customary rules might be an obstacle to the establishment of secure and well-defined land-  
192 property rights.



193 Population growth and the consequent increasing pressure on natural resources create serious  
194 concerns for the functioning of informal customary arrangements. Scholars have noticed that  
195 the absence of written documentation regarding land use gave rise to increasing conflicts over  
196 inheritance and disputes over land use (Deininger and Castagnini, 2006). In Benin, the policy  
197 response to problems due to tenure insecurity has been a land-tenure reform known as the  
198 *Plan Foncier Rural*. The reform consists of socio-land surveys at the village level to identify  
199 rights-holders, their rights, and parcel boundaries. Rights and associated rights-holders are  
200 then recorded in public registries, and a process of land demarcation takes place. The process  
201 allows for public objection to the proposed registration of rights and requires that rights-holders  
202 and neighbors publicly sign survey records. In the following paragraphs, I summarize the main  
203 points of the Beninese PFR, focusing on the aspect most relevant for this study. For a thorough  
204 description of the reform characteristics and implementation process, including details on the  
205 selection of the villages to be included in the RCT pool and the evaluation of the effects on  
206 investments see Goldstein et al. (2018) and Omondi (2019).

207 According to the PFR roadmap, following the processes of land demarcation and public  
208 registry recording, each local administration will create a land registry and issue certificates for  
209 each parcel identified.<sup>3</sup> The registered rights that constitute the basis for the land-demarcation  
210 process assign to right-holders the use of rights recognized by courts. Given these characteristics,  
211 the PFR reform in Benin instituted a major modification of the institution of property rights  
212 over land by creating a system akin to formalized ownership.

213 The implementation of the reform, that was subsidized by the Millennium Challenge Cor-  
214 poration, was carried on by the Beninese government in 2010-2011. The peculiar aspect of the  
215 PFR in Benin is that the implementation followed a randomized control-trial process involving  
216 hundreds of rural villages. The objective of the PFR program was to formalize land rights in  
217 300 rural villages across 40 communes.<sup>4</sup> In the preliminary phase of the PFR project, 2062  
218 rural villages were informed of the PFR reform and invited to apply for the lottery. As a second  
219 step, each application received was examined to verify whether the village fit certain eligibility  
220 criteria.<sup>5</sup> This process led to the identification of 575 rural villages that composed the RCT  
221 pool. From this pool, a subsample of 300 villages was selected via public lottery, and in these

---

<sup>3</sup>According to the original formulation as stated in the Rural Land Act 2007-003, the local administration would issue the “Certificat Foncier Rural,” that is, land certificates that required registration to assign land ownership titles (“Titre Foncier”). The new Rural Land Law 2013-01 creates a unique ownership document, the “Certificat de Propriete Foncier,” that reunifies land certificates and ownership titles. 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.

<sup>4</sup>Communes are institutional units similar to counties. Benin has 77 communes. The communes that were excluded from participating in the PFR lottery were those where NGOs and other organizations were engaging in other programs of land governance at the time of the PFR design.

<sup>5</sup>The 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.

222 villages PFR was implemented. The villages that were not selected for the PFR did not re-  
223 ceive any intervention and, as of today, continue to have customary land rights.<sup>6</sup> Figure A1 in  
224 Appendix A shows a map of the communes and villages where the PFR reforms took place.

## 225 **2.2 The Reform and Villagers' Perception of Tenure Security**

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

235 Confirming these findings, in a follow-up survey that I administered in 2020 to 594 indi-  
236 viduals across 43 villages in the same Beninese communes where the data collection relative  
237 to this paper took place, 93% of respondents consider impossible for customary authorities to  
238 expropriate the land from an household who has registered PFR rights<sup>7</sup>, and 89% of the sample  
239 think that PFR rights are secured even if the rightholder engages in a dispute against a wealth-  
240 ier and more powerful contender. Indeed, 97% of respondents reported that, before purchasing  
241 a land parcel, they have requested or would try to obtain from the seller proof of official PFR  
242 registration.

---

<sup>6</sup>As suggested by one Referee, this context can be prone to “John Henry” experimental effect, namely a re-  
active 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 govern-  
ment, 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 gov-  
ernment 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).

<sup>7</sup>The 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.<sup>8</sup>

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  $\chi^2$  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 ( $\chi^2$  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 ( $\chi^2$  test,  $p < 1\%$ ).

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

## 278 3 Research Design

### 279 3.1 Identification

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

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

300 Concerning the data collected for this study, participants were residents of 32 villages ran-  
301 domly selected among those in the RCT pool. Table A1 in Appendix A reports descriptive  
302 statistics relative to socio-demographic characteristics of these subjects. The sample is well  
303 balanced, with only a weakly statistically difference in age across treatments ( $p=9\%$ ) out of  
304 the 22 variables reported. When comparing separately the subsamples of participants living  
305 closer or more distant to paved roads across treatments, in both cases the comparisons return  
306 balanced across subsamples for most of the variables. The most prominent difference consists  
307 of participants reporting Islam to be their primary religion being over-represented (25% vs.  
308 5%,  $p=.01$ ), and those believing in Animist religions slightly under-represented (39% vs. 52%,  
309  $p=.06$ ), in the sample of treated villages close to paved roads.<sup>9</sup> To account for the imbalance,  
310 in the analysis I control for these characteristics.

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<sup>9</sup>When 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.

311 Concerning migration, as a first step I look at data on migration across villages collected in  
312 a post-experimental survey. The vast majority of the participants live in the village where they  
313 were born. In the seven years preceding my experiment, only 4% of the participants had moved  
314 to a different village, and the reason for the few migrations registered was almost exclusively  
315 connected to marriage. The results of a Chi-square test reject the hypothesis that the likelihood  
316 of migrating out of a treated village differs from the likelihood of migrating out of a control  
317 village. The result is the same whether I use the whole sample of participants or focus on  
318 villages in proximity of, or more distant from, paved roads separately. Moreover, Tables A6 and  
319 A7 in Appendix A replicate the regression models estimated in Tables 1 and 2 in the main text  
320 additionally controlling for whether the participant has migrated and the number of years she  
321 has lived in the village. Results remain qualitatively unaffected.

322 As a second step, I need to verify whether, in the seven years following the reform, different  
323 patterns of out-migration from treated and control villages that cannot be captured by survey  
324 responses collected from my study participants (for instance, because of migration from the  
325 village to cities) are registered. To address this, I first rely on pre- and post-intervention  
326 survey data reported in the MCC impact evaluation (Omondi, 2019). Out of 3,338 households  
327 belonging to villages in the RCT pool who were surveyed before the intervention, only 43  
328 (1,2%) have migrated by the second round of survey in 2015. While the Authors do not report  
329 the proportion of migrating subjects who belonged to control or treated villages, these numbers  
330 confirms a very low propensity to migrate in this context. Moreover, I show that data from 2017  
331 relative to the population of the 32 villages in my sample display no significant differences in  
332 average village size across treatment branches ( $p=.26$ , t-test two-sided), in line with the above-  
333 mentioned absence of imbalances in out-migration in treated and control villages reported by  
334 the study participants. Finally, in a survey that I run in 2020 in a sample of 43 Beninese  
335 villages included in the PFR, no significant population differences was found between treated  
336 and control villages ( $p=.85$ , t-test two-sided). These pieces of evidence mitigate concerns that  
337 migration or selection effects could have compromised the identification strategy.

### 338 **3.2 Experimental Games**

339 To assess the subjects' cooperation and trust, a public goods game (PGG) and a trust game  
340 (TG) were employed. In the PGG, subjects were divided into groups of three, and the identity  
341 of the other group members remained unknown. Each subject received an initial amount of  
342 1,000 XOF (approximately \$ 1.5) in the form of 10 coins worth 100 XOF each. The subject  
343 could then divide the 10 coins between a "private envelope" and a "common envelope." The  
344 coins placed in the private envelope became part of the subject's endowment. Coins placed  
345 in the common envelope were increased by 50% by the experimenter and then equally divided  
346 among the three group members. Given these parameters, contributing nothing to the public  
347 good would be the dominant strategy but such contributions would increase the group earnings.

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

353 The cooperation and trust decisions were one-shot, and no feedback regarding the games’  
354 outcomes was provided until the end of the session. To prevent experimenter effects, we im-  
355 plemented a procedure for which the trust and cooperation decisions made by participants in  
356 the PGG were unknown to the experimenter on site. Specifically, the participants privately  
357 divided their coins into two envelopes of different colors marked by a code in a separate room  
358 and then placed the envelopes in a box. In addition to the elicitation of cooperation and trust,  
359 in the second stage of the trust game we also collected trustees’ choices regarding how many  
360 coins to send back to the trustor. However, due to logistical constraints, the trustees’ decisions  
361 were assessed using the strategy method, with the experimenter asking each trustee to state her  
362 decision for each of the six possible transfer levels received from the trustor. Thus, unlike trust  
363 and cooperation choices, trustworthiness decisions were elicited using the strategy methods and  
364 were not blind to the experimenter on site. We report the analysis of data on trustworthiness  
365 in Figure A2 and Table A2 in Appendix A. Despite the methodological differences in the elic-  
366 itation method and experimental procedure, the pattern of results remains similar to those of  
367 cooperation and trust. Data on the participants’ risk preferences were collected following a  
368 lottery-choice task similar to the one used by Voors et al. (2012).<sup>11</sup>

### 369 3.3 Procedures

370 The data collection for this study took place between December 2017 and February 2018.  
371 Participants were residents of a sample of villages randomly selected within the PFR lottery  
372 pool for the provinces of Coffou, Alibori, and Borgou (highlighted in the rectangular boxes in  
373 figure A1 in Appendix A). Each experimental session was run in a different village, and 32

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

<sup>11</sup>Each subject had to make six choices between participating in a lottery or gaining/losing a certain amount. In the initial three choices, participants could choose a lottery with a 3/10 probability of them winning 500 XOF, a 7/10 probability of them winning nothing, or an equivalent gain of 100, 150, or 200 XOF with certainty. In their last three choices, the participants could decide whether to play a lottery in which they had a 3/10 probability of losing 500 XOF, a 7/10 probability of losing nothing, or a certainty of losing of 100, 150, or 200 XOF. 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 .

374 experimental sessions were conducted. Approximately 18 subjects took part in each session.<sup>12</sup>  
375 In total, 515 subjects (292 from treatment-group villages and 223 from control-group villages)  
376 participated in the experiment.

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

385 The experiment sessions took place in a public space (usually a school or a religious building)  
386 that included a large common room and a separate room where subjects could make decisions  
387 in private. Upon arrival, the participants were randomly assigned a number identifier and com-  
388 pleted a brief sociodemographic questionnaire. They were then informed that they had earned  
389 a participation fee equal to 500 XOF (roughly \$0.75) and that they had the opportunity to earn  
390 additional money by participating in a series of tasks. In each session, the participants initially  
391 made the decisions in the PGG and TG games described above. They then participated in  
392 five additional incentivized experimental games and the sociodemographic survey described in  
393 section 5 below.<sup>13</sup> To avoid potential income effects, the participants did not receive feedback  
394 regarding the game outcomes until the end of the experimental session. Moreover, the partici-  
395 pants were told at the outset that only the income generated in four of the seven games played  
396 during the session would be paid and that these four games would be randomly determined at  
397 the end of the session by lottery. Since the majority of the participants were illiterate, the in-  
398 structions for the experimental games were given orally in public by the experimenter.<sup>14</sup> Before  
399 they were allowed to enter the decision room, each participant had to correctly answer a set of  
400 control questions posed in private by the experimenter. If a participant failed to provide the  
401 correct answers, the experimenter repeated the game explanation until the participant could  
402 answer all control questions correctly. Each experimental session lasted approximately 3 hours.  
403 The participants received an average \$6 as final payment, roughly the equivalent of two days'  
404 wages for the subjects in our sample.

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<sup>12</sup>Most of the sessions were completed by 18 participants. However, there was some variation in the number of participants – the minimum number was 12 subjects and the maximum 22.

<sup>13</sup>The incentivized games, which were played in the same order during each session, are the following: PGG, TG, coordination games, a risk elicitation in both the losses and gains domains, and a donation game. After completing the experimental games described in this paper, the participants also took part in a modified dictator game conducted in the context of a different research project.

<sup>14</sup>A English translation of the instructions is included in Appendix B.

### 405 3.4 Empirical Specifications

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

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

410

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

411 where  $D_i$  is the participant's village distance to the closest paved road,  $T_i$  is a dummy equal to  
412 1 for subjects in treated villages, and  $X_i$  is the vector of individual characteristics collected in  
413 the post-experimental survey.

## 414 4 Results

415 I begin the analysis by focusing on the PGG results. Panel 1a of Figure 1 plots the aver-  
416 age number of coins that participants contribute to the public good across treatment groups.  
417 Participants who experienced the reform contribute slightly more to the common account com-  
418 pared to the control sample (t-test two-sided,  $p=4\%$ ), even if the difference among the samples  
419 averages is small. However, the picture changes when separating the villages who are in close  
420 proximity to paved road to the others. To do so, I divide the participants into two samples  
421 of roughly equal numbers using as a threshold the median distance from paved roads of the  
422 village of origin (3.75 miles). In figure Panel 1c of Figure 1, it is plotted the amount of coins  
423 contributed to the common account in treated and control villages by the sample of participants  
424 living in villages closer to paved roads (left bars) or more distant (right bars). For participants  
425 living in villages close to paved roads, the PFR reform results in a substantial and significant  
426 increase in the average contribution to the public account compared to the contribution of the  
427 control-group villagers (t-test two sided; p-value  $< 1\%$ ). The opposite effect occurs regarding  
428 those living in villages more distant from roads, that is, where the reform results in a reduction  
429 of the participants' contribution to the common account compared to control subjects (t-test  
430 two sided; p-value  $< 1\%$ ).

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

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<sup>15</sup>The controls include age, gender, religion, marital status, a dummy equal to one if the subject is monogamous, an incentivized measure of risk preferences, and a dummy equal to one for villages in communes in the South, a



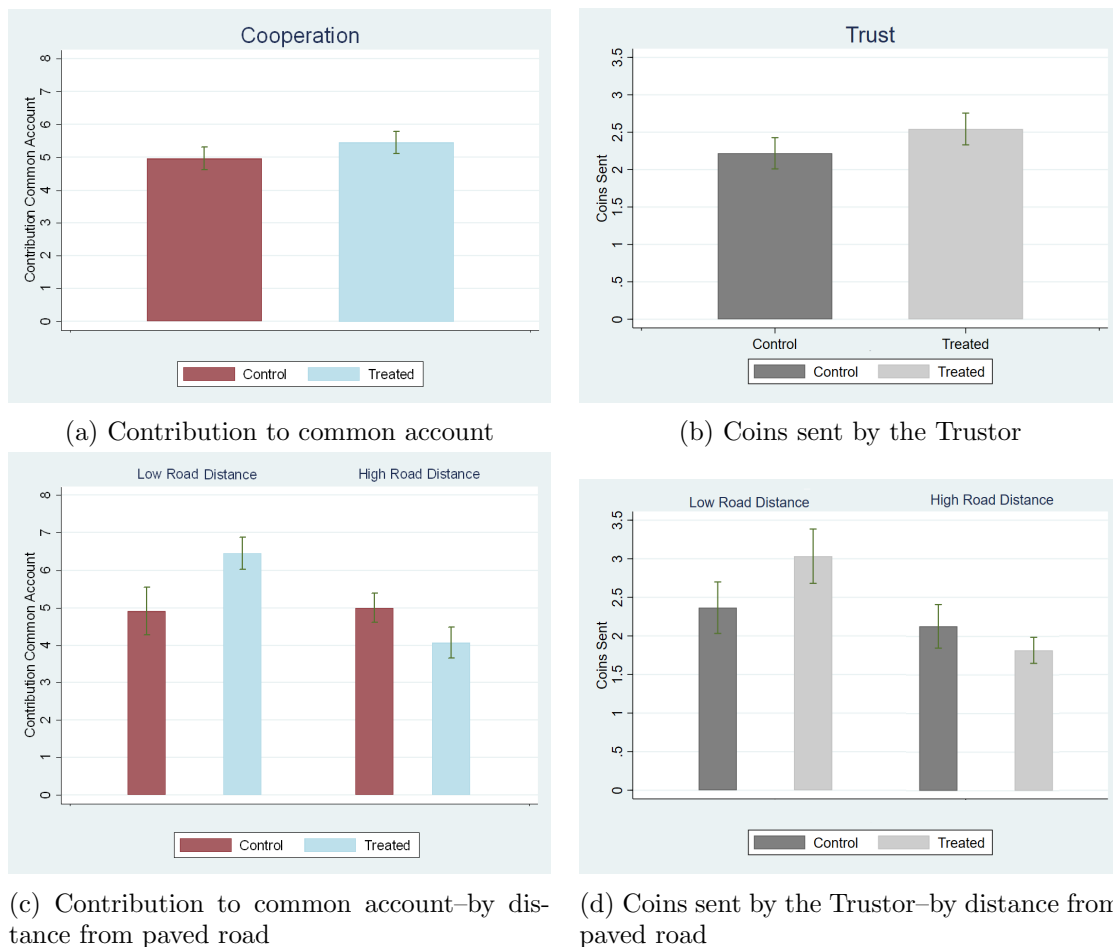


Figure 1: Cooperation and Trust by treatment

435 The coefficient is positive but small and not statistically different from zero, indicating that  
 436 the reform had on average no effect on cooperation for participants in our sample. In Model 2,  
 437 the dummy *treated* is interacted with the variable *road\_distance* indicating the village distance  
 438 from paved roads. The coefficient of *treated* becomes positive and statistically significant at the  
 439 conventional level. The point estimate suggests that the reform caused a roughly 40% increase in  
 440 the public good contribution for villagers having direct access to paved roads in our sample. The  
 441 interaction term *treated\*road\_distance* is instead negative and statistically significant, showing  
 442 that the positive effect of the reform on the public good contribution progressively vanishes as  
 443 the distance from paved roads of the participants' village increases.

444 To better investigate these effects, I consider separately the samples of participants living in  
 445 villages whose distance from paved road is smaller or larger than the sample median. In Model  
 446 3, I focus on the latter group. I regress the number of coins contributed to the public good to  
 447 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.

448 statistically significant at the 5% level, suggesting that the reform induced a reduction in the  
449 number of coins contributed to the public account for participants living in the villages more  
450 distant to paved roads. In contrast, in Model 4, in which the same regression is run on the  
451 sub-sample of participants coming from villages closer to paved roads, the coefficient of *treated*  
452 is positive and statistically significant at the 1% level, confirming that the reform induced a  
453 significant increase in the public good contribution for this category of villagers.

454 I continue the analysis by considering the participants’ choices in the TG. Panel 1b in Figure  
455 1 displays the average number of coins sent by the trustee to the trustor in the first stage of the  
456 TG. The participants who experienced the reform send on average 0.25 coins more than those  
457 in control, a small but significant difference (t-test two-sided,  $p=3\%$ ). In Panel 1d of Figure 1b  
458 I then consider trust choices in villages close or distant from paved roads separately. The two  
459 left bars display the average number of coins sent by the trustors living in the sub-samples of  
460 villages characterized by proximity to paved roads. Villagers who experienced the reform in this  
461 sub-sample on average send significantly more coins than the control-group villagers (two-sided  
462 t-test,  $p=1\%$ ). However, the reform produces the opposite effect for trustors living in villages  
463 more distant to paved roads, as shown by the two bars on the right hand-side of the panel. For  
464 those participants, experiencing the PFR significantly decreases the number of coins sent to the  
465 trustee (t-test two-sided,  $p=4\%$ ).

466 The results from a Tobit regression with standard errors clustered at the village level, re-  
467 ported 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.

468 is regressed on the dummy *treated* and the set of controls described above. The coefficient  
469 of the treatment dummy is not statistically different from zero. In Model 2, where the treat-  
470 ment dummy is interacted with the variable *road\_distance*, the coefficient of *treated* shows a  
471 statistically significant increase in the transfers by trustors living in villages with direct access  
472 to paved roads. The point estimate suggests a roughly 37% increase in average transfer for  
473 these subjects. As it was the case for contributions to the public good, the interaction term  
474 *treated\*road\_distance* is negative and statistically significant. In Models 3 and 4, I again split  
475 the sample and consider separately participants living in villages closer or more distant than  
476 the median distance from paved roads in the sample. For participants coming from villages  
477 more distant from paved roads, the coefficient of the variable *treated* in Model 3 is negative  
478 and marginally significant (p-value = .07), suggesting that the reform reduces trust as mea-  
479 sured in the experiment. Conversely, the positive and statistically significant coefficient of the  
480 dummy *treated* in model 4 shows that experiencing the PFR produces an increase in the average  
481 trustor’s transfers for participants in villages with comparatively high market exposure.

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

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

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

## 498 **5 Mechanisms**

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

### 503 **Conflicts**

504 I verify whether the observed changes in cooperation and trust could be explained by a  
505 change of the land-related conflict rate experienced by participants. The PFR reform introduced  
506 a process of systematic land demarcation and rights recording that included the resolution of  
507 existing land disputes as a preliminary step in the procedure. Therefore, the intervention might  
508 have cleared existing disputes due to unclear land boundaries and their associated land-user  
509 rights. Had this happened, the observed changes in cooperation and trust might have been a  
510 consequence of a reduction in the conflicts experienced by the participants rather than a change  
511 in their values or beliefs.

512 I verify this possibility by comparing land-related conflicts experienced by the participants.  
513 In rural Benin most disputes are solved without resorting to formal courts and no reliable  
514 administrative data source on land-related conflicts is available. Here I rely on self-reported  
515 data on conflict episodes that happened in the seven years before the experiment. However,  
516 this data might suffer of recalling bias. Therefore, the results of this subsection are valid on the  
517 assumption that participants correctly reported the disputes eventually experienced or that, if  
518 misreporting happened, that the recalling bias was similar between treated and control villages.  
519 A Chi-square test cannot reject the hypothesis that the likelihood of experiencing conflicts is  
520 the same across treatment groups whether I consider the whole sample of participants or divide  
521 them according to their villages’ high or low distance from roads. These results are confirmed  
522 by regression analysis reported in Tables A10 and A11 in Appendix A, in which I re-estimated

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

## 525 **Changes in Altruistic Preferences**

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

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

## 539 **Income and Access to Credit**

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

548 A similar conclusion can be drawn when considering participation in the credit market.  
549 Previous research in rural areas of low-income developing countries suggests that land titling  
550 tends to have small effects on improving credit access (Besley and Ghatak, 2010, Deininger  
551 and Feder, 2009). I check whether the Beninese PFR increased participants' borrowing of  
552 resources through the formal or informal credit markets. To do so, I compare self-reported  
553 data on the participants' borrowing choices in the seven years preceding the experiment (please

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<sup>16</sup>Specifically, 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.

554 notice that the caveat concerning possible recall bias discuss in the previous section applies  
555 as well here). The difference in the likelihood of borrowing resources or accessing credit is  
556 not statistically significant for the participants in treatment-group and control-group villages,  
557 neither when looking at the whole sample nor when we split it in sub-samples according to  
558 the villages distance to paved roads (Chi-square test,  $p > 10\%$  in all cases). I also estimate the  
559 likelihood that a participant reports to have used financial instruments in the previous seven  
560 years. Results from Logit specifications reported in Table A13 show that experiencing the  
561 reform does not determine significant differences across treatment groups, neither on average  
562 nor when we focus on sub-samples of villages with small or large distance to paved roads.

### 563 **Individual and Social Approval of Formal Institutions**

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

575 I then move to the social legitimacy of the new institutions and investigate how the PFR  
576 affected the social norms related to use the formal judiciary in order to overrule an unfavorable  
577 decision of the customary authority. To do so, the sample of participants to the PGG and  
578 TG took part in an incentivized coordination game similar to Krupka and Weber (2013). The  
579 participants were presented with the action of a hypothetical agent and were asked to choose  
580 among four possible characterizations of the social appropriateness of the agent's action that  
581 ranged from "Very Socially Inappropriate" to "Very Socially Appropriate". Those who correctly  
582 guessed which characterization would be the modal choice within the village received monetary  
583 compensation equal to two experimental coins, each worth 100 XOF. The situation involves  
584 a conflict over a land parcel between two agents and a judgment by the customary conflict  
585 resolution authority that is unfavorable to one party. The participants had to rate the decision  
586 of the disfavored party to legitimize the judgment of the customary authority and to abstain  
587 from challenging the unfavorable decision through the formal judiciary.<sup>17</sup>

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

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<sup>17</sup>The 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.

589 participants’ rating of the situation. The dependent variable—the perceived appropriateness of  
590 legitimizing the judgment of the customary authority—ranges from 1 (the least appropriate) to  
591 4 (the most appropriate). Regressions include the set of controls specified above. Standard  
592 errors are clustered at the village level. Model 1 considers the whole sample of participants  
593 and interacts the treatment dummy with the village distance from paved roads. While the  
594 coefficient of the treatment dummy is not significant, suggesting no effects on villagers with  
595 direct access to paved roads, the coefficient of the interaction term is positive and significant at  
596 the 1% level. The result shows that, for villagers experiencing the PFR, the legitimacy of the  
597 customary authority increases with distance to paved roads. This result is confirmed by Models  
598 2 and 3, in which I consider the restricted sample of participants living more distant and closer  
599 to paved roads than the sample median, respectively. The coefficient of the treatment dummy  
600 is positive and significant at the 1% level for villagers distant from paved roads in Model 2,  
601 showing an increased legitimization of the customary authorities, while small and insignificant  
602 in Model 3.

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

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

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

## 624 **6 Discussion and Conclusions**

625 This paper studies the impact of a major reform of property rights over land on trust and  
626 cooperation. The reform was implemented in rural Benin and transformed collective and infor-  
627 mal land rights in individual and formally registered property rights that can be defended in  
628 court, sold, or used as collateral. The identification strategy makes use of the peculiar imple-  
629 mentation process of the reform, the first case of large-scale land tenure reform implemented as  
630 randomized control trial. From a sample of hundreds of villages, half were selected by means  
631 of a public lottery for the reform’s implementation. As of today, the villages not selected for  
632 the reform continue to follow the traditional system of customary land rights. The participants  
633 pool in my experiment comprises individuals belonging to 32 villages randomly selected from  
634 the PFR lottery pool. Experimental measures of willingness to cooperate and trust are assessed  
635 using a public goods game and a trust game.

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

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<sup>18</sup>The 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?”.

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



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

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

653 These findings resonate with previous empirical studies on the cultural effects of ownership in  
654 urban settings and industrialized societies (DiPasquale and Glaeser, 1999, Di Tella et al., 2007,  
655 Field, 2007). At the same time, they reconcile the contrasting evidence reporting little impact  
656 or even negative results produced by case studies of land tenure reforms in rural sub-Saharan  
657 Africa and other low-income developing countries (Goulding and Friedman, 2018, Lund et al.,  
658 2006, Platteau, 2000). The results shed light on the key role played by the specific institutional  
659 and legal framework in which a land rights reform takes place, emphasizing that institutional  
660 constraints might offset potential social and cultural gains of formalization.

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

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

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

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

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## 839 Appendix A Supplementary Material and Data Analysis

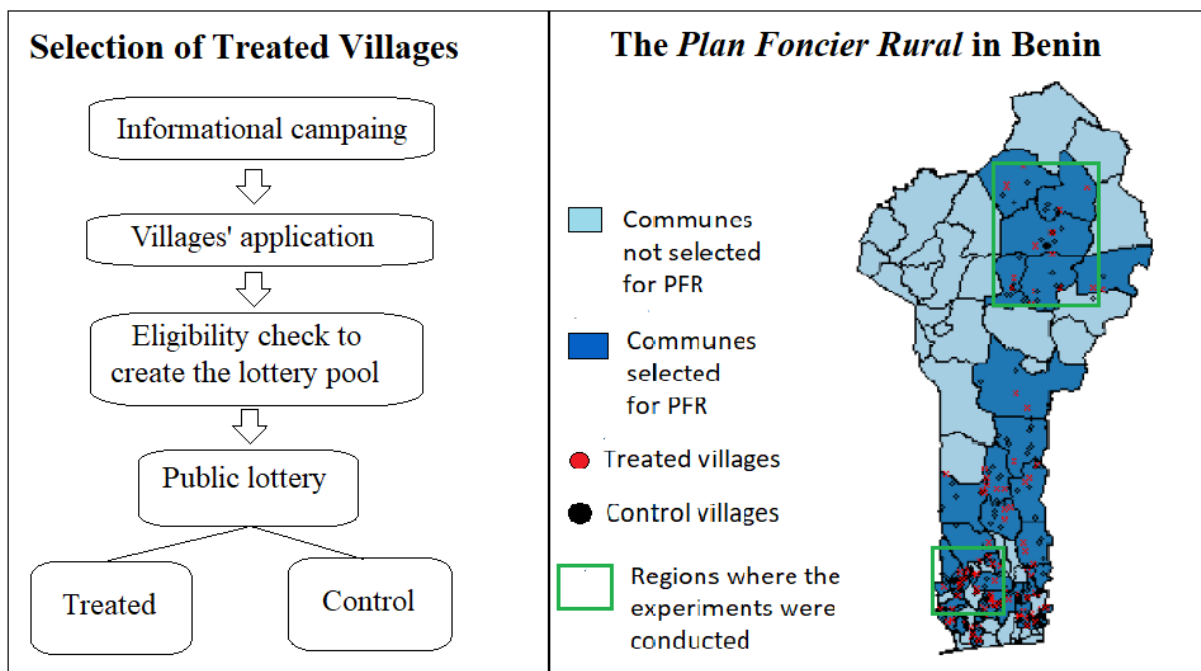


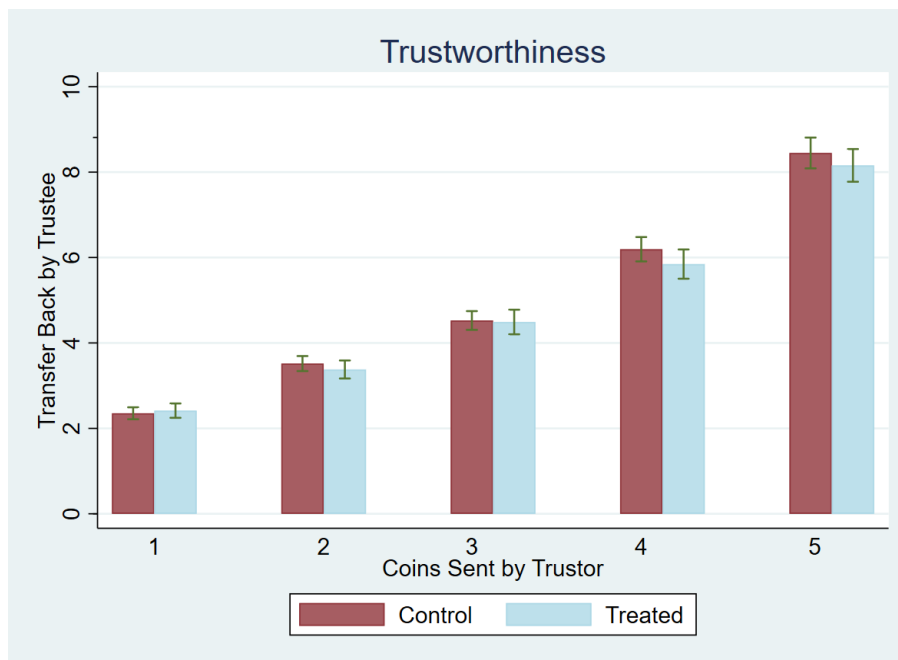
Figure A1: *Left Panel:* The mechanism for selecting treated villages. *Right Panel:* Distribution of treated and control villages after the RCT implementation. The green square identifies the provinces where the data collection took place. The communes that were excluded from participating in the PFR 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

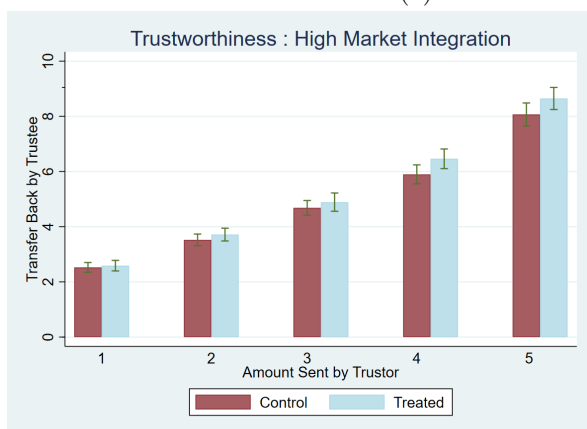
Sample:	Whole			High-dist			Low-dist		
	Treated	Control	diff( $p$ )	Treated	Control	$p$	Treated	Control	$p$
male	.59	.57	.64	.49	.56	.24	.65	.57	.19
age	38.8	37.0	.09	38.4	35.3	.03	39.1	39.6	.75
age-village(%)	85.0	83.2	.24	87.9	83.9	.22	84.5	82.1	.50
literacy	.39	.40	.84	.32	.36	.46	.44	.46	.76
edu-years	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.

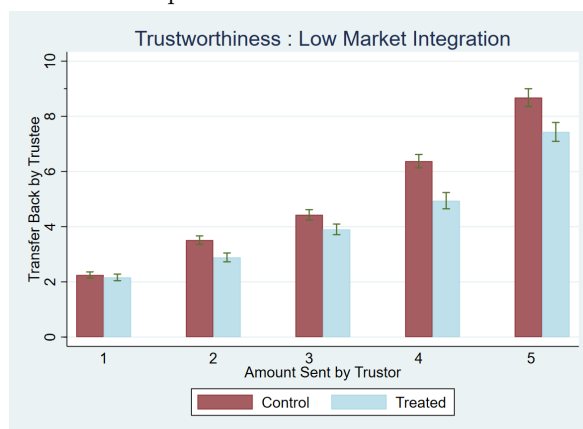




(a) Trustworthiness – whole sample



(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

848 participant decides how many coins to send back for each possible level of the trustor’s initial  
849 transfer. Figure A2a shows that, in our sample of participants, the levels of trustworthiness  
850 are on average left substantially unaffected by the reform. A Hotelling T-squared test confirms  
851 that there is no statistically significant difference in the number of coins sent back to the  
852 trustee between the treatment-group and control-group participants. Figures A2b and A2c  
853 plot the average trustee transfer as a function of the initial amount received by the trustor for  
854 participants in villages with distance from paved roads smaller and larger than the median,  
855 respectively. Trustees on average transfer back more coins in treated villages with distance  
856 from paved roads lower than the sample median. However, a Hotelling T-squared test shows no  
857 statistically significant difference between the two samples. Conversely, for villages with high  
858 distance from paved road, trustees in the treatment-group villages transfer back significantly  
859 less coins to the trustors compared to trustees in the control-group villages (Hotelling T-squared  
860 test two-sided, p-value < 1%).

861 I then conduct a regression analysis. Given the strategy method used to assess trustworthi-  
862 ness, the data are in the form of a panel of individual choices clustered within villages. Therefore,  
863 a hierarchy can be detected in the data structure, and different sources of heterogeneity might  
864 arise at different levels of the hierarchy. To capture the correlation between repeated individual  
865 measurements and the village-specific effect, I implement a hierarchical mixed-effects regression  
866 model.<sup>20</sup> As before, I regress the trustee number of coins sent back to the dummies *treated*,  
867 *road\_distance*, their interaction, and the set of controls specified above.

868 Table A2 displays the results. Model 1 includes the whole sample of observations. The  
869 coefficient *treated* is not statistically different from zero, which shows that the reform has no  
870 significant effects on the trustworthiness choices of participants living in villages with direct  
871 access to paved roads. The coefficient of the interaction term is instead negative and marginally  
872 statistically significant, suggesting that being exposed to the reform causes a progressive re-  
873 duction of participants’ trustworthiness when the distance from paved roads increases. Model  
874 2 restricts the attention to the sample of trustee choices when the trustor’s initial transfer is  
875 more than one coin. The results are confirmed, and the coefficient of the interaction term  
876 *treated\*road\_distance* becomes significant at the conventional level.

877 Models 3 and 4 repeat the analysis but focus only on the participants in villages whose  
878 distance from paved roads is larger than the median. The coefficient of the dummy *treated*  
879 is negative and strongly statistically significant in both models. Point estimates suggest that,  
880 for participants in these villages, experiencing the reform causes a roughly 20% decrease in the  
881 average number of coins returned to the trustee. Models 5 and 6 repeat the analysis with a  
882 focus on the sample of participants living in villages closer to paved roads. The coefficient of the

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<sup>20</sup>I specified a three-level model by introducing random effects for the set of trustee choices for each possible level of trustor transfer received within a village and individual trustees nested within villages. Therefore, the trustor-specific transfer comprises the first level of the model, the individual trustee comprises the second level, and the villages comprise the third level.

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 (0.403)	0.327 (0.478)	-0.878*** (0.260)	-1.106*** (0.328)	0.297 (0.467)	0.373 (0.577)
road_distance	-0.003 (0.024)	0.002 (0.027)				
Treated× road_distance	-0.058* (0.031)	-0.077** (0.035)				
Constant	5.453*** (0.709)	6.139*** (0.819)	5.365*** (0.785)	6.171*** (0.915)	6.200*** (1.161)	7.150*** (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 × 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 × 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	0.879**	-0.288*	1.034**
	(0.278)	(0.439)	(0.170)	(0.503)
road_distance		-0.000		
		(0.017)		
Treated $\times$ road_distance		-0.062**		
		(0.027)		
Constant	2.382***	2.719***	2.464***	1.791*
	(0.475)	(0.583)	(0.256)	(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

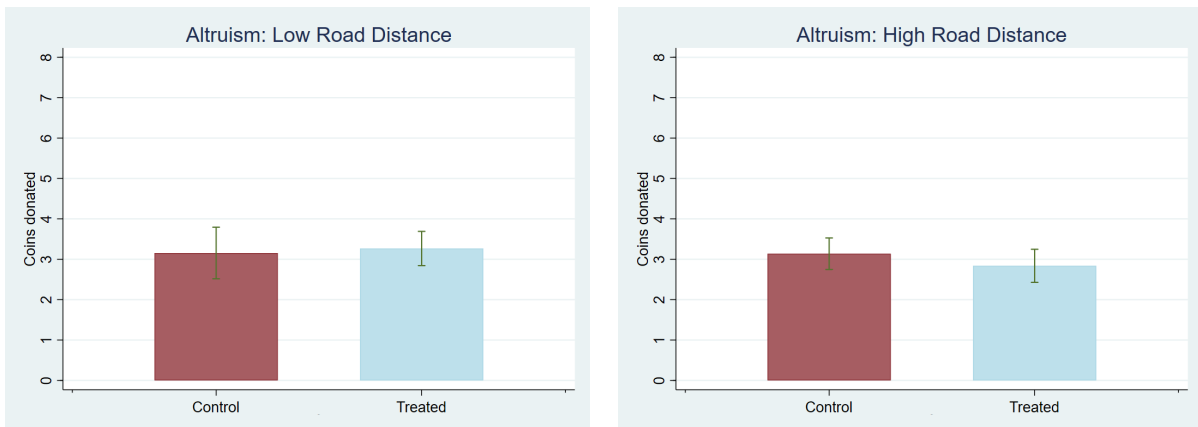
	Model 1	Model 2	Model 3	Model 4
	Whole Sample		High-Dist	Low-Dist
Treated	0.627	1.930**	-0.906**	2.530***
	(0.567)	(0.948)	(0.420)	(0.970)
road_distance		0.050		
		(0.041)		
Treated $\times$ road_distance		-0.142**		
		(0.068)		
conflict	-0.324	-0.273	-0.423	-0.275
	(0.347)	(0.339)	(0.262)	(0.521)
Constant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

## 886 **Appendix B Instructions for Experimental Games**

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

- 892 • The 500 XOF for showing up today
- 893 • The money you earn in 4 games. To determine which 4 games will be selected for payouts  
894 among all the games you will play today, we will draw 4 numbers from this bag, and the  
895 game corresponding to the number extracted will be the one paid. This means that you  
896 should take your decisions in all 7 games seriously because there is a very high chance  
897 that any one game will become relevant to your payment!

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

### 907 **Public goods game (NEVER CALL IT THIS IN FRONT OF THE PARTIC-** 908 **IPANTS!)**

909 The outcome in this game depends on your decisions and the decisions of two others in this  
910 meeting. Note that you will never know who these two others are and these two others will  
911 never know that they played with you. You and the two others will have to make the same  
912 decision. Here are two envelopes. In one envelope, which is denoted as your envelope, are 10  
913 coins worth XOF 100 each. These coins are yours. The other, which is denoted as your group  
914 envelope, is empty. You decide how many of the 10 coins you transfer to your group envelope.

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

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

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

932 *Example:* Imagine Participant 1 gives 10 points to the group envelope, Participant 2 gives  
933 0 points to the group envelope, and you give 4 points to the group envelope. We will then add  
934 0.5 points for each point in the group envelopes, that is, there are  $14 + (10 + 0 + 4) \cdot 0.5 =$   
935 21 coins. Then we divide these coins equally among the three participants so that all get 7  
936 coins in addition to the coins they kept in their individual envelopes. So, Participant 1 gets  $0$   
937  $+ 7 = 7$  coins, Participant 2 gets  $10 + 7 = 17$  coins, and you get  $5 + 7 = 12$  coins. Note that  
938 Participant 2 received more points than you and Participant 1 because he did not transfer any  
939 coins to the group envelope. In contrast, Participant 1 received less because he transferred all  
940 10 coins to the group envelope.

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

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

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

954 *Example:* Imagine Participant 1 sends 0 coins to Participant 2. This means no coins are  
955 sent, and Participant 2 cannot send any coins back. Therefore, the game ends with the initial  
956 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

981 “socially inappropriate” and “inconsistent with moral or proper social behavior.” For socially  
982 appropriate, we mean the behavior that most people think is the “correct” or “ethical” thing  
983 to do. Another way to think what we mean by socially appropriate is that if the person were to  
984 select a socially inappropriate choice, then someone else might be angry at the person for doing  
985 so. We ask you to indicate whether you think the action chosen by the person is “Very socially  
986 inappropriate,” “Somewhat socially inappropriate,” “Somewhat socially appropriate,” or “Very  
987 socially appropriate.” When all the participants to today’s meeting have given an answer for a  
988 situation, for each possible choice we determine which response was selected most often. If you  
989 give the same response as the one selected most often by other participants, then you receive  
990 an additional 10 coins.

991 **Description of the situation 1:** *A farmer living in village A is asked by a farmer living*  
992 *in another village to sell him a lot of his land. The price offered for the land is very good and*  
993 *higher than the money the farmer living in village A can make with that land.*

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

996 **Description of the situation 2:** *Person A and Person B enter into a conflict regarding*  
997 *the use of a parcel of land. Person B has been cultivating that land for a few years. Person A*  
998 *claims that that land belongs to him and that Person B in the past could use it because he did*  
999 *not need it. But Person A now wants to sell the property to someone else, so Person B has*  
1000 *to leave the land. The chief of the village decides that Person B could keep a part of the land*  
1001 *and that Person A can try selling the other part. Person A is not satisfied with the decision,*  
1002 *since the buyer of the land wants either all of the land (including the part allocated by the chief*  
1003 *to Person B) or nothing. Person A has the right to ask the formal judicial authority of the*  
1004 *Republic of Benin to recognize his right over the land and to kick out Person B.*

1005 **Action undertaken by Person A:** *Person A leaves the situation as it is and forgoes the*  
1006 *possibility of selling the land.*

1007