

# Expressive Property\*

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## Abstract

Why are impartial institutions such as formalized property rights so important for the emergence of impersonal trade? Previous literature has stressed the role of such institutions in providing third-party enforcement to shield strangers from locals' opportunism. We document the existence of a second mechanism based on the expressive function of formalized property rights and we study their role in coordinating respect for the property of strangers. Ten years after the randomized introduction of formal property rights across rural Benin, we conducted a taking-dictator-game experiment in which participants can appropriate the endowment of an anonymous stranger from a different village. Even if enforcement institutions are absent and peer effects are silenced by design, participants from villages where the reform was implemented took significantly less than those in control villages. We further give consideration to several possible transmission channels and show that the introduction of formal property institutions may have an "expressive" function, coordinating expectations around non-conflictual outcomes.

*JEL Codes:* D91; K11; K42

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\*The experiment was approved by the Research Ethics Committee Parc de Salut MAR - Barcelona, reference nr. 2018/8015/I. Participants provided informed consent. The empirical strategy was pre-specified in a pre-analysis plan that was registered at the AEA RCT Registry—ID AEARCTR-0005322—before we collected the data, and included specification of the different hypotheses to be tested, of the regression approach, and of the dimensions to be studied in the heterogeneity analysis. The pre-analysis plans concerning the two auxiliary experiments on honesty and coordination were pre-registered at the AEA RCT Registry at the same time of the main experiment (IDs AEARCTR-0005324 and AEARCTR-0005319, respectively). Marco Fabbri gratefully acknowledge financial support by the Marie Curie Individual Research Grants Scheme, grant H2020-MSCA-IF-2017-789596. Giuseppe Dari-Mattiacci gratefully acknowledges research and financial support by Columbia Law School. Matteo Rizzolli gratefully acknowledges financial support by LUMSA University. We are indebted to Deo-Gracias Houndolo for his support during the fieldwork, and to Dr. Kevine Kindji, Dr. Charles Ibikounle, and Csoban Gocze who provided detailed information on the protection of land in Benin. Ametonou Charmelle, Dossou Fiogbe, Gaston Gnonlonfoun, Issifou Gounou, Colin Henderson, Madeline Holbrook, Nice Houngbegnon, Dorothee Lokossou, Aissath Salifou, Aparna Sundaram, Mohamed Sedou, and Israelia Zannou provided excellent research assistance. The usual disclaimer applies.

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21 Reform; Law and Economics; Property Rights; Randomized Control Trial; Taking  
22 Aversion

## 23 1 Introduction

24 When trade transcends family, kin, and friendships circles, an individual's reputation is  
25 no longer a sufficient bond. Society can reap the benefits of impersonal trade only if it  
26 develops solutions to various forms of opportunism by locals at the expense of strangers —  
27 cheating, renegeing on promises and expropriation — which are at the core of the *fundamental*  
28 *problem of exchange* (Greif, 2000).<sup>1</sup> Throughout history, a wide spectrum of institutional  
29 arrangements has emerged to enforce impartiality in the market arena and shield strangers  
30 from locals' opportunism. Institutions for third-party regulation, dispute resolution and,  
31 enforcement especially devoted to protecting strangers can be found in Athens,<sup>2</sup> Rome,<sup>3</sup> and  
32 Mediterranean and North-European trading cities.<sup>4</sup>

33 Among them, institutions supporting and preserving private property — the hallmark of  
34 western legal cultures (Garnsey, 2014) — have taken center stage as propellers of trade and  
35 development both in a vast and influential literature (North and Weingast, 1989; Besley and  
36 Ghatak, 2012) and in a campaign of institutional reforms in developing countries (De Soto,  
37 2000; Lipton, 2009).<sup>5</sup> Formal property rights are inherently designed to be impartial: they

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<sup>1</sup>Kadens (2015, 2019) provides many interesting examples of opportunistic behavior in medieval market and credit transactions.

<sup>2</sup>In order to facilitate exchange, in 375 BC, Nikophon's Laws on Silver Coinage made the *Approver* — a state official charged with the task of probing silver coins — available free of charge to all traders coming to Athens' Piraeus marketplace, located at the city arbor (Ober, 2015).

<sup>3</sup>Catering to the increasing demand for access to justice fostered by Rome's newly acquired dominance in the Mediterranean Sea, around 242 BC a new outward-looking institution was added to the administration of justice, the *praetor peregrinus*, a magistrate with jurisdiction on disputes involving foreigners (de Ligt, 2020; Arrunada, 2020). An even older institution, the *aediles curules*, charged with regulation and adjudication in cattle and slave markets since 449 BC, progressively developed remedies for non-conformity in sale contracts that were more expedient and hence better tailored to the needs of visitors than those afforded by general contract law (Abatino and Dari-Mattiacci, 2020). The features of these remedies included the possibility to rescind the contract rather than asking for damages, shorter statutes of limitations, a standardized list of actionable defects, and the irrelevance of fraudulent conduct.

<sup>4</sup>Starting from the 11<sup>th</sup> century AD, cities like Genova (Greif, 1994*b*), Venice (Gonzalez de Lara, 2008) and, later, Istanbul (Faroqui, 2004) and Amsterdam (Gelderblom, 2013), just to name a few salient examples, were famously welcoming of (selected groups of) foreign merchants and afforded them institutionalized support and protection.

<sup>5</sup>These institutions foster impersonal trade as directly as the crow flies. A second breed of institutional solutions have been used to constrain opportunism indirectly by leveraging on the private governance arrangements within relatively homogeneous groups. Collective liability induces the punishment of opportunists by their own group. Collective liability can be found in both ancient and modern legal system, ranging from the biblical *lex talionis* to the liability of medieval guilds (Greif, 2006, 1994*a*; Greif and Tabellini, 2010). Conversely, a well-organized group can threaten collective retaliation for harm visited upon any of its members. A particularly effectively retaliation strategy was that of the German Hanse (Greif, 1994*b*).

38 are grounded in institutions, such as registries, conceived to provide uninformed strangers  
39 with reliable notice of existing entitlements and to serve as a basis for enforcement against  
40 any third party (Hansmann and Kraakman, 2002; Acemoglu and Johnson, 2005; Ayotte and  
41 Bolton, 2011; Arruñada, 2012).<sup>6</sup>

42 However, making information available to strangers and establishing impartial mecha-  
43 nisms that facilitate third-party enforcement might not be the sole bulwark against oppor-  
44 tunistic behavior. Recent literature points to the existence of values — that is, “personal  
45 and societal preferences” (Benabou and Tirole, 2011) — of respect for the property of others.  
46 At the micro level, previous literature has studied how, in the institutional vacuum created  
47 in laboratory experiments, individuals exhibit a certain degree of respect for the property  
48 of others regardless of enforcement by victims, group members or third parties. This is a  
49 specific social preference that has been recently qualified as *taking aversion* (Korenok, Mill-  
50 ner and Razzolini, 2018; Faillo, Rizzolli and Tontrup, 2019). At the macro level, pro-social  
51 preferences display large cross-cultural variation (Roth et al., 1991; Henrich et al., 2005) and  
52 pro-social behavior is positively associated with a society’s exposure to impersonal markets  
53 and their institutions (Henrich et al., 2001, 2010). To explain this association, it has been  
54 hypothesized that institutions and preferences may co-evolve: individuals internalize the  
55 cultural norms of impersonal pro-sociality that characterize the surrounding institutional  
56 environment, which, in turn, strengthens the demand for even more impartial institutions  
57 (Fehr and Gächter, 2002; Fehr and Fischbacher, 2003; House et al., 2013; Henrich, 2020).

58 Taken together, these two strands of literature point to a possible double dividend of  
59 formal property institutions. On the one hand, they promote impersonal trade through  
60 formal and impartial third-party enforcement of property rights; on the other hand, their  
61 expressive power shapes values that foster respect for the property of others irrespective of  
62 enforcement incentives. While there is ample literature on the former dividend, our paper  
63 provides novel and unique field-experimental evidence of the latter.

64 Establishing a causal link between institutions and values is problematic because of the  
65 rare occurrence of real-world institutional experiments.<sup>7</sup> In order to address this challenge,  
66 we exploit the first case in which different land property institutions were implemented in  
67 a set of Beninese rural villages, in West Africa, via a large-scale randomized control trial  
68 (henceforth, RCT). The Torrens-type land titling reform that we study consisted of the

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<sup>6</sup>Differently from a contractual right, which is *in personam*, that is, it creates a legal relationship between the contracting parties, a property right is fundamentally impersonal, or *in rem*, that is, it creates a relationship between the owner and the “thing”.

<sup>7</sup>In principle, laboratory experiments solve the identification problem by randomly assigning individual to different institutional setting. See for instance Kimbrough, Smith and Wilson (2008); Kimbrough and Wilson (2013); Wilson (2020). However, the artificial features of the institutions studied in the laboratory and the impossibility to investigate medium/long-term effects—which is arguably what institution builders care about—entail limits on the external and ecological validity of laboratory experimental findings (Alesina and Giuliano, 2015).

69 demarcation of land parcels and the registration of land titles in public registries, which  
70 conferred proof of ownership to rightholders. The intervention transformed customary use-  
71 rights over land subject to social control and enforcement by traditional local authorities  
72 into formal titles that are functionally analogous to private, transferable property rights  
73 enforced by state courts.<sup>8</sup> The reform was implemented in 2010-2011 in 294 treated villages,  
74 while in a set of 282 control villages no intervention took place. Crucially, the selection  
75 into treated and control was done via a public lottery organized with the logistical support  
76 of the Millennium Challenge Corporation. Ten years later, in the control villages no other  
77 formalization of rights had taken place and customary land rights remain in place to date  
78 (Goldstein et al., 2018; Omondi, 2019).

79 In early 2020, our research team visited 32 villages randomly selected among those in-  
80 cluded in the original RCT and conducted a lab-in-the-field experiment in which participants  
81 undertook an anonymous *taking* dictator game. In this variant of the game the active player  
82 — the “dictator” — decides how much of the passive player’s endowment to take.<sup>9</sup> To assess  
83 whether the formalization of property rights fosters values of respect for the property of  
84 strangers, in our main experimental manipulation the dictator is asked to make a decision  
85 on how much to take from the endowment of an anonymous passive player who is a resident  
86 of a different village — that is, a *stranger*.

87 In a previous experiment conducted in a different sample of Beninese villages in 2018, two  
88 of us showed that the introduction of formal property rights reduces taking when the game  
89 is played between *locals*, who are members of the same village (Fabbri and Dari-Mattiacci,  
90 2020). Yet, the reduction in taking rates registered within members of small, personally-  
91 interconnected communities counting only few households might not reflect the workings of  
92 values apt to favor the expansion of impersonal trade. For instance, the reform started with  
93 a clearing process of existing boundary disputes, which is likely to have reduced conflicts  
94 within the community in the short term. Therefore, the reduction in taking from a fellow  
95 villager observed by Fabbri and Dari-Mattiacci (2020) in the years immediately following the  
96 reform might reflect this temporary pacification effect and improved relationships among  
97 locals. Alternatively, the new property rights architecture could have enlarged the group  
98 within which rules of reciprocity yield respect for the members’ “mine and yours,” from  
99 the extended family to the entire village community (Wilson, 2020). These considerations

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<sup>8</sup>The reform did not affect access to formal justice directly but, contrary to informal customary rights whose existence is hard to prove in court, registered rights can be used as conclusive evidence of ownership in trial, thus substantially improving the right-holder’s position.

<sup>9</sup>In contrast, in a standard *giving* dictator game, the dictator decides how much of his or her own endowment to give to a passive player. Although the sub-game perfect equilibrium prediction in the two variants of the game is the same, subjects consistently allocate smaller endowments to themselves in the taking variant of the game (Bardsley, 2008; Faillo, Rizzolli and Tontrup, 2019) and are willing to forgo on average 30% of their endowment to be put in the position of the *giving* dictator instead of the *taking* dictator (Korenok, Millner and Razzolini, 2018).

100 suggest that the observed reduction in taking rates among locals would leave unresolved  
101 the fundamental problem of exchange that arises when individuals interact with unrelated  
102 strangers outside their village community (Ridley, 2012). Even worse, it might be the case  
103 that the formalization of property rights increased parochialism by substituting lower taking  
104 rates among locals with more stealing from strangers.

105 To address these concerns, in this paper we focus on whether the introduction of impartial  
106 property institutions fosters the emergence of values of respect for the property of strangers.  
107 Our results show that dictators resident in treated villages took less from a stranger pas-  
108 sive player as compared with dictators in control villages. The reform reduced individuals'  
109 willingness to take from out-group strangers by roughly 12%. A post-experimental survey  
110 and a pre-registered heterogeneity analysis show that the effect is driven by participants  
111 who actually possess land parcels included in the reform and who enjoy comparatively easier  
112 access to the formal legal system — a key benefit for right-holders under the tenure for-  
113 malization program — thus increasing confidence that first-hand experience with the reform  
114 determines the observed reduction in taking. After having established this main result we  
115 then embark on an inquiry into the possible mechanisms underlying the change in behavior  
116 that we document.

117 First, we look at whether the reform affected the socioeconomic environment in ways  
118 that have been recognized to contribute to an individual's respect for the property of oth-  
119 ers. Starting with wealth, richer people might have less need or inclination to take from  
120 others, either locals or strangers. However, in our sample, we do not observe differences in  
121 affluence or access to credit between treated and control participants, and the main results  
122 are robust to controlling for these factors. Another socio-economic factor that could explain  
123 the observed increase in taking aversion is human capital accumulation, which is generally  
124 associated with higher pro-social behavior. For instance, Galiani and Schargrotsky (2010)  
125 show that awarding formal property rights to Argentinian squatters causally increased in-  
126 vestments in offspring's education. In contrast, nothing like this happened in our case. In  
127 our sample, there are negligible differences in literacy between treated and control groups  
128 and neither education levels nor literacy rates are associated with the dictators' behaviour.  
129 A further possible mediating factor we consider is transaction costs: formalized property  
130 rights might decrease conflicts among right holders, in turn leading them to be more willing  
131 to maintain the status quo by respecting each others' property. However, the number of  
132 land-related disputes experienced by participants is not associated with the dictators' taking  
133 behaviour and the results are confirmed when controlling for conflicts.

134 Second, we entertain the possibility that the reform affected moral values that may  
135 have a bearing on taking behavior: universalism/parochialism,<sup>10</sup> honesty and altruism. To

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<sup>10</sup>That individuals favor socially closer fellows — such as members of the same family, clan and village — relative to more socially distant individuals is well known in a vast literature that has qualified this

136 investigate whether formal property rights affected moral universalism in respecting the  
137 property of others, our participants were asked to make a second taking decision as a dictator,  
138 prior to which they had been informed that the passive player was a member from the same  
139 village (thus replicating Fabbri and Dari-Mattiacci, 2020).<sup>11</sup> In line with previous findings,  
140 results confirm that dictators took more from strangers than they did from locals both in  
141 treated and in control villages. However, the difference between taking from strangers and  
142 taking from locals did not change with the reform. The reform induced subjects in treated  
143 villages to take less from strangers but did not make them less parochial. As to honesty, we  
144 measured preferences for truth-telling through an experiment in which individuals self-report  
145 the outcome of 10 dice rolls and get paid accordingly (Fischbacher and Föllmi-Heusi, 2013;  
146 Rosenbaum, Billinger and Stieglitz, 2014; Abeler, Nosenzo and Raymond, 2019). Consistent  
147 with previous results, we observe a contained level of cheating, which is not statistically  
148 different between treated and control villages, suggesting that the increased taking aversion  
149 induced by the reform cannot be explained by an increase in generalized honesty. Finally, we  
150 measured altruism by using a standard dictator game in which the dictator must decide how  
151 much of an amount provided by the experimenter to donate to a charity operating outside  
152 the village. We detect no difference in donations between treated and control villages.

153 A third tentative explanation focuses on whether the formalization of property rights  
154 interacts with the way in which individuals lay claims to things, which in turn has deep  
155 behavioural roots (Zeki, Goodenough and Stake, 2004; Wilson, 2020). As reported in several  
156 experimental studies, subjects are less likely to take from passive players who have earned the  
157 asset through effort (List, 2007; Jakiela, 2011; Korenok, Millner and Razzolini, 2018; Faillo,  
158 Rizzolli and Tontrup, 2019).<sup>12</sup> It might be possible that the introduction of formal property  
159 rights brings with them a perception of property as “rightfully earned” which would deter-  
160 mine an increased respect for others’ property. To explore this channel, in half of the sessions  
161 the passive players earned their endowments through an effort task rather than receiving it  
162 as windfall money. In line with previous experimental findings, dictators handling windfall  
163 money took significantly less when paired with players who had earned the endowment by  
164 exerting effort. However, the reduction in taking was equal in the effort and windfall-money  
165 conditions: the reform resulted in an increase in respect for property regardless of its origin.  
166 This suggests that introducing impartial property institutions leverages on an “abstract”

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phenomenon as *parochialism*, as opposed to *universalism* (Enke, Rodríguez-Padilla and Zimmermann, 2020), and documented it also experimentally (Bernhard, Fehr and Fischbacher, 2006; Romano et al., 2017).

<sup>11</sup>The experimental literature on parochialism/universalism uses many standard games such as trust and public good games to study how social preferences differ when players belong to the same or different groups. Although the *giving* dictator game is also used frequently (see for instance Candelo, Eckel and Johnson 2018), to the best of our knowledge we are the first to employ the *taking* dictator game in an in-group vs. out-group framework.

<sup>12</sup>Philosophers have long justified property as just desert (Locke, 2015; Nozick, 1974) and, indeed, labor contributes one of the primary claims to ownership (Heller and Salzman, 2021).

167 notion of property, which is unrelated to desert.

168 The final channel that we consider zeroes in on the possibility that the reform had the  
169 effect of coordinating expectations. Property rights and, more generally, the law might  
170 have an *expressive function*, that is, they may make selected outcomes focal and hence  
171 coordinate expectations and actions towards those outcomes, irrespective of enforcement  
172 (Sunstein, 1996; Basu, 2018). To scrutinize this notion, we verified whether the reform  
173 affected coordination in a game characterized by multiple equilibria by employing a third  
174 auxiliary experiment. Following Jackson and Xing (2014), we tested the subjects' ability to  
175 coordinate in a modified battle-of-the-sexes game with an additional symmetric option.

176 In this game, players make a choice among three colors and earn a positive payoff only  
177 if they coordinate on the same one. The game has three Nash equilibria in pure strategy:  
178 coordinating on either one of the two colors which maximizes joint payoffs but favors one  
179 player at the expenses of the other (resulting in a 700 to 100 division), or on the third  
180 color which halves total earnings while resulting in equal individual payoffs (200 for both  
181 players). Participants played the game twice. In a “baseline” condition, no asymmetry was  
182 present. In a “prompt” condition, prior to making their choices, participants were exposed  
183 to a visual prompt of one of the colors which advantages one player and disfavors the other.  
184 Results show that, while in the baseline condition coordination is alike in treated and control  
185 villages, after the prompt is introduced participant were able to coordinate significantly more  
186 in treated villages. Further analysis reveals that the result is driven by the behavior of the  
187 subjects disadvantaged by the prompt, who responded by foregoing to choose their own  
188 individual payoff-maximizing action more often.

189 We suggest that these results reflects the expressive effect that the reform had on the  
190 ability of individuals to coordinate expectations (Hayek, 1973). In an environment where  
191 land used to be a common resource — “ours” — formal property rights introduce a notion  
192 of “mine” and, consequently, of “yours”. The reform may have had the effect of making  
193 reciprocal respect for the property of others a salient behavior, which is generally expected of  
194 individuals (Wilson, 2020). We will come back and elaborate on this point in the concluding  
195 section.

196 The remaining of this paper is organized as follows. In the next Section, we briefly  
197 summarize the paper's contributions to the existing literature. Section 3, illustrates the  
198 institutional framework of the Beninese reform. In Section 4, we provide the details of our  
199 lab-in-the-field experiments. Section 5 presents the results and in Section 6 we discuss possi-  
200 ble channels and report the results of several auxiliary experiments. Finally, Section 7 offers  
201 a discussion of our findings and ideas for future research.

202

## 2 Related Literature

Our paper contributes to four strands of literature. First, our results relate to the vast literature on property rights. The study of property in relation to other political and social institutions has engaged giants from Hobbes to Marx, from Hume to Rousseau, from Locke to Nozick, to name just a few (see Waldron 2013 for a review). In the last three decades, literature in economics has revived the role of property institutions in explaining economic development (North, 1981; Acemoglu and Johnson, 2005; Besley and Ghatak, 2012). In this literature, property rights are mainly viewed from a *vertical* perspective, as protection of individual endowments from expropriation by rulers or powerful elites; accordingly, property rights are regarded as endogenous to political institutions that, on one hand, should provide enforcement but, on the other hand, may prey on private property.<sup>13</sup> Yet, there is a more pervasive and endemic problem that property rights address: that of *horizontal* takings by similarly-situated individuals. In every legal system, democratic and despotic alike, most of property law deals with conflicts between the legitimate owner and an unlawful taker, which routinely originate in a host of very common situations ranging from boundary disputes to dealings with unfaithful intermediaries (Dari-Mattiacci and Guerriero, 2015, 2019). The literature (starting from Calabresi and Melamed, 1972) has mostly focused on deterrence of taking behaviour through criminal or tort liability, which are in turn forms of third-party enforcement. Instead, our paper focuses on the fact that the law may activate first-party enforcement.

Second, the fact that laws may affect behavior even without enforcement was noticed two millennia ago by Roman jurists (McGinn, 2001). More recently, this *expressive* effect of the law has been studied in the literature through two, possibly compatible, lenses (see Mc Adams 2015 for an overview). One theory posits that the process through which laws are enacted aggregates individuals' judgments and / or preferences and hence the law conveys information about the collective wisdom of a population as to which course of action is the most desirable. Individuals then respond rationally to such information by adjusting their behavior (Dharmapala and McAdams, 2003; Benabou and Tirole, 2011). An alternative theory postulates that the law helps aligning expectations as to others' behavior and hence can provide a focal point for individual actions (Cooter, 1998; Sunstein, 1999; McAdams, 2000; Basu, 2018). As Hayek (1973, p.579) observed: "The task of the rules of [property] can only thus be to tell people which expectations they can count and which not".<sup>14</sup> Our analysis relates to the latter version of the expressive function of the law and add to the empirical literature documenting an expressive function of the law in the lab (McAdams and

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<sup>13</sup>On the political economy of endogenous property rights see Diermeier, Egorov and Sonin (2013); Guerriero (2016).

<sup>14</sup>For further discussion of the expressive function of the law in philosophy, see (Finnis, 1989; van Den Burg, 2001).



237 Nadler, 2005) and in the field (Funk, 2007).

238 Third, our paper adds to the literature on how preferences and culture<sup>15</sup> can be *en-*  
239 *dogenously* determined by economic incentives and institutions (Frey, 1997; Bowles, 1998;  
240 Bar-Gill and Fershtman, 2005; Jha and Shayo, 2019; Margalit and Shayo, 2020; Bau, 2021).  
241 While there exists a vast experimental literature testing the effect of individuals' incentives  
242 on social preferences (Bowles and Polania-Reyes, 2012), the list of studies where public in-  
243 stitutions — such as property — are randomly manipulated is much shorter.<sup>16</sup> The closest  
244 paper to ours is the work of Di Tella, Galiani and Schargrotsky (2007), which exploited a  
245 quasi-random allocation of formal property titles to squatters in Buenos Aires and showed  
246 that, after few years, they displayed more pronounced pro-market beliefs than a control  
247 group. In this line of research, we are the first to provide lab-in-field evidence based on the  
248 RCT introduction of property institutions and study their effects on pro-social preferences  
249 towards strangers.

250 Finally, our results are also relevant for the literature linking modern-day cultural norms  
251 to institutions in place in a distant past (Henrich, 2015) — such as religion (Lang et al., 2019),  
252 marriage regulations (Henrich, Heine and Norenzayan, 2010; Schulz et al., 2019), education  
253 (Gradstein and Justman, 2002; Dixit, 2009; Lowes et al., 2017), and socialization patterns  
254 (Bisin and Verdier, 2000, 2001) — and pointing to culture as the channel of transmission  
255 between past institutions and current development patterns (Tabellini, 2008). In particular,  
256 we show that the introduction of property institutions activates a change in values which  
257 may foster relationships with strangers and, in turn, set a society on the path towards  
258 the further development of impersonal institutions (Greif and Tabellini, 2010). Related  
259 sociological studies (Yamagishi, Kikuchi and Kosugi, 1999) have suggested that interaction  
260 with strangers may teach individuals how to tell trustworthy from untrustworthy partners  
261 and hence further enhance one's ability to do business with strangers. In this line of research,  
262 an initial institutional shock sets off a chain of social and psychological changes along the  
263 path of impersonal trade.

### 264 **3 Institutional Framework**

265 In recent years, systems of formal land ownership registration have been introduced in vir-  
266 tually any African state. Nonetheless, customary land rights still represent the predominant  
267 land tenure arrangement in most rural areas of the African continent, including the Repub-

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<sup>15</sup>The word “culture” is used somewhat ambiguously in economics. In one interpretation, culture refers *individual values and preferences* (see for instance Akerlof and Kranton 2000; Tabellini 2008). In an alternative interpretation, culture refers to the beliefs or priors about the consequences of one's action (Benabou and Tirole, 2006; Guiso, Sapienza and Zingales, 2008). These two usages are not necessarily in contradiction with each other because beliefs, values and social preferences interact systemically (Tabellini, 2008).

<sup>16</sup>See the references discussed in footnote 7.

268 lic of Benin. Customary rights consist of a set of socially-determined land-use rules, where  
269 access to land is an integral part of the social structure, and tenure is determined by socio-  
270 political relationships. The governance and enforcement that characterize this system are  
271 implemented by customary authorities, who are entities legitimated by tradition or religious  
272 customs. The distribution of land rights is based on the local socio-political structure and  
273 land-related disputes are arbitrated by local authorities (Lavigne-Delville, 2006).

274 Population growth and the consequent increasing pressure on natural resources create se-  
275 rious concerns regarding the functioning and efficiency of informal customary arrangements.  
276 Scholars noticed that the absence of written documentation regarding land use and unclear  
277 parcel boundaries tends to give rise to more frequent conflicts over inheritance and land  
278 use (Deininger and Castagnini, 2006). Moreover, informality and the socially-determined  
279 nature of access to land fuel uncertainty about actual ownership rights, thus limiting the  
280 possibility to collateralize owned parcels, scale up land markets, and engage in impersonal  
281 trade (Arruñada, 2012).

282 In Benin, the policy response to problems caused by tenure insecurity has been a Torrens-  
283 type land titling reform known as the “Plan Foncier Rural” (PFR). The PFR implementation  
284 program, which received technical and financial support from the Millennium Challenge  
285 Corporation, was completed by the Beninese government in 2010-2011. The reform consisted  
286 of socio-land surveys at the village level to identify rights holders, their rights, and parcels  
287 boundaries. Following this initial step, implementation proceeded with land demarcation and  
288 the recording in public registries of land maps, which define rightholders and associated rights  
289 for each parcel of land. Registration affords rightholders a legal presumption of ownership,  
290 which, in turn, dramatically improves the likelihood of success in potential disputes before  
291 state courts. Given these characteristics, the PFR reform in Benin determined a major  
292 modification of the institution of property rights over land by transforming collective and  
293 socially-determined use-rights over land arbitrated by local authorities in formal property  
294 rights subject to legal protection.

295 For purposes of our empirical investigation, the key feature of the PFR titling endeavour  
296 is that the implementation followed a RCT process involving hundreds of rural villages. In  
297 fact, this is the first case of a large-scale land tenure reform implemented as a randomized  
298 control trial. In the preliminary phase of the project, interested rural villages were informed  
299 about the PFR and were invited to apply in order to participate in a lottery. As a second step,  
300 each application received was examined to verify whether the village met certain eligibility  
301 criteria, such as being effectively located in a rural area. Among the 576 villages that applied  
302 and were judged eligible, a subsample of 294 villages was randomly chosen via public lottery.  
303 Consequently, in 2010-2011, a team of local experts implemented the PFR in these selected  
304 villages (the “treated” group). The 282 non-selected villages (the “control” group) did not  
305 receive any intervention and, as of today, continue to have customary land rights. Figure 1

306 summarizes the PFR lottery mechanism. The map shows the areas selected for the lottery  
307 pool and, within these areas, the treated and control villages.

## 308 4 Research Design

### 309 4.1 Experimental Design and Hypothesis

310 The experimental design, hypotheses to be tested, and regression model specifications had  
311 been registered in a pre-analysis plan submitted to the American Economic Association’s  
312 RCT Registry before the data collection took place.<sup>17</sup> The research strategy makes use of  
313 the RCT implementation of the reform to compare values of respect for the property of  
314 anonymous strangers across treated and control villages.

315 To elicit respect for the property of others, we conducted a lab-in-the-field incentivized  
316 experiment in a sample of villages included in the lottery pool. The experiment consists of  
317 a modified dictator game in which the dictator can take (a part of) the endowment owned  
318 by a passive player. More specifically, as initial endowment the passive player owns 10  
319 tokens worth CFA 50 each (in total, approximately \$0,85). The dictator chooses whether  
320 taking some or all of the tokens owned by the passive player and transferring them to her  
321 account. Final earnings are determined by the amount of tokens possessed by each of the two  
322 players. Participants are informed that we adopt a role-reversal protocol. At the beginning  
323 of the experiment, participants do not know which role is assigned to them. Instead, all  
324 participants in our sample state their decisions as if they were playing the game in the  
325 role of dictator. However, only half of the participants are actually assigned to the role  
326 of dictators, whose choices determine both the dictator-own payoffs and the payoffs of the  
327 matched passive participant. The taking decision stated by participants who are assigned to  
328 the role of passive players instead has no consequences on payoffs.

329 The main objective of the study is to test whether the land rights reform affected the  
330 willingness to respect the property rights of an anonymous stranger who is not part of  
331 the reference group to which the decision-maker belongs. Following previous research, we  
332 identified the village community as the relevant reference group for our participants (Bulte  
333 et al., 2017). Accordingly, in the experiment, each participant took two decisions in the  
334 role of dictator. In one case, the paired passive player belonged to the same village as  
335 the dictator (“local” condition), and in the other case the passive player belonged to a  
336 different rural village in Benin (“stranger” condition). The former decision is used as a  
337 benchmark to test whether the reform affected participants’ “universalism”, namely the

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<sup>17</sup>The unique identification number of the main experiment is AEARCTR-0005322. The pre-analysis plans concerning the two auxiliary experiments on honesty and coordination were pre-registered at the AEA RCT Registry at the same time of the main experiment (IDs AEARCTR-0005324 and AEARCTR-0005319, respectively)

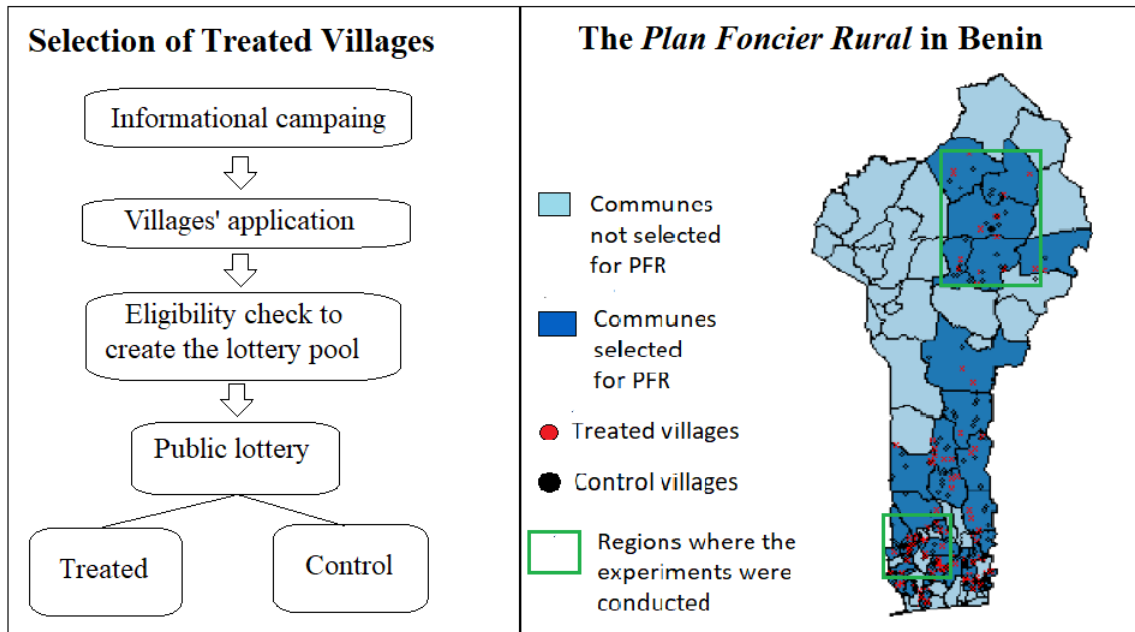


Figure 1: *Left panel:* The lottery mechanism used to select villages where the reform was implemented. *Right panel:* The distribution of treated and control villages.

338 extent to which individuals’ prosocial behavior remains constant as social distance increases  
 339 (Enke, Rodríguez-Padilla and Zimmermann, 2020). To control for possible order and moral  
 340 edging effects, half of the dictators played the locals condition first, while the remaining half  
 341 played the strangers condition first.

342 In addition to our main analysis, we implemented also a treatment variation that concerns  
 343 the way in which the passive player acquires its initial endowment. In the “Luck” treatment,  
 344 the endowment of the passive player comes as windfall money. Participants are informed  
 345 that they received an endowment equal to 10 tokens from the experimenters. In the “Merit”  
 346 treatment, players have to complete an effort task in order to acquire the endowment.<sup>18</sup> This  
 347 treatment variation follows a between subject design so each participant only takes part in  
 348 either the Luck or the Merit treatment. In addition to these treatment variations, individuals  
 349 played several auxiliary games, which are described below.

350 Our main hypothesis concerns the effects of formalizing land rights on the respect that  
 351 participants display for the property rights of out-group strangers not belonging to the same  
 352 village community. We test whether the dictator’s taking rate when the partner is from a  
 353 different village is equal in the treated and control groups. These hypothesis will be tested  
 354 by estimating the following regression equation:

<sup>18</sup>In the effort task, each participant receives a plastic box and 200 toothpicks. The plastic box has a little hole on top. The participant has ten minutes to slide all the 200 toothpicks inside the box from the top hole in order to receive the 10 tokens. If a participant does not complete the task within the time limit, she does not receive any endowment. Out of the 288 participants who performed the effort task, three did not manage to successfully complete it. In Appendix B we included an English translation of the instructions given to the participants in both the Luck and the Merit treatments.

$$t_i = \alpha + \alpha_F F_i + \delta_T T_i + \delta_{FT} F_i T_i + \mathbf{X}_i + \epsilon_i \quad (1)$$

355 where  $t_i$  is the taking decision made by the dictator,  $F_i$  is a dummy equal to one when  
 356 the subject takes decisions in the interaction with individuals belonging to the same village,  
 357  $T_i$  is a dummy equal to 1 for subjects in treated villages, and  $X_i$  is a vector of the individual  
 358 characteristics specified in the post-experimental survey.

359 As specified in the pre-analysis plan, we also investigate possible heterogeneities in dicta-  
 360 tors' taking rate for same-village and different-village interactions by using data on distance  
 361 from paved roads, gender, income and wealth. Moreover, we study whether varying the  
 362 processes through which the passive player acquires the initial endowment affects dictators'  
 363 taking rate. Specifically, we test whether acquiring property by means of luck or by merit  
 364 is an important determinant for our research hypothesis. Furthermore, we verify the effects  
 365 of the reform on universalism by comparing across treatments the difference between dicta-  
 366 tors' taking rates in the out-group and in-group conditions. Finally, we investigate possible  
 367 mechanisms by using the evidence coming from three auxiliary experiments that have been  
 368 made with the same subjects during the same experimental campaign.

## 369 4.2 Fieldwork Procedures

370 The data collection took place between January and March 2020. The procedure to collect  
 371 data worked as follows. We randomly selected the villages where the data collection took  
 372 place from the whole list of villages included in the Beninese PFR that are located in two  
 373 provinces in the south of the country (Mono and Couffou) and in two provinces in the north  
 374 (Alibori and Borgou). In the days before the session, a research assistant visited the selected  
 375 village and requested as many volunteers as possible to gather on a scheduled day in a  
 376 specified location in order to take part in a research project. Participants had to be resident  
 377 in the village and older than 18 years old; and only one participant per household could take  
 378 part in the study. The day of the experiment, the research team randomly selected nine male  
 379 and nine female participants to take part in the experiment among those who answered the  
 380 call. Participants who were not selected were paid a show-up fee of CFA 500 (\$ 0,85) and  
 381 requested to leave. We run 32 fieldwork sessions, each in a different village (16 treated); a  
 382 total of 576 individual households took part in the experiment.<sup>19</sup>

383 During the sessions, the participants convened in a common space — usually a school  
 384 classroom or a public building — and the experimenter read the experimental instructions  
 385 aloud. Then each participants was individually called into a separate room where he or she  
 386 could privately make his or her taking decision as dictator. To limit possible experimenter

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<sup>19</sup>One participant felt unwell during a session and had to leave the session before having completed the experimental choices. Therefore, we actually collected observations from 575 participants.

387 effects, we adopted a procedure that makes the dictators’ taking choices blind to the ex-  
388 perimenter on site.<sup>20</sup> When each participant entered the decision room and before being  
389 left alone to make the taking decision, the experimenter asked him or her control questions  
390 to verify the correct comprehension of the game instructions. In case a participant could  
391 not answer the control questions, the experimenter repeated the instructions in private until  
392 the participants was able to provide the correct answers. In addition to the experimental  
393 tasks described in this paper, participants took part in an incentivized risk elicitation task,  
394 a socio-demographic survey, and additional fieldwork activities not related to this project.  
395 A fieldwork session lasted approximately three hours. Participants earned on average CFA  
396 2800 (\$4,5), roughly the equivalent of the wage earned in one and a half days of work for  
397 the median subject in our sample.

## 398 **5 Results**

### 399 **5.1 Preliminary Analysis**

#### 400 **Sample Balance and Potential Confounding Factors**

401 Our research design is based on comparing the dictators’ taking rate across villages that,  
402 ten years before the experiment, had been randomly selected to have the land tenure reform  
403 implemented against non-selected villages, which maintain customary land rights to date.  
404 In order for this identification strategy to hold, two caveats are in order. First, we need to  
405 show that the random allocation to different property institutions characterizing the PFR  
406 lottery was successful in eliminating pre-reform differences across treatment branches and  
407 that our selection of participants resulted in a balanced sample.

408 With respect to the RCT implementation of the reform across Beninese villages, a thor-  
409 ough impact evaluation of the reform carried out by the World Bank’s Gender Innovation Lab  
410 reports evidence that the randomization determined by the lottery was successful (Omondi,  
411 2019). In particular, the World Bank team made use of both a rich set of pre- and post-  
412 treatment survey data collected by a national agency, as well as administrative monitoring  
413 and evaluation data independently collected by the Millennium Challenge Corporation—  
414 Benin. The impact evaluation, resulting from a cross-evaluation performed by using these  
415 independently-collected data sources, shows pre-intervention balance on outcome variables  
416 between treatment groups and dispels residual concerns regarding the randomization imple-  
417 mented by lottery (Goldstein et al., 2016; Omondi, 2019).

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<sup>20</sup>In the case of the main experiment, the experimenter left the participant alone in the decision room. The participant found two envelopes of different colors marked by a code: an empty “Own” envelope and a “Paired Participant” envelope containing 10 tokens. The participant was instructed that he or she could physically transfer tokens from the partner’s envelope to his or her envelope to determine the final payoff and to seal both envelopes before leaving the room.

418 Concerning our sample of participants, we collected data from residents of 32 villages  
419 randomly selected among those in the RCT pool. In Table A1 in Appendix A, we report  
420 descriptive statistics relative to the pre-registered socio-demographic characteristics that we  
421 collected from the subjects who took part in the experiment. While the sample is well bal-  
422 anced for most of the observables, participants in the treated group are on average older,  
423 slightly more likely to be married, and show a marginally significantly higher literacy rate  
424 than those in the control group. To account for these imbalances, in the analysis we con-  
425 trol for these characteristics. Moreover, as explained in details when discussing our main  
426 results, as a robustness check we also employ a Lasso post-double-selection methodology for  
427 appropriately selecting the controls to be included in the regression (Belloni, Chernozhukov  
428 and Hansen, 2014). This method has been proved useful to improve the robustness of causal  
429 inference when accidental imbalances in the sample occurs (Chernozhukov et al., 2018).

430 Second, we need to verify that, after the reform implementation, participants did not  
431 self-select in one of the treatment branches through migration. To do so, we collected data  
432 regarding the participants' villages of origin, whether they migrated, the reason for it, and  
433 the number of years of residence in the village. Only 35 out of 576 participants were not  
434 already resident in the village when the PFR reform was implemented, 20 in treated villages  
435 and 15 in control. The difference is not statistically significant ( $\chi^2$  test,  $p > 10\%$ ). The  
436 majority of these migrations were reported by female participants, and the stated reason  
437 was marriage in over 90% of the cases. Similarly, we verified that the number of years that  
438 subjects spent in the village where they participated in the experiment is not statistically  
439 different between participants in treated and control villages. Similarly, we verified that the  
440 fraction of participants who were actually born in the village in which they participated in  
441 the experiment does not differ in treated and control villages. Moreover, while we do not have  
442 data concerning out-flow migrations, we see that in our sample of villages the population  
443 size is not statistically different between treated and control (2,934 vs. 2,748 respectively,  
444  $p = .85$  two-sided t test). In the regression analysis reported below, we insert a dummy for  
445 participants who moved to a village different from the one in which they were born, and we  
446 control for the number of years each subject had lived in the village where the data collection  
447 took place.

## 448 **Taking from Locals**

449 As a preliminary step, we estimate the effects that the reform had on the respect for the  
450 property among locals. This exercise replicates in a different set of villages and with a  
451 larger sample size the results of Fabbri and Dari-Mattiacci (2020). In addition, we vary the  
452 source of the passive players' endowment. As shown graphically in Figure A1 in Appendix  
453 A, participants in control villages took on average 3.76 tokens against 3.33 tokens taken by  
454 participants in treated villages. The difference is statistically significant at the conventional

455 level (t-test two-sided,  $p=.02$ ) and it becomes strongly significant if we refine the sample to  
456 include only those participants who had first-hand experience with the reform (see Section  
457 5.2 for details on how this refined sample is constructed). Results from the regression analysis  
458 reported in Table A2 — in which we control for pre-registered individual observables, village-  
459 level characteristics, and additional socio-demographic controls — confirm this finding.

460 **Result 1** *Replication of Fabbri and Dari-Mattiacci (2020): Formal property rights de-*  
461 *crease takings from anonymous individuals from one's own village community (locals).*

## 462 Parochialism in Taking Decisions

463 We also check whether dictators in our experiment display in-group favoritism — favour-  
464 ing locals over strangers — in respecting the property of others, without for the moment  
465 distinguishing between participants in treated and control villages. As shown in Figure A2  
466 in Appendix A, dictators take significantly less tokens when the anonymous paired partici-  
467 pant is a fellow villagers (mean = 3.54) than otherwise (mean = 4.66). A two-sided t-test  
468 rejects the hypothesis that there is no difference between the two sample means at the 1%  
469 level. Models 1-3 in Table A3 in Appendix A — in which we regress the number of tokens  
470 taken from the passive player adding pre-registered individual, village-level, and additional  
471 socio-demographic controls, respectively — confirm the result. This finding suggests that  
472 participants in our sample display the common tendency in humans to favor, all things being  
473 equal, locals as compared to strangers, very much in line with previous literature (Bernhard,  
474 Fehr and Fischbacher, 2006; Romano et al., 2017).

## 475 5.2 Main Result: Taking from Strangers

476 We now move to test our main hypothesis which concerns the impact of the PFR reform  
477 on the respect for others' property rights when the dictator is paired with an anonymous  
478 stranger from a different village. We begin by looking at Figure 2 that shows the average  
479 amount of tokens taken by dictators in control and treated villages when interacting with  
480 strangers. Dictators in the control group took on average 4.95 tokens from the passive players  
481 against the 4.36 taken in the treated group. The difference is statistically significant at the  
482 1% level (t-test two-sided,  $p<1\%$ ).

483 We then proceed with testing the hypothesis in a regression framework. The number of  
484 tokens taken by the dictator are regressed on the dummy *local* equal to 1 when interacting  
485 with a local — that is, a receiver from the same village — the treatment dummy, the interac-  
486 tion of these two variables, and a pre-registered set of individual controls.<sup>21</sup> Coefficients are

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<sup>21</sup>The pre-registered individual controls include: gender, religion, marital status, number of family mem-  
bers, participation to household finance management, education, literacy, village of birth, years of residence  
in the village, incentivized measures of risk preferences, and three proxies for individual wealth: the log of



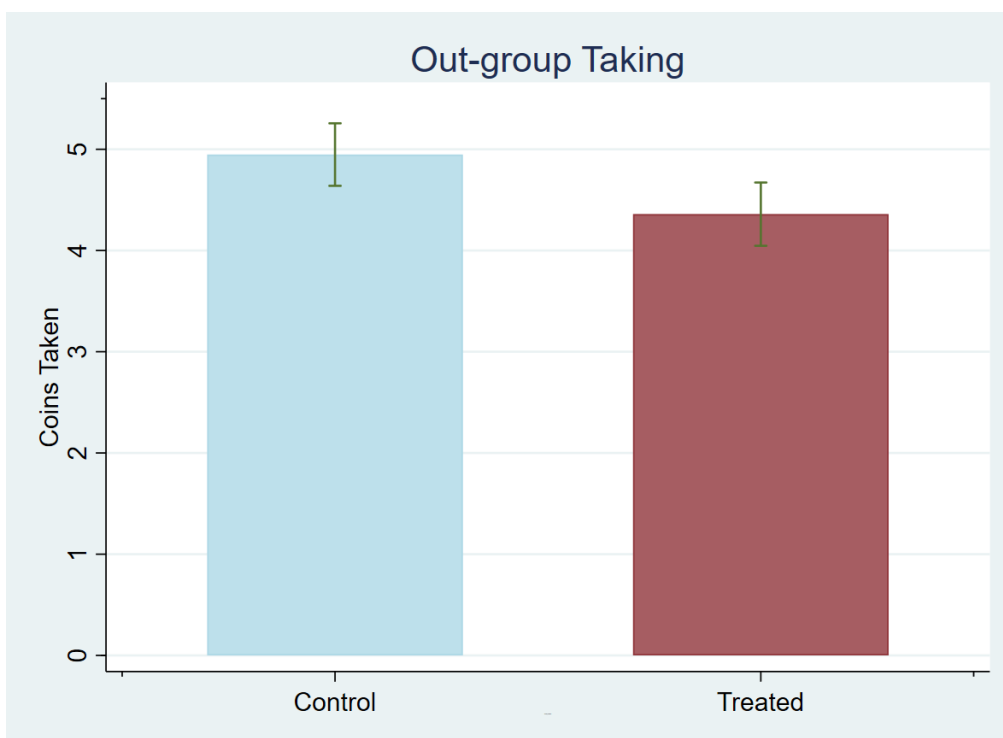


Figure 2: Tokens Taken from a Participant living in a Different Village

487 estimated by using a random-effect generalized least square (GLS) estimator, and standard  
 488 errors are clustered at the village level. Model 1 in Table 1 reports the results. The main  
 489 coefficient of interest is that of the treatment dummy, which isolates the effect of PFR on  
 490 dictators' taking rate when the passive player is a stranger — that is, comes from a differ-  
 491 ent village. The coefficient is negative and statistically significant at the conventional level,  
 492 suggesting that experiencing the reform significantly increases the respect for the property  
 493 of participants from other villages. In Model 2 we add to the previous specification a set of  
 494 village-level controls. The negative point estimate of the treatment dummy increases and the  
 495 coefficient becomes statistically significant at 1% level. The results are confirmed in Model  
 496 3, in which we additionally include a set of proxies for individual wealth.

497 In models 4-6 we verify whether the results are driven by participants who have first-  
 498 hand experience with the reform. We use post-experimental survey data to exclude from the  
 499 analysis participants from treated villages who do not actually own a parcel of land affected  
 500 by the PFR reform ( $n=82$ ), as well as participants belonging to control villages who own

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self-reported measure of weekly household income, the number of bedrooms in the house, and whether the household has running water. In some model specifications presented, we also add village characteristics and additional wealth controls. Village-level controls include: village population, whether the village has a market within its boundaries, distance from the closest public school, distance from the closest public hospital. Additional wealth controls include: acres of land possessed individually, whether the house has concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card.

Table 1: Tokens Taken by the Dictator — In-group and Out-group Interactions

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:	Whole			Refined		
treated	-0.713** (0.302)	-0.791*** (0.306)	-0.822*** (0.300)	-1.044*** (0.343)	-1.152*** (0.347)	-1.201*** (0.336)
local	-1.212*** (0.183)	-1.212*** (0.183)	-1.212*** (0.184)	-1.202*** (0.204)	-1.202*** (0.204)	-1.202*** (0.205)
treated× local	0.192 (0.227)	0.192 (0.228)	0.192 (0.228)	0.284 (0.236)	0.284 (0.236)	0.284 (0.237)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	1.699** (0.682)	1.701** (0.802)	1.669** (0.806)	1.381* (0.796)	1.342 (0.952)	1.568 (0.992)
N.obs.	1150	1150	1150	912	912	912

**Notes:** Dependent variable: tokens taken by the dictator. GLS random-effects estimators. Standard errors robust for clustering at the session level. Compared to Models 1 and 3, Model 2 and 4 include village-level controls; compared to Models 2 and 4, Models 3 and 6 additionally include a set of proxies for individual wealth. Individual Controls include: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, income, whether the household has running water, number of bedrooms; Village-level Controls include: village population, whether the village has a market and market distance, distance from the closest public school, distance from the closest public hospital; Additional Wealth Controls include: acres of land possessed individually, whether the house has concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card. Symbols \*\*\*, \*\*, and \* indicate significance at the 1%, 5% and 10% level, respectively.

501 at least a parcel of land for which they hold a formal title (n=37).<sup>22</sup> Models 4-6 in Table 1  
502 replicate Models 1-3 with this refined sample of participants. The coefficient of the treatment  
503 dummy is negative and significant at the 1% level in all model specifications and the point  
504 estimates become larger.

505 The results of a pre-registered heterogeneity analysis also suggest that the reduction in  
506 taking rate observed in the treated group is driven by those individuals who benefited the  
507 most from the reform. By awarding formal property titles, the reform also allows rightholders  
508 to enforce their land rights in state courts, a possibility that in the customary system was  
509 precluded by the lack of formal proof of land ownership. A post-experimental survey reveals  
510 that the vast majority (over 90%) of our participants considers the ruling of state courts as  
511 conclusive and superior to that of local customary authorities. However, participants also

<sup>22</sup>A resident in a treated village might have not be directly interested by the reform because, for instance, she has no customary user-rights over land at all or because she has customary rights over land parcels which are located outside of the administrative boundaries of the village and so not included in the PFR. Similarly, residents in control villages might have requested a land title through the standard procedure offered to Beninese citizens (thus independently of the PFR reform) or they might have customary rights over land parcels located in a village where the PFR reform took place.

512 report relatively high costs of access to state courts, with the average expected cost of solving  
513 a case in a state tribunal that equals several months of income for the median subject in our  
514 sample (CFA 716,000). These costs are further inflated for those participants who live in  
515 remote areas characterized by the absence of paved roads connecting with the tribunals. In  
516 our sample, subjects living at a larger distance from paved roads than the sample median  
517 report a roughly threefold increase in the expected costs of a lawsuit compared to those  
518 living in the proximity of paved roads (CFA 1,233,000 vs 382,000 respectively). Indeed,  
519 41% of participants living in the proximity of a paved road are aware of at least one person  
520 who solved a land-related conflict by initiating a formal legal procedure in a state tribunal,  
521 against a mere 9% among those living far away from paved roads. *De facto*, the reform has  
522 most likely had a negligible impact on the land tenure of individuals who face financial and  
523 logistical constraints when accessing the formal justice system as compared to the previous  
524 customary system. We thus expect milder effects of the reform on the behavior for these  
525 subjects as compared to those who can easily access justice. We verify this conjecture by  
526 comparing the taking decisions of different subgroups of participants who have different  
527 possibilities to access the justice system.

528 We divide the sample of participants according to whether they have a level of self-  
529 reported income above or below the sample median (“high” and “low”, respectively). We  
530 then compare separately the dictators’ taking decisions of subjects in the high-income and  
531 low-income subgroups across treatments (notice that, within each of the the high- and low-  
532 income subgroups, we are comparing participants who have roughly identical average and  
533 median income in treated and control). Results of a two-sided t-test are summarized in Table  
534 2. Participants in the high-income subgroups who belong to treated villages took significantly  
535 less (at the 1% level) from strangers than those in control villages. Conversely, the difference  
536 is not statistically significant if we focus on participants in the low-income subgroup. The  
537 same results hold if we characterize participants’ affluence by using a composite wealth  
538 index of fourteen proxies for individual wealth.<sup>23</sup> Finally, we repeat the analysis by grouping  
539 participants according to the distance of their residency to the closest paved road. The results  
540 display a similar pattern, with a significant reduction in dictators’ taking rate only observed  
541 among subjects living in the proximity of roads. In Tables A4 and A5 in Appendix A, we  
542 show that these results are confirmed when the main model specification is re-estimated by  
543 dividing subjects according to income and distance from paved roads, respectively. This  
544 evidence further increases confidence in the fact that the estimated reduction in dictators’

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<sup>23</sup>Each of the fourteen proxy takes value  $\{0;1\}$ , so that the wealth index  $\in \{0;14\}$ . The proxies for individual wealth used are: whether the acres of land possessed individually are above the sample median, whether high-income, whether the number of bedrooms in the house are above the sample median, whether the self-reported socio-economic rank is above the sample median, whether more than half of the calories consumed are purchased in the market, whether the house has a concrete floor, electricity, a radio or television, or running water, whether within the household somebody owns a motorbike, a car, a bank account, or a credit card.

Table 2: Tokens Taken by the Dictator in Strangers Interactions — Heterogeneity Analysis

Sample:	Whole			Refined		
	Treated	Control	p-value	Treated	Control	p-value
High-Income	4.51	5.52	<.01	4.43	5.59	<.01
Low-Income	4.53	4.23	.30	3.89	4.60	.03
High-Wealth	4.62	5.54	<.01	4.42	5.63	<.01
Low-Wealth	4.09	4.46	.24	3.81	4.48	.06
High-Road-Dist	5.04	5.16	.73	4.88	5.20	.40
Low-Road-Dist	4.05	4.63	.08	3.85	4.70	.02

**Notes:** Treatment effects across income, wealth, and distance from paved roads. For each of the three variables, we separate between participants higher or lower than the sample median. The wealth analysis is based on an individual wealth index  $\in \{0;14\}$ . The proxies for individual wealth used are: whether the acres of land possessed individually are above the sample median, whether high-income, whether the number of bedrooms in the house are above the sample median, whether the self-reported socio-economic rank is above the sample median, whether more than half of the calories consumed are purchased in the market, whether the house has concrete floor, electricity, radio or television, running water, whether within the household somebody owns a motorbike, a car, a bank account or a credit card. The p-value columns report results of a two-sided t test.

545 taking rate in the treated group is linked to a direct experience with the reform.<sup>24</sup>

546 We then perform a series robustness checks. First, for three villages in the treated sample  
547 the village authorities reported to have further extended the original PFR intervention after  
548 its 2011 implementation by including also some land plots lying outside the official village  
549 borders.<sup>25</sup> In Table A7 in Appendix A, we re-estimated the model specifications reported in  
550 Table 1 by excluding these three villages from the sample. The qualitative results and point  
551 estimates remain similar to those reported for the basic specification.

552 Second, in low- and medium-income countries self-reported income might be a poor  
553 indicator of individual affluence (Arrow et al., 2012; Moser and Felton, 2007). In Table A8  
554 in Appendix A, we verify whether our estimates are sensitive to the way in which participants’  
555 wealth is measured. Accordingly, we re-estimate our main model specifications by including  
556 different combinations of proxies for wealth. Results remain quantitatively very similar and  
557 qualitatively unchanged.

558 Finally, as discussed in Section 5.1, one potential problem with our empirical strategy is

<sup>24</sup>In Table A6 in Appendix A, we also performed the pre-registered heterogeneity analysis on gender. This analysis is motivated by the evidence reported by Goldstein et al. (2018) that the reform increased tenure security in particular for women. In line with this evidence, we find that the negative point estimate for the treated dummy is somehow larger for women participants, albeit the reduction in taking is not statistically different across genders.

<sup>25</sup>In the PFR intervention completed in 2011, only land plots within the administrative village boundaries were subject to the land demarcation and use-rights formalization. Because of this specific feature of the intervention, some villagers were induced to limit long-term investments practices in the now-secured registered parcels and, at the same time, to shift unproductive continuous land-use activities finalized to reduce expropriation risks to unregistered parcels outside the village boundaries (Goldstein et al., 2018).

559 that participants in treated and control lack balance for some individual characteristics (age,  
560 marital status, and marginally literacy). In all model specifications of the analysis presented  
561 above, we controlled for these observables. We additionally address possible concerns deriv-  
562 ing from this imbalance in two ways. As a first step, we show that none of the unbalanced  
563 variables is associated to taking rate. To do so, we show that average taking is statistically  
564 the same between participants older or younger than the sample median (4.11 vs. 4.10,  
565 respectively; two-sided t-test  $p=.98$ ), married or not married (4.10 vs. 4.17, respectively;  
566 two-sided t-test  $p=.73$ ), and literate or illiterate (4.06 vs. 4.13, respectively; two-sided t-test  
567  $p=.62$ ).

568 As a second step, we re-estimate the models presented in Table 1 employing the Lasso  
569 post-double-selection approach proposed by Belloni, Chernozhukov and Hansen (2014).<sup>26</sup>  
570 This methodology has been proved useful to select in a principled way the controls to be  
571 included in a regression when accidental imbalances in the sample occurs (Belloni et al.,  
572 2017; Chernozhukov et al., 2018). Table A9 in Appendix A reports the results, separating  
573 the effects on dictators’ decision to take tokens from a stranger (models 1 and 2) and from  
574 a local (models 3 and 4). The qualitative results remain the same, and point estimates are  
575 very similar, to those of the main model specification.

576 We can summarize the evidence concerning our main hypothesis as follows:

577 **Result 2** *Formal property rights decrease takings from anonymous individuals from out-*  
578 *side one’s own village community (strangers).*

## 579 6 How Property Affects Values

580 What are the determinants of the reduction in takings from strangers induced by the intro-  
581 duction of formal property rights? In this section we present, discuss and, for the most part,  
582 discard a number of explanations that our empirical strategy was designed to probe. We  
583 begin with a more nuanced analysis of some of the individual and village characteristics that  
584 have been already discussed in the previous section in order to examine whether the reform  
585 affected values through its impact on the socio-economic context. Next, we consider the  
586 reform’s impact on moral values and social preferences, on individuals’ fundamental “prop-  
587 erty instincts” as to whether labor supports a legitimate claim to ownership, and, finally, on  
588 making property rights more *expressive* by coordinating expectations.

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<sup>26</sup>We additionally replicated the results discussed here by using the Lasso post-regularization methodology proposed by Chernozhukov, Hansen and Spindler (2015) and developed as STATA package by Ahrens, Hansen and Schaffer (2018). Results are virtually identical.

## 6.1 Socio-economic context

First, we investigate whether the PFR affected investments in human capital. The reason to do so is rooted into previous research, suggesting that Argentinian squatters who were granted land tenure increased investments in their offspring's education (Galiani and Scharrotsky, 2010). Had the Beninese reform resulted in the same increase in education, this might have determined a cultural change toward the idea of (respect for) property in participants from treated villages. To be sure, this mechanism would be relevant with respect to takings from strangers as well as from locals. In our sample, human capital investments are very limited, with only 36% of the participants with basic literacy skills. On average, participants went to school for one year, with negligible differences between treated and control groups. As discussed in section 5.1, in our sample neither education levels nor literacy rate are associated with the dictators' taking rate. Models 1 and 2 of Table A12 in Appendix A replicate the main regression presented in Table 1 by excluding education and literacy rate as controls. Moreover, repeating the estimation by implementing a Lasso post-double-selection approach in which education years and literacy are included in the high-dimensional individual controls does not affect the results, as shown in Table A9 in Appendix A. The results and point estimates remain virtually unchanged, suggesting that human capital investments do not play a relevant role here.

Second, the PFR was introduced to enhance investments and increase individual wealth. Had the reform achieved its goals by the time we ran our experiments, then higher levels of wealth in treated villages might themselves explain lower level of takings (for instance, because richer people are less in need of stealing). We thus verify whether participants in treated villages had experienced an increase in wealth or credit access that might have mediated the lower taking rate. Participants' self-reported income levels as well as any of the others fourteen indicators of wealth collected are statistically the same in treated and control villages. These results are consistent with previous evidence on the short and medium term impact of the reform on income levels (Fabbri and Dari-Mattiacci, 2020; Goldstein et al., 2018). Moreover, in models 3 and 4 of Table A12 in Appendix A, we re-estimate the main regression of Table 1 by excluding income and proxies for wealth. Results remain qualitatively the same and point estimates remain similar to those of the main model specification. These results suggest that variations in income or wealth are unlikely to explain the observed reduction in taking.

Finally, we verify whether the increase in respect for others' property displayed by villagers in the treated sample could be explained by a change in the rate of conflicts over land determined by the reform. Indeed, individuals with a less conflictual history might be less inclined to take hostile actions against other individuals. In Table A13 in Appendix A, we re-estimate the main model specifications by additionally controlling for the number of self-reported conflicts experienced by participants in the previous ten years. The results

627 remain virtually unchanged, suggesting no mediating effects of conflicts on the observed tak-  
628 ing behavior. Taken together, these three pieces of evidence suggest that the impact of the  
629 reform on respect for the property of strangers is not mediated by relevant features of the  
630 socio-economic context.

## 631 **6.2 Moral Values: Universalism, Honesty and Altruism**

632 We investigate how our main result on taking from strangers (Result 2) relates to taking  
633 from locals (Fabbri and Dari-Mattiacci 2020, as confirmed in Result 1). The reduction of  
634 takings observed in both groups may reveal an increase in universalism, namely the extent  
635 to which people’s prosocial behavior remains constant as social distance increases (Enke,  
636 Rodríguez-Padilla and Zimmermann, 2020). To test this hypothesis, we compare dictators’  
637 taking rates across treatment groups in the strangers and locals conditions. We generate the  
638 variable *difftaking*, which is equal to the amount of tokens taken from strangers minus the  
639 amount of tokens taken from locals. While the difference in taking rates is slightly smaller  
640 among participants in treated villages (1.02) compared to those in control (1.19), a two-sided  
641 t-test cannot reject the hypothesis that *difftaking* is the same in the two samples.

642 The analysis then replicates the regressions presented in models 1-3 of Table 1 with  
643 *difftaking* as dependent variable. Results are reported in Table A10 in Appendix A. In all  
644 models specifications, the coefficient of the treatment dummy is not statistically different  
645 from zero. This evidence confirms that the reduction in taking rate generated by the reform  
646 had a similar magnitude with locals as with strangers.<sup>27</sup>

647 Next, we check whether the observed reduction in taking reflects a broader change in  
648 moral values caused by the reform. To study whether this is the case, we ran two auxiliary  
649 experiments which have been widely used to measure social preferences and moral behaviour.  
650 First we study whether the observed reduction in taking results from the reform’s influence  
651 on individuals’ moral attitudes towards cheating (Abeler, Nosenzo and Raymond, 2019).  
652 To measure honesty, we followed Jiang (2013) and had participants take part in a variant  
653 of the dice-rolling task introduced by Fischbacher and Föllmi-Heusi (2013). In this task,  
654 subjects are asked to privately throw a six-face dice 10 times and then report the outcomes  
655 to the experimenter. Subjects are paid linearly in the outcome of one randomly chosen  
656 roll, CFA 100 if the outcome is 1; 200 if 2; and so on up to the maximum payment of 600  
657 if 6 is reported. Since the experimenter does not observe the outcome of the dice rolls, a

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<sup>27</sup>Indeed, the reduction in taking is confirmed when we estimate the effect of formal property rights on taking rates irrespective of whether the passive player is a local or a stranger. When comparing the total amount of tokens taken in the two decisions across treatment groups, participants who experienced the reform took 13% less, on average, than those in control villages — as shown in Figure A3 in Appendix A. The difference is statistically significant at the 1% level (two-sided t-test). An OLS regression analysis reported in models 4-6 of Table A3 in Appendix A, in which we additionally control for individual and village-level characteristics, confirms the result.

658 participant can inflate his or her payoff by over-reporting. However, deviations from the  
659 statistically-predicted mean outcome — both at the individual and at the group level — can  
660 be interpreted as a signal of dishonesty.

661 Table A14 in Appendix A shows the results.<sup>28</sup> Consistent with what has been observed in  
662 other dice-rolling experiments, participants in our sample inflate their payoff by significantly  
663 over-reporting the outcome of their rolls as compared to the statistically-predicted mean  
664 of 3.5 (average outcome reported 3.85, two-sided t-test  $p < 1\%$ ). However, we detect no  
665 significant difference in the average reported outcome between the treated and control villages  
666 (3.83 vs. 3.88, respectively, two-sided t-test  $p = .53$ ).

667 Second, we verify whether the reduction in dictators' taking for the treated sample reflects  
668 a more general increase in altruism toward strangers. To this end, participants took part  
669 in an incentivized standard dictator game framed as a donation to an unspecified charity  
670 that, as we emphasized in the instructions, is located out of the village.<sup>29</sup> As graphically  
671 displayed in Figure A4 in Appendix A, the average donation rate for treated and control  
672 participants is very similar (3.66 in treated vs. 3.70 in control). A two-sided t-test cannot  
673 reject the hypothesis that the mean donation is the same across treatment groups. A similar  
674 result comes from our post-experimental survey where we asked our participants whether  
675 they would support an hypothetical redistribution of land from more wealthy individuals to  
676 those in need.<sup>30</sup> The share of individuals who supported the redistribution was very similar  
677 in the treated and control groups (22% vs. 18% respectively,  $p = .21$ ).

678 Taken together these pieces of evidence suggest that the decrease in taking from strangers  
679 is not mediated by changes in values of universalism, honesty and altruism but it rather  
680 reflects a specific effect of property rights on taking aversion.

### 681 **6.3 Labor as a Claim to Ownership**

682 It has been argued that one of the strongest behavioural mechanisms at the root of property is  
683 its intimate connection with individual's labor and just desert (Nozick, 1974; Locke, 2015).<sup>31</sup>

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<sup>28</sup>We collected data relative to exactly ten dice rolls from 447 subjects. The missing subjects were either not reporting in the outcome-sheet each of the required ten outcomes from the dice rolls, providing inconsistent or ambiguous reporting, or refusing to take part in this experimental task (apparently due to some religious or social stigma toward dice gambling in some communities).

<sup>29</sup>All dictators' offers have been eventually donated to an orphanage in Cotonou.

<sup>30</sup>The question stated: "Imagine in the village that somebody gets rich and owns more land than what he and his family need. Do you think the others village members should force him to give part of his land to poor families who need it?". The possible answer was binary.

<sup>31</sup>A growing body of literature provides evidence of the deep behavioural root of property-like behaviour in both animals (Sherratt and Mesterton-Gibbons, 2015) and humans (Zeki, Goodenough and Stake, 2004; Wilson, 2020; Fabbri, Rizzolli and Maruotti, 2021; Heller and Salzman, 2021). In institution-free environments, such as the animal world or the economic lab, subjects playing Hawk and Dove games display aggressive behaviour when they are possessors and they refrain from taking when they are intruders. The latter behaviour is a close analogue to the behavior exhibited by our subjects when refraining from taking from strangers in our main experiment. Zeki, Goodenough and Stake (2004) call it the "property instinct" while Eswaran and



684 Indeed, experimental evidence shows that, when subjects gain their endowment through  
685 an effort task, dictators playing a taking game are less likely to take (List, 2007; Jakiela,  
686 2011; Korenok, Millner and Razzolini, 2018; Faillo, Rizzolli and Tontrup, 2019). A plausible  
687 hypothesis to explain the reduction of taking displayed by our participants after reform is that  
688 the formalization of property rights *justifies* ownership as legitimate. For instance, awarding  
689 formal property can induce individuals to assume that if somebody owns something, he or  
690 she must have deserved it, possibly because he or she worked to obtain it.

691 We investigate this point by dispelling doubts regarding the origin of the passive players’  
692 endowment. In particular, we explicitly inform dictators whether the passive players acquired  
693 their endowment by mean of luck or through an effort task (the task is described in Section  
694 4.1, footnote 18). As a preliminary observation, in line with previous experimental evidence  
695 dictators take a significantly larger share of the passive players’ endowment when the latter  
696 was windfall money rather than money earned in an effort task. A two-sided t-test suggests  
697 that the difference is strongly statistically significant both if we consider taking from receivers  
698 from a different village (4.27 vs. 5.05) or from fellow village members (3.23 vs. 3.87).

699 We then verify whether the observed reduction in the dictators’ taking rate for partic-  
700 ipants in treated villages is related to the source of passive players’ endowment. First, we  
701 generate the variable *totaltaking* by summing up the tokens taken by each participants in  
702 both the locals and the strangers conditions. We then compare this variable across treated  
703 groups separating between sessions in which merit or luck were the source of the passive  
704 player’s endowment. The reduction in *totaltaking* displayed by participants in treated vil-  
705 lages is similar in the Merit and Luck conditions and in both case statistically significant  
706 (tokens .99, equal to a 12% reduction, and 1.07, equal to a 11% reduction, respectively).  
707 Restricting the analysis to the sample of participants directly affected by the PFR reform  
708 returns similar results.

709 Second, we differentiate between taking from locals and taking from strangers. Table  
710 A11 in Appendix A replicates the same regression models presented in Table 1 separately  
711 estimating the treatment effects when luck or merit are the source of passive players’ en-  
712 dowment. We perform F-tests for the equality of regression coefficients of the interaction  
713 between the treatment dummy and the *luck* and *merit* variables, both when the interaction  
714 takes place between participants of the same village and when the participants belong to dif-  
715 ferent villages.<sup>32</sup> Results confirm that the reduction in taking rate displayed by participants  
716 in the treated group is registered both in the Merit and in the Luck conditions, and that the  
717 magnitude of the estimated effects are statistically the same across the two conditions. It

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Neary (2014) call it “the innate sense of property”.

<sup>32</sup>Therefore, we run the following four tests of equality of the regression coefficients:  
(Treated\*stranger\*Luck) - (Control\*stranger\*Luck)=0; (Treated\*stranger\*Merit) - (Con-  
trol\*stranger\*Merit)= 0; (Treated\*local\*Luck) - (Control\*local\*Luck)=0; (Treated\*local\*Merit) -  
(Control\*local\*Merit)= 0.

718 also shows that the treatment effect is roughly similar in the Luck and Merit conditions both  
719 when the passive player is a local and when he or she is a stranger. These results show that  
720 the effect of the formalization of property rights is orthogonal to the notion of merit. This  
721 suggests that the reform leverages on an “abstract” notion of property, which is unrelated to  
722 desert and it is independent from the prior history of transfers on which the current owner’s  
723 rights are based.

## 724 **6.4 Expressive Function**

725 A key goal of the law is to help people to coordinate toward desirable behaviors. Often —  
726 perhaps, most commonly — coordination is achieved without enforcement. It has long been  
727 recognized that the law has such an “expressive” function (Sunstein, 1996; Cooter, 1998;  
728 Basu, 2018) and the reform might have leveraged on it. By introducing private property  
729 rights, the reform might have made salient notions of “mine” and “yours”, thereby induc-  
730 ing a sense of entitlement in property owners and a corresponding tendency to respect that  
731 property in those who come in contact with it (Wilson, 2020). We provide some preliminary  
732 evidence of the plausibility of this channel by verifying whether the reform affected individ-  
733 uals’ ability to coordinate around mutually beneficial outcomes in a situation characterized  
734 by multiple equilibria. To do so, we employ a modified battle-of-the-sexes game with an  
735 additional symmetric option similar to the game used in Jackson and Xing (2014).

736 Players were assigned either the Row or Column role (which remain the same for the  
737 entire experiment) and had the possibility to choose among three strategies/colors. If the  
738 two players choose the same color, they earn positive payoffs. If they choose different colors,  
739 they earn zero. The game has three Nash equilibria (NE) in pure strategy and four in mixed  
740 strategy. Focusing on the NE in pure strategy, the two asymmetric equilibria are efficient but  
741 inequitable. In the symmetric equilibrium total payoff is equal to half of the payoff generated  
742 in the asymmetric NE and players earn the same amounts. The experimental parameters,  
743 possible strategies, and combinations of payoffs are summarized in Figure 3.

744 Each player made one choice in each of two different conditions (the order in which con-  
745 ditions were presented has been randomized). In the “prompt” conditions, before choosing  
746 their strategies participants received a prompt, consisting in an observation of a color that  
747 corresponds to one of the labels of the three possible actions.

748 In particular, during the explanation of the game instructions in the prompt condition,  
749 the experimenter reproduced the payoffs summarized in Figure 3 using physical coins and  
750 pieces of colored textile. The wood side-table used to place the textiles and coins was covered  
751 by an orange tablecloth. In the “base” condition, the same instructions were provided, but  
752 the wood side-table was not covered by any tablecloth.<sup>33</sup>

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<sup>33</sup>Following Jackson and Xing (2014), we did not call attention to the color as a correlating device in any way: the instructions provided were identical in the two conditions and both of them offered the visual

753 Therefore, one of the players is advantaged by the prompt-suggested equilibrium (the  
 754 Row player), in that coordination on the orange-color equilibrium implies that one of the  
 755 players earns his or her highest possible payoff, while the other player is disadvantaged (the  
 756 Column players).

757 The experiment is designed to investigate whether experiencing formalized property rights  
 758 changes the participants' ability to achieve non-zero payoffs by coordinating on a pure strat-  
 759 egy NE. To do so, we estimate the following regression equation using a Probit model:

$$c_i = \alpha + \delta_T T_i + \mathbf{X}_i + \epsilon_i \quad (2)$$

760 where  $c_i$  is a dummy equal to one when coordination on a non-zero NE in pure strategy  
 761 is achieved,  $T_i$  is a dummy equal to 1 for subjects in treated villages, and  $X_i$  is the vector of  
 762 individual- and village-level characteristics specified above.

763 Tables 3 reports the results, where models 1-6 replicate the inclusion of controls and the  
 764 sample refining as discussed in the analysis of the main results in Table 1. Participants in  
 765 treated villages who take decisions in the baseline conditions are statistically as likely as those  
 766 in control to coordinate on one of the equilibria entailing non-zero payoffs, as suggested by  
 767 the insignificant coefficient of the dummy *treated*. However, the interaction term between the  
 768 treatment dummy and the dummy indicating the prompt condition is positive and significant  
 769 at the 10% level.

770 We investigate further the result suggesting that participants in treated villages manage  
 771 to achieve coordination better than those in control villages when the coordination prompt  
 772 is introduced. As specified in the pre-analysis plan, we separately check how individuals who  
 773 were advantaged and disadvantaged by coordinating on the prompt-suggested equilibrium  
 774 reacted to its introduction. Specifically, we estimated the likelihood that a player opted  
 775 for the choice of the colour that entails her the highest payoff for herself if coordination is  
 776 achieved, that is, “Row chooses orange”; and “Column chooses purple”. Table 4 reports

information by just adding/eliminating the tablecloth before asking the study participants how they would play the game. We intentionally chose to present the prompt in the above form instead of as an explicit recommendation so that the study participants had a common signal that can be used as a cue, thus mimicking something which may be focal in the real world, but without feeling pressured by the experimenter to act in a specific way.

		<i>Column Participant</i>		
		<i>Purple</i>	<i>Orange</i>	<i>Green</i>
<b>Row Participants</b>	<b>Purple</b>	<b>100 ; 700</b>	<b>0 ; 0</b>	<b>0 ; 0</b>
	<b>Orange</b>	<b>0 ; 0</b>	<b>700 ; 100</b>	<b>0 ; 0</b>
	<b>Green</b>	<b>0 ; 0</b>	<b>0 ; 0</b>	<b>200 ; 200</b>

Figure 3: Payoffs in the coordination game (replication of Jackson and Xing, 2014).

Table 3: Coordination (Replication of Jackson and Xing, 2014)

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:	Whole			Refined		
treated	-0.130 (0.172)	-0.131 (0.175)	-0.139 (0.180)	-0.144 (0.187)	-0.140 (0.186)	-0.138 (0.186)
prompt	-0.325** (0.148)	-0.328** (0.148)	-0.327** (0.149)	-0.301* (0.162)	-0.306* (0.162)	-0.307* (0.163)
treated×prompt	0.373* (0.197)	0.374* (0.198)	0.373* (0.198)	0.390* (0.210)	0.394* (0.212)	0.395* (0.212)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Wealth	N	Y	Y	N	Y	Y
Other	N	N	Y	N	N	Y
Constant	1.649** (0.723)	1.311 (0.955)	2.148*** (0.770)	1.849** (0.903)	2.188*** (0.675)	1.701** (0.829)
N.obs.	1150	1150	1150	912	912	912

**Notes:** Dependent variable: dummy=1 when the two individuals achieve coordination. Random-effects Probit estimators. Standard errors robust for clustering at the session level. Compared to Models 1 and 3, Model 2 and 4 include village-level controls; compared to Models 2 and 4, Models 3 and 6 additionally include a set of proxies for individual wealth. Individual Controls include: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, income, whether the household has running water, number of bedrooms; Village-level Controls include: village population, whether the village has a market and market distance, distance from the closest public school, distance from the closest public hospital; Additional Wealth Controls include: acres of land possessed individually, whether the house has concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card. Symbols \*\*\*, \*\*, and \* indicate significance at the 1%, 5% and 10% level, respectively.

777 the results of Probit regressions dividing between Row players advantaged by the prompt  
778 (models 1-3) and Column players disadvantaged by it (models 4-6).

779 The results of models 1-3 show that Row players advantaged by the prompt are equally  
780 likely to choose the strategy that results in the highest own payoff in case coordination is  
781 achieved. Conversely, the negative and significant interaction term  $treated*prompt$  in models  
782 4-6 show that Column participants in treated villages who are disadvantaged by the intro-  
783 duction of the prompt are significantly less likely to choose the strategy maximizing their  
784 own payoff compared to those in control villages. This results suggest that the estimated  
785 increase in coordination on a equilibrium resulting in non-zero payoffs is driven by the be-  
786 havior of those individuals who are less likely to pursue a self-interested payoff maximizing  
787 strategy when a coordination asymmetry disadvantageous for them is introduced. In turn,  
788 this finding lends support to the idea that the reform may have facilitated coordination  
789 among individuals as to the reciprocal respect for the property of each others.

Table 4: Choices of the Self Payoff Maximizing Strategy in the Coordination Game

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:	Prompt-Advantaged Players			Prompt-Disadvantaged Players		
treated	0.003 (0.194)	0.006 (0.191)	-0.009 (0.193)	-0.032 (0.162)	-0.045 (0.175)	-0.054 (0.177)
prompt	-0.018 (0.116)	-0.018 (0.117)	-0.020 (0.118)	0.280*** (0.064)	0.282*** (0.065)	0.286*** (0.067)
treated × prompt	-0.185 (0.184)	-0.188 (0.185)	-0.188 (0.187)	-0.317** (0.157)	-0.323** (0.156)	-0.327** (0.159)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Wealth	N	Y	Y	N	Y	Y
Other	N	N	Y	N	N	Y
Constant	1.649** (0.723)	1.311 (0.955)	2.148*** (0.770)	1.849** (0.903)	2.188*** (0.675)	1.701** (0.829)
N.obs.	576	576	576	574	574	574

**Notes:** Dependent variable: dummy=1 when the individual choose the strategy that maximizes her own payoff in case coordination is achieved. Probit estimators. Standard errors robust for clustering at the session level. Compared to Models 1 and 3, Model 2 and 4 include village-level controls; compared to Models 2 and 4, Models 3 and 6 additionally include a set of proxies for individual wealth. Individual Controls include: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, income, whether the household has running water, number of bedrooms; Village-level Controls include: village population, whether the village has a market and market distance, distance from the closest public school, distance from the closest public hospital; Additional Wealth Controls include: acres of land possessed individually, whether the house has concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card. Symbols \* \* \*, \*\*, and \* indicate significance at the 1%, 5% and 10% level, respectively.

## 7 Conclusions

If individuals thought that “all is mine” there would be no exchange, only conflict. At the core of exchange is the reciprocal recognition of the “mine” and the “yours”. As Wilson (2020, p.175), has recently noted “property is more than just an individual claim. It is rather a socially shared practice which implies the jointly reciprocal acceptance of the condition ‘This is not mine; this is yours’[...] Out of the habit of responding to claims of ‘This is mine’ emerged a fitting custom found presumably in every human society: ‘Do not steal.’” But how do these norms emerge?

The new field evidence reported in this articles shows the existence of a causal link between the introduction of formal property rights and an increased reluctance to take the property of both locals and strangers. The main takeaway of our paper is that the introduction of private property rights strengthens both impartial pro-market institutions and the social preferences supporting them, thus reinforces their co-evolution. This conception is in line with a long legacy of legal scholarship emphasizing that one of the main functions of property institutions is to make the current owner’s right largely independent of the prior

805 history of transfers from prior owners. Indeed, property institutions have evolved largely to  
806 solve the problems caused by “invalid links” along this chain of transfers and hence make  
807 property a “right in a thing” rather than an obligation toward an individual (as contracts  
808 are) (Arruñada, 2012; Dari-Mattiacci and Guerriero, 2017).

809 In addition to this main result, we shed some light on the mechanisms underlying the  
810 connection between formal property rights and the voluntary respect for others’ property. A  
811 combination of treatment manipulations, auxiliary experiments and heterogeneity analysis  
812 allow us to discriminate among some tentative explanations. In accordance with the notion  
813 of property illustrated above, we found that the new formal property institutions fostered an  
814 abstract claim to ownership, which is independent of socio-economic advancements, moral  
815 values, and merit considerations. The results of our coordination game further suggest that  
816 the increased respect for the property of others can be attributed to the expressive function  
817 of the law. Formal property rules “tell people which expectations they can count on and  
818 which not” (Hayek, 1973), thus inducing individuals to coordinate around non-conflictual  
819 outcomes. Indeed, property rights are grounded in the notion that third parties ought  
820 to recognize them. The law facilitates that very recognition through institutions, such as  
821 registries, that provide public notice of property rights (Hansmann and Kraakman, 2002;  
822 Ayotte and Bolton, 2011). However, our results hint toward an additional effect produced  
823 by formal property laws: they foster the formation of “internalized” values of respect for the  
824 property of others. In this sense, the law can make property “moral” (Merrill and Smith,  
825 2006). Further research is necessary to shed more light on this mechanism.

826 We conclude with a cautionary note concerning possible pernicious effects connected to  
827 the introduction of property rights. As Heller and Salzman (2021) have recently emphasized,  
828 individuals may base their claim to ownership on different and possibly mutually incompat-  
829 ible grounds. One contender might claim ownership of a thing based on possession, while  
830 the other may defend her own claim to the same thing based on labor. When property is  
831 up for grabs, conflict will ensue. Given their potential to fuel conflicts in the medium term,  
832 property rights may put under stress possibly weak state institutions struggling to arbitrate  
833 such conflicts (Gambetta, 1996; Bandiera, 2003). Further research will have to identify the  
834 institutional conditions necessary for formal property rights to reinforce pro-market values  
835 without increasing conflictual episodes and to assess the long term effects of their introduc-  
836 tion.

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## Appendix A Supplementary Analysis

Table A1: Balance of Observables Across Treatment Groups (t test two-sided for continuous variable and Chi-square test for dummy variables)

	<b>PFR Reform</b> (n=287)	<b>Control</b> (n=288)	<b>Difference</b> (p-value)
male	.49	.51	.73
age	40.0	36.8	.01
muslim	.45	.41	.27
vodoun	.19	.18	.91
married	.89	.83	.02
householdnr	9.8	10.0	.68
managefinance	.95	.95	.99
literate	.40	.33	.08
bornvillage	.69	.72	.41
yearsinvillage	32.3	30.9	.24
weekly income (CFA)	9,026	8,468	.59
landuse (Hect)	5.47	5.10	.65
concretefloor	.64	.59	.23
electricity	.36	.36	.99
water	.26	.18	.02
radio-TV	.63	.63	.99
car	.09	.07	.28
moto	.77	.78	.69
bank-acc	.33	.27	.12
social-rank	4.45	4.36	.56



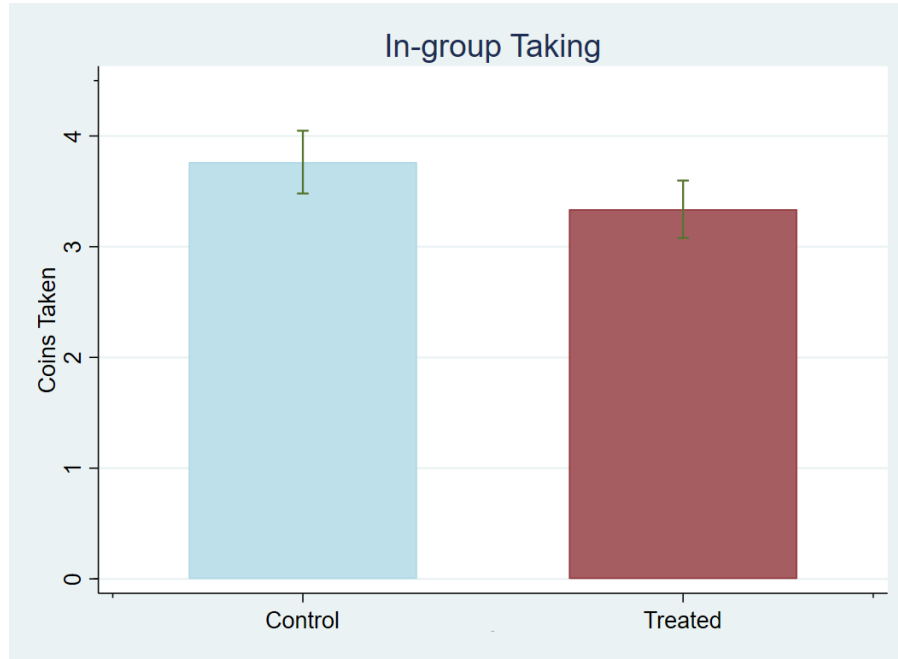


Figure A1: Tokens Taken from a Participant living in the Same Village (replication of Fabbri and Dari-Mattiacci, 2020)

Table A2: Tokens Taken from a Participant living in the Same Village (replication of Fabbri and Dari-Mattiacci, 2020)

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:	Whole			Refined		
treated	-0.548*	-0.637**	-0.674**	-0.772** (0.354)	-0.889** (0.348)	-0.959*** (0.323)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	1.057 (0.766)	1.131 (0.880)	1.093 (0.913)	0.857 (0.958)	0.896 (1.100)	1.088 (1.151)
N.obs.	575	575	575	575	575	575

**Notes:** Dependent variable: tokens taken by the dictator. OLS regression. Standard errors robust for clustering at the session level. Models 1-3 include the whole sample of participants, models 4-6 exclude participants in treated villages who do not own land affected by the PFR and participants in control villages who hold a formal property title over their land parcels. Compared to Models 1 and 3, Model 2 and 4 include village-level controls; compared to Models 2 and 4, Models 3 and 6 additionally include a set of proxies for individual wealth. Individual Controls include: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, income, whether the household has running water, number of bedrooms; Village-level Controls include: village population, whether the village has a market and market distance, distance from the closest public school, distance from the closest public hospital; Additional Wealth Controls include: acres of land possessed individually, whether the house has concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card. Symbols \*\*\*, \*\*, and \* indicate significance at the 1%, 5% and 10% level, respectively.

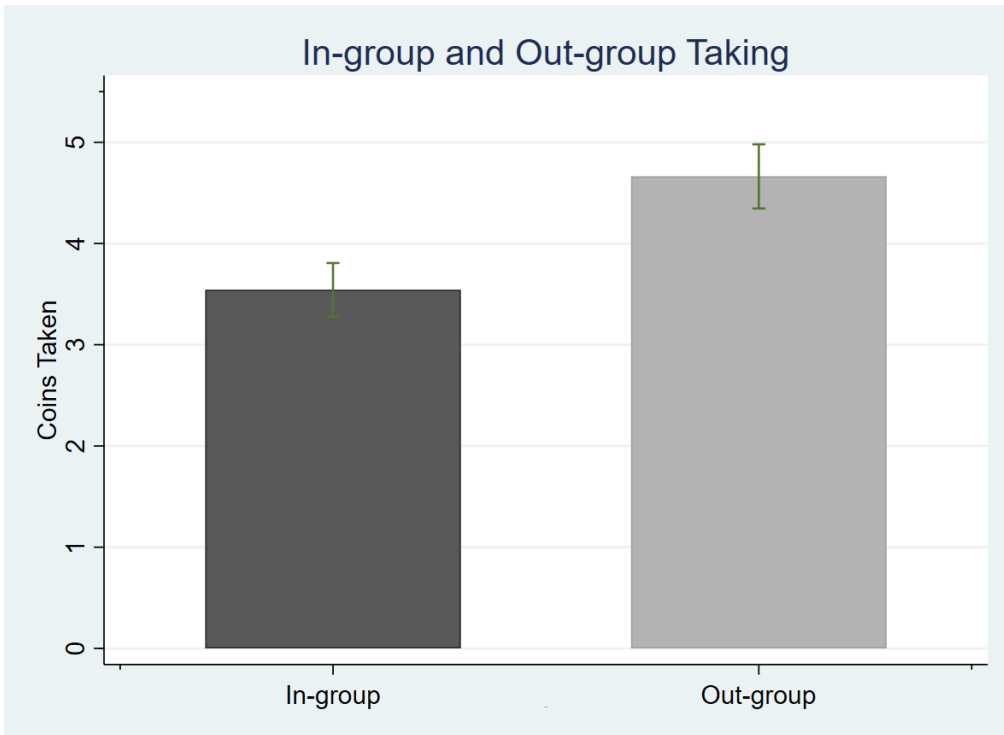


Figure A2: Total Tokens Taken either from a local or from a stranger

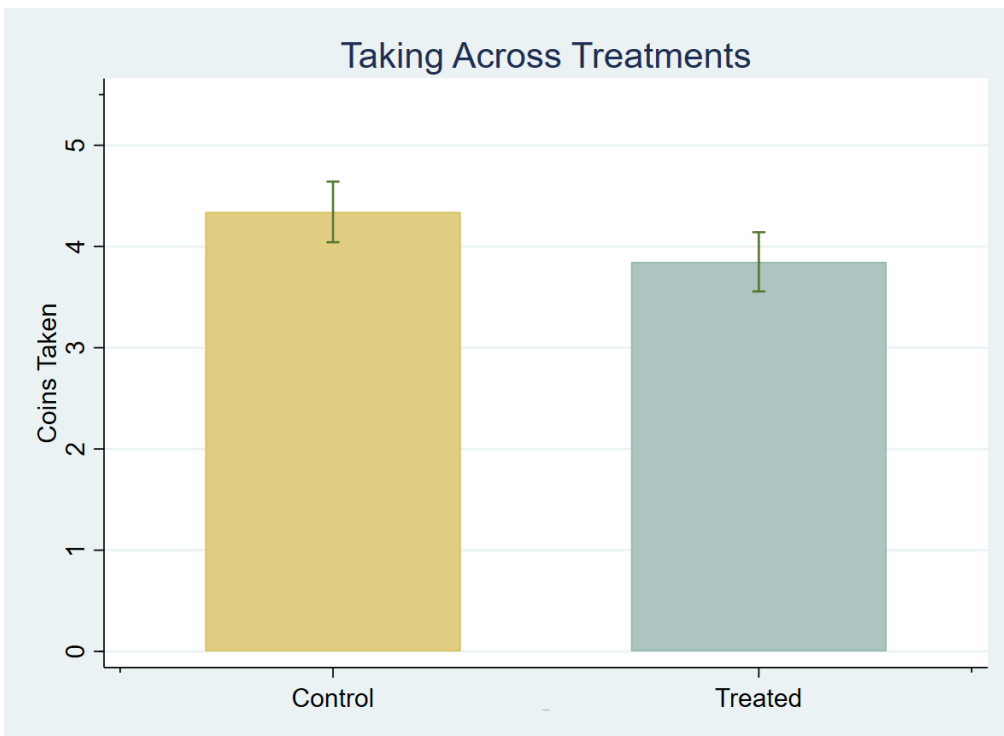


Figure A3: Average Tokens Taken from a Participant in the Two Decisions

Table A3: Tokens Taken from Same-Village vs. Other-Village Participants & Tokens Taken in Treated vs. Control Villages

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
stranger	1.110*** (0.119)	1.110*** (0.119)	1.110*** (0.120)			
treated				-0.620** (0.279)	-0.692** (0.274)	-0.723*** (0.263)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	1.057 (0.766)	1.131 (0.880)	1.093 (0.913)	0.857 (0.958)	0.896 (1.100)	1.088 (1.151)
N.obs.	575	575	575	456	456	456

**Notes:** Dependent variable: tokens taken by the dictator. OLS regression. Standard errors robust for clustering at the session level. Compared to Models 1 and 3, Model 2 and 4 include village-level controls; compared to Models 2 and 4, Models 3 and 6 additionally include a set of proxies for individual wealth. Individual Controls include: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, income, whether the household has running water, number of bedrooms; Village-level Controls include: village population, whether the village has a market and market distance, distance from the closest public school, distance from the closest public hospital; Additional Wealth Controls include: acres of land possessed individually, whether the house has concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card. Symbols \*\*\*, \*\*, and \* indicate significance at the 1%, 5% and 10% level, respectively.

Table A4: Tokens Taken by the Dictator - Heterogeneous Effects of Income

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:	Whole			Refined		
Ctl×H.I× stranger	0.828** (0.385)	0.859** (0.380)	0.773** (0.394)	0.851** (0.377)	0.904** (0.374)	0.772** (0.393)
Trt×L.I× stranger	-0.461 (0.365)	-0.528 (0.371)	-0.545 (0.366)	-0.965** (0.411)	-1.087*** (0.418)	-1.076*** (0.398)
Trt×H.I× stranger	-0.229 (0.444)	-0.280 (0.455)	-0.404 (0.455)	-0.346 (0.530)	-0.385 (0.526)	-0.607 (0.522)
Ctl×L.I× local	-0.994*** (0.267)	-0.994*** (0.268)	-0.994*** (0.269)	-1.000*** (0.287)	-1.000*** (0.288)	-1.000*** (0.290)
Ctl×H.I× local	-0.636 (0.455)	-0.605 (0.452)	-0.691 (0.464)	-0.563 (0.490)	-0.510 (0.490)	-0.642 (0.507)
Trt×L.I× local	-1.377*** (0.335)	-1.445*** (0.334)	-1.461*** (0.333)	-1.625*** (0.403)	-1.748*** (0.409)	-1.737*** (0.394)
Trt×H.I× local	-1.373*** (0.422)	-1.424*** (0.425)	-1.548*** (0.423)	-1.563*** (0.474)	-1.601*** (0.468)	-1.823*** (0.469)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	1.839** (0.737)	1.847** (0.907)	1.711* (0.886)	1.655** (0.830)	1.634 (1.041)	1.653 (1.072)
N.obs.	1150	1150	1150	912	912	912

**Notes:** Dependent variable: tokens taken by the dictator. GLS random-effects estimators. Standard errors robust for clustering at the session level. Models 1-3 include the whole sample of participants. Models 4-6 include only participants in the treated sample who were directly affected by the reform and exclude participants in the control sample who possessed formally registered land rights. Compared to Models 1 and 3, Model 2 and 4 include village-level controls; compared to Models 2 and 4, Models 3 and 6 additionally include a set of proxies for individual wealth. Individual Controls include: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, income, whether the household has running water, number of bedrooms; Village-level Controls include: village population, whether the village has a market and market distance, distance from the closest public school, distance from the closest public hospital; Additional Wealth Controls include: acres of land possessed individually, whether the house has concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card. Symbols \*\*\*, \*\*, and \* indicate significance at the 1%, 5% and 10% level, respectively.

*Legend:* H.I=High Income; L.I= Low Income.

Table A5: Tokens Taken by the Dictator - Heterogeneous Effects of Distance from Paved Roads

Sample:	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
	Whole			Refined		
Ctl×H_MI×	-0.217	-0.290	-0.234	-0.122	-0.194	-0.128
stranger	(0.500)	(0.447)	(0.445)	(0.524)	(0.462)	(0.463)
Trt×L_MI×	-0.202	-0.286	-0.262	-0.384	-0.558	-0.549
stranger	(0.374)	(0.395)	(0.394)	(0.527)	(0.587)	(0.578)
Trt×H_MI×	-1.076***	-1.134***	-1.166***	-1.385***	-1.440***	-1.473***
stranger	(0.412)	(0.395)	(0.406)	(0.429)	(0.403)	(0.410)
Ctl×L_MI×	-1.083***	-1.083***	-1.083***	-1.066***	-1.066***	-1.066***
local	(0.249)	(0.249)	(0.250)	(0.270)	(0.271)	(0.272)
Ctl×H_MI×	-1.606***	-1.678***	-1.623***	-1.554**	-1.626**	-1.560**
local	(0.616)	(0.553)	(0.547)	(0.732)	(0.657)	(0.635)
Trt×L_MI×	-1.113***	-1.198***	-1.173***	-1.168***	-1.341***	-1.333***
local	(0.328)	(0.351)	(0.359)	(0.402)	(0.485)	(0.492)
Trt×H_MI×	-2.147***	-2.205***	-2.238***	-2.358***	-2.413***	-2.446***
local	(0.369)	(0.348)	(0.355)	(0.404)	(0.373)	(0.376)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	1.699**	1.701**	1.669**	1.381*	1.342	1.568
	(0.682)	(0.802)	(0.806)	(0.796)	(0.952)	(0.992)
N.obs.	1150	1150	1150	912	912	912

**Notes:** Dependent variable: tokens taken by the dictator. GLS random-effects estimators. Standard errors robust for clustering at the session level. Models 1-3 include the whole sample of participants. Models 4-6 include only participants in the treated sample who were directly affected by the reform and exclude participants in the control sample who possessed formally registered land rights. Compared to Models 1 and 3, Model 2 and 4 include village-level controls; compared to Models 2 and 4, Models 3 and 6 additionally include a set of proxies for individual wealth. Individual Controls include: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, income, whether the household has running water, number of bedrooms; Village-level Controls include: village population, whether the village has a market and market distance, distance from the closest public school, distance from the closest public hospital; Additional Wealth Controls include: acres of land possessed individually, whether the house has concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card. Symbols \*\*\*, \*\*, and \* indicate significance at the 1%, 5% and 10% level, respectively. *Legend:* *H\_MI*=High Market Integration - proxied as distance from paved roads lower than the sample median; *L\_MI*= Low Market Integration - proxied as distance from paved road larger than the sample median.

Table A6: Tokens Taken by the Dictator - Heterogeneous Effects of Gender

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:	Whole			Refined		
Ctl×M× stranger	-0.320 (0.453)	-0.326 (0.433)	-0.336 (0.454)	-0.256 (0.466)	-0.311 (0.431)	-0.348 (0.466)
Trt×F× stranger	-0.750** (0.304)	-0.848** (0.335)	-0.794** (0.333)	-1.232*** (0.358)	-1.398*** (0.364)	-1.385*** (0.355)
Trt×M× stranger	-0.982*** (0.356)	-1.037*** (0.364)	-1.162*** (0.401)	-1.113** (0.456)	-1.220*** (0.467)	-1.357*** (0.514)
Ctl×F× local	-1.063*** (0.222)	-1.063*** (0.222)	-1.063*** (0.223)	-1.071*** (0.256)	-1.071*** (0.257)	-1.071*** (0.258)
Ctl×M× local	-1.648*** (0.412)	-1.655*** (0.403)	-1.665*** (0.411)	-1.538*** (0.401)	-1.593*** (0.382)	-1.630*** (0.399)
Trt×F× local	-1.619*** (0.283)	-1.717*** (0.304)	-1.663*** (0.306)	-1.970*** (0.337)	-2.136*** (0.347)	-2.124*** (0.346)
Trt×M× local	-2.158*** (0.317)	-2.213*** (0.324)	-2.338*** (0.354)	-2.221*** (0.424)	-2.328*** (0.435)	-2.465*** (0.472)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	1.716** (0.676)	1.730** (0.810)	1.650** (0.827)	1.442* (0.786)	1.387 (0.955)	1.580 (1.010)
N.obs.	1150	1150	1150	912	912	912

**Notes:** Dependent variable: tokens taken by the dictator. GLS random-effects estimators. Standard errors robust for clustering at the session level. Models 1-3 include the whole sample of participants. Models 4-6 include only participants in the treated sample who were directly affected by the reform and exclude participants in the control sample who possessed formally registered land rights. Compared to Models 1 and 3, Model 2 and 4 include village-level controls; compared to Models 2 and 4, Models 3 and 6 additionally include a set of proxies for individual wealth. Individual Controls include: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, income, whether the household has running water, number of bedrooms; Village-level Controls include: village population, whether the village has a market and market distance, distance from the closest public school, distance from the closest public hospital; Additional Wealth Controls include: acres of land possessed individually, whether the house has concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card. Symbols \*\*\*, \*\*, and \* indicate significance at the 1%, 5% and 10% level, respectively.

*Legend:* F=Female; M=Male.

Table A7: Tokens Taken by the Dictator — Excluding Three Villages That Extended the Reform After 2011

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:	Whole			Refined		
treated	-0.622* (0.337)	-0.674** (0.337)	-0.711** (0.334)	-0.977** (0.392)	-1.065*** (0.393)	-1.128*** (0.381)
local	-1.212*** (0.183)	-1.212*** (0.184)	-1.212*** (0.184)	-1.202*** (0.204)	-1.202*** (0.205)	-1.202*** (0.206)
treated× local	0.079 (0.236)	0.079 (0.236)	0.079 (0.237)	0.147 (0.239)	0.147 (0.239)	0.147 (0.241)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	1.456** (0.681)	1.318 (0.806)	1.210 (0.804)	1.192 (0.795)	0.985 (0.933)	1.168 (1.030)
N.obs.	1042	1042	1042	820	820	820

**Notes:** Dependent variable: tokens taken by the dictator OLS regression, GLS random-effects estimators. Standard errors robust for clustering at the session level. Compared to Models 1 and 3, Model 2 and 4 include village-level controls; compared to Models 2 and 4, Models 3 and 6 additionally include a set of proxies for individual wealth. Individual Controls include: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, income, whether the household has running water, number of bedrooms; Village-level Controls include: village population, whether the village has a market and market distance, distance from the closest public school, distance from the closest public hospital; Additional Wealth Controls include: acres of land possessed individually, whether the house has concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card. Symbols \*\*\*, \*\*, and \* indicate significance at the 1%, 5% and 10% level, respectively.

Table A8: Tokens Taken by the Dictator - Different Measures of Wealth

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:	Whole			Refined		
treated	-0.768** (0.319)	-0.808** (0.315)	-0.791** (0.311)	-1.106*** (0.343)	-1.149*** (0.341)	-1.168*** (0.330)
local	-1.218*** (0.184)	-1.212*** (0.184)	-1.212*** (0.183)	-1.208*** (0.205)	-1.202*** (0.204)	-1.202*** (0.204)
treated × local	0.167 (0.230)	0.192 (0.228)	0.192 (0.228)	0.284 (0.236)	0.284 (0.237)	0.284 (0.236)
sec-rank	0.091 (0.060)			0.090 (0.082)		
land- owned		0.024* (0.014)			0.030* (0.017)	
bedrooms		0.034 (0.047)			0.028 (0.049)	
cement- floor		0.080 (0.321)			0.056 (0.381)	
electricity		0.314 (0.267)			0.283 (0.295)	
water		0.377** (0.179)			0.535** (0.245)	
media			-0.147 (0.185)			-0.152 (0.226)
car			0.562* (0.335)			0.980** (0.412)
motorbike			0.600** (0.281)			0.487 (0.300)
credit			0.147 (0.235)			0.340 (0.257)
Other Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	Y	Y	Y	Y	Y	Y
Constant	1.771** (0.777)	1.938** (0.849)	1.832** (0.747)	1.507 (0.955)	1.584 (1.023)	1.619* (0.931)
N.obs.	1150	1150	1150	912	912	912

**Notes:** Dependent variable: tokens taken by the dictator. GLS random-effects estimators. Standard errors robust for clustering at the session level. Individual Controls include: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, whether the household has running water, number of bedrooms; Village-level Controls include: village population, whether the village has a market and market distance, distance from the closest public school, distance from the closest public hospital. Symbols \*\*\*, \*\*, and \* indicate significance at the 1%, 5% and 10% level, respectively.



Table A9: Tokens Taken by the Dictator — Selection of Controls Using Lasso Post-Double-Selection Approach (Belloni, Chernozhukov and Hansen, 2014)

Taking Decision: Sample:	Model 1	Model 2	Model 3	Model 4
	Out-group		In-group	
	Whole	Refined	Whole	Refined
treated	-0.737** (0.358)	-1.050*** (0.597)	-0.610** (0.305)	-0.799** (0.533)
managemoney	2.699*** (0.319)	2.926*** (0.375)	2.111*** (0.457)	
population	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
marketinvillage	0.532 (0.381)	0.622 (0.431)	0.675** (0.329)	0.863** (0.385)
marketdistance	-0.025 (0.076)	-0.025 (0.076)	-0.032 (0.062)	-0.010 (0.063)
state-edu	0.014 (0.078)	0.034 (0.081)	-0.073 (0.048)	-0.063 (0.048)
state-health	0.019 (0.044)	0.016 (0.048)	0.024 (0.038)	0.022 (0.044)
Constant	2.196*** (0.581)	2.000*** (0.597)	1.554** (0.674)	3.488*** (0.533)
N.obs.	575	457	575	457

**Notes:** Dependent variable: tokens taken by the Dictator. Regularized post-double-selection lasso regression. Standard errors robust for clustering at the village level. High-dim individual controls included: age, gender, religion, marital status, whether polygam, number of family members, participation to household finance management, literacy, years of education, whether the village of participation is also the village of birth, years of residence in the village, self-reported weekly income, incentivized measure of risk preferences, acres of land possessed individually, whether the house has concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card. Symbols \*\*\*, \*\*, and \* indicate significance at the 1%, 5% and 10% level, respectively.

Table A10: Difference between Out-group and In-group Taking Rates

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:	Whole			Refined		
treated	-0.143 (0.207)	-0.111 (0.224)	-0.098 (0.197)	-0.285 (0.213)	-0.257 (0.234)	-0.211 (0.214)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	0.222 (0.490)	0.090 (0.604)	0.074 (0.651)	-0.065 (0.588)	-0.274 (0.657)	-0.236 (0.709)
N.obs.	575	575	575	456	456	456

**Notes:** Dependent variable: difference in tokens taken by the Dictator when interacting with an out-group and an in-group partner, respectively. OLS estimators. Standard errors robust for clustering at the session level. Compared to Models 1 and 3, Model 2 and 4 include village-level controls; compared to Models 2 and 4, Models 3 and 6 additionally include a set of proxies for individual wealth. Individual Controls include: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, income, whether the household has running water, number of bedrooms; Village-level Controls include: village population, whether the village has a market and market distance, distance from the closest public school, distance from the closest public hospital; Additional Wealth Controls include: acres of land possessed individually, whether the house has concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card. Symbols \*\*\*, \*\*, and \* indicate significance at the 1%, 5% and 10% level, respectively.

Table A11: Tokens Taken by the Dictator — Different Sources of Passive Player’s Endowment

Sample:	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
	Whole			Refined		
Ctl×Merit× stranger	-0.545 (0.398)	-0.420 (0.431)	-0.361 (0.406)	-0.704* (0.426)	-0.541 (0.501)	-0.468 (0.465)
Trt×Luck× stranger	-0.574 (0.388)	-0.563 (0.359)	-0.554 (0.348)	-0.937** (0.458)	-0.922** (0.430)	-0.907** (0.418)
Trt×Merit× stranger	-1.235*** (0.415)	-1.271*** (0.432)	-1.280*** (0.430)	-1.705*** (0.417)	-1.750*** (0.423)	-1.786*** (0.444)
Ctl×Luck× local	-1.179*** (0.283)	-1.179*** (0.284)	-1.179*** (0.285)	-1.173*** (0.333)	-1.173*** (0.334)	-1.173*** (0.336)
Ctl×Merit× local	-1.767*** (0.440)	-1.642*** (0.453)	-1.583*** (0.419)	-1.906*** (0.480)	-1.743*** (0.522)	-1.670*** (0.471)
Trt×Luck× local	-1.749*** (0.371)	-1.737*** (0.336)	-1.729*** (0.324)	-2.038*** (0.418)	-2.023*** (0.391)	-2.008*** (0.375)
Trt×Merit× local	-2.136*** (0.356)	-2.171*** (0.357)	-2.181*** (0.351)	-2.488*** (0.394)	-2.534*** (0.394)	-2.569*** (0.419)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	1.699** (0.682)	1.701** (0.802)	1.669** (0.806)	1.381* (0.796)	1.342 (0.952)	1.568 (0.992)
N.obs.	1150	1150	1150	912	912	912

**Notes:** Dependent variable: tokens taken by the dictator. GLS random-effects estimators. Standard errors robust for clustering at the session level. Models 1-3 include the whole sample of participants. Models 4-6 include only participants in the treated sample who were directly affected by the reform and exclude participants in the control sample who possessed formally registered land rights. Compared to Models 1 and 3, Model 2 and 4 include village-level controls; compared to Models 2 and 4, Models 3 and 6 additionally include a set of proxies for individual wealth. Individual Controls include: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, income, whether the household has running water, number of bedrooms; Village-level Controls include: village population, whether the village has a market and market distance, distance from the closest public school, distance from the closest public hospital; Additional Wealth Controls include: acres of land possessed individually, whether the house has concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card. Symbols \*\*\*, \*\*, and \* indicate significance at the 1%, 5% and 10% level, respectively.

Table A12: Tokens Taken by the Dictator - Excluding Education and Income

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:	Whole	Refined	Whole	Refined	Whole	Refined
treated	-0.810*** (0.309)	-1.193*** (0.343)	-0.758** (0.321)	-1.083*** (0.349)	-0.746** (0.332)	-1.081*** (0.362)
local	-1.212*** (0.184)	-1.202*** (0.205)	-1.212*** (0.183)	-1.202*** (0.204)	-1.212*** (0.183)	-1.202*** (0.204)
treated× local	0.192 (0.228)	0.284 (0.237)	0.192 (0.227)	0.284 (0.236)	0.192 (0.227)	0.284 (0.236)
education			-0.113 (0.116)	-0.214 (0.133)		
literacy			0.448 (0.367)	0.602 (0.419)		
logincome	0.058 (0.103)	0.003 (0.099)				
Wealth- C.	Y	Y	N	N	N	N
Other-C.	Y	Y	Y	Y	Y	Y
Constant	1.649** (0.723)	1.311 (0.955)	2.148*** (0.770)	1.849** (0.903)	2.188*** (0.675)	1.701** (0.829)
N.obs.	1150	1150	1150	912	912	912

**Notes:** Dependent variable: tokens taken by the dictator. GLS random-effects estimators. Standard errors robust for clustering at the session level. Models 1 and 2 exclude controls for education and literacy; Models 3 and 4 exclude controls for income and proxies for wealth; Models 5 and 6 exclude controls for education, literacy, income, and proxies for wealth. Other Controls include: age, gender, religion, marital status, number of family members, participation to household finance management, village of birth, years of residence in the village, village population, whether the village has a market and market distance, distance from the closest public school, distance from the closest public hospital. Wealth Controls include: number of bedrooms, whether the house has running water, acres of land possessed individually, whether the house has concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card. Symbols \*\*\*, \*\*, and \* indicate significance at the 1%, 5% and 10% level, respectively.

Table A13: Tokens Taken by the Dictator - Control for Land-Related Conflicts

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:	Whole			Refined		
treated	-0.710** (0.302)	-0.788** (0.307)	-0.818*** (0.300)	-1.059*** (0.343)	-1.165*** (0.347)	-1.209*** (0.336)
local	-1.212*** (0.183)	-1.212*** (0.183)	-1.212*** (0.184)	-1.202*** (0.204)	-1.202*** (0.204)	-1.202*** (0.205)
treated× local	0.192 (0.227)	0.192 (0.228)	0.192 (0.229)	0.284 (0.236)	0.284 (0.236)	0.284 (0.237)
conflicts	-0.061 (0.416)	-0.069 (0.409)	-0.112 (0.390)	0.329 (0.395)	0.296 (0.385)	0.225 (0.372)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	1.699** (0.682)	1.701** (0.802)	1.669** (0.806)	1.381* (0.796)	1.342 (0.952)	1.568 (0.992)
N.obs.	1150	1150	1150	912	912	912

**Notes:** Dependent variable: tokens taken by the dictator. GLS random-effects estimators. Standard errors robust for clustering at the session level. Compared to Models 1 and 3, Model 2 and 4 include village-level controls; compared to Models 2 and 4, Models 3 and 6 additionally include a set of proxies for individual wealth. Individual Controls include: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, income, whether the household has running water, number of bedrooms; Village-level Controls include: village population, whether the village has a market and market distance, distance from the closest public school, distance from the closest public hospital; Additional Wealth Controls include: acres of land possessed individually, whether the house has concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card. Symbols \*\*\*, \*\*, and \* indicate significance at the 1%, 5% and 10% level, respectively.

Table A14: Average Outcome Reported in Ten Dice Rolls

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:	Whole			Refined		
treated	0.074 (0.104)	0.052 (0.104)	0.046 (0.100)	0.020 (0.098)	-0.006 (0.100)	-0.009 (0.104)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	3.636*** (0.378)	3.626*** (0.354)	3.642*** (0.356)	3.659*** (0.385)	3.585*** (0.349)	3.664*** (0.360)
N.obs.	447	447	447	447	447	447

**Notes:** Dependent variable: average outcome reported for ten dice rolls. OLS regression. Standard errors robust for clustering at the session level. Models 1-3 include the whole sample of participants, models 4-6 exclude participants in treated villages who do not own land affected by the PFR and participants in control villages who hold a formal property title over their land parcels. Compared to Models 1 and 3, Model 2 and 4 include village-level controls; compared to Models 2 and 4, Models 3 and 6 additionally include a set of proxies for individual wealth. Individual Controls include: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, income, whether the household has running water, number of bedrooms; Village-level Controls include: village population, whether the village has a market and market distance, distance from the closest public school, distance from the closest public hospital; Additional Wealth Controls include: acres of land possessed individually, whether the house has concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card. Symbols \*\*\*, \*\*, and \* indicate significance at the 1%, 5% and 10% level, respectively.

Table A15: Outcome Reported in the First Dice Roll

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:	Whole			Refined		
treated	-0.190 (0.196)	-0.303* (0.158)	-0.316* (0.170)	-0.289 (0.225)	-0.414** (0.180)	-0.424** (0.186)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	2.437*** (0.660)	2.120*** (0.698)	1.995*** (0.658)	2.191*** (0.623)	1.717** (0.661)	1.613** (0.600)
N.obs.	575	575	575	456	456	456

**Notes:** Dependent variable: Outcome reported in the first dice roll. OLS regression. Standard errors robust for clustering at the session level. Models 1-3 include the whole sample of participants, models 4-6 exclude participants in treated villages who do not own land affected by the PFR and participants in control villages who hold a formal property title over their land parcels. Compared to Models 1 and 3, Model 2 and 4 include village-level controls; compared to Models 2 and 4, Models 3 and 6 additionally include a set of proxies for individual wealth. Individual Controls include: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, income, whether the household has running water, number of bedrooms; Village-level Controls include: village population, whether the village has a market and market distance, distance from the closest public school, distance from the closest public hospital; Additional Wealth Controls include: acres of land possessed individually, whether the house has concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card. Symbols \*\*\*, \*\*, and \* indicate significance at the 1%, 5% and 10% level, respectively.



Figure A4: Tokens Taken from a Participant living in the Same Village



## 1133 **Appendix B Instructions**

### 1134 **General instructions**

1135 Thank you for coming to today's meeting. Please note that, if you do not feel comfortable,  
1136 you are free to leave this meeting at any point of time. Today's meeting starts with some  
1137 activities in which you have to make choices. During the activities, you will have the chance  
1138 to earn a substantial amount of money. The money you earn, together with the 500 CFA  
1139 for showing up today, will be paid out in cash at the end of the meeting.

1140 The meeting will last for some hours, and to receive the payment it is necessary that  
1141 you attend the meeting until the end. All the choices you will make will remain strictly  
1142 anonymous. No one other than me will know what you earn today. The payment will be  
1143 private. You should know that the money comes from research funds and not from our  
1144 own pockets or from the pocket of politicians. Please note that there is no right or wrong  
1145 in making the decisions, this is not a test. During today's session you will receive a code.  
1146 This ensures that everything you do (your decisions and your answers in questionnaires) will  
1147 remain anonymous.

1148 During the activities, we will speak of tokens. 1 token is worth 50 CFA.

### 1149 **Activity 1**

1150 In this activity there are two types of participants: Participant A and Participant B.

1151 **Merit treatment** Participant A has the possibility to work in order to earn 10 tokens.  
1152 To earn the 10 tokens, Participant A will need to successfully complete a work assignment.  
1153 Specifically, Participant A will receive a plastic box and 200 toothpicks. The plastic box has  
1154 a little hole on top. Participant A has ten minutes to place all the 200 toothpicks inside the  
1155 box from the top hole. If Participant A manages to complete the work assignment within  
1156 the ten minutes, he/she receives the ten tokens. Otherwise, he/she will not receive any token  
1157 for this part of the study. Participant B initially has zero tokens. If Participant A earned  
1158 the 10 tokens, Participant B can take 0, 1, 2, etc. up to 10 tokens from Participant A.

1159 The final outcome of this activity is: for Participant A, the tokens left by Participant B.  
1160 For Participant B, the tokens taken from Participant A. If Participant A did not manage to  
1161 complete the work assignment within the ten minutes, both Participants get zero. **Luck**  
1162 **treatment** Participant A receives 10 tokens from the experimenter for free. Participant B  
1163 initially has zero tokens. Participant B can take 0, 1, 2, etc. up to 10 tokens from Participant  
1164 A.

1165 The final earnings of this activity are: for Participant A, the tokens left by Participant  
1166 B. For Participant B, the tokens taken from Participant A.

1167 *Which is your role?*

1168 We do not know yet whether you will be the Participant A or B. We ask you to *work*  
1169 *and complete the work assignment as if you are the Participant A, and we also ask you to*<sup>34</sup>  
1170 choose how many tokens you want to take from your partner as if you were the Participant  
1171 B. At the end of the assignment, we will randomly assign you either the role of Participant  
1172 A or the role of Participant B.

1173 *Who is your partner in this activity?*

1174 In this activity you are going to be asked to make decisions with people from this village  
1175 participating to the research project today. At the end of the activity, we will randomly  
1176 match you with another participant in this village who has been assigned the other role.

1177 *How are your earnings in this activity calculated?*

1178 Yours and your partner's earnings will be determined by the actions you made in the as-  
1179 signed role; actions made in the other role will not affect final earnings and will be discarded.  
1180 Your earnings in this activity will be paid cash at the end of today's study.

## 1181 **Activity 2**

1182 The decisions you will make and the earnings you will collect in this second activity are  
1183 completely unrelated to those of the activity that you have just completed.

1184 As in the previous activity, in this activity there are again two types of participants:  
1185 Participant A and Participant B.

1186 **Merit treatment** As before, Participant A has the possibility to work in order to earn  
1187 10 tokens: Participant A has ten minutes to place all the 200 toothpicks inside the box from  
1188 the top hole, and he/she will receive zero tokens if the work assignment will not be completed  
1189 within the ten minutes. **Luck treatment** As before, participant A receives 10 tokens  
1190 from the experimenter for free.

1191 As in the previous activity, Participant B initially has zero tokens. If Participant A  
1192 earned the 10 tokens, Participant B can take 0, 1, 2, etc., up to 10 tokens from Participant  
1193 A.

1194 As before, the final outcome of this activity is: for Participant A, the tokens left by  
1195 Participant B. For Participant B, the tokens taken from Participant A. If Participant A did  
1196 not complete the work assignment, both will earn zero.

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<sup>34</sup>Merit treatment only.

1197 *Which is your role?*

1198 As before, we do not know yet whether you will be the Participant A or B. We ask you  
1199 to *work and complete the work assignment as if you are the Participant A, and we also ask*  
1200 *you to choose how many tokens you want to*<sup>35</sup> *choose how many tokens you want to take*  
1201 *from your partner as if you were the Participant B. At the end of the assignment, we will*  
1202 *randomly assign you either the role of Participant A or the role of Participant B.*

1203 *Who is your partner in this activity?*

1204 In this project you are going to be asked to make decisions with people from other villages  
1205 in Benin. Many people have already made their decisions and other groups are doing the  
1206 same research this week.

1207 At the end of the assignment, we will match you with another participant from another  
1208 village in Benin who has been assigned the other role in order to calculate your earnings.

1209 *How are your earnings in this activity calculated?*

1210 Yours and your partner's earnings will be determined by the actions you made in the as-  
1211 signed role; actions made in the other role will not affect final earnings and will be discarded.  
1212 Your earnings in this activity will be paid cash at the end of today's study.

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<sup>35</sup>Merit treatment only.