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Strangers' property

Marco Fabbri[1,2], Giuseppe Dari-Mattiacci[2,3], Matteo Rizzolli[4,*]

[1]Department of Economics, University of Bologna, Piazza Scaravilli 2, Bologna, 40126, Italy
[2]Amsterdam Center for Law & Economics, Nieuwe Achtergracht 166, Amsterdam, 1018 WV, The Netherlands
[3]Faculty of Law, University of Amsterdam, Nieuwe Achtergracht 166, Amsterdam, 1018WV, The Netherlands
[4]Department of Law, Economics, Politics and Modern languages, LUMSA University, Via Pompeo Magno 28, Rome, 00191, Italy

*Corresponding author: E-mail: m.rizzolli@lumsa.it

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 role of formalized property rights in inducing respect for the property of strangers, regardless of enforcement. Ten years after the randomized introduction of formal property rights across rural Benin, we conducted a taking-dictator-game experiment in which participants could 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 discuss their plausibility (*JEL*: D02, D91, K11, K42).

1. INTRODUCTION

When trade transcends family, kin, and friendship circles, an individual's reputation is no longer a sufficient bond. Society can reap the benefits of impersonal trade only if it develops solutions to various forms of opportunism by locals at the expense of strangers—such as cheating, reneging on promises, and expropriation—which lie at the core of the *fundamental problem of exchange* (Greif 2000). Among a wide array of institutional arrangements that emerged throughout history, institutions supporting and preserving private property—the hallmark of Western legal cultures (Garnsey 2014)—have taken center stage as propellers of trade and development both in a vast and influential literature (North and Weingast 1989; Besley and Ghatak 2012) and in a campaign of institutional reforms in developing countries (De Soto 2000; Lipton 2009). Formal property rights are impartial by design: they are grounded in institutions, such as registries, conceived to provide strangers with reliable notice of existing entitlements and to serve as a basis for enforcement against any third party (Hansmann and Kraakman 2002).

However, enforcement might not be the sole bulwark against opportunistic behavior. Recent literature points to the existence of values of respect for the property of others. At the micro level, experiments have shown that individuals exhibit a certain degree of respect for the property of others regardless of enforcement (Korenok et al. 2018; Faillo et al. 2019). At the macro level, pro-social behavior is positively associated with exposure to impersonal markets (Henrich et al. 2001, 2010). Taken together, these two strands of literature point to a possible double dividend of formal property institutions: they promote impersonal trade both through formal and impartial third-party enforcement and by shaping values of respect for the property of others. Given the popularity of property-rights reforms in developing countries, assessing their impact is of crucial practical relevance. While ample literature exists on the first dividend, our article provides novel and unique field-experimental evidence of the second.

Establishing a causal link between institutions and (personal or social) norms is problematic because of the rare occurrence of real-world institutional experiments.[1] In order to address this challenge, we exploit the first case in which land property institutions were introduced in a set of Beninese rural villages, in West Africa, via a large-scale randomized control trial (henceforth, RCT). The land titling reform that we study—known as *Plan Foncier Rural* (henceforth, PFR)—consisted of the surveying and demarcation of land parcels, and the registration of land titles in public registries, which effectively confers proof of ownership to rightsholders. The intervention transformed customary use rights over land subject to social control and enforcement by traditional local authorities into formal titles that are functionally analogous to private, transferable property rights enforced by state courts. The reform was implemented in 2010–2011 in 294 treated villages, while no intervention took place in a set of 282 control villages. Crucially, the selection into treated and control was made via a public lottery organized with the logistical support of the Millennium Challenge Corporation. Ten years later, in the control villages, no other formalization of rights had taken place, and customary land rights remain in place to date (Goldstein et al. 2018; Omondi 2019).

In early 2020, our research team visited 32 villages randomly selected from those included in the original Randomized Control Trial (RCT) and conducted a series of lab-in-the-field experiments. In the main experiment, participants undertook an anonymous *taking* dictator game. In this variant of the game, the active player—the "dictator"—decides how much of the passive player's endowment to take.[2] To assess whether the formalization of property rights fosters values of respect for the property of strangers, the dictator is asked to decide how much to take from the endowment of an anonymous passive player, resident in a different village—that is, a *stranger*.

Our main result shows that dictators in treated villages took roughly 12% less from strangers than dictators in control villages did. Further analysis shows that the effect is driven by those who possess land parcels included in the reform and enjoy comparatively easier access to the formal legal system—a key benefit for rightsholders under the PFR—which suggests that first-hand experience with the reform determines the observed reduction in taking.

[1] Laboratory experiments (Kimbrough et al. 2008; Kimbrough and Wilson 2013; Wilson 2020) have limited external and ecological validity because of the artificial features of the institutions studied and the impossibility of observing medium- and long-term effects (Alesina and Giuliano 2015).

[2] 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 et al. 2019).

We then investigate how the effects of the reform on taking from strangers relate to taking from *locals*, that is, members of the same village. In a previous experiment conducted in a different sample of Beninese villages in 2017, two of us showed that the introduction of formal property rights reduced taking when the game was played between locals (Fabbri and Dari-Mattiacci 2021). However, taking behavior from locals might not be predictive of taking behavior from strangers.

On the one hand, the PFR may have promoted a form of parochial respect for the property of locals by enlarging the reference group within which rules of reciprocity yield respect for the members' "mine and yours" from the extended family to the entire village community (Wilson 2020). If that were the case, the difference between takings from strangers and takings from locals would rise with the reform. On the other hand, the reform may have reduced parochialism as it recorded information on property titles that were previously accessible only to locals, made it publicly available, and treated it as conclusive evidence of ownership in state courts, thereby lessening the need to resort to local (thus, parochial) connections to resolve conflicts. In this case, the reform would cause a reduction in the difference between the taking rates.

To address this question, our participants took part in an additional taking dictator game played with locals that replicates Fabbri and Dari-Mattiacci (2021). Consistent with the extant literature on parochialism (Bernhard et al. 2006; Romano et al. 2017), participants in our sample took comparatively more from strangers than they did from locals both in the treated and in the control villages. Moreover, the difference between the two taking rates is the same in treated and control villages. The reform induced subjects in treated villages to take less from strangers but did not make them more or less parochial.

We inquire into alternative mechanisms for the observed change in taking behavior through a combination of treatment manipulations, auxiliary experiments, and heterogeneity analysis. While our analysis falls short of unambiguously identifying a single transmission mechanism, it may offer some guidance to future research.

First, we look into the impact of socio-economic factors—namely education, wealth, and the rate of conflicts—only to find that they had no significant effect on taking behavior. Second, we investigate the moral values of honesty—as measured in a truth-telling game—and altruism—as measured in a giving dictator game—but find that they were largely unaffected by the reform. Third, we consider social preferences. The reform increased both cooperation with strangers and cooperation and trust among locals, as measured in public good and trust games (Fabbri 2021, 2022). We observe that more cooperation among locals is associated with more taking from strangers, while more cooperation with strangers is associated with less taking from them, possibly suggesting that ingroup ties and kin closeness impair the development of impersonal pro-sociality. However, the coefficients are imprecisely estimated and not statistically significant, which leaves open the question regarding the potential importance of this mechanism. Fourth, we entertain the possibility that the PFR brought with it a perception that property is rightfully earned. For this purpose, we implemented an alternative treatment manipulation in which the passive players' endowments were earned through an effort task. Indeed, consistent with extant literature, dictators were less willing to take when they faced passive players who had earned their endowment. However, the reduction in taking among the treated is equal in the effort and windfall-money conditions.

Finally, we consider the possibility that the reform may have influenced people's ability to coordinate expectations around non-conflictual—albeit possibly more unequal—outcomes, thereby inducing individuals in treated villages to be more willing to accept the status-quo allocation of resources (as in Hayek 1973). Evidence collected through a modified battle-of-the-sexes experiment provides suggestive but weak support to this hypothesis.

Our article contributes to four strands of literature. The vast literature on property rights views property as protection of individual endowments from expropriation, either *vertically* by rulers (North 1981; Acemoglu and Johnson 2005) or *horizontally* by similarly situated individuals (Calabresi and Douglas Melamed 1972). While this literature has focused on third-party enforcement, our article zeroes in on the fact that the law may activate first-party enforcement.

Roman jurists noticed two millennia ago that laws may affect behavior even without third-party enforcement (McGinn 2001). A more recent literature on the expressive function of the law (Sunstein 1996; Cooter 1998) explains that this is the case because laws aggregate individual judgments and hence convey information about the collective wisdom as to which course of action is the most desirable (Dharmapala and McAdams 2003), or because they align expectations as to others' behavior and hence provide a focal point for individual actions (McAdams 2000). Our analysis adds to the empirical literature that documents the expressive effects of the law (McAdams and Nadler 2005; Funk 2007).

Our article also builds on the literature on how preferences and culture can be *endogenously* determined by economic incentives and institutions (Bowles 1998; Jha and Shayo 2019; Bau 2021; Margalit and Shayo 2021). The closest paper to ours is Di Tella et al. (2007), which exploited a quasi-random allocation of formal property titles to squatters in Buenos Aires and showed that, after a few years, those who had received property titles displayed more pronounced pro-market beliefs than a control group. In this line of research, we are the first to provide lab-in-field evidence based on an RCT.

Finally, our results are also relevant to the literature on the co-evolution of institutions and culture (Henrich 2020). Related sociological studies (Yamagishi et al. 1999) have suggested that interaction with strangers may teach individuals how to tell trustworthy from untrustworthy partners and hence further enhance one's ability to do business with strangers. In this literature, an initial institutional shock sets off a chain of social and psychological changes along the path of impersonal trade (Greif and Tabellini 2010). We study one such shock.

The remainder of this article is organized as follows. In Section 2, we provide the details of the institutional framework of the Beninese reform and explain the design of our lab-in-the-field experiments. In Section 3, we present our main results, and in Section 4, we discuss possible channels and report on several auxiliary experiments. Finally, Section 5 offers a discussion of our findings and ideas for future research. The Appendix contains details of the empirical analysis, the instructions for the experiments, and an overview of the two data-gathering campaigns and the companion studies referenced here.

2. EMPIRICAL STRATEGY

2.1 Institutional framework

Customary land-use rights are a common land-tenure arrangement in most rural areas of the African continent. Customary rights consist of a set of socially determined land-use rules, where access to land is an integral part of the social structure, tenure is determined by socio-political relationships, and land-related disputes are arbitrated by local authorities legitimated by tradition or religious customs (Lavigne-Delville 2006).

Citing concerns about tenure insecurity, the Republic of Benin has enacted a Torrens-type land-titling reform known as the "Plan Foncier Rural" (PFR). The PFR implementation program, which received technical and financial support from the Millennium Challenge Corporation, was completed by the Beninese government in 2010–2011. The reform started with socio-land surveys at the village level to identify rightsholders, their rights, and parcel boundaries. Following them, implementation proceeded with land demarcation and the

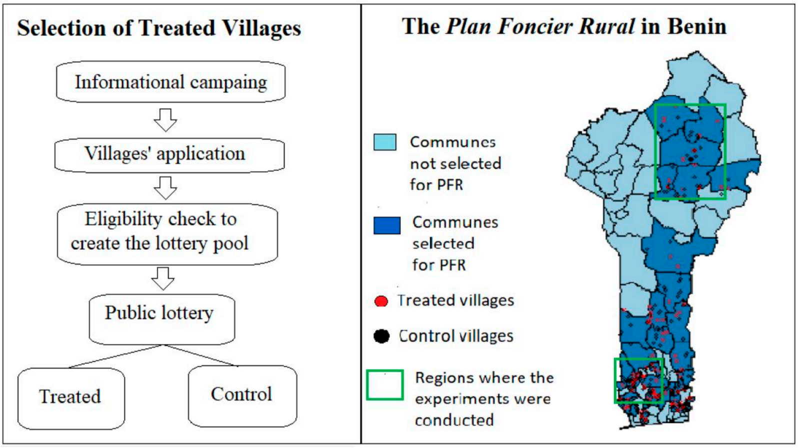

Figure 1. *Left panel:* The lottery mechanism used to select villages where the reform was implemented. *Right panel:* The distribution of treated and control villages. Figure from Fabbri (2021).

recording in public registries of land maps, which define rightsholders and associated rights for each parcel of land. Registration affords rightsholders a legal presumption of ownership, which, in turn, dramatically improves the likelihood of success in potential disputes before state courts. Given these characteristics, the PFR reform in Benin realized a major shift from collective use-rights arbitrated by local authorities to formal property rights subject to legal protection.

For purposes of our empirical investigation, the key feature of the PFR is that the implementation followed an RCT process involving hundreds of rural villages. In fact, this is the first case of a large-scale land tenure reform implemented as an RCT. In the preliminary phase of the project, interested rural villages were informed about the PFR and were invited to apply in order to participate in a lottery. As a second step, each application was examined to verify whether the village met certain eligibility criteria, such as being effectively located in a rural area. Among the 576 villages that applied and were judged eligible, a subsample of 294 villages was randomly chosen via public lottery. Consequently, in 2010–2011, a team of local experts implemented the PFR in these selected villages (the "treated" group). The 282 non-selected villages (the "control" group) did not receive any intervention and, as of today, continue to have customary land rights. Figure 1 summarizes the PFR lottery mechanism. The map shows the areas selected for the lottery pool and, within these areas, the treated and control villages.

2.2 Experimental design, hypotheses, and preregistration details

The experimental design, hypotheses to be tested, and regression model specifications were registered in a pre-analysis plan submitted to the American Economic Association's RCT Registry before the data collection took place.[3] The research strategy makes use of the RCT implementation of the reform to compare values of respect for the property of anonymous strangers across treated and control villages.

[3] The present article reports for the first time the findings of the main experiment (a modified dictator with takings from strangers) as well as of the two ancillary experiments: a dice-rolling experiment testing honesty and a modified battle-of-the-sexes experiment testing coordination. All experiments were preregistered with the American Economic Association's Registry for Randomized Controlled Trials. The unique identification number of the main experiment is AEARCTR-0005322. The pre-analysis plans concerning the two auxiliary experiments on honesty and coordination have IDs AEARCTR-0005324 and AEARCTR-0005319, respectively. The data underlying this article and the replication files are available in the online supplementary material.

To elicit respect for the property of others, we conducted a lab-in-the-field incentivized experiment in a sample of villages included in the lottery pool. The experiment consists of a modified dictator game in which the dictator can take (a part of) the endowment owned by a passive player. More specifically, as an initial endowment, the passive player owns ten tokens worth CFA 50 each (in total, approximately \$0.85). The dictator chooses whether to take some or all of the tokens owned by the passive player and transfer them to her account. Final earnings are determined by the number of tokens each of the two players possesses.

Participants are informed that we adopt a role-reversal protocol. At the beginning of the experiment, participants do not know which role is assigned to them. Instead, all participants in our sample state their decisions as if they were playing the game in the role of dictator. However, only half of the participants are actually assigned to the role of a dictator, whose choices determine both the dictator-own payoffs and the payoffs of the matched passive participant. The taking decision stated by participants who are assigned to the role of passive players instead has no consequences on payoffs.

The main objective of the study is to test whether the land rights reform affected the willingness to respect the property rights of an anonymous stranger who is not part of the reference group to which the decision-maker belongs. Following previous research, we identified the village community as the relevant reference group for our participants (Bulte et al. 2017). Accordingly, in the experiment, each participant takes two decisions in the role of dictator. In one case, the paired passive player belongs to the same village as the dictator ("local" condition), and in the other case, the passive player belongs to a different rural village in Benin ("stranger" condition). The former decision is used as a benchmark to test whether the reform affects participants' parochialism, that is, the extent to which individuals' prosocial behavior changes as social distance increases (Enke et al. 2020). To control for possible order and moral edging effects, half of the dictators play the locals' condition first, while the remaining half play the strangers' condition first.

In addition to our main analysis, we also implement a treatment variation that concerns the way in which the passive player acquires its initial endowment. In the "Luck" treatment, the endowment of the passive player comes as windfall money. Participants are informed that they received an endowment equal to 10 tokens from the experimenters. In the "Merit" treatment, players have to complete an effort task in order to acquire the endowment.[4] This treatment variation follows a between-subject design so each participant only takes part in either the Luck or the Merit treatment.

Our main hypothesis concerns the effects of formalizing land rights on the respect that participants display for the property rights of outgroup strangers not belonging to the same village community. We test whether the dictator's taking rate when the partner is from a different village is equal in the treated and control groups. This hypothesis is tested by estimating the following regression equation:

$$t_i = \alpha + \alpha_F F_i + \delta_T T_i + \delta_F F_i T_i + \boldsymbol{X_i} + \epsilon_i \tag{1}$$

where t_i is the taking decision made by the dictator, F_i is a dummy equal to one when the subject takes decisions in the interaction with individuals belonging to the same village, T_i is

[4] 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 10 min to slide all 200 toothpicks inside the box from the top hole in order to receive the ten 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 A, we included an English translation of the instructions given to the participants in both the Luck and the Merit treatments.

a dummy equal to 1 for subjects in treated villages, and X_i is a vector of the individual characteristics specified in the post-experimental survey.

As specified in the pre-analysis plan, we verify the effects of the reform on parochialism by comparing the difference between dictators' taking rates in the outgroup and ingroup conditions across treatments. We also investigate possible heterogeneities in dictators' taking rate for same-village and different-village interactions by using data on distance from paved roads (as a proxy for access to formal justice and market integration), gender, and income. Moreover, we study whether varying the processes through which the passive player acquires the initial endowment—luck versus merit—affects dictators' taking rate. Finally, we investigate several additional mechanisms by using evidence from three auxiliary experiments made with the same subjects during the same experimental campaign and described below.

2.3 Fieldwork procedures

The data collection took place between January and March 2020. The procedure to collect data worked as follows. We randomly selected the villages where the data collection took place from the whole list of villages included in the Beninese PFR that are located in two provinces in the south of the country (Mono and Couffou) and in two provinces in the north (Alibori and Borgou). In the days before the session, a research assistant visited the selected village and requested as many volunteers as possible to gather on a scheduled day in a specified location in order to participate in a research project. Participants had to be residents of the village and older than 18 years old, and only one participant per household could take part in the study. On the day of the experiment, the research team randomly selected nine male and nine female participants to take part in the experiment among those who answered the call. Participants who were not selected were paid a show-up fee of CFA 500 ($0.85) and requested to leave. We ran 32 fieldwork sessions, each in a different village (16 treated); a total of 576 individual households took part in the experiment.[5]

During the sessions, the participants convened in a common space—usually a school classroom or a public building—and the experimenter read the experimental instructions aloud. Then each participant was individually called into a separate room where he or she could privately make his or her taking decision as a dictator. To limit possible experimenter effects, we adopted a procedure that makes the dictators' taking choices blind to the experimenter on site.[6] When each participant entered the decision room and before being left alone to make the taking decision, the experimenter asked him or her control questions to verify the correct comprehension of the game instructions. In case a participant could not answer the control questions, the experimenter repeated the instructions in private until the participant was able to provide the correct answers. In addition to the experimental tasks described above, participants took part in an incentivized risk elicitation task, a socio-demographic survey, and additional fieldwork activities not related to this project.[7] A fieldwork session lasted approximately 3 hours. Participants earned, on average, CFA 2800 ($4.5), roughly the equivalent of the wage earned in one and a half days of work for the median subject in our sample.

[5] One participant felt unwell during a session and had to leave before completing the experimental choices. Therefore, we actually collected observations from 575 participants.

[6] 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 ten 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.

[7] In each experimental session, participants took part in the same tasks. The order of the tasks was the same in each session. The tasks were administered in the following order: a public good game, an un-incentivized distributive choice among third parties, the modified dictator game described in this paper, a dice-rolling task to elicit group-level truthful behavior, a battle-of-the-sexes game, a donation decision, an incentivized risk task, and the post-experimental survey. No information regarding the game outcomes was released to the players until the end of the experiment. See for a detailed illustration of the tasks related to other projects that were carried out during this field campaign.

3. RESULTS

3.1 Preliminary analysis

3.1.1 Sample balance and potential confounding factors

Our research design is based on comparing the dictators' taking rate across villages that, ten years before the experiment, had been randomly selected to have the land tenure reform implemented against nonselected villages, which maintain customary land rights to date. For this identification strategy to hold, two caveats are in order. First, we need to show that the random allocation to different property institutions characterizing the PFR lottery successfully eliminated pre-reform differences across treatment branches and that our selection of participants resulted in a balanced sample.

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

Concerning our sample of participants, we collected data from residents of 32 villages randomly selected among those in the RCT pool. In Appendix Table A1, we report descriptive statistics relative to the preregistered socio-demographic characteristics that we collected from the subjects who participated in the experiment. While the sample is well-balanced for most observables, participants in the treated group are, on average, older, slightly more likely to be married and to manage the household's finances, and show a marginally significantly higher literacy rate than those in the control group. To account for these imbalances we control for these characteristics in the analysis. Moreover, as explained in detail when discussing our main results, as a robustness check, we also employ a Lasso post-double-selection methodology for appropriately selecting the controls to be included in the regression (Belloni et al. 2014). This method has proved to be useful in improving the robustness of causal inference when accidental imbalances in the sample occur (Chernozhukov et al. 2018).

Second, we need to verify that, after the reform implementation, participants did not self-select in one of the treatment branches through migration. To do so, we collected data regarding the participants' villages of origin, whether they migrated, the reason for it, and the number of years of residence in the village. Only 35 out of 576 participants were not already a resident in the village when the PFR reform was implemented, 20 in treated villages, and 15 in control villages. The difference is not statistically significant (χ^2 test, $p > 10\%$). Female participants reported the majority of these migrations, and the stated reason was marriage in over 90% of the cases. Similarly, we verified that the number of years that subjects spent in the village where they participated in the experiment was not statistically different between participants in treated and control villages. Similarly, we verified that the fraction of participants who were actually born in the village in which they participated in the experiment does not differ in treated and control villages. Moreover, while we do not have data concerning out-flow migrations, we see that in our sample of villages, the population size is not statistically different between treated and control (2934 versus 2748, respectively, $p = .85$ two-sided t-test). In the regression analysis reported below, we insert a dummy for participants who moved to a village different from the one in which they were born, and we control for the number of years each subject had lived in the village where the data collection took place.

We also elicited from the participants a measure (from 0 to 10) of the importance that they have in the social rank of their communities. On average, participants in our sample have a similar social status situation in treated and control villages (4.45 versus 4.36, respectively; $p = .56$). This information, combined with the similar levels of income and measures of wealth reported by participants across treatments, increases confidence in the fact that we are not picking up experimental subjects that have similar characteristics in treated and control villages but come from different parts of the village population distribution. This could be the case, for instance, because individuals in treated villages are richer and bear higher opportunity costs for taking part in our experiment, and hence our sample could be self-selected towards the tail of the distribution.

Finally, because 10 years have elapsed since the intervention, we checked for possible informational spillovers by asking control village participants whether they knew about the PFR reform. In our sample, only 27% of the participants were aware of the reform. This number is remarkably low considering that at least the members of the village council had at some point voted in favor of being eligible for the reform. This evidence appeases concerns regarding possible spillover effects and confirms that villages in rural Benin are relatively isolated, and villagers generally do not interact much outside the village community.

3.1.2 Taking from locals

As a preliminary step, we estimate the effects of the reform on respect for property among locals. This exercise replicates in a different set of villages and with a larger sample size the results of Fabbri and Dari-Mattiacci (2021). In addition, we vary the source of the passive players' endowment. As shown graphically in Appendix Figure A1, participants in control villages took, on average, 3.76 tokens against 3.34 tokens taken by participants in treated villages. The difference is statistically significant at the conventional level (*t*-test two-sided, $p = .03$), and it becomes strongly significant if we refine the sample to include only those participants who had first-hand experience with the reform (see Section 3.2 for details on how this refined sample is constructed). Results from the regression analysis reported in Appendix Table A2—in which we control for preregistered individual observables, village-level characteristics, and additional socio-demographic controls—confirm this finding.

Result 1 *Replication of Fabbri and Dari-Mattiacci (2021): Formal property rights decrease takings from anonymous individuals from one's own village community (locals).*

3.1.3 Parochialism in taking decisions

We also check whether dictators in our experiment display parochialism—favoring locals over strangers—in respecting the property of others, without for the moment distinguishing between participants in treated and control villages. As shown in Appendix Figure A2, dictators take significantly fewer tokens when the anonymous paired participant is a local (mean = 3.55) than otherwise (mean = 4.66). A two-sided *t*-test rejects the hypothesis that there is no difference between the two sample means at the 1% level. Models 1–3 in Appendix Table A3—in which we regress the number of tokens taken from the passive player adding preregistered individual, village-level, and additional socio-demographic controls, respectively—confirm the result. This finding suggests that participants in our sample display a tendency common in humans to favor, all else equal, locals over strangers (Bernhard et al. 2006; Romano et al. 2017).

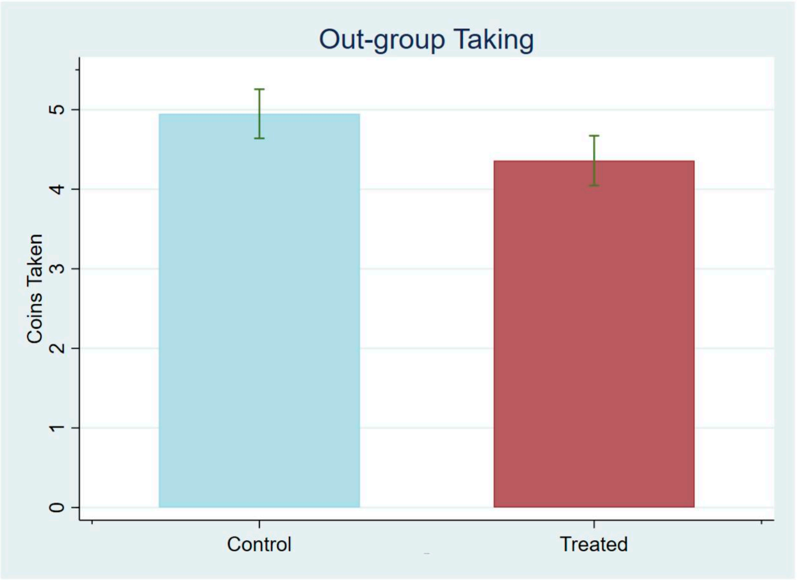

Figure 2. Tokens taken from a participant living in a different village.

3.2 Main result: taking from strangers

We now move to test our main hypothesis, which concerns the impact of the PFR reform on respect for others' property rights when the dictator is paired with an anonymous stranger from a different village. We begin by looking at Figure 2, which shows the average number of tokens taken by dictators in control and treated villages when interacting with strangers (Appendix Figure A5 reports the distribution). Dictators in the control group took, on average, 4.96 tokens from the passive players against the 4.36 taken in the treated group. The difference is statistically significant at the 1% level (t-test two-sided, $p < 1\%$).

We then proceed with testing the hypothesis in a regression framework. As explained, each participant makes two consecutive decisions concerning (1) taking from an individual from the same village (a "local") and (2) taking from a member of a different community (a "stranger"), in random order. The number of tokens taken by the dictator is regressed on the dummy *local* equal to 1 when interacting with a local peer, the treatment dummy, the interaction of these two variables, and a preregistered set of individual controls.[8] We report the results estimated by using a random-effect generalized least square estimator to account for the two sequential decisions reported by each individual subject, with standard errors clustered at the village level.

[8] The preregistered individual controls include: gender, religion, marital status, number of family members, participation in 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 a self-reported measure of weekly household income, the number of bedrooms in the house, and whether the household has running water. However, as noticed by one anonymous Referee, marital status, number of family members, education, literacy, years of residence in the village, and wealth might be endogenous to the reform. Therefore, in Table 1, we reported in Models 2 and 5 the results of the specification as it was preregistered in the pre-analysis plan; and in Models 1 and 4, we report the results of a more parsimonious specification that does not include the aforementioned potentially endogenous controls. Finally, in Models 3 and 6, we 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, and distance from the closest public hospital. Additional wealth controls include: acres of land possessed individually, whether the house has a concrete floor, electricity, radio or television, and whether within the household somebody owns a motorbike, a car, a bank account, or a credit card.

Table 1. Tokens taken by the dictator—ingroup and outgroup interactions.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:		**Whole**			**Refined**	
treated	−0.678**	−0.708**	−0.812***	−0.973***	−1.048***	−1.198***
	(0.343)	(0.309)	(0.310)	(0.371)	(0.349)	(0.343)
local	−1.198***	−1.198***	−1.198***	−1.186***	−1.186***	−1.186***
	(0.193)	(0.194)	(0.195)	(0.215)	(0.216)	(0.217)
treated× local	0.177	0.177	0.177	0.268	0.268	0.268
	(0.235)	(0.236)	(0.238)	(0.245)	(0.246)	(0.248)
Controls:						
Time invariant	Y	Y	Y	Y	Y	Y
Preregistered	N	Y	Y	N	Y	Y
Additional	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)
No. of obs.	1150	1150	1150	912	912	912

Notes: Dependent variable: tokens taken by the dictator. GLS random-effects estimators. Standard errors are robust for clustering at the session level. Compared to Models 1–3, Models 4–6 exclude landless participants in treated villages and participants in control villages who have a formal land property title. Models 1 and 4 include the following controls: age, gender, religion, participation in household finance management, the village of birth, and incentivized measure of risk preferences. Models 2 and 5 include the controls pre-specified in the preregistered analysis plan; the following controls are added to the specification of Model 1: marital status, number of family members, education, literacy, years of residence in the village, income, whether the household has running water, number of bedrooms. Models 3 and 6 add village-level controls and different measures of wealth to Model 2 specification: village population, whether the village has a market, market distance, distance from the closest public school, distance from the closest public hospital, acres of land possessed individually, whether the house has a 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% levels, respectively.

Table 1 reports the results. Model 1 reports the results of a baseline analysis excluding all the pre-specified controls, which might have been affected by the reform, while Model 2 reports the results of the preregistered regression model that includes the controls specified in the pre-analysis plan. The main coefficient of interest is that of the treatment dummy, which isolates the effect of the PFR on dictators' taking rates from a stranger. In both model specifications, the coefficient is negative and statistically significant at the conventional level, and point estimates are similar, suggesting that experiencing the reform significantly increases the respect for the property of participants from other villages. In Model 3, we add a set of village-level controls to the preregistered specification and include a set of proxies for individual wealth. The results are confirmed: the negative point estimate of the treatment dummy increases, and the coefficient becomes statistically significant at the 1% level.

In Models 4–6, we verify whether the results are driven by participants who have first-hand experience with the reform. We use post-experimental survey data to exclude from the analysis participants from treated villages who do not actually own a parcel of land affected by the PFR reform ($n = 82$), as well as participants belonging to control villages who own at least a parcel of land for which they hold a formal title ($n = 37$).[9] Models 4–6 in Table 1 replicate Models 1–3 with this refined sample of participants. The coefficient of the treatment dummy is negative and significant at the 1% level in all model specifications, and the point estimates become larger.

[9] A resident of a treated village might not have been directly affected 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 of 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.

The results of a preregistered heterogeneity analysis also suggest that the reduction in taking rates observed in the treated group is driven by those individuals who benefited the most from the reform.

By awarding formal property titles, the reform also allows rightsholders to enforce their land rights in state courts, a possibility that in the customary system was precluded by the lack of formal proof of land ownership. A post-experimental survey reveals that the vast majority (over 90%) of our participants consider the ruling of state courts as conclusive and superior to that of local customary authorities. However, participants also report relatively high costs of access to state courts, with the average expected cost of solving a case in a state tribunal equaling several months of income for the median subject in our sample (CFA 716,000). These costs are further inflated for those participants who live in remote areas with no paved road connection to the court. In our sample, subjects living a greater distance from paved roads than the sample median report a roughly three-fold increase in the expected costs of a lawsuit compared to those living in the proximity of paved roads (CFA 1,233,000 versus 382,000, respectively).

Indeed, 41% of participants living in the proximity of a paved road are aware of at least one person who solved a land-related conflict by initiating a formal legal procedure in a state tribunal, against a mere 9% among those living far away from paved roads. *De facto*, the reform has most likely had a negligible impact on the land tenure of individuals who face financial and logistical constraints when accessing the formal justice system as compared to the previous customary system. We thus expect milder effects of the reform on the behavior of these subjects as compared to those who can easily access justice. We verify this conjecture by comparing the taking decisions of different subgroups of participants with different possibilities to access the justice system.

We divide the sample of participants according to whether they have a level of self-reported income above or below the sample median ("high" and "low," respectively). We then separately compare the dictators' taking decisions of subjects in the high-income and low-income subgroups across treatments (notice that within each of the high- and low-income subgroups, we are comparing participants who have roughly identical average and median income in treated and control). The results of a two-sided *t*-test are summarized in Table 2. Participants in the high-income subgroups who belong to treated villages took significantly less (at the 1% level) from strangers than those in control villages. Conversely, the difference is not statistically significant if we focus on participants in the low-income subgroup. The same results hold if we characterize participants' affluence by using a composite wealth index of fourteen proxies for individual wealth.[10] Finally, we repeat the analysis by grouping participants according to the distance of their residency to the closest paved road. The results display a similar pattern, with a significant reduction in dictators' taking rate observed only among subjects living in the proximity of roads. In Appendix Tables A4 and A5, we show that these results are confirmed when the main model specification is re-estimated by dividing subjects according to income and distance from paved roads, respectively. This evidence further increases confidence in the fact that the estimated reduction in dictators' taking rate in the treated group is linked to a direct experience with the reform.[11]

[10] Each of the fourteen proxies takes a value in {0;1} so that the wealth index lies in {0;14}. The proxies for individual wealth used are: whether the acres of land possessed individually are above the sample median, whether they have a 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, radio or television, or running water, whether within the household somebody owns a motorbike, a car, a bank account, or a credit card.

[11] In Table A6 in Appendix A, we also performed the preregistered heterogeneity analysis on gender. This analysis is motivated by the evidence reported by Goldstein et al. (2018) that the reform increased tenure security, particularly for women. In line with this evidence, we find that the negative point estimate for the treated dummy is somehow larger for women participants, although the reduction in taking is not statistically different across genders.

Table 2. Tokens taken by the dictator in dtrangers interactions—heterogeneity analysis.

Sample:	**Whole**			**Refined**		
	Treated	**Control**	***p*-value**	**Treated**	**Control**	***p*-value**
High-Income	4.51	5.52	<0.01	4.43	5.59	<0.01
Low-Income	4.53	4.23	0.30	3.89	4.60	0.03
High-Wealth	4.62	5.54	<0.01	4.42	5.63	<0.01
Low-Wealth	4.09	4.46	0.24	3.81	4.48	0.06
High-Road-Dist	5.04	5.16	0.73	4.88	5.20	0.40
Low-Road-Dist	4.05	4.63	0.08	3.85	4.70	0.02

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 is above the sample median, whether high-income, whether the number of bedrooms in the house is 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, 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 the results of a two-sided *t*-test.

We then perform a series of robustness checks. First, we relax assumptions regarding the data-generating process and the data distribution. This robustness analysis accounts for the small number of clusters (32) resulting from clustering at the village-session level. We re-estimated the preregistered model specification described in Table 1 using a double-hurdle regression model with bootstrapped standard errors (Engel and Moffatt 2014). This model better fits the count-data nature of our experimental task by relaxing the assumption of continuously distributed taking outcomes, it accounts for the possibility of never-stealing participants, and it corrects for potential heteroskedasticity and underestimation of standard errors (Cameron et al. 2008). Appendix Table A7, reports the results. The qualitative findings remain the same. Moreover, in Appendix Tables A8 and A9, we also re-estimate the main model specifications using Ordinary Least Squares (OLS) and negative-binomial regression models. The results remain qualitatively the same as in the main model specification.

Second, in three villages in the treated sample, the village authorities reported further extending the original PFR intervention after its 2011 implementation by also including some land plots lying outside the official village borders.[12] In Appendix Table A10, we re-estimated the preregistered model specifications by excluding these three villages from the sample, obtaining qualitative results and point estimates similar to those reported for the basic specification.

Third, in low- and medium-income countries, self-reported income might be a poor indicator of individual affluence (Moser and Felton 2007; Arrow et al. 2012). In Appendix Table A11, we verify whether our estimates are sensitive to the way in which participants' wealth is measured. Accordingly, we re-estimate our main model specifications by including different combinations of proxies for wealth. Results remain quantitatively very similar and qualitatively unchanged.

Finally, as discussed in Section 3.1, a potential problem with our empirical strategy is that some individual characteristics (age, marital status, and, but only marginally, literacy) are

[12] In the PFR intervention completed in 2011, only land plots within the administrative village boundaries were subject to land demarcation and use-rights formalization. Because of this specific feature of the intervention, some villagers were induced to limit long-term investment 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).

unbalanced across treatments. However, the analysis presented above showed that controlling for these observables does not affect the results. We additionally address possible concerns deriving from this imbalance in two ways. First, we show that none of the unbalanced variables is associated with the taking rate. To do so, we show that the average taking rate is statistically the same for participants who are older or younger than the sample median (4.11 versus 4.10, respectively; two-sided t-test $p = 0.98$), married or not married (4.10 versus 4.17, respectively; two-sided t-test $p = 0.73$), and literate or illiterate (4.06 versus 4.13, respectively; two-sided t-test $p = 0.62$).

Second, we re-estimate the models presented in Table 1 employing the Lasso post-double-selection approach proposed by Belloni et al. (2014).[13] This methodology has been proven useful to select in a principled way the controls to be included in a regression when there is accidental imbalance (Belloni et al. 2017; Chernozhukov et al. 2018). Appendix Table A12 reports the results, separating the effects on dictators' decision to take tokens from a stranger (Models 1 and 2) and from a local (Models 3 and 4). The qualitative results remain the same, and point estimates are very similar to those of the main model specification.

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

Result 2 *Formal property rights decrease takings from anonymous individuals from outside one's own village community (strangers).*

We now investigate how our main result on taking from strangers (Result 2) relates to taking from locals (Fabbri and Dari-Mattiacci 2021, replicated in Result 1). We have already remarked above that individuals in our sample exhibit parochialism, that is, they take more from locals than they do from strangers. Did the reform affect their parochial attitudes?

The reduction in takings observed in both groups may reveal a decrease in parochialism, that is, the extent to which people's prosocial behavior decreases with social distance (Enke et al. 2020).[14] To test this hypothesis, we compare taking rates across treatment groups in the strangers and locals conditions. We generate the variable *difftaking*, which is equal to the number of tokens taken from strangers minus the number of tokens taken from locals. While the difference in taking rates is slightly smaller among participants in treated villages (1.02) compared to those in control (1.19), a two-sided t-test cannot reject the hypothesis that *difftaking* is the same in the two samples.

The analysis then replicates the main model specifications, but this time by regressing *difftaking* as the dependent variable. Results are reported in Appendix Table A13. In all model specifications, the coefficient of the treatment dummy is not statistically different from zero. This evidence confirms that the reduction in taking rates induced by the reform had a similar magnitude with locals as with strangers, suggesting that, after the reform, individuals were as parochial as they were prior to it.[15]

[13] We additionally replicated the results discussed here by using the Lasso post-regularization methodology proposed by Chernozhukov et al. (2015) and developed it as STATA package by Ahrens et al. (2018). Results are virtually identical.

[14] The experimental literature on parochialism 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 et al. 2018), to the best of our knowledge, we are the first to employ the *taking* dictator game in an ingroup vs. outgroup framework.

[15] 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.

4. HOW PROPERTY AFFECTS VALUES

What are the determinants of the reduction in takings induced by the introduction of formal property rights? In this section, we present and discuss a number of explanations that our empirical strategy was designed to probe.

4.1 Socio-economic context

First, we look at whether the reform affected the socio-economic environment in ways that have been recognized to contribute to an individual's respect for the property of others. We start with human-capital accumulation, which is generally associated with higher pro-social behavior. For instance, Galiani and Schargrodsky (2010) show that awarding formal property rights to Argentinian squatters causally increased investments in offspring's education. 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. In our sample, human-capital investments are very limited, with only 36% of the participants having basic literacy skills. On average, participants went to school for 1 year, with negligible differences between treated and control groups. As discussed in Section 3.1, neither education levels nor literacy rates are associated with the dictators' taking rate in our sample. Models 1 and 2 of Appendix Table A15 replicate the main regression presented in Table 1 by excluding education and literacy rate as controls using the whole and refined samples, respectively. 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 Appendix Table A12. The results and point estimates remain virtually unchanged, suggesting that human capital investments do not play a relevant role here.

Another socio-economic factor that could explain the observed increase in taking aversion is 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 levels of takings. For instance, one may conjecture that richer people have less of a need to steal. We thus verify whether participants in treated villages had experienced an increase in wealth or access to credit that might have mediated the lower taking rate. Participants' self-reported income levels, as well as any of the other 14 indicators of wealth collected, are statistically the same in treated and control villages. These results are consistent with previous evidence on the reform's short- and medium-term impact on income levels (Goldstein et al. 2018; Fabbri and Dari-Mattiacci 2021). Moreover, in Models 3 and 4 of Appendix Table A15, we re-estimate the main regression of Table 1 by excluding income and proxies for wealth using the whole and refined samples, respectively. Results remain qualitatively the same and point estimates remain similar to those of the main model specification. Furthermore, in Models 5 and 6, we also estimate the same regressions by additionally excluding education and literacy: results remain qualitatively unchanged. These results suggest that variations in income and wealth are unlikely to explain the observed reduction in taking.

Finally, we verify whether the increase in respect for 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 Appendix Table A16, we re-estimate the main model specifications by additionally controlling for the number of self-reported conflicts experienced by participants in the previous 10 years. The results remain virtually unchanged, suggesting no mediating effects of conflicts on the observed taking behavior.

4.2 Moral values

We entertain the possibility that the reform affected moral values of honesty and altruism, which may have a bearing on taking behavior. To study whether this is the case, we ran two auxiliary experiments that have been widely used to measure social preferences and moral behavior. First, we study whether the observed reduction in taking results from the reform's influence on individuals' moral attitudes toward cheating (Abeler et al. 2019). To measure honesty, we followed Jiang (2013) and had participants take part in a variant of the dice-rolling task introduced by Fischbacher and Föllmi-Heusi (2013). In this task, subjects are asked to privately throw a six-face dice 10 times and then report the outcomes to the experimenter. Subjects are paid linearly in the outcome of one randomly chosen roll, CFA 100 if the outcome is 1; 200 if 2; and so on, up to the maximum payment of 600 if 6 is reported. Since the experimenter does not observe the outcome of the dice rolls, a participant can inflate his or her payoff by over-reporting. However, deviations from the statistically predicted mean outcome—both at the individual and at the group level—can be interpreted as a sign of dishonesty.

Appendix Table A17 shows the results.[16] Consistent with what has been observed in other dice-rolling experiments, participants in our sample inflate their payoff by significantly over-reporting the outcome of their rolls as compared to the statistically predicted mean of 3.5 (the average outcome reported is 3.85, two-sided t-test $p<1\%$). However, we detect no significant difference in the average reported outcome between the treated and control villages (3.83 versus 3.88, respectively, two-sided t-test $p=.53$).

Second, we inquire into whether the reduction in taking in the treated sample reflects a more general increase in altruism toward strangers. To this end, participants took part in an incentivized standard dictator game framed as a donation to an unspecified charity that, as we emphasized in the instructions, is located outside of the village.[17] As graphically displayed in Appendix Figure A6, the average donation rate for treated and control participants is very similar (3.66 in treated versus 3.70 in control). A two-sided t-test cannot reject the hypothesis that the mean donation is the same across treatment groups. A similar result comes from our post-experimental survey, where we asked our participants whether they would support a hypothetical redistribution of land from more wealthy individuals to those in need.[18] The share of individuals who supported the redistribution was very similar in the treated and control groups (22% versus 18%, respectively, $p=.21$).

4.3 Social preferences

We further explore whether the reform may have impacted social preferences in a way that fostered respect for the property of strangers. In a companion study using the 2020 data, Fabbri (2022) has shown that villagers who experienced the reform are more likely to cooperate with anonymous strangers from other communities in a public good game. Moreover, another study based on a previous data-collection campaign in Benin has shown that the PFR has determined an increase in cooperation and trust within communities sufficiently connected to courts so that they could, in fact, benefit from the reform (Fabbri 2021). Here,

[16] We collected data relative to exactly ten dice rolls from 447 subjects. The missing subjects did not report all of the required ten outcomes from the dice rolls, provided inconsistent or ambiguous reports, or refused to take part in this experimental task (apparently due to religious or social stigma toward dice gambling in some communities).

[17] All dictators' offers have been donated to an orphanage in Cotonou.

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

we test whether the reduction in taking from strangers that we document in treated villages correlates with these forms of pro-social behavior.

First, we test whether there is an association between taking from strangers (the main experiment of this article) and cooperating with them in a public good game (the experiment in Fabbri 2022). We regress the coins taken from strangers on the contribution in a multilevel public good game played with strangers, which is the measure of outgroup cooperation employed by Fabbri (2022), and add the usual set of controls. Results are reported in Appendix Table A18. The negative coefficient of the contribution-to-outgroup variable suggests a negative association at the individual level between cooperating with outgroups and taking rate. However, the coefficient is not statistically different from zero.

Second, we consider the association between ingroup prosociality and taking from strangers. To do so, we use the data of a linear public good game that subjects in our sample played with members of their own village,[19] and regress taking from strangers on ingroup cooperation. The results are reported in Appendix Table A19. The coefficient of the ingroup cooperation variable is not statistically different from zero. However, it should be noted that, albeit insignificant, this coefficient is positive in all model specifications. Taken together, these two null results hint toward a possible channel for the reduction in taking that we observe: respect for the property of strangers might be negatively associated with the strength of ingroup ties, which, in turn, would be in line with the literature emphasizing that kin closeness impairs the development of impersonal institutions (Greif 1994; Schulz et al. 2019). We emphasize that our results on this channel remain unclear and further research is needed.

4.4 Labor as a claim to ownership

Another channel we explore focuses on whether the formalization of property rights interacts with the way in which individuals lay claims to things. It has been argued that one of the strongest behavioral mechanisms supporting the notion of property leverages its intimate connection with labor and just deserts (Nozick 1974; Locke 2015).[20] A plausible explanation for the reduction in taking following the reform might thus be that the formalization of property rights *justifies* ownership as legitimate. That is, awarding formal property rights could induce individuals to believe that if one owns something, he or she must have deserved it, possibly because he or she worked hard to obtain it.

We investigate this possibility by varying the origin of the passive players' endowment and informing dictators whether the passive players acquired their endowment by means of luck or through an effort task (the task is described in Section 2.2, footnote 4). In line with the previous experimental evidence, dictators took a significantly larger share of the passive players' endowment when the latter was windfall money rather than money earned in an effort task. A two-sided *t*-test suggests that the difference is strongly statistically significant both if we consider taking from strangers (4.27 versus 5.05) and if we focus on taking from locals (3.23 versus 3.87).

We then verify whether the observed reduction in the taking rate in treated villages is related to the source of the passive players' endowment. First, we generate the variable *totaltaking* by summing up the tokens taken by each participant in both the locals and the strangers condition. We then compare this variable across treatments distinguishing between merit and luck as the source of the passive player's endowment. The reduction in *totaltaking*

[19] The details of this game are reported in Fabbri (2022).

[20] Experimental evidence shows that when subjects gain their endowment through an effort task, dictators playing a taking game are less likely to take (List 2007; Jakiela 2011; Korenok et al. 2018; Faillo et al. 2019).

displayed by participants in treated villages is similar in the merit and luck conditions and, in both cases, statistically significant (tokens 0.99, equal to a 12% reduction, and 1.07, equal to an 11% reduction, respectively). Restricting the analysis to the sample of participants directly affected by the PFR reform returns similar results.

Second, we differentiate between taking from locals and taking from strangers. Appendix Table A14 replicates the same regression models presented in Table 1, separately estimating the treatment effects when luck or merit is the source of the passive player's endowment. We perform *F*-tests for the equality of regression coefficients of the interaction between the treatment dummy and the *luck* and *merit* variables, both when the interaction takes place between participants of the same village and participants belonging to different villages.[21] Results confirm that the reduction in taking rates displayed by participants in the treated villages is registered both in the merit and in the luck conditions, and that the magnitudes of the estimated effects are statistically the same across the two conditions. Moreover, the treatment effect is roughly similar in the luck and merit conditions both when the passive player is a local and when he or she is a stranger.

4.5 Coordination of expectations

A key goal of the law is to help people coordinate toward desirable behaviors. Often, perhaps most commonly, coordination is achieved without enforcement. It has long been recognized that the law has such an "expressive" function (Sunstein 1996; Cooter 1998), and this is particularly true for property rights (Maynard Smith and Price 1973; Sugden 1989; Fabbri et al. 2021). By introducing private property rights, the reform might have made the notions of "mine" and "yours" salient, thereby inducing a sense of entitlement in property owners and a corresponding tendency to respect that property in those who come in contact with it (Wilson 2020). We provide some preliminary evidence of the plausibility of this channel by investigating whether the reform affected the individuals' ability to coordinate around mutually beneficial outcomes in a situation characterized by multiple equilibria.

To do so, we employ a modified battle-of-the-sexes game with the typical two asymmetric strategies (with payoffs equal to 700 and 100) and an additional symmetric but inefficient strategy (with payoffs equal to 200 for both players) similar to the game used in Jackson and Xing (2014). Players were assigned either the Row or Column role (which remained the same for the duration of the experiment) and had the possibility to choose among three strategies/colors. If the two players chose the same color, they earned positive payoffs. If they chose different colors, they earned zero. This game has three Nash equilibria (NE) in pure strategy and four in mixed strategy. Focusing on the NE in pure strategy, the two asymmetric equilibria are efficient but inequitable. In the symmetric equilibrium, the total payoff is equal to half of the payoff generated in the asymmetric NE, but players earn the same amounts. Vice versa, in the asymmetric equilibria, one player earns more than the other, but the total is larger.

The experimental parameters, possible strategies, and combinations of payoffs are summarized in Figure 3. Each player made one choice in each of the two different conditions (the order in which conditions were presented was randomized). In the "prompt" conditions, before choosing their strategies, participants received a prompt consisting of an observation of a color that corresponds to one of the labels of the three possible actions. In particular, during the explanation of the game instructions in the prompt condition, the experimenter

[21] Therefore, we run the following four tests of equality of the regression coefficients: (Treated*stranger*Luck) − (Control*stranger*Luck) = 0; (Treated*stranger*Merit) − (Control*stranger*Merit) = 0; (Treated*local*Luck) − (Control*local*Luck) = 0; (Treated*local*Merit) − (Control*local*Merit) = 0.

		Column Participant		
		Purple	*Orange*	*Green*
Row Participants	**Purple**	**100** ; 700	**0** ; 0	**0** ; 0
	Orange	**0** ; 0	**700** ; 100	**0** ; 0
	Green	**0** ; 0	**0** ; 0	**200** ; 200

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

reproduced the payoffs summarized in Figure 3 using physical coins and pieces of colored textiles. The wooden table used to place textiles and coins was covered by an orange tablecloth. In the "base" condition, the same instructions were provided, but the wooden table was not covered by any tablecloth.[22] Accordingly, the Row player was advantaged by the prompt, in that coordination on the orange equilibrium makes Row earn his or her highest payoff, while Column is disadvantaged.

This experimental design was originally introduced by Jackson and Xing (2014) to examine how culture affects coordination. They showed that Indian subjects coordinated more on the asymmetric equilibrium (especially in the presence of the prompt) while US participants coordinated on the symmetric equilibrium, thus suggesting a role for culture in coordinating around a particular focal point. Similarly, in our setup, we investigate if experiencing formalized property rights changes the participants' ability to coordinate and achieve non-zero payoffs. In fact, similarly to culture, legal provisions might also act as a coordination device, which affects people's expectation of what is considered "the right action" to undertake and to expect as much from others (Hayek 1973). We then verify whether villagers' improved coordination mediates the reduction in the taking rate we observed in treated villages.

To test this conjecture, we first estimate the following regression equation using a Probit model:

$$c_i = \alpha + \delta_T T_i + \boldsymbol{X_i} + \epsilon_i \tag{2}$$

where c_i is a dummy equal to one when coordination on a non-zero NE in pure strategy is achieved, T_i is a dummy equal to 1 for subjects in treated villages, and X_i is the vector of the individual- and village-level characteristics specified above. Table 3 reports the results, where Models 1–6 replicate the inclusion of controls and the sample refining, as discussed in the analysis of the main results in Table 1.

Participants in treated villages who make decisions in the baseline conditions are statistically as likely as those in control to coordinate on one of the equilibria entailing non-zero payoffs, as suggested by the insignificant coefficient of the dummy *treated*. However, when the prompt is introduced, the likelihood to coordinate for participants in treated villages becomes significantly larger at the 10% level.

We investigate further this result. As specified in the pre-analysis plan, we separately check how individuals who were advantaged and disadvantaged by coordinating on the prompt-suggested equilibrium reacted to its introduction. Specifically, we estimated the likelihood that a player opted for the choice of the color that yields her the highest payoff if

[22] Following Jackson and Xing (2014), we did not call attention to the color as a coordinating device in any way: the instructions provided were identical in the two conditions, and both of them offered the visual 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 study participants had a common signal that could be used as a cue, thus mimicking something that may be focal in the real world, but without feeling pressured by the experimenter to act in a specific way.

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	−0.060	−0.048	−0.139	−0.066	−0.043	−0.138
	(0.175)	(0.170)	(0.175)	(0.177)	(0.179)	(0.186)
prompt	−0.319**	−0.324**	−0.327**	−0.301*	−0.306*	−0.307*
	(0.147)	(0.148)	(0.149)	(0.162)	(0.162)	(0.163)
treated×prompt	0.366*	0.375*	0.373*	0.382*	0.393*	0.395*
	(0.199)	(0.200)	(0.198)	(0.211)	(0.214)	(0.212)
Controls:						
Time invariant	Y	Y	Y	Y	Y	Y
Preregistered	N	Y	Y	N	Y	Y
Additional	N	N	Y	N	N	Y
Constant	0.032	−0.095	0.076	0.190	0.166	0.367
	(0.405)	(0.412)	(0.421)	(0.440)	(0.431)	(0.467)
No. of obs.	1150	1150	1150	912	912	912

Notes: Dependent variable: dummy = 1 when the two individuals achieve coordination. Random-effects Probit estimators. Standard errors are robust for clustering at the session level. Compared to Models 1–3, Models 4–6 exclude landless participants in treated villages and participants in control villages who have a formal land property title. Models 1 and 4 include the following controls: age, gender, religion, participation in household finance management, the village of birth, and incentivized measure of risk preferences. Models 2 and 5 include the controls specified in the preregistered analysis plan; the following controls are added to the specification of Model 1: marital status, number of family members, education, literacy, years of residence in the village, income, whether the household has running water, number of bedrooms. Models 3 and 6 add village-level controls and different measures of wealth to Model 2 specification: village population, whether the village has a market, market distance, distance from the closest public school, distance from the closest public hospital; acres of land possessed individually, whether the house has a 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% levels, respectively.

coordination is achieved, that is, "Row chooses orange" and "Column chooses purple." Table 4 reports the results of Probit regressions, distinguishing the analysis of the Row players—who were advantaged by the prompt—in Models 1–3 from that of the Column players—who have been disadvantaged by it—in Models 4–6.

The results of Models 1–3 show that players advantaged by the prompt (the Row players) in treated and control villages were equally likely to choose the strategy that resulted in the highest own payoff in case coordination was achieved. Conversely, the negative and significant interaction term *treated*prompt* in Models 4–6 shows that Column participants in treated villages—who were disadvantaged by the introduction of the prompt—were significantly less likely to choose the strategy maximizing their own payoff compared to those in control villages. These results suggest that the estimated increase in coordination on an equilibrium resulting in non-zero payoffs is driven by the behavior of those individuals who are less likely to pursue a self-interested payoff maximizing strategy when a coordination asymmetry disadvantageous for them is introduced. Indeed, 67% of the participants in control villages that were disadvantaged by the prompt nonetheless played the strategy maximizing their individual payoff in case of coordination, against a significantly lower share of 55% in treated villages (chi-square test, $p = 0.04$).

We then verify whether social coordination is linked to respect for the property of others. We test whether, at the individual level, a participant's ability to coordinate is associated with a reduction in taking rates. We regress the dictators' taking rate on a dummy equal to one if a subject achieved coordination in the Battle-of-the-Sexes game and the usual set of controls. The results are reported in Appendix Table A20. The coefficient of the coordination dummy is negative in all model specifications, suggesting that individuals who coordinate on average take less, however, the result is not statistically significant at the

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	0.022	0.003	−0.009	−0.081	−0.032	−0.054
	(0.191)	(0.194)	(0.193)	(0.162)	(0.162)	(0.177)
prompt	−0.019	−0.018	−0.020	0.269***	0.280***	0.286***
	(0.115)	(0.116)	(0.118)	(0.063)	(0.064)	(0.067)
treated×prompt	−0.183	−0.185	−0.188	−0.301**	−0.317**	−0.327**
	(0.181)	(0.184)	(0.187)	(0.153)	(0.157)	(0.159)
Controls:						
Time invariant	Y	Y	Y	Y	Y	Y
Preregistered	N	Y	Y	N	Y	Y
Additional	N	N	Y	N	N	Y
Constant	0.369	−0.152	−0.417	−0.650	−0.158	−0.151
	(0.418)	(0.472)	(0.495)	(0.580)	(0.591)	(0.565)
No. of obs.	576	576	576	574	574	574

Notes: Dependent variable: dummy = 1 when the individual chooses the strategy that maximizes her own payoff in case coordination is achieved. Probit estimators. Standard errors are robust for clustering at the session level. Compared to Models 1–3, Models 4–6 exclude landless participants in treated villages and participants in control villages who have a formal land property title. Models 1 and 4 include the following controls: age, gender, religion, participation in household finance management, village of birth, and incentivized measure of risk preferences. Models 2 and 5 include the controls pre-specified in the preregistered analysis plan; the following controls are added to the specification of Model 1: marital status, number of family members, education, literacy, years of residence in the village, income, whether the household has running water, number of bedrooms. Models 3 and 6 add village-level controls and different measures of wealth to Model 2 specification: village population, whether the village has a market, market distance, distance from the closest public school, distance from the closest public hospital; acres of land possessed individually, whether the house has a 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% levels, respectively.

conventional level. We then focus again on the subsample of participants who were disadvantaged by the prompt. Recall that we found that in treated villages these participants are less likely to choose the strategy that maximizes their own payoff compared to controls. On average, the taking rate of participants who were disadvantaged by the prompt and did not choose the strategy that maximizes their own payoff in the coordination game is lower compared to one of the remaining players disadvantaged by the prompt who, nevertheless, chose the maximizing individual payoff strategy (4.78 versus 5.05 coins taken, respectively)—albeit the sample size is halved and the result is not statistically significant. Nonetheless, this result goes in the direction of suggesting that experiencing the property rights reform induced subjects who had been disadvantaged by an exogenous allocation of the prompt/property rights to be more willing to respect the status quo.

As a final step, we checked whether the increased acceptance of the status quo observed among disadvantaged subjects could be mediated by the increase in utilitarian values that the reform determined among treated villagers, as shown by two of us in a parallel study (Dari-Mattiacci and Fabbri 2023). Indeed, utilitarianism may be associated with a more widespread acceptance of the dispossession of land and unequal outcomes deriving from the commodification of rights. The reform might have facilitated a shift from considering land use as a communal right to viewing land as a private commodity that can be traded in the marketplace. However, our results do not support this conjecture. In our sample, individuals' utilitarianism is not significantly associated with their enhanced acceptance of a disadvantageous property rights allocation.[23]

[23] We explored the conjecture above by verifying whether subjects who reported utilitarian views—when answering a non-incentivized survey on moral dilemmas run during the same field visit and described in detail in Dari-Mattiacci and Fabbri (2023)—were less likely to pursue a self-interested payoff maximizing strategy when a coordination asymmetry

To sum up, the data offers only partial support for the conjecture that respecting others' property rights is a form of social coordination. The results of the tests performed hint toward this conjecture but the evidence is not conclusive.

5. CONCLUSIONS

Property rights are grounded in the notion that third parties ought to recognize them. Wilson (2020: 175) has recently noted that "property is more than just an individual claim. It is rather a socially shared practice which implies the joint 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?

We show that there is 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 from our article is that the introduction of private property rights strengthens both impartial pro-market institutions and the values supporting them. The effects that we estimated are not mediated by socioeconomic advancements, morality, and merit considerations. We find suggestive but weak evidence that formal property rights may "tell people which expectations they can count on and which not" (Hayek 1973: 579), thus inducing individuals to coordinate around non-conflictual outcomes. Further analysis also hints at the weakening of kinship ties, which could in turn shape attitudes toward strangers, as a second possible channel—albeit the effects are imprecisely estimated. In sum, our data fall short of properly identifying a single channel and do not exclude the presence of alternative or concurrent mechanisms. We leave a more systematic investigation of the mechanisms behind our findings to future research.

We conclude with a cautionary note concerning possible pernicious effects of property rights. Heller and Salzman (2021) caution that individuals may base their claim to ownership on mutually incompatible grounds. When property is up for grabs, conflicts may ensue. Property rights may put under stress weak state institutions that are struggling to arbitrate such conflicts (Gambetta 1996; Bandiera 2003). Further research may identify the institutional conditions necessary for formal property rights to reinforce pro-market values without fueling conflicts, and assess the long-term effects of their introduction.

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disadvantageous for them is introduced. In Table A21 in Appendix A, we replicate the models presented in Table 4, augmented by the introduction of a new regressor: whether subjects are categorized as "utilitarian" in responding to the moral survey questions. Results do not report a significant association between participants' enhanced acceptance of a disadvantageous property rights allocation and utilitarianism.

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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).

	PFR reform ($n = 288$)	**Control** ($n = 288$)	**Difference** (p-**value**)
male	0.49	0.51	0.74
age	40.0	36.8	.01
muslim	0.45	0.41	0.28
vodoun	0.19	0.18	0.91
married	0.89	0.83	0.03
householdnr	9.8	10.0	0.69
managefinance	0.99	0.97	0.02
literate	0.40	0.33	0.08
bornvillage	0.69	0.72	0.41
yearsinvillage	32.3	30.9	0.24
weekly income (CFA)	9,026	8,468	0.59
landuse (Hect)	5.47	5.10	0.65
concretefloor	0.64	0.59	0.23
electricity	0.36	0.36	0.99
water	0.26	0.18	0.01
radio–TV	0.63	0.63	0.99
car	0.09	0.07	0.28
moto	0.77	0.78	0.67
bank-acc	0.33	0.27	0.12
social-rank	4.45	4.36	0.56

Table A2. Tokens taken from a participant living in the same village (replication of Fabbri and Dari-Mattiacci 2021).

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:		**Whole**			**Refined**	
treated	−0.548*	−0.637**	−0.674**	−0.772**	−0.889**	−0.959***
				(0.354)	(0.348)	(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	1.131	1.093	0.857	0.896	1.088
	(0.766)	(0.880)	(0.913)	(0.958)	(1.100)	(1.151)
No. of obs.	575	575	575	575	575	575

Notes: Dependent variable: tokens taken by the dictator. OLS regression. Standard errors are 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, Models 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 in household finance management, education, literacy, thevillage 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 a 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% levels, respectively.

Table A3. Tokens taken from same-village versus other-village participants & tokens taken in treated versus control villages.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
stranger	1.110***	1.110***	1.110***			
	(0.119)	(0.119)	(0.120)			
treated				−0.620**	−0.692**	−0.723***
				(0.279)	(0.274)	(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	1.131	1.093	0.857	0.896	1.088
	(0.766)	(0.880)	(0.913)	(0.958)	(1.100)	(1.151)
No. of obs.	575	575	575	456	456	456

Notes: Dependent variable: tokens taken by the dictator. OLS regression. Standard errors are robust for clustering at the session level. Compared to Models 1 and 3, Models 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 in household finance management, education, literacy, the 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 a 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% levels, respectively.

Table A4. Tokens taken by the dictator—outgroup interactions—heterogeneous effects of income.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:		**Whole**			**Refined**	
Treated	−0.449	−0.498	−0.494	−0.978**	−1.080**	−1.022**
	(0.389)	(0.357)	(0.349)	(0.406)	(0.412)	(0.396)
income_d	0.862**	0.765*	0.688	0.808**	0.861**	0.729
	(0.349)	(0.399)	(0.424)	(0.395)	(0.403)	(0.435)
Treated×income_d	−0.506	−0.603	−0.655	−0.233	−0.218	−0.365
	(0.410)	(0.423)	(0.406)	(0.489)	(0.496)	(0.476)
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**	1.847**	1.711*	1.655**	1.634	1.653
	(0.737)	(0.907)	(0.886)	(0.830)	(1.041)	(1.072)
No. of obs.	575	575	575	456	456	456

Notes: Dependent variable: tokens taken by the dictator. OLS estimators. Standard errors are 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, Models 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 in household finance management, education, literacy, the 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 a 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% levels, respectively. *income_d*=1 if the household's income is larger than the sample median.

Table A5. Tokens taken by the dictator—outgroup interactions—heterogeneous effects of distance from paved roads.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:		**Whole**			**Refined**	
Treated	−0.780	−0.756	−0.839*	−1.197**	−1.167**	−1.249**
	(0.511)	(0.505)	(0.485)	(0.514)	(0.506)	(0.498)
dist_d	0.352	0.415	0.367	0.287	0.338	0.269
	(0.555)	(0.510)	(0.504)	(0.583)	(0.527)	(0.535)
Treated×dist_d	0.592	0.505	0.632	0.838	0.624	0.751
	(0.676)	(0.722)	(0.729)	(0.751)	(0.847)	(0.861)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	1.282**	1.440**	1.426**	0.858	0.905	1.121
	(0.785)	(0.954)	(0.941)	(0.864)	(1.103)	(1.125)
No. of obs.	575	575	575	456	456	456

Notes: Dependent variable: tokens taken by the dictator. OLS estimators. Standard errors are 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, Models 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 in household finance management, education, literacy, the 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 a 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% levels, respectively. *dist_d*=1 if the distance from the paved road is larger than the sample median.

Table A6. Tokens taken by the dictator—outgroup interactions—heterogeneous effects of gender.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:		**Whole**			**Refined**	
Treated	−0.737**	−0.819**	−0.746**	−1.261***	−1.414***	−1.377***
	(0.293)	(0.328)	(0.331)	(0.343)	(0.346)	(0.349)
male	−0.406	−0.427	−0.468	−0.381	−0.454	−0.521
	(0.453)	(0.436)	(0.471)	(0.452)	(0.422)	(0.470)
Treated×male	0.091	0.142	−0.052	0.401	0.513	0.397
	(0.489)	(0.462)	(0.464)	(0.557)	(0.496)	(0.515)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	1.301**	1.248**	1.156*	0.903	0.738	0.952
	(0.733)	(0.923)	(0.929)	(0.823)	(1.054)	(1.104)
No. of obs.	575	575	575	456	456	456

Notes: Dependent variable: tokens taken by the dictator. OLS estimators. Standard errors are 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, Models 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 in household finance management, education, literacy, the 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 a 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% levels, respectively.

Table A7. Tokens taken by the dictator—outgroup interactions—double-hurdle estimator with bootstrap (Engel and Moffatt 2014).

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:		**Whole**			**Refined**	
treated	−0.822**	−0.682**	−0.834**	−1.090***	−1.092***	−1.241***
	(0.291)	(0.259)	(0.304)	(0.347)	(0.301)	(0.370)
Controls:						
Time invariant	Y	Y	Y	Y	Y	Y
Preregistered	N	Y	Y	N	Y	Y
Additional	N	N	Y	N	N	Y
Constant	2.269***	1.070	0.966	1.679**	0.470	0.251
	(0.813)	(1.028)	(0.962)	(0.942)	(1.024)	(1.160)
sigma	2.624***	2.578***	2.591***	2.510***	2.493***	2.475***
	(0.13)	(0.091)	(0.091)	(0.133)	(0.125)	(0.111)
No. of obs.	575	575	575	456	456	456

Notes: Dependent variable: tokens taken by the dictator. Double hurdle estimators, block-bootstrap with 50 repetitions. The coefficients display the effect of being in a treated village on taking coins from the receiver, conditional on being an individual type who is willing to take from others. Standard errors are robust for clustering at the session level. Compared to Models 1–3, Models 4–6 exclude landless participants in treated villages and participants in control villages who have a formal land property title. Models 1 and 4 include the following controls: age, gender, religion, participation in household finance management, the village of birth, and incentivized measure of risk preferences. Models 2 and 5 include the controls pre-specified in the preregistered analysis plan; the following controls are added to the specification of Model 1: marital status, number of family members, education, literacy, years of residence in the village, income, whether the household has running water, number of bedrooms. Models 3 and 6 add village-level controls and different measures of wealth to Model 2 specification: village population, whether the village has a market, market distance, distance from the closest public school, distance from the closest public hospital; acres of land possessed individually, whether the house has a 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% levels, respectively.

Table A8. Tokens taken by the dictator—outgroup interactions—OLS estimator.

	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**
Sample:		**Whole**			**Refined**	
treated	−0.691**	−0.747**	−0.773**	−1.057***	−1.146***	−1.170***
	(0.306)	(0.319)	(0.306)	(0.346)	(0.357)	(0.341)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	1.279*	1.221	1.167	0.791	0.622	0.852
	(0.725)	(0.920)	(0.921)	(0.796)	(1.043)	(1.089)
No. of obs.	575	575	575	456	456	456

Notes: Dependent variable: tokens taken by the dictator. OLS estimators. Standard errors are robust for clustering at the session level. Compared to Models 1 and 3, Models 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 in household finance management, education, literacy, the 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 a 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% levels, respectively.

Table A9. Tokens taken by the dictator—outgroup interactions—negative binomial regression.

	Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**	**Model 6**
Sample:		**Whole**			**Refined**	
treated	−0.154**	−0.166**	−0.171**	−0.234***	−0.256***	−0.261***
	(0.066)	(0.069)	(0.067)	(0.077)	(0.080)	(0.076)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	0.566***	0.547**	0.524***	0.375	0.327	0.371
	(0.215)	(0.249)	(0.237)	(0.261)	(0.297)	(0.292)
lnalpha	−2.011***	−2.030***	−2.112***	−2.103***	−2.143***	−2.260***
	(0.305)	(0.305)	(0.327)	(0.383)	(0.392)	(0.440)
No. of obs.	575	575	575	456	456	456

Notes: Dependent variable: tokens taken by the dictator. Negative binomial regression. Standard errors are robust for clustering at the session level. Compared to Models 1 and 3, Models 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 in household finance management, education, literacy, the 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 a 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% levels, respectively.

Table A10. 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	−0.620*	−0.674**	−0.713**	−0.978**	−1.070***	−1.136***
	(0.343)	(0.342)	(0.340)	(0.397)	(0.397)	(0.383)
local	−1.198***	−1.198***	−1.198***	−1.186***	−1.186***	−1.186***
	(0.194)	(0.195)	(0.196)	(0.216)	(0.217)	(0.218)
treated×	0.065	0.065	0.065	0.131	0.131	0.131
local	(0.245)	(0.245)	(0.246)	(0.249)	(0.250)	(0.251)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	1.526**	1.368	1.237	1.230	0.941	1.082
	(0.685)	(0.827)	(0.823)	(0.796)	(0.954)	(1.052)
No. of obs.	1042	1042	1042	820	820	820

Notes: Dependent variable: tokens taken by the dictator OLS regression, GLS random-effects estimators. Standard errors are robust for clustering at the session level. Compared to Models 1 and 3, Models 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 in household finance management, education, literacy, the 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 a 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% levels, respectively.

Table A11. 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	−0.754**	−0.790**	−0.782**	−1.100***	−1.137***	−1.163***
	(0.328)	(0.324)	(0.318)	(0.351)	(0.349)	(0.335)
local	−1.198***	−1.198***	−1.198***	−1.186***	−1.186***	−1.186***
	(0.194)	(0.195)	(0.194)	(0.216)	(0.217)	(0.216)
treated×	0.177	0.177	0.177	0.268	0.268	0.268
local	(0.236)	(0.237)	(0.237)	(0.247)	(0.247)	(0.247)
sec-rank	0.095			0.100		
	(0.062)			(0.087)		
land-owned		0.023*			0.029*	
		(0.014)			(0.017)	
bedrooms		0.032			0.026	
		(0.047)			(0.050)	
cement-floor		0.105			0.054	
		(0.332)			(0.397)	
electricity		0.319			0.295	
		(0.276)			(0.302)	
water		0.395**			0.566**	
		(0.182)			(0.248)	
media			−0.150			−0.157
			(0.194)			(0.235)

(continued)

Table A11. (continued)

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:		**Whole**			**Refined**	
car			0.562*			0.984**
			(0.339)			(0.419)
motorbike			0.663**			0.577*
			(0.288)			(0.302)
credit			0.138			0.330
			(0.234)			(0.255)
Other Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	Y	Y	Y	Y	Y	Y
Constant	1.836**	1.998**	1.880**	1.502	1.582	1.603*
	(0.781)	(0.862)	(0.751)	(0.970)	(1.042)	(0.937)
No. of obs.	1150	1150	1150	912	912	912

Notes: Dependent variable: tokens taken by the dictator. GLS random-effects estimators. Standard errors are robust for clustering at the session level. Individual Controls include: age, gender, religion, marital status, number of family members, participation in household finance management, education, literacy, the 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% levels, respectively.

Table A12. Tokens taken by the dictator—selection of controls using lasso post-double-selection approach (Belloni et al. 2014).

	Model 1	Model 2	Model 3	Model 4
Taking Decision:	**outgroup**		**ingroup**	
Sample:	**Whole**	**Refined**	**Whole**	**Refined**
treated	−0.737**	−1.050***	−0.610**	−0.799**
	(0.358)	(0.597)	(0.305)	(0.533)
managemoney	2.699***	2.926***	2.111***	
	(0.319)	(0.375)	(0.457)	
population	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
marketinvillage 0.532	0.622	0.675**	0.863**	
	(0.381)	(0.431)	(0.329)	(0.385)
marketdistance	−0.025	−0.025	−0.032	−0.010
	(0.076)	(0.076)	(0.062)	(0.063)
state-edu	0.014	0.034	−0.073	−0.063
	(0.078)	(0.081)	(0.048)	(0.048)
state-health	0.019	0.016	0.024	0.022
	(0.044)	(0.048)	(0.038)	(0.044)
Constant	2.196***	2.000***	1.554**	3.488***
	(0.581)	(0.597)	(0.674)	(0.533)
No. of obs.	575	457	575	457

Notes: Dependent variable: tokens taken by the Dictator. Regularized post-double-selection lasso regression. Standard errors are robust for clustering at the village level. High-dim individual controls included: age, gender, religion, marital status, whether polygamous, number of family members, participation in 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 a 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% levels, respectively.

Table A13. Difference between outgroup and ingroup taking rates.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:		**Whole**			**Refined**	
treated	−0.143	−0.111	−0.098	−0.285	−0.257	−0.211
	(0.207)	(0.224)	(0.197)	(0.213)	(0.234)	(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	0.090	0.074	−0.065	−0.274	−0.236
	(0.490)	(0.604)	(0.651)	(0.588)	(0.657)	(0.709)
No. of obs.	575	575	575	456	456	456

Notes: Dependent variable: difference in tokens taken by the Dictator when interacting with an outgroup and an ingroup partner, respectively. OLS estimators. Standard errors are robust for clustering at the session level. Compared to Models 1 and 3, Models 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 in household finance management, education, literacy, the 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 a 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% levels, respectively.

Table A14. Tokens taken by the dictator—outgroup interactions—different sources of passive player's endowment.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:		**Whole**			**Refined**	
Treated	−0.642	−0.655*	−0.636*	−1.078**	−1.011**	−0.973**
	(0.384)	(0.358)	(0.329)	(0.447)	(0.422)	(0.395)
effort	−0.620	−0.529	−0.471	−0.868**	−0.726*	−0.648
	(0.393)	(0.396)	(0.379)	(0.411)	(0.423)	(0.412)
Treated×effort	−0.037	−0.150	−0.237	−0.008	−0.246	−0.375
	(0.635)	(0.656)	(0.678)	(0.634)	(0.705)	(0.759)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	2.250***	1.521	1.458	1.140	0.914	1.156
	(0.709)	(0.946)	(0.958)	(0.834)	(1.084)	(1.129)
No. of obs.	575	575	575	456	456	456

Notes: Dependent variable: tokens taken by the dictator. OLS estimators. Standard errors are 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, Models 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 in household finance management, education, literacy, the 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 a 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% levels, respectively.

Table A15. Tokens taken by the dictator—excluding education and income.

Sample:	Model 1 Whole	Model 2 Refined	Model 3 Whole	Model 4 Refined	Model 5 Whole	Model 6 Refined
treated	−0.811**	−1.199***	−0.745**	−1.075***	−0.741**	−1.081***
	(0.319)	(0.351)	(0.329)	(0.356)	(0.341)	(0.369)
local	−1.198***	−1.186***	−1.198***	−1.186***	−1.198***	−1.186***
	(0.195)	(0.217)	(0.194)	(0.216)	(0.194)	(0.216)
treated×	0.177	0.268	0.177	0.268	0.177	0.268
local	(0.237)	(0.248)	(0.236)	(0.246)	(0.236)	(0.246)
education			−0.131	−0.235*		
			(0.120)	(0.138)		
literacy			0.434	0.592		
			(0.385)	(0.444)		
logincome	0.048	−0.010				
	(0.103)	(0.098)				
Wealth-C.	Y	Y	N	N	N	N
Other-C.	Y	Y	Y	Y	Y	Y
Constant	1.614**	1.208	2.222***	1.874**	2.187***	1.642*
	(0.736)	(0.965)	(0.778)	(0.916)	(0.686)	(0.838)
No. of 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 in household finance management, the 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 a 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% levels, respectively.

Table A16. 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	−0.671**	−0.705**	−0.807***	−0.991***	−1.062***	−1.205***
	(0.339)	(0.309)	(0.309)	(0.370)	(0.349)	(0.343)
local	−1.198***	−1.198***	−1.198***	−1.186***	−1.186***	−1.186***
	(0.193)	(0.194)	(0.195)	(0.215)	(0.216)	(0.217)
treated×	0.177	0.177	0.177	0.268	0.268	0.268
local	(0.235)	(0.236)	(0.238)	(0.245)	(0.246)	(0.248)
conflicts	−0.115	−0.079	−0.132	0.288	0.313	0.199
	(0.477)	(0.431)	(0.405)	(0.437)	(0.411)	(0.385)
Controls:						
Individual	Y	Y	Y	Y	Y	Y
Village	N	Y	Y	N	Y	Y
Wealth_Add	N	N	Y	N	N	Y
Constant	2.658***	1.762**	1.724**	2.167***	1.457*	1.590
	(0.582)	(0.686)	(0.819)	(0.627)	(0.784)	(1.004)
No. of obs.	1150	1150	1150	912	912	912

Notes: Dependent variable: tokens taken by the dictator. GLS random-effects estimators. Standard errors are robust for clustering at the session level. Compared to Models 1 and 3, Models 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 in household finance management, education, literacy, the 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 a 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% levels, respectively.

Table A17. 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	0.052	0.046	0.020	−0.006	−0.009
	(0.104)	(0.104)	(0.100)	(0.098)	(0.100)	(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***	3.626***	3.642***	3.659***	3.585***	3.664***
	(0.378)	(0.354)	(0.356)	(0.385)	(0.349)	(0.360)
No. of obs.	447	447	447	447	447	447

Notes: Dependent variable: average outcome reported for ten dice rolls. OLS regression. Standard errors are 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, Models 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 in household finance management, education, literacy, the 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 a 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% levels, respectively.

Table A18. Tokens taken by the dictator and cooperation with outgroups.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:		**Whole**			**Refined**	
Outgroupcoop	−0.029	−0.024	−0.031	−0.015	−0.014	−0.026
	(0.048)	(0.045)	(0.046)	(0.066)	(0.061)	(0.060)
Controls:						
Time invariant	Y	Y	Y	Y	Y	Y
Preregistered	N	Y	Y	N	Y	Y
Additional	N	N	Y	N	N	Y
Constant	1.991***	1.362*	1.313	1.991***	1.362*	1.313
	(0.586)	(0.745)	(0.974)	(0.586)	(0.745)	(0.974)
No. of obs.	575	575	575	456	456	456

Notes: Dependent variable: tokens taken by the dictator. OLS regression. Standard errors are robust for clustering at the session level. Compared to Models 1–3, Models 4–6 exclude landless participants in treated villages and participants in control villages who have a formal land property title. Models 1 and 4 include the following controls: age, gender, religion, participation in household finance management, the village of birth, and incentivized measure of risk preferences. Models 2 and 5 include the controls pre-specified in the preregistered analysis plan; the following controls are added to the specification of Model 1: marital status, number of family members, education, literacy, years of residence in the village, income, whether the household has running water, number of bedrooms. Models 3 and 6 add village-level controls and different measures of wealth to Model 2 specification: village population, whether the village has a market, market distance, distance from the closest public school, distance from the closest public hospital; acres of land possessed individually, whether the house has a 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% levels, respectively.

Table A19. Tokens taken by the dictator and cooperation with ingroups.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:		**Whole**			**Refined**	
Ingroupcoop	0.076	0.066	0.071	0.063	0.056	0.064
	(0.057)	(0.049)	(0.047)	(0.061)	(0.054)	(0.054)
Controls:						
Time invariant	Y	Y	Y	Y	Y	Y
Preregistered	N	Y	Y	N	Y	Y
Additional	N	N	Y	N	N	Y
Constant	1.607**	1.067	0.975	1.607**	1.067	0.975
	(0.588)	(0.760)	(0.995)	(0.588)	(0.760)	(0.995)
No. of obs.	575	575	575	456	456	456

Notes: Dependent variable: tokens taken by the dictator. OLS regression. Standard errors are robust for clustering at the session level. Compared to Models 1–3, Models 4–6 exclude landless participants in treated villages and participants in control villages who have a formal land property title. Models 1 and 4 include the following controls: age, gender, religion, participation in household finance management, the village of birth, and incentivized measure of risk preferences. Models 2 and 5 include the controls pre-specified in the preregistered analysis plan; the following controls are added to the specification of Model 1: marital status, number of family members, education, literacy, years of residence in the village, income, whether the household has running water, number of bedrooms. Models 3 and 6 add village-level controls and different measures of wealth to Model 2 specification: village population, whether the village has a market, market distance, distance from the closest public school, distance from the closest public hospital; acres of land possessed individually, whether the house has a 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% levels, respectively.

Table A20. Tokens taken by the dictator and coordination.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:		**Whole**			**Refined**	
coord	−0.154	−0.161	−0.163	−0.149	−0.151	−0.158
	(0.165)	(0.164)	(0.163)	(0.176)	(0.175)	(0.174)
Controls:						
Time invariant	Y	Y	Y	Y	Y	Y
Preregistered	N	Y	Y	N	Y	Y
Additional	N	N	Y	N	N	Y
Constant	2.034***	1.282*	1.302	1.469*	0.920	1.155
	(0.595)	(0.743)	(0.928)	(0.702)	(0.903)	(1.169)
No. of obs.	1150	1150	1150	912	912	912

Notes: Dependent variable: tokens taken by the dictator. GLS regression. Standard errors are robust for clustering at the session level. Models 1 and 4 include the following controls: age, gender, religion, participation in household finance management, the village of birth, and incentivized measure of risk preferences. Models 2 and 5 include the controls pre-specified in the preregistered analysis plan; the following controls are added to the specification of Model 1: marital status, number of family members, education, literacy, years of residence in the village, income, whether the household has running water, number of bedrooms. Models 3 and 6 add village-level controls and different measures of wealth to Model 2 specification: village population, whether the village has a market, market distance, distance from the closest public school, distance from the closest public hospital; acres of land possessed individually, whether the house has a 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% levels, respectively.

Table A21. Utilitarianism and 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**		
utilitarian	−0.107	−0.150	−0.151	0.110	0.141	0.143
	(0.177)	(0.165)	(0.158)	(0.161)	(0.174)	(0.169)
prompt	0.080	0.082	0.081	0.117	0.120	0.120
	(0.148)	(0.149)	(0.151)	(0.137)	(0.143)	(0.147)
utilitarian× prompt	−0.253	−0.256	−0.258	−0.000	−0.001	0.002
	(0.185)	(0.188)	(0.189)	(0.196)	(0.202)	(0.208)
Controls:						
Time invariant	Y	Y	Y	Y	Y	Y
Preregister-ed	N	Y	Y	N	Y	Y
Additional	N	N	Y	N	N	Y
Constant	0.515	0.030	−0.209	−0.785	−0.296	−0.281
	(0.448)	(0.476)	(0.476)	(0.515)	(0.555)	(0.539)
No. of obs.	576	576	576	574	574	574

Notes: Dependent variable: dummy = 1 when the individual chooses the strategy that maximizes her own payoff in case coordination is achieved. Probit estimators. Standard errors are robust for clustering at the session level. Compared to Models 1–3, Models 4–6 exclude landless participants in treated villages and participants in control villages who have a formal land property title. Models 1 and 4 include the following controls: age, gender, religion, participation in household finance management, village of birth, and incentivized measure of risk preferences. Models 2 and 5 include the controls pre-specified in the preregistered analysis plan; the following controls are added to the specification of Model 1: marital status, number of family members, education, literacy, years of residence in the village, income, whether the household has running water, number of bedrooms. Models 3 and 6 add village-level controls and different measures of wealth to Model 2 specification: village population, whether the village has a market, market distance, distance from the closest public school, distance from the closest public hospital; acres of land possessed individually, whether the house has a 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% levels, respectively.

APPENDIX A.1: SUPPLEMENTARY FIGURES

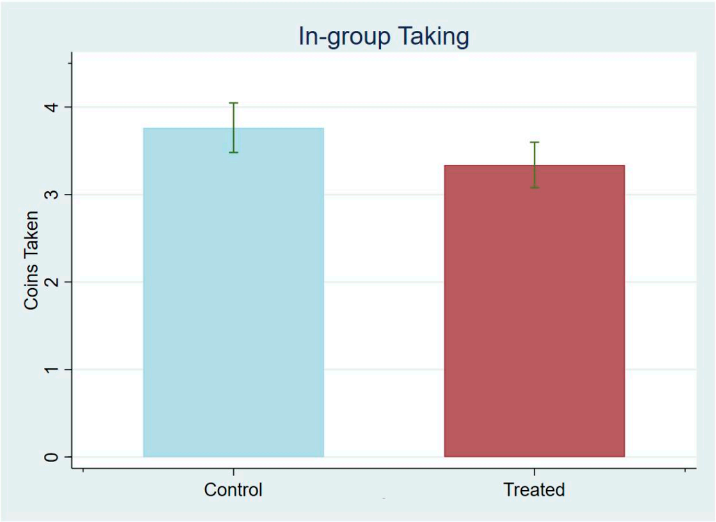

Figure A1. Tokens taken from a participant living in the same village (replication of Fabbri and Dari-Mattiacci 2021).

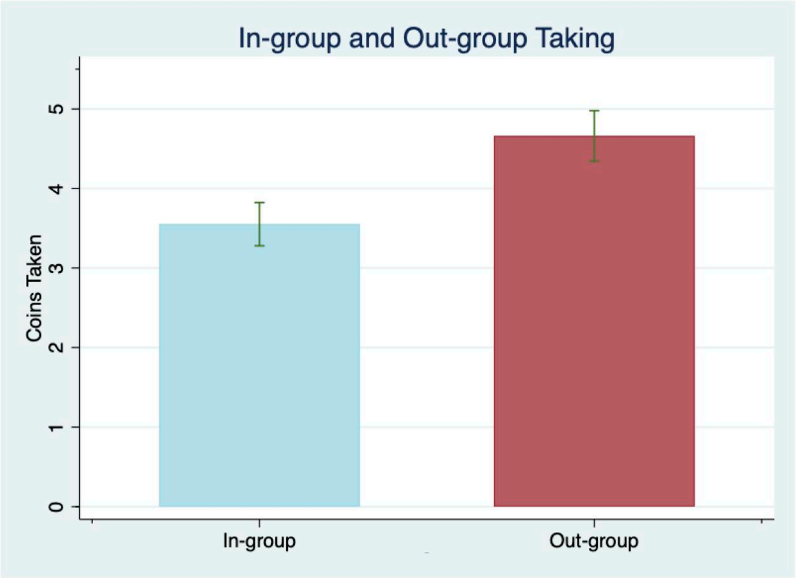

Figure A2. Average tokens taken either from a local or from a stranger.

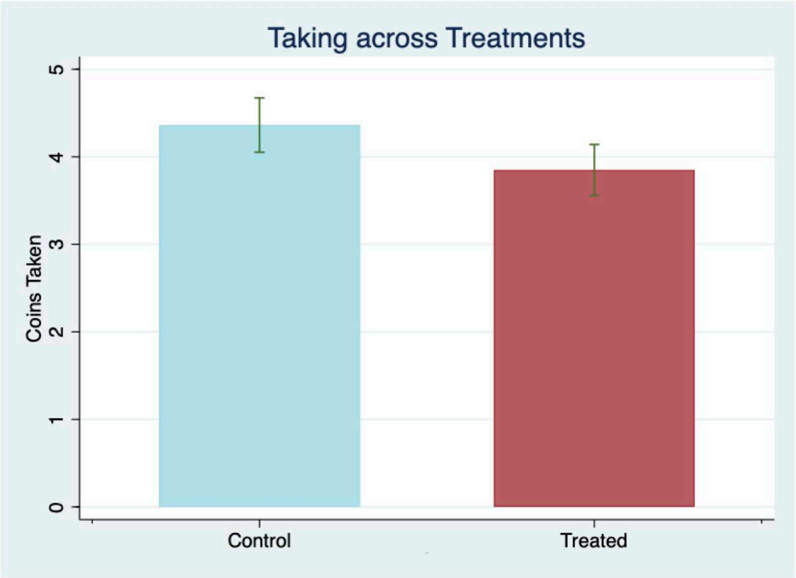

Figure A3. Average tokens taken in treated and control villages.

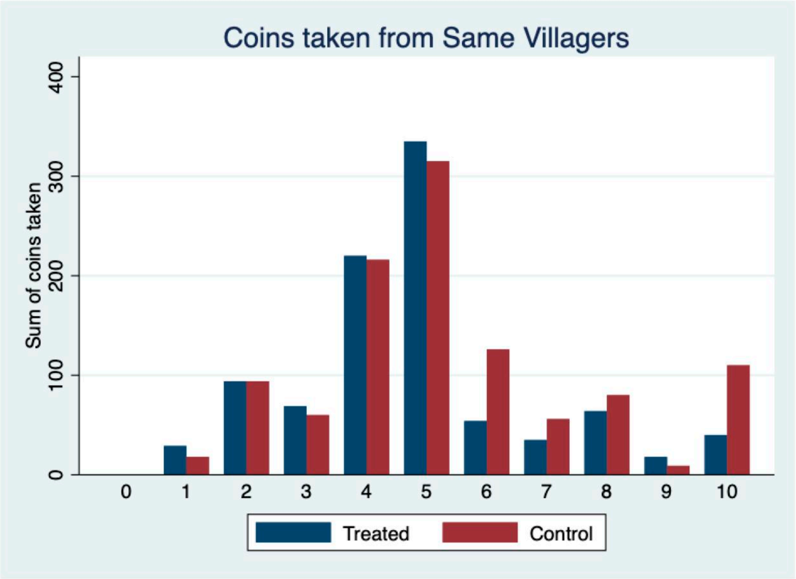

Figure A4. Distribution of tokens taken from locals in both treated and control villages.

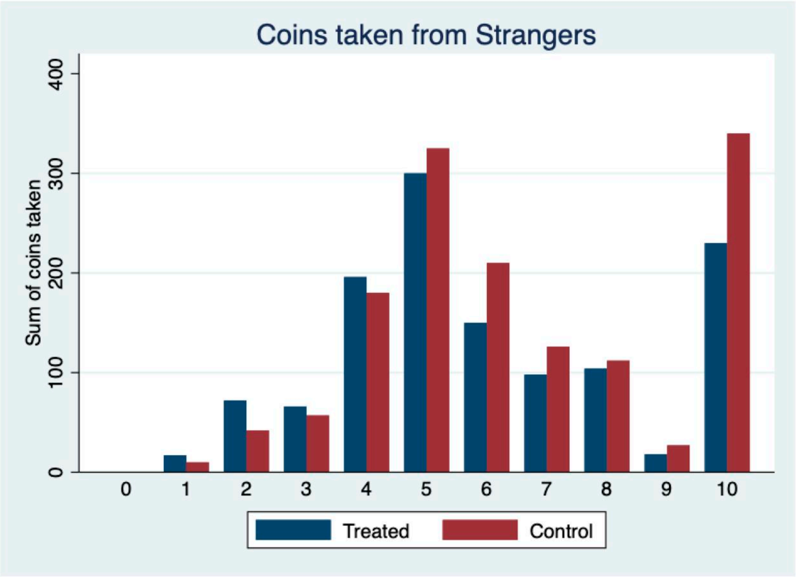

Figure A5. Distribution of tokens taken from strangers in both treated and control villages.

Figure A6. Tokens taken from a participant living in the same village.

APPENDIX B: INSTRUCTIONS

General instructions

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

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

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

Activity 1

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

Merit treatment

Participant A has the possibility to work in order to earn 10 tokens. To earn the 10 tokens, Participant A will need to successfully complete a work assignment. Specifically, Participant A will receive a plastic box and 200 toothpicks. The plastic box has a little hole on top. Participant A has 10 min to place all 200 toothpicks inside the box from the top hole. If Participant A manages to complete the work assignment within 10 min, he/she receives the 10 tokens. Otherwise, he/she will not receive any token for this part of the study.

Participant B initially has zero tokens. If Participant A earned the 10 tokens, Participant B can take 0, 1, 2, etc., up to 10 tokens from Participant A. The final outcome of this activity is: for Participant A, the tokens left by Participant B; for Participant B, the tokens taken from Participant A. If Participant A did not manage to complete the work assignment within ten minutes, both Participants get zero.

Luck treatment

Participant A receives 10 tokens from the experimenter for free. Participant B initially has zero tokens. Participant B can take 0, 1, 2, etc., up to 10 tokens from Participant A. The final earnings of this activity are: for Participant A, the tokens left by Participant B; for Participant B, the tokens taken from Participant A.

What is your role?

We do not know yet whether you will be Participant A or B. We ask you to *work and complete the work assignment as if you are Participant A, and we also ask you to*[24] choose how many tokens you want to take from your partner as if you were Participant B. At the end of the assignment, we will randomly assign you either the role of Participant A or the role of Participant B.

Who is your partner in this activity?

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

How are your earnings in this activity calculated?

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

Activity 2

The decisions you will make and the earnings you will collect in this second activity are completely unrelated to those of the activity that you have just completed. As in the previous activity, in this activity, there are again two types of participants: Participant A and Participant B.

Merit treatment

As before, Participant A has the possibility to work in order to earn 10 tokens: Participant A has 10 min to place all the 200 toothpicks inside the box from the top hole, and he/she will receive zero tokens if the work assignment is not completed within 10 min.

Luck treatment

As before, participant A receives 10 tokens from the experimenter for free.

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

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

What is your role?

As before, we do not know yet whether you will be Participant A or B. We ask you to *work and complete the work assignment as if you were Participant A, and we also ask you to choose how many*

[24] Merit treatment only.

tokens you want to[25] choose how many tokens you want to take from your partner as if you were Participant B. At the end of the assignment, we will randomly assign you either the role of Participant A or the role of Participant B.

Who is your partner in this activity?

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

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

How are your earnings in this activity calculated?

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

APPENDIX C: THE BENIN PFR AND SOCIAL PREFERENCES PROJECT

The present article is part of a broader research agenda by one of us (Marco Fabbri). This appendix provides an overview of the projects carried out so far over two distinct field visits in Benin.

First wave of 2017

In 2017, two research teams visited in total 32 Beninese villages (16 treated) randomly selected among the pool of villages participating in the original RCT, for a total of 515 subjects participating in the experiments. In each of these villages, the following set of experiments were conducted: a public good game, a trust game, an incentivized elicitation of social norms, a risk elicitation in both the losses and gains domains, a donation decision. In half of the 32 villages, following the aforementioned set of experiments, 254 participants additionally took part in a modified dictator game with takings. All games were played among participants from the same village.

These projects led to the following publications:

- Fabbri and Dari-Mattiacci (2021) report the results from a lab-in-the-field experiment using a modified dictator game with takings. This study explores how formalized property rights affect taking aversion. Contrary to the present article, in this experiment, takings were possible only from participants belonging to the same village.
- Fabbri (2021) reports the results from two lab-in-the-field experiments using a public good game and a trust game. This study finds that cooperation and trust substantially increase in those villages served by paved roads that grant better access to institutions and government services introduced by the reform.

Second wave of 2020

In 2020, the research team visited a larger sample of 32 randomly selected villages (16 treated) for a total of 576 subjects. Altogether, the second field campaign administered several experiments in the following order: a public good game played with both villagers and strangers, a non-incentivized distributive choice among third parties, the modified dictator game described in this article, a dice-rolling task to elicit group-level truthful behavior, a modified battle-of-the-

[25] Merit treatment only.

sexes game, a donation decision, an incentivized risk task, and the post-experimental survey which also included a series of moral vignettes exploring moral dilemmas.[26]

The main experiment described in the present article is based on a modified dictator game with takings that differs from the one used in the 2017 study in as much as: (1) it tests explicitly with a new treatment whether formalized property rights increase the respect of strangers' property as well; (2) it studies multiple potential transmission channels by using further experiments and observational data; and (3) it uses a different subject pool (more participants sampled from different villages) and it implements a role-reversal protocol that doubles the number of observations.

The present article reports for the first time the findings of the main experiment (the modified dictator with takings from strangers) as well as of the two ancillary experiments: the dice-rolling experiment testing honesty and the modified battle-of-the-sexes experiment testing coordination. All experiments were preregistered. 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 preregistered at the AEA RCT Registry at the same time of the main experiment (IDs AEARCTR-0005324 and AEARCTR-0005319, respectively).

This second wave led so far to the following publications:

- Dari-Mattiacci and Fabbri (2023) report the results of an experiment using vignettes that presented subjects with ethical dilemmas modeled after the well-known Trolley Problem. This study explores how formalized property rights affect morality.
- Fabbri (2022) illustrates the results of a multilevel public good game experiment with participants both from within and outside the same village. The study explores how property rights affect the ability to cooperate with strangers outside the restricted circle of close-knit communities.
- Fabbri and Bigoni (2021) report the results of a lab-in-the-field distributional task as in Almås et al. (2020) to explore how formalized property rights affect the distributional preferences of subjects.

Finally, Arruñada et al. (2022) is an observational study that uses both the 2017 and 2020 data to assess the impact of formal property rights on litigation.

[26] No information regarding the game outcomes were released to the players until the end of the experiment.