

## THE VIRTUOUS CYCLE OF PROPERTY

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*Abstract*—This paper shows that formalizing private property rights has a positive effect on the propensity to respect the property of others. We study a recent large-scale land tenure reform in West Africa that was the first of its kind to be implemented as a randomized control trial. Results of a modified dictator game show that the formalization of private property rights reduced an individual's willingness to take from others' endowment. We used additional experimental measures and postexperimental survey data to rule out alternative explanations for the observed behavior that do not imply a change in preferences.

### I. Introduction

WHILE there is mounting evidence of the importance of property rights in economic growth and societal development (Acemoglu, Johnson, & Robinson, 2005; Mokyr, 2009; North, 1991), the contours of the concept of property vary, sometime sharply, across disciplines (Hare, Reeve, & Blossey, 2016; Hodgson, 2015; Merrill & Smith, 2001). Yet a common denominator in any definition of property rights is the notion of respect for the property of others, that is, the idea that under a system of property rights, an individual will—spontaneously or out of fear of being punished—refrain from interfering with another's enjoyment of their own property.

Respect for property is considered a key determinant of economic development because it increases the private returns on entrepreneurship in societies where egalitarian norms may act against individual wealth accumulation (Barr

& Stein, 2008; Bernard, De Janvry, & Sadoulet, 2010; Platteau, 2000). More generally, respect for property fosters the emergence and supports the functioning of a market economy, and consequently spurs economic growth (Glaeser et al., 2004).

The literature identifies three possible channels through which respect for property is induced: first-, second-, and third-party enforcement. Starting from the last, third-party enforcement institutions, such as a (formal) legal system or (informal) sanctions triggered by social norms of behavior, provide powerful incentives for would-be takers. Alternatively, possessors may invest in self-protection and display aggressive defensive behavior against potential intruders, following a behavioral pattern that is common not only among humans (Johnson & Toft, 2014; Pape, 2003) but also in several animal species (Kokko, 2013). Virtually any legal system justifies reasonable self-defense of one's own property and various forms of self-help, thereby allowing direct, second-party enforcement. Finally, a burgeoning literature investigates behavior based on an intrinsic motivation to not appropriate another's property. Social scientists report that such an internalization mechanism—alternatively labeled first-party enforcement or taking aversion—is at work in human societies (see, e.g., Bardsley, 2008; Cappelen et al., 2013; Krupka & Weber, 2013; List, 2007; for a survey of the literature reporting evidence of first-party enforcement and a thorough methodological discussion see Faillo, Rizzolli, & Tontrup, 2019).

As compared to the other two mechanisms described above, first-party enforcement yields specific efficiency gains. On the one hand, setting up and maintaining a formal third-party enforcement system requires substantial investments in monitoring and sanctioning institutions, such as police and courts. Even in systems based on informal sanctions, punishment is costly, might be suboptimally provided because of free-riding, and could give rise to arbitrariness, discrimination, and welfare-reducing social norms (Arruñada, Zanzarone, & Garoupa, 2018; Fabbri & Carbonara, 2017; Grechenig, Nicklisch, & Thöni, 2010; Herrmann, Thöni, & Gächter, 2008). On the other hand, second-party enforcement presupposes costly precautions by the owner (Ayres & Levitt, 1998; Heaton et al., 2016), as well as potential losses arising from conflicts between the owner and potential intruders, which might (and often do) degenerate in devastating and

Received for publication June 17, 2019. Revision accepted for publication December 11, 2019. Editor: Shachar Kariv.

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We thank the editor, Shachar Kariv, and two anonymous referees for comments and suggestions that greatly improved the quality of this paper. We are grateful to the Behavioural Approach to Contract and Tort research program at Erasmus University Rotterdam; the Innovation Program at the Erasmus School of Law; and the Erasmus Trustfonds for generous financial support. We are deeply indebted to Michael Faure for his support; this project would have been not possible without him. A special thanks to Deo-Gracias Houndolo for his amazing organizational skills and his support during the fieldwork. We are also grateful to Benito Arruñada, Christoph Engel, Jonah Gelbach, Michael Heller, Jon Klick, Justin McCrary, Giacomo Ponzetto, Matteo Rizzolli, Alexander Stremitzler, Alessandro Tarozzi, and participants at seminars and workshops at the Erasmus University Rotterdam, ETH Zurich, Harvard, University of Amsterdam, and University Pompeu Fabra for many insightful comments. Calixte Adjallala, Kosmas Alofa, Baithatou Amidou, Dossou Fiofio, Gaston Gnonlonfon, Rabitou Gounou, Idrissou Soulé, Mohamed Sedou, Alou Sanni, Rosemonde Sohantode, and Israelia Zannou provided excellent research assistance. Finally, we thank Malissa Bales for her skilled editorial assistance. The usual disclaimer applies.

A supplemental appendix is available online at [https://doi.org/10.1162/rest\\_a\\_00905](https://doi.org/10.1162/rest_a_00905).

long-lasting feuds with reciprocal retaliations (Bolle, Tan, & Zizzo, 2014; Nikiforakis, 2008).

When driven by internalized norms of behavior, respect for property frees up resources that owners would otherwise invest in self-protection and saves the costs associated with formal and informal punishment institutions. For these reasons, scholars are paying increasing attention to the determinants of first-party enforcement. Some authors claim that humans are characterized by an innate sense of property (Sääksvuori et al., 2016; Zeki, Goodenough, & Stake, 2004). Others theorize that respect for property results from the evolutionary and societal forces that shaped our behavior over centuries (Gintis, 2007; Eswaran & Neary, 2014). Despite these important theoretical contributions, only a few recent papers empirically investigate the determinants of first-party enforcement. Notably, Jakiela (2015) and Jakiela, Miguel, and Te Velde (2015) show that educational attainments and market integration are important determinants of increased first-party enforcement of respect for property rights on earned income. Fabbri, Rizzolli, and Maruotti (2018) show that individuals respect significantly more strongly the property of those who acquired it through labor rather than luck or first possession, which nicely aligns with Locke's theory of property (Henry, 1999; Locke, 2014, 1860). However, we are not aware of any empirical study investigating the relationship between the structure and organization of formal institutions and respect for property.

Our study contributes some new pieces of this puzzle by estimating the causal effects of a major reform of property rights on the social preferences governing an individual's propensity to appropriate others' property, as captured in an economic experiment. The reform, implemented by the government of Benin with the support of the Millennium Challenge Corporation and the World Bank between 2009 and 2011 and whose details are provided in the next section, transformed collective informal customary rights over land into formal individual rights akin to private property. Estimating the univariate causal effects of the reform on respect for property faces an empirical challenge, since preferences and institutions are endogenously codetermined variables that coevolve.

A way to overcome the identification problem consists of using natural experiments to study the effects of institutional shocks on preferences.<sup>1</sup> In the context of policy and legal reforms, this approach has been criticized for potential endogeneity biases because the institutional change might actually reflect the preferences of the institution builders rather than represent an endogenous shock (Alesina & Giuliano, 2015). A second set of studies uses laboratory experiments to solve the identification problem.<sup>2</sup> A concern with this approach relates to the external validity of the results, because the in-

stitutions manipulated in the lab bear little resemblance to real-life property institutions and because of the small—and often nonrepresentative—samples of participants (Henrich, Heine, & Norenzayan, 2010; Loewenstein, 1999; Schram, 2005).

We attempt to overcome these problems by proposing an innovative research design that combines laboratory experiments with a unique case of institutional reform implemented as a large-scale randomized control trial. Our identification strategy is based on the peculiar process of implementation that characterized the Beninese land rights reform: the villages in which the reform was implemented were randomly selected from a pool of hundreds of Beninese villages. We then collected data from a lab-in-the-field experiment designed to measure respect for property in both selected and nonselected villages. Compared to previous related studies, our approach has the advantage of relying on a clean identification strategy while mitigating external validity concerns.

The laboratory setting in which we measure the effects of experiencing the land tenure reform on the villagers' respect for property guarantees anonymity and silences the influence of second- and third-party enforcement institutions, as well as other possible confounding factors. Economic experiments are a widely used tool to elicit individual and social preferences (Charness & Rabin, 2002; Croson & Gächter, 2010; Smith, 1994). Our experiment is based on a variant of the traditional dictator game, in which a participant has the opportunity to take some or all of the resources from another passive player's endowment at no cost. The game is designed to measure preferences affecting an individual's willingness to take others' property. This modified dictator game has often been used in previous literature (see, e.g., Dreber et al., 2013; Khadjavi, 2015; Korenok, Millner, & Razzolini, 2018; Oxoby & Spraggon, 2008).

Results from our experiment show that participants who experienced the land tenure reform appropriate significantly less from others than those belonging to the control group. Point estimates suggest a 40% to 60% reduction in the share of endowment that treated subjects take from others. We consider several alternatives to the explanation that experiencing formal individual land rights directly alters social preferences by increasing respect for the property of others. We tested whether, in our sample, experiencing the land tenure reform has affected subjects' observed behavior via channels such as wealth effects, investments in education, access to credit, and likelihood to participate in financial activities. We report evidence suggesting that none of these channels can explain the results. Similarly, we show that neither the likelihood to experience conflicts nor use formal versus customary institutions for conflict resolution differs between treated and control villages, thus providing evidence against the possibility that changes in these factors explain the observed behavior. We also report evidence that the reform did not affect

<sup>1</sup>Examples of (allegedly exogenous) institutional shocks used in previous literature are political events (Alesina & Fuchs-Schündeln, 2007; Botticini & Eckstein, 2007), changes in laws and regulations (Gruber & Hungerman, 2008), and modifications of state borders (Becker et al., 2016).

<sup>2</sup>The approach consists of observing the participants' reactions to exogenous manipulations of the institutional characteristics of the games played

as, for instance, in Bó, Foster, and Putterman (2010), Rodríguez-Sickert, Guzmán, and Cárdenas (2008), Sutter, Haigler, and Kocher (2010).

participants' level of altruism, beliefs about the level of others' altruism, or perception of distributional norms in the society, thus showing that our results cannot be explained by changes in altruism or beliefs regarding the existing social norms. Finally, we show that our results cannot be explained by changes in other individual beliefs related to the establishment of a market economy, such as individualism, self-determination, or the importance of money.

From a methodological perspective, our work is directly related to studies that have used the taking dictator game in experiment conducted in low- and medium-income countries (Barr et al., 2015; Jakiela, 2011). The paper is also related to the literature that employs laboratory experiments to evaluate the impact of development interventions (Ban, Gilligan, & Rieger, 2015; Fabbri, 2018; Jakiela, 2014; Lucas et al., 2014; Paluck & Green, 2009). Similarly, we contribute to a branch of the literature that uses laboratory games to compare the behavior of subjects living in different institutional settings (Bigoni et al., 2016; Henrich et al., 2001; Herrmann et al., 2008). Finally, we contribute to recent studies that combine natural experiments to determine assignment to treatment with laboratory experiments to elicit participants' preferences (Fisman et al., 2015; Gneezy, Leibbrandt, & List, 2016; Voors et al., 2012).

The paper is structured as follows. In the section II, we describe the institutional framework in which the study takes place. Section III presents the experimental design and procedure, and section IV reports the results obtained. Section V discusses whether the empirical evidence supports possible alternative explanations to the hypothesis that the reform increased individual preferences for respect for property. Section VI concludes.

## II. Institutional Framework

While systems of formal land ownership registration have been introduced in virtually every African state, customary land rights still represent the predominant land tenure arrangement in most rural areas, including Benin. Customary land rights are characterized by a complex set of principles and regulations that are typically defined at the village or local level. While customary arrangements vary widely, they have a number of key features in common (Delville, 2006).

Customary rights consist of a set of socially determined land-use rules, where access to land is an integral part of the structure of society and tenure is determined by socio-political and family relationships. Governance and enforcement are left to local authorities, such as village elders, religious authorities, and local political leaders, who arbitrate cases based on previous occupancy or religious norms (Delville, 2006).

This system implies that rights held by individuals are the result of a social and political process of negotiations overseen by customary authorities. This enforcement process has an inherently procedural nature. Rules governing customary arrangements do not provide a precise codification of each

landholder's rights; instead, they only define procedures by which an individual obtains access to the land (Chauveau, Bosc, & Pescay, 1996). Therefore, the informal nature of customary rules prevents upfront the possibility of establishing a set of well-defined land property rights.

Population growth and the resulting increased pressure on natural resources pose serious concerns for the functioning of informal customary arrangements. Scholars notice that the absence of written documentation regarding land use contributes to an increase in conflicts over inheritance rights and land use (Deininger & Castagnini, 2006). In Benin, the policy response to problems resulting from tenure insecurity has been a land tenure reform known as the Plan Foncier Rural (PFR), which was enacted in the 1980s and whose implementation we document in this paper. The reform consists of socio-land surveys at the village level to identify rightholders, their rights, and parcel boundaries. Rights and associated rightholders are then recorded in public registries, and a process of land demarcation takes place. The process allows for public objection to the proposed registration of rights and requires that rightholders and neighbors publicly sign survey records. Registered plots acquire a new legal status, award presumption of ownership recognized by courts, and can be transformed into land titles following a simplified procedure. Moreover, registered rights enjoy legal protection from the formal judicial system and make it possible to sell or use registered plots as collateral. Given these characteristics, even if the registration of rights does not directly confer legal title over the plot, the PFR awards rights that are *de facto* akin to private property.<sup>3</sup> Therefore, the PFR injected a major change into the institutional contours of property rights over land, which is particularly important because land is the only asset of most rural villagers (Goldstein et al., 2016).

Benin started experimenting with a pilot implementation of the PFR in 1993. However, due to lack of resources, the reform interested only a small number of villages until 2006, when the Millennium Change Account subsidized a five-year PFR implementation program under the auspices of the World Bank. The key characteristic of the Beninese PFR is that implementation followed a randomized control trial process involving hundreds of rural villages. In fact, this is the first

<sup>3</sup>From a purely legal perspective, our use of the term "private property" is somewhat imprecise because, according to Beninese law, only formal land titles confer private property rights. In the initial plan for the reform implementation, the Rural Land Act 2007-003 introduced the Certificat Fonciers, land certificates recording rights registered during PFR that the Beninese administrative authority would release automatically to individual rightholders. The certificates could then be converted into land ownership titles (Titre Foncier) following a specific procedure. The release of PFR certificates was muted by Rural Land Law 2013-001, "Land and Property Code." The new law incorporated the Certificat Foncier into the Titre Foncier and further confirmed the importance of registered customary rights by giving them legal recognition and establishing a simplified procedure for their upgrade to formal legal titles on land. In the text, we use the term "private property" because, with the caveats illustrated above, land rights registered during the PFR share the key features characterizing a system of well-defined private property rights: exclusive use, transferability, and the possibility to use the land as collateral.

case of a large-scale land tenure reform implemented as a randomized control trial.

The objective of the reform was to deliver land certificates in 300 rural villages across forty communes.<sup>4</sup> In the preliminary phase of the project, interested rural villages in the communes were informed about the PFR reform and were invited to apply in order to participate in the lottery. As a second step, each application received was examined to verify whether the village met certain eligibility criteria.<sup>5</sup> Among the 1,235 villages that applied for participating in the PFR lottery, 576 were judged eligible.

A subsample of 300 villages was selected via public lottery among the eligible villages. Consequently, in the period 2009 to 2011, the Beninese government supported by the Millennium Challenge Corporation and the World Bank implemented the PFR in these selected villages (the treated group).<sup>6</sup> The remaining 276 nonselected villages (the control group) did not receive any intervention and, as of today, continue to have customary land rights. Figure 1 shows a map of communes and villages interested by PFR.

### III. The Experiment

#### A. Game Design

We implement a modified dictator game, in which the participant, acting in the role of dictator, has the ability to appropriate resources from an anonymous passive player's endowment. Participants are matched in pairs and randomly assigned to the role of dictator or passive player. The passive player receives from the experimenter 10 coins. Each coin is worth XOF 100 (approximately \$0.17).<sup>7</sup> The dictator initially has zero coins. However, the dictator has the opportunity to take some or all the coins from the passive player at no cost.

To limit experimenter demand effects, we followed an experimental procedure that makes the participants' decisions blind to the experimenter onsite.<sup>8</sup> Once in the decision room, the dictator is presented with two envelopes, one yellow and one brown, marked by an identification number. In the yellow envelope are the 10 coins owned by the passive player; the brown envelope is empty. The dictator is instructed that the 10 coins in the yellow envelope are property of the passive

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

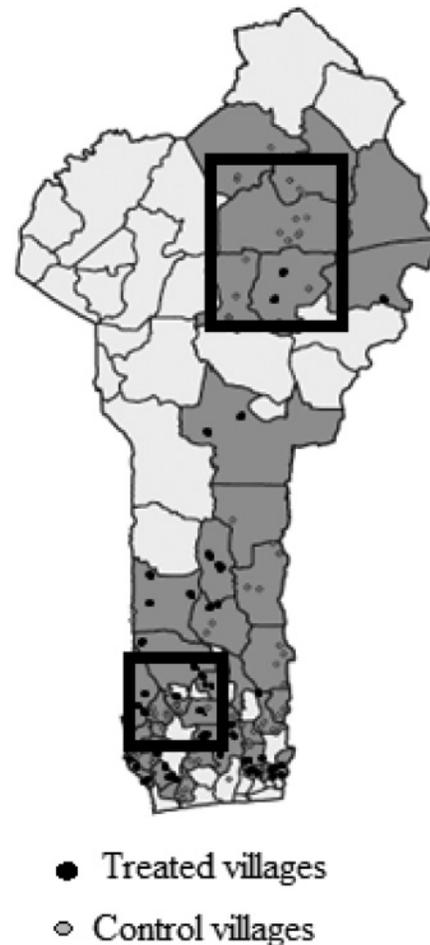
<sup>5</sup>The criteria for eligibility were poverty index, potential for commercial activities, regional market integration, local interest in promoting gender equality, infrastructure for economic activities, compliance with the PFR application procedure, incidence of land conflicts, and the production of main crops.

<sup>6</sup>Since four selected villages refused to complete the program implementation, the treated sample is composed of 296 villages.

<sup>7</sup>The average weekly income for an household in our sample is around XOF 12,000.

<sup>8</sup>Moreover, the experimenter on site and the research assistants did not know whether they were operating in a treated or control village.

FIGURE 1.—COMMUNES AND VILLAGES IN THE PLAN FONCIER RURAL IN THE LOTTERY POOL AND GEOGRAPHICAL AREAS WHERE THE STUDY WAS CONDUCTED



player and that he or she can decide to take some or all of the coins from the yellow envelope and transfer them to the brown one. The dictator is also informed that the coins that will be left in the yellow envelope and those transferred to the brown envelope will be the actual payoffs of the game for the passive player and for him or her, respectively. At that point, the experimenter leaves the decision room. After having decided how many coins, if any, to take from the passive player's endowment, the dictator places the two envelopes in a box. In this way, anonymity is maintained throughout the experiment across participants, and the procedure makes it impossible for the experimenter onsite to match the players' identities and choices.

The final outcome of the game is, for both players, the amount of coins owned after the decision made by the dictator.

*Experimental procedure.* The data collection consists of sixteen experimental sessions that took place in February and March 2017. Participants were residents of a sample of villages randomly selected from the PFR lottery pool for the province of Coffou (in the southwest of the country),

Borgou, and Alibori (both in the northeast of Benin). The rural areas where the fieldwork for the respective studies took place are highlighted in the rectangular boxes in figure 1. Each session was conducted in a different village. The sixteen villages (nine treated) were randomly selected from the entire list of villages included in the lottery pool in the regions noted. No village refused to participate.

The selection of participants within each village proceeded as follows. The day before the experiment, a member of the research team informed the local authority (village chief) and the village residents that the following morning, a team of researchers would come to the village to perform the research and recruit participants among the villagers. From the village residents who came to the meeting, the experimenters randomly selected nine male and nine female participants.<sup>9</sup> Selected participants must have been older than 18 years old, and at maximum, one member per household was included in the experiment. In villages composed of multiple clusters of relatively isolated huts, we split equally the number of participants belonging to each cluster.

A total of 254 participants took part in the study. None of the subjects had participated in an economic experiment before. In each session, subjects completed a brief sociodemographic survey and made decisions in the modified dictator game described above, as well as in other experimental tasks described in the discussion section and two additional experimental games.<sup>10</sup> Data on participants' risk preferences in both the domain of gains and losses were collected following a lottery choice task similar to Voors et al. (2012).<sup>11</sup>

To verify whether migrations between control and treated villages create selection concerns, we asked participants in our sample whether during the time frame following the reform implementation, they had migrated from a different vil-

lage and, if applicable, the reasons why they had migrated. Only two subjects were not already residents of the village where we interviewed them at the time of PFR implementation, and they both reported to have migrated for reasons connected to marriage. The exclusion of these two participants from the analysis leaves the results presented below qualitatively unchanged.<sup>12</sup> Finally, in table A1 in the appendix we report results of a series of *t*-tests (or chi-square tests for dummy variables) for the comparison of participants' sociodemographic characteristics between the treatment and control samples. In no case did we register a statistically significant difference across the two groups.

The procedures for administering the survey, the game instructions, and the order in which the games were played were identical across sessions. Sessions took place in a public space (usually a school or a religious building), composed of a large common room and a separate room where subjects made decisions in private. Upon arrival, participants were randomly assigned an identification number and completed a brief sociodemographic questionnaire. Participants were then informed that they would earn a participation fee equal to XOF 500 and that they had the opportunity to gain additional money by participating in a series of tasks. To avoid potential income effects, we also communicated that only the payoff generated in four out of seven games played during the session would be actually paid out and that the four games would be randomly determined by lottery at the end of the session.

Since the majority of the participants were illiterate, experimental instructions for each game were administered orally in public by the experimenter.<sup>13</sup> To minimize the risk that participants would not fully understand the instructions, each participant, before being able to enter the decision room, had to answer correctly a few control questions posed in private by the experimenter. If the participant failed to provide the correct answers, the experimenter repeated the explanation to the subject until he or she was able to answer the control questions.

A session in a village lasted approximately three hours. Participants received on average \$7 as final payment, roughly the equivalent of three days' wages for subjects in our sample.

#### IV. Results

Figure 2 plots the average amount of coins appropriated by the dictators in the sample of treated participants, who experienced the reform, and, in the control sample, who did not. A mere inspection of the figure suggests that the average amount of coins taken by the dictator is larger for participants in the latter sample. In figure 3, we plot the distribution

<sup>9</sup>Most of the sessions were completed by exactly eighteen participants; however, there was some variation in the number of participants, with a minimum of twelve subjects and a maximum of twenty subjects. Villagers not selected to participate in the study received a show-up fee equal to XOF 500.

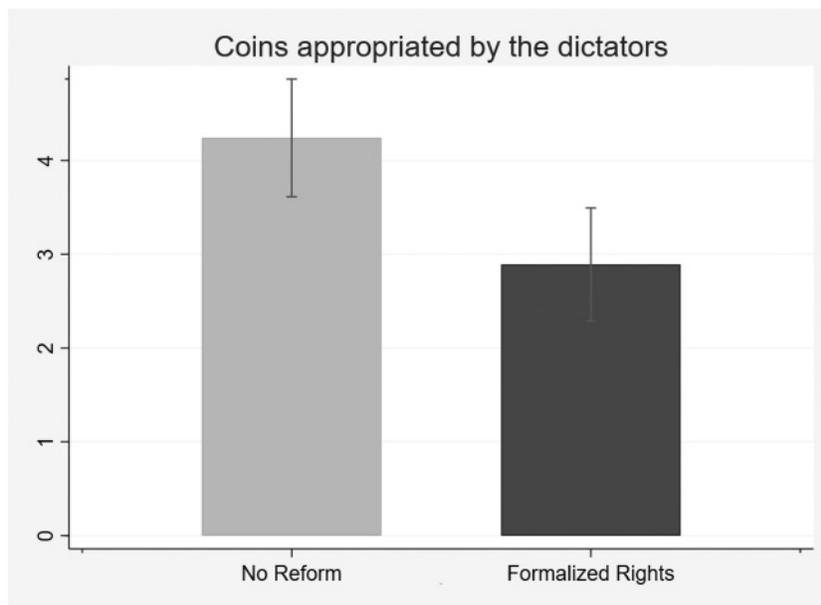
<sup>10</sup>In addition to the survey and the experimental games described in this paper, during the sixteen sessions of data collection, participants took also part in a linear public goods game and a standard trust game that were part of the data collection related to a different research project. In each session, the order in which the games were played was held constant, games were played one-shot, and participants did not receive feedback regarding the game outcome until the end of the experimental session. A description of the two games that were not designed for this project are in Fabbri (2018).

<sup>11</sup>Each subject had to make six choices between participating in a lottery or gaining/losing a certain amount. In the initial three choices, participants could either play a lottery with probability 3/10 they would win 5 coins and with probability 7/10 they would win 0. The certain equivalent gain in the three lotteries was respectively 1, 1.5, and 2 coins. In the last three choices, participants decided whether to play a lottery that implies losing 5 coins with probability 3/10 or losing 0 with probability 7/10, or incurring a certain loss of 1, 1.5, and 2 coins. Notice that the maximum loss of 5 coins is equal to the show-up fee received and that, by design, in none of the games can participants earn negative payoffs). In order to facilitate comprehension of the choice alternatives, the experimenter used colored balls to be withdrawn from a bag for representing the probability of gains and losses and determine the outcome. A die was then thrown to determine which of the six lotteries was paid.

<sup>12</sup>We also asked participants in control villages whether they or their family members owned parcels of land in neighboring treated villages. Three subjects reported so, and we excluded them from the analysis. The inclusion of these three subjects in the sample does not affect the results.

<sup>13</sup>An English translation of the instructions is included in the appendix.

FIGURE 2.—AMOUNT OF COINS FROM OTHERS' ENDOWMENT APPROPRIATED BY THE DICTATORS



of the amount of coins appropriated by dictators in the two treatments. The upper panel reports the choices of participants who did not experience the land tenure reform. The distribution is unimodal, with a spike in correspondence on the choice of taking five coins from the passive participants. The lower panel shows the distribution of choices for participants who experienced the reform. Compared to the control sample, the distribution is skewed to the left. Taking no coins from the other participant's endowment is the modal choice. This evidence suggests that, on average, dictators in the control sample take a larger fraction of the passive players' endowments. Similarly, in figure 4, we plot the total amount of coins taken by the dictator for each level of dictator's choice in the two treatments. While the majority of participants in control villages take half of the endowment from their pairs, villagers who experienced the reform when appropriating a share of the passive player's endowment mostly take less than half of the pair's resources.

To verify the graphical impressions obtained above, we perform a Wilcoxon rank-sum test comparing the distributions of the coins appropriated by the dictators in the two samples. The result confirms that participants in the treated samples take significantly less of the passive players' endowment compared to those who did not experience the land tenure reform (Wilcoxon rank-sum test,  $p$ -value < 1%). A  $t$ -test for comparison of the mean taking rate confirms that, on average, dictators in the treated sample take less money from the passive participant's endowment than those in control samples (two-sided  $t$ -test,  $p$ -value < 1%).

We compare the fraction of participants who did not appropriate any coin in the two samples. Among participants in the control sample, 5% did not take any coin from the passive players. This percentage rises to 26% among participants in

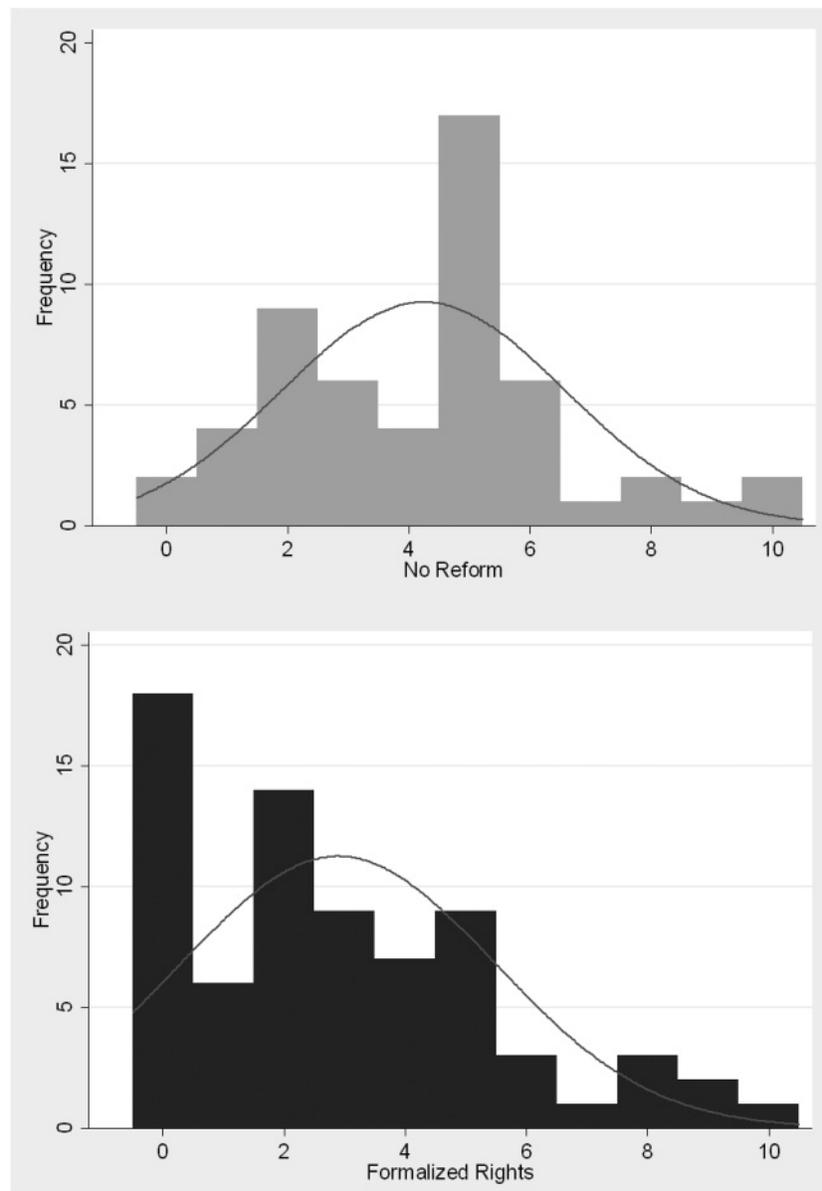
the treated sample. A chi-square test confirms that the fraction of participants taking nothing from the passive player is significantly higher in the treated sample (chi-square test,  $p$ -value < 1%).

We then proceed with a regression analysis. Results are shown in table 1. We regress the amount of coins appropriated by dictators on the dummy *treated*—that is, equal to 1 for participants in the sample who experienced the land tenure reform—and a set of sociodemographic controls.<sup>14</sup> Model 1 implements an OLS regression. Standard errors are clustered at the village level, and we use wild cluster bootstrap with 999 replications to resample with the sixteen clusters (Cameron, Gelbach, & Miller, 2008). The coefficient of the treatment dummy is negative and statistically significant at the 1% level, suggesting that participants in the treated sample take significantly less than those in the control sample. In model 2, we replicate the same specification but for using a censored tobit regression to account for a mass point at zero coins taken and implementing score bootstrap with 999 replications. The result remains significant at the 1% level, confirming that participants who experienced the reform appropriate significantly less of the passive player's endowment than those in the control group.<sup>15</sup> Point estimates suggest that experiencing the reform on average determines a 40% to 60% reduction of the passive players' endowment appropriated by the dictators.

<sup>14</sup>The controls include age, gender, village distance to paved roads, village population, incentivized measure of risk preferences, and religion.

<sup>15</sup>In table A2 in the appendix, we replicate the regression analysis presented in table 1 by implementing a set of zero-inflated negative binomial regression models that would account for the count nature of the data. The results remain qualitatively unchanged.

FIGURE 3.—DISTRIBUTION OF THE AMOUNT OF COINS APPROPRIATED BY THE DICTATORS



## V. Discussion

The evidence we have reported suggests that for the participants in our sample, experiencing a reform transforming collective informal land rights into a formal and legally recognized system akin to private property has a sizable effect on the propensity to appropriate resources owned by an anonymous peer. In our experiment, this effect is generated independently from other factors, such as direct peers effects, legal norms, and enforcement institutions, which are silenced by design.

Thus, one possible explanation for the observed behavior is that experiencing formalized private property rights increases respect for the property of others by directly affecting

preferences, altering the utility associated with appropriating goods owned by others. In the discussion that follows, we call this explanation the “endogenous respect for property hypothesis.”

However, the behavior observed in our experiment might be amenable to alternative explanations. For instance, participants in treated villages might have experienced improved material conditions, invested in education, or obtained access to new resources—such as the ability to resort to a formal legal system or easier access to financial means—which in turn are related to the decrease in taking behavior. Moreover, the reform might have triggered changes in individual values and beliefs other than respect for property, such as an increase in altruism, or a modification of the perception of informal

FIGURE 4.—SUM OF THE COINS APPROPRIATED BY THE DICTATORS FOR EACH LEVEL OF DICTATOR'S TAKING

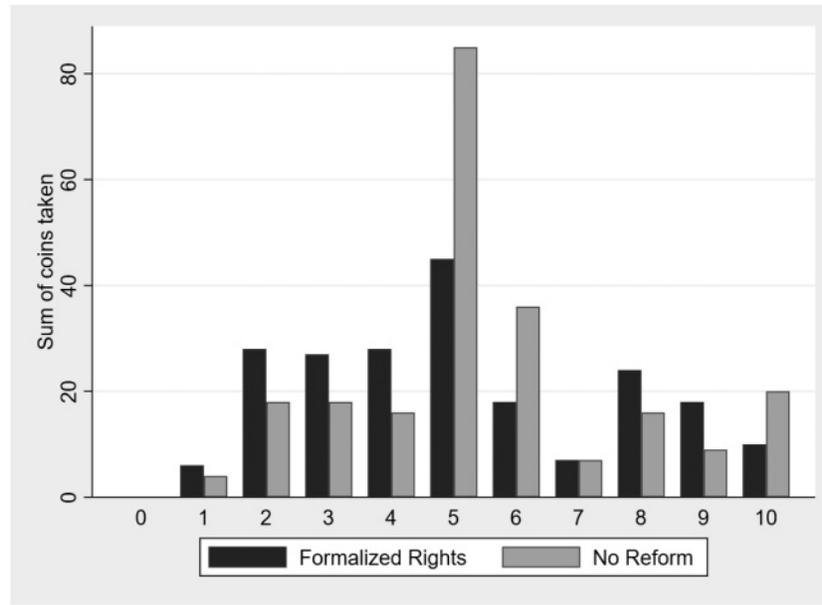


TABLE 1.—COINS APPROPRIATED BY THE DICTATOR

	Model 1	Model 2	Model 3	Model 4
treated	-2.046	-2.518	-2.097	-2.525
<i>C.I.</i>	[-3.53, -.700]		[-3.651, -.691]	
<i>P-value</i>	0.004	0.001	0.005	0.001
education			-0.130	-0.192
			[-.567, .329]	
income10k			0.52	0.38
			-0.118	-0.172
			[-.565, .191]	
credit			0.49	0.38
			0.057	0.157
			[-.681, .697]	
Controls	Y	Y	Y	Y
Constant	3.903	3.643	4.319	4.165
	[1.818, 5.987]		[2.499, 6.203]	
Number of observations	127	127	127	127

Dependent variable: coins appropriated by the dictator. Models 1 and 3: OLS regression, wild cluster bootstrap with 999 replication. Models 2 and 4: left-censored tobit regression, score bootstrap with 999 replications. Standard errors clustered at the village level. Compared to models 1 and 2, models 3 and 4 additionally control for income (in thousands of XOF), access to credit, and education levels. Controls include age, gender, religion, village distance to paved roads, village population, and estimation of risk preferences. The two lines below the coefficient estimates report the confidence intervals (for OLS regressions) and the *p*-value, respectively.

norms of redistribution, which might have affected the taking rate. In this section, we investigate alternative explanations for the observed behavior and contrast them with the endogenous respect for property hypothesis. To do so, we analyze additional data collected through an incentivized experiment and a postexperimental survey.

#### A. Effects of the Reform on Villagers' Material Conditions

*Income and education.* Experiencing the reform might have affected individual behavior through a wealth effect.

To explore this possibility, we compare data on participants' income in treated and control villages. Results of a *t*-test for comparison of means and of a *z*-test for comparison of distributions show that there is no statistically significant difference in income between villagers in treated and control groups (*p*-value > 10% in both tests). Regression analysis reported in models 1 and 2 of table A3 in appendix A confirms the findings.

We also check whether obtaining formalized land rights affected the education level of participants in our sample and whether this fact might explain the behavior observed in the experiment. Indeed, Galiani and Schargrodsky (2010) found that Argentinian peri-urban squatters significantly increased investments in education as a consequence of receiving land titles. The majority of the rural villagers in our sample did not receive any formal education. The fraction of participants who never went to school is statistically the same in treated and control villages (chi-square test, *p* > 10%). Similarly, if we compare the number of school years attended by participants belonging to treated and control groups, the difference is not statistically significant (*t*-test two-sided, *p* > 10%; Wilcoxon rank-sum test, *p* > 10%). These results are confirmed by the regression analysis reported in table A4 in appendix A, in which we regress the number of years of education to the treatment dummy and a set of sociodemographic controls. Finally, in models 3 and 4 of table 1, we reestimate our main model specifications presented in models 1 and 2 by adding income and education as controls. The results remain qualitatively unaffected, and the point estimates are very similar.

*Access to credit and participation to financial activities.* One of the objectives of the PFR reform was to give land

users the possibility of collateralizing land and facilitating the creation of a land market. Had the reform created easier conditions for participation in financial activities and access to credit, then villagers in the treated group might display lower taking rates as a consequence of different exposure to market activities. Indeed, several studies discuss the possibility that participation in market activities has consequences for people's beliefs and attitudes in the nonmarket sphere (Bowles & Hwang, 2008).

We check whether the PFR enhanced credit supply and exposure to financial activities for participants in our sample. Participants were asked to report whether they were participating or had participated in financial activities in the previous seven years and whether they had accessed the credit market—that is, whether they had applied for a loan or mortgage. Engaging in these activities is relatively uncommon in our sample. The likelihood of doing so does not differ between the treated and control groups (chi-square test,  $p$ -value > 10%). Regression analysis reported in table A5 in appendix A confirms the result.

This finding is consistent with a previous assessment of the short-term effects of Benin's PFR on the development of a land market (Goldstein et al., 2016) and, more generally, with evidence suggesting a limited impact of land rights reform programs on access to credit and financial markets (Galiani & Schargrodsky, 2011; Lund, Odgaard, & Sjaastad, 2006).

*Conflicts and conflict resolution mechanisms.* An important goal of PFR is preventing the insurgence and escalation of conflicts over land in rural areas. Had the reform reduced the level of conflicts experienced by participants in our sample, it is possible that the increase in respect for property observed among individuals in treated villages is a consequence of the improved relationships among community members. For instance, one could hypothesize that the psychological cost of appropriating resources owned by a community member selected from a sample of individuals with whom relationships are peaceful is higher compared to a situation in which the sample includes individuals with whom the decision maker is currently litigating.

To verify whether the reform had an impact on the level of conflicts, we asked participants in our experiment whether they had experienced conflicts in the previous seven years. The vast majority of the reported conflicts (about 90% of them) concerned land use and, namely, conflicts between farmers and ranchers as well as boundary issues; the remaining share of conflicts related to public takings, inheritance, and contract enforcement. Participants in treated villages were engaged in conflicts with the same frequency as participants in control villages (chi-squared test,  $p$ -value > 10%). A regression analysis, whose results are reported in table A6 in appendix A, confirms the result.

We also verified whether the reform may have affected the mechanism of conflict resolution on which villagers rely. In both treated and control villages, the majority of participants

who experienced conflicts continue to resort to the traditional customary conflict resolution mechanism (89% and 91% in control and treated villages, respectively). A chi-square test cannot reject the hypothesis that the likelihood to resort to a traditional conflict resolution mechanism is the same across treatments.

Finally, we checked whether the PFR affected the quality of the conflict resolution mechanism. For instance, Deininger and Feder (2009) argue that the introduction of a new formal judicial system—at least in the short term—might not replace the traditional customary dispute resolution mechanism, but rather may create a parallel judicial channel that makes it possible for litigants to engage in forum shopping, thus complicating and delaying the resolution of conflicts. To do so, we asked the fifty participants who reported having experienced a conflict whether the conflicts they experienced had been successfully solved. We find no statistically significant difference in this measure between respondents in treated and control villages (chi-square test,  $p$ -value > 10%).

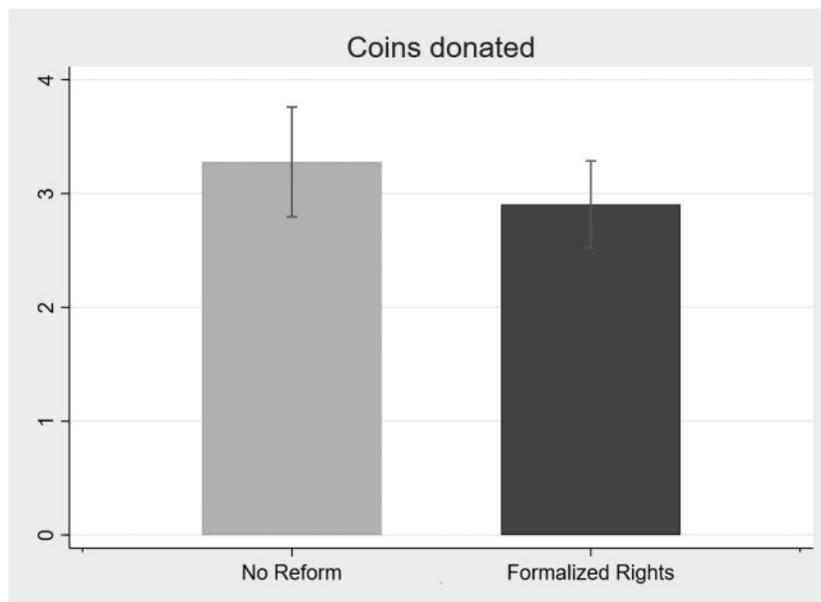
#### B. *Effects of the Reform on Altruism, Distributional Norms, and Pro-Market Beliefs*

*Altruism.* An alternative explanation to the endogenous respect for property hypothesis is that participants who experienced the property rights reform were subject to an increase in pure altruism. Should that be the case, more altruistic dictators would allocate a larger fraction of the endowment to the passive player, independent of whether they are donating part of their endowment (as in the standard dictator game) or taking resources from the passive player (as in our modified dictator game).

To verify whether the observed reduction in taking can be explained through a change in altruistic preferences, we asked the same subjects who acted as dictators in our taking dictator game to participate in a standard dictator game framed as a donation choice. In this game, participants initially received from the experimenter 10 coins worth XOF 100 each. The participants were then informed that they could choose to donate some or all of the coins received to an orphanage. Following the same procedure that was adopted in the modified dictator game, the participants' donation decisions were blind to the experimenter on site.

Figure 5 plots the average amount of coins donated in treated and control villages. Participants in villages who experienced the reform on average donate *less* than those in control villages (2.9 versus 3.25, respectively), albeit the mean donation and the distribution of donation choices are not statistically different between treated and control participants ( $t$ -test two-sided  $p$  > 10%; Mann-Whitney test two-sided,  $p$  > 10%). Table A7 in the appendix, in which we replicate the analysis presented in table 1 but for using as a dependent variable the amount of coins donated, confirms that there are no statistically significant differences in donation rate between treatments. Therefore, in our sample, experiencing the

FIGURE 5.—COINS DONATED BY PARTICIPANTS IN A DICTATOR GAME FRAMED AS A DONATION DECISION



land tenure reform did not produce a significant increase in villagers' altruism that could explain the observed increase in respecting others' property.

*Beliefs regarding others' altruism and profit redistribution.* A possible explanation for the lower taking rate recorded among treated participants is that the reform changed their beliefs regarding how other villagers would behave in the same situation, and these modified expectations in turn determined the observed change in behavior. For instance, Levine (1998) proposes a model of behavior in which the level of prosociality displayed by an agent depends on her beliefs regarding what others in her reference group will do in the same situation.

A different behavioral mechanism that would lead to similar results is that if agents derive utility from conforming to the behavioral standard of the reference group, a participant might reduce her taking rate because of the expectation that after the PFR implementation, others will also do so (Hung & Plott, 2001; Manski, 2000). Therefore, had the reform modified expectations regarding how other villagers would behave in the role of dictator, the observed reduction in taking for treated subjects might be explained in terms of a change in beliefs rather than preferences.

To verify whether, in treated villages, the reform affected a participant's expectations regarding his or her peers' willingness to respect property rights, we asked passive players how many coins they expected the dictator to appropriate from their endowment. Figure 6 plots the average beliefs regarding dictators' taking rate reported by passive players across treatments. On average, passive players underestimated the amount of coins effectively taken by the dictators. We do not find evidence that there is an expectation of enhanced re-

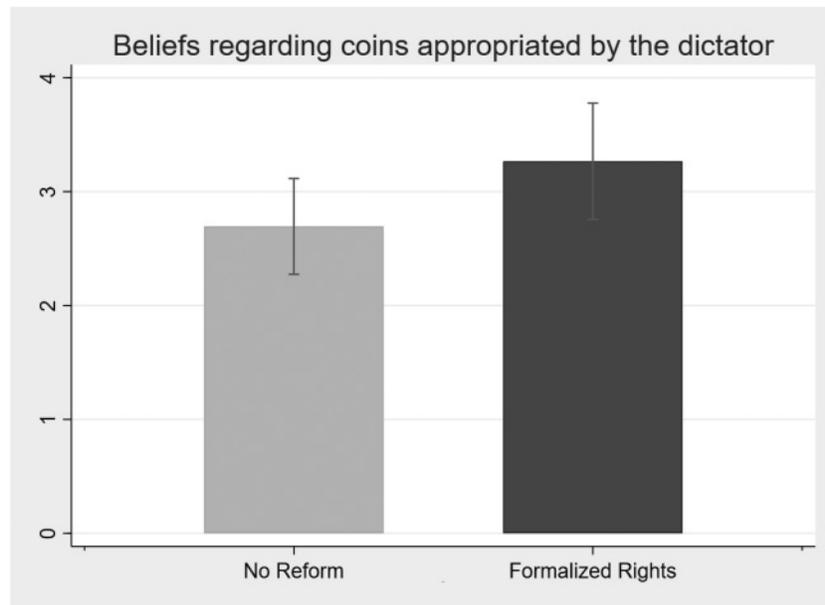
spect for property in treated villages. If anything, in treated villages, passive players expect *more* taking from the dictators compared to those in control villages (3.27 versus 2.69, respectively), albeit the difference is only marginally statistically significant ( $t$ -test two-sided,  $p$ -value < 10%; Wilcoxon rank-sum test two-sided,  $p$ -value < 10%).

We also checked whether the reform affected the social norms for profit redistribution. To test this possibility, using an incentivized coordination games similar to Krupka & Weber (2013), we elicited a set of social norms concerning the distributions of profits from an investment. Subjects were incentivized to correctly guess how much the majority of the village would consider it appropriate to share profits proportionally to initial investments made by each party instead of performing an egalitarian split.<sup>16</sup> A Kruskal-Wallis test cannot reject the hypothesis that participants in treated and control villages share the same perception of shared social norms regarding egalitarian versus meritocratic division of earned income ( $p > 10\%$ ).

*Beliefs regarding individualism, importance of money, and self-determination.* In a postexperimental survey, we collected nonincentivized measures concerning a set of beliefs that are associated with favoring the development of a free-market economy (Di Tella, Galiani, & Schargrodsky, 2007). If affected by the reform, these beliefs might have affected

<sup>16</sup>Participants were described a situation in which two parties make a joint investment contributing unequal initial amounts of resources. The subjects could rate the decision to split the profits obtained (a) equally and (b) proportionally to the initial contribution to the investment. Specifically, participants must choose from four options: "Very socially inappropriate," "Somewhat socially inappropriate," "Somewhat socially appropriate," and "Very socially appropriate." The complete instructions to the coordination game are reported in appendix B.

FIGURE 6.—AMOUNT OF COINS THAT PASSIVE PLAYERS EXPECT THE DICTATORS WILL TAKE FROM THEIR ENDOWMENTS



participants' respect for others' property. To do so, we asked a set of questions taken from the World Values Survey.<sup>17</sup> First, we focused on measures of individualism versus collectivism by asking whether participants believe that success requires a large group or can be achieved alone. The vast majority of participants in our sample (87%) consider a large group a necessary condition to achieve success. A chi-square test shows that there is no statistically significant difference in beliefs regarding individualism and collectivism in our sample ( $p$ -value > 10%).

Second, we asked participants their beliefs regarding the importance of work, as opposed to luck, to achieve success. In total, 36% of our respondents consider work important to achieve success. These subjects are evenly distributed between the treated (37%) and the control (35%) groups. A chi-square test fails to reject the hypothesis that the difference in response rate across the two groups is statistically significant.

Third, we asked participants a question regarding the importance of money for happiness. In our sample, 9% of the respondents state that money is not important, 52% that money is important to some extent, and 40% that money is very important. A Kruskal-Wallis test fails to reject the hypothesis that the response rates differ across treatments.

*Unobserved local characteristics and learning from neighboring villages.* Finally, we compare the behavior of treated and control villages separating those located in the north of the country (where the distance between villages is larger) and the south (where villages are comparatively closer to

each other's), respectively. Under the assumption that villages closer to each other share more homogeneous characteristics and cultural traits, if neighboring villages behave very differently, that would rule out possible channels explaining the observed results such as unobserved cultural characteristics at a local level. Another possibility is that simply observing (rather than experiencing directly) the formalization of property rights in neighboring villages is sufficient to determine a shift in subjects' willingness to respect others' property. Assuming that villages geographically located closer to each other have more opportunities to interact and observe neighbors, if this hypothesis holds, we should expect participants resident in the southern region, where villages are located closer to each other compared to those in the north, to behave more similarly across treatment groups.<sup>18</sup>

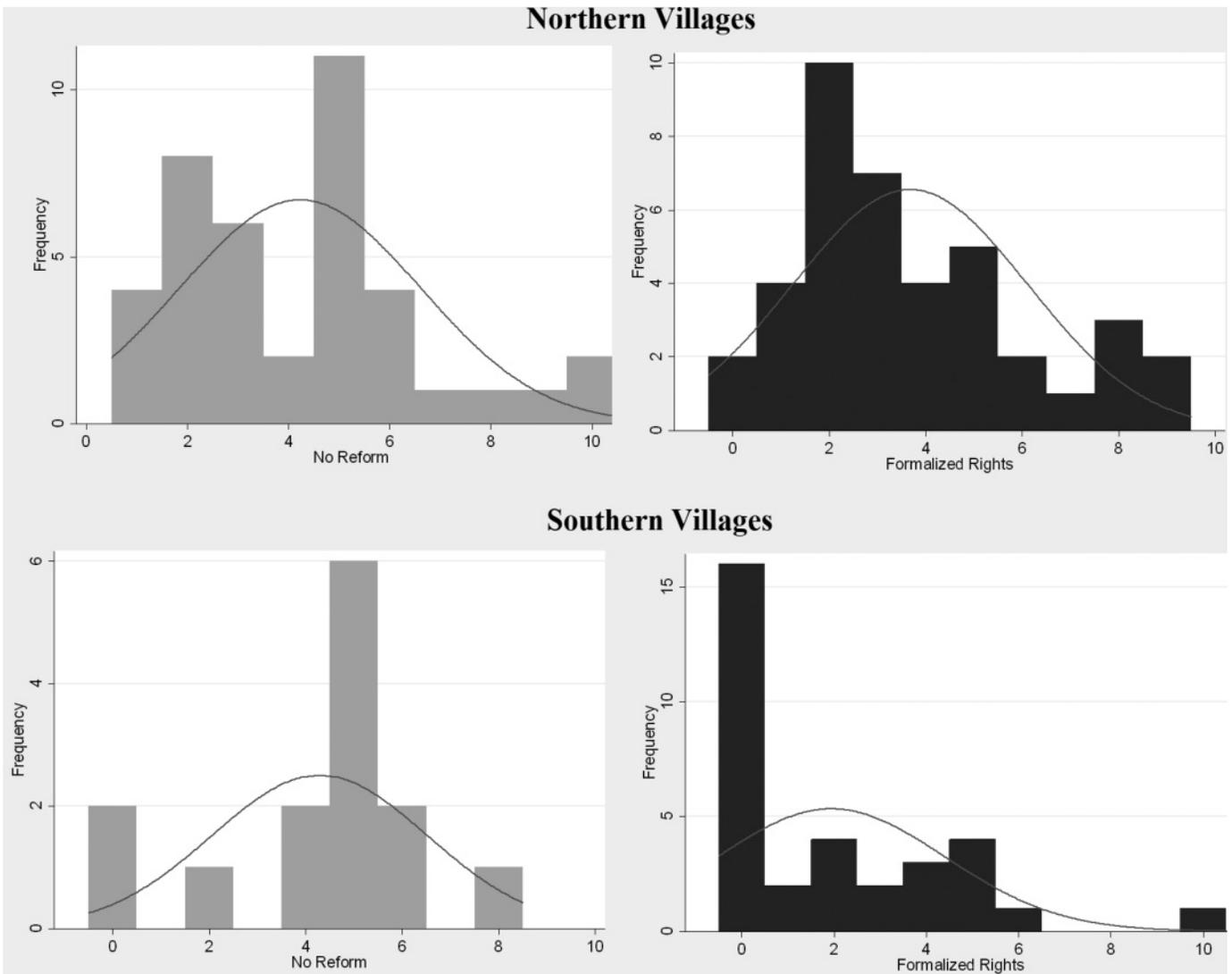
In figures 7 and 8, we plot the distribution and mean taking rate of the dictators in the north and in the south, respectively. Both the distribution and the average taking rate in the two subgroups mimic those we registered in the aggregate. In particular, the increase in respect for others' property rights after having experienced the reform seems to be the largest for villagers in the southern region.

The finding is confirmed by regression analysis reported in tables A8 and A9 in the appendix, in which we replicate the regression analysis proposed in table 1, including only the subsample of more dispersed villages in the northern region and only the subsample of villages in the more densely populated southern region, respectively. These results suggest that in both regions, experiencing the reform significantly reduces

<sup>17</sup>The complete text of questions that were asked is reported in appendix B.

<sup>18</sup>An important caveat is that the analysis comprises only data from sixteen villages and the statistical power limited. Therefore, while the findings reported in this section provide suggestive evidence regarding these two channels, readers should take into account these limitations.

FIGURE 7.—DISTRIBUTION OF DICTATOR'S TAKING RATE BY REGION



the amount of coins taken by dictators, and do not support the hypothesis that the effect is driven by background unobservables or by a learning-from-neighbors process.

## VI. Conclusion

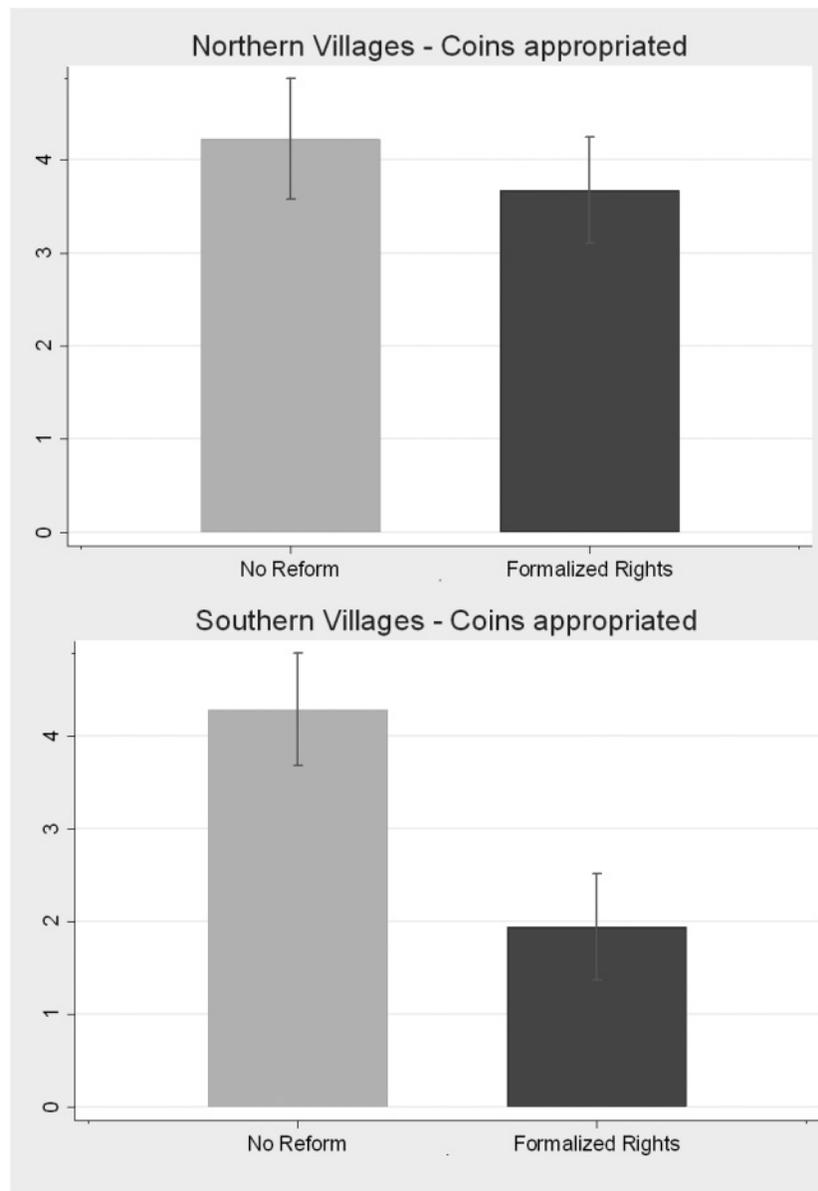
This paper investigates how a major change in the structure of property rights over land affects respect for property. We study a reform implemented in Benin from 2009 to 2011 by the World Bank. The reform resulted in the registration and formalization of rights over land, turning the existing collective customary rights over land into individual rights akin to private property. Our identification strategy relies on the unique process of implementation of this reform, the first case of large-scale land tenure reform implemented as a randomized control trial. Seven years after the reform implementation, we conducted a set of laboratory experiments implementing a modified dictator game in which participants

can appropriate others' endowments, while enforcement institutions are silenced.

We show that the reform significantly and substantially lowered the willingness of Beninese villagers to appropriate the endowments of others in a modified dictator game. We attribute this result to a change in preferences for respect for property and exclude competing explanations based on changes in material conditions, beliefs, or other preferences using additional experimental measures and a post-experimental survey.

These findings demonstrate that the structure and design of property rights institutions may have important consequences for social preferences. In this respect, this paper is broadly related to the literature investigating the relationship between acquiring property rights and economic prosperity. Scholars have shown that tenure security is a key determinant for increasing residential investments, labor supply, education, and social capital accumulation (Field, 2005, 2007; Galiani

FIGURE 8.—AVERAGE DICTATOR'S TAKING RATE BY REGION



& Schargrodsky, 2010; DiPasquale & Glaeser, 1999). The findings of Di Tella, Galiani, and Schargrodsky (2007) suggest that one avenue through which well-defined property rights could determine these improvements is by reinforcing pro-market beliefs. More radically, McCloskey (2010) aims at dispelling the idea that property rights matter at all. She argues that it is the “liberty” and in particular the “dignity”—that is, respect for work, entrepreneurship, and innovation—afforded to those involved in productive activities that have fostered growth over the past centuries. Our results shed light on a possible additional channel. Formalizing property rights activates a virtuous cycle of first-party enforcement in the form of an internalized norm of behavior supporting respect for property—possibly, a form of dignity.

Tentatively, our results suggest that formalizing property rights may have a positive feedback effect on the costs of enforcing such rights, since part of it possibly comes in the form of inexpensive (from the perspective of public authorities) first-party enforcement, reducing the scope for formal enforcement institutions. Reflecting on two waves of law and development reforms—of which the formalization of property rights is an important component—Berkowitz, Pistor, and Richard (2003) argue that the successful transplantation of Western legal institution requires a “receptive” environment and that recent history provides countless examples of failure to take into account local institutional and cultural conditions. Here we show that there could be an additional inverse relationship between institutional transplants

and local conditions: the process of transplantation could contribute to alter those conditions.

Finally, a possible channel explaining the results observed could be that the reform affected the evolution of fairness norms (Almås et al., 2010).<sup>19</sup> In our experiment, the majority of participants in the control sample choose a taking rate that produces an egalitarian distribution of the endowments, while participants who experienced better-defined property rights predominantly choose to not take anything from the pair's property, thus abandoning the fifty-fifty rule of resources allocation. While our study was not designed to investigate the impact of the reform on distributional preferences and so we can only provide suggestive evidence of these effects, we believe that studying how property rights institutions shape people's acceptance of inequality can be an interesting topic for future research.

<sup>19</sup>We thank an anonymous referee for suggesting this possible explanation.

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