How Property Shapes Distributional Preferences

By Marco Fabbri and Maria Bigoni*

We investigate the impact of a significant property rights reform on distributional preferences in rural Beninese villages. This reform replaced the informal use-rights over land, which were traditionally prevalent, with a system resembling private ownership. Our study employs a combination of a randomized controlled trial implementation of the reform across villages and lab-in-the-field experiments to elicit the distributional choices of villagers. We examine participants' preferences in situations where inequality arises from luck as well as situations where inequality is based on merit considerations. The findings reveal that the reform, which aligns allocation rules with impersonal market-like institutions, enhances participants' acceptance of luck-based inequality. However, we find no discernible effect of the reform on participants' tolerance for merit-based inequality. These results contribute to our understanding of the impact of institutional changes on distributional preferences and have implications for the design of economic systems.

JEL: D31; C93; D01

Keywords: Fairness; Institutional Change; Lab-in-the-field Experiment; Land Tenure Reform; Land Titling

I. Introduction

Rising economic inequality is considered one of the greatest challenges of our time (PEW, 2014) and has been proved to be harmful for individuals and society (Haushofer and Fehr, 2014; Piketty and Saez, 2014; Underwood, 2014). For these reasons, in recent years scholars have been devoting increasing effort to investigate what the reasons for the persistence of social inequality (Arrow, Bowles and Durlauf, 2018; Dorling, 2015; Gilens, 2009; Starmans, Sheskin and Bloom, 2017) and the determinants of distributional preferences are (Alesina, Stantcheva and Teso, 2018; Blake et al., 2015; Cappelen et al., 2007; Engelmann and Strobel, 2004).

In an influential contribution to this strand of research, Almås, Cappelen and Tungodden (2020) present a large-scale experimental survey asking US and Norwegian citizens to redistribute resources between pairs of workers who initially received unequal payments and in which the source of inequality was either luck or merit. The authors report evidence that the striking differences in economic inequality that can be observed between the American and Scandinavian societies persist in the allocations chosen by the participants in the controlled experiment. Since their experimental design rules

* Fabbri: Department of Economics, University of Bologna, fabbrimf83@gmail.com. Bigoni: Department of Economics, University of Bologna, IZA & CEPR, maria.bigoni@unibo.it. The experiment was approved by the Research Ethics Committee Parc de Salut MAR - Barcelona, reference nr. 2018/8015/I. Participants provided informed consent. The empirical strategy was pre-specified in a pre-analysis plan that was registered at the AEA RCT Registry–ID AEARCTR-0005292–before we collected the data, and included specification of the different hypotheses to be tested, of the regression approach, and of the dimensions to be studied in the heterogeneity analysis. MF acknowledges financial support by the Marie Curie Individual Research Grants Scheme, grant H2020-MSCA-IF-2017-789596. The authors declare no competing interests. We are grateful to Bertil Tungodden and to the participants to the CESifo workshop in Venice for comments and suggestions. Giulia Baldini, Ametonou Charmelle, Dossou Fiogbe, Gaston Gnonlonfoun, Issifou Gounou, Colin Henderson, Madeline Holbrook, Nice Houngbegnon, Dorothee Lokossou, Aissath Salifou, Aparna Sundaram, Mohamed Sedou, and Israelia Zannou provided excellent research assistance. The usual disclaimer applies.

out that differences in the distributive choices can be ascribed to beliefs regarding the cost of redistributing resources or to the source of inequality, the authors conclude that American and Norwegian citizens must have radically different fairness views.

In this paper, we adopt the experimental design of Almås, Cappelen and Tungodden (2020) and implement it within the very different socio-economic and cultural context of Benin, to investigate whether the type of property institutions characterizing a society can be a source of people's variation in distributional preferences. We study a large-scale reform implemented in 2010 in Benin, which replaced informal and socially-determined use rights over land, with a system of registered and legally protected property rights.[1] The customary rights system that traditionally regulates access to land in rural West African villages is characterized by informality, collective rights, and customary norms of redistribution applied within the village community. The reform transformed this system by recording rights over land parcels in public registries and granting to rightholders the possibility to (i) sell land parcels, (ii) use them as collaterals, and (iii) defend registered rights in formal state courts, thus introducing a system akin to private ownership.

The key element for our identification strategy is that the Beninese reform is the first case of property rights reform that was implemented as a large-scale randomized controlled trial (RCT). We exploit this RCT to verify whether an exogenous variation in individual experiences of formal property institutions influences fairness views, and if the effect of this institutional change on inequality acceptance depends on whether inequality originates from merit or from luck. To do so, ten years after the reform implementation

[1] The details of the reform are described in Section III.B.

we conduct a lab-in-the-field experiment – described in Section III – that replicates Almås, Cappelen and Tungodden (2020)'s design in a sample of villages included in the RCT pool. In the experiment, participants take the role of third-party spectators and must decide how to distribute payments among anonymous pairs of workers who have previously completed an online effort task. Our study thus adds to the findings of Almås, Cappelen and Tungodden (2020) by investigating the effects of a specific institutional shock on distributional preferences, by comparing randomly selected groups of individuals belonging to the same country rather than focusing on cross-country differences, and by studying whether fairness views may change in the short-medium run, due to an exogenous institutional change.

Our research is motivated by the long-debated argument that economic institutions have an important influence on the evolution of values, tastes, and behavioral traits (Bowles, 1998; Ockenfels and Weimann, 1999; Corneo and Grüner, 2002; Fehr and Hoff, 2011; Ostrom, 2009; Rodriguez-Sickert, Guzmán and Cárdenas, 2008). We focus specifically on the relation between preferences and market institutions (Henrich et al., 2010; Jakiela, 2015; Boesch and Berger, 2019). Within this context, empirical evidence shows that operating in market environments can modify participants' fairness views by evoking self-regarding behaviors in their preference repertoire (Alesina and Giuliano, 2015; Hirschman, 1982; Jha and Shayo, 2019; Roth et al., 1991). In the village communities where our study was conducted, the informal allocation rules traditionally applied to coordinate the use and transfer of land are based on personal relationships among individuals, which are regulated by status and social rank and correspond to specific rights and obligations (Delville, 2000). In contrast, market-like institutions like the property rights system introduced by the Beninese reform are characterized by impersonal and ephemeral interactions (Weber, 1978; Lane, 1991, p.636). We verify whether the replacement of socially-determined land rights with market-like property institutions influences participants' concept of fairness and redistributive norms.

Our contribution consists in documenting that the structure of property rights has a causal effect on the fairness views that prevail at a society level. We do so by proposing a research design based on a unique real-world institutional experiment that overcomes the endogeneity issues that typically characterize the relationship between institutions and fairness views. Our results suggest that the land reform had an impact on inequality tolerance: treated participants redistribute significantly less than controls when the initial inequality of workers' payoffs is generated by pure luck. Instead, the reform did not affect the redistribution decisions when the inequality between workers was originated by merit considerations.

We show that the observed variation in fairness views is concentrated among women and less affluent subjects who – thanks to the reform – now enjoy better access to markets and state courts. This suggests that villagers who de facto benefited the most from the institutional change in terms of access to credit opportunities and enhanced legal protection are also those whose distributional preferences were most affected. In the Conclusions, we discuss some tentative explanations for the change in preferences that we document. We notice how these results are consistent with the argument that market-like institutions can boost individuals' self-enhancing attribution and connect our findings with the recent literature in psychology and economics on motivated beliefs, suggesting an avenue for future research aimed at disentangling the mechanisms behind these effects.

The remainder of the paper is organized as follows: Section II presents the literature review, Section III describes the pre-registered empirical strategy, including details of the reform and of the experimental design, Section IV illustrates the results, and Section V discusses possible interpretations of our findings and concludes.

II. Related Literature

The idea of studying how a society's organization and its institutions influence distributional preferences is not new in the literature. There is ethnographic and anthropological evidence on the effects of formalizing land rights institutions on fairness ideals. For example, André and Platteau (1998) argue that formal property rights can clash with customary norms in determining villagers' fairness ideals. The authors report descriptive evidence from rural Rwanda where the customary norms of redistributing land in favor of landscarce community members suddenly ceased to be applied after the introduction of formalized land rights and the possibility to privately purchase land parcels (on this point, see also Deininger and Feder, 2009). A limitation of this strand of research is that case studies and research based on observational data cannot sort out endogeneity issues.

Several contributions have studied how cross-country differences in redistributive policies correlate with cultural heterogeneity in the beliefs concerning the determinants of poverty (Aarøe and Petersen, 2014; Alesina, Stantcheva and Teso, 2018; Arrow, Bowles and Durlauf, 2018; Gilens, 2009), with the efficiency of redistributive agencies (Hoy and Mager, 2018; Kuziemko et al., 2015; Sands, 2017), and with the distributional preferences held by individuals in a given society (Alesina et al., 2015). Moreover, evidence obtained comparing attitudes toward inequality across cultural or social groups (Cappelen et al., 2013; Henrich et al., 2010; Huppert et al., 2019; Rey-Biel, Sheremeta and Uler, 2018) suggests that, for instance, elites have distributional preferences that differ from those of the general population (Fisman et al., 2015), that high-inequality environments or relative income improvements are associated with larger inequality tolerance for wealthy individuals (Côté, House and Willer, 2015; Karadja, Mollerstrom and Seim, 2017; Nishi and Christakis, 2015), and that cross-cultural differentiation in distributional choices can be observed already in children (Blake et al., 2015).

A limitation that characterizes studies based on the comparison of different societies is that those cannot cleanly isolate the effects of institutions on distributional preferences, since cross-country or cross-population comparisons do not account for possible self-selection of people into specific social groups. More generally, empirical studies attempting to isolate the causal effects of property institutions on tolerance for inequality face a major challenge – to identify institutional changes that are exogenous to the evolution of fairness views. Individuals choose institutions reflecting their preferences. At the same time, those institutions shape people's values and beliefs. This "reflection" problem makes it challenging to find a suitable identification strategy to isolate the causal effects of institutions on distributional preferences.

A line of research has addressed this identification problem using laboratory experiments to observe subjects' behavioral reactions to exogenous manipulations of lab games institutions (Balafoutas et al., 2013; Deffains, Espinosa and Thöni, 2016; Engelmann and Strobel, 2004). However, modifications of rules characterizing stylized games are barely comparable to real-world institutional changes. Moreover, only very short-term effects can be detected (i.e. changes registered within the duration of a lab session), and the sample of participants is usually composed of college students not representative of the general population (Henrich, Heine and Norenzayan, 2010). Other studies have attempted to isolate the causal effects of institutions on distributional preferences by looking at historical changes in state regimes, laws, or regulations that are treated as orthogonal to tolerance for inequality (Alesina and Fuchs-Schündeln, 2007; Becker et al., 2016; Di Tella, Galiani and Schargrodsky, 2007; Kim et al., 2017; Shiller et al., 1992). However, this approach does not fully address endogeneity concerns, since the replacement of existing institutions could possibly reflect the mutated preferences of the institutions' builders (Alesina and Giuliano, 2015; Becker, Mergele and Woessmann, 2020; Hollander, 1999). Moreover, one problem common to these contributions is that they cannot sort out whether the observed differences in behavior that correlate with subjects' exposure to the different institutional environments reflect a modification of distributional preferences or, for instance, a change in beliefs concerning the efficiency of the redistributive system or the deservedness of wealth.[2]

We contribute to this literature by adopting an identification strategy based on a RCT that dispels endogeneity concerns present in cross-cultural studies and, at the same time, makes it possible to isolate the causal effects of institutions on distributional preferences. We combine this exogenous

[2]One notable exception is the study by Somville et al. (2020). The authors exploit an exogenous variation in wealth created by a housing lottery in Ethiopia to provide causal evidence that general attitudes toward economic inequality are unaffected by the increase in wealth. However, they show that house-winners are more likely to disfavor taxing homeowners and to attribute poverty to individual characteristics rather than to bad luck.

institutional variation with an experimental survey that replicates the research design proposed by Almås, Cappelen and Tungodden (2020). In this way, we resolve possible ambiguities regarding the role played by beliefs on the redistribution costs and the source of inequality. The use of lab-in-the-field experiments also mitigates the concerns for external validity intrinsic to laboratory approaches – as our treatment manipulation consists of a major institutional shock entailing real-world consequences and the pool of participants consists in a sample of the population involved in the RCT not limited to students. In this respect, our paper is methodologically related to the works of Barr (2003) and Jakiela, Miguel and Te Velde (2015), who combine real-world shocks to lab-in-the-field experiments to investigate the effects of resettlement and education, respectively, on social preferences.

This paper is part of a larger research project initiated by Marco Fabbri in 2017 to study the effects of the 2010-2011 Beninese property rights reform. Experimental findings from previous waves of data collection show that formalized property rights increase the propensity to respect others' property (Fabbri and Dari-Mattiacci, 2021) and that the reform also boosted levels of cooperation and trust but only in those villages served by paved roads that grant better access to the new institutions and government services introduced (Fabbri, 2021). Arruñada, Fabbri and Faure (2022) use data on land-related conflicts to show that land demarcation increases non-violent litigation aimed at purging property titles. Using experimental data collected in 2020 in parallel with those presented in this paper, Fabbri, Dari-Mattiacci and Rizzolli (2022) replicate Fabbri and Dari-Mattiacci (2021) with a different sample of participants and test if the results also apply to out-group interactions. Fabbri (2022) shows that formalized property rights increase villagers' cooperation with out-group strangers and Dari-Mattiacci and Fabbri (2021) report the results of vignette study presenting the trolley's dilemma, showing that formalized property rights increase the propensity to make consequentialist rather than deontologist choices.

III. Empirical Strategy

A. Experimental Design

The empirical strategy was specified in a pre-analysis plan that was registered at the AEA RCT Registry[3] before we collected the data, and included the hypotheses to be tested, the econometric approach to be adopted, and the dimensions to be studied in the heterogeneity analysis. Our experiment consists of a distributional task where a spectator has to allocate resources between two workers and involves a total of 1152 participants. In the experiment, *workers* individually complete an effort task and are then paired to determine a provisional payment, while *spectators* can redistribute resources among the paired workers in order to determine their final payments.[4]

Workers (n=576) were recruited from Amazon Mechanical Turk (AMT), an international online marketplace, to individually complete four effort tasks. Each worker received a fixed payment of $1 for participating in the experiment plus a variable payment for each effort task as explained below. After the completion of each effort task, workers were randomly paired, and their provisional payment was determined. In every pair, the provisional payment for each specific effort task was equal to 600 CFA (equivalent to

[3]AEA RCT Registry, ID AEARCTR-0005292.

[4]See Appendix C for an English version of the experimental instructions. These instructions are the same as those used in Almås, Cappelen and Tungodden (2020), with minimal variations.

$1) for one worker in the pair and 0 CFA for the other worker. The allocation of the provisional payment, and the consequent source of inequality, depended on the experimental condition. In the first two tasks the provisional payment was determined by "Luck" and a lottery randomly selected which of the two workers would receive the 600 CFA. In the last two tasks instead, the provisional payment was determined by "Merit": the 600 CFA were allocated to the worker with the best performance in the pair. The workers were informed that the amounts granted as provisional payments could be redistributed within the pair by an anonymous third-party, whose decision will determine the workers' final earnings. To sum up, we have 1152 random pairs of workers, and for half of these pairs the provisional payment was determined by luck, while for the others it was determined by merit.

Spectators (n=576) were recruited during fieldwork sessions among the local population of 32 Beninese rural villages, and had to make choices with monetary consequences for the workers but not for themselves (the details of the recruitment procedures are reported in Section III.D). Each spectator was matched with a pair of workers, and told that the two workers got a fixed payment of $1 to take part in four effort tasks, plus a variable payment for each effort task completed. The spectator then received information on the provisional payments the workers obtained for the task, and on the rule – Merit or Luck – that determined these payments. At this point, the spectator was asked to either confirm the provisional payments that had allocated CFA 600 to one worker and nothing to the other, or to redistribute the resources in multiples of CFA 100 among the two workers. The spectator was also informed that redistributing the payment would not imply any cost, and that the decision would determine the two workers' final payments relative to that effort task. Each spectator was asked to take two sequential distributive choices. Half of the spectators first received the instructions and took the distributive decision relative to the Luck condition and, subsequently, the decision relative to the Merit condition. The other half were exposed to the two conditions in reverse order. The spectators made their choices in the first condition without knowing that they would then be asked to take a second decision.

B. The "Plan Foncier Rural"

We combined the experimental design borrowed from Almås, Cappelen and Tungodden (2020) with the RCT implementation of the land rights reform we study. In Africa – Benin not being an exception – customary tenure characterized by collective property and informal possession largely predominates in rural areas (Deininger and Feder, 2009). In the attempt to improve access to land, tenure security and the development of a land market, the Beninese government with the support of the Millennium Challenge Corporation developed an approach for systematic identification and registration of customary rights to parcels of agricultural land, the "Plan Foncier Rural" (PFR). PFR consists of socio-land surveys at the village level to identify rights holders, their rights, and demarcate parcels boundaries. The process allows for public contestation of the proposed registration of rights and requires that rights holders and neighbors publicly sign survey records stored in public repositories (Delville, 2006). While registration of customary rights does not directly confer de jure legal ownership, nonetheless it awards presumption of ownership recognized by courts, making it possible to sell registered plots or use them as collateral, and the certificates registering posses-

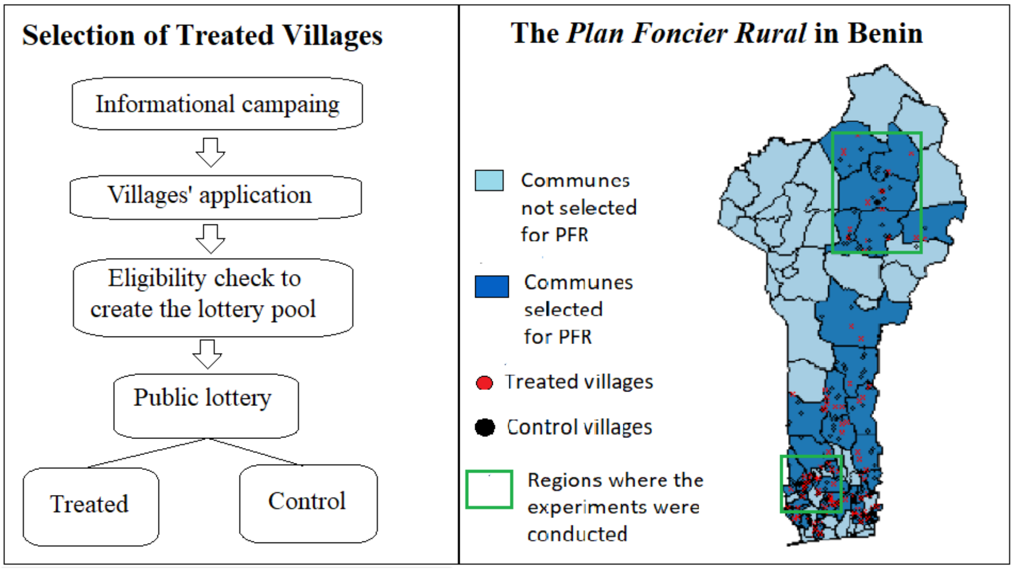

FIGURE 1. THE PLAN FONCIER RURAL IN BENIN

Note: The left panel displays the lottery mechanism for selecting villages to be included in the land tenure reform. The right panel displays the resulting implementation and the regions where the data collection took place. *Figure taken from Fabbri (2021).*

sory rights can be converted into land titles by following a shorter, cheaper, and simplified procedure compared to the regular process for titling uncertified land. Given these characteristics, registered land under PFR shares basic features akin to formal land ownership (Fabbri and Dari-Mattiacci, 2021).

The implementation process of PFR took place in 2010-2011 and is summarized in Figure 1. The Beninese PFR is the first case of land tenure reform implemented as a large-scale randomized control trial. First, 576 eligible villages willing to implement the reform were identified (eligibility concerns village characteristics such as population size and location in rural areas).[5] Second, a subsample of 291 villages was selected via public lottery,

[5]Each of the 576 villages included in the lottery pool volunteered to receive the PFR. This implies that the villages included in our sample displayed a demand for institutional change. Therefore, our study is not designed to answer the question of what effect of

and PFR was actually implemented ("treatment"). Non-selected villages ("control") did not receive any intervention and, as of today, continue to have customary land rights.

The actual implementation of the reform on the ground faced some challenges, such as resources and time constraints, the complexity to ascertain the customary rights in place, and inconsistencies due to some legal vacua. These difficulties occasionally resulted in the impossibility to survey the entire universe of land parcels in a village (Delville and Moalic, 2019). Moreover, the new Land Code introduced in 2013, while confirming the legal validity of PFR-registered rights, interrupted the process of automatic release of PFR certificates, thus de facto emphasizing the process of physical demarcation of parcel boundaries and clarification of the existing rights rather than conferring transferable property rights (Goldstein et al., 2018). Despite these documented difficulties in the implementation of the reform, we discuss evidence that, at the time when we collected our data, PFR registered rights were perceived by stakeholders as a conclusive proof of ownership that dramatically improved the chances of adjudicating disputes in courts and increased the perception of tenure security. In Appendix A, we discuss in detail the evidence collected by an impact evaluation study (Goldstein et al., 2016), and by an extensive survey we run on participants in our sample. While between one and three years after the reform modest or no effects on most economic outcomes were observed, villagers with easier access to legal facilities declare to perceive registered land as substantially more secure against conflicting claims and report a more frequent and more successful reliance on formal courts as conflict resolution mechanisms in land-related disputes.

a super-imposed institutional reform, for which there is no explicit local demand, would have on preferences.

C. Empirical strategy

We exploit the RCT implementation of the property rights reform in order to compare spectators' distributive decisions from participants in villages that have been randomly selected to have the reform implemented (treated villages) with decisions from participants in villages belonging to the RCT pool but not selected by the random assignment (control villages). Our main variable of interest e_i is the inequality implemented by the spectators which, in our two-person setting, is equal to the Gini coefficient:

$$e_i = \frac{|income\ worker\ A_i - income\ worker\ B_i|}{total\ income} \in [0,1]$$

where worker A_i is the one who was originally assigned CFA 600, while worker B_i was assigned CFA 0. Therefore, a Gini coefficient equal to 1 implies that the spectator did not redistribute at all, while a Gini equal to zero implies that the spectator divided earnings equally.[6]

The main empirical specification used in the analysis is the following:

$$e_i = \alpha + \alpha_M M_i + \delta_T T_i + \delta_M M_i T_i + \boldsymbol{X_i} + \epsilon_i \tag{1}$$

where M_i is a dummy equal to one when the subject takes decisions in the Merit treatment, T_i is a dummy equal to 1 for subjects in treated villages, and $\boldsymbol{X_i}$ is the pre-specified vector of individual characteristics collected in the post-experimental survey. We also perform a heterogeneity analysis to verify whether the effects of the institutional environments depend on the proximity to paved roads – which we use as a proxy for market integration

[6]In principle, a Gini coefficient of 1 might also imply that the spectator allocated all the money to worker B_i, but this never happened in practice (Figure B2 in Appendix B).

and access to the formal legal system – on gender, and on income.

D. Experimental Procedures

The data collection took place between January and March 2020. In the remainder of the section, we separately provide details regarding the recruitment processes and tasks of workers and spectators.

Workers. Workers were recruited from Amazon Mechanical Turk (AMT), an international online crowdsourcing marketplace. We posted an assignment on the platform in which we specified the conditions and reward for completing the task. Workers had to accept the stated conditions to participate. We recruited 576 workers. After having signed up for the experiment at the AMT website, each worker completed four real effort tasks. At the completion of each effort task, each worker was randomly paired with another worker who had also completed the same assignment, to determine the provisional payment for the specific effort task (before the spectator's redistribution takes place). The pair formed in such a way was then matched with a spectator. The assignment published in AMT and the instructions for the participating workers can be found in Appendix C.

Spectators. The spectators were recruited during fieldwork sessions in Beninese rural villages. A team of research assistants visited 32 villages that had been randomly selected from the list of villages included in the PFR in the regions of Couffou and Mono (in the South of the country) and Alibori and Borgou (in the North). The day before the experiment, an RA visited the village and asked the local population for voluntary participation in the research study. Among the people who showed up at the convened time, we randomly recruited 18 participants (9 males and 9 females[7], older than 18 years old, with a maximum of one participant per household) for each village, for a total of 576 participants. Non-selected participants were paid a show-up fee equal to CFA 500 (approximately $ 0,85) and were requested to leave. Spectators received a flat participation fee equal to CFA 500 for taking part in the study. They took part in the experiment described above, with the two distributive choices, in a post-experimental survey, and in other incentivized tasks not related to this project.[8] Each session lasted three hours and on average participants earned CFA 2600 ($ 4,8) in total.

Each distributive choice taken by a spectator corresponds to a different condition. The two conditions differ in terms of the source of inequality. Condition "Luck" is designed to elicit inequality acceptance when earnings are determined by luck. Condition "Merit" is designed to elicit participants' acceptance of inequality when earnings are determined by merit. Half of the spectators first took the distributive decision under the Luck condition and then under the Merit condition; the other half of the subjects were exposed to the two conditions in reverse order. In Appendix C we provide an English translation of the instructions given to the spectators in the two conditions.

In addition to the distribution choices, the spectators answered a pre-specified set of non-incentivized survey questions regarding: age, gender, religion, marital status, number of family members, participation to household

[7]In one village we had seven male and eleven female participants.

[8]In each experimental session, participants took part to the same tasks. The order of the tasks was the same in each session. The tasks were administered in the following order: a public goods game, the distributive choices described in this section, a modified dictator game, 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. The details of the public goods game are reported in Fabbri (2022) and the other experimental games are described in Fabbri, Dari-Mattiacci and Rizzolli (2022). Before each task, participants were gathered in a common room in which the instructions were read aloud. Participants then stated their decisions one-by-one in a private room.

finance management, education, literacy, village of birth, years of residence in the village, income.

IV. Results

As a preliminary check, we compare the observables elicited in the post-experimental survey between treatments (Table B1 in Appendix B). The samples are well-balanced, with the exception of participants in the treated sample being on average slightly older, and more likely to be married and to live in houses with running water (we include these variables as controls in all model specifications presented below). In order for our identification strategy to hold, we need to verify that, after the reform implementation, participants have not self-selected into one of the treatment arms, through migration. To do so, we collected data regarding participants' village of origin, the reason leading to migration (if any), and the number of years they have been living in the village. Only 35 out of 576 participants were not already residents of the village when the PFR reform was implemented, 20 in treated villages and 15 in control ones. The difference in not statistically significant (χ^2 test, p>10%). The majority of these migrations were reported by female participants, and the reason in over 90% of the cases was declared to be marriage. Similarly, we verified that there is no statistically significant difference between the fraction of participants who were actually born in the village where they took part in the experiment (χ^2 test, p>10%) nor between the number of years they spent in that village (two sided t-test, p>10%).

We proceed by comparing distributional choices under Merit and Luck. On average, 69% of participants chose a fully-equalizing strategy in the Luck condition and 16% in the Merit condition (see Figure B2 in Appendix B).

These results are similar to those reported by Almås, Cappelen and Tungodden (2020) for a sample of Norwegian and US citizens. The main difference that emerges between our data and those collected in these two countries, is the substantially lower fraction of subjects who do not redistribute anything, which ranges between 6% and 10% in Benin, while it is higher than 10% in Norway, and above 30% in the US. We then test whether tolerance for inequality is affected by the source that generated the unequal initial distribution without distinguishing between spectators' institutional environment. Fig. B1 in Appendix B shows that, after spectators' redistribution decisions, the Gini index is on average substantially larger in the Merit than in the Luck condition. The difference is indeed strongly statistically significant ($p < 0.001$, two-sided t-test). This finding is in line with previous evidence that people's demand for redistribution depends on the source that generated the inequality (Almås, Cappelen and Tungodden, 2020).

We then move to our main research question, which investigates the effects of experiencing the land rights formalization on inequality acceptance. The upper panel of Figure 2 displays the Gini index after spectators' redistribution has taken place in Merit and Luck, distinguishing between participants resident in treated and control villages (the frequency of Spectators' choice of each of the six possible distribution options is reported in Figure B2 in Appendix B). The Gini index after spectators' redistribution in the Merit condition is virtually identical for participants in treated and control villages ($e_i = 0.359$ and 0.366 in treated and control, respectively). A formal t-test confirms the visual impression and rejects the hypothesis of a significant difference between the average inequality between treated and control villages

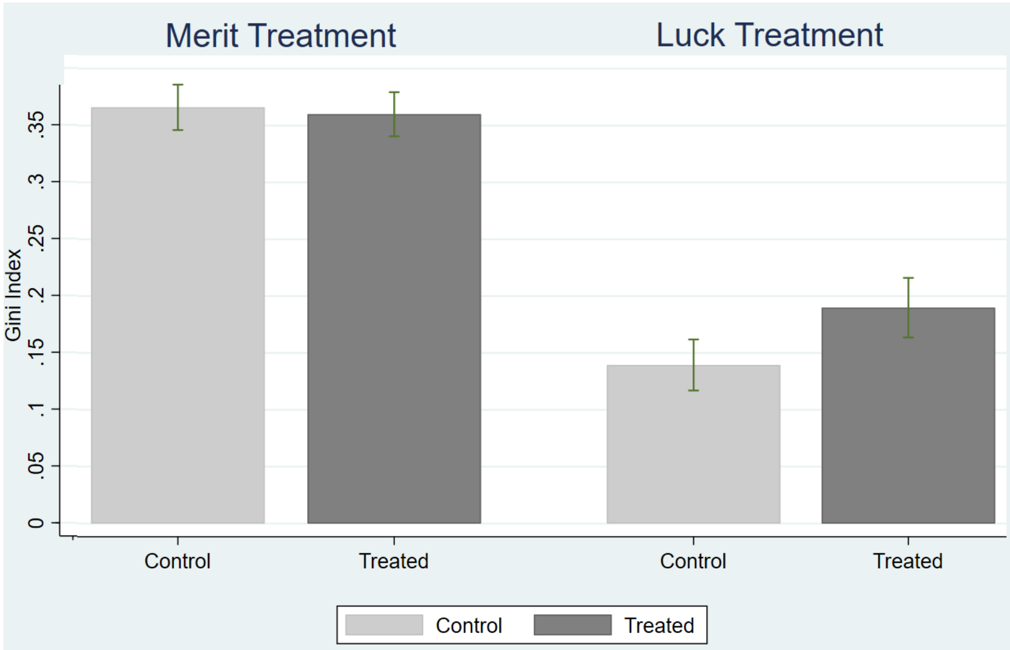

Figure 2. Gini Index as Resulting from Observers' Distributive Choices by Treatment and Condition

Note: The whiskers represent 95% confidence intervals.

under Merit (p=0.755, two-sided t-test)[9]. Similarly, in the Merit condition there is no significant difference across treatments in the share of participants who did not modify the initial unequal allocation of endowments (19 in treated vs 17 in control villages, χ^2 test, p=0.731).

The difference between the level of inequality chosen by spectators in treated and control villages is instead significant when inequality is determined by luck. In particular, participants in treated villages allocate significantly more to the lucky worker who initially received the whole endowment,

[9]The two-sided t-tests used for the main analysis compare the individual decisions of two samples N_t=N_c=288 of participants. The number of participants belonging to the subsamples that are compared in the heterogeneity analyses described below in this section is reported in Table B2 in Appendix B.

thus determining a significantly higher level of Gini index compared to participants in the control sample (two-sided t-test, p=0.027). We also notice that, under Luck, 30 participants in treated villages did not engage in redistribution at all, while only 17 participants in control villages left the initial allocation unmodified. These shares are statistically significantly different at the conventional level (χ^2 test, p=0.048).[10]

These results are confirmed when investigated in a regression framework. In model 1 of Table 1 we run a GLS regression with random effects at the subject level to account for the two allocation decisions reported by each individual subject. The dependent variable is the post-redistribution Gini index and the regressors include a dummy for Merit, a treatment dummy for villages where the reform was implemented, their interaction, and the set of controls specified in the pre-analysis plan. The positive and significant coefficient of *Merit* confirms that spectators implement larger inequality in Merit compared to Luck. The positive and significant coefficient of the variable *Treated* indicates that spectators in villages where the reform was implemented tolerate significantly more inequality compared to those in control villages, when inequality is determined by luck. By contrast, the sum of the coefficient for *Treated* and for the interaction term *Merit*Treated* is not statistically different from zero, confirming that, when inequality is determined by merit, the level of inequality chosen by spectators is on average not different in villages with or without PFR. The point estimate suggests that experiencing the PFR reform induced an increase of roughly 60% in tolerance for the inequality generated by luck.

[10]Results of a Logit regression with random effects at the individual level reported in Table B3 in Appendix B confirm that the likelihood to engage in redistribution is significantly lower for participants living in treated villages.

Table 1—Spectators' Distributional Choices

	Model 1	Model 2	Model 3	Model 4
Constant	0.099	0.100	0.125**	0.129**
	(0.061)	(0.061)	(0.058)	(0.059)
Merit	0.228***	0.228***	0.228***	0.228***
	(0.021)	(0.021)	(0.021)	(0.021)
Treated	0.061**	0.066**	0.068**	0.066**
	(0.030)	(0.030)	(0.030)	(0.030)
Merit×Treated	-0.058	-0.053	-0.053	-0.053
	(0.043)	(0.043)	(0.043)	(0.042)
dPFR		-0.021	-0.020	-0.019
		(0.038)	(0.037)	(0.038)
dPFR×Merit		-0.016	-0.016	-0.016
		(0.053)	(0.053)	(0.053)
Village Controls	N	N	Y	Y
Wealth Controls	N	N	N	Y
N.obs.	1152	1152	1152	1152
R2 (overall)	0.149	0.150	0.152	0.154

Note: Dependent variable: Gini index. GLS regression with random effects at the subject level. Robust standard errors clustered at the village level. Controls included in all regressions: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, self-reported weekly income, hectares of land owned, whether the house has cement floor, whether the household possess either a radio or a television, a motorbike or car, whether in the household somebody holds a bank account or a credit card, treatment order (the order in which the games are played). dPFR includes a dummy equal to 1 for participants in treated villages who do not have land parcels included in the PFR. Village Controls include: village population and whether the village is located in the South. Wealth Controls include: whether the house has electricity, whether the house has running water, hectares of land possessed by the family. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

In model 2 we introduce the dummy *dPFR*, which controls for those households resident in treated villages who took part in our experiment but who, in the post-experimental survey, reported to have never possessed a parcel of land which was included in the PFR reform.[11] We also include the

[11] This could have happened because a household does not possess land at all or because the parcels she possesses are located outside the administrative village boundaries – indeed the 2009-2011 PFR plan was only implemented for parcels of land within the selected villages. In our sample, we have 78 of these households.

interaction between *dPFR* and the Merit variable. In this way, the coefficient of the dummy *dPFR* captures the difference in the post-redistribution Gini index in the Luck condition between subjects living in treated villages who are landowners and those who did not possess land parcels affected by the reform. The qualitative results and point estimates of the *Merit* and *Merit*Treated* terms remain virtually unaffected. However, the sum of the estimated coefficients for *Treated* and *dPFR* is not statistically significant (χ^2 test, p=0.289), suggesting that first-hand experience of the land rights formalization plays a key role in determining the increase in inequality tolerance observed in Luck.

We then verify the robustness of these results by introducing additional controls for village-level characteristics (model 3) and by adding a series of proxies for individual wealth (model 4). In both cases, point estimates are very close to those resulting from model 2 specification and qualitative results remain unchanged. In Table B4 reported in Appendix B, we further investigate the robustness of our results re-estimating the models presented in Table 1 by implementing four different specifications of individual wealth – ranging from the self-reported rank of socio-economic conditions within the community to indicators of material wealth that could be inferred from the participant's house facilities. In all cases, results remain qualitatively the same. Moreover, we check whether the (randomized) order of the Merit or Luck condition in which spectators state their redistribution decisions affects the result. Table B5 reported in Appendix B replicates models 1 and 4 of Table 1 by separating between Merit-first and Luck-first conditions. The estimated coefficients of the variables *Treated*, *Merit*, and their interactions are quantitatively similar when the Merit or the Luck condition is proposed first, and point estimates are comparable to those of the main model specification (albeit the halved samples result in larger standard errors and weakly or not significant coefficients). Figure B3 in Appendix B displays the distribution choices in the Merit and Luck conditions dividing between the order of decisions. Finally, in Table B6 reported in Appendix B we re-estimated the main model specifications presented in Table 1 controlling for participants' experience of land-related conflicts, since conflicts frequency could have been affected by the reform. In all cases, the estimated results remain qualitatively the same and quantitatively similar to those presented in the main text.

We continue by performing the heterogeneity analysis as specified in the pre-analysis plan. First, we test whether the land rights formalization has produced diverse effects on tolerance for inequality, depending on whether participants in our sample had relatively easy access to paved roads or not. Indeed, distance from paved roads has proved to be strongly correlated with villagers' participation in market activities and access to the formal judiciary (Bonjean and Brunelin, 2013; Casaburi, Glennerster and Suri, 2013; Fabbri, 2021; for the sake of brevity, we refer to these two characteristics as "market integration" onward). We consider villagers living closer to a paved road than the sample median to be part of the high-market integration subsample and the remaining participants to have low market integration. In the top and bottom panels of Figure 3, the first block of bars displays the post-redistribution inequality that spectators have chosen in Merit and Luck, respectively, breaking up the sample into two groups of participants, characterized by high and by low levels of market integration, respectively. When inequality is determined by merit, spectators in treated

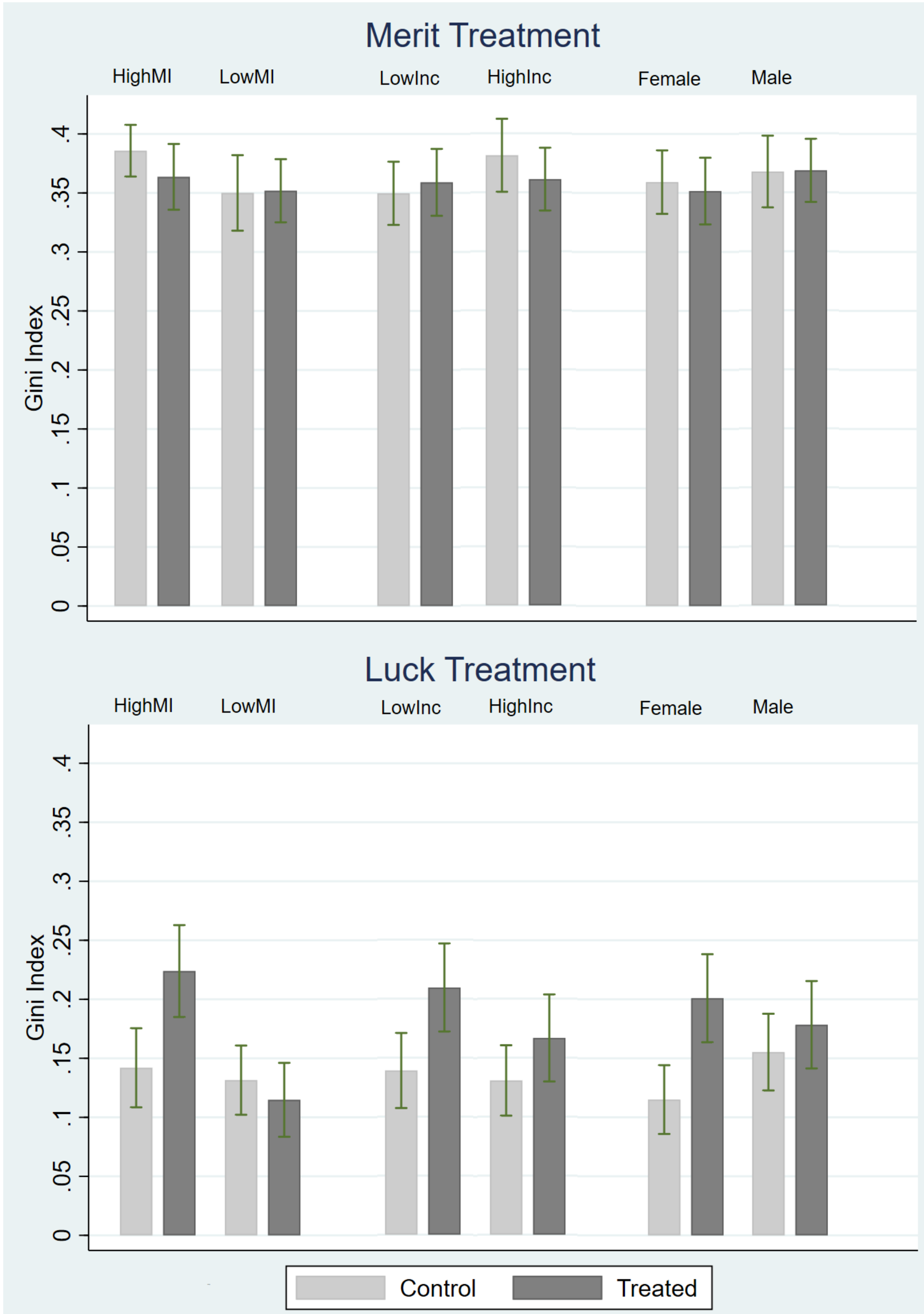

FIGURE 3. HETEROGENEITY ANALYSIS ON MARKET INTEGRATION, INCOME, AND GENDER: GINI INDEX AS RESULTING FROM OBSERVERS' DISTRIBUTIVE CHOICES BY TREATMENT AND CONDITION

Note: MI = Market Integration, as proxied by distance from paved roads; Inc = Income. "High" and "Low" indicates whether participants' market integration and income are above or below the sample median. The whiskers represent 95% confidence intervals.

villages choose levels of inequality that are not statistically different from those of the controls, both in the low- (two-sided t-test, p=0.96) and high-market integration conditions (p=0.41). Similarly, when the inequality is determined by luck and we focus on villagers in the low-market integration condition, there is no statistically significant difference between treated and control villagers (p=0.62). However, when focusing on the sample of participants characterized by high-market integration, the level of inequality chosen by spectators who have experienced the land rights formalization is significantly higher than the one chosen by control villagers (p=0.03).

These latter findings are confirmed in a regression framework. Table 2 implements the main model specifications of Table 1 but separates participants characterized by high or low levels of market integration. In model 1, the baseline category consists of spectators in Control villages in the Luck treatment who are characterized by a high level of market integration. The estimated coefficients *LowMI* and *LowMI**Treated* – which refer to control spectators with low-market integration and treated spectators with low market integration in the Luck treatment, respectively – are small, both not statistically different from zero, and not statistically different from each other, suggesting that participants in these two categories make on average similar redistribution choices. However, the coefficient *HighMI**Treated* which refers to spectators in treated villages characterized by high market integration in the Luck treatment, is positive and statistically significant at the conventional level. The point estimate suggests that the level of inequality generated by luck that this category of spectators chose is approximately twice as large as the one chosen by the three other categories. The negative and significant coefficient associated to *HighMI**Merit**Treated* indicates that, consistently with what already emerged from Table 1, the treatment effect on distributional preferences only emerges when inequality is generated by luck, but not when it originates from merit. The results of models 2, 3, and 4 – in which we added the controls for possessing land affected by PFR, village characteristics, and additional proxies for wealth, respectively – confirm that the increase in inequality that we observed in the Luck condition for spectators who have experienced the land rights formalization is driven by participants characterized by high-market integration.

The second dimension of heterogeneity we investigate concerns income. We divide spectators into a "high" and a "low" income category, depending on whether their household's weekly income is larger than the sample median. In the top and bottom panels of Figure 3, the second block of bars displays the post-redistribution inequality that spectators chose in Merit and Luck, respectively, breaking up the sample by income category. In Merit, the average level of inequality chosen by spectators is not statistically different across the four categories of high/low income in treated and control villages. The same result is true for inequality generated by luck for high-income spectators, since the post-redistribution inequality levels chosen by participants in control and treated villages in this income category are not statistically different (two-sided t-test, p=0.32). Instead, in the low-income category spectators in treated villages choose a significantly higher level of inequality compared to those in control (p=0.04). A regression analysis confirms the results. In Table 3, we re-estimated the main model specifications of Table 1 by breaking up the sample into income categories. Compared to the baseline category of low-income participants in control villages in the Luck condition, the coefficient of the interaction term *LowInc*Treated* indicating low-income participants in treated villages is positive and statistically significant in all model specifications. The point estimates suggest

Table 2—Spectators' Distributional Choices – Heterogeneity Analysis on Market Integration

	Model 1	Model 2	Model 3	Model 4
Constant	0.086	0.087	0.119**	0.122**
	(0.063)	(0.063)	(0.060)	(0.060)
LowMI	0.001	0.002	-0.019	-0.015
	(0.031)	(0.031)	(0.038)	(0.038)
HighMI×Merit	0.244***	0.244***	0.244***	0.244***
	(0.024)	(0.024)	(0.024)	(0.024)
LowMI×Merit	0.219***	0.219***	0.219***	0.219***
	(0.030)	(0.030)	(0.030)	(0.030)
HighMI×Treated	0.094**	0.100**	0.090**	0.092**
	(0.046)	(0.046)	(0.046)	(0.046)
LowMI×Treated	-0.011	-0.003	0.004	0.001
	(0.034)	(0.034)	(0.033)	(0.031)
HighMI×Merit×Treated	-0.104**	-0.104**	-0.104**	-0.104**
	(0.053)	(0.053)	(0.053)	(0.053)
LowMI×Merit×Treated	0.019	0.019	0.019	0.019
	(0.058)	(0.058)	(0.058)	(0.058)
PFR-Land Control	N	Y	Y	Y
Village Controls	N	N	Y	Y
Wealth Controls	N	N	N	Y
N.obs.	1152	1152	1152	1152

Notes: Dependent variable: Gini index. GLS regression with random effects at the subject level. Robust standard errors clustered at the village level. Controls included in all regressions: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, self-reported weekly income, hectares of land owned, whether the house has cement floor, whether the household possess either a radio or a television, a motorbike or car, whether in the household somebody holds a bank account or a credit card, treatment order. PFR-Land Control includes a dummy equal to 1 for participants in treated villages who do not have land parcels included in the PFR. Village Controls include: village population and whether the village is located in the South. Wealth Controls include: whether the house has electricity, whether the house has running water, self-reported rank of socio-economic status within the village (1-10). Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

Table 3—Spectators' Distributional Choices – Heterogeneity Analysis on Income

	Model 1	Model 2	Model 3	Model 4
Constant	0.098	0.099	0.124**	0.128**
	(0.061)	(0.061)	(0.057)	(0.058)
HighInc	0.005	0.005	0.005	0.005
	(0.025)	(0.025)	(0.025)	(0.026)
LowInc×Merit	0.211***	0.211***	0.211***	0.211***
	(0.027)	(0.027)	(0.027)	(0.027)
HighInc×Merit	0.251***	0.251***	0.251***	0.251***
	(0.024)	(0.024)	(0.024)	(0.024)
LowInc×Treated	0.081**	0.090***	0.089***	0.087***
	(0.032)	(0.032)	(0.032)	(0.032)
HighInc×Treated	0.036	0.044	0.048	0.047
	(0.038)	(0.039)	(0.038)	(0.038)
LowInc×Merit×Treated	-0.061	-0.061	-0.061	-0.061
	(0.043)	(0.043)	(0.043)	(0.043)
HighInc×Merit×Treated	-0.056	-0.056	-0.056	-0.056
	(0.059)	(0.059)	(0.059)	(0.059)
PFR-Land Control	N	Y	Y	Y
Village Controls	N	N	Y	Y
Wealth Controls	N	N	N	Y
N.obs.	1152	1152	1152	1152

Notes: Dependent variable: Gini index. GLS regression with random effects at the subject level. Robust standard errors clustered at the village level. Controls included in all regressions: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, self-reported weekly income, hectares of land owned, whether the house has cement floor, whether the household possess either a radio or a television, a motorbike or car, whether in the household somebody holds a bank account or a credit card, treatment order. PFR-Land Control includes a dummy equal to 1 for participants in treated villages who do not have land parcels included in the PFR. Village Controls include: village population and whether the village is located in the South. Wealth Controls include: whether the house has electricity, whether the house has running water, self-reported rank of socio-economic status within the village (1-10). Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

that the reform determined an increase of 60%-80% in tolerance for inequality generated by luck for low-income spectators in treated villages. Results

Table 4—Spectators' Distributional Choices - Heterogeneity Analysis by Gender

	Model 1	Model 2	Model 3	Model 4
Constant	0.082	0.083	0.109*	0.114*
	(0.062)	(0.062)	(0.058)	(0.060)
Male	0.051*	0.050*	0.052*	0.048*
	(0.029)	(0.029)	(0.029)	(0.028)
Female×Merit	0.244***	0.244***	0.244***	0.244***
	(0.031)	(0.031)	(0.031)	(0.031)
Male×Merit	0.212***	0.212***	0.212***	0.212***
	(0.026)	(0.026)	(0.026)	(0.026)
Female×Treated	0.091**	0.099***	0.100***	0.096***
	(0.037)	(0.036)	(0.035)	(0.035)
Male×Treated	0.029	0.037	0.039	0.041
	(0.042)	(0.044)	(0.044)	(0.044)
Female×Merit×Treated	-0.093*	-0.093*	-0.093*	-0.093*
	(0.050)	(0.050)	(0.050)	(0.050)
Male×Merit×Treated	-0.022	-0.022	-0.022	-0.022
	(0.054)	(0.054)	(0.054)	(0.054)
dPFR	N	Y	Y	Y
Village Controls	N	N	Y	Y
Wealth Controls	N	N	N	Y
N.obs.	1152	1152	1152	1152
R2 (overall)	0.151	0.152	0.154	0.155

Note: Dependent variable: Gini index. Random effects GLS regression. Robust standard errors clustered at the village level. Controls included in all regressions: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, self-reported weekly income, hectares of land owned, whether the house has cement floor, whether the household possess either a radio or a television, a motorbike or car, whether in the household somebody holds a bank account or a credit card, treatment order. dPFR includes a dummy equal to 1 for participants in treated villages who do not have land parcels included in the PFR. Village Controls include: village population and whether the village is located in the South. Wealth Controls include: whether the house has electricity, whether the house has running water, hectares of land possessed by the family. L = Luck treatment; M = Merit treatment; Trt = Treated villages where the reform was implemented; Ctrl = Control villages. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

in Table 3 also confirm that the treatment effect on distributional preferences only emerges when inequality is generated by luck but not when it originates from merit and that, regardless of the level of income, inequality is perceived as more acceptable in the Merit than in the Luck condition.

Finally, the third pre-registered dimension of heterogeneity that we consider concerns gender. The third block of columns in Figure 3 displays the post-redistribution inequality chosen in Merit (top panel) and Luck (bottom panel), separating between male and female participants. The results confirm the absence of any statistically significant difference across treatments for both genders, in Merit. Similarly, in Luck male participants display an indistinguishable level of redistribution in treated and control villages (t-test two-sided, p=0.51). Instead, female spectators in treated villages chose a level of inequality generated by pure luck that is roughly double and significantly higher than those in control (p=0.01). The results reported in Table 4 – that replicates the heterogeneity analyses described in Tables 2 and 3 above, this time controlling for gender – confirm that experiencing the reform reduced redistribution for female but not for male participants, and this difference emerges only when the original inequality is determined by pure luck.

Participants with comparatively low levels of tenure security under the customary system, who are well integrated in a market economy and with the logistical possibility of accessing the formal judiciary, are arguably those benefiting the most from the reform. In the rural context of a low-income developing country, individual wealth often reflects social rank and political connections. It is likely that more affluent rightholders already enjoyed a relatively good level of property rights protection under the socially-determined customary system. Therefore, the reform might have generated a comparatively stronger feeling of securing land ownership for villagers with relatively low socio-economic status. Indeed, previous research conducted in rural Benin shows that female farmers, who were the social group reporting the lowest level of perceived tenure security in a survey conducted before the land tenure reform, substantially increased the amount of long-term investments and virtually closed the investment gap between them and male farmers after receiving formal land titles (Goldstein et al., 2018). Similarly, it has been suggested that the possibility to adjudicate adverse claims over land using documentary evidence is more important where formal courts are easily accessible (Casaburi, Glennerster and Suri, 2013; Fabbri, 2021), and that using land as collateral is more valuable where market transactions are the norm (Arruñada, 2018). In sum, the heterogeneity analysis suggests that participants who experienced the greatest improvement in their tenure situations turn out to be also those displaying the strongest changes on distributional preferences.

V. Discussion and Conclusion

We identify a previously undocumented effect of assigning formalized property rights to individuals, showing that it fosters a tendency to tolerate higher levels of inequality when the unequal distributions depend exclusively on contingent factors, while it does not affect distributional preferences when inequality derives from merit. This pattern is driven by female participants and households with comparatively lower affluence, who live in villages characterized by easier access to markets and the formal judiciary; this suggests that those who benefited the most from the reform–in terms of increased tenure security–display the largest change in fairness views. This result stems from a research design that dispels ambiguities regarding participants' beliefs on the sources of inequality and that overcomes endogeneity issues characterizing non-experimental approaches, at the same time mitigating the external validity concerns connected to laboratory studies.

Previous studies conducted in rural Benin have shown that the Beninese property reform generated a sizable increase in prosociality for subjects with high levels of market integration (Fabbri, 2021). Our results document a parallel reduction in the willingness to redistribute for this category of participants. These findings do not contradict what the literature has found regarding the link between prosociality and distributional preferences in western societies. While a direct comparison between rural Africa and the different context of highly industrialized countries should be handled with care, previous research shows that US citizens display a lower willingness to redistribute (Almås, Cappelen and Tungodden, 2020) and, at the same time, higher levels of interpersonal trust and altruism compared to Norwegian ones (Falk et al., 2018).

But why does the assignment of property rights increase participants' tolerance for inequality generated by luck? Evidence from researches conducted in Benin suggests that the changes in distributional choices that we observe in treated villages are unlikely to be mediated by the reform's effects on possible determinants of distributional preferences, since altruism, risk preferences, wealth, or economic vulnerability were not significantly affected (Fabbri, 2021; Goldstein et al., 2016; Omondi, 2019). While the experiment was not specifically designed to investigate the drivers of a behavioral change, which at the time when we planned this study was still to be ascertained, we speculate about two possible explanations.

First, experiencing formal property rights might have reinforced spectators' perception that workers deserved their payments, even when the initial allocations are determined by pure luck (Lane, 1991). There is abundant evidence that interactions regulated by market-like institutions reduce participants' redistributive behavior and increase feelings of self-attribution (Babcock and Loewenstein, 1997; Bowles and Polania-Reyes, 2012; Hoffman et al., 1994). It is thus possible that, by repeatedly interacting in a reformed framework that approximates market-like situations, villagers have developed a traversal feeling of deservedness for owned goods that blurs the distinctions between acquisition processes based on merit or fortuitous circumstances. In line with this explanation, using a lab experiment Fabbri and Dari-Mattiacci (2021) showed that the reform significantly increased the willingness of Beninese villagers to respect the property rights of unknown strangers.

A second possibility might be that villagers in the treated sample adopt a dissonance-reduction strategy to self-justify their ownership of land (Bowles, 1998), which links our results to the recent literature on motivated beliefs (Bénabou and Tirole, 2016; Gino, Norton and Weber, 2016; Zimmermann, 2020). In the customary system, land cannot be individually owned or freely disposed of, and tenure rights are subject to redistributive obligations shared by all community members (Boltz, Marazyan and Villar, 2019). However, with the reform an "external" intervention awards to participants in treated villages the enjoyment of exclusive property rights. To morally justify their new condition, people "convince themselves [...] that the appropriate notions of fairness and justice are those that also happen to correspond to their own self-interest" (Gino, Norton and Weber, 2016, p.207). The acceptance of higher inequality determined by luck might reflect the process of self-adaptation and talking oneself into the legitimacy of individual ownership. Future research is necessary to shed light on the contribution of each of these possible mechanisms to determine the effects of property institutions on fairness views that we documented.

Our research suggests that a society's redistributive system is not uniquely a byproduct of its members' preferences for redistribution. Instead, it confirms that economic institutions play a key role in shaping people's acceptance of inequality. One implication is that institutional reforms which privatize access to economic resources may unintentionally reduce people's demand for redistribution and crystallize (or even worsen) social inequalities unrelated to individuals' achievements. While we clearly acknowledge that promoting individual ownership can improve the efficient use of resources and provide optimal incentives for economic development, our research warns that such reform efforts might need to be complemented by policies designed to prevent resulting aggravations of social inequalities and their associated problems.

REFERENCES

Aarøe, Lene, and Michael Bang Petersen. 2014. "Crowding out culture: Scandinavians and Americans agree on social welfare in the face of deservingness cues." *The Journal of Politics*, 76(3): 684–697.

Alesina, Alberto, and Nicola Fuchs-Schündeln. 2007. "Good-bye Lenin (or not?): The effect of communism on people's preferences." *The American Economic Review*, 97(4): 1507–1528.

Alesina, Alberto, and Paola Giuliano. 2015. "Culture and institutions." *Journal of Economic Literature*, 53(4): 898–944.

Alesina, Alberto, Stefanie Stantcheva, and Edoardo Teso. 2018. "Intergenerational mobility and preferences for redistribution." *American Economic Review*, 108(2): 521–54.

Alesina, Alberto, Yann Algan, Pierre Cahuc, and Paola Giuliano. 2015. "Family values and the regulation of labor." *Journal of the European Economic Association*, 13(4): 599–630.

Ali, Daniel Ayalew, Klaus Deininger, Godfrey Mahofa, and Rhona Nyakulama. 2019. "Sustaining land registration benefits by addressing the challenges of reversion to informality in Rwanda." *Land Use Policy*, 104317.

Almås, Ingvild, Alexander W Cappelen, and Bertil Tungodden. 2020. "Cutthroat capitalism versus cuddly socialism: Are Americans more meritocratic and efficiency-seeking than Scandinavians?" *Journal of Political Economy*, 128(5): 1753–1788.

André, Catherine, and Jean-Philippe Platteau. 1998. "Land relations under unbearable stress: Rwanda caught in the Malthusian trap." *Journal of Economic Behavior & Organization*, 34(1): 1–47.

Arrow, Kenneth, Samuel Bowles, and Steven N Durlauf. 2018. *Meritocracy and economic inequality.* Princeton University Press.

Arruñada, Benito. 2018. "Evolving practice in land demarcation." *Land Use Policy*, 77: 661–675.

Arruñada, Benito, Marco Fabbri, and Michael Faure. 2022. "Land Titling and Litigation." *The Journal of Law and Economics*, 65(1): 131–156.

Babcock, Linda, and George Loewenstein. 1997. "Explaining bargaining impasse: The role of self-serving biases." *Journal of Economic perspectives*, 11(1): 109–126.

Balafoutas, Loukas, Martin G Kocher, Louis Putterman, and Matthias Sutter. 2013. "Equality, equity and incentives: An experiment." *European Economic Review*, 60: 32–51.

Barr, Abigail. 2003. "Trust and expected trustworthiness: experimental evidence from Zimbabwean villages." *The Economic Journal*, 113(489): 614–630.

Becker, Sascha O, Katrin Boeckh, Christa Hainz, and Ludger Woessmann. 2016. "The empire is dead, long live the empire! Long-run persistence of trust and corruption in the bureaucracy." *The Economic Journal*, 126(590): 40–74.

Becker, Sascha O, Lukas Mergele, and Ludger Woessmann. 2020. "The separation and reunification of Germany: Rethinking a natural experiment interpretation of the enduring effects of communism." *Journal of Economic Perspectives*, 34(2): 143–171.

Blake, PR, K McAuliffe, J Corbit, TC Callaghan, O Barry, A Bowie, L Kleutsch, KL Kramer, E Ross, H Vongsachang, et al. 2015. "The ontogeny of fairness in seven societies." *Nature*, 528(7581): 258–261.

Boesch, Lukas, and Roger Berger. 2019. "Explaining Fairness." *Human Nature*, 30(4): 398–421.

Boltz, Marie, Karine Marazyan, and Paola Villar. 2019. "Income hiding and informal redistribution: A lab-in-the-field experiment in Senegal." *Journal of Development Economics*, 137: 78–92.

Bonjean, Catherine Araujo, and Stéphanie Brunelin. 2013. "Agricultural Trade in West and Central Africa: Are the Borders Abolished?" *Revue deconomie du developpement*, 21(1): 5–31.

Bowles, Samuel. 1998. "Endogenous preferences: The cultural consequences of markets and other economic institutions." *Journal of economic literature*, 36(1): 75–111.

Bowles, Samuel, and Sandra Polania-Reyes. 2012. "Economic incentives and social preferences: substitutes or complements?" *Journal of Economic Literature*, 50(2): 368–425.

Bubb, Ryan. 2013. "The evolution of property rights: state law or informal norms?" *The Journal of Law and Economics*, 56(3): 555–594.

Bénabou, Roland, and Jean Tirole. 2016. "Mindful Economics: The Production, Consumption, and Value of Beliefs." *Journal of Economic Perspectives*, 30(3): 141–164.

Cappelen, Alexander W, Astri Drange Hole, Erik Ø Sørensen, and Bertil Tungodden. 2007. "The pluralism of fairness ideals: An experimental approach." *American Economic Review*, 97(3): 818–827.

Cappelen, Alexander W, Karl O Moene, Erik Ø Sørensen, and Bertil Tungodden. 2013. "Needs versus entitlements—an international fairness experiment." *Journal of the European Economic Association*, 11(3): 574–598.

Casaburi, Lorenzo, Rachel Glennerster, and Tavneet Suri. 2013. "Rural roads and intermediated trade: Regression discontinuity evidence from Sierra Leone." *Available at SSRN 2161643*.

Corneo, Giacomo, and Hans Peter Grüner. 2002. "Individual Preferences for Political Redistribution." *Journal of Public Economics*, 83(1): 83–107.

Côté, Stéphane, Julian House, and Robb Willer. 2015. "High economic inequality leads higher-income individuals to be less generous." *Proceedings of the National Academy of Sciences*, 112(52): 15838–15843.

Dari-Mattiacci, Giuseppe, and Marco Fabbri. 2021. "How Institutions Shape Morality." *The Journal of Law, Economics, and Organization*. ewab016.

Deffains, Bruno, Romain Espinosa, and Christian Thöni. 2016. "Political self-serving bias and redistribution." *Journal of Public Economics*, 134: 67–74.

Deininger, Klaus, and Gershon Feder. 2009. "Land registration, governance, and development: Evidence and implications for policy." *The World Bank Research Observer*, 24(2): 233–266.

Delville, Philippe Lavigne. 2000. "Harmonising formal law and customary land rights in French-speaking West Africa." In *Evolving land rights, policy and tenure in Africa.* , ed. Camilla Toulmin and Julian Quan, 97–122. International Institute for Environment and Development (IIED).

Delville, Philippe Lavigne. 2006. "Registering and administering customary land rights: PFRs in West Africa."

Delville, Philippe Lavigne, and Anne-Claire Moalic. 2019. "Territorialities, spatial inequalities and the formalization of land rights in Central Benin." *Africa: The Journal of the International African Institute*, 89(2): 329–352.

Di Tella, Rafael, Sebastian Galiani, and Ernesto Schargrodsky. 2007. "The formation of beliefs: evidence from the allocation of land titles to squatters." *The Quarterly Journal of Economics*, 122(1): 209–241.

Dorling, Danny. 2015. *Injustice (revised edition): Why social inequality still persists.* Policy Press.

Engelmann, Dirk, and Martin Strobel. 2004. "Inequality aversion, efficiency, and maximin preferences in simple distribution experiments." *American economic review*, 94(4): 857–869.

Fabbri, Marco. 2021. "Property rights and prosocial behavior: Evidence from a land tenure reform implemented as randomized control-trial." *Journal of Economic Behavior & Organization*, 188: 552–566.

Fabbri, Marco. 2022. "Institutional quality shapes cooperation with out-group strangers." *Evolution and Human Behavior*, 43(1): 53–70.

Fabbri, Marco, and Giuseppe Dari-Mattiacci. 2021. "The Virtuous Cycle of Property." *Review of Economics and Statistics*, 1–48.

Fabbri, Marco, Giuseppe Dari-Mattiacci, and Matteo Rizzolli. 2022. "Expressive Property." *mimeo.*

Falk, Armin, Anke Becker, Thomas Dohmen, Benjamin Enke, David Huffman, and Uwe Sunde. 2018. "Global evidence on economic preferences." *The Quarterly Journal of Economics*, 133(4): 1645–1692.

Fehr, Ernst, and Karla Hoff. 2011. *Tastes, castes, and culture: the influence of society on preferences.* The World Bank.

Fisman, Raymond, Pamela Jakiela, Shachar Kariv, and Daniel Markovits. 2015. "The distributional preferences of an elite." *Science*, 349(6254): aab0096.

Gilens, Martin. 2009. *Why Americans hate welfare: Race, media, and the politics of antipoverty policy.* University of Chicago Press.

Gino, Francesca, Michael I. Norton, and Roberto A. Weber. 2016. "Motivated Bayesians: Feeling Moral While Acting Egoistically." *Journal of Economic Perspectives*, 30(3): 189–212.

Goldstein, Markus, Kenneth Houngbedji, Florence Kondylis, Michael O'Sullivan, and Harris Selod. 2018. "Formalization without certification? Experimental evidence on property rights and investment." *Journal of Development Economics*, 132: 57–74.

Goldstein, Markus P, Kenneth Houngbedji, Florence Kondylis, Michael B O'Sullivan, Harris Selod, et al. 2016. "Formalizing rural land rights in West Africa: early evidence from a randomized impact evaluation in Benin." The World Bank.

Haushofer, Johannes, and Ernst Fehr. 2014. "On the psychology of poverty." *Science*, 344(6186): 862–867.

Henrich, Joseph, Jean Ensminger, Richard McElreath, Abigail Barr, Clark Barrett, Alexander Bolyanatz, Juan Camilo Cardenas, Michael Gurven, Edwins Gwako, Natalie Henrich, et al. 2010. "Markets, religion, community size, and the evolution of fairness and punishment." *Science*, 327(5972): 1480–1484.

Henrich, Joseph, Steven J Heine, and Ara Norenzayan. 2010. "The weirdest people in the world?" *Behavioral and brain sciences*, 33(2-3): 61–83.

Hirschman, Albert. 1982. "Rival Interpretations of Market Society: Civilizing, Destructive, or Feeble?" *Journal of Economic Literature*, 20(4): 1463–84.

Hoffman, Elizabeth, Kevin McCabe, Keith Shachat, and Vernon Smith. 1994. "Preferences, Property Rights, and Anonymity in Bargaining Games." *Games and Economic Behavior*, 7(3): 346–380.

Hollander, Paul. 1999. "Political will and personal belief: the decline and fall of Soviet communism."

Hoy, Christopher, and Franziska Mager. 2018. "Can Information About Inequality and Social Mobility Change Preferences for Redistribution? Evidence from Randomized Controlled Trials in 11 High and Middle-Income Countries." Tax and Transfer Policy Institute Working Paper 1/2018 Available at SSRN: https://ssrn.com/abstract=3104379 or http://dx.doi.org/10.2139/ssrn.3104379.

Huppert, Elizabeth, Jason M Cowell, Yawei Cheng, Carlos Contreras-Ibáñez, Natalia Gomez-Sicard, Maria Luz Gonzalez-Gadea, David Huepe, Agustin Ibanez, Kang Lee, Randa Mahasneh, et al. 2019. "The development of children's preferences for equality and equity across 13 individualistic and collectivist cultures." *Developmental science*, 22(2): e12729.

Jakiela, Pamela. 2015. "How fair shares compare: Experimental evidence from two cultures." *Journal of Economic Behavior & Organization*, 118: 40–54.

Jakiela, Pamela, Edward Miguel, and Vera L Te Velde. 2015. "You've earned it: estimating the impact of human capital on social preferences." *Experimental Economics*, 18: 385–407.

Jha, Saumitra, and Moses Shayo. 2019. "Valuing peace: the effects of financial market exposure on votes and political attitudes." *Econometrica*, 87(5): 1561–1588.

Karadja, Mounir, Johanna Mollerstrom, and David Seim. 2017. "Richer (and holier) than thou? The effect of relative income improvements on demand for redistribution." *Review of Economics and Statistics*, 99(2): 201–212.

Kim, Byung-Yeon, Syngjoo Choi, Jungmin Lee, Sokbae Lee, and Kyunghui Choi. 2017. "Do institutions affect social preferences? Evidence from divided Korea." *Journal of Comparative Economics*, 45(4): 865–888.

Kuziemko, Ilyana, Michael I Norton, Emmanuel Saez, and Stefanie Stantcheva. 2015. "How elastic are preferences for redistribution? Evidence from randomized survey experiments." *American Economic Review*, 105(4): 1478–1508.

Lane, Robert E. 1991. *The market experience.* Cambridge University Press.

Nishi, Akihiro, and Nicholas A Christakis. 2015. "Human behavior under economic inequality shapes inequality." *Proceedings of the National Academy of Sciences*, 112(52): 15781–15782.

Ockenfels, Axel, and Joachim Weimann. 1999. "Types and Patterns: An Experimental East-West-German Comparison of Cooperation and Solidarity." *Journal of Public Economics*, 71(2): 275–287.

Omondi, Keneth. 2019. "MCC Evaluation Report - Impact Evaluation of Access to Land Project in Benin."

Ostrom, Elinor. 2009. *Understanding institutional diversity.* Princeton university press.

PEW. 2014. "Emerging and developing economies much more optimistic than rich countries about the future."

Piketty, Thomas, and Emmanuel Saez. 2014. "Inequality in the long run." *Science*, 344(6186): 838–843.

Rey-Biel, Pedro, Roman Sheremeta, and Neslihan Uler. 2018. "When income depends on performance and luck: The effects of culture and information on giving." *Experimental Economics and Culture*, 167.

Rodriguez-Sickert, Carlos, Ricardo Andrés Guzmán, and Juan Camilo Cárdenas. 2008. "Institutions influence preferences: Evidence from a common pool resource experiment." *Journal of Economic Behavior & Organization*, 67(1): 215–227.

Roth, Alvin E, Vesna Prasnikar, Masahiro Okuno-Fujiwara, and Shmuel Zamir. 1991. "Bargaining and market behavior in Jerusalem, Ljubljana, Pittsburgh, and Tokyo: An experimental study." *The American economic review*, 1068–1095.

Sands, Melissa L. 2017. "Exposure to inequality affects support for redistribution." *Proceedings of the National Academy of Sciences*, 114(4): 663–668.

Shiller, Robert J, Maxim Boycko, Vladimir Korobov, Sidney G Winter, and Thomas Schelling. 1992. "Hunting for Homo Sovieticus: situational versus attitudinal factors in economic behavior." *Brookings Papers on Economic Activity*, 1992(1): 127–194.

Somville, Vincent, Asbjørn Andersen, Simon Franklin, Tigabu Getahun, Andreas Kotsadam, and Espen Villanger. 2020. "Does wealth reduce support for redistribution? Evidence from an Ethiopian housing lottery." 18.

Starmans, Christina, Mark Sheskin, and Paul Bloom. 2017. "Why people prefer unequal societies." *Nature Human Behaviour*, 1(4): 0082.

Underwood, Emily. 2014. "Can disparities be deadly?" *Science*, 344(6186): 829–831.

Weber, Max. 1978. *Economy and society: An outline of interpretive sociology.* Vol. 1, Univ of California Press.

Zimmermann, Florian. 2020. "The Dynamics of Motivated Beliefs." *American Economic Review*, 110(2): 337–361.

Appendix A: Evidence that the Reform Affected the Mechanisms of Conflict Resolution and Villagers' Perception of Tenure Security

Studies on the effects of land rights formalization programs have shown that in some circumstances the titling efforts were not followed by changes in the existing systems of property rights and that, if not perceived useful by the local populations, formalized rights tend to revert to informality (Ali et al., 2019; Bubb, 2013). In Benin, two impact evaluations carried out one and three years after the reform report evidence that the randomization was successful. The reform produced an increase in long-term agricultural investments and fallowing. Goldstein et al. (2018) shows that this increase is concentrated on women and minorities who, under the customary regime, enjoyed a comparatively lower level of tenure security. However, no relevant changes in average income, farm yields, labor market participation, or conflict rate were registered (albeit, as noted by Goldstein et al., 2016, 2018, these results might depend on the short time-span between the impact evaluation and such a reform, whose effects are likely to take some time to materialize). However, importantly for our argument an increase in the use of documentary evidence to enforce land rights was observed in treated villages.

We confirm the latter finding in a survey that we administered to the participants contextually to our experiment both in treated and control villages. Results show that 93% of the respondents consider impossible for customary authorities to expropriate the land from a household who has registered PFR rights[12], and 89% of the sample think that PFR registered rights are

[12]The questions that were asked stated, respectively: "Imagine that a person in the

secured even if the rightholder engages in a dispute against a wealthier and more powerful contender. Indeed, 97% of respondents reported that, before purchasing a land parcel, they have requested or would try to obtain from the seller proof of official land title (either the cheaper and faster-to-obtain PFR registration introduced by the reform or, for respondents in control villages, the "Titre Foncier Rural" that is the standard formal property title offered by the Beninese government). No statistically significant differences emerged between treated and control villages in the answers to these questions.

An important finding from the survey is that the accessibility of those institutional facilities which make it possible to enforce the rights registered through the PFR – such as formal state courts – is strongly associated with a village's proximity to paved roads. If we split the sample of participants between those living closer than the median distance to paved roads and the others, in the latter subsample only 9% of the respondents report to know somebody who solved a land-related conflict in a state tribunal, compared to the 41% of respondents living closer to paved roads (the difference is strongly statistically significant, χ^2 test, $p < 1\%$). These proportions roughly match the share of subjects in our sample who actually experienced a conflict and solved the dispute in a formal court (40% of those living closer than the sample median to paved roads versus 16% of those living more distant). The finding is easily understood in light of the costs associated to accessing the formal judiciary for these two categories of respondents. Among the

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

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

Appendix B: Supplementary Analysis (intended for online publication)

Table B1—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 (d)	.49	.51	.73
age	40.0	36.8	.01
muslim religion (d)	.45	.41	.27
vodoun religion (d)	.19	.18	.91
married (d)	.89	.83	.02
nr. household members	9.8	10.0	.68
managing household finances (d)	.95	.95	.99
literate (d)	.40	.33	.08
born in village (d)	.69	.72	.41
years in village	32.3	30.9	.24
weekly income (CFA)	9,026	8,468	.59
land owned (Hect)	5.47	5.10	.65
house has concrete floor (d)	.64	.59	.23
house has electricity (d)	.36	.36	.99
house has water (d)	.26	.18	.02
house has radio-TV (d)	.63	.63	.99
household owns car (d)	.09	.07	.28
household owns moto (d)	.77	.78	.69
household has bank account (d)	.33	.27	.12
self-reported social-rank (1-10)	4.45	4.36	.56

Table B2—Number of Participants belonging to Treated and Control Villages in the Samples used for the Heterogeneity Analyses

	PFR Reform	Control
HighMI	198	108
LowMI	90	188
HighInc	132	125
LowInc	156	163
Male	142	146
Female	146	142

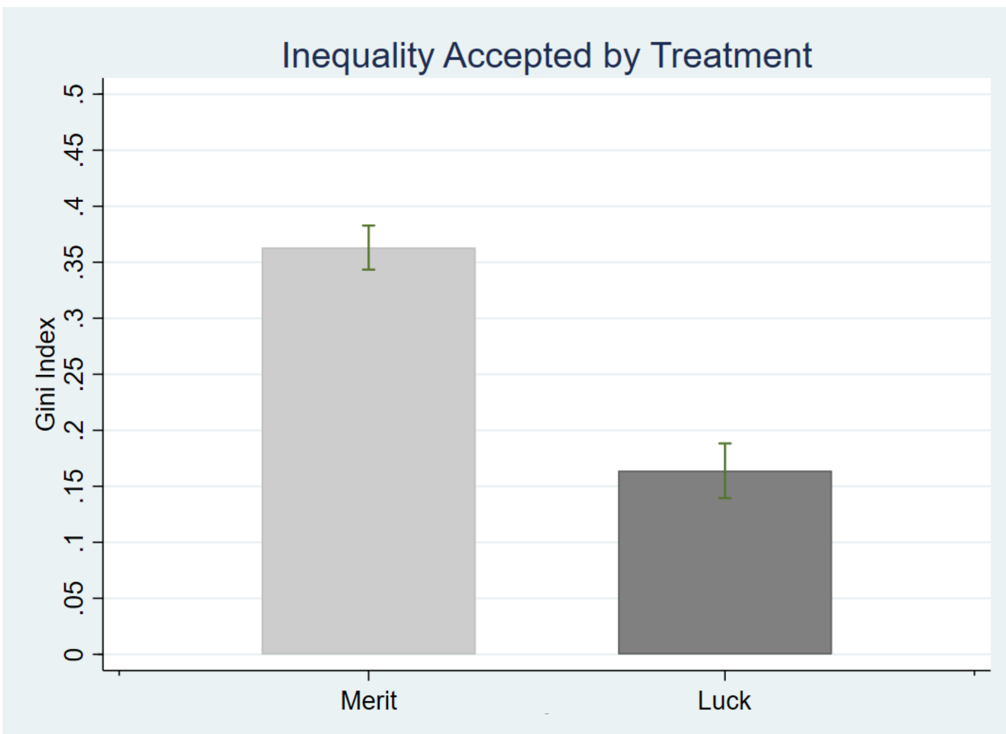

Figure B1. Gini Index as Resulting from Observers' Distributive Choices by Condition

Note: The whiskers represent 95% confidence intervals.

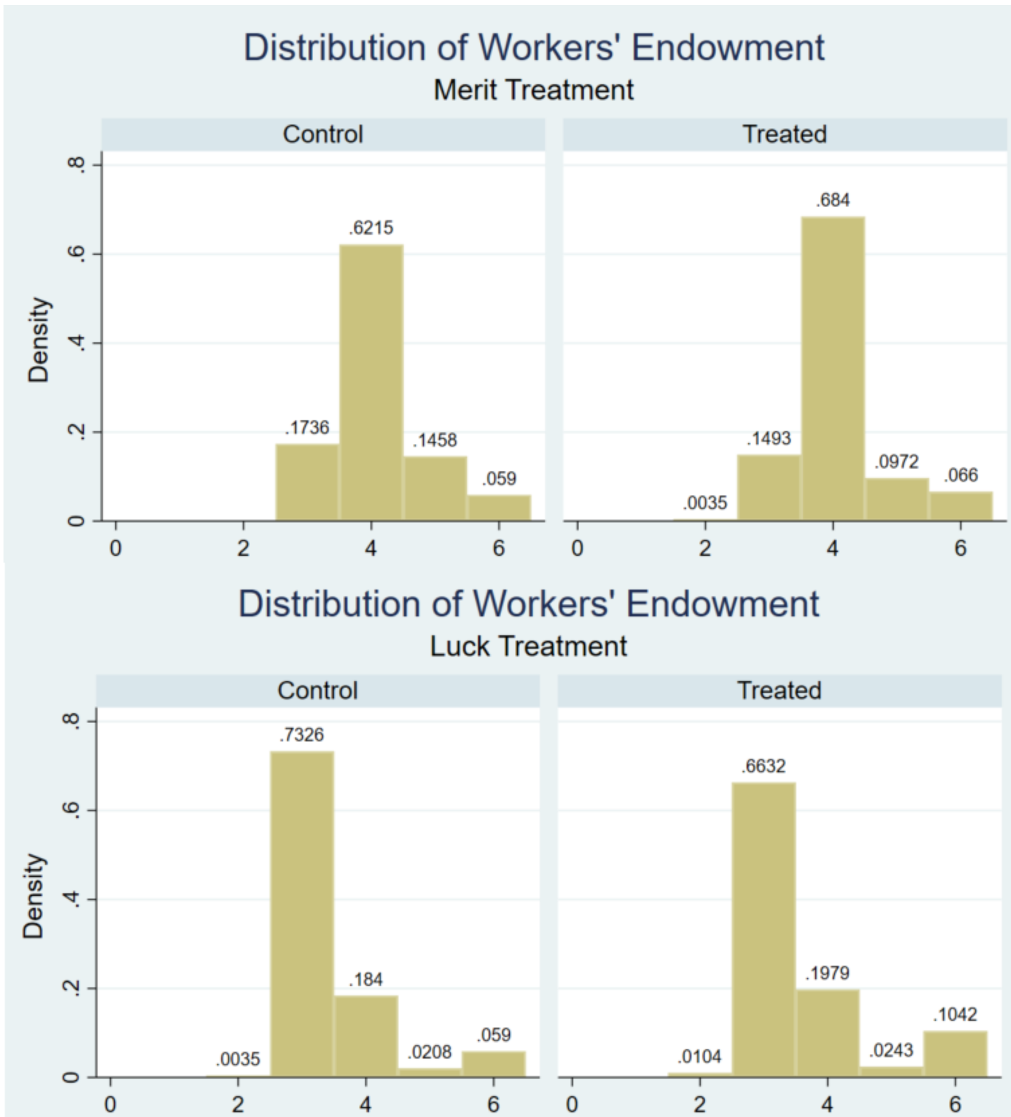

FIGURE B2. FREQUENCY OF SPECTATORS' CHOICE OF EACH OF THE SIX POSSIBLE DISTRIBUTION OPTIONS BY CONDITION

TABLE B3—SPECTATORS' LIKELIHOOD TO AVOID REDISTRIBUTION

	Model 1	Model 2	Model 3	Model 4
Merit	-0.000	0.000	0.000	-0.000
	(0.472)	(0.473)	(0.471)	(0.471)
Treated	0.835**	0.742*	0.812*	0.784*
	(0.401)	(0.409)	(0.446)	(0.442)
Merit×Treated	-0.583	-0.352	-0.350	-0.349
	(0.614)	(0.631)	(0.627)	(0.626)
dPFR		0.344	0.335	0.407
		(0.395)	(0.379)	(0.411)
dPFR×Merit		-1.002	-0.996	-0.991
		(0.878)	(0.870)	(0.866)
Village Controls	N	N	Y	Y
Wealth Controls	N	N	N	Y
Constant	-4.112***	-4.123***	-3.418***	-3.404***
	(1.055)	(1.061)	(0.981)	(1.005)
N.obs.	1152	1152	1152	1152
rho	0.306	0.103	0.146	0.534

Note: Dependent variable: dummy equal to 1 if the Spectator does not engage in redistribution. Logit regression with random effects at the subject level. Robust standard errors clustered at the village level. Controls included in all regressions: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, self-reported weekly income, hectares of land owned, whether the house has cement floor, whether the household possess either a radio or a television, a motorbike or car, whether in the household somebody holds a bank account or a credit card, treatment order (the order in which the games are played). dPFR includes a dummy equal to 1 for participants in treated villages who do not have land parcels included in the PFR. Village Controls include: village population and whether the village is located in the South. Wealth Controls include: whether the house has electricity, whether the house has running water, hectares of land possessed by the family. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

Table B4—Spectators' Distributional Choices - Different Measures of Individual Wealth

	Model 1	Model 2	Model 3	Model 4
Merit	0.228***	0.228***	0.228***	0.228***
	(0.021)	(0.021)	(0.021)	(0.021)
Treated	0.069**	0.070**	0.069**	0.070**
	(0.030)	(0.030)	(0.030)	(0.030)
Merit×Treated	-0.058	-0.058	-0.058	-0.058
	(0.043)	(0.043)	(0.043)	(0.043)
logincome	-0.009			
	(0.006)			
SEC-rank		-0.003		
		(0.005)		
Wealth-Land			-0.000	
			(0.001)	
Wealth-House	N	N	N	Y
Constant	0.134**	0.142***	0.128**	0.132**
	(0.056)	(0.054)	(0.059)	(0.059)
N.obs.	1152	1152	1152	1152
R2 (overall)	0.150	0.149	0.149	0.151

Note: Dependent variable: Gini index. Random effects GLS regression. Robust standard errors clustered at the village level. Controls included in all regressions: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, treatment order, a dummy equal to 1 for participants in treated villages who do not have land parcels included in the PFR, village population, whether the village is located in the South. Logincome = logarithm of household weekly income; Wealth-Land = hectares of land possessed; Wealth-House = whether the house has concrete floor, electricity, running water; SEC-rank = self-reported rank of socio-economic status within the village (1-10). Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

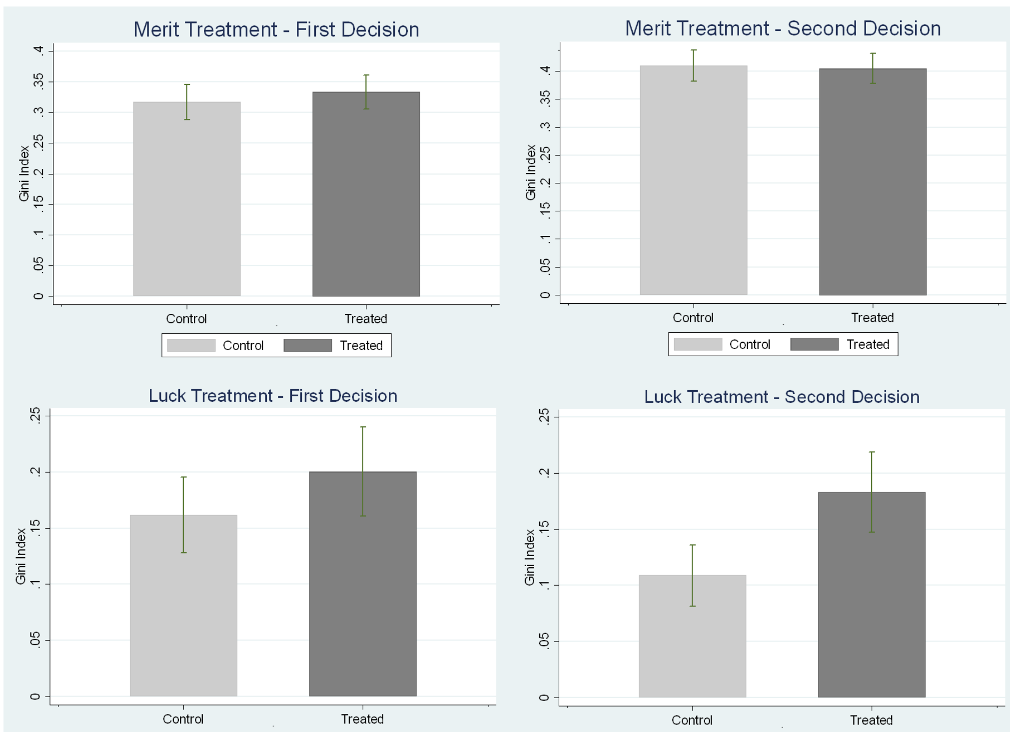

FIGURE B3. SPECTATORS' DISTRIBUTIONAL CHOICES - SEPARATING ORDER OF DECISIONS

Note: The whiskers represent 95% confidence intervals.

Table B5—Spectators' Distributional Choices - Separating Order of Decisions

	Model 1	Model 2	Model 3	Model 4
	Luck 1st		Merit 1st	
Merit	0.248***	0.248***	0.208***	0.208***
	(0.022)	(0.023)	(0.035)	(0.036)
Treated	0.060	0.077*	0.058	0.064*
	(0.050)	(0.047)	(0.036)	(0.034)
Merit×Treated	-0.044	-0.044	-0.058	-0.058
	(0.064)	(0.064)	(0.060)	(0.060)
Conflicts	-0.017	-0.018	-0.013	-0.008
	(0.034)	(0.034)	(0.033)	(0.034)
dPFR	N	Y	N	Y
Village Controls	N	Y	N	Y
Wealth Controls	N	Y	N	Y
Constant	0.203***	0.205**	0.072	0.109
	(0.072)	(0.096)	(0.100)	(0.091)
N.obs.	576	576	576	576
R2 (overall)	0.175	0.181	0.133	0.141

Note: Dependent variable: Gini index. Random effects GLS regression. Robust standard errors clustered at the village level. Models 1 and 2 include only those sessions with the first decision as *Luck*. Models 1 and 2 include only those sessions with the first decision as *Merit*. Controls included in all regressions: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, self-reported weekly income, hectares of land owned, whether the house has cement floor, whether the household possess either a radio or a television, a motorbike or car, whether in the household somebody holds a bank account or a credit card, treatment order. dPFR includes a dummy equal to 1 for participants in treated villages who do not have land parcels included in the PFR. Village Controls include: village population and whether the village is located in the South. Wealth Controls include: whether the house has electricity, whether the house has running water, hectares of land possessed by the family. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

Table B6—Spectators' Distributional Choices - Controlling for Conflicts Experienced

	Model 1	Model 2	Model 3	Model 4
Merit	0.228***	0.228***	0.228***	0.228***
	(0.021)	(0.021)	(0.021)	(0.021)
Treated	0.061**	0.069**	0.070**	0.069**
	(0.030)	(0.030)	(0.030)	(0.030)
Merit×Treated	-0.058	-0.058	-0.058	-0.058
	(0.043)	(0.043)	(0.043)	(0.043)
Conflicts	-0.017	-0.018	-0.013	-0.008
	(0.034)	(0.034)	(0.033)	(0.034)
dPFR	N	Y	Y	Y
Village Controls	N	N	Y	Y
Wealth Controls	N	N	N	Y
Constant	0.099	0.100	0.125**	0.137***
	(0.061)	(0.061)	(0.058)	(0.049)
N.obs.	1152	1152	1152	1152
R2 (overall)	0.150	0.151	0.152	0.154

Note: Dependent variable: Gini index. Random effects GLS regression. Robust standard errors clustered at the village level. Controls included in all regressions: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, self-reported weekly income, hectares of land owned, whether the house has cement floor, whether the household possess either a radio or a television, a motorbike or car, whether in the household somebody holds a bank account or a credit card, treatment order. dPFR includes a dummy equal to 1 for participants in treated villages who do not have land parcels included in the PFR. Village Controls include: village population and whether the village is located in the South. Wealth Controls include: whether the house has electricity, whether the house has running water, hectares of land possessed by the family. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

Table B7—Spectators' Distributional Choices - Difference Between Gini Indexes in Luck and Merit Treatment for each Participant

	Model 1	Model 2	Model 3	Model 4
Treated	0.049	0.042	0.041	0.035
	(0.041)	(0.041)	(0.041)	(0.042)
dPFR	N	Y	Y	Y
Village Controls	N	N	Y	Y
Wealth Controls	N	N	N	Y
Constant	-0.115	-0.116	-0.089	-0.103
	(0.086)	(0.087)	(0.093)	(0.100)
N.obs.	576	576	576	576
R2	0.030	0.031	0.030	0.041

Note: Dependent variable: Difference between Gini indexes in Luck and Merit treatment for each individual in the sample. OLS regression. Robust standard errors clustered at the village level. Controls included in all regressions: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, self-reported weekly income, hectares of land owned, whether the house has cement floor, whether the household possess either a radio or a television, a motorbike or car, whether in the household somebody holds a bank account or a credit card, treatment order. dPFR includes a dummy equal to 1 for participants in treated villages who do not have land parcels included in the PFR. Village Controls include: village population and whether the village is located in the South. Wealth Controls include: whether the house has electricity, whether the house has running water, hectares of land possessed by the family. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

As stated in Hypothesis 4 of the pre-analysis plan, we test whether the difference between the level of inequality chosen in Merit and Luck by each spectator is statistically significant across treatments.[13] Table B7 shows that the difference is not statistically significant in treated and control villages in any of the model specifications.

[13]Specifically, in the pre-analysis plan, we stated the following null hypothesis: "The difference between the levels of inequality acceptance when inequality is determined by luck and when inequality is determined by merit is the same in treated and control".

Appendix C: Experimental Instructions (intended for online publication)

B.1 Instructions for Workers

General Instructions

Please read the instructions below carefully.

The results from this experiment will be used in a research project. It is therefore important that you carefully read and follow all instructions. Note that you will remain anonymous throughout the experiment. We will only use your Worker ID to assign payments and check that you have not participated in this experiment before.

You will be paid a fixed participation fee of $1.00 and you may, depending on the actions you and others take, earn additional money. During the experiment you will earn points. The conversion rate is 6 points = $1.

You will be given detailed instructions on your screen before each part of the experiment. Please read the instructions to each part carefully.

If you have any questions regarding this experiment, you may contact projectMC2020@gmail.com.

I have read and understood the the above and want to participate in this study.

Yes No

Next

Part 1 - Production Phase

The first part ot the experiment is a production phase where you are given four assignments to work on. Go on to the next page to receive instructions for the first assignment.

Next

Instructions Assignment 1

In the first assignment you are asked to work on a sentence unscrambling task tor 5 minutes. Your performance will not be measured as there is no right or wrong answer, but we do ask you to work continuously on this assignment.

Description of the assignment:
You will be shown five English words and are asked to form a sentence or an expression by using four of these words. This means that each sentence or expression must only contain four words.

For example, it the words given to you are **"sky blue is the old"**, then you can construct the following sentence:

the sky is blue

Write the sentence or expression that you form into the blank space using your keyboard. Your answer will be submitted automatically after 20 seconds and you will auto-advance to five new words. This assignment will last for 5 minutes and we ask you to work continuously. When you have read and understood the instructions press Next to start the assignment.

Next

Word Unscrambling Task

Time left for this sentence: **0:04**

Time left for this task: **02:37**

red coat the was cows

Please form a sentence of four words with the above words.

the coat was red

Next

Instructions Assignment 2

In the second assignment you are once again asked to work on a sentence unscrambling task for 5 minutes. As before, your answer will be submitted automatically after 20 seconds and you will auto-advance to five new words.

Your performance will still not be measured as there is no right or wrong answer, but we do ask you to work continuously on this assignment as well.

Press Next to start the second assignment.

Next

Instructions Assignment 3

In the third assignment you are asked to work on a code recognition task for 5 minutes. For this assignment we will measure your performance by the number of points you receive. You will be informed about your score at the end of the assignment.

Description of the assignment:
On top of the page you will be shown a 3-digit code that you must find and check off from a matrix of 3-digit codes in random order. The assigned code will occur multiple times in the same matrix and you will be given 1 point for each correct marking. You will be subtracted 1 point if you check off a wrong code, but you will not lose any points for failing to check off all occurrences of the correct code.

Remark: The conversion rate is 6 points = $1.

Your matrix will be submitted automatically after 60 seconds and you will auto advance to the next page. This assignment will last for 5 minutes and after 5 minutes you will be taken to the fourth assignment.

Below you are shown a simplified example to make sure you understand the assignment. When you have read and understood the instructions press Next to start the assignment.

For example, the code you must check off is: 123

123	283
231	123
952	641
864	820
123	462
791	123

Next

Code Recognition Task

Time left for this matrix: **0:47**

Time left for this task: **04:03**

The code you must check off is: 600

226	709	119	173	763	506	553	375	715	170	262	918	137	252	926	926	599
962	673	641	275	150	250	989	387	764	333	126	265	600	904	818	787	368
374	769	638	338	476	187	516	582	408	486	139	270	821	233	449	690	593
620	714	681	883	196	555	330	926	983	604	625	699	130	259	128	238	101
880	952	979	502	455	286	702	212	785	570	200	210	786	299	463	201	191
151	668	449	477	164	635	279	582	508	921	788	277	543	324	244	327	947
879	864	137	313	566	307	578	132	677	234	147	769	564	114	103	723	874
858	793	278	678	258	481	775	654	732	181	804	967	760	226	600	780	853
375	695	648	749	906	503	489	196	709	824	174	884	251	592	509	228	873
353	301	771	973	534	598	111	650	344	334	866	766	116	572	184	642	652
133	372	801	440	484	229	710	941	319	299	670	759	995	216	913	193	352
820	578	288	695	115	256	945	743	817	848	294	226	283	824	518	561	952
433	608	171	996	903	200	862	285	199	757	669	199	896	429	494	561	629
124	739	756	248	912	600	621	927	127	264	977	260	543	724	326	925	720
172	600	954	895	589	102	148	584	907	919	178	694	784	903	442	869	569
108	795	734	768	758	430	234	380	359	173	852	832	125	794	753	390	643
653	383	427	431	545	961	577	600	882	161	600	866	479	855	950	745	352

Next

Instructions Assignment 4

In the fourth assignment you are once again asked to work on a code recognition task for 5 minutes. For this assignment we will measure your performance by the number of points you receive. You will be informed about your score at the end of the assignment.

On top of the page you will be shown a 3-digit code that you must find and check off from a matrix of 3-digit codes in random order. The assigned code will occur multiple times in the same matrix and you will be given 1 point for each correct marking. You will be subtracted 1 point if you check off a wrong code, but you will not lose any points for failing to check off all occurrences of the correct code.

Remark: The conversion rate is 6 points = $1.

Your matrix will be submitted automatically after 60 seconds and you will auto advance to the next page. This assignment will last for 5 minutes and after 5 minutes you will be taken to the second part of the experiment.

Press Next to start the fourth assignment.

Next

Part 2 - Determination of Payments

You nave now completed your work on all four assignments. We will now explain how you will paid for this work. After you have completed this HIT we will, for each assignment, match you with another participant who has completed the same assignment. The payment to you and the other participant is determined by a two-stage process. Below we explain this process in more detail.

First Stage:
Assignment 1: For this assignment, your earnings are determined by a lottery where each of you wins with equal probability 6 points or 0 points.

Assignment 2: For this assignment, your earnings are determined in the same way as for assignment 1.

Assignment 3: For this assignment, your earnings are determined by how productive you are. The participant with the highest score earns 6 points and the other participant earns 0 points. If you both have the same score, you will be matched with another participant.

Assignment 4: For this assignment, your earnings are determined in the same way as for assignment 3.

Remark: The conversion rate is 6 points = $1.

Second Stage:
For each assignment, a randomly selected third person will be given the opportunity to redistribute the earnings between you and the other participant. This person will not know the identity of you or the other participant, but will be informed about the nature of the assignment and your earnings for this assignment.

For each assignment, either you or the other participant earn 6 points and the other participant earns 0 points. If the third person chooses not to redistribute, each of you will be paid your earnings from the assignment. If the third person chooses to redistribute earnings, increasing the payment of the participant with the low earnings by 1 point decreases the other participant's payment by 1 point.

You will receive your payments for the four assignments within three weeks and it will be paid separately from your fixed participation fee of $1.00.

B.2 Instructions for Spectators

Instructions Condition Luck We now ask you to make a choice that has consequences for a real-life situation. A few days ago two individuals, let us call them worker A and worker B, were recruited via an international on-line market platform to conduct an assignment. They were each offered a participation compensation of 600 XOF regardless of what they were paid for the assignment. After completing the assignment, they were told that their earnings from the assignment would be determined by a lottery. The worker winning the lottery would earn 600 XOF for the assignment and the other worker would earn nothing for the assignment. They were not informed about the outcome of the lottery. However, they were told that a third person would be informed about the assignment and the outcome of the lottery, and would be given the opportunity to redistribute the earnings and thus determine how much they were paid for the assignment.

You are the third person and we now want you to choose whether to redistribute the earnings for the assignment between worker A and worker B. Your decision is completely anonymous. The workers will receive the payment that you choose for the assignment within a few days, but will not receive any further information.

Worker A won the lottery and earned 600 XOF for the assignment, thus worker B earned nothing for the assignment.

Please state which of the following alternatives you choose:

I do not redistribute:

- worker A is paid 600 XOF and worker B is paid 0 XOF.

I do redistribute:

- worker A is paid 500 XOF and worker B is paid 100 XOF.
- worker A is paid 400 XOF and worker B is paid 200 XOF.
- worker A is paid 300 XOF and worker B is paid 300 XOF.
- worker A is paid 200 XOF and worker B is paid 400 XOF.
- worker A is paid 100 XOF and worker B is paid 500 XOF.
- worker A is paid 0 XOF and worker B is paid 600 XOF.

Instructions Condition Merit We now ask you to make a choice that has consequences for a real-life situation. A few days ago two individuals, let us call them worker A and worker B, were recruited via an international on-line market platform to conduct an assignment. They were each offered a participation compensation of 600 XOF regardless of what they were paid for the assignment. After completing the assignment, they were told that their earnings from the assignment would be determined by their productivity. The most productive worker would earn 600 XOF for the assignment and the other worker would earn nothing for the assignment. They were not informed about who was the most productive worker However, they were told that a third person would be informed about the assignment and who was the most productive worker, and would be given the opportunity to redistribute the earnings and thus determine how much they were paid for the assignment.

You are the third person and we now want you to choose whether to redistribute the earnings for the assignment between worker A and worker B. Your decision is completely anonymous. The workers will receive the payment that you choose for the assignment within a few days, but will not receive any further information.

Worker A was most productive and earned 600 XOF for the assignment, thus worker B earned nothing for the assignment.

Please state which of the following alternatives you choose:

I do not redistribute:

- worker A is paid 600 XOF and worker B is paid 0 XOF.

I do redistribute:

- worker A is paid 500 XOF and worker B is paid 100 XOF.
- worker A is paid 400 XOF and worker B is paid 200 XOF.
- worker A is paid 300 XOF and worker B is paid 300 XOF.
- worker A is paid 200 XOF and worker B is paid 400 XOF.
- worker A is paid 100 XOF and worker B is paid 500 XOF.
- worker A is paid 0 XOF and worker B is paid 600 XOF.