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## Abstract

We study how distributional preferences are affected by a major property rights reform that transformed informal use-rights over land traditionally characterizing rural Beninese villages in a system akin to private ownership. The design combines the randomized control-trial implementation of the reform across villages with lab-in-the-field experiments eliciting villagers' distributional choices -- both when luck is the source of situational inequality and when an unequal distribution is originated by merit considerations. Results show that reforming allocation rules in the direction of impersonal market-alike institutions increases participants' acceptance of inequality determined by luck, while leaving participants' tolerance for inequality generated by merit unaffected.



JEL Classification: D31, C93, D01

Keywords: Fairness, Institutional Change, lab-in-the-field experiment, Land Tenure Reform, Land Titling



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# How Property Shapes Distributional Preferences

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*We study how distributional preferences are affected by a major property rights reform that transformed informal use-rights over land traditionally characterizing rural Beninese villages in a system akin to private ownership. The design combines the randomized control-trial implementation of the reform across villages with lab-in-the-field experiments eliciting villagers' distributional choices – both when luck is the source of situational inequality and when an unequal distribution is originated by merit considerations. Results show that reforming allocation rules in the direction of impersonal market-alike institutions increases participants' acceptance of inequality determined by luck, while leaving participants' tolerance for inequality generated by merit unaffected.*

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* Fabbri: Department of Economics and Business, University Pompeu Fabra & Barcelona GSE, 08005 Barcelona (ES), marco.fabbri@upf.edu. 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.

## I. Introduction

Rising economic inequality has been proved to be harmful for individuals and society (Haushofer and Fehr, 2014; Piketty and Saez, 2014; Underwood, 2014), and it is considered one of the greatest challenges of our time (PEW, 2014). For these reasons, in recent years scholars are 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). We contribute to this body of research by shedding light on the causal effects that different property rights institutions have on distributional preferences.

We study a large-scale reform – whose details are described in the next section – implemented in 2010 in Benin that transformed informal and socially-determined use rights over land in a system of registered and legally protected property rights. 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 transforms this system by recording rights over land parcels in public registries and granting to rightholders the possibility to defend registered rights in formal state courts, sell, or use land parcels as collateral, thus introducing a system akin to private ownership. We test how the experience of the reform have shaped distributional preferences and what kind of inequalities can be considered fair.

Our research focus is motivated by the long-debated argument that economic institutions have an important influence on the evolution of values, tastes, and behavioral traits (Fehr and Hoff, 2011; Ostrom, 2009; Rodriguez-Sickert, Guzmán and Cárdenas, 2008). With respect to the effects of markets on preferences, 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 regulated by status and social rank and corresponding to specific rights and obligations (Delville et al., 2000). In contrast, market-alike institutions like the property rights system introduced by the Beninese reform are characterized by impersonal and ephemeral interactions (Weber, 1978, p.636). Thus, allocation rules in kith-and-kin communities characterized by stable membership differ from market-alike institutions for the lack of impersonality and ephemerality (Lane, 1991). We verify whether the replacement of socially-determined land rights with market-alike property institutions influences participants' concept of fairness and redistributive norms.[1]

Our main contribution consists in proposing a research design that overcomes the endogeneity issues characterizing the relationship between insti-

[1]There are 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 idea. The authors report descriptive evidence from rural Rwanda where the customary norms of redistributing land in favor of landscarce community members were suddenly ceased to apply following the introduction of formalized land rights and the possibility to privately purchase land parcels (on this point, see also Deininger and Feder, 2009). As we discuss below, case studies and research based on observational data cannot sort out endogeneity issues. We complement these approaches by proposing a research design that can establish the direction of the causal link between institutions and fairness views.

tutions and distributional preferences, making possible to isolate the causal effects of different types of property institutions on fairness views while mitigating external validity concerns. The key element for our identification strategy is that the Beninese reform is first case of property rights reform that was implemented as randomized control-trial (RCT) on a large scale. We make use of the RCT implementation of the reform to verify whether experiencing formal property institutions influences the level of inequality tolerance and on which types of inequalities (i.e. whether generated by individual merit or depending from luck) can be considered fair. To do so, ten years after the reform implementation we conduct a lab-in-the-field experiment – described in Section II – that replicates Almås, Cappelen and Tungodden (2020) in a sample of villages included in the RCT pool, in which participants are requested to choose how to distribute payments among anonymous pairs of workers who have previously completed an online effort task.

The idea of studying how a society's organization and its institutions influence distributional preferences is not new in the literature. Several contributions have analyzed cross-country variations in redistributive policies, reporting that cultural heterogeneity concerning causal attribution to poverty (Aarøe and Petersen, 2014; Alesina, Stantcheva and Teso, 2018; Arrow, Bowles and Durlauf, 2018; Gilens, 2009), beliefs regarding the efficiency of redistributive agencies (Hoy and Mager, 2018; Kuziemko et al., 2015; Sands, 2017), and the distributional preferences held by individuals in a given society are associated with the observed differences (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) suggest that, for instance, elites have distributional preferences that differ from those of the general population (Fisman et al., 2015), that high-inequality environments are associated with larger inequality tolerance for wealthy individuals (Côté, House and Willer, 2015; Nishi and Christakis, 2015), and that cross-cultural differentiation in distributional choices can be observed already in children (Blake et al., 2015).

One problem common to these contributions is that they cannot sort out whether the observed changes in behavior that follow subjects' exposure to the new institutional environment reflect a modification of distributional preferences or, for instance, a change in beliefs concerning the efficiency of the redistributive system or the deservedness of the wealthier. To answer this question, in a recent contribution Almås, Cappelen and Tungodden (2020) run 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 the experimental design rules 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 share different preferences for redistribution.

We complement the aforementioned literature and the findings of Almås, Cappelen and Tungodden (2020) by proposing a research design that resolves possible ambiguities regarding the role played by beliefs on the redistribution costs and the source of inequality and, at the same time, makes it possible to isolate the causal effects of institutions on distributional preferences. Our identification strategy based on a RCT dispels endogeneity concerns present in cross-cultural studies.[2] Furthermore, the use of lab-in-the-field experiments 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 experiment participants involves a sample of the population of the societies we visited which is not limited to students.[3]

The results show that participants who experienced the introduction of formal property rights redistribute significantly less when the initial inequal-

[2]Studies based on the comparison of different societies cannot be sure to isolate the effects of institutions on distributional preferences, since cross-country or cross-population comparisons do not account for possible self-selection into a specific social group of the individuals considered. 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 preferences' evolution. 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.

[3]Previous studies have used 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, only short-term effects can be detected, and the sample of participants is usually composed of college students not representative of the general population (Henrich, Heine and Norenzayan, 2010). Others 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). 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, the use of survey responses and observational data often makes it impossible to attribute the estimated differences in inequality tolerance to a change in distributional preferences rather than, for instance, a modification of beliefs regarding the source of inequality (Cappelen et al., 2007).

ity of workers' payoffs is generated by pure luck. Instead, the reform does not affect the redistribution decisions when the inequality between workers was originated by merit considerations. We show that these results are driven by low-income subjects living close to paved roads – and so to markets and state courts, suggesting that villagers who de facto benefited the most from the reform in terms of access to credit opportunities and enhanced legal protection are also those displaying the largest effects on distributional preferences. In Section IV, we discuss how these results are consistent with the argument that market-alike institutions boost individuals' self-attribution, as well as with the implementation of a dissonance-reduction strategy displayed by treated participants.

The remainder of the paper presents the pre-registered empirical strategy, including details of the reform and of the experimental design (Section II), followed by the results (Section III), and by a concluding discussion (Section IV).

## II. Empirical Strategy

### *A. Research Design*

The empirical strategy was specified in a pre-analysis plan that was registered at the AEA RCT Registry[4] before we collected the data, and included the different hypotheses to be tested, the regression approach, the dimensions to be studied in the heterogeneity analysis. Our experiment consists in a distributional task where a spectator has to allocate resources between two workers, as in Almås, Cappelen and Tungodden (2020), and involves a total of 1152 participants. In the experiment *workers* individually complete

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

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[5].

Workers (n=576) have been 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 to determine the variable payment provisionally received (before the spectator's redistribution takes place). The provisional payment relative to each specific effort task was equal to CFA 600 (equivalent to $1) to one worker in the pair and nothing to the other worker. There were two different conditions to determine which worker in the pair received the provisional payment – and so which was the source of inequality. In the first two tasks the provisional payment was determined by "Luck" and a lottery randomly selected one of the two workers who received the 600 CFA. In the last two tasks instead the provisional payment was determined by "Merit". The worker in the pair who performed the best in the effort task received the 600 CFA. The workers were informed that the amount granted to one worker in the pair as provisional payment could be redistributed within the pair by an anonymous third-party, and that the decision of the third-party will determine the final payments.

Spectators (n=576) were recruited during fieldwork sessions among the local population of 32 Beninese rural villages to make choices that have monetary consequences for the workers but not for themselves (the details

[5]Please 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).

of the recruitment procedures are reported in Section II). Each spectator was matched with a pair of workers, informed that the workers received a fixed payment of $1 to take part to four effort tasks plus a variable payment for each effort task completed, shown the provisional payments for the two workers, and explained 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 does not involve costs and that the decision will determine the final payments relative to that effort task for the workers. 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 of the spectators were exposed to the two conditions in reverse order. The spectators made their choices in the first condition without knowledge that they will be asked to take a second decision.

We combined this experimental design to the RCT implementation process 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

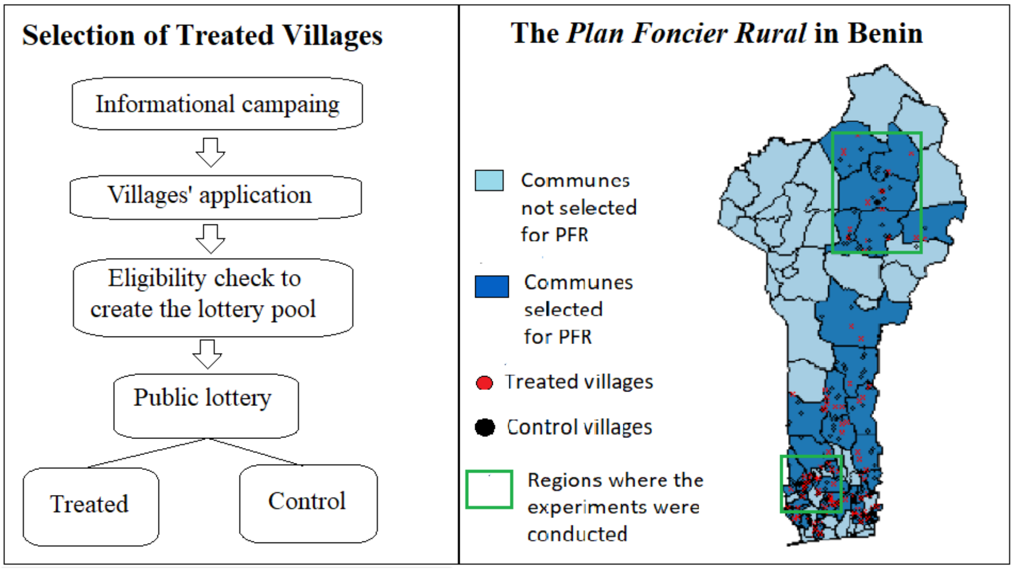


Figure 1. The Plan Foncier Rural in Benin

*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.

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 repository (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 or use registered plots as collateral, and the certificates registering possessory 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, 2020).

The implementation process of PFR took place in 2010-2011 and it 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 for instance population size and being located in rural areas).[6] Second, a subsample of 291 villages was selected via public lottery, 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.

In Appendix A, we discuss in details the evidence collected by an impact evaluation (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 are observed, villagers with easier access to legal facilities declare to perceive registered land as substantially more secure against conflicting claims and report to use more often and more successfully formal courts as conflict resolution mechanisms in land-related disputes.

We make use of the RCT implementation of the property rights reform in order to elicit spectators' distributive decisions from participants in villages that have been randomly selected to have the reform implemented (treated villages) and compare them 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

[6] Each of the 576 villages included in the lottery pool volunteered to receive the PFR. This imply 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 would be the effect of a super-imposed institutional reform, for which there is no explicit local demand, on preferences.

the spectators which, in our two-person setting, is equal to the (slightly modified version of) Gini coefficient[7]:

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

where worker $A_i$ is the one who has the highest pre-distribution earnings. 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.

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 – a proxy for market integration and access to the formal legal system, gender, and income.

### *B. Experimental Procedures*

The data collection took place between December 2019 and March 2020. In the remainder of the section, we separately provide details regarding the

[7]Calculating the two-person Gini coefficient would imply dividing the payoffs' difference by average income, thus yielding in our setting an interval [0, .66]. Since it does not affect the analysis, we prefer using the adapted version of the coefficient, which uses total income at the denominator and results in an interval [0, 1].

recruitment processes and tasks of workers and spectators.

*Workers.* Workers (whose identity remained unknown) have been recruited from Amazon Mechanical Turk (AMT), an international online 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, workers were randomly paired with another worker who has also completed the same assignment to determine the endowment received for the specific effort task (before the spectator's redistribution takes place). The pair formed in such a way is then matched with a spectator. The worker does not make distributive choices. 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 have been randomly selected among the list of villages included in the PFR for the regions of Couffou and Mono (in the South of the country) and Alibori and Borgou (in the North). The day before the experiment one RA visited the village and requested voluntary participation in the research study to the local population. Among the people who showed up at the convened time, we randomly recruited 18 participants (9 males and 9 females, older than 18 years old, and maximum 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. The spectators took part in the experiment described above in which two distributive choices are taken, in a post-experimental survey, and in other incentivized tasks not related to this project. Each session lasted three hours and on average participants earned CFA 2600 ($ 4,8) in total. In stating each distributive choice, the spectators will determine the payment of a pair of workers.

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 relative to Condition Luck and subsequently the decision relative to Condition Merit. 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 the pre-specified set of non-incentivized survey questions regarding: age, gender, religion, marital status, number of family members, participation to household finance management, education, literacy, village of birth, years of residence in the village, income.

## III. Results

In Table B1 in Appendix B we compare the observables elicited in the post-experimental survey across the treatment branches. The samples are well-balanced, with the exception of participants in treated sample being on average slightly older, more likely to be married, and living 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 through migration in one of the treatment branches. To do so, we collected data regarding participants' village of origin, the eventual reason leading to migration, and the number of years they have been living in the village. Only 35 out of 576 participants were not already resident of the village when the PFR reform was implemented, 20 in treated villages and 15 in control. The difference in not statistically significant ($\chi^2$ 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 to the experiment ($\chi^2$ test, p>10%) nor between the number of years they spent in that village (two sided t-test, p>10%).

As a preliminary step in the analysis, we compare distributional choices in Merit and Luck, testing 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, in the Merit condition, after spectators' redistribution decisions the Gini index is on average substantially larger 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

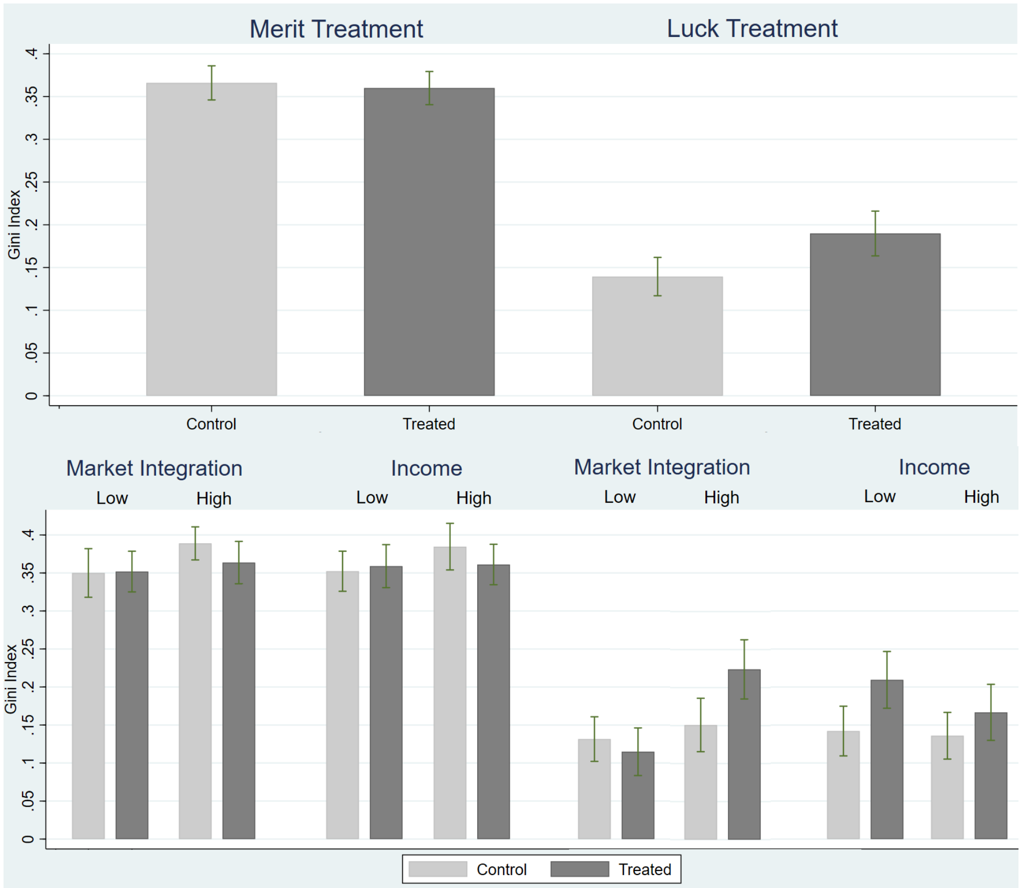


Figure 2. Gini Index as Resulting from Observers' Distributive Choices by Treatment and Condition

*Note:* The upper panel consider separately when Merit or Luck determine the initial inequality. The lower panel further isolates the heterogeneous effects in terms of participants' high or low levels of market integration and income. Vertical bars report standard errors.

inequality (Almås, Cappelen and Tungodden, 2020).

We proceed 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

Table 1—Spectators' Distributional Choices

| | 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** | 0.068** | 0.070** | 0.070** |
| | (0.030) | (0.030) | (0.030) | (0.030) |
| merit×Treated | -0.058 | -0.058 | -0.058 | -0.059 |
| | (0.043) | (0.043) | (0.043) | (0.042) |
| PFR-Land Control | 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 |

*Note:* Dependent variable: Gini index. Random effects OLS regression. Standard errors robust for clustering 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 Control includes: 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.

resident in treated and control villages. The Gini index after spectators' redistribution in the Merit treatment 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 in the Merit treatment ($p$=0.755, two-sided t-test, $N_t$=$N_c$=288). 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, thus determining a significantly higher level of Gini index compared to participants in the control sample (two-sided t-test, $p$=0.027, $N_t$=$N_c$=288).

These results are confirmed when investigated in a regression framework. In model 1 of Table 1 we run a OLS regression with random effects where 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, when inequality is determined by luck. By contrast, the sum of the coefficient for *Treated* and of 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 estimated increase of roughly 60% of tolerance of inequality generated by luck.

In model 2 we introduce an additional dummy 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.[8] The qualitative results and point estimates of the *merit* and *merit**Treated* terms remain virtually unaffected. We then verify the robustness of these results by introducing additional controls for village-level characteristics (model 3) and

[8]This could have happened because an 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.

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 B2 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 B3 reported in Appendix B replicates models 1 and 4 of Table 1 by separating between the Merit and Luck conditions order. 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 B2 in Appendix B displays the distribution choices in the Merit and Luck conditions dividing between the order of decisions. Finally, in Table B4 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 for participants who have relatively easy access to paved roads or not in our sample. 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; to be brief, we refer to these two characteristics as "market integration" onward). We consider villagers living closer than the sample median to a paved road to be part of the high-market integration subsample and the remaining participants to have low market integration. In the bottom panel of Figure 2, the first and third blocks of bars display the post-redistribution inequality that spectators have chosen in Merit and Luck, respectively, breaking up the sample between participants characterized by high or low levels of market integration. When inequality is determined by Merit, spectators in villages with PFR and those in control villages choose levels of inequality that are not statistically different both in the low- (two-sided t-test, $p$=0.96, $N_t$=90 $N_c$=180) and high-market integration conditions (two-sided t-test, $p$=0.41, $N_t$=198 $N_c$=108). 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 (two-sided t-test, $p$=0.62, $N_t$=90 $N_c$=180). 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 that chosen by control villagers (two-sided t-test, $p$=0.03, $N_t$=198 $N_c$=108).

These latter findings are confirmed in a regression framework. Table

2 zooms on spectators' distributional choices in the Luck treatment and implements the main model specifications of Table 1 but separates participants characterized by high and low levels of market integration. In model 1, the baseline category consists of spectators in Control with low market integration. The estimated coefficients of the interaction terms *Control*High–MI* and *Treated*Low–MI*, which refer to control spectators with high-market integration and treated spectators with low market integration, respectively–are small and not statistically different from zero, suggesting that participants in these two categories make on average similar redistribution choices. However, the coefficient *Treated*High–MI* which refers to spectators in treated villages characterized by high market integration is positive and statistically significant at the 1% level. The point estimate suggests that the level of inequality generated by luck that this category of spectators chose is approximately double of that chosen by the three other categories. The results of model 2, in which we added the controls for possessing land affected by PFR, village characteristics, and additional proxies for wealth, confirm that the increase in inequality generated by luck that we observed for spectators who have experienced the land rights formalization is driven by participants characterized by high-market integration in our sample.

The second dimension of the heterogeneity analysis that we investigate concerns income. We divide spectators in two "high" and "low" income categories, according to whether their household's weekly income is larger than the sample median. In the bottom panel of Figure 2, the second and fourth blocks of bars display the post-redistribution inequality that spectators have chosen in Merit and Luck, respectively, breaking up the sample

Table 2—Spectators' Distributional Choices - Heterogeneity Analysis

| | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| **Luck** | | | | |
| Ctrl*H_MI | -0.001 | 0.015 | | |
| | (0.031) | (0.038) | | |
| Trtd*L_MI | -0.011 | 0.001 | | |
| | (0.034) | (0.031) | | |
| Trtd*H_MI | 0.093*** | 0.107*** | | |
| | (0.034) | (0.036) | | |
| Ctrl*H_Inc | | | 0.005 | 0.005 |
| | | | (0.025) | (0.026) |
| Trtd*L_Inc | | | 0.081** | 0.087*** |
| | | | (0.032) | (0.032) |
| Trtd*H_Inc | | | 0.042 | 0.052 |
| | | | (0.043) | (0.043) |
| (Trtd*H_MI)-(Ctrl*H_MI) | p=.040 | p=.047 | | |
| (Trtd*H_Inc)-(Ctrl*H_Inc) | | | p=.337 | p=.224 |
| **Merit** | | | | |
| Ctrl*L_MI | 0.219*** | 0.219*** | | |
| | (0.030) | (0.030) | | |
| Ctrl*H_MI | 0.243*** | 0.258*** | | |
| | (0.023) | (0.033) | | |
| Trtd*L_MI | 0.226*** | 0.238*** | | |
| | (0.034) | (0.036) | | |
| Trtd*H_MI | 0.233*** | 0.246*** | | |
| | (0.029) | (0.035) | | |
| Ctrl*L_Inc | | | 0.211*** | 0.211*** |
| | | | (0.027) | (0.027) |
| Ctrl*H_Inc | | | 0.256*** | 0.256*** |
| | | | (0.034) | (0.034) |
| Trtd*L_Inc | | | 0.231*** | 0.237*** |
| | | | (0.031) | (0.032) |
| Trtd*H_Inc | | | 0.236*** | 0.246*** |
| | | | (0.035) | (0.038) |
| (Trtd*L_MI)-(Ctrl*L_MI) | p=.861 | p=.668 | | |
| (Trtd*H_MI)-(Ctrl*H_MI) | p=.732 | p=.696 | | |
| (Trtd*L_Inc)-(Ctrl*L_Inc) | | | p=.526 | p=.427 |
| (Trtd*H_Inc)-(Ctrl*H_Inc) | | | p=.604 | p=.811 |
| Controls: | | | | |
| PFR-Land | N | Y | N | Y |
| Village | N | Y | N | Y |
| Wealth | N | Y | N | Y |
| Constant | 0.087 | 0.107 | 0.104 | 0.133** |
| | (0.069) | (0.069) | (0.068) | (0.067) |
| N.obs. | 1152 | 1152 | 1152 | 1152 |

*Note:* Dependent variable: Gini index. Random effects OLS regression. Standard errors robust for clustering at the village level. Trtd = Treated; Ctrl = Control; H = High; L = Low; MI = Market Integration (proxied by distance from paved road); Inc = Household Weekly Income. The controls are described in 1. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively. For the tests of equality of coefficients (Trtd*H_)-(Ctrl*H_) it is reported the p-value of the F-statistic.

between income categories. In Merit, the average level of inequality chosen by spectators is not statistically different between 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, $p$=0.33). Instead, in the low-income category spectators in treated villages choose a significantly higher level of inequality compared to those in control (two-sided t-test, $p$=0.01). A regression analysis confirms the results. Models 3 and 4 of Table 2 focus on decisions taken in the Luck treatment and re-estimate the main model specifications of Table 1 by breaking up the sample in income categories. Compared to the baseline category of low-income participants in control villages, among the remaining three categories only low-income spectators in villages where the PFR reform was implemented choose to implement significantly higher levels of inequality. In particular, for low-income spectators in treated villages point estimates indicate an increase of 60%-80% in tolerance for inequality generated by luck compared to low-income spectators in control. Finally, we show in Table B5 in Appendix B that heterogeneity analysis relative to the gender dimension does not find significant differences.

Participants with low levels of tenure security under the customary system who are well integrated in a market economy and with the logistical possibility of relatively easy access to 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 wealthy rightholders already enjoyed a good level of property rights protection under the socially-determined customary system. Therefore, the reform increased the securing of land rights comparatively more for villagers with relative low socio-economic status (Goldstein et al., 2018). Similarly, it is likely 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.

## IV. 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 situational factors. No difference across treatments are instead observed when merit originated the initial unequal distribution. This pattern is driven by low-income households who live in villages characterized by easier access to markets and the formal judiciary, suggesting that those who benefited the most from the reform display the largest change in distributional preferences.

These findings contribute to the understanding of the causes for cross-cultural variation in distributional preferences, suggesting a possible explanation for the puzzling evidence that differences in distributional preferences persist even when it is unambiguously clear that inequalities are determined by factors outside individuals' control (Almås, Cappelen and Tungodden, 2020). The results stem 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.

But why the reform increases participants tolerance for inequality generated by luck? Evidence from previous studies 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 designed to distinguish between possible motivations, 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-alike 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-alike 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 (2020) 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). 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 avoid feeling at odd with previously-shared traditional norms, beneficiaries might feel psychologically compelled to modify their dispositions and opinions toward redistribution. The acceptance of higher inequality determined by luck might reflect the process of self-adaptation and talking oneself into the legitimacy of individual ownership.

Our research suggests that a society's redistributive system is not uniquely a byproduct of the its' members preferences for redistribution. Instead, it confirms that economic institutions responsible for redistribution 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 should be complemented by policies designed to prevent resulting aggravations of social inequalities and their associated problems.

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## 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 from 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 an household who has registered PFR rights[9], and 89% of the sample think that PFR registered rights are 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

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

know somebody who solved a land-related conflict in a state tribunal, compared to the 41% of respondents living closer to paved roads (the difference is strongly statistically significant, two-sided $\chi^2$ test, $p < 1\%$). These proportions roughly match the share of subjects in our sample who actually experienced a conflict and solved the dispute in a formal court (40% of those living closer than the sample median to paved roads versus 16% of those living more distant). The finding is easily understood in light of the costs associated to accessing the formal judiciary for these two categories of respondents. Among the respondents who had first-hand experience of a land-related conflict and who solved it in an formal court, those in the sample more distant from paved roads reported to have born total costs more than three times larger on average compared to those participants living in proximity of paved roads (CFA–thousands 1,233 vs. 382; a two-sided t-test shows that the difference is statistically significant at the 1% level).

## Appendix B: Supplementary Analysis (intended for online publication)

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. Table B6 shows that the difference is not statistically significant in treated and control villages in any of the model specifications.

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


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

*Note:* Vertical bars report SE.

Table B2—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 |

*Note:* Dependent variable: Gini index. Random effects OLS regression. Standard errors robust for clustering 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.

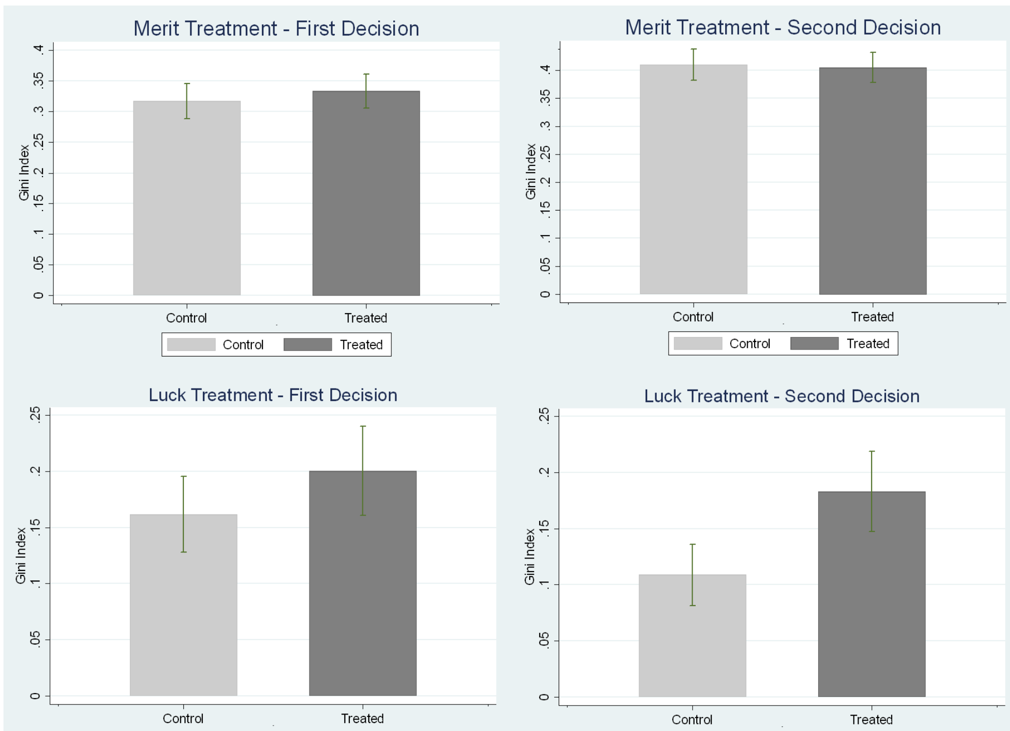


Figure B2. Spectators' Distributional Choices - Separating Order of Decisions

*Note:* Vertical bars report SE.

Table B3—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) |
| PFR-Land Control | 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 |

*Note:* Dependent variable: Gini index. Random effects OLS regression. Standard errors robust for clustering 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. 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 Control includes: 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 - 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) |
| PFR-Land Control | 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 |

*Note:* Dependent variable: Gini index. Random effects OLS regression. Standard errors robust for clustering 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 Control includes: 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 B5—Spectators' Distributional Choices - Heterogeneity Analysis by Gender

| | Model 1 | Model 2 | Model 3 | Model 4 |
|---|---|---|---|---|
| Ctrl*Female*L | -0.051* | -0.050* | -0.052* | -0.048* |
| | (0.029) | (0.029) | (0.029) | (0.028) |
| Trt*Male*L | 0.029 | 0.037 | 0.039 | 0.041 |
| | (0.042) | (0.044) | (0.044) | (0.044) |
| Trt*Female*L | 0.040 | 0.049 | 0.048 | 0.048 |
| | (0.036) | (0.035) | (0.034) | (0.035) |
| Ctrl*Male*M | 0.212*** | 0.212*** | 0.212*** | 0.212*** |
| | (0.026) | (0.026) | (0.026) | (0.026) |
| Ctrl*Female*M | 0.193*** | 0.194*** | 0.192*** | 0.196*** |
| | (0.029) | (0.029) | (0.029) | (0.029) |
| Trt*Male*M | 0.220*** | 0.228*** | 0.229*** | 0.231*** |
| | (0.031) | (0.032) | (0.032) | (0.034) |
| Trt*Female*M | 0.191*** | 0.199*** | 0.198*** | 0.198*** |
| | (0.030) | (0.031) | (0.031) | (0.032) |
| PFR-Land Control | 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. | 1152 | 1152 | 1152 | 1152 |

*Note:* Dependent variable: Gini index. Random effects OLS regression. Standard errors robust for clustering 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 Control includes: 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.

Table B6—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) |
| PFR-Land Control | 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 |

*Note:* Dependent variable: Difference between Gini indexes in Luck and Merit treatment for each individual in the sample. OLS regression. Standard errors robust for clustering 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 Control includes: 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.

# 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
☐ 231
☐ 952
☐ 864
☐ 123
☐ 791

☐ 283
☐ 123
☐ 641
☐ 820
☐ 462
☐ 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.