PHILOSOPHICAL TRANSACTIONS B

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Research

Cite this article: Fabbri M, Nosenzo D, Schulz JF. 2025 Land rights institutions and the scope of cooperation. *Phil. Trans. R. Soc. B* **380**: 20240270.
https://doi.org/10.1098/rstb.2024.0270

Received: 11 February 2025
Accepted: 2 September 2025

One contribution of 16 to a theme issue 'Transforming cultural evolution research and its application to global futures'.

Subject Areas:
behaviour, cognition

Keywords:
lab-in-the-field experiment, land tenure reform, randomized control trial, property rights, parochialism

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Land rights institutions and the scope of cooperation

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Impersonal prosociality—the inclination to trust and cooperate outside one's social circle—varies widely across societies. This study examines whether formalizing land property rights in a society previously governed by informal use rights expands the circle of prosocial behaviours. We hypothesized that formal land property institutions, backed by state enforcement, reduce reliance on local social networks for property protection, potentially broadening social interaction and fostering prosociality toward strangers. To test this hypothesis, we leverage a large-scale land rights formalization programme in Benin, implemented as a randomized control trial. Using lab-in-the-field experiments, vignette studies and attitudinal surveys, we find no evidence that the reform increased the circle of trust, trustworthiness or prosocial norm enforcement toward strangers in other villages. Instead, exploratory analyses suggest that formalized land rights tend to enhance prosociality among co-villagers. While we examine changes in kinship structures as a possible mechanism, results remain inconclusive. These findings suggest that land tenure reforms may reshape social behaviour by reinforcing local ties rather than by broadening prosociality beyond immediate communities.

This article is part of the theme issue 'Transforming cultural evolution research and its application to global futures'.

1. Introduction

Land tenure reforms play a critical role in shaping economic and social outcomes, particularly in rural areas of developing countries. These reforms aim to secure property rights, thereby fostering investment [1–3], enhancing agricultural productivity [4,5] and improving the welfare of rural populations [6–8]. However, the broader socio-behavioural effects of such reforms—specifically, their influence on interpersonal trust, cooperative behaviour and prosocial norms—remain underexplored. This article examines the socio-behavioural consequences of a large-scale land rights reform implemented as a randomized controlled trial (RCT) in Benin. Specifically, it examines whether and how the reform affects trust, trustworthiness and the enforcement of prosocial norms both within and across villages.

In rural Benin, the *Plan Foncier Rural* reform ('PFR' hereafter) was designed to formalize land rights, improve access to land resources and provide state judicial protection to rightholders, particularly women and marginalized groups. Previous studies—both on the Beninese reform and similar interventions elsewhere—have highlighted that such reforms can lead to increased agricultural investments and economic benefits [9–12]. However, there is a growing consensus among scholars and policymakers that understanding these reforms' impact on social norms and cooperative behaviour is equally crucial for assessing their broader effects [13–18].

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We hypothesize that the Beninese PFR (described in §2a) loosens villagers' dependence on the in-group for land protection and thereby fosters impersonal prosociality. State-recognized land titles let owners defend their claims directly in the courts, reducing the need to mobilize extended family, lineage groups or village councils in property disputes. This weakens traditional kin-based institutions that have been linked to a narrower radius of trust [19]. Freed from the costly obligation to maintain dense local networks, landowners can reallocate resources toward building broader economic and social connections. This shift toward non-local partners is expected to drive market integration, expand access to credit and diversify occupational opportunities. Ultimately, we propose that the PFR reform, through these changes in social dynamics, fosters trust and cooperation beyond local networks, aligning with theories on institutional change and evolving social norms [20–23]. We further elaborate on our main hypothesis in §2c.

This study leverages a natural field experiment created by the introduction of the PFR land tenure reform to examine its effects on prosocial behaviour. The backbone of our research design rests on the unique process of implementation of the reform, which was implemented as a large-scale RCT. This design enables us to estimate the causal effects of the reform on prosocial norms and behaviour, by comparing outcomes between villages where the land reform was implemented (treated villages) and villages where the reform was not implemented (control villages).

To gauge villagers' prosocial behaviour, we rely on a lab-in-the-field experimental methodology and implement an incentivized trust game with third-party punishment. This trust game provides a controlled environment to measure trust (the willingness to extend resources to others), trustworthiness (the likelihood of reciprocating trust) and punishment (the willingness to penalize others for breaches of trust). Crucially, we add an in-group and an out-group manipulation; participants either interact with anonymous members of their own village or with anonymous strangers from other villages. This allows us to test whether the reform increases the circle of trust. To complement the experimental data, we also elicit social norms and gather additional attitudinal survey data.

Our analysis reveals that the land tenure reform, contrary to our hypotheses, did not expand the radius of prosociality to include residents of other villages. Trust, trustworthiness and prosocial norm enforcement towards non-village residents is not higher in the treated compared with the control villages. In exploratory, non-preregistered analyses, we observe evidence that prosociality towards co-villagers increases in treated villages compared with control ones. We speculate that this may reflect greater village cohesion, as co-villagers are perceived as less of a threat in land-related conflicts. As part of these dynamics, reliance on kin for property protection may diminish, potentially redirecting the radius of prosociality from kin to co-villagers (while not to the non-village out-group). However, we did not find conclusive evidence to substantiate this interpretation.

In summary, these findings contribute to the question of if and how land tenure reforms influence prosociality. By examining changes in trust, trustworthiness and punishment and by investigating possible mechanisms behind the results, this study sheds light on the broader implications of land reforms for social cohesion and the dynamics of intergroup relations. And while our main pre-registered analysis that focuses on trust towards non-village strangers revealed a null result, our exploratory analysis and other existing research provide evidence that the land tenure reform can affect prosociality. Future work can dive deeper into the role of networks and the radius of trust to reconcile these findings.

The next sections of this article will examine the conceptual framework underpinning our study, describe the methodology and data used, present detailed results and discuss the implications of our findings in the context of policy and future research.

2. Background: land rights and cultural changes

(a) The Beninese *Plan Foncier Rural*

In Sub-Saharan Africa, rural areas predominantly follow customary tenure systems characterized by informal and undocumented use rights. These systems often involve the coexistence of individual–private and collective ownership, as well as local rules that restrict individual rights and uses. The absence of written documentation defining land rights, coupled with overlapping rights over land parcels and the lack of demarcated parcel boundaries, has led to high levels of disputes, undermining tenure security—particularly for minorities and vulnerable groups—and hindering the development of an active land market [14].

Rural villages in Benin faced similar challenges. According to data from the Integrated Modular Survey of Household Living Conditions (EMICoV), in 2006 over 60% of the 996 surveyed rural villages reported at least one ongoing land dispute and more than a quarter of villages experienced five or more conflicts. Despite this prevalence, a 2011 World Bank survey across 290 Beninese villages found that, prior to 2009, only five villages had established an organization specifically dedicated to land issues [24]. Indeed, studies on conflict resolution mechanisms in Beninese rural communities suggest the coexistence of multiple, competing arenas for dispute settlement [24,25]. Litigants typically seek justice from local organizations, which are more accessible and rely on local knowledge in the absence of formal documentation. However, those dissatisfied with local rulings can appeal to regional or state tribunals if they possess documented evidence and can afford court fees.[1] As a result, land rights remained weakly protected for those lacking either connections within local dispute-resolution bodies or the means to access formal courts.

To enhance land access, strengthen tenure security—including for vulnerable rightholders—and, in this way, facilitate the development of a land market, the Beninese government initiated the '*Plan Foncier Rural*' (PFR) in the 1990s. PFR entails a systematic process for identifying and registering customary land rights. This process involves demarcating land plots, creating a land registry that maps all landholdings and conducting socio-land surveys in villages to ascertain rights holders and document their specific rights. These rights may be private or communal, such as rights to occupation without the right to sell, or trustee-based land allocation within lineages. As such, the PFR provided formal recognition of pre-existing rights held by stakeholders

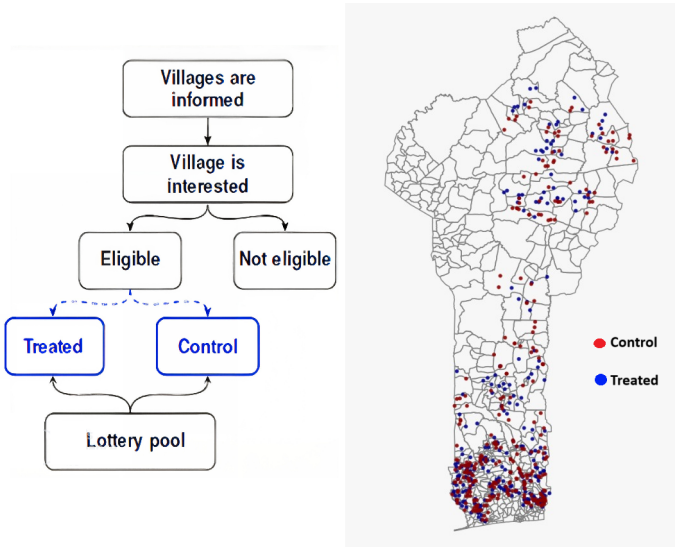

Figure 1. *Left panel*: The lottery mechanism used to select villages where the reform was implemented. *Right panel*: The distribution of treated and control villages in Benin. *Source*: [30].

over land parcels. Public consultations are held to address any disputes over proposed registrations, requiring rights holders and neighbours to publicly validate survey records. These finalized records are then stored in a public repository [26].

Although PFR registration does not immediately confer de jure legal ownership, it provides a presumption of ownership that is recognized by judicial authorities. In the intentions of the reform's promoters, this recognition was meant to facilitate the sale of registered plots or their use as collateral, while possessory right certificates could be converted into formal land titles through a more streamlined and cost-effective process than titling uncertified land.[2] Most importantly, right holders can use PFR registries in formal courts to document their rights over land parcels, thus reshaping the interactions between local actors and dispute resolution organizations under the shadow of the legal protection offered by the central state [28]. This represents a transformative change in property rights institutions, particularly in rural areas where land is the primary asset for most inhabitants [29].

The PFR was not executed until 2006, when the Millennium Challenge Account funded a 5-year implementation programme. The execution, outlined in figure 1, was conducted in partnership with the Beninese government from 2009 to 2011. We provide a timeline of MCA-PFR implementation and details of the various steps in figure 2 in appendix A. Notably, the PFR reform in Benin is the first land tenure reform executed as a large-scale RCT. An initial pool of 576 villages was identified as eligible and willing to implement the reform. The eligibility criteria included specific village characteristics (such as being in rural areas) and, crucially, the village council's formal acceptance of a set of conditions, including the registration of land rights for female members and the organization of informational meetings for land owners whose rights were being registered. From this pool, 300 villages were randomly selected through a public lottery to implement the PFR (the 'treatment' group), while the non-selected villages (the 'control' group) received no intervention and continued to operate under customary land rights.

The World Bank impact evaluation [24], completed 1 year after the intervention, confirms the effectiveness of the randomization process. It reveals balanced observable characteristics between the control and treatment groups and provides evidence that migration patterns are unlikely to be a major factor diluting the RCT [31]. In addition, we report the results of balance tests for socio-demographic and village-level characteristics across treatment branches of the villages in the sample of our experiment in table 4 of appendix B. The results confirm that characteristics are balanced across our sample, alleviating concerns about migrations and other potential events that may have differentially affected treated and control villages between the time of the PFR intervention and our data collection.

Finally, it is worth reporting that an extensive survey conducted in 2019/2020 among residents of PFR villages indicates that stakeholders viewed PFR-registered rights as definitive proof of ownership and that this perception significantly increased the likelihood of resolving disputes through the courts and improved tenure security [25]. Villagers with better access to legal resources reported perceiving registered land as considerably more secure against conflicting claims. They also noted a higher frequency and success rate when relying on formal courts to resolve land-related disputes.

(b) Literature on the interaction between institutions, values and beliefs

Scholars have extensively argued that institutional arrangements significantly impact individual behavioural traits and societal norms [20,21,32–35]. Prominent examples of historical shocks to institutions and downstream effects on economic, political and behavioural outcomes include political upheavals [36,37], wars [38], regulatory changes [39], economic crises [40] and alterations in state borders [41]. Within this extensive literature, a critical debate centres on the consequences of interventions aimed at formalizing land rights—arguably the most crucial assets in rural areas of low-income countries—on the values and beliefs of individuals. Contrasting perspectives have emerged in this discourse.

A substantial body of research posits that well-defined property rights and formalized ownership promote prosocial behaviour, enhance civic engagement and foster the formation of prosocial norms [11,32,42,43]. According to this view, formal land titling

systems can create a more structured and secure environment that nurtures trust and cooperation among individuals, thereby contributing to higher societal welfare.

However, other scholars caution that land tenure reforms might precipitate social imbalances, exacerbate inequalities and dismantle the social safety nets inherent in collective property systems. These changes can lead to social disruptions and a reduction in prosocial behaviour [25,44–47]. The concern is that formalization might prioritize individual ownership at the expense of communal bonds and support systems, potentially eroding the fabric of rural communities. In fact, several contributions suggest that land tenure reforms may foster trust and cooperation only when they align with preexisting social norms and local governance structures [2,48,49]. Conversely, when reforms disrupt traditional arrangements or exacerbate inequality, they can undermine social cohesion and weaken prosocial behaviour [6,50].

In line with this body of evidence, previous studies in Benin suggest that property rights formalization can facilitate the establishment of cooperative networks and prosocial behaviour toward strangers. However, these effects remain contingent on enforcement mechanisms and the equitable distribution of benefits, which can either nurture trust or deepen social divisions [51,52].

This article contributes to the ongoing debate on the effects of land rights institutions on prosocial behaviour in low-income countries by examining a unique case of large-scale land titling reform implemented as a RCT in Benin. Our research design leverages the random allocation of real-world property institutions to Beninese villagers, enabling us to estimate the causal effects of the land tenure reform. This approach eliminates issues of self-selection and biased estimates owing to confounding factors by design.

(c) How formal land rights can enlarge the circle of trust

We hypothesized that land titling fosters impersonal prosociality by altering the social structure in treated villages. Specifically, we conjectured that by reducing households' reliance on kin and co-villagers for land property protection, land titling will expand cooperation beyond the resident village. This hypothesis is consistent with existing work linking lower kinship intensity to greater impersonal prosociality [19,53–55].

As detailed in §2a, the PFR programme allows rightholders to acquire documentary evidence of their land rights that is recognized by state courts.[3] Consequently, registered rights grant individual owners access to state-enforced legal protections, a safeguard previously unavailable in settings governed by customary tenure.

In the absence of formal land titles, neighbours, co-villagers and kin are pivotal in protecting landholders against adverse claims: they may be called upon to testify in disputes, mobilize support in conflicts and often serve on land adjudication committees.

The establishment of state-recognized titles through the PFR reform fundamentally changes incentives. Once landowners can enforce claims in statutory courts, the value of maintaining dense, obligation-laden local ties for protection declines. Informal village institutions that formerly monopolized conflict resolution lose influence, and the returns from networking, reciprocal labour exchange and preferential trading within tight networks diminish.

The ensuing reallocation of social effort impacts social structure and trust. First, households no longer invest as strongly in ceremonial visits, gift exchange or kin-based alliances to defend property; land titles also reduce transaction costs in land markets. Second, the time and resources thus liberated can be channelled toward forging broader economically profitable links between villagers and external traders, credit providers and potential business partners beyond the immediate local community. Consequently, we expect the formalization process to reduce the importance of traditional local institutions, increase reliance on state actors, deepen market integration, enhance access to credit and, through this integration, yield greater occupational diversity in treated villages.

Ultimately, we hypothesize that these mechanisms foster new forms of trust and cooperation that extend beyond the village of residence. Hence, state-backed titling should promote what we call 'out-group prosociality'. This hypothesis aligns with theories proposing that institutional changes reshape social norms and behaviours by altering the contexts in which individuals interact [20,21]. Finally, unlike our predictions regarding prosociality toward outsiders, we advance no directional hypothesis for prosociality toward neighbours and fellow villagers. Fewer local interactions and a more dispersed network could weaken trust among co-villagers, yet well-defined property rights might reduce conflicts and curb local power imbalances. We report exploratory evidence on parochial prosociality in §4.

3. Empirical strategy

(a) Fieldwork activities and procedures

To examine how land-tenure institutions affect prosocial behaviour and norms, and to explore potential mechanisms, particularly the role of people's social networks, we conducted two distinct fieldwork activities applying several tailored data collection tools. We randomly selected a sample of 52 villages among those included in the PFR programme RCT pool (26 'treated' villages randomly selected among those where the reform has been implemented and 26 'control' ones where the reform had not been implemented), and which, according to the 2013 census, had a number of households below the sample median.[4]

The experiment was implemented between March and December 2022. As a preliminary step, we hired a team of geographers who conducted a geo-referenced demarcation of the administrative borders of each village in our sample. This step was necessary because, in Benin, no land registry has geo-referenced village borders.[5] Then, in each village, we conducted two separate fieldwork activities.

In the first fieldwork activity, a team of research assistants completed a full household census in the villages by conducting a survey with one member of each household within the village's borders.[6] The respondent for the survey was required to be an adult household member, though not necessarily the household head or the individual participating in the subsequent experimental games described below. The survey collected socio-demographic data as well as information on values and beliefs using questions that were similar in nature to those used in the World Values Survey. On average, the survey took 25 min to complete and participation was entirely voluntary, without financial incentives. In total, we interviewed 9669 household representatives to approximate the total number of households residing in the 52 villages, in the absence of official statistics.

The second fieldwork activity occurred a few days after completing the full household census and consisted of two parts. In each village, we randomly selected two subsamples from the full household census. The first subsample of 50 households participated both in a network elicitation survey and a series of incentivized experimental tasks (part 1 and part 2 of the second fieldwork activity, respectively). The second subsample of 30 households participated in the network survey only.[7]

Only one individual participated in part 2 of the second fieldwork activity (randomly selected from the two partners, if part 1 was conducted with two people). In part 2, the experimental tasks consisted of a financially incentivized trust game with third-party punishment (our main experimental game), the elicitation of several distinct social norms, a socio-demographic survey and survey and experimental tasks related to different research projects.[8] The main focus of the article is on the behaviour elicited in the trust game and in the social norms elicitation task, which we describe as follows.

Participants in the trust game received an initial endowment of 200 CFA and were assigned one of three possible roles: Trustor, Trustee or Punisher. The Trustor, the first mover, could either keep their endowment, ending the game with a payoff of 200 CFA for both Trustee and Trustor, or transfer the 200 CFA to the Trustee, sextupling the amount to 1200 CFA. In the latter case, the Trustee decided how much of the 1200 CFA to return to the Trustor, choosing from increments of 200 CFA: 0; 200; …; 800. The Trustee's final payoff was determined by the Punisher, who could assign 0 to 4 'punishment points' at no cost to themselves, reducing the Trustee's final payoff by 100 CFA per point assigned.[9] The complete experimental instructions are accessible via appendix D.

The norm elicitation task consisted of several vignettes, of which two were designed for this study as they involve behaviour related to trust and trustworthiness (specifically, dishonesty and collusion in business interactions). The vignettes were read out aloud, and for each scenario, we elicited perceptions of injunctive norms (what 'ought to be done'), descriptive norms (what is commonly done) and norm enforcement (whether dishonest or collusive behaviour is commonly sanctioned).[10]

We introduced a between-subject treatment variation both in the trust game and in the norm elicitation task by informing participants whether the interactions were with co-villagers or strangers from another village/city, creating either an 'in-group' or 'out-group' condition. Specifically, in the trust game, Trustees and Punishers were always from the same village (in-group), but they were matched with a Trustor who was either an anonymous co-villager (in-group) or an anonymous Beninese person from another unknown rural village (out-group). Similarly, in the norms elicitation task, vignettes described the victim of pre-contractual dishonesty or collusive behaviour as either an anonymous co-villager or a stranger from another village or from Benin's main city. Each participant was randomly assigned to either the in-group or out-group condition (between-subject design).

In part 2, as well as in the first fieldwork activity, we also collected data on socio-demographics, kinship structures and the role of family as a safety net, access to credit from various sources, land disputes and conflict-resolution mechanisms used to settle disputes. All decisions, including those in the trust game and the norm elicitation task, were made individually in a private space ensuring that participants' anonymity was guaranteed. In total, 4103 subjects participated in the second fieldwork activity, of whom 2576 household representatives participated in both parts and the remaining only in the initial network elicitation part. Each household earned on average CFA 3500 (approx. EUR 5.3), which is equivalent to the average wage earned in two working days by a Beninese rural household [51].

(b) Hypotheses

We pre-registered experimental procedures, hypotheses and empirical specifications in the Open Science Framework public repository.[11] Our primary objective is to evaluate whether land titling enhances the radius of prosocial values and behaviours.[12] We formulated three behavioural hypotheses:

Hypothesis B1. *In villages that have received land titling ('treated'), trust towards an unknown individual from a different village ('out-group') is higher compared with control villages.*

Hypothesis B2. *In treated villages, trustworthiness towards the out-group is higher compared with control villages.*

Hypothesis B3. *In treated villages, the severity of punishment of a co-villager who defects on an out-group (breach of out-group trust) is stronger compared with control villages.*

The key dependent variables used to test these behavioural hypotheses are derived from the incentivized trust game played in the out-group condition. Specifically, for Hypothesis B1, we use the binary variable indicating whether a Trustor chose to send the fixed amount of 200 CFA to the Trustee, who is an out-group member. For Hypothesis B2, we assess how much a Trustee

Table 1. Behaviour in the trust game (village-level analysis).

	trust		trustworthiness		punishment	
	(1)	(2)	(3)	(4)	(5)	(6)
interaction:	out-group	in-group	out-group	in-group	out-group	in-group
treated (PFR reform) ($n = 26$)	0.64	0.74	474	484	2.77	2.83
s.d.	(0.17)	(0.17)	(48.5)	(56.2)	(0.31)	(0.34)
control ($n = 26$)	0.74	0.64	479	452	2.73	2.78
s.d.	(0.19)	(0.20)	(74.6)	(74.4)	(0.41)	(0.41)
p-value difference	0.96	0.06	0.67	0.09	0.45	0.90

Notes: Outcomes in the trust game with punishment by treatment condition, averaged at the village level. Columns 1 and 2 report the fraction of Trustors who transferred their endowment; columns 3 and 4 display the average CFA amount returned by the Trustees; and columns 5 and 6 display the average number of punishment points assigned by the Punisher when the Trustee returned 0, 200 or 400 CFA (thus, less than half of the amount received) to the out-group Trustor. *p*-values in the out-group columns are based on pre-registered one-sided Wilcoxon rank-sum tests, whereas those in the in-group columns are based on non-pre-registered two-sided Wilcoxon rank-sum tests. All tests are conducted at the village level, with 26 observations per condition.

returns to the Trustor, who is an out-group member. For Hypothesis B3, we aggregate the punishment points administered by the Punisher based on the three outcomes that result in disadvantageous inequality for the out-group Trustor (i.e. the cases where the Trustee sends back 0, 200 or 400 CFA).

In addition to this, we performed further pre-registered analyses with a larger sample, using survey items collected during the first fieldwork activity (i.e. the full household census). Note that the unincentivized survey data on prosociality were collected from potentially different individuals compared with those in the second fieldwork activity and employed different measurement scales. Specifically, while our Trust game measures trust preferences using a binary variable (elicited in a game with punishment), the survey question adapted from the World Value Survey asks, 'In general, how much do you trust an unknown stranger in your country?', with responses ranging from 'Not at all' to 'Completely' on a 4-point scale. Similarly, the trustworthiness and punishment measurements in our unincentivized survey use the same four-point scale.

4. Results

Table 1 presents village-level averages for our key dependent variables from the trust game, separately for each role. The columns labelled 'outgroup' use data from participants who played the trust game in the out-group condition, while the columns labelled 'ingroup' use data from participants who played the trust game in the in-group condition (we will discuss these data at the end of this section).

In line with our pre-registration, our analyses primarily focus on the out-group condition. In villages where the land rights reform was implemented, 64% of Trustors chose to transfer the initial endowment to the Trustee, compared with 74% in control villages (table 1, column 1). This higher transfer rate in the control condition contrasts with Hypothesis B1, which posited that land titling would increase trust towards out-group members. Unsurprisingly, hence, the pre-registered one-sided Wilcoxon rank-sum test, conducted at the village level, fails to reject the null hypothesis that there is no difference in out-group trust between treated and control villages.

We then analyse the average amount of coins sent back by Trustees. Based on a pre-registered, one-sided Wilcoxon rank-sum test conducted at the village-level, we conclude that, contrary to our directional Hypothesis B2, out-group trustworthiness is not significantly different between treated and control villages.

Finally, we examine the average number of punishment points assigned by the Punisher when the Trustee behaved selfishly by returning 0, 200 or 400 CFA (thus, less than half of the initial endowment) to the out-group Trustor. The results of a pre-registered, one-sided Wilcoxon rank-sum test conducted at the village level show that the average number of punishment points is not significantly different between treated and control villages. This finding also contradicts our behavioural Hypothesis B3.

Pre-registered multivariate analysis using individual-level data with controls confirms these findings. Model 1 in table 2 presents the results of a linear probability specification, where the dependent variable equals 1 if the Trustor sent the initial endowment to an anonymous out-group Trustee (column labelled 'out-group') and zero otherwise. Model 3 is an Ordinary Least Squares regression with the amount of coins sent back by the Trustee as the dependent variable. Model 5 employs an OLS regression to analyse the punishment points assigned by the Punisher when the Trustee returns 0, 200 or 400 coins to an out-group Trustor. All models include a dummy variable to estimate the treatment effect of the land tenure reform and socio-demographic controls. Standard errors are clustered at the village level.[13] The regressions confirm the results discussed above. We fail to find evidence of higher out-group trust, higher out-group trustworthiness, or stronger punishment of breach of out-group trust in treated compared with control villages.[14]

These null results are confirmed by analyses of trust, trustworthiness and punishment derived from unincentivized survey questions administered during the first fieldwork activity as part of the full household census with a much larger sample size of approximately 9000 respondents (see table 9 of appendix C). Moreover, we find analogous null results when we examine the effects

Table 2. Behaviour in the trust game—individual-level analysis.

	model 1	model 2	model 3	model 4	model 5	model 6
	trust		trustworthiness		punishment	
	out-group	in-group	out-group	in-group	out-group	in-group
treated (PFR reform)	−0.076	0.065	−7.459	29.713*	0.007	0.044
	(0.044)	(0.046)	(16.109)	(15.259)	(0.087)	(0.077)
controls:						
time-invariant	Y	Y	Y	Y	Y	Y
household-level	Y	Y	Y	Y	Y	Y
village-level	Y	Y	Y	Y	Y	Y
individual (add.)	Y	Y	Y	Y	Y	Y
constant	0.895***	0.751***	459.353***	443.667***	2.911***	2.753***
	(0.203)	(0.244)	(75.568)	(81.341)	(0.306)	(0.360)
N.obs.	427	365	427	443	1158	1293

Notes: Dependent variable: for models 1 and 2, dummy = 1 if the Trustor sends money to the Trustee in the Trust game (binary choice); models 3 and 4, coins returned by the Trustee to the Trustor in the Trust game 0; 200; 400; 600; 800; models 5 and 6 punishment points that a third party assigns to the Trustee in the Trust game 0; 1; 2; 3; 4 for each of the three possible cases in which the Trustee transfers back to the Trustor less than half of the endowment. Models 1–4 are OLS regressions, and models 5 and 6 are Generalized Least Squares regressions. Standard errors robust for clustering at the village level (52 clusters). In models 1–3–5, participants are informed that the Trustor is from a different village from the Trustee and Punisher; in models 2–4–6, all participants are from the same village. Individual time-invariant controls include: age, gender and whether born in the village. Household-level controls include: household size, religion of the household head, ethnicity of the household head, marital status of the household head, and whether the household owns land; village-level controls include whether the village was part of the historical Dahomey kingdom, distance from the closest market, distance from the paved road; additional individual controls include whether the participant is household head, literacy, whether participant knows how to read numbers, education, whether employed in agriculture, whether unemployed, survey experimentally validated measures of risk aversion and time preferences, self-reported measure of market integration, index of access to credit. ***$p < 0.01$; **$p < 0.05$; *$p < 0.01$. p-values of the treatment dummy in models 1–3–5 are based on pre-registered one-sided tests, whereas those in models 2–4–6 are based on non-pre-registered two-sided tests.

of the reform on social norms regulating dishonesty and collusion at the expense of out-groups. Detailed results are presented in appendix B.

(a) Mechanisms

In our pre-registration, we had planned a detailed analysis of mechanisms to explain any observed difference in impersonal prosociality between treated and control villages. Given the null results presented above, these analyses lack meaningful interpretive value. We nevertheless list the pre-registered hypotheses and summarize the main outcomes below, referring the reader to appendix C2 for the pre-registered tests and supplementary analyses.

As elaborated in §2c, we hypothesized several mechanisms that may drive an increase in the radius of prosociality in treated villages compared with control villages. Specifically, we hypothesized that land titling would foster impersonal prosociality through a change in social structure in treated villages, with a decreased reliance on co-villagers and neighbours to protect land property.

Therefore, we pre-registered the hypotheses that, compared with control villages, in treated villages:

M1 … traditional kin-based institutions are relatively less important.
M2 … village cohesion, reliance on traditional institutions (such as the village chief and village elders) and within-village reputational standing are lower.
M3 … institutions and agents of the central government are relatively more important.
M4 … connections between households within a village become more diverse.
M5–M6–M7 … market integration, access to credit and occupational diversity are higher.

The data we use to investigate the mechanisms broadly fall into two categories (please refer to appendix C.2 and to the online pre-registration for a detailed list of these measures):

(i) Survey questions that capture how the relative importance of different actors and institutions (family, lineage, co-villages, etc.) differs between treated and control villages.
(ii) Vignettes to elicit injunctive norms, descriptive norms and expectations regarding punishment of norm violations that are related to obligations towards the family.

Our analysis reveals no differences across any of these dimensions related to social structure between treated and control villages. These results are reported in detail in appendix C. We summarize the following findings:

Table 3. Parochial prosociality in the trust game (Avg. village in-group–out-group measures).

	(1) trust	(2) trustworthiness	(3) punishment
treated (PFR reform) ($n = 26$)	0.10	9.66	0.16
s.d.	(0.20)	(77.12)	(0.88)
control ($n = 26$)	−0.09	−27.48	0.13
s.d.	(0.29)	(88.30)	(1.73)
p-value difference (rank-sum test)	0.02	0.11	0.97

M1—We do not find support for the hypothesis that traditional kin-based institutions play a relatively smaller role in treated villages compared with control villages. Additionally, we find no evidence that lineage is relied upon less as a conflict resolution mechanism in land ownership disputes in treated villages.
M2—We fail to reject the hypothesis that the village community is relatively less important in treated villages compared with control villages. Additionally, we do not find evidence that customary village institutions for conflict resolution are used less frequently in treated villages.
M3—We do not find evidence supporting the hypothesis that institutions and agents of the central government play a relatively larger role in treated villages compared with control villages. Additionally, we find no indication that state courts are used more frequently as a conflict resolution mechanism in land ownership disputes in treated villages.
M4—We fail to reject the hypothesis that participants in treated villages have significantly more outside connections compared with control ones.
M5–M6–M7—We do not find evidence that treated subjects have higher levels of market integration, better access to credit or more occupational diversity.

(b) Exploratory analyses

As a last step, we report the results of a secondary, exploratory analysis of trust-game outcomes in the in-group condition. Table 1 also presents village-level averages of trust, trustworthiness and punishment of trust breaches. It shows that in-group trust and trustworthiness are weakly and significantly higher in treated villages compared with control villages. However, there are no significant differences in punishment behaviour. Models 2, 4 and 6 in table 2 display the coefficients from individual-level regressions, mirroring the analyses conducted for the out-group condition (Models 1, 3 and 5) but applied to participants in the in-group condition. The point estimates for in-group trust and trustworthiness among treated participants are positive, with the latter being marginally significant (p-value = 0.057). The coefficient for in-group punishment is not statistically different from zero.

These results provide tentative evidence that the land-rights reform may have increased prosociality toward co-villagers. Furthermore, while treated villages, contrary to our pre-registered hypothesis, do not show an increase in out-group trust compared with control villages, exploratory analysis even points in the opposite direction: trust toward the out-group is weakly significantly lower in treated relative to control villages ($p = 0.07$, two-sided Wilcoxon rank sum), although in multivariate stack specifications this result is less robust as it is only significant when we include covariates in the regression; see appendix C, table 8).

To probe the overall effect of land-titling on parochialism, we construct measures of village-level 'parochial prosociality' by computing the differences between in-group and out-group trust, trustworthiness and punishment, averaged at the village level. A positive gap between in-group and out-group measures indicates village-level parochialism. Table 3 summarizes the village-level averages of these measures. The data reveal that the level of parochial trust in villages where the reform was implemented is significantly greater than in control villages, with p-values of 0.02. Participants in our sample also display a higher parochial trustworthiness in treated compared with control villages, albeit this difference is not statistically significant (p-value = 0.11). In contrast, the level of parochialism in punishment is virtually identical in treated and control villages, with a p-value of 0.97.

In summary, this exploratory analysis provides some evidence that the land-rights reform increased village-level parochialism. However, this analysis was not pre-registered. Considering that our pre-specified analysis and all proposed mechanisms show null effects, we view this result as suggestive rather than conclusive, potentially reflecting a false positive. Still, even when applying a conservative Bonferroni correction for multiple hypothesis testing—given that we simultaneously test for differences in trust, trustworthiness and punishment—the difference in parochial trust between treatment conditions remains marginally statistically significant (Bonferroni-adjusted p-value = 0.06).

5. Conclusion

This article investigates the impact of a land rights reform in Beninese villages, which involved the formal registration of customary land rights, on the extent of prosocial norms and behaviour. Building on a RCT, lab-in-the-field experiments, norm elicitations and surveys, our research design aims to rigorously identify whether formalizing land tenure expands prosociality beyond the

local community. Contrary to our initial hypotheses, we do not find evidence that land rights formalization increases trust, trustworthiness or prosocial norm enforcement toward strangers from other Beninese villages. However, our exploratory analyses provide some suggestive evidence that the reform has strengthened trust and trustworthiness among co-villagers.

These findings indicate that formal property rights do not necessarily foster broad, impersonal prosociality that extends to strangers in other villages. Instead, they suggest a reconfiguration of social interactions within treated villages. Several factors may have contributed to this result. One possibility is that the increase in prosociality among co-villagers could reflect a shift in the boundaries of trust at the kinship level: while individuals may have previously relied primarily on kin relationships to secure property in the absence of formal land tenure rights, the reform may have extended this trust to co-villagers, even if it did not reach outsiders. However, an examination of this mechanism does not reveal evidence of changes in the self-reported importance of kin ties. Another closely related possibility is that, before land formalization, co-villagers were seen as the primary competitors for land. By reducing this competition, the reform may have fostered prosociality within villages while leaving interactions with outsiders unaffected.

Temporal dynamics may also play a role. Our study, conducted more than 13 years after the initial implementation of the RCT, captures only a single point in time, leaving open the question of how land tenure reform may shape social behaviour in the even longer run. Broader market integration—through reduced barriers to land trade and the use of land as collateral—could need more time to extend prosocial behaviour beyond the local community. Alternatively, it is possible that, after 13 years, the distinction between treatment and control conditions has blurred, making differences harder to detect.

Our findings contribute to the discourse on the broader social implications of policy interventions. Given the central role of land in rural Sub-Saharan Africa—both as a critical asset and a status symbol—changes in property rights structures are likely to induce social transformations that extend beyond economic considerations. Our study provides causal evidence on the effects of land rights reforms on values and behaviour, an endeavour that is often challenging in such contexts. It underscores the importance of examining effects across social categories and assessing the scope of trust by considering its reach at different social levels. Future research should further investigate how these effects evolve over time, particularly in relation to market integration and broader institutional changes.

Ethics. The project underwent ethical review and was approved by the Aarhus University Research Ethics Committee (BSS-2022-011). Authorization by the Beninese institutions to conduct the research was granted under VISA STATISTIQUE nN 01/2022/MEF/INStaD/DCSFR.

Data accessibility. Replication package is available here: [59].

Declaration of AI use. Language check and editing coding.

Authors' contributions. M.F.: conceptualization, data curation, formal analysis, funding acquisition, investigation, methodology, project administration, resources, software, validation, visualization, writing—original draft, writing—review and editing; D.N.: conceptualization, data curation, formal analysis, funding acquisition, investigation, methodology, project administration, resources, software, supervision, validation, visualization, writing—review and editing; J.F.S.: conceptualization, funding acquisition, investigation, methodology, resources, software, supervision, validation, visualization, writing—review and editing.

All authors gave final approval for publication and agreed to be held accountable for the work performed therein.

Conflict of interest declaration. We declare we have no competing interests.

Funding. This project was made possible through the support of a grant from the Cultural Evolution Society Transformation Fund; underwritten by the John Templeton Foundation, Grant 61913. The opinions expressed in this publication are those of the authors and do not necessarily reflect the views of the John Templeton Foundation. Jonathan Schulz acknowledges funds received from Emergent Ventures. Daniele Nosenzo acknowledges funds received from the Aarhus University Research Foundation (AUFF Starting Grant 36835) and the Carlsberg Foundation (CF21-0280).

Endnotes

[1]For instance, among the 91 Beninese villages surveyed in 2011 that did not implement the land tenure reform described in the next section, 81% attempted to resolve conflicts within families, 63% engaged village-level institutions and 50% sought mediation from local notables. At the same time, 44% also pursued cases in regional or state courts [24]. Similarly, Arruñada *et al*. [25] report that state courts are 4–10 times more expensive than local courts.

[2]The reform's implementation encountered various challenges, including constraints on resource and time, the complexity of identifying existing customary rights and inconsistencies arising from legal ambiguities. These challenges occasionally led to incomplete surveying of land parcels within some villages. Additionally, the 2013 Land Code (https://actualites.visages-du-benin.com/wp-content/uploads/2013/02/DSL-LOI-PORTANT-CODE-FONCIER-I1.pdf), while reaffirming the legal validity of PFR-registered rights, halted the automatic issuance of PFR certificates. This change placed greater emphasis on the physical demarcation of parcels and the clarification of rights rather than on the conferring of transferable property rights [27].

[3]Although the Beninese PFR formalized and registered both individual and collective rights over agricultural land, more than 85% of agricultural parcels in the villages belong to individual owners.

[4]We selected villages as follows: first, we restricted the sample to villages with a population smaller than the median of 333 households (according to the Beninese Census 2014). This restriction to small villages made our full-census data collection (described below) feasible. We then randomly chose a treated-control balanced sample of 120 villages stratified by geographical region. Geographic stratification was driven partly by another project on the role of the Dahomey kingdom in women's agency. From the sample of 120 villages, we excluded 15 villages owing to administrative reforms changing village boundaries (2 villages), unclear historical borders (12 villages) and rural reforms by other organizations (1 village). The remaining pool of 105 villages constituted the potential sample. Before fieldwork began, a terrorist attack attributed to a jihadist group in the Alibori province (northern Benin) prompted authorities to recommend avoiding areas near the Niger border. We therefore excluded three additional villages from the sample for security reasons. From this set, we randomly selected 24 villages within the historical Dahomey border, 12 villages just outside the Dahomey border and 24 villages from other regions of the country. Within each category, we ensured a balance between treated and control villages. This gave us a sample of 60 villages that was our initial target for data collection. Resource limitations ultimately allowed for data collection in 52 villages. In one of the 52 selected villages, the preliminary survey team was informed upon arrival by the village representative of recent tensions between the Beninese and Nigerian police regarding the exact location of the village border. This border, which is poorly demarcated, also serves as the international boundary between the two countries. In particular, some neighbourhoods were being contested by both states. Given that our survey procedure required geo-localizing the village borders, interviewing all households

within the village—including those in the disputed neighbourhoods—and asking control questions about respondents' village of residency, we anticipated that proceeding could have further exacerbated the tensions. We therefore decided not to conduct data collection in this village and replaced it with the first non-selected village from our original list. In all other cases, the villages were cooperative during data collection. Only once was fieldwork temporarily suspended, when a village chief delayed activities for a few hours to verify our authorizations with the local town hall. Once the documents were confirmed, data collection resumed without further interruption.

[5]This ensured that our fieldwork activities, conducted in participants' houses, could verify that individuals taking part in our study were indeed residents of the selected villages.

[6]Government census data for the Beninese rural villages were outdated. Therefore, our team identified households residing within the village borders through preparatory consultations with village leaders and local administrators, door-to-door interviews and satellite image checks.

[7]Analysis of this network survey will be predominantly used in a second article. The survey required the presence of the household head and, for households where the head was married or cohabiting, at least one of their partners. Participants were surveyed about the social networks of the household. For example, respondents were asked to list all village-resident or out-of-village households with whom they exchange gifts and visits, seek loans or request help with financial decisions and children's education. The complete list of questions is reported in Appendix D. Households received a flat participation fee of CFA 1000 (approx. EUR 1.5) for their participation.

[8]The experimental tasks used for different projects measured competitiveness [56] and intra-household bargaining power [57]; the experimentally validated survey focused on female agency and empowerment [58]. See https://osf.io/5bgvt/, https://osf.io/ntq83/, https://osf.io/5dqc6/ for the pre-registrations related to these other research projects. These projects use the same identification strategy described in the present paper, but focus on different outcome variables.

[9]Trustee and Punisher choices were elicited via the strategy method: each player specified their response to every possible action by the other, knowing that only the choice corresponding to the actual play would affect payoffs. For example, Trustees indicated in advance how much they would return if the Trustor transferred 200 CFA, even though this return decision only mattered for payoffs if the Trustor indeed made that transfer. Consequently, each Trustee made one return decision even if the Trustor ultimately had chosen not to transfer.

[10]We provide a comprehensive description of the norm elicitation task in appendix B.

[11]Available at https://osf.io/h2wrz.

[12]In addition to the hypotheses focusing on individual behaviour and values, our pre-analysis plan also includes hypotheses related to perceived injunctive and descriptive norms and expectations regarding norm enforcement. For brevity, we discuss these hypotheses in detail in appendix B.

[13]The regressions account for individual, household and village characteristics detailed in the notes of table 2. We also ran versions of models 1–2 using Probit regressions and versions of models 3–4 using Ordinal Probit regressions, with qualitatively similar results (available upon request).

[14]We conducted power analyses to assess whether these null results might reflect low statistical power. Our findings indicate that, while our village-level analyses, based on 26 observations per condition, are only powered to detect effect sizes of approx. Cohen's $d = 0.71$ or larger (with $\alpha = 0.05$; $(1 - \beta) = 0.8$), the individual-level analyses are much better powered, with minimum detectable effect sizes—given our sample size—in the range of Cohen's $d = 0.29$ or larger. These effects correspond to average increases of +11 percentage points in the share of Trustors who trust in treated villages (relative to an average share of 74% in control villages), +55 CFA returned by Trustees (relative to an average of 475 CFA returned in control villages) and +0.26 punishment points assigned by Punishers (relative to an average of 2.73 points assigned in control villages). For a comparison with the existing literature, as summarized in table 15 in appendix C.3, we report the Cohen's effect sizes from six prior experimental studies based on independent fieldwork in Benin studying the impact of the land titling on a number of behavioural outcomes. The average reported Cohen's d across these studies is 0.41. This suggests that the minimum detectable effect sizes in our study are within the range of substantively meaningful and previously observed effects, and that our study is sufficiently powered to detect small to moderate effects.

[15]In the pre-registered analysis, we also mentioned that we will test whether a larger fraction of the number of connections for other households in control villages are to relatives on the father's side compared with the treated villages. We were actually unable to collect this information during the network elicitation.

Appendix A. Timeline of the PFR project and balance of observables

The implementation of the *Plan Foncier Rural* (PFR) in Benin, financed under the Millennium Challenge Account (MCA-Benin I), took place between 2006 and 2011 with the intention to improve land tenure security in rural areas. The programme was launched in 2006 following the signing of the compact between the Government of Benin and the Millennium Challenge Corporation (MCC), which included land access as a core component. In 2007, pilot communes were selected, and extensive community sensitization and institutional preparations began to support participatory land mapping. By 2008, physical demarcation of land parcels was underway, using a participatory approach involving local landowners and land management committees.

The year 2009 marked the collection of land data and validation of village maps, with significant community engagement to ensure accuracy and legitimacy. In 2010, certified rural land titles (Certificats Fonciers Ruraux, CFRs) were issued to landholders, alongside efforts to train local institutions and formalize legal frameworks. The compact concluded in 2011, with a final evaluation confirming the project's impact and offering recommendations for scaling up the PFR as part of national land governance reform. These results were assessed using multiple data sources, including the nationally representative EMICoV surveys from 2006 (https://instad.bj/images/docs/insae-statistiques/enquetes-recensements/EMICoV/2006-2007/Rapport-preliminaire-emicov-2006.pdf) and 2010 (https://instad.bj/images/docs/insae-statistiques/enquetes-recensements/EMICoV/2010/Rapport-preliminaire-emicov%202010.pdf), as well as a dedicated impact evaluation survey conducted by the World Bank in partnership with the Institut de Recherche Empirique en Économie Politique (IREEP) [24]. Figure 2 summarizes this timeline (table 4).

Appendix B. Pre-registered hypotheses and analysis concerning impersonal prosocial norms

In addition to the hypotheses that focus on individual behaviour and attitudes, in the pre-analysis plan, we also registered hypotheses about perceived injunctive and descriptive norms and expectations on norm enforcement. In the norm elicitation task, participants were exposed to a series of vignettes to elicit injunctive and descriptive norms as well as norm enforcement, including two vignettes about pre-contractual dishonesty and collusion, which we used to test hypotheses about impersonal prosociality.

In these two vignettes, participants were randomly allocated to either an 'in-group' treatment or an 'out-group' treatment. In the in-group treatment, the interactions described in the vignettes were between fictitious people from the same village, and thus

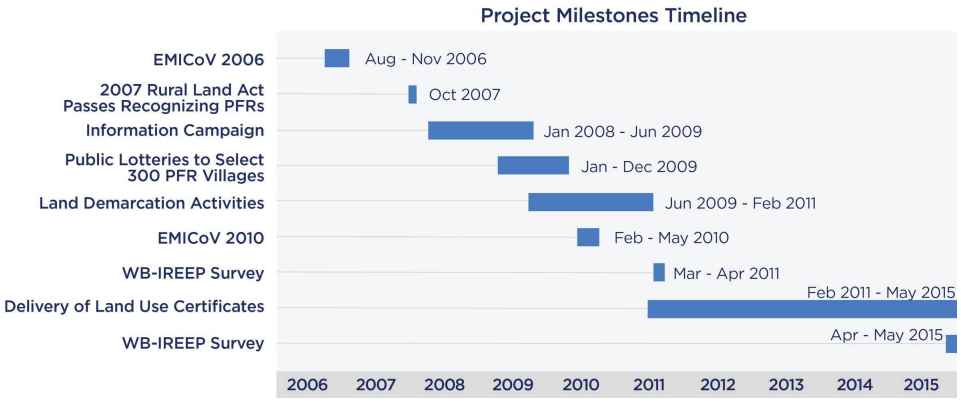

Figure 2. Timeline of the implementation of the *Plan Foncier Rural* (PFR) in Benin.

Table 4. Balance of observables across treatment and control villages (variables are averaged at village level, T-test two-sided for continuous variable and Chi-square test for dummy variables (d)).

	treated (PFR reform) ($n = 26$)	control ($n = 26$)	difference (p-value)
male (d)	0.49	0.50	0.84
age	41.8	42.4	0.55
household head (%)	0.65	0.65	0.76
read numbers (%)	0.36	0.37	0.90
born in village (d)	0.69	0.71	0.50
literate (d)	0.33	0.33	0.87
education levels completed	0.61	0.64	0.73
nr. household members	3.83	3.75	0.92
employed agriculture (d)	0.29	0.27	0.63
unemployed (d)	0.08	0.09	0.98
household owns land (d)	0.74	0.74	0.90
market distance	8.15	6.42	0.24
road distance	8.84	10.73	0.56
village former Dahomey Kingdom (d)	0.46	0.46	1.00
ethnic group 1 (%)	0.48	0.47	0.92
ethnic group 2 (%)	0.12	0.13	0.91
ethnic group 3 (%)	0.02	0.05	0.39
ethnic group 4 (%)	0.07	0.08	0.90
ethnic group 5 (%)	0.18	0.09	0.28
other ethnic groups (%)	0.11	0.17	0.27
religion 1 (%)	0.42	0.49	0.27
religion 2 (%)	0.23	0.17	0.52
religion 3 (%)	0.29	0.30	0.90
other/multiple religions (%)	0.04	0.03	0.12

the victim of dishonesty and collusion described in the vignettes was a fellow villager. In the 'out-group' treatment, the interactions were between a fictitious co-villager and a fictitious person from another village, and the victim was an unknown stranger from a different village or from the city.

The English translation of the text used in the two vignettes was the following (text that varies between treatments in brackets):

Vignette 1—Pre-contractual dishonesty: *A man in your village sells some wood for construction to another man who lives [in the same village/in a different village without ties to your village]. The seller knows that the wood was damaged by water and that the quality is very low, even if it is not possible to see any differences from outside relative to good quality wood. The seller does not reveal the true quality of the*

wood to the [other villager/stranger] who buys it, and the [other villager/stranger] therefore purchases the wood at a high price believing that it is of good quality. After having started the construction work, the [other villager/stranger], however, realizes that the wood is low-quality material.

Vignette 2—In-group collusion and honesty in land sales: *A man in your village owns a parcel of land and sells it to [another person who lives in the city without ties to your village/another village member]. After the payment has been made, the man tries to trick [the person from the city/the other villager]: he asks one of his neighbours to falsely claim that the neighbour is the actual legal owner of the land, and so the man could not have sold the land and [the person from the city/the other villager] cannot keep it. The neighbour accepts the man's request and makes the false claim about the land's ownership.*

After each vignette, participants were asked two questions. The first question, posed to all participants, elicited their perception of the injunctive norm for the behaviour described in the vignette. Following Krupka and Weber's [60] methodology for injunctive norm elicitation, participants indicated whether the behaviour was 'highly socially appropriate', 'somehow socially appropriate', 'somehow socially inappropriate' or 'highly socially inappropriate'. Importantly, participants were incentivized to estimate the modal rating that their fellow villagers would assign to the vignette. Correctly guessing this most frequent rating earned the participant CFA 1000, while incorrect guesses earned nothing. This methodology elicits each participant's perception of the prevailing social norm in their own village.

The second question differed across participants. Half of the participants were randomly assigned to a question eliciting their perception of the descriptive norm. They were asked: 'Out of ten individuals, how many among your co-villagers do you think would behave this way in this situation?'. The other half of participants were assigned to a question measuring their perceptions of social norm enforcement. They were asked: 'How many among your co-villagers do you think would punish a co-villager who behaves this way?'. The response scale in both questions ranged from 0 to 10, in increments of 1. These questions were elicited without providing financial incentives.

We had the following directional hypotheses:

Hypothesis N1. *In treated villages, perceived injunctive norms against dishonesty and in-group collusion at the expense of an unknown stranger (out-group) are stronger compared with control villages.*

Hypothesis N2. *In treated villages, perceived descriptive norms against dishonesty and in-group collusion at the expense of an unknown stranger are stronger compared with control villages.*

Hypothesis N3. *In treated villages, punishment of a co-villager who defects against an unknown person from a different village is stronger compared with control villages.*

To formally analyse treatment differences, we follow our pre-analysis plan and construct three variables, averaging the responses across the two vignettes. We test hypotheses N1–N3 via non-parametric tests at the village level, and we conduct individual-level regression analysis that controls for covariates to ensure robustness.

Starting with non-parametric tests, our one-tailed rank-sum tests fail to reject the null hypotheses that injunctive, descriptive and enforcement norms are different between treated and control villages at the conventional level of significance. This result is further confirmed by the pre-registered regression analysis reported in table 5.

Appendix C. Supplementary analyses

C.1. Replication of the main analysis excluding controls, using alternative specifications and using survey data

Tables 6–9.

C.2. Analyses of mechanisms

In what follows, we report the test results of the pre-registered hypotheses—listed and explained in §4—following the construction of variables and test statistics that we have specified in the pre-registration. We refer to the preregistration for additional details regarding how the variables were constructed.

C.2.1. Social structure

C.2.1.1. Capturing relative importance of family, village community and government

As a first step, we created a survey item intended to capture the importance of family (M1); co-villagers and family level institutions (M2); and the national government (M3) in protecting land. This item was administered in the first fieldwork activity. Based on participants' responses to the pre-specified battery of survey questions, we create three indicators to test M1 to M3.

To test hypothesis M1, the first step of the pre-registered analysis requires the comparison of the first indicator—a family importance index—between treated and control villages using non-parametric one-sided tests. Following the result of a one-tailed rank-sum test, we fail to reject the hypothesis that in treated villages traditional kin-based institutions are relatively less important

Table 5. Norms for pre-contractual dishonesty and in-group collusion at the expense of an unknown stranger.

	model 1 injunctive	model 2 norm	model 3 descriptive	model 4 norm	model 5 punishment	model 6 norm
treated (PFR reform)	−0.001	0.007	−0.187	−0.228	−0.034	−0.018
	(0.031)	(0.032)	(0.463)	(0.430)	(0.367)	(0.334)
controls						
time-invariant	N	Y	N	Y	N	Y
household-level	N	Y	N	Y	N	Y
village-level	N	Y	N	Y	N	Y
additional individual	N	Y	N	Y	N	Y
constant	2.185***	2.296***	6.570***	5.766***	15.734***	15.504***
	(0.025)	(0.073)	(0.357)	(1.410)	(0.244)	(1.069)
N.obs.	1262	1241	630	620	632	621

Notes: Dependent variable: we built three indexes averaging participants' norm elicitations across two vignettes: one concerning pre-contractual dishonesty and one concerning in-group collusion in land sale at the expense of an unknown stranger. The indexes used as dependent variables are: for models 1 and 2, injunctive norm index; models 3 and 4, descriptive norm index; models 5 and 6, norm enforcement index. All models are OLS regressions. Robust standard errors are clustered at the village level (52 clusters). Individual time-invariant controls include: age, gender and whether the respondent is born in the village. Household-level controls include household size, religion of the household head, ethnicity of the household head, marital status of the household head and whether the household owns land; village-level controls include whether the village was part of the historical Dahomey kingdom, distance from the closest market, distance from the paved road; additional individual controls include whether the participant is household head, literacy, whether the participant knows how to read numbers, education, whether employed in agriculture, whether unemployed, survey experimentally validated measures of risk aversion and time preferences, self-reported measure of market integration, index of access to credit. ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. p-values of the treatment dummy are based on pre-registered one-sided tests.

Table 6. Behaviour in the trust game: excluding all controls.

treatment:	model 1 trust		model 2 trustworthiness		model 5 punishment	model 6
	outgroup	ingroup	outgroup	ingroup	outgroup	ingroup
treated (PFR reform)	−0.072	0.075	−0.325	27.470	0.064	0.057
	(0.048)	(0.048)	(15.750)	(17.089)	(0.097)	(0.088)
controls	N	N	N	N	N	N
constant	0.737***	0.654***	475.439***	457.851***	2.729***	2.784***
	(0.034)	(0.036)	(13.295)	(13.049)	(0.080)	(0.066)
N.obs.	427	365	427	443	1158	1293

Notes: Dependent variable: for models 1 and 2, dummy = 1 if the Trustor sends money to the Trustee in the Trust game (binary choice); models 3 and 4, coins returned by the Trustee to the Trustor in the Trust game 0; 200; 400; 600; 800; models 5 and 6, punishment points that a third party assigns to the Trustee in the Trust game 0; 1; 2; 3; 4 for each of the three possible cases in which the Trustee transfers back to the Trustor less than half of the endowment. Models 1–4 are OLS regressions, models 5 and 6 are GLS regressions. Standard errors robust for clustering at the village level (52 clusters). In models 1–3–5, participants are informed that the Trustor is from a different village from the Trustee and Punisher; in models 2–4–6, all participants are from the same village. ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. p-values of the treatment dummy in models 1–3–5 are based on pre-registered one-sided tests, whereas those in models 2–4–6 are based on two-sided tests.

compared with the control villages. Models 1 and 2 in table 10 report the results of the pre-registered regressions investigating family importance, confirming the finding.

We repeat the steps just described for both hypotheses M2 and M3 that refer to the relative importance of the village community and the state relative importance, respectively. In both cases, the non-parametric analysis reveals no significant differences. The regression analyses reported in models 3–6 of table 10 confirm the result.

Finally, we hypothesize that in treated villages (i) the importance of both sides of the family becomes more equal (based on the absolute difference) and (ii) the importance of the mother's side increases (based on the difference). To test this hypothesis, we subtract the self-reported importance attributed to the father's side of the family from that attributed to the mother's side, which reveals the differential importance placed on the father's in relation to the mother's side. In both cases, the pre-registered tests fail to reject the hypotheses (the results of the regression analysis are available upon request).

Table 7. Behaviour in the trust game: excluding time-varying controls

	model 1	model 2	model 3	model 4	model 5	model 6
	trust		trustworthiness		punishment	
	outgroup	ingroup	outgroup	ingroup	outgroup	ingroup
treated (PFR reform)	−0.068	0.078*	−1.427	31.243**	0.004	0.041
	(0.050)	(0.046)	(14.466)	(14.723)	(0.080)	(0.077)
time-invariant						
controls	Y	Y	Y	Y	Y	Y
constant	0.721***	0.738***	579.954***	492.220***	2.935***	2.836***
	(0.089)	(0.090)	(31.456)	(30.551)	(0.148)	(0.165)
N.obs.	427	365	427	443	1158	1293

Notes: Dependent variable: for models 1 and 2, dummy = 1 if the Trustor sends money to the Trustee in the Trust game (binary choice); models 3 and 4, coins returned by the Trustee to the Trustor in the Trust game 0; 200; 400; 600; 800; models 5 and 6, punishment points that a third party assigns to the Trustee in the Trust game 0; 1; 2; 3; 4 for each of the three possible cases in which the Trustee transfers back to the Trustor less than half of the endowment. Models 1–4 are OLS regressions, and models 5 and 6 are GLS regressions. Standard errors robust for clustering at the village level (52 clusters). In models 1–3–5, participants are informed that the Trustor is from a different village from the Trustee and Punisher; in models 2–4–6, all participants are from the same village. Time-invariant controls include: age, gender, whether born in the village, religion of the household head, ethnicity of the household head, whether the village was part of the historical Dahomey kingdom, distance from the closest market, distance from the paved road. ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. p-values of the treatment dummy in models 1–3–5 are based on pre-registered one-sided tests, whereas those in models 2–4–6 are based on two-sided tests.

Table 8. Behaviour in the trust game—stacked specification.

	model 1	model 2	model 3	model 4	model 5	model 6
	trust		trustworthiness		punishment	
treated (PFR reform)	−0.072	−0.092	−0.325	−7.244	0.064	0.044
	(0.048)	(0.043)	(15.758)	(16.397)	(0.097)	(0.084)
ingroup	−0.083	−0.071	−17.587	−25.189	0.056	0.040
	(0.050)	(0.045)	(15.601)	(16.542)	(0.104)	(0.100)
treated × ingroup	0.147**	0.158***	27.795	41.832*	−0.007	−0.011
	(0.062)	(0.056)	(21.526)	(23.081)	(0.116)	(0.107)
controls						
time-invariant	N	Y	N	Y	N	Y
household-level	N	Y	N	Y	N	Y
village-level	N	Y	N	Y	N	Y
individual (add.)	N	Y	N	Y	N	Y
constant	0.737***	0.955***	475.439***	473.060***	2.729***	2.708***
	(0.034)	(0.154)	(13.302)	(55.067)	(0.080)	(0.265)
N.obs.	814	792	909	870	2529	2451

Notes: Dependent variable: for models 1 and 2, dummy = 1 if the Trustor sends money to the Trustee in the Trust game (binary choice); models 3 and 4, coins returned by the Trustee to the Trustor in the Trust game 0; 200; 400; 600; 800; models 5 and 6, punishment points that a third party assigns to the Trustee in the Trust game 0; 1; 2; 3; 4 for each of the three possible cases in which the Trustee transfers back to the Trustor less than half of the endowment. Models 1–4 are OLS regressions, and models 5 and 6 are GLS regressions. Standard errors robust for clustering at the village level (52 clusters). Individual time-invariant controls include age, gender and whether born in the village. Household-level controls include household size, religion of the household head, ethnicity of the household head, marital status of the household head and whether the household owns land; village-level controls include whether the village was part of the historical Dahomey kingdom, distance from the closest market, distance from the paved road; additional individual controls include whether the participant is household head, literacy, whether the participant knows how to read numbers, education, whether employed in agriculture, whether unemployed, survey experimentally validated measures of risk aversion and time preferences, self-reported measure of market integration, index of access to credit. ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. p-values of the treatment dummy are based on pre-registered one-sided tests.

Table 9. Prosocial behaviour in the trust game: survey data.

	model 1	model 2	model 3	model 4	model 5	model 6
	trust		trustworthiness		punishment	
	out	in	out	in	out	in
treated (PFR reform)	0.064	0.045	0.048	0.091	0.010	0.009
	(0.066)	(0.080)	(0.064)	(0.066)	(0.058)	(0.054)
controls:	Y	Y	Y	Y	Y	Y
cut1	−0.862***	−1.020***	−0.942***	−0.940***	−1.626***	−1.604***
	(0.186)	(0.260)	(0.195)	(0.214)	(0.148)	(0.160)
cut2	−0.132	−0.346	−0.195	−0.253	−1.091***	−1.052***
	(0.181)	(0.253)	(0.187)	(0.199)	(0.152)	(0.164)
cut3	0.620***	0.369	0.578***	0.516***	−0.556***	−0.464***
	(0.184)	(0.254)	(0.191)	(0.194)	(0.158)	(0.175)
N.obs.	9506	9506	9507	9506	9506	9508

Notes: Dependent variable: Models 1 and 2, reply to the question about trusting others outside (model 1) or inside (model 2) the village; four options, 0 = not at all, 3 = fully. Models 3 and 4 reply to question about reciprocating trust outside (model 3) or inside (model 4) the village; four options, 0 = not at all, 3 = fully. Models 5 and 6 reply to the question about enforcing punishment of anti-social behaviour outside (model 5) or inside (model 6) the village; four options, 0 = not at all, 3 = fully. All models report results of Ordinary Probit regressions. Individual time-invariant controls include age, gender and whether born in the village. Controls include data collection wave, whether the village is inside the former Dahomey kingdom, religion of the household head, ethnicity of the household head, whether the respondent is the household head, age and language. ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. p-values of the treatment dummy in models 1–3–5 are based on pre-registered one-sided tests, whereas those in models 2–4–6 are based on two-sided tests.

Table 10. Relative importance of family, village community and government.

	model 1	model 2	model 3	model 4	model 5	model 6
	family		village community		state	
treated (PFR reform)	0.062	−0.005	0.072	0.009	0.083	−0.019
	(0.086)	(0.061)	(0.064)	(0.039)	(0.104)	(0.047)
controls	N	Y	N	Y	N	Y
constant	−0.031	−0.120	−0.036	−0.700**	−0.042	−0.083
	(0.067)	(0.337)	(0.043)	(0.278)	(0.077)	(0.275)
N.obs.	7063	7051	7054	7042	7064	7054

Notes: Dependent variable: for models 1 and 2, index for importance of the family; models 3 and 4, index for the importance of the village community; models 5 and 6, index for the importance of the state. All models are OLS regressions. Standard errors robust for clustering at the village level (52 clusters). Controls include age, gender, whether born in the village, data collection wave, religion of the household head, ethnicity of the household head, whether the respondent is household head, language, distance of the village from paved road, distance of the village from the closest market, whether the village is inside former Dahomey kingdom. ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. p-values of the treatment dummy are based on pre-registered one-sided tests.

C.2.1.2. Conflict resolution mechanism

As part of the main survey, we asked respondents to which institution they would bring a land ownership dispute if one arose (State court; Customary law court; Village elders; Lineage; Other). We asked two versions of this question, the only difference being that one scenario described a dispute between two co-villagers and the other between a villager and someone from another village.

Consistent with hypotheses M1, M2 and M3, we hypothesized that in treated villages, kin and customary institutions were comparatively less important compared with control villages.

A one-tailed rank-sum test fails to provide evidence that the pre-registered indicator of lineage importance is less important in treated villages. The same test fails to provide evidence that customary authorities are more important in control villages than in treated ones as conflict resolution mechanisms and that state courts are more important in treated villages compared with control ones. Regression analyses reported in table 11 confirm the results. The analyses replicate the model specifications described in table 10, but this time focusing on the dependent variables that capture the importance of lineage/extended families (models 1–2), of customary village authorities (models 3–4) and of state courts (models 5–6).

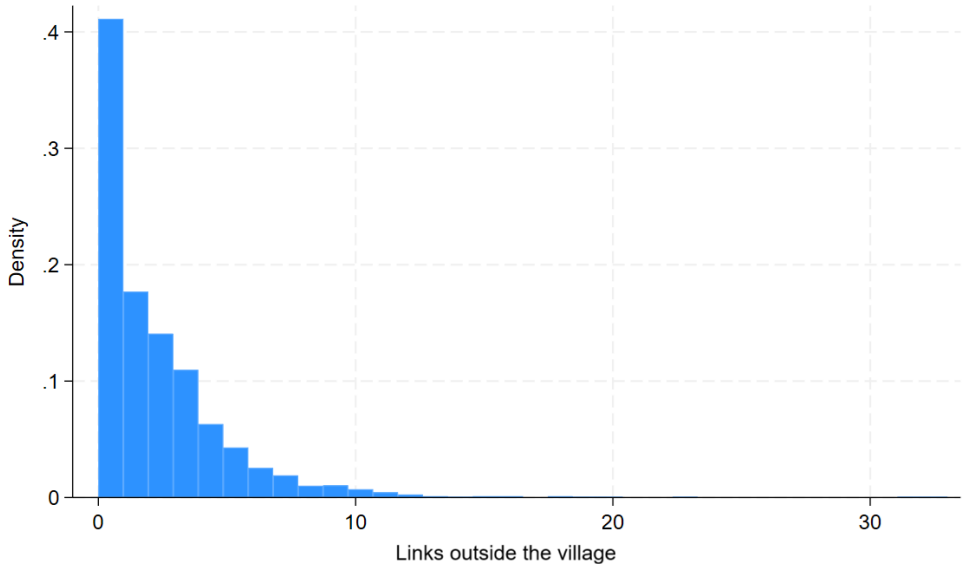

Figure 3. Network links that an individual has outside of the village of residence—density function.

Table 11. Relative importance of family, village authorities and state courts as conflict resolution mechanisms.

	model 1 family	model 2	model 3 village authority	model 4	model 5 state courts	model 6
treated (PFR reform)	0.055	−0.005	0.072	0.007	0.089	−0.021
	(0.077)	(0.054)	(0.060)	(0.039)	(0.112)	(0.051)
controls:	N	Y	N	Y	N	Y
constant	4.295***	4.216***	4.218***	3.875***	4.035***	3.991***
	(0.059)	(0.300)	(0.042)	(0.254)	(0.084)	(0.296)
N.obs.	7063	7051	7060	7048	7064	7054

Notes: Dependent variable: for models 1 and 2, importance of the extended family for resolving conflicts; models 3 and 4, importance of the village customary authorities for solving conflicts; models 5 and 6, importance of the state courts for solving conflicts. All models are OLS regressions. Standard errors are robust for clustering at the village level (52 clusters). Controls include age, gender, whether born in the village, data collection wave, religion of the household head, ethnicity of the household head, whether the respondent is household head, language, distance of the village from paved road, distance of the village from the closest market, whether the village is inside former Dahomey kingdom. ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. p-values of the treatment dummy are based on pre-registered one-sided tests.

C.2.1.3. Networks: links to people outside the village

To get a sense of network links to people who reside outside of the villages, we collected information on the number of households from another village with which a household interacts. We did this across four domains of social life (asking for advice regarding land, borrowing, asking for advice regarding children's health and education, and socializing). In this way, we can calculate the average number of degrees that include people outside of the village.

Village communities that we visited in the context of rural Benin are relatively isolated social units. On average, a household in our sample has slightly fewer than two links towards people outside of their own village, and the modal number of connections outside the village is zero among our 2761 respondents. Figure 3 plots the density function of respondents' replies.

We observe that, on average, participants in treated villages have slightly more outside connections relative to control ones (2.04 versus 1.81, respectively). However, a one-tailed rank-sum test fails to reject the null hypothesis that there is no difference across treated and control villages. The pre-registered regression analysis, reported in models 1 and 2 of table 12, confirms the result.

C.2.1.4. Household composition

To get a sense of whether land titling impacts household composition, we test whether household size (based on adult members) and polygyny vary between treated and control villages. We use adult household size as an indicator of the presence of extended families (we collected this data only in the main survey). We hypothesize that household size will be larger in control villages since a larger cohabiting and thus more cohesive group allows for more efficient land protection.

Results of a non-parametric one-sided test do not support our directional hypothesis. Regression analyses reported in models 3 and 4 of table 12 confirm that household size is not statistically different between treated and control in our sample. Repeating

Table 12. Networks, changes in household composition and importance of patrilineage versus matrilineage.

	model 1	model 2	model 3	model 4	model 5	model 6
	out-of-village links		household size		patri-lineage versus matri-lineage	
treated (PFR reform)	0.229	0.141	−0.072	0.033	0.127	0.151
	(0.249)	(0.135)	(0.197)	(0.048)	(0.115)	(0.099)
controls	N	Y	N	Y	N	Y
constant	1.813***	1.399*	2.835***	1.819***	4.643***	6.706***
	(0.185)	(0.707)	(0.139)	(0.286)	(0.082)	(0.616)
N.obs.	2761	1676	2550	2495	2550	2495

Notes: Dependent variable: for models 1 and 2, number of out-of-village links; models 3 and 4, household size; models 5 and 6, self-reported relative importance of the patri- vs. matri-lineal side of the family. All models are OLS regressions. Standard errors robust for clustering at the village level (52 clusters). Controls include age, gender, whether born in the village, data collection wave, religion of the household head, ethnicity of the household head, whether the respondent is household head, language, distance of the village from paved road, distance of the village from the closest market, whether the village is inside former Dahomey kingdom. ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. p-values of the treatment dummy are based on pre-registered one-sided tests.

Table 13. Cultivating land to prevent misappropriation, market integration and credit access.

	model 1	model 2	model 3	model 4	model 5	model 6
	land cultivation		market integration		credit access	
treated (PFR reform)	0.219	0.190	0.016	−0.016	0.022	0.008
	(0.132)	(0.071)	(0.109)	(0.043)	(0.048)	(0.023)
controls	N	Y	N	Y	N	Y
constant	3.831***	2.612***	2.258***	2.579***	0.738***	-0.244*
	(0.100)	(0.656)	(0.088)	(0.186)	(0.031)	(0.126)
N.obs.	2990	1840	2550	2495	2531	2483

Notes: Dependent variable: for models 1 and 2, self-reported importance of cultivating land to prevent misappropriation; models 3 and 4, indicator of market integration; models 5 and 6, index of credit access. All models are OLS regressions. Standard errors robust for clustering at the village level (52 clusters). Controls include age, gender, whether born in the village, data collection wave, religion of the household head, ethnicity of the household head, whether the respondent is household head, language, distance of the village from paved road, distance of the village from the closest market, whether the village is inside former Dahomey kingdom. ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. p-values of the treatment dummy are based on pre-registered one-sided tests.

the analyses by comparing the fraction of polygamous households among treated and control villages, a dataset that we elicited in our full census survey determines the same qualitative null result (results unreported and available upon request).

C.2.1.5. Importance of patrilineage versus matrilineage

A dominating aspect of social structure in many societies is the lineage. In Benin, the patrilineal tracing of descent and patrilineal communal property holding are frequent among many ethnicities. The consequence of patrilineal societies is that the father's side of the family is considerably more important than the mother's side of the family. We are interested in the importance of the patrilineage since it has been associated with a culture of honour, conflict and trust. We hypothesize that formal land titling will decrease the importance of the father's side of the family and increase the importance of the mother's side (in unilineal descent societies, people only belong to one non-overlapping group, while if descent is traced bilaterally through both the mother's and the father's side, no two people (except siblings) have the same group of relatives). This will create more diverse networks and ultimately foster trust and trustworthiness.

We investigate hypothesis M4 by looking at the self-reported relative importance of the mother's and the father's side of the family across treatment branches.[15] We test the relative importance of the father's and mother's side using a survey question that asks participants to self-report the relative importance of each side. The pre-registered one-tailed rank-sum tests suggest that, for participants in treated villages, the relative importance of the mother side of the family is weakly significantly larger compared with control ones (p-value = 0.09). However, the pre-registered regression analyses, reported in models 5 and 6 of table 12, provide evidence that contrasts with this result. The coefficient associated with the treatment dummy is in no case statistically different from zero at the conventional level of significance.

Finally, to test the relative importance of the two family sides, we use the answers to two survey questions from our full census: 'How important is the husband's/the wife's family for protecting the household land from misappropriation?' One-tailed rank tests suggest that for neither of the two family sides is there a difference in the importance self-reported by our participants. A

Table 14. Norms related to family obligations.

	model 1 injunctive norm	model 2	model 3 descriptive norm	model 4	model 5 punishment norm	model 6
treated (PFR reform)	−0.068	−0.045	−0.063	−0.057	−0.028	−0.073
	(0.076)	(0.045)	(0.106)	(0.063)	(0.091)	(0.051)
controls:	N	Y	N	Y	N	Y
constant	0.034	0.217	0.032	0.414	0.014	0.179
	(0.051)	(0.280)	(0.084)	(0.288)	(0.058)	(0.303)
N.obs.	2568	2495	1342	1307	1228	1190

Notes: Dependent variable: for models 1 and 2, index of injunctive norms intensity; models 3 and 4, index of descriptive norms intensity; models 5 and 6, index of enforcing norms intensity. All models are OLS regressions. Standard errors robust for clustering at the village level (52 clusters). Controls include age, gender, whether born in the village, data collection wave, religion of the household head, ethnicity of the household head, whether the respondent is household head, language, distance of the village from paved road, distance of the village from the closest market, whether the village is inside former Dahomey kingdom. ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. p-values of the treatment dummy are based on pre-registered one-sided tests.

Table 15. Detected effect size (Cohen's *d*) in related studies.

publication	outcome variable	control			treated			Cohen's *d*	year of data collection
		N	mean	s.d.	*N*	mean	s.d.		
Fabbri & Dari-Mattiacci [28]	taking from same-villager in a modified Dictator game	7	4.08	1.37	9	2.83	1.49	−0.87	2017
Fabbri [30]	cooperation with same-villager in a Public Goods game	16	5.05	1.26	16	5.40	1.93	0.21	2017
Fabbri & Bigoni [16]	Gini index in a distributional game	16	0.137	0.184	16	0.190	0.230	0.26	2020
Fabbri [51]	cooperation with out-of-village participants in a multilevel public goods game	16	2.90	0.73	16	3.40	0.73	0.35	2020
Dari-Mattiacci and Fabbri [61]	utilitarian choice in a Trolley moral dilemma	16	0.694	0.122	16	0.715	0.093	0.19	2020
Fabbri, Dari-Mattiacci and Rizzolli [52]	taking from out-of-village participant in a modified Dictator game	16	4.96	0.92	16	4.35	1.15	−0.58	2020

regression analysis replicating models 5 and 6 of table 12 but for using the self-reported importance of husband and wife's family to protect land as elicited in the full census survey confirms the lack of significant differences across treatments (unreported, available from the authors upon request).

C.2.1.6. Importance of family help in cultivating land to protect land property

While a stand-off over land between families or the deterring potential of a clan may come to mind as ways to protect property without formal rights, the protection offered by the family may be much subtler. Without formal property rights, uncultivated land is at a higher risk of being appropriated (and informal norms might offer less protection in this case). However, family and communal ownership might make it easier to ensure that land (at risk of appropriation) is cultivated. We investigated this with a survey question asking for the importance of cultivating land to protect it from misappropriation. The responses range from 1 to 5, increasing in importance.

A one-sided rank-sum test fails to provide evidence that the importance of cultivating land to avoid misappropriation is higher in control villages. Models 1 and 2 of table 13 confirm the result. If anything, the coefficient of the treatment dummy is positive, suggesting a possible increased importance of not leaving land plots uncultivated in treated villages that goes in the opposite direction to our predictions.

C.2.2. Market integration and access to credit

We have included a question aimed at capturing the degree of market integration. The questions ask how much the household consumes of the food that it produces and how much is purchased by other people in the village or at the market, on a 4-point

scale. Moreover, we included two questions that capture access to credit (binary outcome measure). One question asks about access to credit from a formal financial institution, and the other asks about informal access to credit. We test whether land titling increased access to credit.

The result of one-sided non-parametric tests fails to provide evidence that treated subjects report higher levels of market integration or better access to credit. Results of regression models 3–6 reported in table 13 confirm these results.

Finally, we hypothesized that occupational diversity is higher in treated villages. While we were unable to collect data concerning specific jobs, we asked participants whether they were employed in agriculture and/or in non-agricultural sectors. Results of a one-tailed rank-sum test fail to provide evidence that occupational diversity is greater in treated villages.

C.2.3. Norms related to social structure

We included three vignettes that capture aspects of norms related to obligations towards the family. For each of these vignettes, we elicit the perceived injunctive norms, descriptive norms and perceived punishment of norm violations, using the same methods described earlier. This allows us to test for hypothesis M1 with a focus on norms. The three vignettes are as follows:

> V1: Helping the family in land disputes. This vignette describes how a cousin refuses to guard land whose boundaries are contested. It thus captures norms related to the obligations towards family in protecting land.
> V2: Selling land to outsiders without asking permission from family. This vignette describes how a man sells his land to an outsider without asking permission from his family. It thus captures norms related to the obligations towards family in selling land.
> V3: Fairness norms: Borrowing within the family. This vignette describes how a person asks to borrow money from his cousin and the cousin refuses. It thus captures norms related to the obligations to helping family members.

Based on those three vignettes, we create three composite indices, each capturing a different dimension of norms.

- *Perceived injunctive norms*: We calculate the average of the elicited injunctive norms in all three vignettes and standardize the indicator.
- *Perceived descriptive norms*: We calculate the average of the elicited descriptive norms in all three vignettes and standardize the indicator.
- *Perceived norm enforcement*: We calculate the average of the elicited perceived punishment in all three vignettes.

For each of the three norm types, we performed the pre-registered one-sided rank sum test at the village level. In all cases, the results fail to support the hypothesis that norms prescribing obligation towards the family are weaker in treated villages. Results of regression analyses replicating the models described above and reported in table 14 confirm these results.

C.3. Effects sizes reported in the existing literature

Table 15 above gives an overview of the effect sizes reported in related studies on the impact of the PFR reform on various behavioural outcomes. The effect sizes are computed as Cohen's *d* based on village-level statistics.

Appendix D. Experimental instructions

An English translation of the whole set of experimental and survey instructions is available at the following reference: [62].

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