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RESEARCH PAPER

Property Rights and Honest Behavior

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Abstract
We study whether the formalization of property rights affects honest behavior. We conduct a lab-in-the-field experiment based on a standard dice-rolling task in rural Benin, in the same villages where, ten years earlier, a large-scale randomized controlled trial introduced the formalization of land property rights. We find that individuals whose land rights were formalized exhibit significantly stronger preferences for truth-telling. This effect is entirely driven by women with direct exposure to the reform, while no comparable effect is observed among men or at the village level alone. A natural interpretation is that formalization represents a more salient institutional change for women—whose land claims under customary tenure are typically weaker and more contingent—than for men, for whom property rights are already relatively more secure. Our findings thus suggest that property-rights institutions shape moral preferences particularly when they meaningfully alter individuals' economic and legal status, supporting the view that institutions and moral behavior coevolve.

Keywords Property rights · Honesty · Institutions and preferences · Gender · Field experiment

JEL Classification K11 · O17 · D03 · C93

1 Introduction

Since Adam Smith's *Wealth of Nations*, economists have recognized well-defined property rights as fundamental in fostering economic development. Besley and Ghatak (2010) list four principal channels: property rights (i) enable individuals to

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internalize returns from investment and effort (Smith 1976; Locke 1980; Grossman and Hart 1983); (ii) decrease unproductive defense costs while enhancing coordination (Hobbes 2017; Hume 2007; Smith and Price 1973); (iii) facilitate gains from trade as assets can be transferred to their most productive users (Coase 1960); and finally (iv) provide collateral for capital market access, promoting further investment and thus development (Feder and Feeny 1991; De Soto 2000). Beyond these established mechanisms, this paper explores an additional channel—one that operates through the influence of property rights on social and moral preferences, essential lubricants of well-functioning markets and societies (Bowles 2016).

Our contribution to this literature lies in a lab-in-the-field investigation of the effects of land titling reform on the moral norm of honesty—specifically, truth-telling. We conducted a lab-in-the-field experiment employing a variant of the dice-rolling task (Jiang 2013; Fischbacher and Föllmi-Heusi 2013) across dozens of villages of rural Benin that participated in a randomized controlled trial conducted between 2009 and 2011 under the auspices of the World Bank and the Millennium Challenge Corporation to study the impact of formalizing land property rights on economic development (Goldstein et al. 2018).

In general, institutions such as property rights are known to shape preferences and influence behavior beyond mere incentive effects (Bowles 1998). However, the overall direction of these effects remains theoretically ambiguous—institutions may either reinforce (crowd-in) or undermine (crowd-out) intrinsic motivation, with potential implications for economic behavior (Frey and Jegen 2001; Gneezy et al. 2011; Bowles and Polania-Reyes 2012). In our specific context, the formalization of property rights through land titling reform—thereby establishing a more predictable rule of law—may have competing effects on the moral norms underlying individuals' preferences for truth-telling. On the one hand, formalizing property rights could promote law-abiding behavior, reinforcing intrinsic adherence to moral norms like truth-telling. On the other hand, it may lead to cheaper third-party enforcement, which might crowd out the intrinsic motivation to abide by these norms.

Our results corroborate previous findings showing that ownership fosters pro-market beliefs and behaviors (Di Tella et al. 2007) and strengthens social capital (DiPasquale and Glaeser 1999), supporting an additional channel linking property rights to market functioning through moral preferences like trust, risk aversion and honesty (e.g., Smith 1976; Genovesi 2005; Bowles 1998, 2016; Frey and Jegen 2001; Bruni and Sugden 2013; Fabbri 2021; Dari Mattiacci and Fabbri 2023; Fabbri and Dari Mattiacci 2021; Fabbri et al. 2021, 2024).

The remainder of this article proceeds as follows: Sect. 1 provides a literature review, detailing the institutional framework of the Beninese reform alongside our hypothesis; Sect. 2 presents the design of our lab-in-the-field experiments and fieldwork procedures; Sect. 3 reports our main results, while Sect. 4 illustrates truthful reporting. In Sect. 5, we discuss potential mechanisms underlying our findings before presenting concluding remarks. The Appendix contains additional details of the empirical analysis and the instructions for the experiments.

2 On Markets and Honesty

Our paper contributes to the enduring debate on the interplay between markets and morals. Following Hirschman's (1982) classic discussion, this relationship has been interpreted in different ways. In this paper, we focus on two contrasting views. On the one hand, the *Doux-commerce* thesis" posits that commerce serves as a moralizing force that improves society (as in Hume (1739), Smith (1776), and Genovesi (1765); see also Bruni and Sugden (2000, 2008). On the other hand, the *Self-Destruction* thesis contends that wealth accumulation erodes moral values and fosters unethical behavior (drawing on Marx, Veblen, and, more recently, Sandel 2012).

In recent decades, the *Self-Destruction* thesis has experienced a disguised revival. Several authors (e.g., Anderson 1993; Sandel 2012) have criticized the market and ongoing commodification for their capacity to erode moral values. Falk and Szech (2013) provided experimental evidence supporting this market-corrupts-morals hypothesis, with recent research by Ziegler et al. (2024) reaching similar conclusions. Conversely, proponents of the *Doux-commerce* thesis include Bowles (2016), Bruni and Sugden (2013), Choi and Storr (2020, 2023), Bartling et al. (2023), and Dewatripont and Tirole (2024). Bruni and Sugden (2013) identify specific market virtues contingent upon recognizing mutual benefit as the market's fundamental purpose. Hugh-Jones (2016) showed that honesty correlates positively with historical GDP and Protestantism, suggesting long-run linkages with economic development, and Mokyr (2008) suggested that social conventions were critical to the early rise of the industrial revolution in the eighteenth-century UK.

While the role of morals—and honesty in particular—in economic development has a long intellectual tradition, the experimental study of honesty and cheating in economics emerged in the early 2000s with Gneezy (2005) who studies how individuals behave when they can benefit from lying and showing that cheating is sensitive to economic incentives and social costs, laying the groundwork for later research on dishonesty and moral trade-offs. Truth-telling has since been examined using two principal approaches: the "deception game" that follows Gneezy (2005) and the "cheating game" (Fischbacher and Föllmi-Heusi 2013). In deception games, participants privately receive information and can misreport it for monetary gain, providing a simple yet powerful tool for assessing dishonesty. Cheating games, on the other hand, typically involve self-reporting outcomes from random processes (e.g., die rolls), allowing individuals to inflate earnings through dishonesty. Although cheating appears more robust in deception games (Gerlach et al. 2019), most studies favor cheating games for their ability to isolate self-signaling from social-signaling (Fries et al. 2021). This rapidly growing literature has been surveyed by Rosenbaum et al. (2014) and synthesized in a comprehensive meta-analysis by Abeler et al. (2019), which documents a robust baseline preference for truth-telling: on average, three-quarters of potential gains from cheating are left on the table. This finding holds under high stakes and repetition and, with some variation, across countries and cultures. Cohn et al. (2019) corroborated this pattern through a lost-wallet field experiment conducted in 355 cities across 40 countries. The external validity of these laboratory measures has been established through correlations with real-world behavior: dice-rolling honesty predicts positive

school conduct among US students (Cohn and Maréchal 2018), work ethics (no absenteeism) among Indian nurses (Hanna and Wang 2017), fare payment on French public transport (Dai et al. 2018), and good conduct among Swiss maximum-security inmates (Cohn et al. 2015). It negatively correlates with country-level indices of rule violations encompassing corruption, tax evasion, and political fraud across 23 countries (Gächter and Schulz 2016).

While many scholars view Abeler et al. (2019) three-quarters of potential gains from cheating as a "glass half full," others argue that the remaining quarter of potential gains from lying—which is still exploited—represents a significant and non-negligible portion worth investigating. While the literature on the determinants of cheating is extensive, here we focus on factors that serve as covariates in our analysis.

Beginning with *gender*, Grosh and Rau (2017) showed that women are significantly more honest than men, especially when it comes to selfish black lies (e.g., Kajackaite and Gneezy 2017; Jung and Vranceanu 2017; Erat and Gneezy 2012), tournament incentives schemes (Nieken and Dato 2016), school tests (e.g., Case et al. 2019), or competitive settings (Gneezy et al. 2003; Jorgensen et al. 2022). Nonetheless, some studies did not find any difference (e.g., Pascual-Ezama et al. 2015). Regarding *education*, Harvey (2015) found that generalized morality is negatively correlated with adult literacy rates, while Alan et al. (2020) documented a higher likelihood of cheating among children with higher IQs and socioeconomic status. *Age* effects remain inconclusive. Azar and Applebaum (2020) find older Israeli children more honest than younger ones, with no gender differences, whereas Friesen and Gangadharan (2012) observe age increasing dishonesty only among men. Evidence on *wealth* is similarly mixed. Piff et al. (2012) find upper-class individuals behave more unethically than lower-class individuals, though Boonmanunt et al. (2020) show poverty itself does not affect willingness to cheat. Yaniv et al. (2017) suggest that higher achievers may be bigger cheaters.

Religion also yields conflicting findings (e.g., Kirchmaier et al. 2018; Shariff et al. 2014; Marquette 2012; Allmon et al. 2000). Some studies report negative correlations between religiosity and cheating (Bloodgood et al. 2008; Rettinger and Jordan 2005), while others find no significant effects (Teo et al. 2024; Mensah and Azila-Gbettor 2018; Shariff and Norenzayan 2011). Kirchmaier et al. (2018) find Christians less prone to betrayal than non-believers, while Meiseberg et al. (2017) identify no behavioral differences between business and theology students, attributing cheating primarily to incentives and opportunity costs.

While this literature has identified a rich set of individual-level correlates of honest and dishonest behavior, it has largely focused on personal characteristics, incentives, and situational features. We contribute to this line of work by additionally examining, in Sect. 6.3, how reporting behavior varies with the key covariates discussed above. However, much less is known about whether—and how—core market institutions shape moral preferences themselves. In particular, existing experimental studies rarely examine the role of institutionalized market settings that systematically alter individuals' economic and legal environment. Formalized property rights constitute a central pillar of market exchange, yet their potential impact on honesty and truth-telling remains theoretically ambiguous and empirically unexplored. Building on the insight that institutions shape preferences beyond pure incentive effects (Bowles 1998), our

study exploits exogenous variation in property-rights formalization to move beyond individual correlates and assess whether institutional change affects honest behavior. The individual-level determinants reviewed above therefore serve both as controls and as benchmarks, allowing us to isolate the moral impact of property-rights institutions from compositional differences in the population.

3 On Formalized Property Rights and Development

This paper builds on a Randomized Control Trial (RCT) that formalized property rights in approximately 300 randomly selected villages in rural Benin. Before delving into our analysis, we find it necessary to outline what the alternative customary property regime was, what the aim of the reform was, and the hypothesis that guided it, what the design and procedures of this earlier RCT were, which, although not conducted by us, has significantly informed our research.

Following the influential work of the World Bank (see *inter-alia* Feder and Feeny 1991 and Hernando De Soto 2000) in the last thirty years, several developing agencies have promoted many projects to formalize previously undocumented, informal, or customary property rights. Their hypothesis was as powerful as simple: if land and housing assets informally owned by poor people were to be legalized and recorded, they could be used as collateral to gain access to capital markets and hence promote investment and development. The hypothesized "De Soto effect" (Besley et al. 2012) has met with mixed evidence of success: Jacoby and Minten (2007) found that formalization had no appreciable impact, while Field (2005), Galiani and Shargrodsky (2010), and Deininger and Feder (2009) found that formalization had a positive impact but it was not channeled through credit as the De Soto effect hypothesized. The only two Randomized Control Trial studies conducted so far on the matter found minor average effects on only some measures of investment in Benin (Goldstein et al. 2018), and in Zambia, no significant effect on any measured investment outcome (Huntington and Shenoy 2018). The first of the two RCTs is the baseline of our lab-in-the-field experiment.

3.1 The Benin Plan Foncier Rural

Until the nineties, virtually all land property in Benin was held under the customary tenure regime. This regime characterized all villages of rural Benin interested by the RCT and persists to these days in the control villages. In these villages, customary owners hold a set of (potentially overlapping) informal use rights over a parcel. Access to land and arbitration of disputes is demanded by customary laws, and local customary authorities (usually at the village level) arbitrate disputes. This authority is embedded in kinship and local governance, and it frequently takes a collective or patriarchal form. Importantly, customary tenure also includes multiple, overlapping claims on the same land. Alongside *primary* (management/administrative) rights sit *secondary* or *operational* rights—use rights that are delegated or derived from social relationships

(for instance, wives cultivating plots allocated by husbands, junior relatives farming family land, or groups using commons) (Giovarelli et al. 2013).

In the early nineties, the Beninese government implemented a land-tenure reform known as the *Plan Foncier Rural* (PFR), which, however, remained only on paper until the Millennium Challenge Corporation proposed the realization of the RCT. In the selected villages, public officials operating under the PFR carried out socio-land surveys at the village level to identify rights holders and their rights and to demarcate parcel boundaries for registration in a public repository (Lavigne Delville 2006). While this process does not confer de jure legal ownership, it provides a presumption of ownership recognized by courts, enabling the sale or use of registered plots as collateral. The certificates registering possessory rights can be converted into land titles through a shorter, cheaper, and simplified procedure compared to the regular process for titling uncertified land.

Within the RCT design, after a preliminary phase, 576 out of 2062 rural villages were identified, and a subsample of 300 villages was randomly selected via public lottery (the PFR Villages). The other 276 non-selected villages did not receive any intervention, being used as control, and, as of today, continue to have customary land rights. For a detailed description of the reform's characteristics, implementation process, and its effects on investments, please refer to Goldstein et al. (2018) and Omondi (2019).

3.2 Formalized Property Rights and Gender

The move from customary to formal land tenure is particularly consequential for women (Goldstein et al. 2018), as women's access to land is typically grounded in *secondary* rights rather than in *primary*, inheritable ownership (Botea et al. 2026). Even where statutory law is formally gender-equal, as in Benin, land relations remain strongly gendered in practice. Women's effective rights are often mediated by marital status and by the informal or unregistered nature of rural unions—especially in early or polygamous marriages—thereby limiting the practical reach of legal marital protections. As a result, women's land claims tend to be socially recognized but weakly protected.

Because formalization produces an official and legally salient record, it carries the risk of crystallizing pre-existing gender hierarchies when land is primarily identified as men's property. In such cases, formalization may strengthen male ownership while delegitimizing women's secondary use rights derived from customary arrangements. Once rights are documented in favor of a single primary holder, subsequent processes—such as titling or land transactions—can further erode women's ability to defend their claims. At the same time, women routinely cultivate land, invest labor, and engage in collective production. When a formalization process explicitly recognizes these claims, it can instead strengthen women's tenure security by making their rights visible and enforceable (Goldstein et al. 2016).

The MCC-supported implementation of the PFR in Benin was explicitly aware of these risks and opportunities and sought to ensure that formalization would operate as a force for gender inclusion rather than exclusion (Botea et al. 2026; Goldstein

et al. 2018). Women's groups were identified as key stakeholders; surveys attempted to document both primary and secondary rights; and the PFR process combined parcel surveys, household interviews, and public validation of landholder lists. Field teams were trained to consult women directly, and mitigation measures—including standardized contracts for secondary rights and training of local officials—were introduced to protect claims not fully captured in the land plans. Additional efforts included women's representation in local land institutions, targeted support for women's productive activities, and information campaigns with explicit gender components (Giovarelli et al. 2013). Whether village-level formalization ultimately translated into meaningful empowerment for women, however, remains an empirical question.

Taken together, these features imply that the institutional meaning of property-rights formalization is not uniform across individuals. Under customary tenure, men typically already enjoy relatively secure and socially recognized primary claims, while women's access to land is more often indirect, contingent, and weakly protected. As a result, the transition from customary to formalized tenure is likely to represent a qualitatively more salient institutional change (for the better or for the worse) for women than for men.

This asymmetry is central for our empirical analysis (see in particular Sect. 6.5). If property-rights institutions affect moral behavior by altering individuals' perceived responsibility, security, or relationship to formal rules, such effects should be most visible where formalization constitutes a genuine change in economic and legal status rather than a mere codification of existing arrangements. The gendered structure of land tenure in rural Benin therefore provides a natural setting to explore heterogeneity in the moral effects of property-rights formalization.

4 Our Own Lab-in-the-Field Experiment

In our own study, we visited 32 of the 576 villages that were part of the original RCT pool: 16 among the PFR villages and 16 among the control villages (hereinafter also referred to as non-PFR villages). In particular, the 32 villages were randomly selected from the villages included in the original RCT, stratified across treatment status and geography; in particular, they were randomly selected from a subset of villages located in two areas: the first one in the southern provinces of Mono and Couffou, and the other one in the northern provinces of Alibori and Borgou. For a map of the intervention, readers may refer to Fabbri et al. (2024).

In each village, we run a lab-in-the-field cheating experiment to measure the level of cheating in both PFR villages and non-PFR villages. Participants were asked to privately roll a six-sided die ten times,[1] and report their outcomes on a sheet collected by the experimenter. They received a payoff proportional to one randomly chosen

[1] The dice-rolling task was repeated ten times for each participant, following standard practice in the experimental literature on dishonesty (Jiang 2013, Ariely et al. 2019). Repetition increases statistical power relative to a single-roll design and allows systematic reporting patterns to be distinguished from random noise. With multiple realizations per individual, persistent deviations from the truthful distribution are informative about underlying reporting behavior, whereas single outcomes are largely uninformative. Repetition also mitigates the impact of occasional mistakes or misunderstanding in any one round, which would otherwise disproportionately affect single-shot measures.

report among the ten submitted. Since participants self-reported their dice outcomes, they had the potential to cheat. While the experimenter did not verify the true outcomes, we could assess whether the group's reports were distributed truthfully.

Data collection occurred from January to March 2020. Before each session, a research assistant visited the selected village and invited as many volunteers as possible to gather at a scheduled time and place for the research project. Participants had to be village residents over 18 years old, with only one participant per household allowed to participate.

On the day of the experiment, the research team randomly selected nine male and nine female participants from those who responded to the invitation. Those not selected received a show-up fee of CFA 500 ($0.85) and were asked to leave. The team conducted 32 fieldwork sessions across different villages (16 of which were PFR villages), with 575 individuals participating in the experiment.

Sessions were held in common spaces like school classrooms or public buildings. The experimenter read the instructions aloud; for participants who were not fluent in French, these were translated into local languages (*fon* for the southern part, *bariba* for the northern part of Benin); then participants were called individually into a separate room to make their decisions privately. All entered the decision room, answered control questions to ensure they understood the instructions, and then made their decisions alone. To minimize experimenter effects, a blind procedure was used for the subject's decision. Participants rolled the dice inside a cup with a hole on the bottom that allowed the subject to privately read the outcome of the roll. They transferred tokens from a "Paired Participant" envelope to their "Own" envelope to determine their payoff, sealing both envelopes before leaving.

Participants also engaged in an incentivized risk elicitation task, a socio-demographic survey, and additional unrelated fieldwork activities. Each session lasted about three hours, and participants earned an average of CFA 2800 ($4.50), equivalent to approximately one and a half days' wages for the median participant in the sample.

5 Main Hypotheses

The primary aim of the study is to determine if the land rights reform influenced the willingness to report dice outcomes truthfully. Our hypothesis is that formalizing land rights affects the degree of truth-telling, tested by comparing the average reported outcomes between those who have experienced formalized property rights and those who have not. The direction of the effect is not self-evident as it might depend on whether property rights prevailing crowding-in o crowding-out honest behavior. Identifying exposure to formalized property rights is non-trivial. The original RCT randomized the reform at the village level, so that formalization was implemented across all land parcels within selected villages. Within these PFR villages, some individuals previously held informal land and thus personally experienced formalization (PFR individuals), while others did not own land and were therefore not directly exposed. Conversely, some individuals residing in non-PFR villages owned land in areas covered by the PFR program and thus experienced formalization despite living in control villages.

Both dimensions -the one of being part of a *PFR village* where formalization took place and the one of being a *PFR individual* directly exposed to the formalization process- are potentially relevant. Therefore, in addition to the simple observation of whether the subject lives in a PFRvillage or a non-PFR village, we have collected additional data on whether subjects actually owned property individually or as a family, and whether this property was held under customary or under the PFR program in both PFR villages and non-PFR villages. We thus can test the following hypotheses:

Hypothesis 1: Preferences for truth-telling are the same in PFR and non-PFR villages.

This hypothesis captures potential village-level moral externalities of the reform, such as changes in local norms, expectations about enforcement, or exposure to formal legal institutions, even among individuals who did not personally experience land formalization.

Hypothesis 2: Preferences for truth-telling are the same among PFR and non-PFR individuals.

This hypothesis isolates the moral effect of direct, personal exposure to formalized property rights and tests whether experiencing the transition from customary to formal tenure affects intrinsic honesty.

Both hypotheses are tested using simple non-parametric tests and by estimating statistical models that differ in the primary independent variable (either PFR village or PFR individual). In both cases, the dependent variable is the reported outcome of the dice roll, and each model includes a vector of individual characteristics specified in the post-experimental survey.

We assess the impact of the reform on the preference for truth-telling across different treatments. Additionally, we explore potential variations in reported outcomes based on factors such as distance from paved roads (used as a proxy for access to formal justice and market integration), gender, income, age, education, religion, and ethnicity. We shall mention the fact that hypothesis 1 and the heterogeneity analysis have been pre-registered in the pre-analysis plan.[2]

6 Results

6.1 Descriptive Statistics and Sample Balance

The experiment involved 576 subjects organized into 32 sessions. Of these, 94 participants did not report any outcomes for this activity, leaving 482 subjects who reported at least one roll. In addition, 35 participants reported only the outcome of their first attempt rather than all ten attempts. As a result, the number of observations is 482 for the first roll and 447 for each of rolls 2 through 10, yielding a final total of 4505 reported outcomes. Without supervision during the activity—necessary to preserve

[2] The experimental design, hypotheses to be tested, and regression model specifications were documented in a pre-analysis plan submitted to the American Economic Association's RCT Registry (ID: AEARCTR-0005324).

Table 1 Subjects across village and individual treatment variables

	Non-PFR individual (Does not own registered land)	PFR individual (Owns registered land)	
Non-PFR village	209	35	244
PFR village	72	166	238
	281	201	482

Subjects for which observations on the dice-roll are at least partly available in treated and non-PFR villages, and that have their own land under PFR

protocol anonymity—we could neither correct misunderstandings nor request repetitions. In some cases, participants' illiteracy rendered results after the first attempt indecipherable.[3]

Concerning the successful randomization of the original RCT, a comprehensive impact evaluation by the World Bank's Gender Innovation Lab confirmed the success of lottery-based randomization. Using extensive pre- and post-treatment survey data collected by a national agency and administrative data from the Millennium Challenge Corporation—Benin, the evaluation demonstrated a pre-intervention balance between treatment groups. This cross-evaluation dispelled concerns about the randomization process (Omondi 2019; Goldstein et al. 2018).

Another important validity check concerns potential treatment spillovers. Several mechanisms could generate contamination: villagers migrating between control and PFR villages post-RCT, or residents of control villages owning land in PFR villages, and vice versa. To assess these possibilities, we collected data on participants' origins, migration status, migration reasons, and village residence duration. Individuals in control villages might also have learned about the PFR and its benefits over the ten-year intervention period, though only 27% of participants in our sample reported awareness of the reform.

In addition to the variable indicating residence in a *PFRvillage*, we coded another variable *PFRindividual*, coded as 1 when the participant's household possesses a land parcel registered under PFR. Table 1 illustrates how our two main treatment variables overlap.

Indeed, 72 individuals in PFRvillage were not owners exposed to the PFR, perhaps because they did not own any land or because their plot was excluded from the program for various reasons (for example, due to a lack of agreement with neighbors or absence during the survey). On the other hand, 35 individuals in non-PFR villages owned land under the PFR program. This might occur if a respondent lived in a non-PFR village but owned land in a PFRvillage or if they belonged to the 0.2% of Beninese who independently acquired a PFR property certificate, even though they reside in a non-PFR village, by self-financing the process. We will illustrate results both for *PFRvillage* and *PFRindividual*. It should be noted forefront that the original RCT randomized the property rights formalization at the village level, not at the individual

[3] A significant number of participants had never used a pen before; they would mark the initial figure, rendering subsequent results incomprehensible.

level and this raises potential issues of endogeneity and selection bias of the results when using *PFRindividual*. We discuss this possibility at length in Paragraph 6.5.

Tables 5 and 6 in Appendix A provide descriptive statistics for the pre-registered socio-demographic characteristics of our participants across our two treatment variables. Although the sample is well-balanced for most variables such as social status, income, and wealth levels, participants in the PFR villages tend to be older, more likely to be Christians, to be married and to manage household finances, and have a slightly higher literacy rate than in the non-PFR villages. The sample is more balanced across the PFR individual variable with some differences emerging in age, religion, and some measures of wealth. To address these imbalances, we include these characteristics as controls in some of the models of our regression analysis.

6.2 Truthful Reporting

We begin by illustrating the data graphically (See Fig. 1 below and Fig. 2 in the Appendix). Note that participants can report a number between 1 and 6 (displayed on the horizontal axis), while the vertical axis shows the percentage of reports for each outcome. The dashed line represents the expected distribution under truthful reporting, where each outcome has a 1/6 probability. If participants were reporting truthfully, their outcomes would cluster around this dashed line. Conversely, if there were no preference for truth-telling, all participants would report the maximum value of 6, with no reports below that. In each quadrant of Fig. 1, we distinguish between participants based on our main treatments PFR village and PFR individual as well as

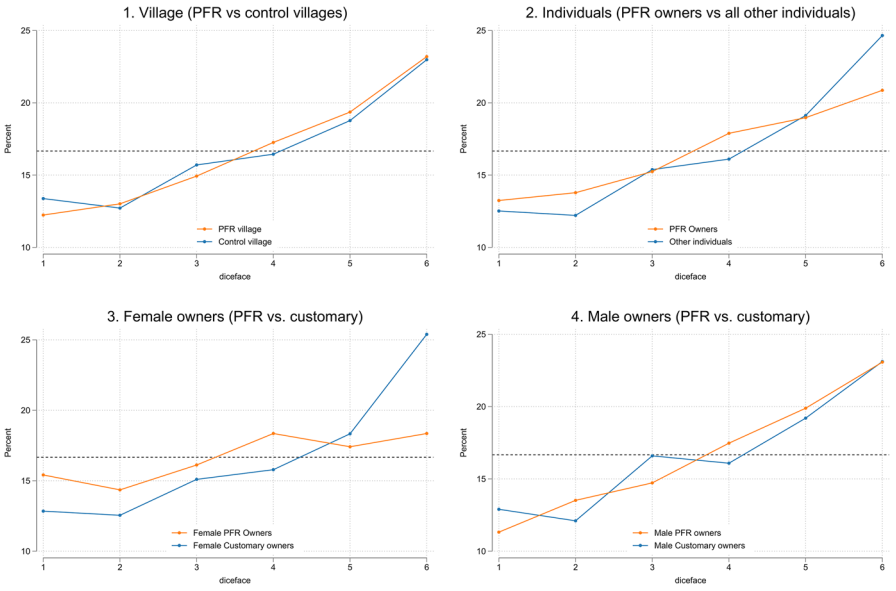


Fig. 1 Distribution of reported dice outcomes by treatment status.

some restrictions applied on treatment variables. In Fig. 2 in the Appendix, we also distinguish participants along other relevant covariates such as age, literacy, wealth, religion, and ethnicity. We also compute statistical tests of the deviations from truthful reporting using the Wilcoxon rank-sum test, given that the standardized reporting measure is bounded and non-normally distributed.

Overall, the average dice report is 3.86, which is only 0.36 above the expected truthful average. In aggregate, subjects appropriate just 0.14[4] of the potential gains from lying. Although this deviation is relatively small, it is strongly statistically significant relative to truthful reporting (zero in the standardized reporting measure), as shown by a Wilcoxon signed-rank test.

Our main treatment variables yield two different results, illustrated by the first two quadrants of * MERGEFORMAT Fig. 1. Residents of PFR villages report dice outcomes that are approximately 2% higher than those in non-PFR villages, though this difference is not statistically significant (two-sample Wilcoxon rank-sum test, $p = 0.47$). In contrast, individuals with direct personal experience of PFR schemes report dice scores that are about 13% lower than those without such experience, a difference that is highly statistically significant (two-sample Wilcoxon rank-sum test, $p = 0.007$).

6.3 Individual Characteristics and Reporting Behavior

Following the extensive literature that explores individual determinants of truth-telling behavior surveyed in Sect. 5, we further explore some interesting patterns in reporting behavior conditional on some of our main covariates. These are illustrated in Fig. 2 in the Appendix. *Age* emerges as a significant determinant of truth-telling behavior. Younger individuals—defined as those below the median age of 35—exhibit lower levels of truthful reporting than older participants, and this difference is large and highly statistically significant according to a Wilcoxon rank-sum test ($p < 0.001$). By contrast, *gender* per se does not affect honesty: although men report slightly more truthfully than women, the difference is not statistically significant (Wilcoxon rank-sum test, $p = 0.117$). Gender nevertheless plays a central role when interacting with property rights formalization, as discussed in Sect. 6.5. *Education* is negatively associated with truthfulness. Literate participants report significantly less honestly than illiterate participants, leaving on average 10.6% more of the potential gains from lying unclaimed among the illiterate. This difference is highly statistically significant (Wilcoxon rank-sum test, $p < 0.001$).

Socioeconomic conditions also matter, though in a nuanced way. Participants with lower-calorie dietary intake—a proxy for lower socioeconomic status—exhibit lower levels of truthful reporting than those with higher-calorie diets and this difference is highly statistically different (Wilcoxon rank-sum test, $p < 0.001$). Conversely, differences in reporting behavior by income and by asset-based wealth are small and

[4] We compute this measure following the standardization of reporting index as in Abeler et al (2019). Specifically, reported outcomes on the 1–6 scale are linearly rescaled to lie between − 1 and 1, with zero corresponding to truthful average reporting. This transformation allows us to directly quantify the extent and direction of misreporting, as well as the implied monetary gains forgone relative to truthful behavior.

statistically insignificant (Wilcoxon rank-sum tests, $p = 0.391$ and $p = 0.154$, respectively). Participants living closer to a *paved road* report less truthfully than those living farther away, and this difference is statistically significant according to a Wilcoxon rank-sum test ($p = 0.018$). *Religious* affiliation further differentiates behavior. Muslim participants report significantly more truthfully than non-Muslims, with the difference being large and highly statistically significant (Wilcoxon rank-sum test, $p < 0.001$). Finally, *ethnicity* also matters: individuals belonging to the Adja ethnic group engage in significantly less truthful reporting than participants from other ethnic groups, with differences that are again highly statistically significant (Wilcoxon rank-sum test, $p < 0.001$). More details on the covariates and relative non-parametric tests are reported in Table 4 in Appendix A.

To conclude, several individual characteristics—most notably age, wealth, and education—are significantly associated with reporting behavior. Given that existing evidence on the relationship between these characteristics and honesty is mixed, our results add new and informative evidence to the literature on honesty and cheating. These effects highlight substantial heterogeneity in individual moral behavior. At the same time, they differ in nature from the effect of property-rights formalization, which reflects exposure to an institutional change rather than stable individual traits. For this reason, we treat these dimensions primarily as covariates and sources of robustness, rather than as separate objects of analysis.

6.4 Regression Analysis

To complement the non-parametric analysis, we estimate a series of parametric models. Because reported dice outcomes are ordered categorical variables and multiple observations are collected from each subject within experimental sessions, we employ mixed-effects ordered logit (cumulative link) models with random intercepts at the subject and session level. This framework allows us to account for within-subject dependence across repeated reports as well as unobserved heterogeneity across individuals and sessions.

Table 2 reports the results from a sequence of models that progressively incorporate these features. A detailed discussion of the econometric specification, including the treatment of dynamic dependence and initial conditions, is provided in Appendix A.[5]

In Model (1), we regress *dicereport* on the village-level treatment indicator (*PFRvillage*). The coefficient is small and not statistically significant, confirming the absence of systematic differences between treated and control villages observed in the non-parametric Mann–Whitney tests.

Models (2)–(5) shift the focus to individual-level exposure to the reform (*PFRindividual*). Model (2) includes *PFRindividual* as the sole explanatory variable and thus

[5] Furthermore, as a robustness check, Table 11 in Appendix A re-estimates the baseline models using simpler ordered logit specifications that alternatively (i) remove session and/or individual random effects (model 13, 14), (ii) include or exclude lagged outcomes (model 15) and use a simpler ologit specification (model 16 and 17). The estimated effects of formalization, gender, and their interaction remain qualitatively similar across these alternatives, suggesting that the main conclusions are not sensitive to the treatment of unobserved heterogeneity or dynamics.

Table 2 Main regression results

	(1) Dicereport	(2) Dicereport	(3) Dicereport	(4) Dicereport	(5) Dicereport
PFRvillage	0.079				
	(0.140)				
PFRindividual		− 0.203**	− 0.353***	− 0.321**	− 0.234*
		(0.099)	(0.133)	(0.130)	(0.135)
1.male			− 0.071	− 0.155	− 0.127
			(0.110)	(0.114)	(0.116)
1.male#c.PFRindividual			0.296*	0.353**	0.234
			(0.172)	(0.172)	(0.176)
Controls	No	No	No	Yes	Yes
Gender heterogeneity	No	No	Yes	Yes	Yes
Initial conditions	No	No	No	No	Yes
Lag terms	No	No	No	No	Yes
Session random effects	Yes	Yes	Yes	Yes	Yes
Subject random effects	Yes	Yes	Yes	Yes	Yes
Observations	4505	4505	4505	4505	4023

Standard errors in parentheses. Dependent variable: dicereport (ordinal). Mixed-effects ordered logit with random intercepts at the session and subject level. Controls (when included) are here condensed and include: age, school, education, married, polygam, christian, vodoun, otherreligion, marketinvillage, incomeweek, wealth_2_2 and are reported fully in Table 9. Model (5) includes initial and lagged dice report dummies; coefficients are displayed in in Table 9. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

captures a reduced-form association between formalization and honesty. The coefficient is negative and statistically significant, indicating that individuals whose land was formalized report lower dice outcomes on average, consistent with increased truth-telling. As subsequent models show, this average effect masks substantial heterogeneity by gender and is driven primarily by women.

Model (3) makes this heterogeneity explicit by interacting *PFRindividual* with gender. The results reveal that the effect of formalization is concentrated among women, while the corresponding effect for men is small and statistically indistinguishable from zero. Model (4) adds a rich set of individual controls, including demographic, socioeconomic, and religious characteristics. The estimated gender-specific effects remain stable in magnitude and significance, indicating that the results are not explained by observable differences across individuals.

Finally, Model (5) constitutes our most complete and demanding specification. It is estimated as a dynamic cumulative link mixed model for ordered outcomes, explicitly tailored to the longitudinal and hierarchical structure of the data. The model includes subject- and session-level random effects to capture unobserved heterogeneity and within-cluster dependence, as well as lagged reported outcomes to model genuine state dependence in individual reporting behavior. Because the inclusion of autoregressive terms in the presence of random effects gives rise to an initial conditions

problem, we address this issue using an approximate conditional likelihood approach. By jointly accounting for ordinal outcomes, unobserved heterogeneity, apparent and true behavioral persistence, and clustering at multiple levels, Model (5) provides the most rigorous framework for inference across our set of specifications (more details on the specifications are reported in Appendix A) The main patterns identified in earlier models persist: truthful reporting is generally high, cheating behavior is concentrated among a subset of individuals, and the effect of property-rights formalization remains concentrated among women with direct exposure to the reform. Reading our five models together with the results from the non-parametric analysis, we highlight the following results:

Result 1. Honest behavior, as measured by preference for truth-telling, is generally strong.

We observe a sustained preference for truth-telling in our sample of Beninese villages, as subjects only appropriate 14% of the potential gains from cheating (see non-parametric analysis), which is around 60% of what has been observed in the meta-analysis of Abeler et al. (2019). The moderate amount of cheating observed is concentrated on over-reporting intermediate dice numbers (4 and 5) rather than on the maximal dice outcome (6) (See variables *lag_dicereport* in model 5B of Table 9 in Appendix A). This finding aligns with the literature from Gneezy and Kajackaite (2020), Gneezy et al. (2018), and Kajackaite (2018), among others. In other words, cheating behavior is concentrated among a few individuals who exhibit a consistent and recurrent cheating pattern. This is evident when examining the variable *init_dicereport* in Model 5B (Table 9 in Appendix A), which shows that individuals who report numbers higher than 3 in their first dice roll are more likely to continue reporting higher numbers subsequently.

Result 2: Formalization of Property rights increases the honest behavior of land owners whose property has been formalized.

The formalization of property rights has a statistically significant effect on honesty and truthful reporting in the dice-rolling game. This relationship is driven primarily by direct individual exposure—i.e., being a landowner with property formalized under the PFR program (captured by PFRindividual in Models 2–5)—rather than by simply residing in a village where the program was implemented (captured by PFRvillage in Model 1). Since formalization was randomized at the village level, the presence of a significant effect only at the individual level raises the concern that the estimate may partly reflect selection into formalization rather than a purely causal effect. We discuss and rule out this potential selection bias and its implications in the next paragraph.

Result 2.1. The Effect of Formalization is concentrated among women.

The effect of property-rights formalization on honest behavior observed in our data is concentrated among female landowners. Across Models 3–5, women whose land rights were formalized under the PFR scheme exhibit significantly lower levels of cheating than women whose customary rights remained informal, whereas no comparable effect is observed among men. This pattern indicates that formalization is associated with changes in honest behavior, primarily where it represents a substantive change in property-rights security.

This finding stands in contrast to concerns in the land-tenure literature surveyed in Sect. 3.2 that formalization may reinforce male control while leaving women's claims largely unchanged. In the Beninese context we study, however, the PFR altered women's position in a meaningful way. Formalization transformed women's land access from largely secondary, socially contingent claims into formally recognized ownership. The behavioral response we observe suggests that this institutional shift is associated with consequences extending beyond land use and investment decisions, affecting moral behavior as measured by truth-telling. That the strongest associations emerge among women—rather than among men, who typically already held more secure rights under customary tenure—is consistent with the interpretation that formalization matters most when it constitutes a genuine institutional change.

A natural alternative explanation is that this pattern reflects pre-existing gender differences in honesty or social preferences. Previous studies have shown that women may behave more honestly than men in some contexts, particularly in the case of selfish black lies (e.g., Erat and Gneezy 2012; Kajackaite and Gneezy 2017), and that gender differences in behavior may arise from socialization processes (Gilligan 1982; McCabe et al. 2006). Women have also been found to exhibit higher Social Value Orientation on average (Grosh and Rau 2017; Balliet et al. 2009; Murphy et al. 2011), which could be associated with greater concern for others. However, this explanation is difficult to reconcile with the observed heterogeneity in our data. Not all women display higher levels of honesty; rather, the association is confined to women who directly experienced the PFR reform. Women whose land rights remained informal do not differ systematically from other women who do not own any land or men or from men in their reporting behavior. This pattern suggests that the observed differences are not driven by gender per se (see also Sect. 6.3 where the gender covariate is considered in absence of the main treatment variable), but by exposure to the institutional recognition of property rights. One plausible mechanism is that formal ownership increases the perceived responsibility associated with asset control (Kobayashi and Fukushima 2012), as well as the perceived individual and social costs of dishonest behavior (Gneezy 2005; Fischbacher and Föllmi-Heusi 2013; Abeler et al. 2019; Gneezy et al. 2018).

6.5 Identification, Selection and the Role of Gender

The gender-specific pattern documented in Result 2.1 raises an important identification concern. Individual exposure to the PFR program is not randomly assigned: while villages were randomly selected for the intervention, formalization at the individual level

reflects pre-existing landholding status and participation in the registration process. As a result, the association between formalized property rights and honest behavior—particularly among women—could in principle reflect selection rather than a causal effect of formalization. For example, women who obtained formally registered land may differ from other women in unobserved traits such as socioeconomic status, social embeddedness, or attitudes toward rules and institutions, which could independently affect reporting behavior.

To address this concern in this section, we adopt a stepwise robustness strategy that progressively narrows the comparison and isolates the institutional component of the treatment. First, we restrict the analysis to landowners only, thereby comparing individuals with formally registered land to those who hold land under customary arrangements and ruling out differences driven by landlessness. Second, within the owners-only sample, we control for the size of landholdings using a logarithmic transformation of reported hectares to account for heterogeneity in land-related wealth and status that may correlate with honest behavior. Third, we further restrict the analysis to female landowners, for whom the PFR represented a qualitatively distinct institutional change relative to customary tenure. Finally, we complement these sample restrictions with a control-function approach that exploits the original village-level randomization of the PFR program to assess whether remaining individual-level selection is driving the results. Throughout this sequence, balance checks on observable characteristics are used as supporting diagnostics within each restricted sample.

Step 1. Owners-only sample: restricting comparisons to landholding households.

Our first robustness step examines whether the association between property-rights formalization and honest behavior is mechanically driven by differences between landless respondents and those embedded in landholding arrangements. To address this concern, we restrict the analysis to respondents whose families own land and compare individuals with formally registered land to those whose land rights remain customary.[6]

Before turning to the regression results, Appendix Table 7 reports descriptive statistics for formal and customary landowners. The two groups are broadly comparable across a wide range of demographic, socioeconomic, and village characteristics, although some differences remain, notably in age and religious affiliation. Restricting the sample to landholding households allows us to eliminate landlessness as a potential source of spurious differences in reporting behavior, while preserving a meaningful comparison between formalized and customary owners.

To isolate this specific channel in Model (6), we mirror the reduced-form specification of Model (2) and re-estimate it on the owners-only subsample, without introducing additional controls. This specification is designed to answer a narrow diagnostic question: whether the main association persists once landless respondents are excluded.

[6] Landholding households are identified using a survey question on whether the respondent's family owns land. We rely on this family-level indicator rather than an individual-only measure because land rights in rural Benin are commonly organized at the household or lineage level, and individual self-reports may not fully capture access, control, and use rights. The cross-tabulation shows that the vast majority of both formalized and non-formalized respondents belong to landholding households, while a small number of landless respondents appear in both groups.

The estimated coefficient on *PFRindividual* remains negative and similar in magnitude to the full-sample estimate, although it loses some statistical power because of the reduced sample size. This pattern indicates that the association observed in the full sample does not hinge on differences between landless and landholding respondents, but persists when attention is restricted to landholding households.

Step 2. Accounting for heterogeneity in land size among owners.

While Step 1 rules out landlessness as a mechanical driver of the observed association, landholding households may still differ substantially in the *scale* of their land endowments. Larger landholdings may proxy for higher wealth, social status, or integration into local institutions, which could plausibly correlate with honest behavior independently of formalization. To address this concern, we further refine the owners-only comparison by controlling for land size. Specifically, within the subsample of landholding households, we augment the reduced-form specification with the logarithm of the reported family land size (measured in hectares). The log transformation mitigates the influence of extreme values and allows for diminishing marginal effects of land size. Results from this specification (model 7) show that the estimated coefficient on *PFRindividual* remains negative and similar in magnitude to the owners-only estimate, while land size itself is not significantly associated with reporting behavior. The inclusion of land size, therefore, does not alter the qualitative pattern documented in Step 1 and suggests that heterogeneity in land endowments and associated wealth differences do not account for the observed association between formalization and truthful reporting (Table 3).

Table 3 Robustness checks: landowners-only restrictions and land-size controls

	Dicereport (6)	Dicereport (7)	Dicereport (8)	Dicereport (9)	Dicereport (10)
PFR individual	– 0.170*	– 0.175*	– 0.342**	– 0.299**	– 0.233*
	(0.101)	(0.101)	(0.139)	(0.139)	(0.138)
Log(hect family land)		– 0.045	– 0.026	0.074	– 0.051
		(0.044)	(0.065)	(0.073)	(0.064)
Sample	Landowners	Landowners	Female landown	Female landown	Female landown
Controls	No	No	No	Yes	Yes
Land size control	No	Yes	Yes	Yes	Yes
Initial conditions	No	No	No	No	Yes
Lag terms	No	No	No	No	Yes
Observations	4191	4191	1886	1886	1683

Standard errors in parentheses. Dependent variable: dicereport (ordinal). Mixed-effects ordered logit (meologit) with random intercepts at the session and subject level. Controls in models 9 and 10 (not displayed) include: age, school, education, married, polygam, christian, vodoun, otherreligion, marketinvillage, incomeweek, wealth_2_2. In model 10, i.init_dicereport and i.lag_dicereport are included as factor dummies; their coefficients are omitted for readability

*p < 0.10, **p < 0.05, ***p < 0.01

Step 3. Gendered effects: focusing on female landowners.

The third step exploits the gendered structure of customary land tenure to further isolate the institutional component of property-rights formalization. Under customary arrangements in rural Benin, women's access to land is typically mediated through secondary or derived rights, whereas men more often hold primary and relatively secure claims (see Sect. 3.2). As a result, formalization represents a qualitatively different institutional change for women than for men. Restricting attention to female landowners, therefore, allows us to focus on the group for whom the PFR plausibly altered the nature of property rights most substantially. Appendix Table 7 reports descriptive statistics comparing women with formally registered land to those whose land rights remain customary. In this restricted sample, the two groups appear even more balanced than in the broader owners-only sample, with very similar demographic, socioeconomic, and village characteristics. We illustrate graphically this difference in the third and fourth quadrant of Fig. 1, where we report the outcomes of the dice rolls for male (on the right) and female (on the left) when we restrict the sample only to landowners, and we compare owners of formal PFR land and informal customary land. The graphs immediately show that the result is totally driven by differences in female behavior.

Step 3a. Female landowners: reduced-form specification

We begin by estimating in Model (8) a reduced-form specification on the subsample of female respondents from landholding households, mirroring the approach used in Steps 1 and 2. This specification includes *PFRindividual* as the sole explanatory variable and therefore captures the unconditional association between individual exposure to formalization and reporting behavior among women. So it is the analogue of Model 2 but with the sample restricted to female owners.

Results from this specification show a statistically significant negative coefficient on *PFRindividual*, indicating substantially lower levels of cheating among women whose land rights were formalized relative to women whose rights remained customary. Compared to the owners-only estimates in Steps 1 and 2, the magnitude of the coefficient increases and precision improves markedly even if the sample size is markedly reduced. This pattern indicates that the attenuation observed in pooled specifications is driven by the inclusion of men, for whom formalization does not generate comparable behavioral responses.

Step 3b. Female landowners: full specification with controls

When we restrict the analysis to female landowners and include the full set of observable controls (Model 9, which is analogous to Model 4, except for the sample restriction), the estimated coefficient on *PFRindividual* remains negative and statistically significant. This indicates that the association between formalization and truthful reporting among women is not explained by observable socioeconomic characteristics. In a further step (Model 10), we augment the specification by including initial conditions and lagged reporting behavior to account for persistence and unobserved heterogeneity in reporting (analogous to Model 5). In this dynamic specification, the estimated coefficient on *PFRindividual* remains negative and of comparable magnitude, but is estimated with lower precision. This attenuation reflects the inclusion of strong dynamic controls that absorb a substantial share of within-individual variation,

rather than a reversal of the underlying pattern. Taken together, the static and dynamic specifications indicate that the association between formalization and honest behavior among female landowners is robust in sign and magnitude, while being estimated with increasing uncertainty as more demanding identifying assumptions are imposed.

Step 4. Addressing residual selection with a control-function approach.

Finally, we complement the sample restrictions with a control-function (two-stage residual inclusion – 2SRI) approach that exploits the randomized village-level assignment of the PFR program. The appeal of this strategy is that any non-random selection of individuals into obtaining a PFR title within treated villages is allowed to occur, but it is purged of the part that is correlated with unobserved honesty by using random assignment as an exogenous source of variation. The 2SRI approach maintains consistency in non-linear models by including first-stage residuals as an additional control rather than replacing the endogenous variable with predicted values, thereby preserving the non-linear structure of the logit/probit likelihood while controlling for endogeneity. In the first stage, we estimate a probit model for individual formalization status (PFRindividual) as a function of randomized village treatment (PFRvillage) and the same exogenous covariates included in the outcome equation, and we compute the corresponding generalized residual (Gourieroux et al. 1987; Terza et al. 2008). In the second stage, we re-estimate the mixed-effects ordered logit model for dice reports, adding this residual (the control-function term) as an additional regressor. alongside PFRindividual and its interaction with gender. Intuitively, this residual acts as a proxy for the unobserved factors (e.g., motivation, ability, connections, or baseline honesty) that jointly affect both the likelihood of becoming formalized and reporting behavior; conditioning on it absorbs the endogenous part of selection into PFRindividual. As a result, identification comes from the randomized shift in individual PFR take-up induced by village assignment, allowing the coefficient on PFRindividual to be interpreted as a causal effect for compliers—those whose formalization status changes because their village was assigned to the program.

Table 10 in Appendix A reports the resulting second-stage estimates for the full sample (Model 11) and for the female subsample (Model 12). In both specifications, the estimated coefficient on the control-function term is statistically indistinguishable from zero, providing no evidence that selection into *PFRindividual* is correlated with unobserved determinants of reporting behavior. At the same time, the coefficients on *PFRindividual* are estimated with limited precision in this framework, which is expected given that the instrument operates at the village level and therefore provides only restricted exogenous variation at the individual level. For this reason, we treat the control-function exercise as a diagnostic check rather than our preferred estimator; the main results are therefore discussed using the baseline specifications and the sample-restriction strategy above. The female-only estimates in Table 10 follow the same pattern, again yielding an insignificant control-function term.

Taken together, these arguments indicate that while individual-level selection cannot be entirely ruled out—as is typical in field settings—the combination of institutional context, gender-specific tenure arrangements, balance checks, and

instrumental-variable evidence support a causal interpretation of the effect of property-rights formalization on honest behavior, particularly among women for whom the reform represented a genuine institutional transformation.

7 Conclusions

This paper documents three main findings. First, honest behavior is generally strong in rural Benin, with participants forgoing most of the potential gains from cheating. Second, exposure to formalized property rights is associated with increased truth-telling, but only when individuals directly experience the reform; merely residing in a treated village is not sufficient. Third, this effect is entirely driven by women, for whom formalization represents a substantive institutional change rather than a simple codification of pre-existing rights.

This pattern suggests that property-rights institutions influence moral behavior not merely through incentives or enforcement, but through their capacity to alter individuals' perceived responsibility and status within a legal and economic order. Where formalization represents a genuine institutional change—as it does for women whose customary land claims are weak or contingent—it is associated with measurable changes in moral preferences. Where it merely codifies already secure claims, its behavioral impact appears limited.

Our findings align with a broader set of studies conducted by one of the co-authors, Marco Fabbri (e.g., Dari Mattiacci and Fabbri 2023; Fabbri 2021; Fabbri and Dari Mattiacci 2021; Fabbri et al. 2021; 2024), demonstrating that premarket institutions, such as the formalization of property rights, influence social and moral preferences. Together with these findings, our research contributes to the literature highlighting a co-evolution of pro-market institutions with pro-market preferences and behaviors, a perspective recently popularized by Henrich et al. (2010) and Henrich (2020).

More broadly, our findings suggest that property-rights institutions may be associated with differences in moral behavior when they constitute a meaningful change in individuals' legal and economic position. This evidence is consistent with theoretical accounts emphasizing the interaction between institutions and preferences, while highlighting the importance of heterogeneity in exposure and institutional salience.

This study also has limitations. Honesty is measured through a self-reporting task, which, while standard in the literature, may be affected by misunderstanding or experimenter demand, particularly in low-literacy settings. Moreover, our design captures behavioral responses at a specific point in time and cannot speak to the long-run persistence of the observed effects. As with all field evidence, caution is required in extrapolating beyond the institutional and cultural context of rural Benin.

Future research could explore whether similar moral effects arise in other domains of institutional reform, whether they persist over time, and how they interact with complementary policies aimed at inclusion and empowerment. Understanding how institutions shape not only economic incentives but also moral behavior is crucial for assessing the full consequences of development interventions.

Appendix A: Supplementary analysis

A.1 Heterogeneity. Additional figures and tables

See Fig. 2 and Table 4.

A.2 Sample balance tables

See Tables 5, 6, 7, and 8.

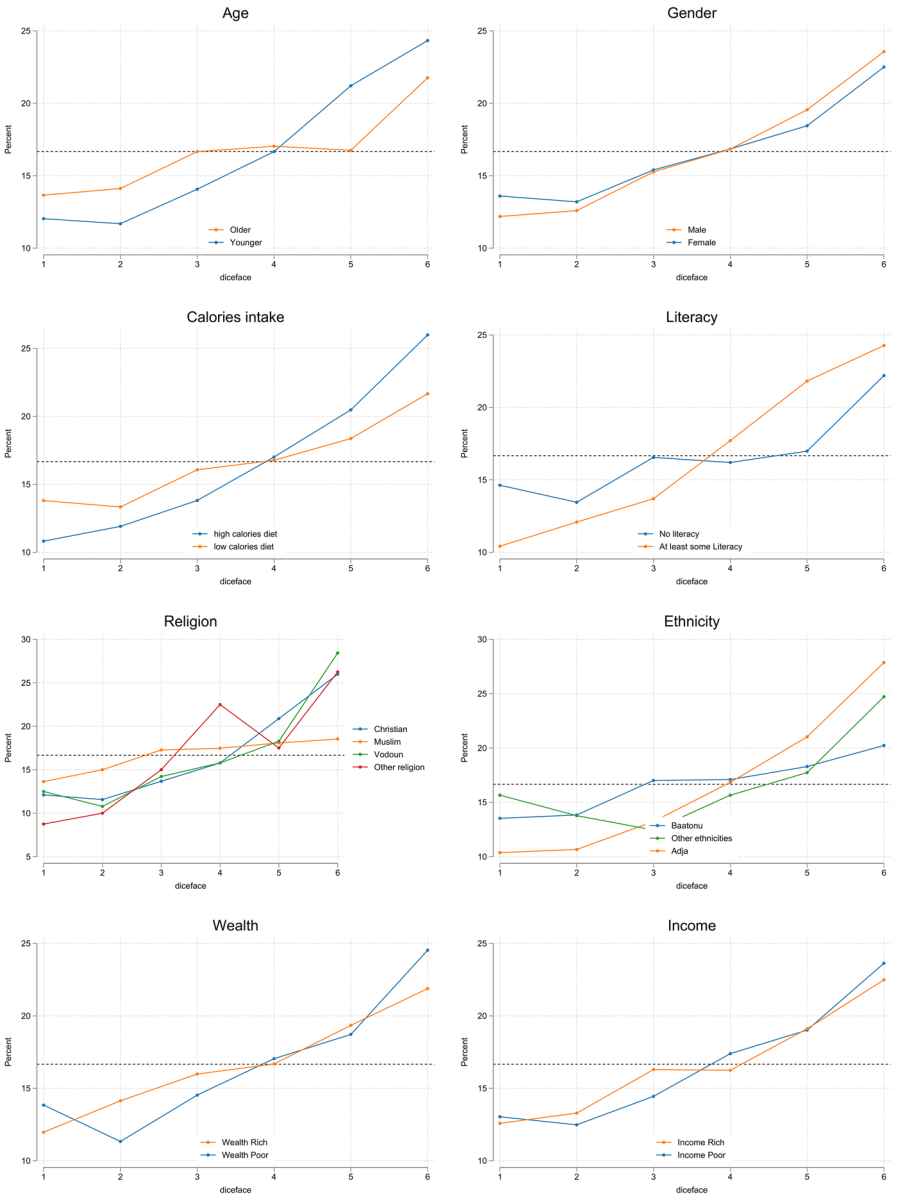


Fig. 2 Distribution of reported dice outcomes by Additional Covariates. Frequencies of reported dice outcomes for additional covariates such as age, gender, literacy, diet, wealth and income, religion and ethnicity Dashed lines indicate the expected frequencies under truthful reporting, assuming a uniform distribution. Statistical tests of deviations from truthful reporting and differences across treatment groups are reported in the text

Table 4 Differences in truthful reporting across relevant heterogeneous variables

Dimension	Comparison (reference group first)	N	Mean (ref)	Mean (alt)	z	p-value
Age	Younger (≤ 38) vs older (> 38)	4505	0.183	0.096	4.29	< 0.001
Gender	Female vs male	4505	0.125	0.158	− 1.57	0.117
Literacy	Illiterate vs literate	4505	0.098	0.204	− 4.95	< 0.001
Diet (calories)	High-calorie vs low-calorie	4505	0.208	0.111	4.47	< 0.001
Income	Low income vs high income	4505	0.151	0.135	0.86	0.391
Wealth	Low wealth vs high wealth	4505	0.157	0.132	1.43	0.154
Distance to road	Far vs near paved road	4505	0.166	0.121	2.37	0.018
Religion	Non-Muslim vs Muslim	4505	0.202	0.078	6.42	< 0.001
Ethnicity	Non-Adja vs Adja	4505	0.099	0.242	− 6.56	< 0.001
Village treatment	Control vs PFR village	4505	0.135	0.151	− 0.72	0.475
Individual PFR	No PFR land vs PFR land	4505	0.165	0.112	2.69	0.007
Individual PFR (only female owners)	No PFR land vs PFR land	1886	0.164	0.054	3.58	< 0.001

All tests are two-sided Wilcoxon rank-sum (Mann–Whitney) tests. Means are descriptive; inference relies on rank distributions. Positive z-values indicate higher truthful reporting in the reference group. "Only female owners" restricts the sample to women in land-owning households

Table 5 Sample balance across treated and control villages

	Non-PFR village (N = 244)	PFR village (N = 238)	Test
Male	0.53 (0.50)	0.55 (0.50)	0.631
Age	35.95 (11.76)	39.40 (13.58)	0.008
Muslim	0.41 (0.49)	0.48 (0.50)	0.087
Vodoun	0.17 (0.38)	0.18 (0.38)	0.900
Christian	0.43 (0.50)	0.32 (0.47)	0.009
Married	0.83 (0.38)	0.90 (0.30)	0.023
Familymembers	10.17 (6.70)	9.97 (7.15)	0.467
Managemoney	0.96 (0.19)	0.99 (0.09)	0.036
Literate	0.38 (0.49)	0.47 (0.50)	0.038
Bornvillage	0.72 (0.45)	0.72 (0.45)	0.975
Yearsvillage	29.90 (13.91)	32.32 (16.26)	0.110
Incomeweek	8763.73 (12,493.33)	9509.24 (13,675.29)	0.302
Wealth_1_1	0.59 (0.49)	0.64 (0.48)	0.273
Wealth_1_2	0.39 (0.49)	0.38 (0.49)	0.875
Wealth_1_3	0.19 (0.39)	0.27 (0.45)	0.027
Wealth_2_1	0.64 (0.48)	0.64 (0.48)	0.862
Wealth_2_2	0.07 (0.26)	0.09 (0.28)	0.561
Wealth_2_3	0.78 (0.42)	0.81 (0.40)	0.502
Wealth_2_4	0.29 (0.45)	0.33 (0.47)	0.286
Sec	4.39 (1.98)	4.47 (1.67)	0.243
Population	2923.77 (3030.53)	3129.83 (2761.28)	0.436

For continuous variables, differences are tested using the Mann–Whitney U test (non-parametric test for equality of distributions). For categorical and binary variables, differences are tested using the Pearson chi-squared test. Household wealth is measured using binary indicators of housing quality and basic services (wealth_1_1: finished floor; wealth_1_2: electricity; wealth_1_3: water), ownership of durable goods (wealth_2_1: radio/TV; wealth_2_2: car; wealth_2_3: motorcycle), and access to formal financial services (wealth_2_4: bank account or credit card)

Table 6 Sample balance across PFR and non-PFR individuals

	Non-PFR individual (N = 281)	PFR individual (N = 201)	Test
Male	0.57 (0.50)	0.51 (0.50)	0.247
Age	36.29 (12.68)	39.56 (12.75)	0.003
Muslim	0.41 (0.49)	0.49 (0.50)	0.070
Vodoun	0.17 (0.37)	0.18 (0.39)	0.632
Christian	0.42 (0.49)	0.30 (0.46)	0.007
Married	0.85 (0.36)	0.89 (0.32)	0.225
Familymembers	9.99 (6.79)	10.18 (7.11)	0.923
Managemoney	0.97 (0.17)	0.99 (0.12)	0.327
Literate	0.42 (0.49)	0.43 (0.50)	0.719
Bornvillage	0.72 (0.45)	0.71 (0.45)	0.792
Yearsvillage	30.12 (14.73)	32.45 (15.64)	0.080
Incomeweek	9316.19 (13,937.43)	8874.13 (11,811.86)	0.695
Wealth_1_1	0.59 (0.49)	0.65 (0.48)	0.201
Wealth_1_2	0.40 (0.49)	0.37 (0.48)	0.627
Wealth_1_3	0.20 (0.40)	0.27 (0.44)	0.091
Wealth_2_1	0.61 (0.49)	0.68 (0.47)	0.100
Wealth_2_2	0.07 (0.26)	0.09 (0.29)	0.557
Wealth_2_3	0.76 (0.44)	0.84 (0.37)	0.034
Wealth_2_4	0.24 (0.43)	0.40 (0.49)	< 0.001
Sec	4.33 (1.91)	4.57 (1.71)	0.062
Population	2961.92 (2899.67)	3114.43 (2904.24)	0.570

For continuous variables, differences are tested using the Mann–Whitney U test (non-parametric test for equality of distributions). For categorical and binary variables, differences are tested using the Pearson chi-squared test. Household wealth is measured using binary indicators of housing quality and basic services (wealth_1_1: finished floor; wealth_1_2: electricity; wealth_1_3: water), ownership of durable goods (wealth_2_1: radio/TV; wealth_2_2: car; wealth_2_3: motorcycle), and access to formal financial services (wealth_2_4: bank account or credit card)

Table 7 Sample balance across formal (PFR) and informal (customary) owners

	Formal PFR owners (N = 256)	Informal (customary) owners (N = 191)	Test
Male	0.57 (0.50)	0.51 (0.50)	0.164
Age	36.35 (12.76)	39.88 (12.82)	0.001
Muslim	0.43 (0.50)	0.51 (0.50)	0.096
Vodoun	0.17 (0.37)	0.17 (0.38)	0.894
Christian	0.40 (0.49)	0.30 (0.46)	0.032
Married	0.85 (0.36)	0.89 (0.31)	0.194
Familymembers	10.31 (6.91)	10.30 (7.24)	0.746
Managemoney	0.98 (0.15)	0.99 (0.10)	0.307
Literate	0.41 (0.49)	0.42 (0.50)	0.768
Bornvillage	0.73 (0.44)	0.71 (0.46)	0.521
Yearsvillage	30.33 (14.79)	32.86 (15.63)	0.073
Incomeweek	9192.77 (11,482.87)	9050.79 (11,989.40)	0.736
Wealth_1_1	0.60 (0.49)	0.68 (0.47)	0.110
Wealth_1_2	0.40 (0.49)	0.38 (0.49)	0.667
Wealth_1_3	0.21 (0.41)	0.28 (0.45)	0.103
Wealth_2_1	0.63 (0.48)	0.70 (0.46)	0.109
Wealth_2_2	0.08 (0.27)	0.09 (0.29)	0.794
Wealth_2_3	0.78 (0.43)	0.85 (0.35)	0.043
Wealth_2_4	0.25 (0.43)	0.42 (0.49)	< 0.001
Sec	4.27 (1.85)	4.53 (1.65)	0.054
Population	3034.18 (2992.22)	3197.38 (2941.17)	0.566

For continuous variables, differences are tested using the Mann–Whitney U test (non-parametric test for equality of distributions). For categorical and binary variables, differences are tested using the Pearson chi-squared test. Household wealth is measured using binary indicators of housing quality and basic services (wealth_1_1: finished floor; wealth_1_2: electricity; wealth_1_3: water), ownership of durable goods (wealth_2_1: radio/TV; wealth_2_2: car; wealth_2_3: motorcycle), and access to formal financial services (wealth_2_4: bank account or credit card)

A.3 Econometric Specification of the 1–5 Models

Turning to a more structured parametric approach, we formalize our empirical analysis within a generalized linear modeling framework (McCullagh and Nelder 1989), which is well-suited to handling continuous, discrete, and categorical outcomes in a unified regression setting. In the basic model, a monotone function of the mean known as the link function is regarded as a linear combination of known covariates; conditionally on these, the outcomes are assumed statistically independent. Nevertheless, in our empirical framework, this independence assumption is untenable. Indeed, the data at hand have a longitudinal/hierarchical structure. Subjects are playing multiple times, i.e., reported draws are nested within subjects, and one can generally test that subjects belonging to the same session are more similar than those belonging to different

Table 8 Sample Balance across women who own formal (PFR) and informal (customary) land

	Women who own PFR land (N = 109)	Women who own customary land (N = 94)	Test
Age	37.03 (12.19)	39.02 (11.61)	0.154
Muslim	0.46 (0.50)	0.50 (0.50)	0.559
Vodoun	0.10 (0.30)	0.14 (0.35)	0.413
Christian	0.41 (0.49)	0.38 (0.49)	0.667
Married	0.88 (0.33)	0.87 (0.34)	0.857
Familymembers	10.17 (6.81)	10.06 (7.15)	0.695
Managemoney	0.97 (0.16)	0.98 (0.15)	0.776
Literate	0.28 (0.45)	0.27 (0.44)	0.771
Bornvillage	0.57 (0.50)	0.53 (0.50)	0.600
Yearsvillage	28.41 (14.86)	28.40 (14.58)	0.816
Incomeweek	5661.47 (4530.68)	5831.91 (7443.32)	0.488
Wealth_1_1	0.64 (0.48)	0.68 (0.47)	0.564
Wealth_1_2	0.35 (0.48)	0.34 (0.48)	0.903
Wealth_1_3	0.15 (0.36)	0.28 (0.45)	0.023
Wealth_2_1	0.60 (0.49)	0.70 (0.46)	0.117
Wealth_2_2	0.11 (0.31)	0.10 (0.30)	0.739
Wealth_2_3	0.78 (0.44)	0.83 (0.38)	0.368
Wealth_2_4	0.29 (0.46)	0.46 (0.50)	0.016
Sec	4.15 (1.55)	4.36 (1.50)	0.195
Population	3098.26 (3082.67)	3302.13 (2968.69)	0.633

For continuous variables, differences are tested using the Mann–Whitney U test (non-parametric test for equality of distributions). For categorical and binary variables, differences are tested using the Pearson chi-squared test. Household wealth is measured using binary indicators of housing quality and basic services (wealth_1_1: finished floor; wealth_1_2: electricity; wealth_1_3: water), ownership of durable goods (wealth_2_1: radio/TV; wealth_2_2: car; wealth_2_3: motorcycle), and access to formal financial services (wealth_2_4: bank account or credit card)

sessions. Moreover, due to the longitudinal structure of the data, outcomes collected on the same subjects must be generally regarded as correlated. These data features call for the use of generalized mixed effects multilevel models (Goldstein 2011), allowing for subject and session-specific effects.

Another key aspect of our data is time dependence. This means that an individual's behavior at one point in time may be influenced by their own previous behavior. For instance, if a participant reports a high dice value once, they may be more likely to do so again in the future—either due to habit, perceived success, or some strategic reasoning. This kind of temporal association in the data can arise from two different sources:

1. True contagion: Here, the participant's past behavior directly influences their future behavior. We capture this dynamic by including autoregressive terms in our model. These terms explicitly model the dependence of current outcomes on past values.

2. Apparent contagion: In this case, the dependence is not truly behavioral but arises from unobserved individual differences. For example, some people may inherently be more prone to report higher values than others. These differences can look like time dependence, but in reality, they reflect heterogeneity across participants. Random effects in the model help us account for this type of variation.

When both autoregressive components (true contagion) and random effects (apparent contagion) are included in the model, the statistical estimation becomes more complex. This is due to what is known as the initial conditions problem (Heckman 1981; Aitkin and Alfò, 2003; Wooldridge 2005; Rabe-Hesketh and Skrondal 2013; Maruotti and Raponi 2014; Maruotti et al. 2022). In simple terms, this means that the first observed outcome for each participant is not independent of their unobserved characteristics. Failing to account for this can bias the results.

To handle this, we apply an approximate conditional likelihood approach, which simplifies the estimation by conditioning on the initial observed outcomes. This method was originally developed by Follmann and Wu (1995) and later adapted for mixed models by Rabe-Hesketh and Skrondal (2014).

In our setup, the reported dice result dicereport is a categorical ordinal variable. That means it's a variable with a fixed number of ordered categories (e.g., dice outcomes from 1 to 6). We assume that each reported outcome is a realization of a latent (unobserved) variable that reflects the level of "constraint" or honesty at time t for subject f, within session c.

To model this, we use a statistical method called a Cumulative Link Mixed Model (CLMM), as described in Agresti (2002, 2007, 2010). This model is well-suited for ordered categorical data and allows us to include both fixed effects (for the explanatory variables) and random effects (to account for individual and session-level variability). The general form of the model is:

$$G^{-1}\left[Pr\,Pr\left(y_{tfc} \le r\right)\right] = G^{-1}\left[\pi_{rtfc}\right] = \alpha_r - \sum\nolimits_{p=1}^{P} x_{tfcp}\beta_p - b_f^* - \widetilde{b_c},$$

where y_{tfc} is the reported dice outcome for subject f, in session c, at time t, $\beta = \{\beta_1, \ldots, \beta_p\}$ is the vector of the regression coefficients associated to the set of p covariates $x_{tfc} = \left(x_{tfc1}, \ldots, x_{tfcp}\right)\prime$, α_r is the threshold for category r, $r = 1, \ldots, R$ for an ordinal variable with R categories, and G^{-1} is the link function, b_f^* and $\widetilde{b_c}$ are subject- and session-specific random effects, respectively, with $b_f^* \sim N\left(0, \sigma_{b^*}^2\right)$ and $\widetilde{b_c} \sim N\left(0, \sigma_{\widetilde{b}}^2\right)$. In Table 9 we report the results of a sequence of specifications that progressively incorporate village- and individual-level treatment indicators, observed covariates, and (in the most complete model) dynamic components and initial-conditions adjustments.

A.4 Control-function approach using randomized village assignment (step 4)

To complement the institutional argument and the sample-restriction strategy, we implement a control-function approach in which individual PFR status is instrumented

Table 9 Appendix: main regression results (controls and dynamic terms reported)

	(1B) Dicereport	(2B) Dicereport	(3B) Dicereport	(4B) Dicereport	(5B) Dicereport
PFRvillage	0.079				
	(0.140)				
PFRindividual		− 0.203**	− 0.353***	− 0.321**	− 0.234*
		(0.099)	(0.133)	(0.130)	(0.135)
1.male			− 0.071	− 0.155	− 0.127
			(0.110)	(0.114)	(0.116)
1.male#c.PFRindividual			0.296*	0.353**	0.234
			(0.172)	(0.172)	(0.176)
Age				− 0.001	− 0.002
				(0.004)	(0.004)
School				− 0.251	− 0.159
				(0.173)	(0.177)
Education				0.153***	0.139***
				(0.052)	(0.053)
Married				0.040	0.063
				(0.141)	(0.143)
Polygam				− 0.022	− 0.045
				(0.092)	(0.093)
Christian				0.242**	0.147
				(0.107)	(0.110)
Vodoun				0.299**	0.179
				(0.147)	(0.154)
Otherreligion				0.133	0.034
				(0.262)	(0.269)
Marketinvillage				− 0.117	− 0.109
				(0.109)	(0.113)
Incomeweek				0.000	0.000
				(0.000)	(0.000)
Wealth_2_2				0.161	0.122
				(0.152)	(0.154)
2.init_dicereport					0.219
					(0.179)
3.init_dicereport					0.213
					(0.162)
4.init_dicereport					0.447***
					(0.162)

Table 9 (continued)

	(1B) Dicereport	(2B) Dicereport	(3B) Dicereport	(4B) Dicereport	(5B) Dicereport
5.init_dicereport					0.426***
					(0.165)
6.init_dicereport					0.630***
					(0.153)
2.lag_dicereport					0.131
					(0.117)
3.lag_dicereport					0.214*
					(0.114)
4.lag_dicereport					0.327***
					(0.114)
5.lag_dicereport					0.211*
					(0.114)
6.lag_dicereport					0.063
					(0.115)
Observations	4505	4505	4505	4505	4023

Standard errors in parentheses. Dependent variable: dicereport (ordinal). Mixed-effects ordered logit with random intercepts at the session and subject level. Model (5) includes factor dummies for initial and lagged dice report; coefficients are reported in this table. Cutpoints are omitted where applicable. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

by the randomized village-level assignment of the PFR program. While this strategy necessarily relies on limited variation—since the instrument operates at the village level—it allows us to test whether unobserved selection into *PFRindividual* is driving the results.

Within our dynamic mixed-effects ordered logit framework, we adopt a control-function (two-stage residual inclusion) approach, using the original random assignment of the PFR program at the village level as an instrument. In a first stage, we estimate a probit model for *PFRindividual* as a function of the randomized village-level treatment indicator (*PFRvillage*) and the same set of exogenous covariates used in the outcome equation. From this model, we compute a generalized residual (Gourieroux et al. 1987; Terza et al. 2008). In the second stage, we re-estimate our dynamic random-effects ordered logit model for dice reports, adding this residual as an additional regressor alongside *PFRindividual* and its interaction with gender. This control-function term captures the component of selection into *PFRindividual* that is correlated with unobserved determinants of honesty.

Table 9 reports the second-stage estimates. The coefficient on the control-function term is not statistically significant, indicating no evidence of endogeneity. The point estimate of the *PFRindividual* effect remains stable in sign but is estimated with lower precision once the control-function term is included. This loss of precision is expected because the control-function term is highly associated with *PFRindividual*,

Table 10 Control-function test for endogenous selection

	Dicereport Model (11)	Dicereport Model (12)
PFR individual	– 0.006	– 0.105
	(0.231)	(0.238)
Male	– 0.084	
	(0.117)	
PFR × male	0.225	
	(0.175)	
CF term (generalized residual)	– 0.184	– 0.122
	(0.138)	(0.161)
Sample	Full sample	Female only
Controls	Yes	Yes
Initial conditions	Yes	Yes
Lag terms	Yes	Yes
Session random effects	Yes	Yes
Subject random effects	Yes	Yes
Observations	4023	1800

Standard errors in parentheses. Second-stage estimates from mixed-effects ordered logit (meologit) including the control-function term (lambda). Lambda is constructed from a first-stage probit of PFRindividual on randomized village assignment (PFRvillage) and exogenous covariates (two-stage residual inclusion). Controls, initial-condition and lag dummies, and random effects are included but not displayed. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

and the instrument operates only at the village level, limiting the amount of independent exogenous variation available. Given no evidence of endogeneity (i.e., an insignificant control-function term), the baseline model without the control function is the more efficient estimator of the *PFRindividual* effect in this setting.

We repeat the analysis on the women subsample only, and the results are consistent with the above ones (Table 9, column 2). Details on estimation and implementation (including the computation of the generalized residual and the merge back into the roll-level panel) are available upon request / in the replication files (Table 10).

A.5 Alternative Specifications of Baseline Models

We complement our baseline specification with a set of simpler robustness checks that relax, one at a time, the assumptions embedded in our preferred mixed-effects ordered logit model. We retain an ordered logit framework throughout because it naturally matches the discrete, ordered structure of the dice-report outcome, while allowing us to assess how sensitive the estimated treatment patterns are to alternative ways of modeling dependence in the data. First, we estimate a mixed-effects ordered logit with subject random effects but no session effects (13), isolating the role of within-subject

Table 11 Robustness: different specifications

	(13) Dicereport	(14) Dicereport	(15) Dicereport	(16) Dicereport	(17) Dicereport
expPFR = 1	− 0.265**	− 0.226***	− 0.223*	− 0.226***	− 0.215***
	(0.131)	(0.087)	(0.119)	(0.087)	(0.082)
male = 1	− 0.121	− 0.107	− 0.114	− 0.107	− 0.114
	(0.118)	(0.078)	(0.104)	(0.078)	(0.074)
expPFR = 1 # male = 1	0.286	0.252**	0.218	0.252**	0.239**
	(0.176)	(0.117)	(0.156)	(0.117)	(0.110)
Estimator	ME-ologit	ME-ologit (pooled)	ME-ologit	ologit	ologit
Session random effects	No	No	Yes	No	No
Subject random effects	Yes	No	Yes	No	No
Lag terms	Yes	Yes	No	Yes	No
Initial conditions	Yes	Yes	Yes	Yes	Yes
Observations	4023	4023	4505	4023	4505

Standard errors in parentheses. All specifications include the same covariates (age, schooling/education, marital status, religion, market in village, income and wealth controls). Reduced-form coefficients reported: PFRindividual, male, and PFRindividual × male. Standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

correlation in repeated reporting while abstracting from session-level clustering. Second, we remove both session and individual random effects (14), effectively treating observations as pooled; this provides a stringent benchmark that asks whether the results survive even when we do not explicitly model unobserved heterogeneity. Third (15), we reintroduce both session and subject random effects but omit lagged outcomes. Finally, we estimate standard (non–mixed-effects) ordered logit models with (16) and without lagged outcomes (17), which further simplifies the structure and clarifies whether the core coefficients are supported by the cross-sectional variation alone. Across all these alternative specifications, the key reduced-form relationships between formalization (PFR), gender, and their interaction remain qualitatively unchanged, indicating that our conclusions are not an artifact of a particular modeling choice regarding random effects or dynamics (Table 11).

Appendix B: Instructions

Instructions are adapted from Ariely et al. (2019).

In this task, you are asked to throw a 6-faces dice 20 times. Every time, before you roll the dice, you will be asked to choose one side of the dice in your mind: top or bottom. Be sure to make your choice before you roll the dice. Then, after rolling the

dice, please enter the outcome of your roll, i.e., the number %of dots shown on the chosen side of the dice (top or bottom) on each line of this sheet.

Bear in mind that there are two possible sides to each roll: top or bottom. Here, the figure shows the different outcome combinations.

Please return your record sheet to the experimenter after you throw the dice 20 times. The experimenter will then randomly draw a number from 1 to 20 by rolling a 20-faces dice. This dice determines which of your rolls is relevant for your payment.

Once determined the relevant roll, you will be paid a number of tokens equal to the number you reported for that particular line of the sheet. For example, assume the experimenter rolls the 20 faces dice and obtains the number 3, then the number you reported on the third line determines your payment. The experimenter will check your record sheet and pay you a number of tokens equal to the number you reported on the third line.

Please discuss any questions with the experimenter before starting the task!

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Data availability The data that support the findings of this study are available from the authors upon reasonable request.

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