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Insider Collusion as a Threat to Property Rights: Experimental Evidence from West Africa*

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Abstract

We provide causal evidence on how a community's formal institutions and social structure jointly affect the value of its land to outside investors. Using field research and a lab-in-the-field experiment in rural Benin, we show that potential urban investors perceive a higher risk of expropriatory collusion among villagers—and thus invest less—when villages lack formal land records and exhibit strong social tightness. We also find that, although formalizing land rights increases the confidence of outsiders, it does not eliminate their concerns about collusion: outsiders remain wary of investing in villages with a tight social structure even with formal property rights, indicating that local collusion continues to pose a barrier to developing impersonal property markets. Our findings therefore suggest that in addition to facilitating intra-community investment and trade (e.g., by formalizing land ownership), well-designed property institutions should also guarantee the impartial treatment of outsiders.

JEL-Classification: D02; O02; Z1

Keywords: Collusion; Lab-in-the-field Experiment; Property Rights; Randomized Control Trial; Social Structure.

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1 Introduction

Property rights are the cornerstone of modern markets: by transferring control over assets, they facilitate productive investment, access to credit, and, ultimately, economic development (Demsetz, 1967, Grossman and Hart, 1986, Barzel, 1977). Legal property rights create stronger economic incentives than contractual promises because they are tied to an asset (“in rem”), and thus their value to a buyer does not depend on the seller’s personal financial liability (Merrill and Smith, 2001, Hansmann and Kraakman, 2002). For example, a creditor can seize a defaulting borrower’s mortgaged home even if the borrower has sold it to someone else and fled with the proceeds; Without the mortgage, the contractual promise of that borrower to repay the loan would be worthless to the creditor.

However, property rights cannot fulfill their promise if buyers fears that their ownership of an asset might be disputed after the purchase due to prevailing property rights over the same asset (Arruñada, 2003, Arruñada and Garoupa, 2005, Libecap and Lueck, 2011). This risk is especially relevant when buyers are outsiders with respect to the community where the asset is located: being both better informed about pre-existing rights over the asset and tied by personal relationships, community insiders may strategically collude against outside buyers (Ayotte and Bolton, 2011, Arruñada et al., 2019). For instance, a dishonest agent may sell (or mortgage) to a buyer a parcel of land that was previously sold (mortgaged) to a neighbor of the agent, who, in exchange for a kickback, may tacitly agree with this agent to enforce her hidden property rights only after the buyer has paid for the land (provided credit). If the buyer anticipates such collusion risk, she will have to incur costs to verify pre-existing property rights over the land, and may ultimately decide not to buy (provide credit).

While it is theoretically clear that collusion between insiders should reduce the value and transferability of property rights, identifying empirically this mechanism, its determinants, and its economic significance, is challenging: the risk of collusion perceived by outsiders is hard to measure directly, and the social and institutional factors that may facilitate collusion – tight bonds and linkages between insiders (hereafter, “tightness”) and the lack of formal property rights – are endogenous and may not vary enough to enable systematic empirical analysis.

This paper tackles both challenges by conducting a lab-in-the-field experiment on land transactions in Beninese villages. Benin is an ideal setting for this effort, for two reasons.

First, Beninese rural villages are characterized by a large variation in their level of ethnic concentration – a plausible proxy for how tight a village’s social structure is, and hence how easily villagers can collude against outsiders. While some villages are characterized by a single dominant ethnic group, several minority groups coexist in other villages.¹ As documented by a vast literature, kinship and ethnic ties enhance parochial biases and in-group favoritism (Buonanno and Vanin, 2017, Akbari et al., 2019, Schulz et al., 2019, Enke, 2019, Henrich, 2020), which may in turn induce village insiders to collude against outside land buyers. Moreover, ethnic ties may enhance villagers’ coordination capabilities and the probability that they will interact repeatedly with each other in the future, thus making self-enforcing collusive agreements easier to sustain.

Second, a recent institutional reform conducted by local authorities in collaboration with the World Bank – the “Plan Foncier Rural” or PFR, described in detail below – created exogenous variation in the clarity of property rights in Benin: formal property registries have been developed in the villages randomly selected for the implementation of PFR, while in the other villages property disputes are adjudicated solely based on informal evidence.

Our experiment leverages both of these characteristics of the Beninese setting. First, we test how social tightness affects outsiders’ land investment decisions, by shaping their expectation of a risk of collusion between insiders. Second, we investigate how the presence of formal property rights interacts with social tightness in determining investment behavior.

For the experiment, we recruited potential investors from Cotonou, the largest Beninese city, to simulate a land investment decision. Participants were endowed with real money and presented with the hypothetical opportunity to purchase land in an undisclosed rural village. If successful, they would triple the invested amount; otherwise, they would obtain a payoff of zero. Participants were informed that the success of their investment hinged upon the willingness of local villagers to collude in perpetrating fraud against outside investors. Specifically, we revealed that, in a preliminary phase, we had collected data in rural villages eliciting villagers’ inclination to collude against a hypothetical outsider purchasing a parcel of land in their village (as explained in detail in Section 4). Then, we explained that the probability that their potential investment turns out successful is inversely related to the likelihood of collusion among local villagers as elicited in the preliminary phase.

¹The Republic of Benin’s Official Census lists 42 ethnic groups currently populating the country.

In line with real circumstances, participants were not informed of the actual probability of collusion. Instead, they were given specific village characteristics, enabling them to form their own beliefs about collusion risk. Our treatment manipulation varied precisely these characteristics: whether the village population was ethnically homogeneous or diverse (social tightness), and whether it had undergone formal property rights registration (the PFR program). Since these village characteristics were randomly assigned to potential investors, this design allows us to causally identify how social tightness and formal land rights affect their investment decisions.

The results indicate that outsiders are significantly less inclined to invest when the village has a single dominant ethnic group compared to villages marked by ethnic diversity. Moreover, our findings confirm that potential investors exhibit a significantly higher propensity to invest when a village has formal land rights.

At the same time, we also find that formal land rights are not sufficient to remove the expectation of insiders' collusion created by social tightness: the negative effects of social tightness on investments persists also when land rights have been formalized in the village. In line with this finding, a substantial share of urban investors who participated in a qualitative survey we conducted believe that while useful, the PFR reform does not fully protect outside investors against collusion among villagers. A hypothesis consistent with these findings, which should be investigated in future research, is that outside investors may be worried about the integrity and impartiality of the local agents managing the registries and the customary judges in charge of resolving property disputes between villagers and outsiders.

Our paper contributes to the economic literature on property rights (Grossman and Hart, 1986, De Soto, 2000, Acemoglu et al., 2001), and particularly to the strand of this literature that examines the effects of formal property institutions on economic outcomes. Extensive investigations have examined land rights formalization initiatives in developing countries, showing that on the one hand, these policy efforts enhance access to credit and investment but on the other hand, they are often limited by implementation challenges and local conditions (Bruce et al., Deininger and Feder, 2009). In particular, several studies in this literature have applied quasi-experimental designs – from differences-in-differences to regression discontinuity and instrumental variables – to disentangle the effects of property formalization from confounding factors.

Thus, Field (2007) identifies increased labor supply in urban Peru after property titling, as

families no longer needed to guard their homes. Galiani and Schargrodsy (2010) demonstrate that land titling in Argentina increased housing and offspring’s education investments but had limited effects on credit markets. Jacoby and Minten (2007) provides evidence that formal titling had little if any effect on land investment, productivity and value in Madagascar. D’Arcy et al. (2024) show some linkage between the development of cadasters and income levels. Given the big-stakes nature of land formalization, there have been limited opportunities to perform Randomized Controlled Trials. Two exceptions are Goldstein et al. (2018), who report that land demarcation of parcels’ boundaries improved long-term land investments by female-headed owners; and Arruñada et al. (2022), who provide evidence that titling effectiveness depends on the clarity of the legal framework, as ambiguous titling systems may lead to greater litigation.²

Our paper contributes to this literature by identifying an understudied mechanism through which formal property rights facilitate investment and market transactions – namely, by reducing the risk of collusion among community insiders against outside market participants. Relatedly, our paper shows that besides formal property rights, a community’s internal social structure importantly affects collusion risks and outsiders’ willingness to invest. Understanding the distinctive formal and informal institutional obstacles to outside investment is important because outside investments are crucial for the development and growth of poor rural communities, which often lack the necessary capital.

We also contribute to the economic literature on the interplay between personal relationships and impersonal markets. A strand of this literature emphasizes how the lack of long-term bilateral relationships may hamper trade by exposing market participants to agency problems, and explores institutional solutions that substitute for relationships by coordinating the collective punishment of defectors (Milgrom et al., 1990, Greif, 1993, Dixit, 2004, Masten and

²A related strand of research, initiated by Marco Fabbri in 2017, examines the consequences of the 2009–2011 Beninese property rights reform for social preferences and norms. Experimental evidence demonstrates that the formalization of property rights enhances individuals’ propensity to respect others’ property (Fabbri and Dari-Mattiacci, 2021). The reform is further shown to have increased levels of in-group cooperation and trust, but only in villages connected by paved roads, which facilitated access to the newly established institutions and government services (Fabbri, 2021). In addition, Fabbri et al. (2025a) replicate the results of Fabbri and Dari-Mattiacci (2021) with an independent sample and confirm their validity in the context of out-group interactions. Consistently, Fabbri (2022) provide evidence that the formalization of property rights strengthens villagers’ cooperation with out-group strangers. However, a more recent data collection reported in Fabbri et al. (2025b) find that villages treated with the reform do not display more trust or trustworthiness towards out-groups compared to control villages. Finally, Dari-Mattiacci and Fabbri (2023) report the findings of a vignette experiment based on the trolley dilemma, showing that the formalization of property rights increases the likelihood of adopting consequentialist rather than deontological choices.

Prufer, 1990). Kranton (1996) argues that the existence of strong personal relationships may actually crowd out impersonal markets and reduce economic efficiency. In her model, crowding out occurs because relationships make spot markets thin and raise buyers’ search costs. Our experimental study documents a different and novel crowding-out channel: when the object of exchanges are property rights over fixed assets, personal relationships between local insiders may facilitate opportunistic collusion that discourages outsiders from entering market transactions with them. Our experiment also shows that by reducing collusion risks, property formalization institutions can serve as a mechanism to enable and preserve impersonal markets (Arruñada, 2012, Fabbri, 2022).

The remainder of this paper is organized as follows. The next section presents a simple model of collusion in property transactions, which guides the development of our experimental hypotheses. Section 3 describes the institutional setting of our study. Sections 4 and 5 discuss, respectively, the experimental design and the empirical strategy. Results are presented in Section 6. Section 7 concludes.

2 Theoretical framework

In this section we present a simple model of property transactions between insiders and outsiders, which captures the key features of our empirical setting. Since our purpose is to illustrate the mechanisms behind our experimental hypotheses, and not to produce novel theoretical insights, we keep complexity at a minimum. For a richer theory this model draws upon, see Arruñada et al. (2019).

2.1 Setting

There is a parcel of land located in a village, and there are three players – a seller (S , he) and his neighbor (N , he), both living in the village, and an outside buyer (B , she), who lives in the city and is interested in purchasing the land. All players are risk-neutral.

The game is one-shot and proceeds as follows. At stage 1, S offers to sell the asset to B at some price t . If B rejects S ’s offer the game ends, in which case B and N obtain a payoff of zero while S obtains a payoff of v_S (her valuation of the land). If B accepts to buy the land and pays the price, at stage 2 S may ask N to “collude” against B – that is, to claim to be the

land's owner, sue to reclaim it from B , and split the expected gains with S through a transfer q .³ If N refuses to collude, the game ends and B obtains a payoff of $v_B - t$ (where v_B is B 's valuation of the land), S obtains a payoff of t , and N obtains a payoff of zero. If N accepts to collude, S obtains a payoff of $t + q$, while N 's and B 's payoffs are determined at the subsequent litigation stage. At stage 3, when N sues to reclaim the land from B , with probability $1 - \tau$ the court rules for B and gives her the land. In that case, B 's payoff is $v_B - t$ (as in the absence of collusion), and N 's payoff is $\lambda - f - q$, where λ and f , the private benefit and cost of collusion, will be defined momentarily. With probability τ , however, the court rules for N and gives him the land. In that case, B obtains a payoff of $-t$, and N obtains a payoff of $v_S + \lambda - f - q$ (we assume for simplicity that the two insiders, S and N , have the same valuation of the land – for instance, because they have access to the same technology).

Four parameters in the model drive the extent and effectiveness of collusion, and hence B 's willingness to pay for the land: (1) the insiders' valuation of the land (v_S), (2) N 's private benefit from colluding (λ), (2) N 's cost of colluding (f), and (3) the likelihood that collusion succeeds in court (τ). Consistent with our empirical setting, we interpret λ as the strength of social tightness in the village, which determines N 's willingness to help his neighbor S . We interpret f as the opportunity cost of the time and effort N must spend to execute the collusive scheme. Lastly, we interpret $1 - \tau$ as the quality of formal property rights in the village, such that the lower τ , the more likely it is that the court has objective evidence that S (and not N) owned the land at the time of the sale. For instance, τ may be large if a property registry is absent or dysfunctional (Arruñada and Garoupa (2005), Arruñada (2012)) or if boundaries between parcels of land are not clearly demarcated (Libecap and Lueck (2011), Arruñada (2018)).

We assume the village's institutional features, λ and τ , are observed by the outside buyer B . For simplicity, we assume that B can also observe the insiders' valuation v_S (for instance, because she knows the village's production technology). In contrast, B cannot observe whether collusion occurs and the cost of collusion f , which depends on N 's private circumstances: all B knows is the cumulative probability distribution function of f , which is given by $G(f)$, with $G' > 0$.

³The specific timing of collusion is not important: identical results would obtain if S and N negotiated their collusive agreement before B buys the land. What matters, as discussed below, is that B cannot observe collusion.

2.2 Discussion of the assumptions

Two assumptions implicit in our definition of the payoffs deserve further discussion. First, we assume the collusive agreement between N and S is self-enforcing: N is not tempted to renege on the transfer q – that is, to keep the land without splitting its value with S . While this assumption fits the collusive situation in our experimental exercise below, it is not necessary for the model’s results: N can attempt to reclaim the land from B even unilaterally and as we shall see, B ’s willingness to buy does not depend on how the land’s value is split between N and S .

Our second assumption is that if the court gives the land to N , B cannot recover from S the price that she paid for it. Specifically, we assume that S is “judgment-proof,” in the sense that he has no other property that can be seized by B , and can consume or hide any cash he has (including the price paid by B for the land), or simply flee the village, before a court may order him to compensate B . This assumption is consistent with the characters in our empirical setting – villagers in a developing country, whose personal wealth is likely to be low relative to the value of their land. While assuming that S is completely judgment-proof is clearly a simplification, it is easy to show that the model’s results would continue to hold if we allowed B to recover part but not all of the land’s price from S (see the appendix in Arruñada et al. (2019) for an analysis of this case).

2.3 Determinants of the buyer’s willingness to pay

To determine B ’s willingness to pay for the land, we proceed by backward induction. At stage 2 of the game, S and N collude if they expect a positive joint surplus from doing so, that is, if $\tau v_S + \lambda - f > 0$. If this condition holds, there will be values of the transfer q such that both neighbors are better off colluding.⁴ Anticipating that, when B receives S ’s sale offer at stage 1, she expects collusion to occur with probability

$$p(\lambda, \tau) = \text{Prob}(f < \lambda + \tau v_S) = G(\lambda + \tau v_S), \quad (1)$$

which increases in both λ and τ . Accordingly, B accepts S ’s offer if her expected benefit from

⁴To formally see this, notice that N wants to collude if $\tau v_S + \lambda - f - q \geq 0$ while S wants to collude if $t + q \geq t$.

buying the land exceeds the price, that is:

$$v_B[(1 - p(\lambda, \tau) + (1 - \tau)p(\lambda, \tau)] \geq t, \quad (2)$$

where $1 - p(\lambda, \tau)$ is the probability that N does not collude with S at stage 2, while $(1 - \tau)p(\lambda, \tau)$ is the probability that the court gives the land to B at stage 3 in the event of collusion. Rearranging condition (2) we obtain B 's maximum willingness to pay for the land:

$$t^* = v_B(1 - \tau p(\lambda, \tau)). \quad (3)$$

In words, B 's willingness to pay equals her valuation of the land, v_B , times the probability $1 - \tau p(\lambda, \tau)$ that B gets to keep the land after paying for it. The following testable prediction immediately follows from (3).

Proposition 1. *An outside buyer's willingness to pay for land located in a village (i) decreases in the tightness of social ties between villagers (λ), and (ii) increases in the quality of formal property rights in the village ($1 - \tau$).*

3 Empirical Setting: Social Structure and Property Rights in Benin

The Republic of Benin is an ideal setting for our research because it exhibits variation that we can leverage in our experiment in the two determinants of collusion we are interested in – namely, the extent of social tightness, proxied by ethnic concentration (λ in the model), and the extent to which property rights are formalized (τ).

Regarding social tightness, Benin encompasses 42 different distinct ethnic groups.⁵ In rural regions, villages vary from being ethnically homogeneous, with nearly all inhabitants belonging to a single ethnic group, to exhibiting extensive ethnic diversity. In developing our experiment, we capitalize on this variation in ethnic concentration across otherwise similar rural villages, as

⁵See for instance the “Background Note: Benin”. U.S. State Department. December 2001. Accessed on August 30, 2025, at <https://web.archive.org/web/20020808174502/http://www.state.gov/r/pa/ei/bgn/6761.htm>

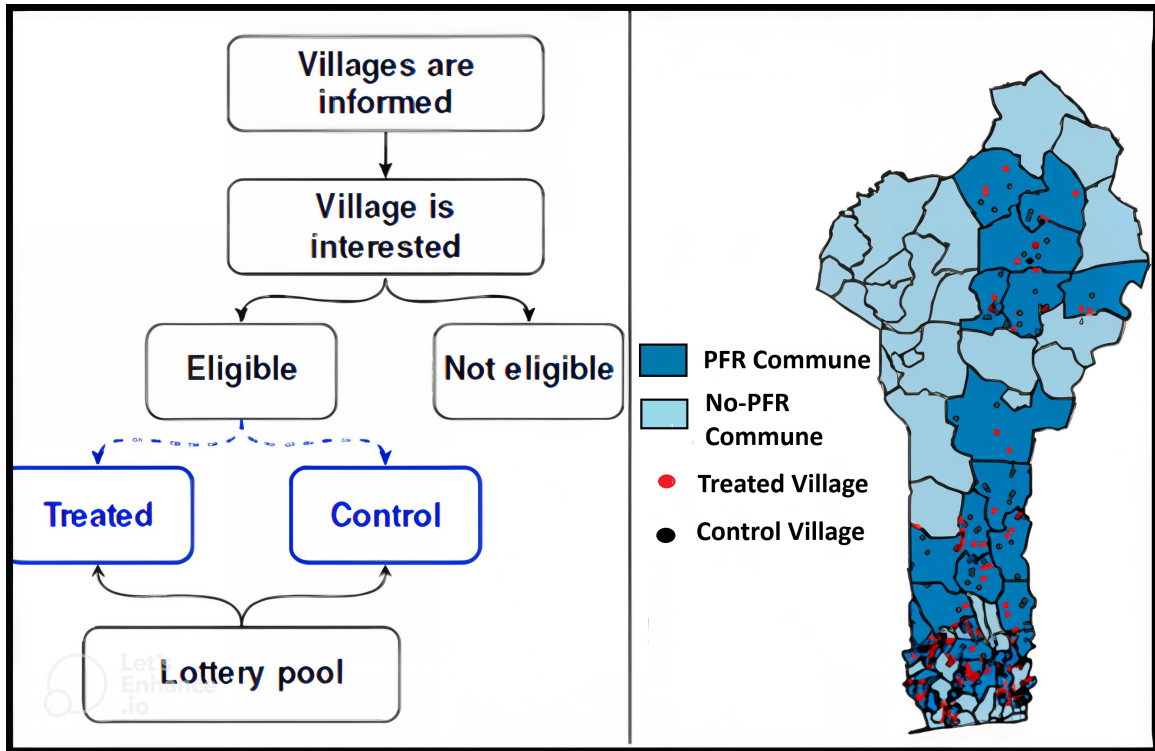


Figure 1: *Left panel:* The lottery mechanism used to select villages where the reform was implemented. *Right panel:* The distribution of treated and control villages in Benin. *Source:* Fabbri (2021)

discussed in section 4.

Regarding property rights, in rural Benin (as in most of the African continent), are predominantly governed by customary land tenure systems characterized by informal collective property arrangements (Boone, 2014, Wily, 2003). However, in an effort to enhance access to land property, tenure security, and the development of a land market, in 1993 the Beninese government introduced a systematic approach for identifying and registering customary rights to agricultural land, known as the “Plan Foncier Rural” (hereafter, PFR). Due to resource constraints, PFR was effectively implemented between 2009 and 2011 through a five-year program funded by the Millennium Challenge Account and carried out in collaboration with the World Bank.

The PFR entails land surveys conducted at the village level to identify the holders of rights over a given parcel of land and the nature of their rights, and to demarcate the boundaries between different parcels. The PFR allows for public challenges to the proposed property

records as resulting from the survey, and requires public endorsement of these records by rights holders and neighbors (Delville, 2006, 2020). Subsequently, PFR-generated maps outlining parcel boundaries are produced and archived for public access, and rights holders are offered the option to request formal certificates of land registration.

Although these certificates do not directly confer full legal ownership, they allow villagers to acquire it through a faster and more cost-effective process compared to unregistered plots. Moreover, certificates confer a presumption of ownership in court, and enable the sale of registered plots or their use as collateral in financial transactions (Fabbri and Dari-Mattiacci, 2021). Overall, PFR significantly strengthened the clarity of property rights over land in Beninese villages, thus decreasing the probability that an outside buyer might see her property rights successfully disputed in court after the purchase. This was an important reform given that land represents the primary asset for most rural villagers (Goldstein et al., 2018).

Importantly for the purposes of our study, the PFR represents the first – and thus far, only – instance of a large-scale institutional reform implemented as a randomized control trial (RCT). The implementation, outlined in the left panel of Figure 1, proceeded as follows. First, 576 eligible villages expressing willingness to undergo the reform were identified. Second, a subset of 300 villages was randomly selected through a series of public lotteries to receive the PFR intervention. The remaining eligible villages maintained the pre-PFR legal regime and, to date, continue to operate under customary land rights.⁶ Previous studies indicate that the two groups of villages had similar characteristics and socio-economic observables before the PFR treatment (Arruñada et al., 2022, Goldstein et al., 2016, Fabbri and Dari-Mattiacci, 2021), and that migration between villages is not a significant concern (Fabbri, 2021). In our experimental design, described in detail in the next section, we exploit both the historical diversity of ethnic concentration across villages and the random variation in property rights formality generated by the PFR to evaluate the effect of collusion on the market for property rights.

⁶In recent years, a few control villages located in Borgou, one of the twelve departments of Benin, were exposed to an intervention implemented by the German international cooperation agency, which aimed at formalizing land rights in a manner similar to that described above for the treated villages. This occurrence does not compromise our research design, as will become clear in the detailed discussion provided in the following section.

4 The Experiment

To estimate how formal property rights and social tightness affect collusion between villagers and the market for village land, we conducted a lab-in-the-field experiment involving an incentivized investment decision by residents of Cotonou, the largest Beninese city (hereafter, the “outsiders”). The experimental design, hypotheses and empirical specifications were pre-registered in the Registry AsPredicted.⁷ The experiment was designed to determine the outsiders’ investment in a given village (computed as the share of their initial monetary endowment invested in land located in such village) as a function of the outsider’s (experimentally manipulated) information on collusion determinants (social tightness and property rights formality) in the village.

Our research assistants recruited 397 participants residing in Cotonou to partake in the experiment.⁸ Each participant was recruited individually. On a daily basis, research assistants were assigned a neighborhood in Cotonou by the local research supervisor and instructed to approach business owners, employees, and local vendors to inquire whether they would be willing to take part in a short incentivized task. The instructions were delivered by the experimenter in isolation, and each participant privately reported her investment decision. The sessions were conducted in a private setting, typically the office or shop of the respondent. Appendix C reports an English translation of the original French experimental instructions.

At the outset, participants were informed that additional tasks would follow, though no further details were provided. The specifics of the second and final task were revealed only after participants had completed the first. At the beginning of the first task, the experimenter provided each participant with an endowment of ten coins worth 50 West African CFA francs (hereafter, CFA) each, and explained to her that she could freely choose how many of these coins to invest in the hypothetical purchase of a parcel of land in an unnamed rural village (0, 1, 2, ..., 10). The uninvested coins would be sure earnings. In contrast, the invested coins

⁷Pre-registration nr.153364, accessible at <https://aspredicted.org/kt823.pdf>.

⁸Our target was to recruit 400 participants. However, by the time we started the data collection in Cotonou, we only had enough funds to recruit half of this targeted sample. Thus, as specified in the pre-registration, we began in November 2023 by collecting data on 203 participants and we committed to recruit the remaining participants as soon as additional funds would be available, which happened in April 2024. In the analysis reported below, we include a dummy equal to one for the participants recruited in the second wave. After completing the experiment and survey, the 194 participants in the second wave of data collection also replied to three open-ended questions regarding the importance of tightness and formalized property rights for land investment security, as described in section 6.

would be tripled in the event of a successful investment, and lost otherwise. The experimenter then explained to each participant the process determining the success of her investment.

First, we informed the participants that, in a preliminary step completed before coming to the meeting, we had visited several Beninese rural villages. In each village, we described to interviewed residents a hypothetical scenario in which an outside investor purchases land there. We then asked them to estimate the likelihood that locals would collude to deceive the investor and expropriate his land.⁹ These responses were used to calculate the village's average expected collusion rate, which in turn was used to determine the probability of success for a hypothetical investment in that village. Specifically, we informed participants that this probability was equal to the complement of the average expected collusion rate. However, we did not disclose the actual value of this probability to participants at the time they made their investment decision.

Instead, participants were given a description of the unnamed rural village from which the corresponding data had been collected and that was assigned to them for the investment decision. The description included a set of fixed village characteristics, such as the fact that “the village has less than 400 households” and “is located less than 13km from the closest paved road”, as well as two specific pieces of information that we varied systematically across four treatment arms.¹⁰ Specifically, our treatment manipulation involved randomly selecting and describing to the participant one of out of four rural villages where we had collected data, that varied along two dimensions: whether the village has High or Low social tightness, and whether it has formal property rights (PFR) or not (No-PFR).¹¹ Having received the information about

⁹The situation described to village residents was as follows: “A man in your village owns a parcel of land and sells it to another person who lives in the city. After the payment has been made, the man tries to trick the person from the city: he asks one of his neighbors to falsely claim that the neighbor is the actual legal owner of the land, and so the man could not have sold the land and the person from the city cannot keep it. The neighbor accepts the man's request and makes the false claim about the land's ownership.” Residents then were asked the following question: “[O]ut of 10 people in your village, how many in this situation would agree to falsely testify?”. In Appendix A, we describe and report details concerning the preliminary step.

¹⁰All information provided to participants was truthful.

¹¹The precise wording for High-tightness villages was “The village is ethnically homogeneous compared to the Benin average, with over 90% of households belonging to the same dominant ethnic group.” The wording for Low-tightness villages was “The village is ethnically fragmented compared to the Benin average: only 50% of households belong to the same dominant ethnic group, while the rest belong to other groups.” The wording for PFR villages was “In 2011, the government implemented the Rural Land Plan in the village, so that the titles and owners of every plot of land in the village are officially recorded in a publicly accessible register that is maintained and updated by the village authorities.” Lastly, the wording for NO-PFR villages was: “The government has not implemented the Rural Land Plan. As a result, villagers have no formal land titles, and access, use and management of land are governed by customs and traditional law.”

the village in which their hypothetical investment would take place, participants decided how many of their ten coins to invest or keep.

After completing the first investment decision, participants were asked to make a second investment decision. The setting and payoffs were identical to the first investment decision, except that the village treatment selected for this second decision was different, and accordingly, the village described to the investor had a different combination of ethnic concentration and property rights formalization. It was specified this would be the last decision of the experiment. We randomized the treatment and order of appearance across subjects.

Following the two investment decisions, participants were asked to complete a brief socio-demographic survey and, for participants taking part to the second wave of data collection, three open-ended questions on the motivations behind their investments' decisions. Finally, we randomly selected one of the two decisions to determine the condition relevant to participants' payment. To determine whether the investment in that condition succeeded, they drew a ball from an opaque bag containing ten balls of either white or black color. Participants knew that the proportion of black balls in the bag was equal to the expected collusion rate in the corresponding village.¹² If the drawn ball was white, the participant's final payment equaled the number of uninvested coins plus three times the number of coins invested. If instead the ball was black, their payment consisted solely of the uninvested coins.

5 Empirical Strategy

5.1 Hypotheses

Our theoretical model predicts that the more a seller of land and his neighbor benefit from helping each other (i.e., the greater the parameter λ), and the easier it is for the neighbor to convince a court that he – and not the buyer – owns the land (i.e., the greater the parameter τ), the higher the buyer's risk to be a victim of collusion, and hence the lower her willingness to pay for the land. By adapting these predictions to our empirical setting, we formulated and pre-registered two directional hypotheses.

¹²Based on the data collected in the preliminary stage, the expected and undisclosed collusion rate was as follows: 40% in the High-tightness & PFR village, 40% in the Low-tightness & PFR village, 30% in the High-tightness & NO-PFR village, and 30% in the Low-tightness & NO-PFR village. These probabilities were deliberately withheld from the potential investors, who remained unaware of them throughout and even after the experiment; they only observed the outcome of the ball draw.

In developing the first hypothesis, we argue that a tight intra-village social structure, characterized by deep ethnic concentration between inhabitants, increases the villagers’ willingness to help each other in a collusive scheme against an urban land buyer, and thus the buyer’s perceived risk of losing the land after purchasing it. All else being equal, this will reduce the number of coins the buyer is willing to invest to buy land in the village.

Hypothesis 1 (Tightness Reduces Investment): *Urban buyers will invest fewer coins to buy land in ethnically homogeneous villages (High social tightness) compared to more ethnically diverse villages (Low social tightness).*

In developing our second hypothesis, we rely on previous studies of the PFR. These studies have shown that while PFR registration certificates do not directly confer ownership over land, they are perceived by rural villagers as a *de facto* proof of ownership that suffices to secure their title (Fabbri and Dari-Mattiacci, 2021). Indeed, there is evidence that PFR registries are used by local customary judges, and even state courts, as evidence of ownership (Arruñada et al., 2022). Based on these features of the PFR, we hypothesize that urban buyers believe their property rights over land are more strongly protected from collusion in the PFR villages, and thus are willing to invest more of their coins to buy land in those villages, relative to the No-PFR ones.

Hypothesis 2 (Titling Enhances Investment): *Urban buyers will invest more coins to buy land in the villages where the PFR reform was implemented, compared to the villages where property rights over land remain unregistered.*

5.2 Regression Specification

We test our two hypotheses by estimating the following regression equation:

$$c_i = \alpha + \delta_T T_i + \delta_P P_i + \mathbf{x}_i + \epsilon_i \quad (4)$$

where c_i is the number of coins invested by urban resident i to buy land in the experimentally assigned village, T_i is a dummy equal to one if the village has high social tightness, P_i is a dummy equal to one if the village received the PFR land registration reform, and \mathbf{x}_i is a set of

investor-specific controls from the socio-demographic survey. The controls include gender, age, occupation, ethnicity, whether the investor is literate, education, income, and an experimentally-validated survey measure of ambiguity preferences (Cavatorta and Schröder, 2019).

6 Results

6.1 Testing the Hypotheses

Table 1: Covariate Balance Across Treatment Arms

Covariate	Tight & PFR	Tight & NO-PFR	Loose & PFR	Loose & NO-PFR	F-stat	p-value
male	0.699	0.657	0.693	0.657	0.503	0.681
age	37.648	36.477	37.551	36.537	0.823	0.482
literate	0.827	0.738	0.800	0.721	2.847	0.037
education	3.607	2.919	3.613	3.025	4.902	0.002
income-mth	41672	28019	45547	34326	2.008	0.112
eth_fon	0.439	0.453	0.431	0.443	0.067	0.977
eth_adja	0.133	0.151	0.156	0.174	0.439	0.725
eth_yoru	0.148	0.186	0.138	0.169	0.690	0.559
eth_dendi	0.041	0.035	0.040	0.035	0.104	0.958
eth_other	0.194	0.151	0.196	0.159	0.715	0.543
job_craft	0.168	0.192	0.191	0.214	0.447	0.720
job_commerce	0.342	0.424	0.316	0.383	1.941	0.122
job_admin	0.041	0.029	0.044	0.035	0.224	0.880
job_trans	0.194	0.134	0.173	0.119	1.753	0.156
job_other	0.250	0.221	0.262	0.239	0.317	0.813
secondwave	0.490	0.453	0.516	0.488	0.501	0.682
ambiguity_avx	2.180	2.333	2.154	2.281	0.454	0.715

Table 1 presents descriptive statistics for experiment participants who made investment decisions in the different treatments. We assessed covariates balance across treatment arms by regressing each baseline characteristic on treatment indicators and testing the joint significance of the treatment coefficients. Standard errors were clustered at the participant level to account for repeated observations. Overall, covariates are well-balanced across treatment arms, with the exception of education levels that display some significant imbalance.

Before presenting our regression analyses, we compare the amount of coins invested by urban investors across the different treatments, as depicted in Figure 2. The left panel compares

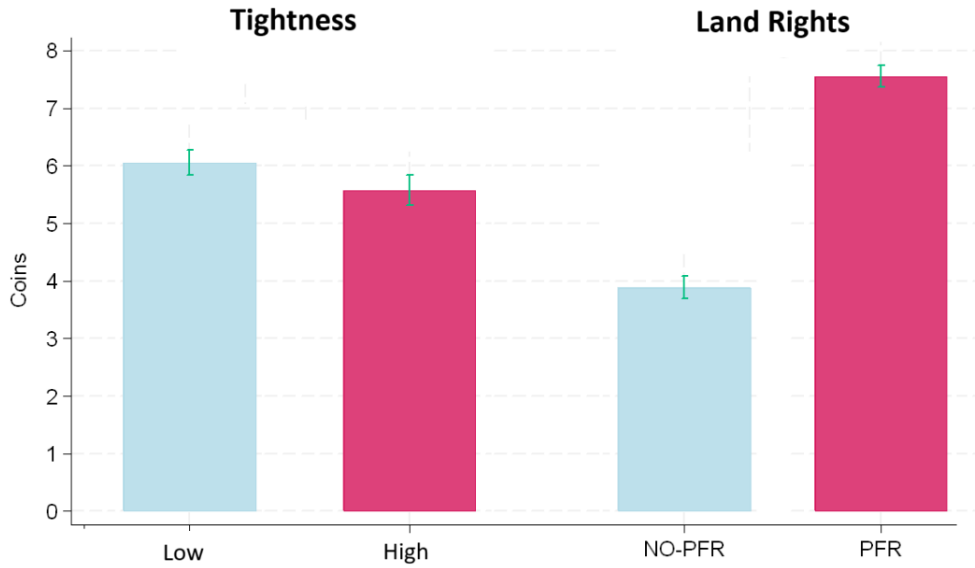


Figure 2: Coins invested by the investors, separated by treatment.

investments in villages described as ethnically diverse (Loose) to those in ethnically homogeneous villages (Tight). Consistent with Hypothesis 1, experiment participants invest more, on average, when the village has low social tightness (6.05 coins) than when it has high tightness (5.58 coins). To test for mean differences between the two conditions, we estimated a linear mixed-effects model in which the outcome variable was regressed on a fixed effect for condition. Participant ID was included as a random intercept to account for within-subject correlation arising from repeated measurements. This specification appropriately accommodates participants with either one or two observations. Statistical significance of the mean difference was assessed using a joint test of the fixed effect coefficient. Standard errors were estimated accounting for the clustering at the participant level. The difference is statistically significant ($p < .01$, one-sided t-test).

The right panel compares investments in villages that did not receive the PFR land registration reform (No-PFR) to those in villages that did receive the reform. Consistent with Hypothesis 2, participants invest substantially more in PFR villages (7.56 vs. 3.89 coins), the difference being statistically significant repeating the same test as above ($p < .01$, one-sided t-test).

We estimate the pre-registered regression (4) via a Generalized Least Squares model with random effects, using the participant as the unit of observation. All model specifications control

Table 2: Coins Invested by the Urban Residents

	Model 1	Model 2	Model 3	Model 4
high-tightness	-0.483*** (0.149)	-0.485*** (0.150)	-0.478*** (0.149)	-0.463*** (0.150)
PFR	3.608*** (0.163)	3.627*** (0.164)	3.605*** (0.165)	3.567*** (0.171)
male		-0.136 (0.187)	-0.124 (0.185)	-0.136 (0.213)
age		-0.003 (0.011)	-0.003 (0.010)	-0.005 (0.010)
ambiguity_avx			-0.199*** (0.056)	-0.186*** (0.057)
literate				-0.101 (0.311)
income(k)				0.021 (0.014)
d_order	Y	Y	Y	Y
d_wave	Y	Y	Y	Y
d_ethnicities	N	Y	Y	Y
d_occupations	N	N	N	Y
d_education	N	N	N	Y
Constant	4.796*** (0.292)	4.911*** (0.479)	5.359*** (0.493)	5.830*** (0.571)
N.obs.	794	794	794	794

Notes: Dependent variable: coins invested by the urban resident investors. GLS regression with random effects. Robust standard errors clustered at the subject level. Compared to model 1, model 2 additionally controls for time-invariant characteristics: gender, age, ethnicity. Model 3 adds controls for ambiguity preferences. Model 4 adds controls for literacy, education level, income, and occupation. All models control for the order of the decisions and for the data collection wave. Symbols ***, **, and * indicate significance at the 1%, 5% and 10% level, respectively.

for the order in which the choices were presented to the investor and for the data collection wave. The results are presented in Table 2. In model 1, which does not include additional controls, the dummy for high social tightness in the village is negative and significant, suggesting that participants invest roughly half a coin (or 10%) less when facing a village with tight social structure, compared to when they face a village with low social tightness. The dummy for PFR villages is also positive and significant, indicating that a village in which the PFR reform was implemented attracts approximately three additional coins of investment (a 75% increase) compared to No-PFR villages.

In model 2, we include time-invariant investor characteristics (age, gender, ethnicity) in the regression. Model 3 additionally controls for investors' ambiguity preferences elicited using an

experimentally-validated survey module (Cavatorta and Schröder, 2019). Finally, in model 4, we add controls for investors’ literacy, education, income, and job occupation. The estimated effects of social tightness and formal property rights in these models are qualitatively consistent with those of model 1.

We conduct two additional exercises to ensure robustness of our test above. First, we re-estimate the basic regression specification using a post-double selection Lasso model and let the unsupervised algorithm select the controls to be included as regressors (Belloni et al., 2016). Results are reported in model 1 of Table A1 in Appendix B. The qualitative results remain the same and point estimates are very similar compared to our pre-registered specification. In models 2-4 in the same table, we replicate the Lasso selection procedure but additionally imposing as controls either the selection of time-invariant characteristics (model 2), or education and income levels (model 3), or a control for the order of the decisions (model 4). Point estimates remain very similar and qualitative results are unchanged.

Second, we replicate the regression analysis by considering only the first investment decision taken by participants (thus dropping the second decisions). Results are reported in Table A2 in Appendix B. The estimated coefficients of our variables of interest (the PFR and Tight dummies) are qualitatively the same and quantitatively similar to those in the pre-registered regression.

6.2 Exploratory Analysis of Moderating Effects

In this section we investigate whether social tightness in villages and the presence of formal property rights moderate each other as determinants of urban buyers’ willingness to invest in the purchase of land. We did not pre-register a directional hypothesis on moderating effects because it is not theoretically clear what such effects one should expect: in the model presented in section 2, the rate at which the probability of collusion increases in λ may increase or decrease in τ , depending on the assumptions one makes on the cumulative distribution function of f , the cost of collusion. However, our experimental treatments allow us to explore interactions between social tightness and property rights formalization, and we report the results of this exploration below.

In the left panel of Figure 3, we compare investors’ decisions in NO-PFR and PFR villages, distinguishing within each property rights regime between treatment villages with low social

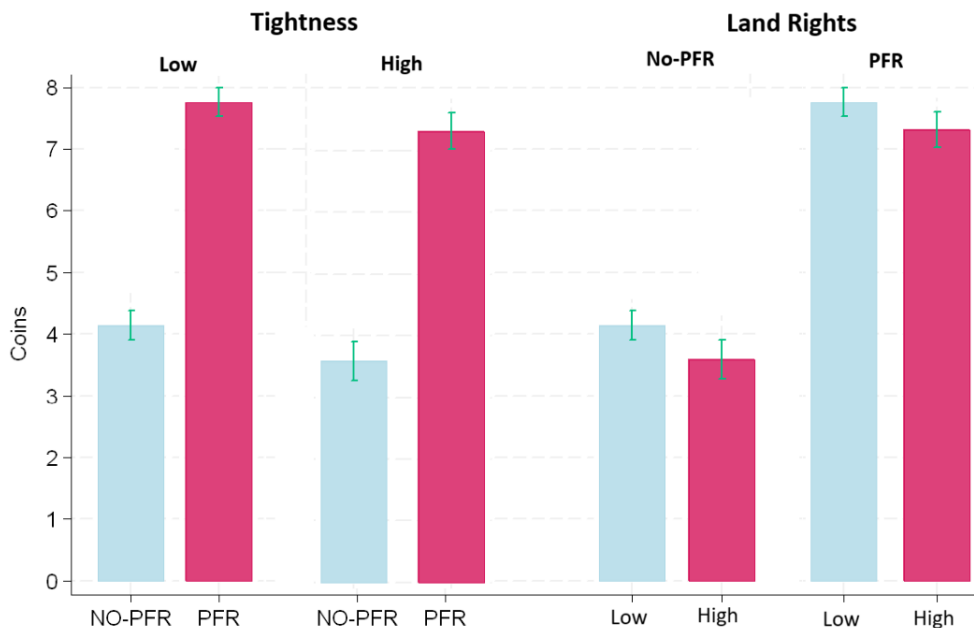


Figure 3: Coins invested by the investors, moderating effects.

tightness (first two columns from the left) and villages with high tightness (third and fourth columns). We estimated the linear mixed-effects model described above, but separating between high- and low-social tightness conditions. Regardless of the village’s tightness level, experiment participants invest significantly more coins when the PFR reform was implemented in the village (7.32 vs. 3.59 under high tightness, and 7.76 vs. 4.14 under low tightness, with $p < 0.01$, two-sided tests).

In the right panel of Figure 3, we compare investors’ decisions in villages with low and high social tightness, distinguishing within each social structure between treatment villages where PFR was not implemented (first two columns from the left) and those where PFR was implemented (third and fourth columns). Again we employ the same mixed-effects model. Regardless of the village’s property rights regime, the amount of coins invested in high-tightness villages is significantly lower than in low-tightness ones (3.59 vs. 4.14 under PFR, with $p < 0.01$, and 7.76 vs. 7.32 in the absence of PFR, with $p = 0.02$, two-sided tests).

To further explore these heterogeneous effects while controlling for observables, we add an interaction term between variables T_i and P_i to our baseline regression equation (4) and estimate its coefficient. The results, reported in Table 3, show that, consistent with the descriptive analysis, the interaction between social tightness and property rights formalization is small and

never statistically different than zero. Robustness checks reported in Tables A3 and A4 in Appendix B confirm this result.

Table 3: Coins Invested by the Urban Residents - Moderating Effects

	Model 1	Model 2	Model 3	Model 4
high-tightness	-0.586*** (0.221)	-0.590*** (0.222)	-0.581*** (0.221)	-0.563** (0.224)
PFR	3.520*** (0.193)	3.538*** (0.193)	3.517*** (0.192)	3.483*** (0.199)
tigth×PFR	0.193 (0.279)	0.196 (0.280)	0.192 (0.281)	0.186 (0.284)
male		-0.136 (0.187)	-0.124 (0.185)	-0.135 (0.214)
age		-0.003 (0.011)	-0.003 (0.010)	-0.005 (0.010)
ambiguity_avx			-0.199*** (0.056)	-0.186*** (0.057)
literate				-0.175 (0.306)
income(k)				0.021 (0.014)
d_order	Y	Y	Y	Y
d_wave	Y	Y	Y	Y
d_ethnicities	N	Y	Y	Y
d_occupations	N	N	N	Y
d_education	N	N	N	Y
Constant	4.859*** (0.302)	4.976*** (0.488)	5.422*** (0.501)	5.890*** (0.578)
N.obs.	794	794	794	794

Notes: Dependent variable: coins invested by the urban resident investors. GLS regression with random effects. Robust standard errors clustered at the subject level. Compared to model 1, model 2 additionally controls for time-invariant characteristics: gender, age, ethnicity. Model 3 adds controls for ambiguity preferences. Model 4 adds controls for literacy, education level, income, and occupation. All models control for the order of the decisions and for the data collection wave. Symbols ***, **, and * indicate significance at the 1%, 5% and 10% level, respectively.

6.3 Responses from the Qualitative Survey

As a complement to our test of the experimental hypotheses, we report here the replies to three open-ended questions that were administered to the 194 experiment participants who took part in the second wave of data collection after the decisions were made. The aim of these questions was to shed some light on the motivations driving respondents' behavior in the

experimental investment task.

First, we asked whether the formalization of land rights is important for the decision to invest in land.¹³ Virtually every respondent (192 out of 194) confirmed that the formalization of land rights is considered an important factor in securing investments, corroborating the experiment's results. Regarding motivations, 57% of respondents mentioned that formalization secures the property title, 16% stated that it reduces conflicts, and 8% noted that having a formal title makes it safer to rent the land and access credit.

In the second question, we asked whether participants perceive an investment made in a village with high or low socially tightness as safer, and the motivations behind their perceptions¹⁴. Three-quarters of the respondents believe that land investment is safer in a society with weaker ethnic ties. Regarding the motivations reported by the respondents, 31% mention that tightness facilitates collusion. Moreover, 9% responds that loose social ties likely reflect a large degree of social mobility, and migrants are less likely to challenge the land acquisition of a non-village resident.

Finally, we asked whether tightness in the village's social structure is important for investment decisions even if formal property rights are present¹⁵. Out of the 165 respondents who expressed an opinion on this matter, the majority believes that the formal property rights created by the PFR reform are a sufficient condition to secure investments, irrespective of the social structure of the village. At the same time, a sizable 24% of respondents believe that the formal land title awarded by the PFR reform is not enough to secure investments. For instance, some of these respondents highlight that "without their [i.e. the other villagers'] consent, you can also lose your land," while others point out that "[The] paper alone isn't enough. You have to take into account the ethnic group next to you to live in harmony".

Altogether, the qualitative survey is in line with our experimental results, suggesting that both the lack of tight social linkages between villagers and the formalization of land property rights decreases the collusion risk perceived by urban investors. At the same time, responses to

¹³The first question was: "For your investment decisions, do you think it's important for a village to have formal property rights (the PFR)? Why or why not?"

¹⁴The second question was: "Do you think your investment in a plot of land in a village you don't know (as in our examples) is safer if: 1) the village's population has a high ethnic and family concentration among its members, or if: 2) there are many different ethnic groups and families in the village with no links between them? Why is this?"

¹⁵The third question was: "For the security of your investment in buying land in a village you don't know (as in today's examples), even if the village has formal land rights and the PFR, do you think it is important for the families in the village to be of the same ethnicity and kinship, or to be of different ethnicities and families?"

the third question suggest that formal property rights cannot fully eliminate such a risk. This result is consistent with our finding that social tightness reduces investment even in villages where the PFR reform was implemented and calls for future research, as discussed in our concluding remarks below.

7 Conclusion

Using experimental data from Benin, this paper provides causal evidence that collusion among informed local parties significantly reduces the value of property rights to outside market participants (Ayotte and Bolton, 2011, Arruñada et al., 2019). Through fieldwork in rural Beninese villages, we elicited villagers' willingness to engage in collusive behaviors aimed at expropriating urban buyers of village land. Subsequently, we conducted an incentivized lab-in-the-field experiment in Benin's largest city, presenting participants with hypothetical investment decisions involving village land purchases. Investors were informed that the profitability of their investment depended on the likelihood of local collusion, and we experimentally varied the characteristics of the villages disclosed to investors, enabling them to form expectations regarding the associated collusion risk. Our findings reveal that urban investors are significantly less inclined to invest in villages characterized by tight social structures or lacking formal property records.

The finding that communities with a collusion-enhancing social structure discourage outside investment (even in the presence of formal property rights) suggests new dimensions along which policy efforts may facilitate property transactions. In particular, our results suggest that in addition to formalizing land ownership, policy initiatives may expand land markets and enhance efficiency by ensuring that the institutions overseeing and enforcing property rights (from land registries to courts) are independent from local communities and impartial towards outside investors.

Future research should explore in greater depth the specific mechanisms that enable the collusion of insiders against outsiders in property transactions. In particular, it would be interesting to investigate whether corruption and management inefficiencies within local land formalization institutions contribute to the concerns expressed by outside investors in our qualitative survey, and complement social ties between insiders as a determinant of the collusion risk

these investors expect. For example, inaccuracies or biases introduced by Millennium Challenge Account researchers during initial land demarcation and assignment of rights under the PFR could have inadvertently facilitated fraudulent activities or made these processes susceptible to local capture (Lavigne-Delville, 2014). A deeper investigation of such implementation weaknesses could further enhance our understanding of property institutions and inform future land formalization and development programs.

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Appendix A Preliminary Step: Eliciting Villagers' Expected Collusion Rate

To assess the likelihood that rural villagers collude against a buyer of land from the city, a team of local research assistants collected data in a sample of 52 Beninese rural villages, randomly selected from those included in the RCT pool used for the implementation of the PFR reform. The data collection took place between March and November 2022 and consisted of several tasks and elicitation, many of which are unrelated to this study and are the basis for different research projects.¹⁶ In each village, a random sample of 50 households was selected from the full census of village families to participate in the collusion elicitation task described below. Only one member per household (of legal age) was invited to participate, and interviews were conducted in a private space.

During the interviews, the research assistants described to participants a situation in which an unknown investor from the main city purchases a land parcel in the village. This situation outlined a judgment-proof seller who may collude with a neighbor to expropriate the buyer after her purchase of the land, as in our theoretical model. Specifically, the situation described to interview participants is as follows: “A man in your village owns a parcel of land and sells it to another person who lives in the city. After the payment has been made, the man tries to trick the person from the city: he asks one of his neighbors to falsely claim that the neighbor is the actual legal owner of the land, and so the man could not have sold the land and the person from the city cannot keep it. The neighbor accepts the man’s request and makes the false claim about the land’s ownership.”

After describing the collusive situation to villagers, we elicited from them the perceived likelihood that a person from their own village would agree to falsely testify to be owner of the land purchased by the city investor. Specifically, participants were asked: “In your opinion, out of 10 people in your village, how many in this situation would agree to falsely testify?”. Individual responses were used to calculate, for each village, the expected collusion rate, representing the probability that the land investment made by the urban resident would be lost. Contrary to the investors’ decision in the subsequent main step of the experiment, elicitation of the perceived

¹⁶See <https://osf.io/h2wrz>, <https://osf.io/5bgvt/>, <https://osf.io/ntq83/>, <https://osf.io/5dq6/> for the pre-registrations related to these other research projects and Fabbri et al. (2025b) as the one paper that, at the time of writing, has been written-up based on such data.

likelihood of collusion was not incentivized:¹⁷ villagers received a flat participation fee equal to 1000 West African Francs (CFA), roughly equivalent to 1,7 US Dollars.¹⁸

Lastly, using census data on ethnic concentration in each village and information on the implementation of the PFR reform, we proceeded to select four villages, each corresponding to a different treatment branch in the main step of the research (the investment decision). Each selected village was characterized by one of four possible combinations of social tightness (“High” or “Low”), and property right formalization (present in the “PFR” villages or absent in the “No-PFR” ones). Villages were selected to achieve maximum variation in the two dimensions of interest. For each selected village/treatment, we computed the probability of collusion as determined by the villagers’ responses described above.

¹⁷Mistakenly, in our pre-registration we wrote that this task was incentivized. However, it was not.

¹⁸Villagers also received a variable payment conditional on their performance in the other experimental tasks unrelated to this paper.

Appendix B Supplementary Analysis

Table A1: Coins Invested by the Urban Residents - Post-Double Selection Lasso

	Model 1	Model 2	Model 3	Model 4
high-tightness	-0.486*** (0.150)	-0.490*** (0.151)	-0.466*** (0.148)	-0.462*** (0.151)
PFR	3.641*** (0.170)	3.666*** (0.170)	3.605*** (0.173)	3.622*** (0.171)
ambiguity-avx	-0.212*** (0.056)	-0.198*** (0.056)	-0.198*** (0.055)	-0.212*** (0.056)
income(k)	0.022** (0.011)	0.021 (0.013)	0.021* (0.013)	0.022** (0.011)
male		-0.127 (0.183)		
age		-0.003 (0.010)		
literate			-0.195 (0.302)	
order				-0.431*** (0.138)
d_wave	Y	Y	Y	Y
d_ethnicities	N	Y	N	N
d_occupations	N	N	Y	N
d_education	N	N	Y	N
Constant	4.600*** (0.186)	4.703*** (0.413)	4.963*** (0.348)	5.246*** (0.308)
N.obs.	794	794	794	794

Notes: Dependent variable: coins invested by the urban resident investors. Post-Double Selection Lasso regression estimated using the Stata package PDS-Lasso (Ahrens et al., 2019). Robust standard errors clustered at the subject level. Model 1 leaves the algorithm unsupervised when choosing the regressors to be included. Compared to model 1, model 2 additionally imposes to include as controls time-invariant characteristics (gender, age, ethnicity); model 3 additionally imposes to include as controls literacy, education, occupation, and income; model 4 additionally imposes to include as controls the order of the decisions. Symbols ***, **, and * indicate significance at the 1%, 5% and 10% level, respectively.

Table A2: Coins Invested by the Urban Residents - Only First Investment Decision

	Model 1	Model 2	Model 3	Model 4
high-tightness	-0.823*** (0.242)	-0.817*** (0.246)	-0.824*** (0.244)	-0.875*** (0.257)
PFR	3.176*** (0.238)	3.187*** (0.241)	3.171*** (0.240)	3.109*** (0.252)
male		-0.135 (0.253)	-0.125 (0.252)	-0.095 (0.281)
age		-0.011 (0.013)	-0.012 (0.013)	-0.016 (0.013)
ambiguity_avx			-0.181*** (0.065)	-0.170** (0.067)
literate				-0.088 (0.359)
income(k)				0.022 (0.020)
d_order	Y	Y	Y	Y
d_wave	Y	Y	Y	Y
d_ethnicities	N	Y	Y	Y
d_occupations	N	N	N	Y
d_education	N	N	N	Y
Constant	5.009*** (0.255)	5.529*** (0.566)	5.942*** (0.587)	3.905*** (0.737)
N.obs.	397	397	397	397

Notes: Dependent variable: coins invested by the urban resident investors in the first investment decision. OLS regression with random effects. Robust standard errors clustered at the subject level. Compared to model 1, model 2 additionally controls for time-invariant characteristics: gender, age, ethnicity. Model 3 adds controls for ambiguity preferences. Model 4 adds controls for literacy, education level, income, and occupation. All models control for the order of the decisions and for the data collection wave. Symbols ***, **, and * indicate significance at the 1%, 5% and 10% level, respectively.

Table A3: Coins Invested by the Urban Residents - Moderating Effects - Post-Double Selection Lasso

	Model 1	Model 2	Model 3	Model 4
high-tightness	-0.522** (0.225)	-0.527** (0.225)	-0.512** (0.226)	-0.550** (0.219)
PFR	3.574*** (0.194)	3.598*** (0.193)	3.566*** (0.197)	3.508*** (0.195)
tight×PFR	0.087 (0.281)	0.092 (0.281)	0.085 (0.281)	0.188 (0.278)
ambiguity-avx	-0.210*** (0.055)	-0.198*** (0.055)	-0.198*** (0.055)	-0.211*** (0.055)
income(k)	0.022** (0.011)	0.021 (0.013)	0.021* (0.013)	0.022** (0.011)
male		-0.131 (0.184)		
age		-0.005 (0.010)		
literate			-0.195 (0.302)	
order				-0.444*** (0.139)
d_wave	Y	Y	Y	Y
d_ethnicities	N	Y	N	N
d_occupations	N	N	Y	N
d_education	N	N	Y	N
Constant	4.543*** (0.192)	4.765*** (0.418)	4.983*** (0.353)	5.232*** (0.312)
N.obs.	794	794	794	794

Notes: Dependent variable: coins invested by the urban resident investors. Post-Double Selection Lasso regression estimated using the Stata package PDS-Lasso (Ahrens et al., 2019). Robust standard errors clustered at the subject level. Model 1 leaves the algorithm unsupervised when choosing the regressors to be included. Compared to model 1, model 2 additionally imposes to include as controls time-invariant characteristics (gender, age, ethnicity); model 3 additionally imposes to include as controls literacy, education, occupation, and income; model 4 additionally imposes to include as controls the order of the decisions. Symbols ***, **, and * indicate significance at the 1%, 5% and 10% level, respectively.

Table A4: Coins Invested by the Urban Residents - Moderating Effects - Only First Investment Decision

	Model 1	Model 2	Model 3	Model 4
high-tightness	-0.548 (0.385)	-0.529 (0.384)	-0.540 (0.378)	-0.602 (0.393)
PFR	3.405*** (0.292)	3.428*** (0.295)	3.408*** (0.292)	3.339*** (0.312)
tight×PFR	-0.512 (0.498)	-0.541 (0.504)	-0.532 (0.499)	-0.511 (0.514)
male		-0.153 (0.255)	-0.142 (0.253)	-0.117 (0.284)
age		-0.012 (0.013)	-0.012 (0.013)	-0.016 (0.013)
ambiguity_avx			-0.181*** (0.065)	-0.169** (0.066)
literate				-0.103 (0.361)
income(k)				0.023 (0.021)
d_order	Y	Y	Y	Y
d_wave	Y	Y	Y	Y
d_ethnicities	N	Y	Y	Y
d_occupations	N	N	N	Y
d_education	N	N	N	Y
Constant	4.880*** (0.268)	5.420*** (0.562)	5.834*** (0.580)	3.716*** (0.746)
N.obs.	397	397	397	397

Notes: Dependent variable: coins invested by the urban resident investors in the first investment decision. OLS regression. Robust standard errors clustered at the subject level. Compared to model 1, model 2 additionally controls for time-invariant characteristics: gender, age, ethnicity. Model 3 adds controls for ambiguity preferences. Model 4 adds controls for literacy, education level, income, and occupation. All models control for the order of the decisions and for the data collection wave. Symbols ***, **, and * indicate significance at the 1%, 5% and 10% level, respectively.

Appendix C English Translation of the Experimental Instructions

We present here, as an example, the instructions for the decision taken in the condition combining ethnically-fragmented village with formalized property rights. Sentences in bold indicate the portions that were substituted in the three other experimental conditions, as described in the main text. We report at the end the three open-ended questions answered by the 194 participants in the second wave of data collection.

Hello. In this part of the interview, you will be making some decisions. We will then randomly choose one of the decisions you have made. We will calculate the earnings you obtained from this decision and pay them to you in cash at the end of the interview. So please listen carefully.

We will now give you ten coins, each worth 50 CFA [show the ten coins to the participant]. You must now decide how many of these coins you want to keep and how many you want to invest.

The coins you keep belong to you. They will be part of your earnings for this decision.

The coins you invest can be tripled by us and paid to you, or they can be lost, depending on whether the investment turns out to be “good” or “bad.”

I will now explain how we determine the probability that the investment will be good, tripling the coins, or bad, causing you to lose the coins. Over the past few weeks, we interviewed several residents of a rural village in Benin. This village has the following characteristics:

- **The village is ethnically fragmented compared to the average in Benin: only 50% of households belong to the same dominant ethnic group, while the other households belong to various other groups.**
- There are fewer than 400 households.
- The nearest paved road is less than 13 km away.
- **In 2011, the government implemented the Rural Land Plan in the village, so that property titles and the owners of each plot of land in the village are**

officially recorded in a public registry maintained and updated by the village authorities.

When we visited this village, we described the following situation to the residents we interviewed:

“A man in your village owns a plot of land and sells it to someone who lives in the city. After being paid, the man tries to deceive the person from the city by asking a neighbor to falsely claim that the neighbor is the legal owner, so that the seller could not sell the land and the city buyer cannot keep it.”

We then asked each person interviewed the following question:

“In your opinion, out of 10 people in your village, how many would agree to make such a false claim about the ownership of land to the detriment of a person from the city?”

We will use the answers given by the villagers to determine the probability that the coins you decide to invest will be tripled or lost. We filled a bag in front of you with 10 balls that are either yellow or white. The number of yellow balls we put inside corresponds to the number of villagers who, in the situation described above, according to the participants, would on average agree to make a false claim to deceive the city investor. Therefore, you do not know the proportion of yellow and white balls in the bag, but you can estimate it based on what you think the villagers answered.

For example, if the participants in this village said that 7 out of 10 people in their village would be willing to make a false claim to deceive the city investor, we put 7 yellow balls and 3 white balls in the bag. Or, if participants said that 2 out of 10 would agree to make a false claim, we put 2 yellow balls and 8 white balls in the bag. And so on. The number of yellow balls in the bag is determined by the villagers’ answers.

After deciding how many of your 10 coins you want to invest and how many you want to keep, you will blindly draw one of the ten balls from the bag. If the ball you choose is white, we will triple the coins you invested and pay them to you, in addition to the coins you kept. On the other hand, if the ball you choose is yellow, the investment is “bad” and you lose the invested coins; in this case, we will only pay you the coins you kept.

For example, if you decide to invest 6 coins and the ball you choose is white, we will pay you 22 coins: $6 \times 3 = 18$ coins from the “good” investment plus the 4 coins you did not invest

and kept. Conversely, if the ball you choose is yellow, the investment is “bad,” and we will only pay you the 4 coins you decided not to invest; in this case, you lose the 6 coins you chose to invest.

Remember, you do not know how many yellow or white balls are in the bag. The number of yellow and white balls, and thus the probability that the investment will be good or bad, depends on the answer given by the villagers we interviewed to the question:

“In your opinion, out of 10 people in your village, how many would agree to make such a false claim about land ownership to the detriment of a person from the city?”

Also remember the following information about the village where we conducted the interviews:

The village is ethnically fragmented compared to the average in Benin: only 50% of households belong to the same dominant ethnic group, while the other households belong to various other groups.

There are fewer than 400 households.

The nearest paved road is less than 13 km away.

In 2011, the government implemented the Rural Land Plan in the village, so that property titles and the owners of each plot of land in the village are officially recorded in a public registry maintained and updated by the village authorities.

(Control questions not reported).

How many of the ten coins would you like to invest?

Open-ended Questions

1. For your investment decisions, do you think it is important for a village to have formal property rights (the Rural Land Plan, PFR)? Why?
2. Do you think that your investment in buying land in a village you don't know (as in our examples) is safer if the village population has a strong concentration of a single ethnic group and related families, or if the village has many different ethnic groups and families with no ties to each other? Why?
3. For the security of your investment in buying land in a village you don't know (as in today's examples), if the village has formal land rights and the Rural Land Plan (PFR),

do you think it is important that the families in the village belong to the same ethnic group and are related, or that they belong to different ethnic groups and families? Why?