

# Land Titling and Litigation

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## Abstract

We study a land-titling reform implemented as a randomized control trial to isolate its effects on litigation. The reform consisted of demarcating land parcels, registering existing customary rights, and granting additional legal protection to right holders. Ten years after implementation, the reform doubled the likelihood of households experiencing land-related litigation, but disputes did not escalate into more frequent violent episodes. We suggest that this increase in litigation reflects the complementarity of land titling by registration and by judicial procedures aimed at further clarifying property rights, as the reform registered titles to all parcels but left many titles subject to adverse claims. This raised the demand for complementary litigation aimed at perfecting titles for low-value parcels that, under the customary system, were optimal to keep unclarified. Consistent with this explanation, we find that the increase in litigation took place among households that plausibly own land of lower value.

## 1. Introduction

Establishing secure property rights and granting fair access to land are key drivers of economic development (Banerjee and Duflo 2007; Besley and Ghatak 2010; Deininger 2003; De Soto 2000). For example, one of the United Nations' Sustain-

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able Development Goals for 2030 aims to “ensure that all . . . have access to . . . ownership and control over land and other forms of property” (G.A. Res. 70/1, p. 15 [September 25, 2015]) and it estimates countries’ achievements in this regard with an indicator measuring the “proportion of total adult population with secure tenure rights to land.”<sup>1</sup> In particular, the idea has taken root that development would be fostered by facilitating access to legality. It is thought that if those in possession of even small buildings and plots of land have good titles, they will enjoy better incentives to invest and can use their real assets as collateral for credit. To this end, in recent decades governments and international development agencies have implemented various interventions aimed at formalizing the existing customary tenure institutions that predominate in developing countries.

In this article, we contribute to the understanding of the consequences of formalizing land rights by focusing on the causal effects that different types of land rights institutions have on land-related litigation. Using data collected in Beninese rural villages, we investigate the impact of registration efforts on land-related conflicts that, over time, were peacefully resolved through institutions for dispute resolution or escalated into violence.<sup>2</sup> According to common wisdom, one of the benefits of formalization and a key factor in igniting the virtuous cycle of economic growth is to reduce litigation (Holden, Deininger, and Ghebru 2019). Commentators hypothesize that clearly demarcated plot boundaries and formally recorded legal rights will reduce conflicts between individuals regarding contested boundaries (Di Falco et al. 2020), claims over land parcels (Fearon 1998), and larger-scale social and ethnic conflicts over land, which often have devastating consequences (André and Platteau 1998). From this perspective, formal land titling can be seen as a substitute for litigation directed to solve conflicts over land.

However, apart from the above-mentioned benefits, land rights reforms can introduce social tensions that, from a theoretical standpoint, make the net effect of formalizing property rights on litigation unclear. Scholars argue that tenure reforms could exacerbate social disputes by introducing competition with the incumbent customary system (Atwood 1990) and overlapping and contradicting legal sources. The combined application of customary and formal law, or legal pluralism, increases institution shopping, legal uncertainty, and the frequency of disputes (Firmin-Sellers 2000; Platteau 1996).<sup>3</sup> Moreover, registration can con-

<sup>1</sup> United Nations, Sustainable Development Goals, SDG Indicators (<https://unstats.un.org/sdgs/metadata?Text=&Goal=1&Target=1.4>).

<sup>2</sup> To avoid repetition, throughout the article we use the terms “conflict” and “dispute” interchangeably when referring to a disagreement about land-related issues.

<sup>3</sup> When low-income countries try to replace a customary-law system with formal land rights, they often face supply-side constraints such as insufficient budgets, incompetent agencies, and inadequate legitimacy that make the transition incomplete and result in the combined application of customary and formal law (Fitzpatrick 2005). For example, according to Barrows and Roth (1990, p. 273) a recent land reform in Kenya “failed to gain popular understanding or acceptance, individuals continued to convey rights to land according to customary law, and a gap developed between the control of rights as reflected in the Land Register and as recognized by most local communities.” With legal pluralism the question of which institution defines and enforces property rights becomes ambiguous because traditional authorities have lost much of their power of control over land, but the state has not yet developed the capacity to take full control (Fred-Mensah 1999; Xu 2014).

concentrate titles in a few rent seekers (Binswanger, Deininger, and Feder 1995; Goldstein and Udry 2008) and jeopardize the role that land plays as a redistributive system and social safety net (Deininger and Feder 2009; Delville 2000). Finally, when there is disagreement between possessory and formal boundaries, the implicit georeferencing of plots via their cadastral identification numbers and the cadastral boundaries assigned to such numbers can make owners in peaceful possession uneasy about their position. Possessors are happy about their boundaries until a third party draws them and points out a contradiction, but if contradictions are made salient and registration efforts are not accompanied by a thorough clarification of land titles, parties may be motivated to litigate (Arruñada 2018). In cases like these, litigation may become a complement to formal titling.

We shed light on how land titling affects litigation by studying the effects of a land tenure reform named Plan Foncier Rural (PFR) approximately 10 years after its implementation in Benin. The reform systematically identified customary rights to land parcels, demarcated boundaries, and created public land registries, which made it possible to sell or use registered rights as collateral and defend them in court against contenders. Our main contribution consists in isolating the causal effects that the reform had on land-related litigation by implementing a research design that dispels endogeneity concerns commonly associated with the titling of land. The identification strategy is based on the process of implementation characterizing the Beninese PFR, which, to the best of our knowledge, is the first case of large-scale land tenure reform implemented as a randomized control trial (RCT). In Section 2, we describe in detail the RCT's structure, characterized by random selection of treated villages where the reform was implemented and control villages where, as of this writing, customary land rights remain. To gather information regarding the effects of the PFR on land-related litigation, the types of conflicts experienced, and the dispute resolution mechanisms used, we administered two rounds of surveys to individuals 7 and 10 years after the reform's implementation, conducting in-depth interviews with a sample of 1,086 respondents across 43 randomly selected villages.

Our results show that, for average effects over the sample, the formalization of land rights significantly increases the likelihood of land-related litigation. Point estimates suggest that participants in treated villages have double the probability of engaging in land-related disputes. The majority of conflicts concern contested parcel boundaries, but we also observe a significant increase in disputes related to land inheritance. Participants who experienced the reform show only minor differences in the choice of conflict resolution mechanism—customary, religious, or formal courts—and we do not observe significant changes in the frequency of conflict-related violence or in beliefs that land-related disputes can escalate into violent episodes.

This increase in litigation is puzzling if we conceive titling as an all-or-nothing phenomenon or as taking place only through land registries. However, it is consistent with a view of the complementarity of titling by registration and by judicial procedures aimed at clarifying property rights. Along these lines, we suggest an explanation for the increase in litigation observed after the reform. The

starting consideration is that to reduce the probability of suffering from adverse claims, right holders can engage in private and public activities to clarify their existing rights and protect their title—that is, purge their (formal or informal) ownership titles. To this end, they can engage in private physical practices to better define and proclaim their rights, such as fencing parcels (Anderson and Hill 1975; Hornbeck 2010), demarcating boundaries (Libecap and Lueck 2011), and enforcing possession of their assets by physically staying in place (Field 2007; Goldstein et al. 2018). They can also clarify their titles using public means, by litigating against individual claimants—as in a boundary dispute—or by initiating a general judicial procedure against all potential adverse claimants, similar to the quiet-title suit used in the United States (Bray 2010).

In a customary system, parties who possess land parcels can keep their rights unclarified, make investments to reinforce them, or purge them from possible adverse claims. This was the case in rural Benin, where possessors of parcels traditionally resorted to various means to enhance their titles and purge potential adverse claims. Private means included engaging in vodou practices and the ritual planting of a shea or *karité* tree to proclaim ownership (Adjahouhoué 2013), which made the ownership claim public and motivated potential claimants to bring suit or implicitly concede. As in many developing countries, in recent times these proclamations have also included posting ownership claims indicating the name and phone number of the owner, fencing and building houses and wells, and employing guards. In addition, possessors resort to fully public means ranging from requesting that local authorities issue certificates attesting ownership and having conveyances endorsed by the village chief in a certificate of nonlitigation (an *attestation de non litige*; Bierschenk and Olivier de Sardan 2014) to starting expensive first-registration procedures (*immatriculation*) at the land register created in colonial times (Delville 2019), which is an option that, given its cost, is suitable only for the most valuable land.

In places as different as Benin and the United States, whatever private or public means are used, the economic logic remains the same. On the one hand, right holders will make investments to clarify existing rights only for land parcels whose value increases more than the purging costs. We refer to these as high-value parcels. On the other hand, right holders will leave unpurged the rights to low-value parcels.

As we explain in detail in Section 2, the Beninese PFR, like most other recent titling efforts, is characterized by two features important for our argument: the reform titled all parcels, but it did so imperfectly. First, all valuable land in a village was registered, irrespective of a parcel's value. Second, the reform faced time and resource constraints that resulted in an incomplete purging of the land titles awarded. The Beninese PFR included a procedure for purging land titles, but the purging process was largely imperfect because of the lack of time to solve the most controversial cases and because some customary rights were excluded from the registration process but were successfully claimed by absent parties at a later stage (Delville and Moalic 2019).

We advance the view that the activities subsidized by the PFR, such as demar-

cating boundaries and identifying owners, reduced the likelihood of eviction and therefore increased parcels' value. However, since the land titles awarded remained incomplete, villagers who experienced the reform had greater incentive to perfect their ownership titles through litigation aimed at further clarifying property rights. In Section 5 we elaborate on this point. This argument is consistent with the observation that the demand for purging titles affected mostly lower-value land parcels, as right holders who possessed higher-value parcels under the customary system had already made private investments to clarify their rights. Indeed, we report evidence that the observed increase in litigation is driven by treated households with low income levels and low levels of market integration, who are likely to possess land parcels of relatively low value. Conversely, the effects on households with high income and greater market integration are small and insignificant, arguably because before formalization they had invested in bettering the titles of their higher-value land.

It is worth emphasizing that the type of land-related litigation we observe is not necessarily a negative outcome, since disputes conducted via an institutionalized process might contribute to a beneficial clarification of ownership rights. In a sense, it is titling by different means and makes it possible to adjust the quality of the title to the land's value and therefore introduces some flexibility into a system of universal titling. Moreover, in our sample we do not observe significant changes in the frequency of conflict-related violence in treated villages or in beliefs that land-related disputes can escalate into violent episodes. Finally, while assessing whether the observed increase in litigation produced by the reform is efficient lies outside the scope of this article, previous research shows that clarifying property rights can substitute for inefficient expenditures in private protection and can increase investments (Field 2007; Galiani and Scharfrodsky 2010; Goldstein et al. 2018). In Section 6, we return to this discussion.

Our article contributes to a growing empirical literature that reports mixed evidence on the effects of land rights formalization on conflicts. Two waves of research based on case studies (André and Platteau 1998; Jansen and Roquas 1998; Kalabamu 2019; Peters 2009) and cross-sectional observational data (Alston, Libecap, and Mueller 2000; Deininger and Castagnini 2006; Holden, Deininger, and Ghebru 2019) show that tenure formalization is associated with no reduction, or at times even an increase, in conflicts over land. However, those research designs cannot account for endogeneity and self-selection issues concerning villagers' decisions to title only land parcels that are more likely to be contested or authorities' choices to apply selective formalization programs involving only parcels or territories with comparatively high value.<sup>4</sup>

<sup>4</sup> Titling decisions and formalization policies are often endogenous, and hidden causal variables may influence both the titling of land and its supposed consequences. For example, the implementation of titling projects often starts with the regions that have the best economic outlook. In other cases, reverse causation may also be present, as when investments enhance the quality of title, a phenomenon observed, for example, in Ghana with respect to the planting of trees (Besley 1995). Conversely, under voluntary titling, those with insecure title (and therefore less incentive to invest) may be more inclined to title (Arruñada 2012), which could bias results toward underestimating a positive effect of titling.

Only a few studies that investigate the link between institutions and litigation have quasi-random allocation of titles across the sample of households, which lends more confidence to identifying causal relationships instead of mere correlations. Two studies that focus on the relationship between property rights and violence find a moderating effect of formalization on homicide rates. The first article, Fetzer and Marden (2017), exploits spatial and temporal variation in the availability of forestland protected by natural conservation laws—which are therefore not vulnerable to requests for titles by squatters—in the Brazilian Amazon region, in combination with the constitutionally provided right to occupy unused land, to show that the expansion of territories for which land titles cannot be requested reduces the rate of violent conflicts in a municipality.<sup>5</sup> The second study, Dower and Pfütze (2020), shows that land certification in Mexico reduces violent deaths. The authors provide evidence that the reduction in violence stems from formalized reductions in politicians' discretion in the allocation of land rights and, as a consequence, in the amount of disputes. Our article complements those contributions because we collect data on the full set of disputes over land, including those escalating into violence and those resulting in nonviolent contentions. Di Falco et al. (2020) compare the rate of land-related conflicts experienced by Ethiopian rural villagers the year before and the year following the rollout of a land-rights-certification program. The authors show that villagers who received formal land certificates experienced significantly fewer land-related conflicts. We complement these findings regarding the immediate effects of formalization on conflicts by studying the medium-term effects of the intervention (that is, 10 years after implementation).<sup>6</sup>

The article is organized as follows. In Section 2, we describe the main features of the Beninese legal and institutional framework and of the PFR. Section 3 explains the research design and reports details of the survey and the data collection. In Section 4 we present the results. Section 5 discusses the findings and suggests a general framework that connects land titling and conflicts. Section 6 concludes.

<sup>5</sup> One important difference between Fetzer and Marden (2017) and our work is that in the Brazilian Amazon, acquiring the status of protected forest land implies severe limits on the possibilities of future uses, which lowers the land's economic value. Therefore, in contrast with the Beninese reform, the intervention studied in Fetzer and Marden (2017) does not simply assign well-defined property rights but also reduces the choice set of potential investors and leaves doubts about whether the estimated decrease in conflicts is driven by formalization or the jointly determined decrease in land value.

<sup>6</sup> As in the case of the Beninese reform, the decentralized process of land rights formalization in Ethiopia includes as a precondition for receiving land certificates a dispute resolution process that resolves any ongoing conflict (Deininger et al. 2008). This dispute resolution mechanism is likely to have cleared pending disputes and resolved latent conflicts that the formalization of land rights had induced. Therefore, the immediate reduction in conflicts estimated by Di Falco et al. (2020) in the year following formalization might reflect the temporary clearing of existing disputes, while our estimation is more likely to reflect the performance of the new institutional environment and the emergence of adverse claims in the medium term.

## 2. Institutional Framework

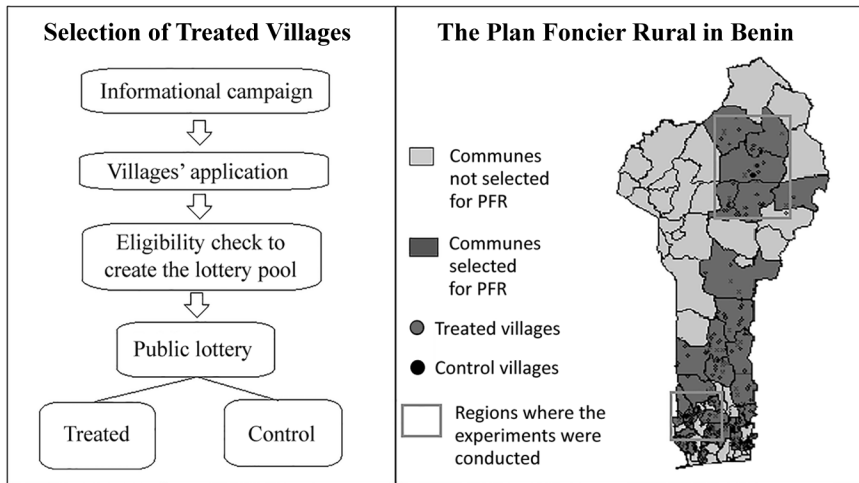
As in many African countries, well-defined individual property rights did not exist in Benin until European colonization in the 18th century. The land was inalienable, it belonged to the gods and the community, and villagers could use it only for agriculture. Access to land was regulated by customary law applied by traditional chiefs. As a result of French colonization, the customary-law system was supplemented with the Napoleonic Code of 1804, and some ownership of private property was introduced. The coexistence of customary and formal law led to insecurity concerning land rights and exacerbated land-related conflicts, allegedly because of judicial decisions not being enforced, uncertainties concerning boundaries, errors in the identification of owners, increasing illegal occupations, and a lack of publicity regarding property titles (Tchoca 2019).

It is against the background of these problems related to insecurity of tenure that the Beninese government launched the PRF land tenure reform. This plan registered *de facto* private property rights after mapping all parcels, investigating the correspondence between parcels and right holders, and registering right holders in each village. The PFR was introduced on a large scale in 2010–11, when the Millennium Change Account subsidized an implementation program and the Beninese government enacted a law introducing Torrens-type title certificates (Goldstein et al. 2018).<sup>7</sup>

From the perspective of our empirical contribution, the key attribute of the PFR titling effort is that implementation followed an RCT process involving hundreds of rural villages. In fact, this is the first case of a large-scale land tenure reform implemented as an RCT. In the preliminary phase of the project, interested rural villages were informed about the PFR and were invited to apply to participate in a lottery. Each application received was examined to verify whether the village met certain eligibility criteria, such as being in a rural area. Among the 576 villages that applied to participate in the PFR lottery and were judged eligible, a subsample of 300 villages were randomly chosen via public lottery. Consequently, in 2010–11, a team of local experts implemented the PFR in those villages (the treated group). The remaining unselected villages (the control group) did not receive any intervention and as of this writing have customary land rights. Figure 1 shows the lottery procedure characterizing the PFR and the resulting map of communes and villages included in the lottery pool. Two additional features of the PFR are particularly relevant for our study. First, the reform aimed for universal demarcation of boundaries and rights in each treated village, which means that all valuable land in a village was registered regardless of a parcel's value.

<sup>7</sup> Some amendments of the original legal framework that supported the 2010–11 implementation plan did not modify the validity of the formalization intervention. For instance, the Beninese government initially created certificates identifying the right holders as they appeared during the creation of the Plan Foncier Rural (PFR) by enacting Law 2007–003 on land rights. The release of certificates was suspended with the creation of a new land code in 2013 that reunified certificates and property titles in a unique ownership document and confirmed the legal validity of PFR-registered rights. For our purpose the relevant title effort is the program financed by the Millennium Change Account.





**Figure 1.** The lottery procedure (*left*) and the distribution of control and treated villages (*right*) (from Fabbri 2021, App. A, p. 33).

Second, it was to consider the importance of both the physical demarcation of boundaries—by marking them with cornerstones—and the legal demarcation of rights—by conditioning registration on gathering the public consent of neighbors on adjoining parcels. However, limited resources, the complexity of customary bundles of rights, and some legal deficiencies resulted in a *de facto* emphasis on physical demarcation (Delville and Moalic 2019).

### 3. Research Design

Our research design is based on the RCT implementation of the PFR. We randomly selected for data collection 43 villages from the full list of villages included in the PFR located either in two provinces in the north (Mono and Couffou) or two provinces in the south (Alibori and Borgou). To isolate the causal effects of land titling on litigation, we compare conflicts experienced by residents in villages selected for PFR titling with those in control villages that were not chosen for the PFR and therefore remained under customary titling.

Several caveats are in order. First, pretreatment data on litigation are not available for participants in our sample. Therefore, our identification strategy relies on the random selection of the 43 villages where we collected data from the original RCT pool for cancelling out potential preexisting differences in land-related litigation across treatment groups. An impact evaluation of the original PFR implementation shows that the randomization of the PFR lottery was successful (Goldstein et al. 2015). Moreover, as we show in Section 4, participants in our sample are well balanced on observable characteristics, which increases confidence in the validity of our approach. In addition, for the identification strategy to work we



must ensure that there is no self-selection of individuals into treatment following the PFR randomization (for instance, because of migration from control to treated villages after the reform's implementation). As explained in Section 4, we verified that migrating out of the village of origin is rare for participants in our sample and that migration flows are similar across research groups. Finally, official statistics relative to land-related litigation in Benin are available only for disputes resolved through formal judiciary. This is a small fraction of the total number of conflicts experienced by rural villagers because, as we elaborate in Section 4, customary, informal, and religious dispute resolution mechanisms coexist with state courts. Therefore, to avoid possible nonclassical measurement errors and to gather a comprehensive picture of all land-related conflicts, the data we analyze were collected by administering an in-depth survey during fieldwork sessions in the sample of selected villages.

We conducted two survey rounds, the first in the initial trimesters of 2017 (approximately 7 years after the reform's implementation) and the second at the beginning of 2020 (10 years after implementation). We held 65 fieldwork sessions (32 in 2017) during which we visited 43 villages (24 treated) and interviewed members of 1,086 households (493 in 2017).<sup>8</sup> The survey collected sociodemographic information and responses to a set of questions about land-related disputes. In particular, we asked participants whether they had experienced at least one conflict related to land after 2010 and if so which type it was.<sup>9</sup> The survey round of 2020 included the same questions that were asked in 2017 plus an additional set of questions about details relative to market integration, the conflict resolution mechanism, and the solution to the dispute (we return to this point in Section 5).

In both survey rounds, data collection proceeded as follows. In the days before the session, a research assistant visited the village and requested that residents gather on the scheduled day to be part of the research project. On the established day, we randomly selected 18 individuals to participate in the data collection, with those not selected receiving a show-up fee equal to 500 CFA (roughly \$.90) and dismissed.<sup>10</sup> The research assistant first checked that each participant was a resident of the village, was older than 18, and had no other household members being surveyed. Then each participant privately answered the questions posed by

<sup>8</sup> According to the original PFR formulation, only parcels of land within the village's administrative borders were subject to the intervention. In three villages in the treated group, authorities reported having extended the PFR intervention after 2011 by demarcating and formally registering some additional land parcels outside the village's official borders. Moreover, in one control village that is close to a treated village, half of the participants reported having access to land plots within the borders of the confining village, so those plots were included in the PFR intervention. In the regressions reported in the text, we include a dummy identifying this village. In addition, a replication of the analysis that excludes these villages from the sample yields qualitatively the same results (available from the authors on request).

<sup>9</sup> Participants were asked whether there was a conflict related to boundaries, inheritance, second sales, or expropriation by the state or another type of conflict.

<sup>10</sup> The majority of the data collection sessions involved 18 participants, but because of logistical constraints and specific circumstances the number varied between 12 and 20 individuals.

the research assistant and performed additional fieldwork activities unrelated to the project. Administering the survey to an individual took about 40 minutes.

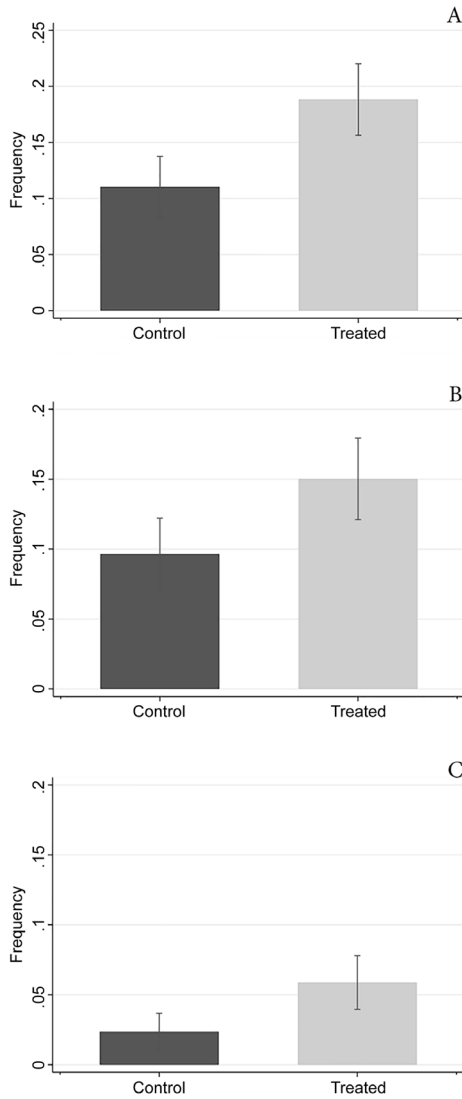
#### 4. Results

In Table A1 we compare the observable characteristics elicited in the postexperimental survey across the research groups. The samples are well balanced, with the exception that participants in the treated sample are on average slightly older and more likely to be polygamous. Furthermore, in the sample of participants surveyed in 2020 for whom we collected additional data, there are some minor differences in the likelihood of managing the household's money, having a concrete floor, and having running water at home. For our identification strategy to hold, we needed to verify that, after the reform's implementation, participants had not self-selected through migration into the treatment group. To do so, we collected data regarding each participant's village of origin, the number of years living in the village, and the reason for migration. The vast majority of participants reside in the village where they were born, and the likelihood of having migrated is the same across research groups (69 percent in the treated group and 72 percent in the control group;  $\chi^2$ -test,  $p > 10$  percent). The majority of migrations were reported by female participants, with marriage the reason commonly declared for a move. Similarly, we verified that there is no statistically significant difference across groups for the fraction of adult life a participant had spent in the village where the data were collected (two-sided  $t$ -test,  $p > 10$  percent).

As a first step in the analysis, we examine the likelihood of experiencing land-related litigation after the implementation of the PFR as reported by survey respondents. Figure 2A shows the frequency with which participants report experiencing land-related conflicts in treated and control villages. Participants in treated villages report litigation significantly more often than those in control villages ( $\chi^2$ -test,  $p < 1$  percent). Figure 2B and C show that the majority of disputes arise because of contested parcel boundaries and, to a lesser extent, inheritance. In both cases, the likelihood of experiencing these types of conflicts is significantly larger in treated villages than in control villages ( $\chi^2$ -test,  $p < 1$  percent in both cases).

Secondly, we verify these findings in a regression framework. Table 1 reports the results of a linear probability model<sup>11</sup> in which the dependent variable is a dummy equal to one if a participant reported experiencing land-related litigation after 2010. Standard errors are clustered at the village level to account for possible levels of intravillage correlation. Model 1 controls for the two observable characteristics that are unbalanced in our sample (age and whether a household is polygamous). The coefficient of the treatment dummy is positive and statistically significant at the 1 percent level. The point estimate increases from 10 percent to

<sup>11</sup> We report in the text the results of a linear probability model that simplifies the interpretation of the coefficients. We reestimate the regressions using a nonlinear probit model in Table A3. Results are qualitatively the same.



**Figure 2.** Land-related conflicts: full sample. A, All land-related conflicts; B, boundary conflicts; C, inheritance conflicts.

18 percent, which suggests that participants who had the PFR implemented are approximately 76 percent more likely to experience land-related conflicts. Model 2 also controls for the individual characteristics we collected in a postexperimental survey. The coefficient of the treatment dummy remains positive and strongly significant, and the point estimates are very similar. Model 3 includes further controls for village characteristics. The coefficient remains strongly significant,

Table 1  
Likelihood of Experiencing Land-Related Conflicts

	Full Sample ( $N = 1,086$ )			Reduced Sample ( $N = 977$ )		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Treated	.078** (.028)	.075** (.027)	.101** (.029)	.092** (.030)	.091** (.030)	.115** (.030)
Individual controls	No	Yes	Yes	No	Yes	Yes
Village controls	No	No	Yes	No	No	Yes
Constant	.103* (.041)	.108 (.083)	.094 (.087)	.115* (.044)	.063 (.089)	.059 (.095)

**Note.** Results are from ordinary least squares regressions; robust standard errors clustered at the village level are in parentheses. The reduced sample excludes respondents in treated villages who do not own land. All regressions control for age and whether the participant is polygamous. Individual controls include gender, religion, a measure of risk preferences, whether the respondent is married, a dummy for literacy, and income. Village controls include population, distance from the closest paved road, a dummy for villages in the South, and a dummy for four villages that further developed the original land tenure reform.

\* Significant at the 5% level.

\*\* Significant at the 1% level.

and the point estimate suggests that being in a village where the reform was implemented roughly doubles the likelihood of experiencing litigation.

We continue the analysis by excluding from the sample 106 households in treated villages that took part in the survey but that did not own land parcels affected by the PFR. This could happen for various reasons, for example because all the land belonging to the household was located outside the village's borders—and so was not included in the PFR—or because the respondents' household did not own land. All coefficients of the treatment dummies in the reduced sample are positive and strongly statistically significant. The estimated increase in the probability of litigation is larger than when the full sample of participants is considered, ranging between 80 percent and 190 percent. In Table A2, as a robustness check we reestimate the specifications presented in Table 1 using wild clustered bootstrapped standard errors with 999 repetitions. The qualitative results are the same.

We then verify what types of land-related conflicts were affected the most by the reform. In Tables A4 and A5, we replicate Table 1 including only conflicts related to parcel boundaries and land inheritance, respectively. The results suggest that conflicts over parcel boundaries significantly increased in treated villages. A large share of the estimated increase in conflicts generated by the reform can be attributed to this type of dispute. To a lesser extent, we also observe a significant increase in litigation concerning the inheritance of land.

We next examine the subsample of 594 participants surveyed in 2020, who answered a supplementary set of questions that make it possible to explore channels through which the PFR determined the observed increase in litigation. Those who experienced conflicts after the PFR's implementation were asked questions about

how the disputes had been managed. Participants in the treated group reported a slightly longer average litigation process compared with the control group (30 versus 22 months, respectively), although the difference is not statistically significant. In the treated sample, 50 percent of respondents reported having resolved their disputes, a marginally significant lower amount than the 78 percent of control respondents ( $\chi^2$ -test,  $p = 8$  percent). Participants were also asked whether the conflicts they experienced involved violence and their opinion about the likelihood that a hypothetical land-related dispute may escalate into a violent episode (on a 1–7 Likert scale, where 1 represents the lowest probability). Participants report a similar frequency of violent episodes and belief in the likelihood of an escalation (2.53 for the treated group versus 2.63 for the control group; Kruskal-Wallis test,  $p > 10$  percent).

We check whether villagers who experienced the reform made different choices of conflict resolution mechanism. In particular, we asked which authority they initially approached: formal state courts, village authorities, or religious authorities. The majority of respondents in both groups first approached local village authorities (63 percent in the treated group and 72 percent in the control group;  $p > 10$  percent). However, the majority of participants who solved their conflicts reported that the final adjudication was done by a formal state tribunal—again with no significant differences between groups (73 percent in the treated group and 69 percent in the control group). This is in line with the predominant beliefs of participants in the 2020 survey, who reported that formal state courts have the last word when there are different adjudication outcomes from the three conflict resolution mechanisms. Of the 594 participants, 77 percent in the treated group and 70 percent in the control group consider the decision of a formal state tribunal to be unappealable (the difference is not statistically significant at conventional levels). Similarly, 83 percent of participants in both groups reported believing that state tribunals can be used as an appeals court by litigants who are not satisfied with the judgment of local or religious authorities (and who can afford to access the formal justice system). This evidence indicates that the PFR did not modify villagers' choices or perceptions concerning which mechanism to use to address land-related disputes, which suggests that this channel is unlikely to be responsible for the observed increase in litigation.

### 5. Why Formalization May Encourage Purging Litigation

In this section, we suggest an explanation for the increase in litigation observed in treated villages. We use some simple algebra to clarify the argument. Following Arruñada and Garoupa (2005), let us assume that title defects are represented by the probability  $\theta$  that a conflicting claim for ownership fully succeeds ( $0 < \theta < 1$ ). This probability depends on which titling system (customary or formal registration) is in place and on the owners' decision to additionally perfect their titles. Under customary titling, such probability is  $\theta_0$ , but, as explained in Sec-

tion 1, even if land ownership remains informal owners could spend resources to protect a title by activities such as planting a *karité* tree, fencing a parcel, requesting an ownership certificate from the village chief, litigating the boundaries of a neighboring parcel, or clarifying ownership against all potential claimants in procedures functionally similar to the quiet-title suit used in the United States (Bray 2010).

Figure 3 shows the value of land when title conflicts exist as a function of its value  $V$  in an ideal world without conflicting claims, under different titling institutions, and with owners being able to make additional efforts to protect and perfect property rights. Let us assume that by spending a fixed amount  $p_0$  per parcel, owners can make a title safer by reducing the probability of losing the land to  $\theta_{0p}$ . When deciding whether to purge a title, owners will compare the value of the land without purging, given by a fraction  $(1 - \theta_0)$  of land value  $V$ , with the value after purging, given by  $(1 - \theta_{0p})V - p_0$ . The break-even point is

$$V_0^* = \frac{p_0}{\theta_0 - \theta_{0p}}.$$

Therefore, it is worthwhile for owners of informally held land to purge their titles if the value of the parcel is higher than  $V_0^*$ , but it is not worthwhile for land values lower than  $V_0^*$ . When a government introduces land titling, it usually does so—and it did so in Benin—for all relevant parcels independent of their value and without cost for owners but imperfectly, so the probability of eviction after titling,  $\theta_1$ , remains positive even if it is lower than  $\theta_0$ . This decrease in the probability of eviction  $(\theta_0 - \theta_1)$  is what causes the increase in land value driven by formalization and increases the slope of the value line in Figure 3.<sup>12</sup> Again, owners can spend resources to additionally protect and purge their titles. Let us assume that by spending a fixed amount  $p_1$  per parcel, owners can make their titles safer by reducing the probability of losing the land from  $\theta_1$  to  $\theta_{1p}$ . When deciding whether to purge titles, owners compare the value of the formally titled land without additional purging, given by  $(1 - \theta_1)V$ , with the value after titling and purging, given by  $(1 - \theta_{1p})V - p_1$ . The new break-even point is

$$V_1^* = \frac{p_1}{\theta_1 - \theta_{1p}},$$

<sup>12</sup> While estimating the effects of land titling on economic growth lies outside the scope of this article, some suggestive evidence that the Beninese PFR may have increased land value comes from a survey administered in 2011 to 154 chiefs of villages included in the PFR lottery pool (Goldstein 2011). Participants were asked to report the price for renting a hectare of good-quality agricultural land in their villages. The 82 respondents from villages in which the PFR had been implemented reported a renting price 31 percent higher than respondents in villages without PFR (two-sided  $t$ -test,  $p = .02$ ). Similarly, respondents in treated villages reported a hypothetical selling price that is 14 percent higher than that reported by respondents in control villages—although in this case the difference is not statistically significant (two-sided  $t$ -test,  $p = .39$ ).

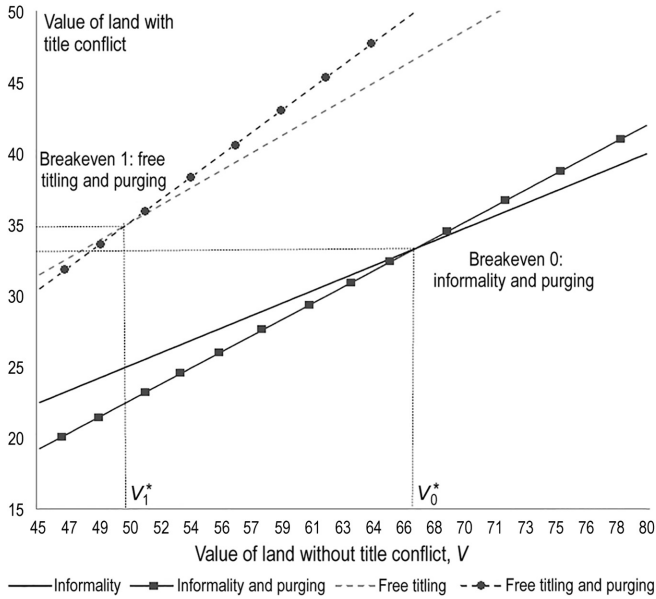


Figure 3. Possible functions of land values

and the two break-even points are related by

$$\frac{V_1^*}{V_0^*} = \frac{(p_1/p_0)(\theta_0 - \theta_{0p})}{\theta_1 - \theta_{1p}}$$

In principle,  $V_1^*$  can be lower or higher than  $V_0^*$ . However, it seems sensible to assume that after titling the identification of right holders and neighbors makes judicial purging cheaper, so  $p_1 < p_0$ , and/or more effective in reducing the probability of eviction, so  $(\theta_1 - \theta_{1p}) > (\theta_0 - \theta_{0p})$ , given that some collisions of rights are purged by titling and most right holders and claimants are identified.<sup>13</sup> This results in  $V_1^* < V_0^*$ , as in Figure 3. In that case, after titling, owners will spend additional resources to protect their titles on land of relatively lower value. Thus, litigation is expected predominantly over low-value parcels, since under the customary regime parties had already clarified existing rights for parcels of higher value—for instance, by fencing their property, litigating with neighbors, or or-

<sup>13</sup> As argued in Arruñada (2012, p. 56), “Compared to privacy, deed recordation provides more possibilities for contracting the removal of defects, because defects are better known to buyers and insurers. The identification of right holders also gives greater security to the summary judicial hearings that serve to identify possible adverse claims and publicly reallocate *in rem* rights. These summary hearings continue to exist today in, for example, the French judicial purge and the US ‘quiet title’ suit. In addition to purging titles directly, the existence of such a court-ordered purging possibility also reduces bargaining costs indirectly by encouraging recalcitrant claimants to reach private agreements (Cabrillac and Mouly 1997, pp. 732–40).”



ganizing public ceremonies attended by the community for the conveyance and public notice of rights.<sup>14,15</sup>

We investigate whether in our sample land parcels of different values and productivity were affected differently in terms of changes in the conflict rate by the formalization of land rights. Accounting for the value of a land plot in rural African villages is a complex task, since the productivity and value of land parcels are characterized by substantial within-village variability (Beaman et al. 2015). Given that we lack data on land quality, we proxy land value by the level of market integration and wealth of the households possessing the land and thus rely on the evidence that wealthier villagers own more productive land, operate more in markets, and are more active in the market economy (Beaman et al. 2015; Fabbri 2021). We first collected data on the share of calories consumed in households from food purchased in the market (rather than self-produced). We classified those who reported purchasing food for more than half of the consumed calories from the market as respondents with a high level of market integration. We then reestimated the main model specification in Table 1 by separating and comparing within categories the effects of the reform for households characterized by high and low levels of market integration.

The results are reported in model 1 of Table 2. The baseline category is control households with a low level of market integration. The small and insignificant coefficient of the term Control  $\times$  High MI shows that the likelihood of experiencing conflicts is the same for control subjects characterized by high levels of market integration. Similarly, the coefficient of the term Treated  $\times$  High MI is not statistically different from coefficients of the baseline and high-market-integration control groups ( $F$ -test,  $p = .36$  and  $p = .26$ , respectively). What drives the esti-

<sup>14</sup> As suggested by a referee, if owners' protection efforts made high-value land relatively more secure before titling, then the reform should have produced the largest increase in tenure security for low-value land, and we should observe a larger increase in investments for low-value parcels. While we do not have data on investments for land parcels owned by participants in our sample, evidence from a World Bank survey collected in 2011 from owners of 5,634 land parcels in PFR-affected villages suggests that this seems to be the case (Goldstein 2011). We classify each parcel as high value or low value depending on whether its selling value as reported by the owner is above or below the sample average. We then compare as a proxy for investments whether trees were planted in the previous 12 months (tree planting is a long-term investment since trees take some years before starting production). In low-value parcels, the propensity to plant trees in the previous year is significantly higher for land parcels included in the PFR compared with non-PFR parcels (6.7 percent versus 4.4 percent; two-sided  $z$ -test,  $p = .01$ ). Conversely, in high-value land parcels tree planting is on average more frequent but equally likely for land parcels included in the PFR and for control parcels (8.2 percent versus 7.1 percent; two-sided  $z$ -test,  $p = .47$ ).

<sup>15</sup> Considering that subsidized titling is usually replaced by costly titling, owners may consider this higher future cost of titling. Again, assuming a fixed cost per parcel  $r$  of registering subsequent transactions, only parcels valued above a new threshold such as  $V_2^* = (r + p_2)(\theta_2 - \theta_{2p})$  between  $V_1^*$  and  $V_0^*$  would be registered and purged. Other break-even points are possible depending on the relative value of the parameters, most likely with some land among the three possibilities; that is, informal, registered but not purged, or registered and purged. What matters for our purposes is that, in anticipation of costly titling, voluntary purging would probably focus on land between  $V_2^*$  and  $V_0^*$  on relatively lower-value land.

Table 2  
Likelihood of Experiencing Land-Related Conflict: Heterogeneity Analysis

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Control × High MI	−.023 (.040)	−.024 (.042)				
Treated × Low MI	.082* (.032)	.100** (.034)				
Treated × High MI	.029 (.032)	.032 (.043)				
Control × High Income			.050* (.023)	.033 (.022)		
Treated × Low Income			.097** (.034)	.123** (.033)		
Treated × High Income			.083* (.032)	.069* (.030)		
Control × High Wealth					.025 (.020)	.030 (.020)
Treated × Low Wealth					.104** (.036)	.124** (.032)
Treated × High Wealth					.059+ (.032)	.067* (.031)
Constant	−.033 (.102)	−.055 (.115)	−.023 (.113)	−.065 (.126)	−.137 (.087)	−.077 (.084)
Sample	Full	Reduced	Full	Reduced	Full	Reduced
N	593	515	593	515	593	515

**Note.** Results are from ordinary least squares regressions; robust standard errors clustered at the village level are in parentheses. High and low levels of market integration (MI) are determined by whether the fraction of caloric intake from food purchased in the market is larger than the sample median. Income is self-reported weekly income. Wealth includes the following indicators: whether the house has a concrete floor, electricity, or radio or television; whether someone in the household owns a motorbike, car, bank account, or credit card; and whether the household has exclusive use of a land parcel. Individual controls include age, gender, religion, a measure of risk preferences, whether the respondent is married, whether the respondent is polygamous, a dummy for literacy, education level, and income. Village controls include population, distance from the closest paved road, a dummy for villages in the South, and a dummy for four villages that further developed the reform.

+ Significant at the 10% level.

\* Significant at the 5% level.

\*\* Significant at the 1% level.

mated increase in conflicts that we observe for subjects in the treated group is the sample of households with low levels of market integration. Compared with the baseline, for this group the increase in the likelihood of experiencing conflicts after the PFR was implemented is large and statistically significant (at the 5 percent level in model 1, in which we include the whole sample of participants, and at the 1 percent level in model 2, in which we exclude 78 households in treated villages that did not own land subject to the PFR).

We replicate the analysis using self-reported household income as a proxy for land value (in models 3 and 4) and an index of household wealth based on eight

indicators (in models 5 and 6).<sup>16</sup> In all cases and specifications, the qualitative results are the same. In particular, *F*-tests comparing treated and control respondents in the high-income conditions or in the high-wealth conditions find no statistically significant difference in the likelihood of experiencing conflicts. Conversely, comparing Treated  $\times$  Low Income with the baseline category Control  $\times$  Low Income or Treated  $\times$  Low Wealth with the baseline category Control  $\times$  Low Wealth returns differences in conflict rate that are statistically significant at the conventional level or better in all cases. To summarize, in our sample PFR titling caused a significant increase in litigation for respondents with low levels of market integration, income, and wealth—indicators that we use to identify them as owning low-value parcels. However, the reform had no effect on conflict for individuals owning high-value parcels.

## 6. Conclusion

The relationship between the formalization of land rights and litigation has sparked a heated debate, with some scholars arguing that well-defined land rights resolve ambiguous claims and prevent conflicts, while others hold that land demarcation favors the emergence of latent disputes, displaces the social safety net of collective tenure, and enhances litigation. We shed light on this topic by verifying empirically the effects of a land-rights-formalization program in rural Benin on land-related litigation 10 years after its implementation and by proposing an explanation for the observed effects based on the complementary character of formal titling and title-driven litigation.

Our identification strategy is based on random allocation of rights formalization across villages through a public lottery, which makes the Beninese PFR the first case of a large-scale land tenure reform implemented as an RCT. We find that, when looking at average effects over the full sample, formalization significantly increased the likelihood of experiencing land-related conflicts, roughly doubling households' litigation rate in the villages where the reform was implemented. Litigation mostly concerned parcels' boundaries and, importantly, did not increase violence. Through an identification strategy based on an unquestionably random allocation of titles, which is uncommon to observe in the literature, these results suggest that property rights for land affect land-related litigation.

We assert that the reform, by registering all land parcels in a village irrespective of their value and at the same time awarding incomplete land titles not fully purged, increased demand for litigation aimed to clarify existing rights. Consistent with this explanation, we find that formalization had no effect on the litigation rate of wealthier and more market-integrated households that are likely to own land parcels that, given their greater value, had already had titles purged

<sup>16</sup> Seven indicators are from the World Values Survey: whether the house has a concrete floor, electricity, or radio or television and whether someone in the household owns a motorbike, car, bank account, or credit card. We add an indicator of whether the household has exclusive access to and use of a land parcel.

before formal titling. Conversely, the estimated increase in litigation is concentrated in households characterized by lower levels of market integration and wealth, which are likely to own parcels of lower value whose titles were not privately profitable to purge under the customary system.

Some caveats are in order. First, it is worth emphasizing that the increase in litigation observed in villages where the reform was implemented is not necessarily an inefficient outcome. Indeed, litigation aimed at clarifying existing rights likely represent a positive step toward establishing more secure property rights. More generally, such increases seem to suggest that the desirability of litigation to clarify property rights depends on the situation under scrutiny and should be established on a case-by-case basis. For instance, while purging property titles through judicial means may increase land value and investments in a society endowed with well-functioning dispute resolution mechanisms, policy makers designing tenure reforms may want to take steps to mitigate the emergence of additional conflicts in contexts already plagued by social or ethnic tensions.

Second, the extent to which these results apply outside the context of our study requires some additional qualifications. Beninese villages are characterized by conditions that are common to most rural areas of other low-income countries worldwide, such as high costs to obtain formal land titles relative to parcel values and the consequent predominance of customary tenure. In addition, case studies and ethnographic evidence indicate how both in the French-speaking African context (Delville 2019) and in other African countries (Deininger and Castagnini 2006; Platteau 1996) land rights formalization and universal titling efforts can generate the same types of conflicts we observe in Benin. In this sense, we suggest that the argument presented in this article might be relevant for other low-income countries that are similar to rural Benin.

However, care must be exercised when applying our main argument to other contexts because, even if the environment of Benin is quite common in low-income countries, part of the results may hinge on the circumstances of its PFR intervention. In particular, two contextual variables may play an important role by affecting the presence of latent conflicts and the extent of private (pretitling) purging. On the one hand, in Benin all assets were land parcels and not buildings (the vast majority of buildings in these villages are mud structures and are not titled). Parcels are likely to be the source of more latent conflicts than buildings because of greater difficulties in physically demarcating them using identification and boundaries. Consequently, one could expect different results in urban areas. On the other hand, our results are consistent with the existence of land parcels with values heterogeneous enough to trigger different degrees of private purging before public titling. These differences could be absent in environments at an earlier stage of market integration and economic development, and therefore it is possible that litigation will be affected differently in such contexts. Moreover, in more economically developed areas where more valuable land has been used as collateral for credit, given the abstract nature of these mortgage-like property

rights, private purging is often out of the question. Consequently, it is conceivable that litigation around such abstract rights to higher-value land could increase after titling.

Our results contribute to the broader debate regarding the costs and benefits of universal versus selective land titling (Arruñada 2015; Connelly 2016). For the past few decades, governments in developing countries have usually introduced land titling on a universal basis, registering all parcels in a given area. However, more recently universal titling has been subject to scrutiny (Arruñada 2017; Bruce 2012; Connelly 2016), and some land-titling projects were redesigned accordingly (for example, Ali, Deininger, and Goldstein 2014; Ali, Deininger, and Duponchel 2017; Deininger et al. 2008). Here we contribute to the discussion by focusing on a specific externality of universal titling: given its emphasis on coverage and quantity, it leads to minimizing average cost and thus to sacrificing the quality of title and possibly an increase in the demand of complementary purging. Moreover, the standard policy of subsidizing initial formal titling may also cause a transitory surge in litigation if owners expect titling prices to continue to be subsidized in the future (for example, 0 instead of  $r$ ). Although our study is not aimed at assessing the (in-)efficiency of title-driven litigation, our findings emphasize the importance of taking this externality into account when designing tenure reforms and evaluating the consequences and trade-offs associated with different approaches to land rights formalization.

## Appendix

### Supplementary Analysis

Table A1  
Balance of Observable Characteristics across Experimental Groups

	2017 and 2020			2020		
	Treated ( <i>n</i> = 578)	Control ( <i>n</i> = 508)	Difference ( <i>p</i> -value)	Treated ( <i>n</i> = 306)	Control ( <i>n</i> = 287)	Difference ( <i>p</i> -value)
Male	.53	.52	.75			
Age	41.0	37.8	.01			
Muslim	.40	.39	.80			
Vodoun	.20	.18	.63			
Christian	.36	.37	.77			
Married	.88	.86	.24			
Polygam	.53	.45	.01			
Literate	.43	.38	.10			
Foodssatisfact	3.13	3.44	.01			
Housesatisfact	3.14	3.29	.08			
Healthsatisfact	3.01	3.17	.10			
Moneysatisfact	3.10	3.24	.10			
Bornvillage	.69	.72	.36			
Fracyearsinvil	.81	.79	.38			
Householdnr				9.84	9.78	.91
Managefinance				.99	.96	.03
Land (hectares)				4.94	5.74	.33
Rooms				3.88	3.51	.14
Concretefloor				.66	.59	.05
Electricity				.38	.35	.44
Water				.27	.18	.01
Radio-TV				.65	.61	.23
Car				.09	.06	.17
Moto				.82	.78	.24
Social-rank				4.43	4.33	.50

**Note.** The *p*-values are calculated using a two-sided *t*-test for continuous variables and a  $\chi^2$ -test for dummy variables.

**Table A2**  
**Robustness Check for Likelihood of Experiencing Land-Related Conflicts**

	Full Sample (N = 1,086)			Reduced Sample (N = 977)		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Treated	.078	.075	.101	.092	.091	.115
Confidence interval	[.02, .13]	[.019, .13]	[.04, .17]	[.03, .16]	[.03, .15]	[.05, .18]
p-Value	.009	.013	.003	.007	.005	.002
Individual controls	No	Yes	Yes	No	Yes	Yes
Village controls	No	No	Yes	No	No	Yes
Constant	.103* (.041)	.108 (.083)	.094 (.087)	.115* (.044)	.063 (.089)	.059 (.095)

**Note.** Results are from ordinary least squares regressions. Wild cluster bootstrapped robust standard errors estimated with 999 repetitions and clustered at the village level are in parentheses. The reduced sample excludes respondents in treated villages who do not own land. All regressions control for age and whether the respondent is polygamous. Individual controls include gender, religion, a measure of risk preferences, whether the respondent is married, a dummy for literacy, and income. Village controls include population, distance from the closest paved road, a dummy for villages in the South, and a dummy for four villages that further developed the reform.

\* Significant at the 5% level.

**Table A3**  
**Probit Regression for Likelihood of Experiencing Land-Related Conflict**

	All Conflicts		Boundaries		Inheritance	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Treated	.432** (.120)	.361* (.140)	.424* (.187)	.481** (.120)	.427** (.141)	.422* (.186)
Risk	.001 (.027)	-.009 (.029)	.015 (.039)	-.008 (.027)	-.008 (.030)	-.017 (.033)
Male	.278* (.119)	.331* (.137)	.119 (.186)	.266* (.133)	.330* (.149)	-.014 (.189)
Age	.005 (.004)	-.000 (.004)	.016** (.006)	.003 (.005)	-.001 (.004)	.011+ (.006)
School	-.049 (.169)	-.237 (.176)	.212 (.285)	-.006 (.180)	-.160 (.179)	.432 (.282)
Education	.077 (.048)	.091+ (.050)	-.000 (.067)	.073 (.052)	.087+ (.050)	-.033 (.069)
Log Income	-.024 (.030)	.018 (.035)	.184** (.064)	.007 (.035)	.012 (.039)	.193** (.071)
South	-.090 (.179)	-.362+ (.204)	1.146** (.321)	-.132 (.186)	-.395+ (.219)	1.060** (.322)
Constant	-1.415** (.364)	-1.381** (.456)	-5.128** (.734)	-1.527** (.405)	-1.319** (.490)	-4.967** (.810)

**Note.** Robust standard errors clustered at the village level are in parentheses. Individual controls include age, gender, religion, a measure of risk preferences, whether the respondent is married, whether the respondent is polygamous, a dummy for literacy, education level, and income. Village controls include population, distance from the closest paved road, a dummy for villages in the South, and a dummy for four villages that further developed the reform. N = 1,086.

+ Significant at the 10% level.

\* Significant at the 5% level.

\*\* Significant at the 1% level.



**Table A4**  
**Likelihood of Experiencing Boundary Conflicts**

	Full Sample ( <i>N</i> = 1,086)			Reduced Sample ( <i>N</i> = 977)		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Treated	.049 <sup>+</sup> (.027)	.048 <sup>+</sup> (.027)	.071* (.028)	.068* (.031)	.066* (.031)	.088** (.031)
Individual controls	No	Yes	Yes	No	Yes	Yes
Village controls	No	No	Yes	No	No	Yes
Constant	.115** (.035)	.055 (.081)	.095 (.089)	.119** (.039)	.057 (.093)	.101 (.100)

**Note.** Results are from ordinary least squares regressions; robust standard errors clustered at the village level are in parentheses. The reduced sample excludes respondents in treated villages who do not own land. All regressions control for age and whether the respondent is polygamous. Individual controls include gender, religion, a measure of risk preferences, whether the respondent is married, a dummy for literacy, and income. Village controls include population, distance from the closest paved road, a dummy for villages in the South, and a dummy for four villages that further developed the reform.

<sup>+</sup> Significant at the 10% level.

\* Significant at the 5% level.

\*\* Significant at the 1% level.

**Table A5**  
**Likelihood of Experiencing Conflicts over Land Inheritance**

	Full Sample ( <i>N</i> = 1,086)			Reduced Sample ( <i>N</i> = 977)		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Treated	.033 <sup>+</sup> (.018)	.030 <sup>+</sup> (.016)	.032 <sup>+</sup> (.016)	.031 <sup>+</sup> (.018)	.029 <sup>+</sup> (.016)	.032* (.016)
Individual controls	No	Yes	Yes	No	Yes	Yes
Village controls	No	No	Yes	No	No	Yes
Constant	-.000 (.024)	-.079* (.036)	-.170** (.046)	.018 (.025)	-.072* (.035)	-.152** (.045)

**Note.** Results are from ordinary least squares regressions; robust standard errors clustered at the village level are in parentheses. The reduced sample excludes respondents in treated villages who do not own land. All regressions control for age and whether the respondent is polygamous. Individual controls include gender, religion, a measure of risk preferences, whether the respondent is married, a dummy for literacy, and income. Village controls include population, distance from the closest paved road, a dummy for villages in the South, and a dummy for four villages that further developed the reform.

<sup>+</sup> Significant at the 10% level.

\* Significant at the 5% level.

\*\* Significant at the 1% level.

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