Land titling and litigation

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July 2021

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Abstract

We study a large-scale land titling reform implemented as a randomized control-trial to isolate its causal effects on litigation. The reform consisted of demarcating land parcels, registering existing customary rights, and granting additional legal protection to rightholders. We find that, ten years after implementation, the reform doubled the likelihood of households experiencing land-related litigation, but disputes do not escalate into more frequent violent episodes. We suggest that this litigation increase is likely to reflect 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 of these titles subject to adverse claims. This raised the demand for complementary litigation aimed at perfecting titles for low value parcels which, under the customary system, it was individually optimal to keep unclarified. Consistent with this explanation, we find that the observed increase in litigation takes place among households characterized by low levels of wealth and market integration, who are likely to own land of lower value.

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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 et al., 2003, De Soto, 2000). For example, one of the UN's Sustainable Development Goals for 2030 aims to "ensure that all [...] have access to [...] ownership and control over land and other forms of property", and then it estimates countries' achievements in this regard with an indicator measuring the "proportion of total adult population with secure tenure rights to land".[1] 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 these real assets as collateral for credit. To this end, in recent decades governments and international development agencies have implemented various types of interventions aimed at formalizing the existing set of customary tenure institutions which 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, have been peacefully resolved through institutions for dispute resolution, or have escalated into violence.[2] According to common wisdom, one of the benefits of formalization and a key factor for igniting the virtuous cycle of economic growth would be to reduce litigation (Holden et al., 2019). Commentators have hypothesized that clearly-demarcated plot boundaries and formally recorded legal rights would reduce the emergence of both conflicts between individuals regarding contested boundaries (Di Falco et al., 2020), colliding claims over land parcels (Fearon, 1998), and larger-scale social and ethnic conflicts over land, which often entail devastating consequences (André and Platteau, 1998). From this perspective, formal land titling can be seen as a substitute of litigation directed to solve conflicts over land.

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

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[1]UN Sustainable Development Knowledge Platform, https://sustainabledevelopment.un.org/sdg1, accessed July 15, 2020.

[2]To avoid repetitions, throughout the article we will use the terms "conflict" and "dispute" interchangeably when referring to a disagreement about land-related issues.

[3]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

concentrate titles in a few rent-seekers (Binswanger et al., 1995, Goldstein and Udry, 2008) and jeopardize the role that land plays as a redistributive system and social safety net (Deininger and Feder, 2009, Lavigne Delville, 2000). Finally, when there is disagreement between possessory and formal boundaries, the implicit "georeferencing" of plots via their cadastral ID and the cadastral boundaries assigned to such ID 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 when contradictions are made salient and registration efforts are not accompanied by a thorough clarification of land titles, this may motivate parties 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) that was implemented in Benin approximately ten years afterwards. The reform systematically identified customary rights over land parcels, demarcated parcel boundaries, and created public land registries, making it possible to sell or use registered rights as collateral and to defend them in formal 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 to 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 the next section, we describe in detail the RCT structure which randomly selects "treated" villages where the reform has been implemented and "control" villages where, as of today, customary land rights remain in place. To gather information regarding the effects of the PFR on land-related litigation, what types of conflicts have been experienced, and what dispute resolution mechanisms have been used, we administered two rounds of surveys to individuals respectively seven and ten years after the reform's implementation, conducting in-depth interviews with a sample of 1086 respondents across 43 randomly selected villages included in the RCT.

Our results show that, when looking at average effects over the whole sample, the formalization of land rights significantly increases the likelihood of land-related litigation. Point estimates suggest that participants in treated villages have doubled the probability to engage in land-related disputes. The majority of conflicts concerns contested parcel boundaries, but we observe also a significant increase in disputes related to land-inheritance. Participants who experienced the reform show only minor differences in the choice of the conflict resolution mechanism – customary, religious, of formal courts – and we do not observe significant changes of

the transition incomplete and result in the combined application of customary and formal law (Fitzpatrick, 2005). For example, according to Barrows and Roth, 1990, pp 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).

the frequency of conflict-related violence nor of 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 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 that it is observed after the reform. The starting consideration is that, in order to reduce the probability of suffering from adverse claims, rightholders can engage in private and public activities to clarify their existing rights and protect their title – that is, to "purge" their (formal or informal) ownership title. To this end, they can do private physical investments to better define and proclaim their rights, such as fencing parcels (Anderson and Hill, 1975, Hornbeck, 2010), demarcating boundaries (Libecap and Lueck, 2011), or enforce possession of the asset by physically "staying in place" (Field, 2007, Goldstein et al., 2018). They can also clarify their title using public means, by litigating against specific 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 USA (Bray, 2010).

In a customary system, parties who hold possession of land parcels can also keep their rights unclarified or 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, revealingly, the ritual planting of a shea or *karité* tree to proclaim ownership (Adjahouhoué, 2013), thus making the ownership claim public and inciting potential claimants to suit or implicitly concede. As in many developing countries, in recent times these proclamations also include posting ownership claims indicating the name and phone number of the owner, as well as fencing and building houses and wells or employing a guard. In addition, possessors resort to fully public means, from requesting local authorities to issue certificates merely attesting ownership, and having conveyances endorsed by the village chief in a "Certificate of non-litigation" or *Attestation de Non Litige* (Bierschenk and Olivier de Sardan, 2014); to starting expensive first registration procedures (*Inmatriculation*) at the land register created in colonial times (Lavigne Delville, 2019), an option that, given its high cost, was only suitable for the highest value land.

Both in places as different as Benin or the USA, and whatever private or public means are used, the economic logic remains the same. On the one hand, rightholders will make investments for clarifying existing rights only for land parcels whose value increases more than the purging costs – we name these "high-value parcels". On the other hand, right-holders will leave unpurged the rights possessed over "low-value parcels".

As we will explain in Section 2, the Beninese PFR, as most other titling efforts lately, is characterized by two features important for our argument: the reform titled all parcels, but

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imperfectly. First, all valuable land within a village was registered, irrespectively of a parcel's value. Second, the reform faced time and resource constraints which resulted in an incomplete purging of the land titles awarded. Certainly, the Beninese PFR included a procedure for purging land titles. However, commentators note that 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 are successfully claimed by absent parties at a later stage (Lavigne Delville and Moalic, 2019).

We advance the view that the activities subsidized by the PFR, such as demarcating boundaries and identifying owners, reduced the likelihood of eviction and therefore increased parcels' value. However, since the land titles awarded still remained incomplete, villagers who experienced the reform have greater demand for perfecting their ownership titles through litigation aimed at further clarifying property rights. In Section 5 we will elaborate on this point. This argument would be consistent with observing that the demand for purging titles affects mostly lower-value land parcels. This follows from the fact that, as explained above, rights-holders who possess higher-value parcels under the customary system had already made private investments to clarify their existing rights. Indeed, we report evidence that the observed increase in litigation is driven by treated households with low income levels and low market integration, who are likely to possess land parcels of relatively low value. Conversely, the effects on participants characterized by high income and greater market integration are small and insignificant, arguably because, before formalization, they had already invested to bettering the titles of their higher-value land.

It is worth emphasizing that the type of land-related litigation we observe here 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, making it possible to adjust the quality of the title to land value, and therefore introducing some flexibility into a system of universal titling. Moreover, in our sample we do not observe significant changes of the frequency of conflict-related violence in treated villages nor of 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 increase investments (Field, 2007, Galiani and Schargrodsky, 2010, Goldstein et al., 2018). In Section 6, we will come back to this discussion.

Our paper contributes to a growing empirical literature which 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 et al., 2000, Deininger and Castagnini, 2006, Holden et al., 2019) show that tenure formalization is associated with no reduction, or at times even an increase in conflicts over land. However, these research designs cannot account for endogeneity

and self-selection issues concerning villagers' decisions to title only specific land parcels which are more likely to be contested, or authorities' choices to roll over selective formalization programs involving only parcels or territories with comparatively high value.[4]

Only a few studies that investigate the institutions-litigation link have quasi-random allocation of titles across the sample of households, which lends more confidence in identifying causal relationships instead of mere correlations. Two studies which focus on the relationship between property rights and violence show a moderating effect of formalization on homicides rate. In the first article, Fetzer and Marden (2017) exploit spatial and temporal variation in the availability of forest land protected by natural conservation laws – which are therefore not vulnerable to requests for title 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.[5] The second study by Dower and Pfutze (2020) shows that land certification in Mexico reduces violent deaths. The authors provide evidence that the reduction in violence stems from formalization reducing politicians' discretion in the allocation of land rights and, as a consequence, in the amount of disputes. Our paper is complementary to these contributions because we collect data on the whole set of disputes over land, including both those escalating into episodes of violence and those resulting instead in non-violent contentions. In a recent contribution, 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 roll-out of a land rights certification program. The authors show that villagers who received formal land certificates have experienced significantly less land-related conflicts. We complement these findings on the immediate effects of formalization on conflicts by studying the medium-term effects of the intervention (i.e. 10 years after the implementation).[6]

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[4]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, it may happen that those with insecure titling (and therefore less incentive to invest) are more inclined to title (Arruñada, 2012), which could bias results toward underestimating a positive effect of titling.

[5]One important difference of Fetzer and Marden (2017) compared to our work is that, in the Brazilian Amazon region, to acquire the status of "protected forest land" implies severe limits on the possibilities of future uses, thus lowering the land's economic value. Therefore, in contrast with the Beninese reform, the intervention studied by Fetzer and Marden (2017) does not simply assign well-defined property rights, but also reduces the choice set of potential investors, leaving doubts about whether the estimated decrease in conflicts is driven by formalization or by the jointly-determined decrease in land value.

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

The paper is organized as follows. In the next section, 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 about the survey and the data collection. In Section 4 we present the results. Section 5 discusses the findings and suggests a general framework which connects land titling and conflicts. Section 6 concludes.

2   Institutional Framework

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

It is against the background of these problems related to insecurity of tenure that the Beninese Government launched a land tenure reform known as the Plan Foncier Rural (PFR). This Plan registered de facto private property rights after (1) mapping all parcels, (2) investigating the correspondence between parcels and right holders, and (3) registering the whole set of parcel right holders in each village. The PFR was introduced on a large scale in the period 2010-2011, when the Millennium Change Account (MCA) subsidized an implementation program and the Beninese government enacted a new law introducing Torrens-types title certificates (Goldstein et al., 2018).[7]

From the perspective of our empirical contribution, the key attribute of the PFR titling effort is that implementation followed a randomized control trial process involving hundreds of rural villages. In fact, this is the first case of a large-scale land tenure reform implemented as a randomized control trial. In the preliminary phase of the project, interested rural villages were informed about the PFR and were invited to apply in order to participate in a lottery. As a second step, each application received was examined to verify whether the village met certain eligibility criteria – such as being effectively located in a rural area. Among the 576 villages that applied for participating in the PFR lottery and were judged eligible, a subsample of 300

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[7]Some amendments of the original legal framework that supported the 2010-2011 implementation plan did not modify the validity of the formalization intervention. For instance, the Beninese government initially created the PFR certificates (certificat foncier rural – CFR onward), a document identifying the right holders as they appeared during the creation of the PFR by enacting the law of 2007-003 on land rights. The release of CFRs was suspended with the creation of a new Land Code in 2013 which reunified CFR and property titles in a unique ownership document and confirmed the legal validity of PFR registered rights. So for our purpose the relevant title effort is the MCA-financed program.
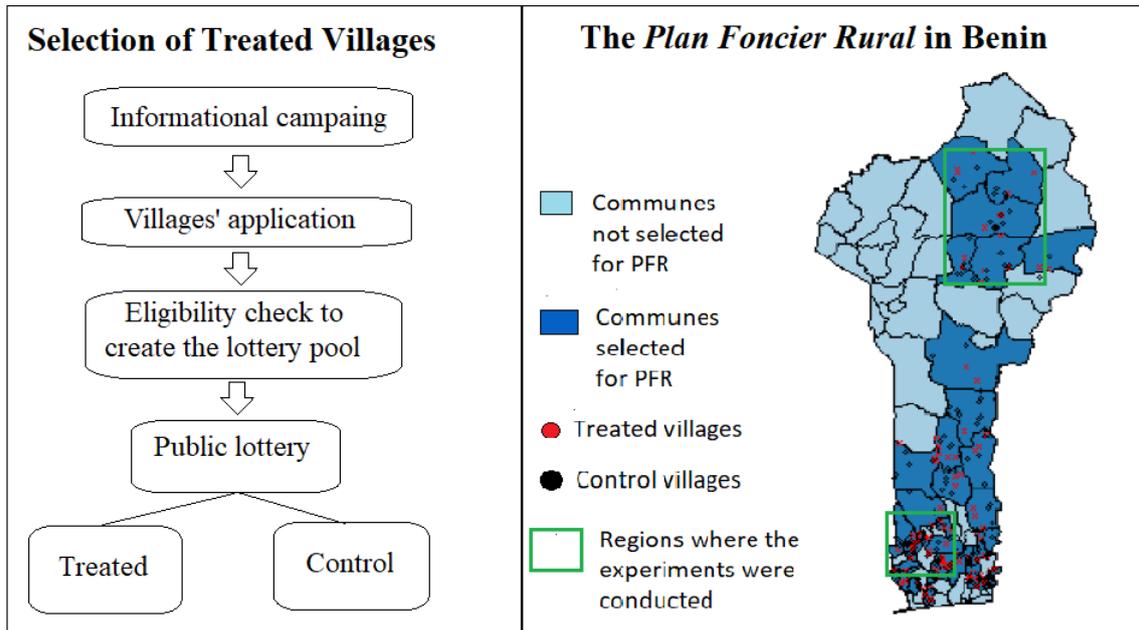
Figure 1: The lottery procedure characterizing the Plan Foncier Rural in Benin (left panel) and the resulting distribution of control and treated villages (right panel)

villages was randomly chosen via public lottery. Consequently, in 2010-2011, a team of local experts implemented the PFR in these selected villages (the "treated" group). The remaining non-selected villages (the "control" group) did not receive any intervention and, as of today, continue to have customary land rights. Figure 1 shows the lottery procedure characterizing the Beninese 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 at universal demarcation of boundaries and rights in each treated village, meaning that all valuable land within a village was registered, irrespectively of a parcel's value. Second, it intended to consider both the importance of the physical demarcation of boundaries – by marking them with cornerstones – and of legal demarcation of rights – by conditioning registration to gathering the public consent of neighbors of 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 (Lavigne Delville and Moalic, 2019).

3   Research Design

Our research design is based on the RCT implementation of the Plan Foncier Rural reform. We randomly selected from the whole list of villages included in the PFR and that are located in two provinces in the north of the country (Mono and Couffou) and two provinces in the south (Alibori and Borgou) the 43 villages where our data collection took place. To isolate the causal

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effects of land titling on litigation, we compare conflicts experienced by residents in (a) villages selected for PFR titling, against (b) those in control villages which were not chosen for the PFR and have therefore remained under customary titling.

Three caveats are in order. First, pre-treatment 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 pre-existing differences in land-related litigation across treatment groups. An impact evaluation of the original PFR reform shows that the randomization of the PFR lottery was successful (Goldstein et al., 2015). Moreover, as we show in the next session, participants in our sample are well-balanced on observables, thus increasing confidence in the validity of our approach. Second, 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 implementation). As explained in the next section, we have verified that migrating out of the village of origin is rare for participants in our sample and that migration flows are similar across treatment branches. Finally, official statistics relative to land-related litigation in Benin are available only for those disputes which are resolved through the formal judiciary. This is a small fraction of the total number of conflicts experienced by rural villagers since, as we elaborate in the Results section, customary-informal and religious dispute resolution mechanisms coexist with state courts. Therefore, to avoid possible non-classical measurement errors and to gather a comprehensive picture of all land-related conflicts, the data that we analyze in our study 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 (thus approximately seven years after the reform implementation) and the second at the beginning of 2020 (ten years later). In total, we held 65 fieldwork sessions (32 in 2017) visiting 43 villages (24 treated) and interviewing 1,086 individual households (493 in 2017).[8] The survey collected socio-demographic information and asked a set of questions related to land-related disputes. In particular, the question that we use to answer our main research question asked participants whether they had experienced at least one conflict related to land after 2010 and, if so, which type of dispute(s) it was.[9] The survey round of 2020 included exactly the same questions that were asked in 2017, plus an additional set of questions on details relative to market integration,

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

[9]Specifically, participants were asked whether there was a conflict related to boundaries, inheritance of land, second sales, expropriation by the state, or other types of conflicts.

the conflict resolution mechanism and the solution of the dispute (we will come back to this point in the Section 5).

In both survey rounds, the procedure to collect data worked as follows. In the days before the session, a research assistant visited the village and requested residents to gather for the scheduled day to be part of a research project. On the established day, we then randomly selected among the convened people 18 individuals to participate in the data collection. Those not selected received a show-up fee equal to 500 CFA (roughly \$0.9) and were requested to leave.[10] The research assistant first checked that each participant was a resident in the village, older than 18 years old, and that no other members of household were being surveyed. Then, each participant individually and in private answered the questions posed by the research assistant, and performed additional fieldwork activities unrelated to this project. Administering the survey once took about forty minutes.

4   Results

In Table A1 in Appendix A we compare the observables elicited in the post-experimental survey across the treatment branches. The samples are well-balanced, with the exception of participants in the treated sample being on average slightly older and more likely to be in a polygamous marriage. Moreover, in the sample of participants surveyed in 2020 for whom we collected additional data, we have some minor differences in the likelihood of managing household's money, and having a concrete floor and running water at home. In order for our identification strategy to hold, we needed to verify that, after the reform implementation, participants had not self-selected through migration into one of the treatment branches. To do so, we collected data regarding participants' village of origin, the number of years they have been living in the village, and the eventual reason leading to migration. The vast majority of participants reside in the same village where they were born, and the likelihood of having migrated is the same across treatment branches (69% in treated and 72% in control, χ^2 test p>10%). The majority of migrations were reported by female participants, and marriage is the reason commonly declared for the move. Similarly, we verified that there is no statistically significant difference across treatments between the fraction of adult life a participant had spent in the village where she took part to the data collection (t-test two sided, p>10%).

As a first step of the analysis, we look at the likelihood of experiencing land-related litigation after the implementation of the PFR reform as reported by survey respondents. The upper panel of Figure 2 displays 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 (Chi-square test, $p < 1\%$). The bottom panels

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[10]The majority of the data collection sessions involved exactly 18 participants, however, due to logistical constraints and specific circumstances, the session number varied between 12 and 20 individuals.
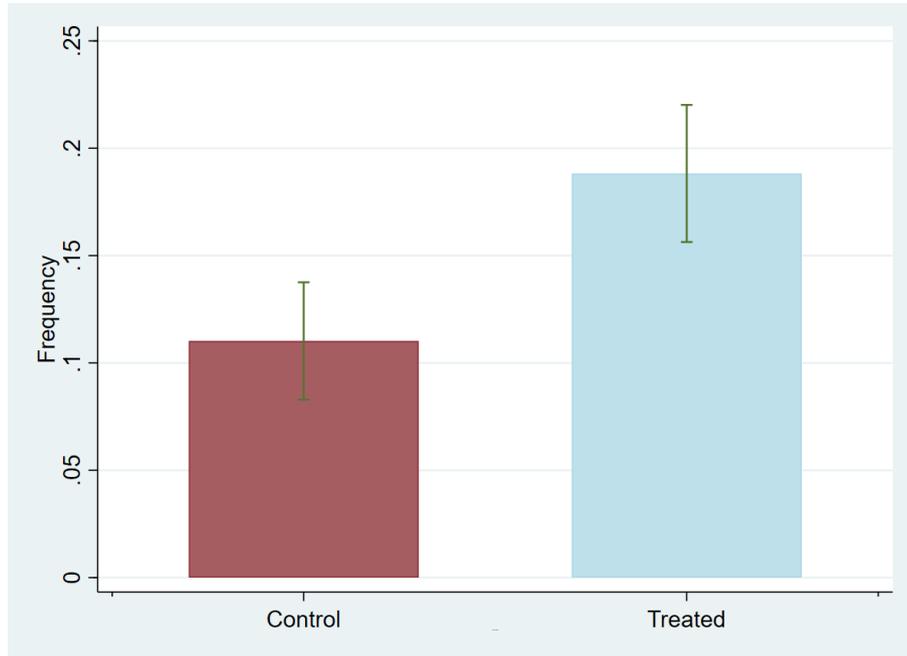
(a) Total Land-Related Conflicts
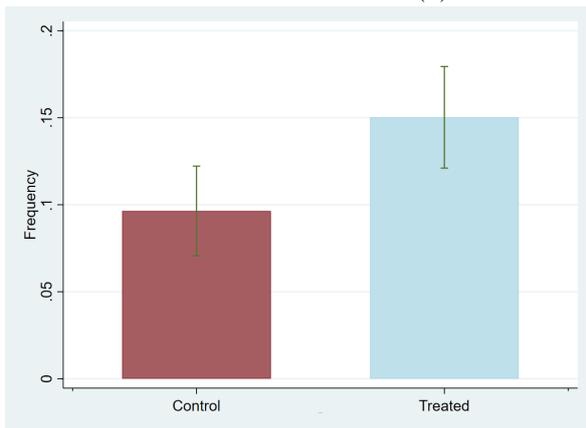
(b) Parcel Boundary Conflicts
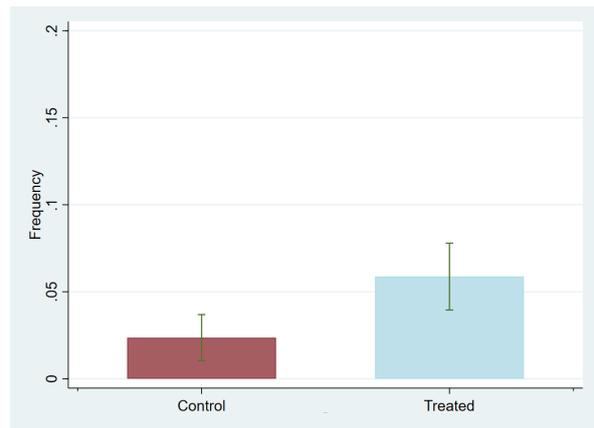
(c) Land Inheritance Conflicts

Figure 2: Land-Related Conflicts – Whole Sample

of Figure 2 show that the majority of disputes arise because of contested parcel boundaries (panel 2b) and, to a lesser extent, because of inheritance reasons (panel 2c). In both cases, the likelihood of experiencing these types of conflicts is significantly larger in treated than in control villages (Chi-square test, $p < 1\%$ in both cases).

Secondly, we verify these findings in a regression framework. Table 1 reports the results of a linear probability model.[11] In all the model specifications the dependent variable is a

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[11]We report in the main text the results of a linear probability model which simplifies the interpretation of the coefficients. We re-estimated all the regression presented also using a non-linear Probit regression in Table A3 in Appendix A. Results remain qualitatively the same.

dummy equal to one if a participant reported to have experienced a land-related litigation after 2010. Standard errors are clustered at the village level to account for possible levels of intra-village correlation. Model 1 controls for the two observables 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% level. The point estimate suggests that participants who had the PFR reform implemented are approximately 75% more likely to experience land-related conflicts. Model 2 additionally controls for the individual characteristics we collected in the post-experimental survey.[12] The coefficient of the treatment dummy remains positive and strongly significant, and point estimates very similar. Model 3 includes further controls for village characteristics. The coefficient remains strongly significant 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 who took part in the survey but who do not own land parcels that have been affected by the PFR. This could happen for various reasons, for example because all the land belonging to the household is located outside the village borders – and so not included in the PFR – or because the respondents' household does not own land. Models 4-6 replicate models 1-3 by excluding those participants from the sample. All coefficients of the treatment dummies are positive and strongly statistically significant. The estimated increase in the probability to litigate is larger than when the whole sample of participants is considered, ranging between 80% and 190%. In Table A2 in Appendix A, as a robustness check we re-estimated the model specifications presented in Table 1 by using Wild clustered bootstrapped standard errors with 999 repetitions. The qualitative results remain the same.

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

Our next step consists in zooming into the sub-sample of 594 participants surveyed in 2020, who answered a supplementary set of questions that make it possible to explore possible channels through which the PFR reform determined the observed increase in litigation. Those who experienced conflicts in the period following the PFR implementation were asked questions regarding how the disputes had been managed. Participants in the treated group reported a slightly longer average duration of the litigation process compared to the control group (30

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[12]The controls are: gender, religion, estimated measure of risk preferences, whether a subject is married, a dummy for literacy, income.

Table 1: Likelihood of Experiencing Conflicts

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:		Whole			Reduced	
treated	0.078***	0.075***	0.101***	0.092***	0.091***	0.115***
	(0.028)	(0.027)	(0.029)	(0.030)	(0.030)	(0.030)
Ind-Ctrl	N	Y	Y	N	Y	Y
Vil-Ctrl	N	N	Y	N	N	Y
Constant	0.103**	0.108	0.094	0.115**	0.063	0.059
	(0.041)	(0.083)	(0.087)	(0.044)	(0.089)	(0.095)
N.obs.	1086	1086	1086	977	977	977

Notes: Dependent variable: dummy equal to 1 if reported to have experienced a land-related conflict. OLS regressions. Standard errors robust for clustering at the village level. Models 1-3 include the whole sample, models 4-6 exclude from the sample participants in treated villages who do not own land. All regressions control for age and whether polygamous. Ind-Ctrl includes: gender, religion, estimated measure of risk preferences, whether a subject is married, a dummy for literacy, income. Vil-Ctrl includes: village population, village distance from the closest paved road, a dummy for villages in the South, a dummy for four villages that had further developed the original PFR. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

vs. 22 months, respectively), albeit the difference is not statistically significant. In the treated sample, 50% of respondents reported to have solved the dispute, a marginally significant lower amount than the 78% of control (Chi-square test, $p=8\%$). Participants were also asked whether the conflict they eventually experienced involved violence, and what is in their opinion the likelihood that an hypothetical land-related dispute may escalate in a violent episode (on a Likert scale from 1-7, where 1 represents the lowest probability of violence). In both cases, participants report a very similar frequency of violent episodes experienced in treatment and control, and beliefs regarding the likelihood of an escalation into violent conflicts that do not different across treatment groups (2.53 treated vs. 2.63 control, Kruskal-Wallis test $p>10\%$).

We check whether villagers who experienced the reform made different choices of the conflict resolution mechanism. Specifically, we asked them which conflict resolution mechanism among formal state courts, village authorities, or religious authorities they initially referred to. The local village authorities were first approached by the majority of respondents in both treatment groups (63% in treated and 72% in control, $p >10\%$). However, the majority of participants who reportedly had solved the conflict stated that the final adjudication was done by a formal state tribunal – with again no significant differences between treatment groups (73% in treated and 69% in control). This is in line with the predominant beliefs shared by the whole sample of participants in the 2020 survey, who reported that formal state courts have the last word in case of different adjudication outcomes reported by the three conflict resolution mechanisms. Specifically, of the 594 participants, 77% in treated and 70% in control consider the decision of a formal state tribunal as unappealable (the difference is not statistically significant at the conventional level). Similarly, 83% of participants in both groups reported believing that state

tribunals can be used as an "appeal court" by litigants who are not satisfied with the judgement 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' choice or perception concerning which conflict resolution mechanism should address land-related disputes, thus suggesting 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 will use some simple algebra to clarify the argument. Following Arruñada and Garoupa (2005), let us assume that title defects are represented by the probability θ 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 θ_0 but, as explained in the introduction, even if land remains informal owners could always spend resources to protect their title by different activities, such as planting a *karité* tree, fencing the parcels, requesting an ownership certificate from the village chief, litigating the boundaries of a neighbouring parcel, or clarifying ownership against all potential claimants in procedures functionally similar to the "quiet title suit" used in the USA (Bray, 2010).

Figure 3 represents the value of land when title conflicts exist as a function of its value V that would correspond to an ideal world without conflicting claims (represented on the horizontal axis), and 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 their title safer by reducing the probability of losing the land to θ_{0p}. When deciding whether to purge or not their 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:

$$\text{V}_0^* = p_0/(\theta_0 - \theta_{0p})$$

Therefore, it is worthwhile for owners of informally held land to purge their titles if the parcel value is higher than V_0^*; while it is not worthwhile for land values lower than V_0^*. When the government introduces land titling it usually does so – and it did so in Benin – for all relevant parcels independently of their value and without cost for owners, but imperfectly, so that the probability of eviction after titling, θ_1, still remains positive even if lower than θ_0. This

decrease in the probability of eviction $(\theta_0\text{-}\theta_1)$ is what causes the increase in land value driven by formalization, increasing the slope of the value line in Figure 3.[13] Again, owners can still 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 title safer by reducing the probability of losing the land from θ_1 to θ_{1p}. When deciding whether to purge it or not, owners will compare the value of the titled land with formal titling but without additional purging, given by $(1\text{-}\theta_1)V$, with the value after titling and purging, given by $(1\text{-}\theta_{1p})V\text{-}p_1$. The new break-even point is now:

$$\text{V}_1^* = p_1/(\theta_1 - \theta_{1p})$$

and both break-even points are related by:

$$\text{V}_1^*/V_0^* = (p_1/p_0)(\theta_0 - \theta_{0p})/(\theta_1 - \theta_{1p})$$

In principle, V_1^* can be lower or higher than V_0^*. However, it seems sensible to assume that after titling the identification of rightholders and neighbors makes judicial purging cheaper, so that $p_1 < p_0$; and/or more effective in reducing the probability of eviction, so that $(\theta_1\text{-}\theta_{1p}) > (\theta_0\text{-}\theta_{0p})$, given that some collisions of rights have been purged by titling and most rightholders and claimants have been identified.[14] This results in $V_1^* < V_0^*$, as represented in Figure 3. In that case, after titling, owners will spend additional resources to protect their titles on relatively lower-value land. Thus, litigation is expected to happen predominantly among those low-value parcels, since under the customary regime parties had already clarified existing rights for higher-

[13]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 village. The 82 respondents from villages in which the PFR had been implemented reported a renting price 31% higher compared to participants in villages without PFR (two-sided t test, p=.02). Similarly, respondents in treated villages reported a hypothetical selling price for such a land parcel that is 14% higher than the one stated by participants in control ones - albeit in this case the difference is not statistically significant (two-sided t test, p=.39).

[14]As argued by 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 rightholders 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)".

value parcels – for instance, by fencing their property, litigating with neighbors, or by organizing public ceremonies attended by the whole community for the conveyance and public notice of rights.[15],[16]

We investigate whether, in our sample, land parcels of different value and productivity were affected differently in terms of changes in 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 we know that 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 qualities, we proxy land value by looking at the level of market integration and wealth of the households possessing it, thus relying 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, 2018). We then first collected data on the share of calories consumed in the households that were purchased in the market (rather than self-produced). We classified as "High-Market Integrated" those respondents who reported purchasing in the market more than half of the consumed calories. We then re-estimated the main model specification presented in Table 1 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 are control households with low market integration. The small and insignificant coefficient of the interaction term *Control*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 interaction term *Treated*High-MI* is not statistically different from those of the baseline and high-market integration control groups (F-test, $p=0.36$ and $p=0.26$, respectively). What drives the estimated increase in conflicts that we observe for subjects in the treated group is the sample

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[15] 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 these 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 (Goldstein, 2011) collected in 2011 from owners of 5,634 land parcels in PFR villages suggests that this seems to be the case. To observe it, we classify each parcel either as "high-value" or "low-value" depending on whether its selling value as reported by the owner is above or below the sample average, respectively. We then compare as a proxy for investments whether trees were planted in the previous twelve 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 to non-PFR parcels (6.7% vs. 4.4%, two-sided z tests 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% vs. 7,1%, two-sided z test p=.47).

[16] 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 $V_2^* = (r+p_2)/(\theta_2-\theta_{2p})$ falling 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 falling within the three possibilities: i.e., informal, registered but not purged and registered as well as 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.
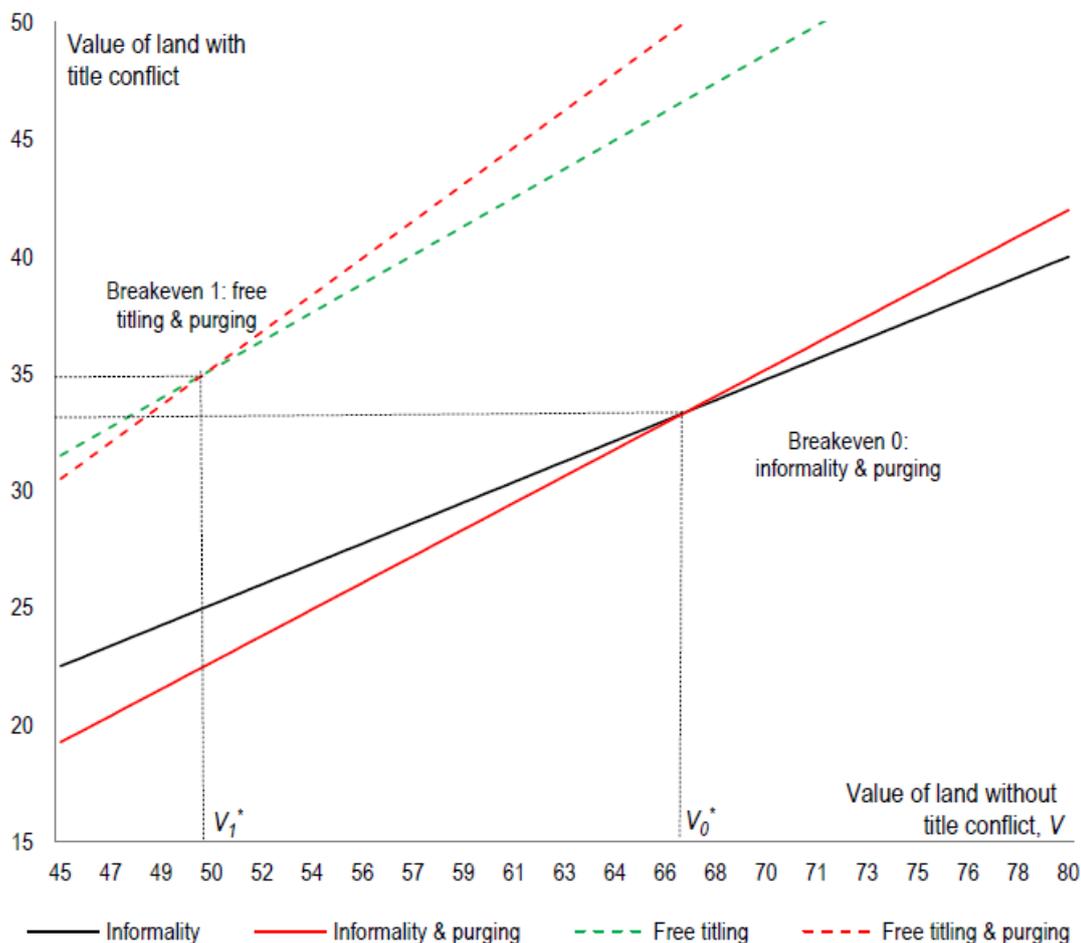
Figure 3: Four possible functions of land values depending on the availability of titling institutions and owners' additional voluntary protection of their property rights.

of households characterized by low market integration. Compared to the baseline, for this group the increase in the likelihood of experiencing conflicts after the PFR has been implemented is large and statistically significant (at 5% in model 1, in which we include the whole sample of participants, and at 1% level in model 2 in which we exclude 78 households in treated villages who did not own land subject to the PFR).

We replicate the analysis by 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 different indicators (in models 5 and 6).[17] In all cases and model specifications, the qualitative results remain the same. Specifically, F-tests comparing treated and control respondents in the high-income (models 3 and

[17]Seven indicators are taken by the World Value Survey: whether the house has concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card. We also add an indicator of whether the household has exclusive access and use of a land parcel.

Table 2: Likelihood of Experiencing Conflicts - Heterogeneity Analysis

| | Model 1 | Model 2 | Model 3 | Model 4 | Model 5 | Model 6 |
Sample:	Whole	Reduced	Whole	Reduced	Whole	Reduced
Ctrl*H-MI	-0.023	-0.024				
	(0.040)	(0.042)				
Trtd*L-MI	0.082**	0.100***				
	(0.032)	(0.034)				
Trtd*H-MI	0.029	0.032				
	(0.032)	(0.043)				
Ctrl*H-Inc			0.050**	0.033		
			(0.023)	(0.022)		
Trtd*L-Inc			0.097***	0.123***		
			(0.034)	(0.033)		
Trtd*H-Inc			0.083**	0.069**		
			(0.032)	(0.030)		
Ctrl*H-W					0.025	0.030
					(0.020)	(0.020)
Trtd*L-W					0.104***	0.124***
					(0.036)	(0.032)
Trtd*H-W					0.059*	0.067**
					(0.032)	(0.031)
Constant	-0.033	-0.055	-0.023	-0.065	-0.137	-0.077
	(0.102)	(0.115)	(0.113)	(0.126)	(0.087)	(0.084)
N.obs.	593	515	593	515	593	515

Notes: Dependent variable: Experience of land-related conflicts (dummy). OLS regressions. Standard errors robust for clustering at the village level. MIcal: fraction of calories intake that was purchased in the market is larger than the sample median; Inc: self-reported weekly income; Wealth combination of eight indicators of households' wealth (whether the house has a concrete floor, electricity, radio or television, whether within the household somebody owns a motorbike, a car, a bank account or a credit card, whether the household have exclusive use of a land parcel). Controls include: age, gender, religion, estimated measure of risk preferences, whether a subject is married, whether polygamous, a dummy for literacy, level of education completed, income, village population, village distance from the closest paved road, a dummy for villages in the South, a dummy for four villages that had further developed the original PFR. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

4) or in the high-wealth (models 5 and 6) conditions find no statistically significant difference in the likelihood of experiencing conflicts. Conversely, comparing *Treated*Low-Inc* against *Control*Low-Inc* (models 3 and 4) or *Treated*Low-Inc* against *Control*Low-Inc* (models 5 and 6) returns differences in conflict rate 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 characterized by low levels of market integration, income, and wealth – indicators that we use to identify them as owning low-value land parcels. However, the reform had no effect on conflicts for individuals owning high-value land parcels.

6 Conclusion

The relationship between land rights formalization and litigation has sparked a heated debate, with some scholars arguing that well-defined land rights resolve ambiguous claims and prevent conflicts, while others holding that land demarcation favours the emergence of latent disputes and displaces the social safety net of collective tenure, enhancing 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 ten 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 the random allocation of the 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 randomized control-trial. We find that, when looking at average effects over the whole 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 parcel boundaries and, importantly, did not increase violence. These results suggest that property rights in land affect land-related litigation by using an identification strategy based on an unquestionably random allocation of titles which is uncommon to observe in the literature.

We put forward that the reform, by registering all land parcels in a village irrespectively of their value and at the same time awarding incomplete land titles not fully purged, increased demand for litigation aimed at clarifying existing rights. Consistent with this explanation, we find that formalization determined no effect on the litigation rate of wealthier and more market-integrated households who are likely to own land parcels which, given their greater value, had already been purged before formal titling. Conversely, the estimated increase in litigation is concentrated on those households characterized by lower levels of market integration and wealth, who are likely to own lower-value land parcels whose title was 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 episodes aimed at clarifying existing rights likely represent a positive step toward establishing more secure property rights. More generally, such increase seems to suggest that the desirability of litigation aimed at clarifying property rights depends on the specific situation under scrutiny and should be established on a case-by-case basis. For instance, while the purging through judicial means of property titles may increase land value and investments in a society endowed with well-functioning dispute resolution mechanisms, policymakers designing tenure reforms may want to take steps in order to mitigate the emergence of additional conflicts in contexts already plagued by pre-existing social or ethnic tensions.

Second, the extent to which results presented here also apply outside the specific context of our study requires some additional considerations. The reality of Beninese villages is characterized by key aspects for our argument which 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. Additionally, case studies and ethnographic evidence have already pointed out how both in the French-speaking African context (Lavigne 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 observed in Benin. In this sense, we suggest that the argument presented in this paper might be relevant also for other low-income countries which share similar characteristics with 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 the Benin PFR intervention. In particular, two contextual variables may be playing an important role, by affecting both the presence of latent conflicts and the extent of private (pre-titling) purging. On the one hand, in the Benin case, all assets were land parcels with no buildings being titled (buildings in these Benin villages are for the vast majority mud structures and were not titled). Parcels are likely to suffer more latent conflicts than buildings because of greater difficulties for physical demarcation, both in terms of identification and definition of boundaries. Consequently, one could expect different results in urban areas. On the other hand, it is assumed, and 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 had 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 on 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 an 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 (e.g. Ali et al., 2014, 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, possibly leading to an increase in the demand of complementary purging. Moreover, the

standard policy of subsidizing initial formal titling may also cause a somehow transitory surge in litigation if owners expect that titling prices will continue to be subsidized in the future (e.g. zero instead of r). While, as said above, our study is not aimed at assessing the (in-)efficiency of title-driven litigation, our findings emphasize the importance of taking into account this externality when designing tenure reforms and evaluating consequences and trade-offs associated with different approaches to land rights formalization.

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Appendix A   Supplementary Analysis

Table A1: Balance of Observables Across Treatment Groups (t test two-sided for continuous variable and Chi-square test for dummy variables)

Sample:	2017+2020			2020		
	Treated	**Control**	**Diff**	**Treated**	**Control**	**Diff**
	(n=578)	(n=508)	(p-value)	(n=306)	(n=287)	(p-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			
foodsatisfact	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(hect)				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

Table A2: Likelihood of Experiencing Conflicts - Wild Cluster Bootstrapped S.E.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:		Whole			Reduced	
treated	0.078	0.075	0.101	0.092	0.091	0.115
C.I.	[.02, .13]	[.019, .13]	[.04, .17]	[.03, .16]	[.03, .15]	[.05, .18]
p-value	.009	.013	.003	.007	.005	.002
Ind-Ctrl	N	Y	Y	N	Y	Y
Vil-Ctrl	N	N	Y	N	N	Y
Constant	0.103**	0.108	0.094	0.115**	0.063	0.059
	(0.041)	(0.083)	(0.087)	(0.044)	(0.089)	(0.095)
N.obs.	1086	1086	1086	977	977	977

Notes: Dependent variable: dummy equal to 1 if reported to have experienced a land-related conflict. OLS regressions. Wild cluster bootstrapped standard errors estimated with 999 repetitions robust for clustering at the village level. Models 1-3 includes the whole sample, models 4-6 exclude from the sample participants in treated villages who do not own land. All regressions control for age, whether polygam. Ind-Ctrl includes: gender, religion, estimated measure of risk preferences, whether a subject is married, a dummy for literacy, income. Vil-Ctrl includes: village population, village distance from the closest paved road, a dummy for villages in the South, a dummy for four villages that had further developed the original PFR. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

Table A3: Likelihood of Experiencing Conflicts – Whole Sample, Probit Regression

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
treated	0.432***	0.361**	0.424**	0.481***	0.427***	0.422**
	(0.120)	(0.140)	(0.187)	(0.120)	(0.141)	(0.186)
risk	0.001	-0.009	0.015	-0.008	-0.008	-0.017
	(0.027)	(0.029)	(0.039)	(0.027)	(0.030)	(0.033)
male	0.278**	0.331**	0.119	0.266**	0.330**	-0.014
	(0.119)	(0.137)	(0.186)	(0.133)	(0.149)	(0.189)
age	0.005	-0.000	0.016***	0.003	-0.001	0.011*
	(0.004)	(0.004)	(0.006)	(0.005)	(0.004)	(0.006)
school	-0.049	-0.237	0.212	-0.006	-0.160	0.432
	(0.169)	(0.176)	(0.285)	(0.180)	(0.179)	(0.282)
education	0.077	0.091*	-0.000	0.073	0.087*	-0.033
	(0.048)	(0.050)	(0.067)	(0.052)	(0.050)	(0.069)
logincome	-0.024	0.018	0.184***	0.007	0.012	0.193***
	(0.030)	(0.035)	(0.064)	(0.035)	(0.039)	(0.071)
south	-0.090	-0.362*	1.146***	-0.132	-0.395*	1.060***
	(0.179)	(0.204)	(0.321)	(0.186)	(0.219)	(0.322)
Controls	Y	Y	Y	Y	Y	Y
Constant	-1.415***	-1.381***	-5.128***	-1.527***	-1.319***	-4.967***
	(0.364)	(0.456)	(0.734)	(0.405)	(0.490)	(0.810)
N.obs.	1086	1086	1086	1086	1086	1086

Notes: Dependent variable: models 1 and 4 all land-related conflicts; models 2 and 5 conflicts relative to parcel borders; models 3 and 6 conflicts relative to land inheritance. Probit regressions. Standard errors robust for clustering at the village level. Controls include: age, gender, religion, estimated measure of risk preferences, whether a subject is married, whether polygam, a dummy for literacy, level of education completed, income, village population, village distance from the closest paved road, a dummy for villages in the South, a dummy for four villages that had further developed the original PFR. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

Table A4: Likelihood of Experiencing Parcel Boundary Conflicts

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:		Whole			Reduced	
treated	0.049*	0.048*	0.071**	0.068**	0.066**	0.088***
	(0.027)	(0.027)	(0.028)	(0.031)	(0.031)	(0.031)
Ind-Ctrl	N	Y	Y	N	Y	Y
Vil-Ctrl	N	N	Y	N	N	Y
Constant	0.115***	0.055	0.095	0.119***	0.057	0.101
	(0.035)	(0.081)	(0.089)	(0.039)	(0.093)	(0.100)
N.obs.	1086	1086	1086	977	977	977

Notes: Dependent variable: dummy equal to 1 if reported to have experienced a conflict related to parcels' boundaries. OLS regressions. Standard errors robust for clustering at the village level. Models 1-3 includes the whole sample, models 4-6 exclude from the sample participants in treated villages who do not own land. All regressions control for age, whether polygam. Ind-Ctrl includes: gender, religion, estimated measure of risk preferences, whether a subject is married, a dummy for literacy, income. Vil-Ctrl includes: village population, village distance from the closest paved road, a dummy for villages in the South, a dummy for four villages that had further developed the original PFR. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.

Table A5: Likelihood of Experiencing Conflicts over Land Inheritance

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Sample:		Whole			Reduced	
treated	0.033*	0.030*	0.032*	0.031*	0.029*	0.032**
	(0.018)	(0.016)	(0.016)	(0.018)	(0.016)	(0.016)
Ind-Ctrl	N	Y	Y	N	Y	Y
Vil-Ctrl	N	N	Y	N	N	Y
Constant	-0.000	-0.079**	-0.170***	0.018	-0.072**	-0.152***
	(0.024)	(0.036)	(0.046)	(0.025)	(0.035)	(0.045)
N.obs.	1086	1086	1086	977	977	977

Notes: Dependent variable: dummy equal to 1 if reported to have experienced a conflict related to land inheritance. OLS regressions. Standard errors robust for clustering at the village level. Models 1-3 includes the whole sample, models 4-6 exclude from the sample participants in treated villages who do not own land. All regressions control for age, whether polygam. Ind-Ctrl includes: gender, religion, estimated measure of risk preferences, whether a subject is married, a dummy for literacy, income. Vil-Ctrl includes: village population, village distance from the closest paved road, a dummy for villages in the South, a dummy for four villages that had further developed the original PFR. Symbols $***$, $**$, and $*$ indicate significance at the 1%, 5% and 10% level, respectively.