

# Land Titling and Litigation\*

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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.

**JEL-Classification:** K11; K4; Q15

**Keywords:** Experimental Survey; Informal Institutions; Land Rights Formalization; Land Tenure Reform; Litigation; Randomized Control Trial

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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”.<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 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.<sup>2</sup> 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).<sup>3</sup> Moreover, registration can

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<sup>1</sup>UN Sustainable Development Knowledge Platform, <https://sustainabledevelopment.un.org/sdg1>, accessed July 15, 2020.

<sup>2</sup>To avoid repetitions, throughout the article we will 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

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

62 We shed light on how land titling affects litigation by studying the effects of a land tenure  
63 reform named “Plan Foncier Rural” (PFR) that was implemented in Benin approximately ten  
64 years afterwards. The reform systematically identified customary rights over land parcels, de-  
65 marcatd parcel boundaries, and created public land registries, making it possible to sell or use  
66 registered rights as collateral and to defend them in formal court against contenders. Our main  
67 contribution consists in isolating the causal effects that the reform had on land-related litigation  
68 by implementing a research design that dispels endogeneity concerns commonly associated to  
69 the titling of land. The identification strategy is based on the process of implementation char-  
70 acterizing the Beninese PFR which, to the best of our knowledge, is the first case of large-scale  
71 land tenure reform implemented as a randomized control-trial (RCT). In the next section, we  
72 describe in detail the RCT structure which randomly selects “treated” villages where the reform  
73 has been implemented and “control” villages where, as of today, customary land rights remain  
74 in place. To gather information regarding the effects of the PFR on land-related litigation,  
75 what types of conflicts have been experienced, and what dispute resolution mechanisms have  
76 been used, we administered two rounds of surveys to individuals respectively seven and ten  
77 years after the reform’s implementation, conducting in-depth interviews with a sample of 1086  
78 respondents across 43 randomly selected villages included in the RCT.

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

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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).

86 the frequency of conflict-related violence nor of beliefs that land-related disputes can escalate  
87 into violent episodes.

88 This increase in litigation is puzzling if we conceive titling as an all-or-nothing phenomenon  
89 or taking place only through land registries. However, it is consistent with a view of the com-  
90 plementarity of titling by registration and by judicial procedures aimed at clarifying property  
91 rights. Along these lines, we suggest an explanation for the increase in litigation that it is ob-  
92 served after the reform. The starting consideration is that, in order to reduce the probability of  
93 suffering from adverse claims, rightholders can engage in private and public activities to clarify  
94 their existing rights and protect their title – that is, to “purge” their (formal or informal) own-  
95 ership title. To this end, they can do private physical investments to better define and proclaim  
96 their rights, such as fencing parcels (Anderson and Hill, 1975, Hornbeck, 2010), demarcating  
97 boundaries (Libecap and Lueck, 2011), or enforce possession of the asset by physically “staying  
98 in place” (Field, 2007, Goldstein et al., 2018). They can also clarify their title using public  
99 means, by litigating against specific claimants – as in a boundary dispute – or by initiating  
100 a general judicial procedure against all potential adverse claimants, similar to the “quiet title  
101 suit” used in the USA (Bray, 2010).

102 In a customary system, parties who hold possession of land parcels can also keep their  
103 rights unclarified or make investments to reinforce them or purge them from possible adverse  
104 claims. This was the case in rural Benin, where possessors of parcels traditionally resorted  
105 to various means to enhance their titles and purge potential adverse claims. Private means  
106 included engaging in Vodou practices and, revealingly, the ritual planting of a shea or *karité*  
107 tree to proclaim ownership (Adjahouhoué, 2013), thus making the ownership claim public and  
108 inciting potential claimants to suit or implicitly concede. As in many developing countries, in  
109 recent times these proclamations also include posting ownership claims indicating the name and  
110 phone number of the owner, as well as fencing and building houses and wells or employing a  
111 guard. In addition, possessors resort to fully public means, from requesting local authorities to  
112 issue certificates merely attesting ownership, and having conveyances endorsed by the village  
113 chief in a “Certificate of non-litigation” or *Attestation de Non Litige* (Bierschenk and Olivier de  
114 Sardan, 2014); to starting expensive first registration procedures (*Inmatriculation*) at the land  
115 register created in colonial times (Lavigne Delville, 2019), an option that, given its high cost,  
116 was only suitable for the highest value land.

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

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

124 imperfectly. First, all valuable land within a village was registered, irrespectively of a parcel's  
125 value. Second, the reform faced time and resource constraints which resulted in an incomplete  
126 purging of the land titles awarded. Certainly, the Beninese PFR included a procedure for  
127 purging land titles. However, commentators note that the purging process was largely imperfect  
128 because of the lack of time to solve the most controversial cases and because some customary  
129 rights were excluded from the registration process but are successfully claimed by absent parties  
130 at a later stage (Lavigne Delville and Moalic, 2019).

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

145 It is worth emphasizing that the type of land-related litigation we observe here is not nec-  
146 essarily a negative outcome, since disputes conducted via an institutionalized process might  
147 contribute to a beneficial clarification of ownership rights. In a sense, it is titling by different  
148 means, making it possible to adjust the quality of the title to land value, and therefore intro-  
149 ducing some flexibility into a system of universal titling. Moreover, in our sample we do not  
150 observe significant changes of the frequency of conflict-related violence in treated villages nor  
151 of beliefs that land-related disputes can escalate into violent episodes. Finally, while assessing  
152 whether the observed increase in litigation produced by the reform is efficient lies outside the  
153 scope of this article, previous research shows that clarifying property rights can substitute for  
154 inefficient expenditures in private protection and increase investments (Field, 2007, Galiani and  
155 Schargrodsky, 2010, Goldstein et al., 2018). In Section 6, we will come back to this discussion.

156 Our paper contributes to a growing empirical literature which reports mixed evidence on  
157 the effects of land rights formalization on conflicts. Two waves of research based on case studies  
158 (André and Platteau, 1998, Jansen and Roquas, 1998, Kalabamu, 2019, Peters, 2009) and cross-  
159 sectional observational data (Alston et al., 2000, Deininger and Castagnini, 2006, Holden et al.,  
160 2019) show that tenure formalization is associated with no reduction, or at times even an  
161 increase in conflicts over land. However, these research designs cannot account for endogeneity

162 and self-selection issues concerning villagers’ decisions to title only specific land parcels which are  
163 more likely to be contested, or authorities’ choices to roll over selective formalization programs  
164 involving only parcels or territories with comparatively high value.<sup>4</sup>

165 Only a few studies that investigate the institutions-litigation link have quasi-random al-  
166 location of titles across the sample of households, which lends more confidence in identifying  
167 causal relationships instead of mere correlations. Two studies which focus on the relationship  
168 between property rights and violence show a moderating effect of formalization on homicides  
169 rate. In the first article, Fetzer and Marden (2017) exploit spatial and temporal variation in  
170 the availability of forest land protected by natural conservation laws – which are therefore not  
171 vulnerable to requests for title by squatters – in the Brazilian Amazon region, in combination  
172 with the constitutionally-provided right to occupy unused land, to show that the expansion of  
173 territories for which land titles cannot be requested reduces the rate of violent conflicts in a  
174 municipality.<sup>5</sup> The second study by Dower and Pfutze (2020) shows that land certification in  
175 Mexico reduces violent deaths. The authors provide evidence that the reduction in violence  
176 stems from formalization reducing politicians’ discretion in the allocation of land rights and, as  
177 a consequence, in the amount of disputes. Our paper is complementary to these contributions  
178 because we collect data on the whole set of disputes over land, including both those escalating  
179 into episodes of violence and those resulting instead in non-violent contentions. In a recent  
180 contribution, Di Falco et al. (2020) compare the rate of land-related conflicts experienced by  
181 Ethiopian rural villagers the year before and the year following the roll-out of a land rights  
182 certification program. The authors show that villagers who received formal land certificates  
183 have experienced significantly less land-related conflicts. We complement these findings on  
184 the immediate effects of formalization on conflicts by studying the medium-term effects of the  
185 intervention (i.e. 10 years after the implementation).<sup>6</sup>

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<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, 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.

<sup>5</sup>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.

<sup>6</sup>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.

186 The paper is organized as follows. In the next section, we describe the main features of  
187 the Beninese legal and institutional framework and of the PFR. Section 3 explains the research  
188 design and reports details about the survey and the data collection. In Section 4 we present  
189 the results. Section 5 discusses the findings and suggests a general framework which connects  
190 land titling and conflicts. Section 6 concludes.

## 191 2 Institutional Framework

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

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

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

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<sup>7</sup>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.

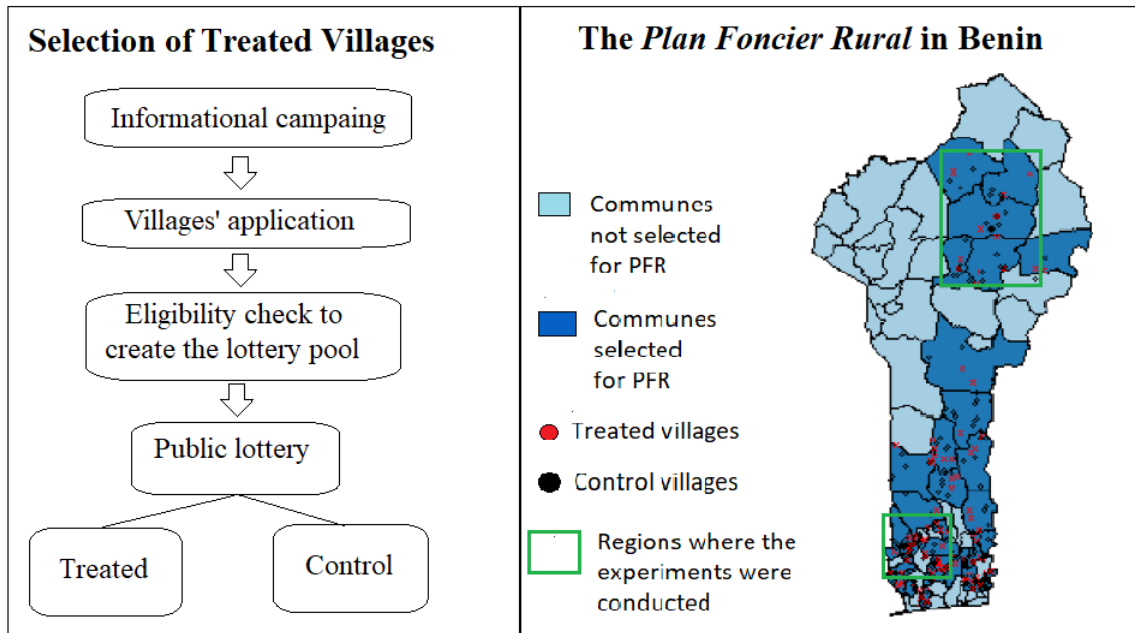


Figure 1: The lottery procedure characterizing the Plan Foncier Rural in Benin (left panel) and the resulting distribution of control and treated villages (right panel)

217 villages was randomly chosen via public lottery. Consequently, in 2010-2011, a team of local  
 218 experts implemented the PFR in these selected villages (the “treated” group). The remaining  
 219 non-selected villages (the “control” group) did not receive any intervention and, as of today,  
 220 continue to have customary land rights. Figure 1 shows the lottery procedure characterizing  
 221 the Beninese PFR and the resulting map of communes and villages included in the lottery pool.

222 Two additional features of the PFR are particularly relevant for our study. First, the reform  
 223 aimed at universal demarcation of boundaries and rights in each treated village, meaning that  
 224 all valuable land within a village was registered, irrespectively of a parcel’s value. Second, it  
 225 intended to consider both the importance of the physical demarcation of boundaries – by mark-  
 226 ing them with cornerstones – and of legal demarcation of rights – by conditioning registration  
 227 to gathering the public consent of neighbors of adjoining parcels. However, limited resources,  
 228 the complexity of customary bundles of rights, and some legal deficiencies resulted in a de facto  
 229 emphasis on physical demarcation (Lavigne Delville and Moalic, 2019).

### 230 3 Research Design

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



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

238 Three caveats are in order. First, pre-treatment data on litigation are not available for  
239 participants in our sample. Therefore, our identification strategy relies on the random selection  
240 of the 43 villages where we collected data from the original RCT pool for cancelling out potential  
241 pre-existing differences in land-related litigation across treatment groups. An impact evaluation  
242 of the original PFR reform shows that the randomization of the PFR lottery was successful  
243 (Goldstein et al., 2015). Moreover, as we show in the next session, participants in our sample  
244 are well-balanced on observables, thus increasing confidence in the validity of our approach.  
245 Second, for the identification strategy to work, we must ensure that there is no self-selection of  
246 individuals into treatment following the PFR randomization (for instance, because of migration  
247 from control to treated villages after the reform implementation). As explained in the next  
248 section, we have verified that migrating out of the village of origin is rare for participants in  
249 our sample and that migration flows are similar across treatment branches. Finally, official  
250 statistics relative to land-related litigation in Benin are available only for those disputes which  
251 are resolved through the formal judiciary. This is a small fraction of the total number of  
252 conflicts experienced by rural villagers since, as we elaborate in the Results section, customary-  
253 informal and religious dispute resolution mechanisms coexist with state courts. Therefore, to  
254 avoid possible non-classical measurement errors and to gather a comprehensive picture of all  
255 land-related conflicts, the data that we analyze in our study were collected by administering an  
256 in-depth survey during fieldwork sessions in the sample of selected villages.

257 We conducted two survey rounds, the first in the initial trimesters of 2017 (thus approxi-  
258 mately seven years after the reform implementation) and the second at the beginning of 2020  
259 (ten years later). In total, we held 65 fieldwork sessions (32 in 2017) visiting 43 villages (24  
260 treated) and interviewing 1,086 individual households (493 in 2017).<sup>8</sup> The survey collected  
261 socio-demographic information and asked a set of questions related to land-related disputes. In  
262 particular, the question that we use to answer our main research question asked participants  
263 whether they had experienced at least one conflict related to land after 2010 and, if so, which  
264 type of dispute(s) it was.<sup>9</sup> The survey round of 2020 included exactly the same questions that  
265 were asked in 2017, plus an additional set of questions on details relative to market integration,

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<sup>8</sup>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).

<sup>9</sup>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.

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

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

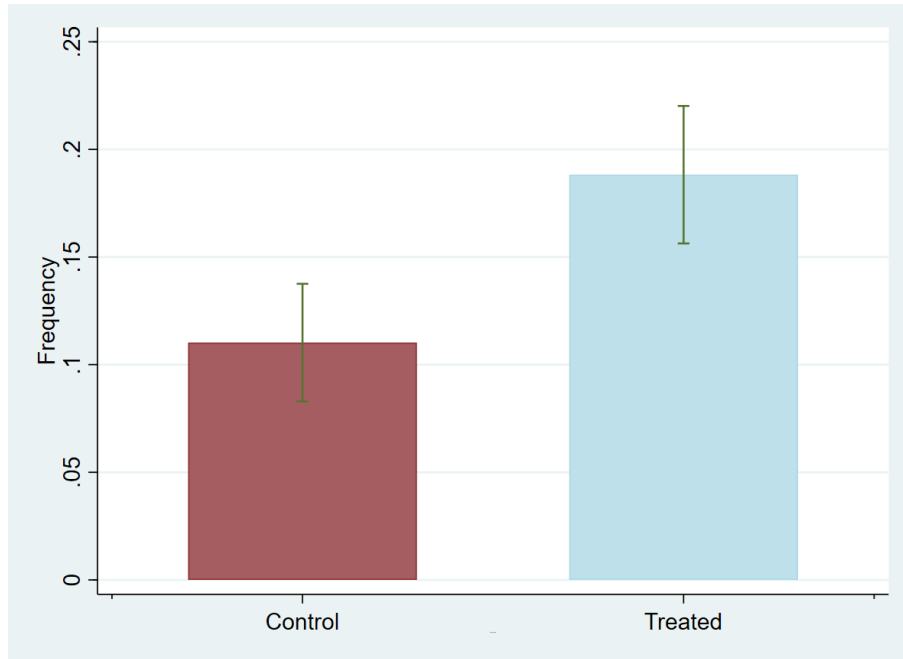
## 278 4 Results

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

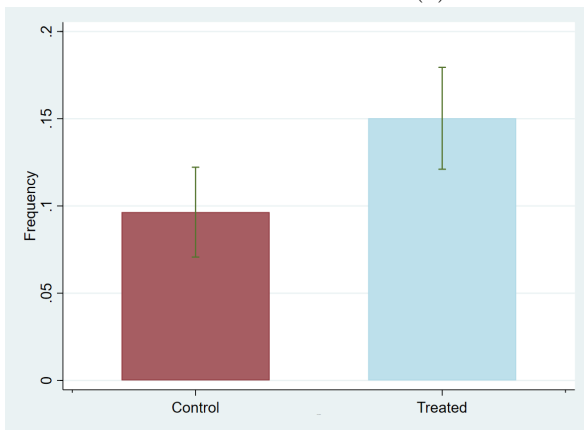
295 As a first step of the analysis, we look at the likelihood of experiencing land-related litigation  
296 after the implementation of the PFR reform as reported by survey respondents. The upper  
297 panel of Figure 2 displays the frequency with which participants report experiencing land-  
298 related conflicts in treated and control villages. Participants in treated villages report litigation  
299 significantly more often than those in control (Chi-square test,  $p < 1\%$ ). The bottom panels

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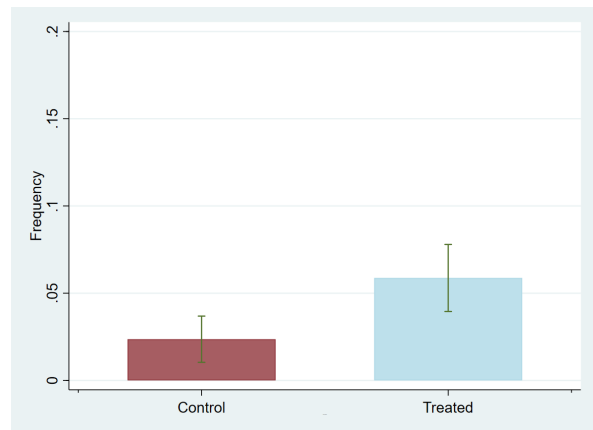
<sup>10</sup>The majority of the data collection sessions involved exactly 18 participants, however, due to logistical constraints and specific circumstances, the session number varied between 12 and 20 individuals.



(a) Total Land-Related Conflicts



(b) Parcel Boundary Conflicts



(c) Land Inheritance Conflicts

Figure 2: Land-Related Conflicts – Whole Sample

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

304 Secondly, we verify these findings in a regression framework. Table 1 reports the results  
 305 of a linear probability model.<sup>11</sup> In all the model specifications the dependent variable is a

<sup>11</sup>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.

306 dummy equal to one if a participant reported to have experienced a land-related litigation after  
307 2010. Standard errors are clustered at the village level to account for possible levels of intra-  
308 village correlation. Model 1 controls for the two observables unbalanced in our sample (age  
309 and whether a household is polygamous). The coefficient of the treatment dummy is positive  
310 and statistically significant at the 1% level. The point estimate suggests that participants  
311 who had the PFR reform implemented are approximately 75% more likely to experience land-  
312 related conflicts. Model 2 additionally controls for the individual characteristics we collected  
313 in the post-experimental survey.<sup>12</sup> The coefficient of the treatment dummy remains positive  
314 and strongly significant, and point estimates very similar. Model 3 includes further controls  
315 for village characteristics. The coefficient remains strongly significant and the point estimate  
316 suggests that being in a village where the reform was implemented roughly doubles the likelihood  
317 of experiencing litigation.

318 We continue the analysis by excluding from the sample 106 households in treated villages  
319 who took part in the survey but who do not own land parcels that have been affected by  
320 the PFR. This could happen for various reasons, for example because all the land belonging  
321 to the household is located outside the village borders – and so not included in the PFR –  
322 or because the respondents’ household does not own land. Models 4-6 replicate models 1-3  
323 by excluding those participants from the sample. All coefficients of the treatment dummies  
324 are positive and strongly statistically significant. The estimated increase in the probability to  
325 litigate is larger than when the whole sample of participants is considered, ranging between  
326 80% and 190%. In Table A2 in Appendix A, as a robustness check we re-estimated the model  
327 specifications presented in Table 1 by using Wild clustered bootstrapped standard errors with  
328 999 repetitions. The qualitative results remain the same.

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

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

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<sup>12</sup>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*** (0.028)	0.075*** (0.027)	0.101*** (0.029)	0.092*** (0.030)	0.091*** (0.030)	0.115*** (0.030)
Ind-Ctrl	N	Y	Y	N	Y	Y
Vil-Ctrl	N	N	Y	N	N	Y
Constant	0.103** (0.041)	0.108 (0.083)	0.094 (0.087)	0.115** (0.044)	0.063 (0.089)	0.059 (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.

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

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

364 tribunals can be used as an “appeal court” by litigants who are not satisfied with the judgement  
365 of local or religious authorities (and who can afford to access the formal justice system). This  
366 evidence indicates that the PFR did not modify villagers’ choice or perception concerning which  
367 conflict resolution mechanism should address land-related disputes, thus suggesting that this  
368 channel is unlikely to be responsible for the observed increase in litigation.

## 369 5 Why Formalization May Encourage Purging Litigation

370 In this section, we suggest an explanation for the increase in litigation observed in treated  
371 villages. We will use some simple algebra to clarify the argument. Following Arruñada and  
372 Garoupa (2005), let us assume that title defects are represented by the probability  $\theta$  that a  
373 conflicting claim for ownership fully succeeds ( $0 < \theta < 1$ ). This probability depends on which  
374 titling system (customary or formal registration) is in place and on the owners’ decision to  
375 additionally perfect their titles. Under customary titling, such probability is  $\theta_0$  but, as explained  
376 in the introduction, even if land remains informal owners could always spend resources to protect  
377 their title by different activities, such as planting a *karité* tree, fencing the parcels, requesting an  
378 ownership certificate from the village chief, litigating the boundaries of a neighbouring parcel,  
379 or clarifying ownership against all potential claimants in procedures functionally similar to the  
380 “quiet title suit” used in the USA (Bray, 2010).

381 Figure 3 represents the value of land when title conflicts exist as a function of its value  $V$  that  
382 would correspond to an ideal world without conflicting claims (represented on the horizontal  
383 axis), and under different titling institutions and with owners being able to make additional  
384 efforts to protect and perfect property rights. Let us assume that by spending a fixed amount  
385  $p_0$  per parcel, owners can make their title safer by reducing the probability of losing the land  
386 to  $\theta_{0p}$ . When deciding whether to purge or not their title, owners will compare the value of the  
387 land without purging, given by a fraction  $(1-\theta_0)$  of land value  $V$ , with the value after purging,  
388 given by  $(1-\theta_{0p})V-p_0$ . The break-even point is:

389

$$V_0^* = p_0 / (\theta_0 - \theta_{0p})$$

390 Therefore, it is worthwhile for owners of informally held land to purge their titles if the  
391 parcel value is higher than  $V_0^*$ ; while it is not worthwhile for land values lower than  $V_0^*$ . When  
392 the government introduces land titling it usually does so – and it did so in Benin – for all  
393 relevant parcels independently of their value and without cost for owners, but imperfectly, so  
394 that the probability of eviction after titling,  $\theta_1$ , still remains positive even if lower than  $\theta_0$ . This

395 decrease in the probability of eviction ( $\theta_0 - \theta_1$ ) is what causes the increase in land value driven by  
 396 formalization, increasing the slope of the value line in Figure 3. Again, owners can still spend  
 397 resources to additionally protect and purge their titles. Let us assume that by spending a fixed  
 398 amount  $p_1$  per parcel, owners can make their title safer by reducing the probability of losing the  
 399 land from  $\theta_1$  to  $\theta_{1p}$ . When deciding whether to purge it or not, owners will compare the value  
 400 of the titled land with formal titling but without additional purging, given by  $(1 - \theta_1)V$ , with the  
 401 value after titling and purging, given by  $(1 - \theta_{1p})V - p_1$ . The new break-even point is now:

402

$$V_1^* = p_1 / (\theta_1 - \theta_{1p})$$

403 and both break-even points are related by:

404

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

405 In principle,  $V_1^*$  can be lower or higher than  $V_0^*$ . However, it seems sensible to assume that  
 406 after titling the identification of rightholders and neighbors makes judicial purging cheaper, so  
 407 that  $p_1 < p_0$ ; and/or more effective in reducing the probability of eviction, so that  $(\theta_1 - \theta_{1p}) > (\theta_0 -$   
 408  $\theta_{0p})$ , given that some collisions of rights have been purged by titling and most rightholders and  
 409 claimants have been identified.<sup>13</sup> This results in  $V_1^* < V_0^*$ , as represented in Figure 3. In that  
 410 case, after titling, owners will spend additional resources to protect their titles on relatively  
 411 lower-value land. Thus, litigation is expected to happen predominantly among those low-value  
 412 parcels, since under the customary regime parties had already clarified existing rights for higher-  
 413 value parcels – for instance, by fencing their property, litigating with neighbors, or by organizing  
 414 public ceremonies attended by the whole community for the conveyance and public notice of  
 415 rights.<sup>14</sup>

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<sup>13</sup>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)”.

<sup>14</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  $V_2^* = (r + p_2) / (\theta_2 - \theta_{2p})$  falling between  $V_1^*$  and  $V_0^*$  would be registered and

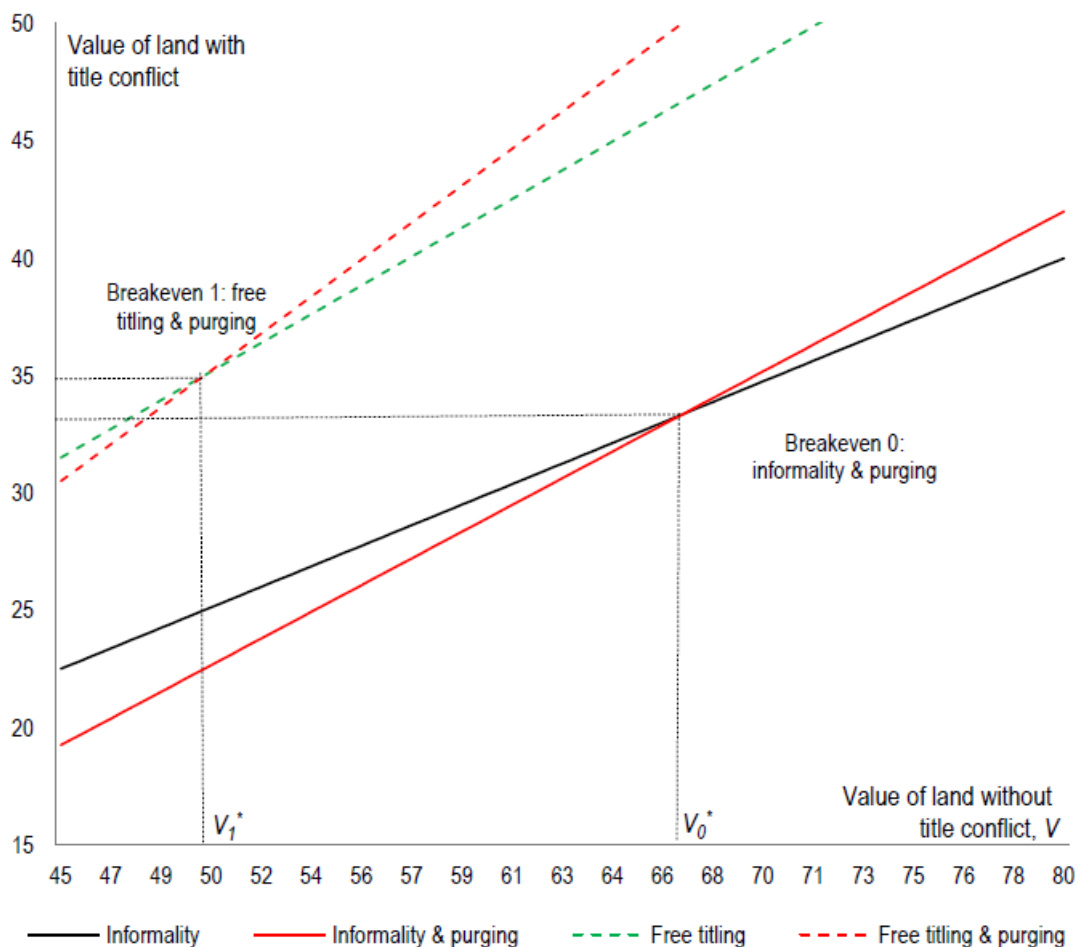


Figure 3: Four possible functions of land values depending on the availability of titling institutions and owners' additional voluntary protection of their property rights.

416 We investigate whether, in our sample, land parcels of different value and productivity  
 417 were affected differently in terms of changes in conflict rate by the formalization of land rights.  
 418 Accounting for the value of a land plot in rural African villages is a complex task, since we know  
 419 that the productivity and value of land parcels are characterized by substantial within-village  
 420 variability (Beaman et al., 2015). Given that we lack data on land qualities, we proxy land  
 421 value by looking at the level of market integration and wealth of the households possessing it,  
 422 thus relying on the evidence that wealthier villagers own more productive land, operate more in  
 423 markets, and are more active in the market economy (Beaman et al., 2015, Fabbri, 2018). We

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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.



Table 2: Likelihood of Experiencing Conflicts - Heterogeneity Analysis

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

424 then first collected data on the share of calories consumed in the households that were purchased  
425 in the market (rather than self-produced). We classified as “High-Market Integrated” those  
426 respondents who reported purchasing in the market more than half of the consumed calories. We  
427 then re-estimated the main model specification presented in Table 1 separating and comparing  
428 within categories the effects of the reform for households characterized by high and low levels  
429 of market integration.

430 The results are reported in model 1 of Table 2. The baseline category are control households  
431 with low market integration. The small and insignificant coefficient of the interaction term  
432 *Control\*High-MI* shows that the likelihood of experiencing conflicts is the same for control

433 subjects characterized by high levels of market integration. Similarly, the coefficient of the  
434 interaction term *Treated\*High-MI* is not statistically different from those of the baseline and  
435 high-market integration control groups (F-test,  $p=0.36$  and  $p=0.26$ , respectively). What drives  
436 the estimated increase in conflicts that we observe for subjects in the treated group is the sample  
437 of households characterized by low market integration. Compared to the baseline, for this group  
438 the increase in the likelihood of experiencing conflicts after the PFR has been implemented is  
439 large and statistically significant (at 5% in model 1, in which we include the whole sample of  
440 participants, and at 1% level in model 2 in which we exclude 78 households in treated villages  
441 who did not own land subject to the PFR).

442 We replicate the analysis by using self-reported household income as a proxy for land value  
443 (in models 3 and 4) and an index of household wealth based on eight different indicators (in  
444 models 5 and 6).<sup>15</sup> In all cases and model specifications, the qualitative results remain the same.  
445 Specifically, F-tests comparing treated and control respondents in the high-income (models 3 and  
446 4) or in the high-wealth (models 5 and 6) conditions find no statistically significant difference  
447 in the likelihood of experiencing conflicts. Conversely, comparing *Treated\*Low-Inc* against  
448 *Control\*Low-Inc* (models 3 and 4) or *Treated\*Low-Inc* against *Control\*Low-Inc* (models 5 and  
449 6) returns differences in conflict rate statistically significant at the conventional level or better in  
450 all cases. To summarize, in our sample PFR titling caused a significant increase in litigation for  
451 respondents characterized by low levels of market integration, income, and wealth – indicators  
452 that we use to identify them as owning low-value land parcels. However, the reform had no  
453 effect on conflicts for individuals owning high-value land parcels.

## 454 6 Conclusion

455 The relationship between land rights formalization and litigation has sparked a heated de-  
456 bate, with some scholars arguing that well-defined land rights resolve ambiguous claims and  
457 prevent conflicts, while others holding that land demarcation favours the emergence of latent  
458 disputes and displaces the social safety net of collective tenure, enhancing litigation. We shed  
459 light on this topic by verifying empirically the effects of a land rights formalization program in  
460 rural Benin on land-related litigation ten years after its implementation, and by proposing an  
461 explanation for the observed effects based on the complementary character of formal titling and  
462 title-driven litigation.

463 Our identification strategy is based on the random allocation of the rights formalization  
464 across villages through a public lottery, which makes the Beninese PFR the first case of a large-  
465 scale land tenure reform implemented as randomized control-trial. We find that, when looking  
466 at average effects over the whole sample, formalization significantly increased the likelihood of

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<sup>15</sup>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.

467 experiencing land-related conflicts, roughly doubling households' litigation rate in the villages  
468 where the reform was implemented. Litigation mostly concerned parcel boundaries and, im-  
469 portantly, did not increase violence. These results suggest that property rights in land affect  
470 land-related litigation by using an identification strategy based on an unquestionably random  
471 allocation of titles which is uncommon to observe in the literature.

472 We put forward that the reform, by registering all land parcels in a village irrespectively of  
473 their value and at the same time awarding incomplete land titles not fully purged, increased  
474 demand for litigation aimed at clarifying existing rights. Consistent with this explanation, we  
475 find that formalization determined no effect on the litigation rate of wealthier and more market-  
476 integrated households who are likely to own land parcels which, given their greater value, had  
477 already been purged before formal titling. Conversely, the estimated increase in litigation is  
478 concentrated on those households characterized by lower levels of market integration and wealth,  
479 who are likely to own lower-value land parcels whose title was not privately profitable to purge  
480 under the customary system.

481 It is worth emphasizing that the increase in litigation observed in villages where the reform  
482 was implemented is not necessarily an inefficient outcome. Indeed, litigation episodes aimed at  
483 clarifying existing rights likely represent a positive step toward establishing more secure property  
484 rights. More generally, it seems to suggest that the desirability of litigation aimed at clarifying  
485 property rights depends on the specific situation under scrutiny and should be established on  
486 a case-by-case basis. For instance, while the purging through judicial means of property titles  
487 may increase land value and investments in a society endowed with well-functioning dispute  
488 resolution mechanisms, policymakers designing tenure reforms may want to take steps in order  
489 to mitigate the emergence of additional conflicts in contexts already plagued by pre-existing  
490 social or ethnic tensions.

491 Finally, our results contribute to the broader debate regarding the costs and benefits of uni-  
492 versal versus selective land titling (Arruñada, 2015, Connelly, 2016). For the past few decades,  
493 governments in developing countries have usually introduced land titling on an universal basis,  
494 registering all parcels in a given area. However, more recently universal titling has been subject  
495 to scrutiny (Arruñada, 2017, Bruce, 2012, Connelly, 2016) and some land titling projects were  
496 redesigned accordingly (e.g. Ali et al., 2014, 2017, Deininger et al., 2008). Here we contribute  
497 to the discussion by focusing on a specific externality of universal titling: given its emphasis on  
498 coverage and quantity, it leads to minimizing average cost and thus to sacrificing the quality of  
499 title, possibly leading to an increase in the demand of complementary purging. Moreover, the  
500 standard policy of subsidizing initial formal titling may also cause a somehow transitory surge  
501 in litigation if owners expect that titling prices will continue to be subsidized in the future (e.g.  
502 zero instead of  $r$ ). While, as said above, our study is not aimed at assessing the (in-)efficiency  
503 of title-driven litigation, our findings emphasize the importance of taking into account this ex-  
504 ternality when designing tenure reforms and evaluating consequences and trade-offs associated

505 with different approaches to land rights formalization.

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628 **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 (n=578)	Control (n=508)	Diff (p-value)	Treated (n=306)	Control (n=287)	Diff (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.

Sample:	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
	Whole			Reduced		
treated	0.078	0.075	0.101	0.092	0.091	0.115
<i>C.I.</i>	[.02, .13]	[.019, .13]	[.04, .17]	[.03, .16]	[.03, .15]	[.05, .18]
<i>p-value</i>	.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** (0.041)	0.108 (0.083)	0.094 (0.087)	0.115** (0.044)	0.063 (0.089)	0.059 (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*** (0.120)	0.361** (0.140)	0.424** (0.187)	0.481*** (0.120)	0.427*** (0.141)	0.422** (0.186)
risk	0.001 (0.027)	-0.009 (0.029)	0.015 (0.039)	-0.008 (0.027)	-0.008 (0.030)	-0.017 (0.033)
male	0.278** (0.119)	0.331** (0.137)	0.119 (0.186)	0.266** (0.133)	0.330** (0.149)	-0.014 (0.189)
age	0.005 (0.004)	-0.000 (0.004)	0.016*** (0.006)	0.003 (0.005)	-0.001 (0.004)	0.011* (0.006)
school	-0.049 (0.169)	-0.237 (0.176)	0.212 (0.285)	-0.006 (0.180)	-0.160 (0.179)	0.432 (0.282)
education	0.077 (0.048)	0.091* (0.050)	-0.000 (0.067)	0.073 (0.052)	0.087* (0.050)	-0.033 (0.069)
logincome	-0.024 (0.030)	0.018 (0.035)	0.184*** (0.064)	0.007 (0.035)	0.012 (0.039)	0.193*** (0.071)
south	-0.090 (0.179)	-0.362* (0.204)	1.146*** (0.321)	-0.132 (0.186)	-0.395* (0.219)	1.060*** (0.322)
Controls	Y	Y	Y	Y	Y	Y
Constant	-1.415*** (0.364)	-1.381*** (0.456)	-5.128*** (0.734)	-1.527*** (0.405)	-1.319*** (0.490)	-4.967*** (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.