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Does insecure land tenure deter investment? Evidence from a randomized controlled trial☆

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ABSTRACT

There is broad agreement among the most prominent observational studies that tenure insecurity deters investment. We present new experimental evidence testing this proposition: a land certification program randomized across villages in Zambia. Our results contradict the consensus. Though the intervention improved perceptions of tenure security, it had no impact on investment in the following season. The impact is still zero even after a cross-randomized agroforestry extension relaxes financial and technical constraints to agroforestry investment. Though relaxing these constraints has a direct effect, it is not enhanced by granting land tenure, implying tenure insecurity had not been a barrier to investment.

1. Introduction

Of all institutional failures the risk of arbitrary expropriation is widely held as the most pernicious. The usual argument is that an investor, be she the biggest industrialist or the smallest farmer, will refuse to invest if the fruits of her investment may be seized. Several cross-country studies have claimed that expropriation risk is a bigger barrier to investment than other market imperfections (Acemoglu and Johnson, 2005; Johnson et al., 2002). But the case for a link between aggregate insecurity and the individual's decision to invest has rested on household-level studies of the security of land tenure. Since land is often central to a poor household's livelihood, this literature has argued that insecure tenure distorts a wide range of household decisions. Besley (1995), Field (2007), Goldstein and Udry (2008), De Janvry et al. (2015), and many others have argued that granting a household tenure over its land triggers responses ranging from agricultural investment to international migration.[1]

But any observational study can only succeed if it sifts exogenous variation from the endogenous political forces that dispense property rights. Tenure security is usually granted to those with power and status, which also ease investment for reasons unrelated to expropriation risk. Even when tenure is granted through, say, a government titling program, it is unlikely to arrive in some places before others by sheer chance. Well-organized communities or those with the best investment opportunities might lobby to get the program first. Given these challenges one may wonder whether even the most thoughtful analysis can uncover a valid natural experiment.

This paper sidesteps such challenges by using a randomized experiment. We evaluate the short-run effects of an intervention in Zambia that cross-randomized an agroforestry extension with a program that strengthened customary land tenure through field demarcation and certification. We test for whether tenure security affects a host of outcomes drawn from prior observational studies. Our experimental results do not corroborate these studies. We estimate with reasonable precision that tenure security has zero effect.

The null result comes despite our focus on two pre-registered outcomes, agroforestry and land fallowing, that are very similar to those studied in the two most prominent studies to use data from Sub-Saharan

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[1] There are a few observational studies, as reviewed in Fenske (2011), that find null results. These studies have generally been less prominent, possibly in part because null results are often less likely to publish in top journals.

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Africa (Besley, 1995; Goldstein and Udry, 2008). Agroforestry, the cross-cropping of nitrogen-fixing trees with traditional field crops, is an innovation that keeps soil fertile and prevents field erosion. But growing these trees requires a viable seedling, technical knowledge, and 5–8 years of care. As per the tenure hypothesis, households may be unwilling to put in so much money and effort over so many years if they fear other households are likely to encroach or that the chief will reallocate the land (as is his right by custom). They may be even less willing to fallow their land, an investment that not only takes time but makes the land less secure. Leaving land fallow restores soil nutrients that would otherwise be drained by years of replanting. But customary land that is not planted may be seen by other households or the chief as no longer needed. It may be taken over by neighbors or given to households with immediate needs.

The land tenure intervention was designed to alleviate such fears. Households in treated villages joined in a community mapping exercise that established common knowledge of the boundaries of each household's land. Villages were encouraged to maintain a publicly accessible register of names and boundaries. Households were then issued customary certificates endorsed by the chief, which serve as physical proof of support for their claim. One advantage of this intervention is that it is a discrete improvement in land rights that is not conflated with a transfer of wealth or improved access to credit. Customary land may not legally be sold or used as collateral, and the customary certificates did not change this fact.[2] A certificate grants only recognition of a perpetual right to hold and farm the land. Nevertheless, demand for such certificates is overwhelming at baseline. Most households say a certificate will make them feel more secure. The endline survey suggests treatment made households feel their land is more secure from encroachment by other households and reallocation by the chief.

And yet we find no impact on any measure of investment. We reject any meaningful impact on either the planting of agroforestry trees or the fallowing of land. We find no impact even among households who report feeling insecure at baseline. And though agroforestry and fallowing are the primary outcomes of interest, we also test for effects on a host of others drawn from prior studies. Unlike Field (2007) we find no impact on the likelihood of market work. Unlike Ali et al. (2014) we find no impact on soil conserving investments. And unlike Valsecchi (2014) and De Janvry et al. (2015) we find no impact on the likelihood of having a household member migrate.

A skeptic might argue that the tenure intervention failed because households face other binding constraints. These constraints might make investment impossible regardless of whether improved tenure security makes it more attractive. Households may not, for example, have access to agroforestry seedlings or the knowledge of how to grow them. But it is precisely to relax such constraints that the tenure intervention is cross-randomized with the agroforestry extension, which gave farmers the resources and training needed to overcome financial and technical constraints.

We confirm that the extension substantially relaxed constraints preventing agroforestry investment. In villages given only the extension program, households were 30 percentage points more likely to plant trees (over a control mean of 11 percent). If tenure insecurity were a major disincentive to investment, households given both the extension and the tenure intervention should be more willing to take up agroforestry than those given the extension program alone. But we find no additional take-up in villages given both treatments. The tenure intervention has neither a direct nor catalyzing effect, suggesting insecure tenure is not a meaningful barrier to investment.

Could this stark difference in results be explained not by a difference in research design but one of context? We argue that is unlikely. The relation between households and traditional authorities in Zambia is not unlike that in Ghana, which was studied in Besley (1995) and Goldstein and Udry (2008). The likelihood of expropriation by authorities is not much different from that reported by Markussen and Tarp (2014) in their study of Vietnam, and even higher than in the Rwandan sample studied in Ali et al. (2014). The risk of private disputes over land in our sample is far higher than in the sample of Ali et al. (2014).

Finally, we discard our experimental variation and apply several observational research designs similar to those used in prior studies. We show that had we used such a design we would have spuriously concluded that tenure security has positive and significant effects. This exercise does not necessarily imply the estimates of the observational studies were flawed. But it does show that the key moments used by these studies for identification also appear in our Zambian sample. That implies the context is not entirely different and that it is possible to find these moments even in a sample where granting tenure security has no effect.

That leaves the difference in research design as the most salient explanation for the difference in results. To our knowledge the recently published work of Goldstein et al. (2018) is the only other study that, like ours, uses a randomized controlled trial to study the impact of tenure security. Our work differs from theirs for several key reasons. The most important is that our study cross-randomized the tenure intervention with an extension program that relaxes technical and financial constraints to investment. Aside from showing that tenure security has no impact even when other constraints are relaxed, the cross-randomization lets us benchmark the tenure intervention against a more traditional intervention. Our results suggest these other constraints are far more important, suggesting the impact of tenure security (or at least recognition of perpetual land-use rights by traditional authorities) is both statistically and economically insignificant. Meanwhile, the tenure program they study differs because it grants recognition by the central government whereas the program studied here grants recognition by the chiefs responsible for allocating land. Finally, our study also differs in that Goldstein et al. (2018) measure the impact of receiving formal recognition without documentation of land rights, whereas roughly half of our sample had received paper documentation by the time of the endline survey.

To be clear, our results do not necessarily rule out that the risk of expropriation or theft deters industrial or foreign investment. Industrialists and foreigners may think of expropriation risk differently than poor farmers. And since our results are based on a one-to-two year follow-up we cannot rule out an effect that only arises in the very long run. But we can rule out that farmers in Zambia regard tenure insecurity as a highly salient and immediate deterrent to investment. And although we cannot claim our null results are universal to any context, our results suggest the positive impacts found in prior studies are likewise not universal. Even in a context that resembles those previously studied, a well-implemented certification program may have no impact, suggesting the link from tenure security to investment is less well understood than is often assumed.

2. Tenure insecurity in theory and in Zambia

2.1. The tenure hypothesis

Several studies have made the formal argument for why tenure insecurity might deter investment (e.g. Besley, 1995; Goldstein and Udry, 2008; Besley and Ghatak, 2010). Investment, they argue, has an unconditional cost but pays a return only if the investment is not seized. A nitrogen-fixing tree takes time and money to plant but raises the farmer's income only if her land is not reallocated by the chief and the sapling is not destroyed by encroaching neighbors. The farmer may be unwilling to pay these costs if she thinks either risk is likely. The problem is aggravated when the investment actually raises the risk of losing land, as in the case of fallowing (see below). By reducing this risk, the

[2] The only way to sell or collateralize land is by first converting it to titled land, which is difficult and rare.

theory goes, land tenure raises the expected return to investment, which encourages more investment.

This prediction assumes households do not face other constraints (Besley and Ghatak, 2010). If the farmer lacks the needed resources or knowledge he cannot invest regardless of how secure he feels. This caveat explains why it is crucial for our study to cross-randomize land tenure with another intervention that alleviates any constraints of financing or knowledge for at least one investment. If strengthening tenure does not encourage investment even after other constraints are eased, it would cast doubt on the tenure hypothesis.

Finally, some studies have adapted the model to describe labor allocation or migration (e.g. Field, 2007; Valsecchi, 2014; De Janvry et al., 2015). If labor can be used either to generate income or to guard an untitled plot of land (by staying at home for example), granting tenure would free up labor for market work or migration. One could argue that farming a depleted plot of land—one in need of fallowing—is a costly means of guarding it. Granting secure tenure, as per the theory, would let the household leave depleted land fallow and either use labor on more fertile plots, hire out to other farmers, or migrate in search of work.

2.2. Land rights in Zambia

During the intervention, land in Zambia was governed under the Land Act of 1995. The act groups land into separate systems of customary land and land exclusively under central government administration. The vast majority of Zambian households fall under the customary system. They do not own their land but at least in principle have rights that include use and inheritance. But the law devolves the administration of these rights to traditional authorities, namely the chief and the village headmen who act as her representatives. Hence in practice these authorities have near complete discretion to grant use and occupancy rights, and likewise the power to revoke these rights.

Traditionally the chief and the village headmen have used this power to ensure all households have land (Mudenda, 2006). In our sample most headmen report that they aim to reallocate land away from wealthy households. While such progressive redistribution ensures the poor are not allowed to starve, it may make households reluctant to appear so prosperous that they can afford to lose some land.

Even more troubling are recent reports that the 1995 Land Act has distorted the incentives of chiefs, putting even the smallest farmers at risk. Brown (2005) writes that under the law the chief can convert customary land to titled land, which can then be sold to developers. He describes one notorious case in which a chief let two investors bid and counter-bid on riverfront property, ultimately earning 30,000 dollars (more than 30 times Zambia's per capita GDP). Such conversions have the potential to displace customary farmers. Brown cites the example of Chief Mukonochi, who granted 26,000 ha of land to a tobacco plantation. This conversion displaced some 2000 people. Such incidents are relatively rare but heavily reported in the Zambian press. That may explain why we find in Section 2.3 that a sizable minority of households think it likely their land could soon be reallocated.

Reallocation by the chief or village head person is only one potential risk. The other is encroachment by other households. Since there is no official record of anyone's boundaries, neighbors may expand their crops or let their animals graze on land another household considers its own.[3] Over 20 percent of households at baseline report having had at least one land dispute in the past 3 years. Finally, even if a plot of land has been farmed for generations by a single family whose rights are commonly acknowledged, there is no guarantee that one household in the family can protect its claim from relatives (especially when someone dies and inheritance must be decided).[4]

These two risks—reallocation by the authorities (expropriation) and encroachment by private actors (theft)—are conceptually distinct, but both threaten the household's investment. Both also become more likely if the land looks unused, as it might if left fallow. As noted above, fallowing is one of the main investments a household can make in its land. A plot is more productive if its soil is richer in nutrients. Fallowing increases the stock of nutrients by eliminating the outflow for one season and potentially adding compost through a cover crop.

The household could also increase the inflow of nutrients by planting nitrogen-fixing agroforestry trees (nitrogen is one of three major nutrients that crops draw from the soil). Zambian farmers in our sample have heard from newspapers and NGOs that planting such trees can improve soil fertility. But agroforestry requires an upfront cost of buying seedlings and learning how to plant them. Even after paying those costs the seedlings must be tended for years before they yield any benefit. The household may be unwilling to make such costly investments if it fears it cannot keep the returns—if the chief reallocates fallow land, or if another household's grazing animals destroy agroforestry seedlings.

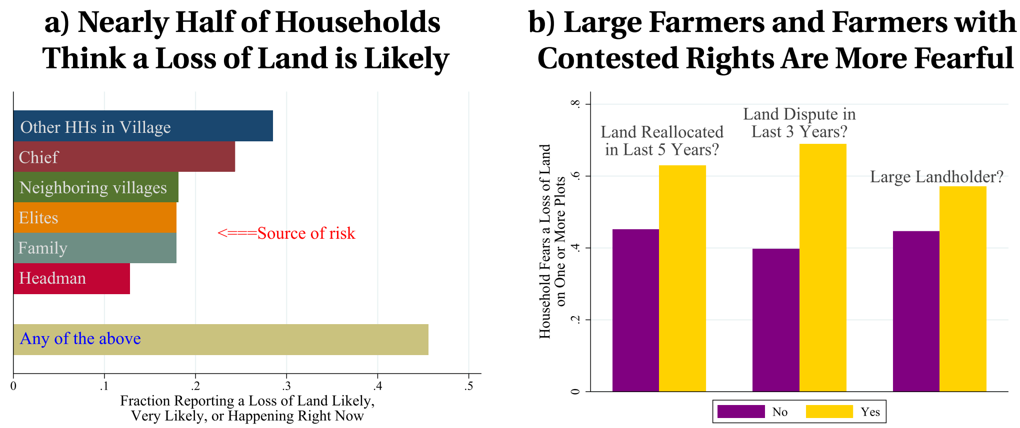
2.3. Perceptions of insecurity at baseline

Given the uncertainty of their tenure it is no surprise that households report that they would like to have documentation. At baseline over 88 percent say they would like paper documentation of their customary tenure rights on at least one of their plots. When asked directly about their perceptions of insecurity, a slight majority report feeling relatively secure but a substantial minority do not. The survey asks households, plot by plot, how likely it is (on a 1 to 6 scale) that in the next 3 years the plot will be encroached by any of several private actors, and how likely it is that the chief or village head will reallocate their land. The plot level data imply that while encroachment by neighbors is judged "impossible" on roughly 60 percent of plots, it is judged "likely" on 20 percent. Households report a similar pattern when asked about perceived insecurity from the chief. They feel secure in most of their plots, but a substantial minority of plots are deemed insecure.

We aggregate the plot-level responses by defining dummies for whether the household "fears a loss of land" to any of these six different agents. The dummy equals 1 if the household states that encroachment or reallocation of one or more plots by that agent is likely, very likely, or happening right now. Fig. 1a shows the fraction of households who fear encroachment by each of six sources of risk. Nearly 30 percent of households are worried about encroachment by other households in the village, suggesting many households expect their claims to be disputed by neighbors. Less than half that number fear reallocation by the headman, though many more fear the chief. The difference may be driven by distance (households trust their local leader above a hereditary monarch they may never have met), or it may be driven by the aforementioned stories of chiefs cutting deals with foreign investors. These results suggest households consider both the authorities and private actors potential threats to their land. The bottom-most bar shows that nearly half of households feel threatened by one or more of these actors.

[3] Private encroachment is typically gradual, though a particularly overbearing neighbor may attempt to take over an entire plot. A neighbor might also argue to the village headman that their neighbor is not using a plot, and get official sanction to take over an entire plot of land.

[4] Roughly 60 percent of households in our baseline sample are of the N'goni tribe, while the bulk of the remainder are of the Chewa tribe. In both cases a household typically acquires land through an allocation from the headman, as a gift from a parent, or through inheritance. The two tribes differ in that the former traditionally follow patrilineal rules of inheritance while the latter are matrilineal. In both cases there is some preference given to successors who remain in their natal village, and the choice between successors often falls to the head of the lineage (Takane, 2008). Ultimately, however, the rules of customary tenure in Zambia apply to both major tribes in our study sample and therefore grant the chief ultimate authority to reallocate land (Chinene et al., 1998).



Note: The survey asks households how likely it is (on a 1 to 6 scale) that in the next 3 years each plot will be encroached by any of several private actors, and how likely it is that the chief or village head will reallocate their land. **Panel a:** We plot the fraction of households that "fear a loss of land," meaning they think it likely, very likely, or happening right now on one or more of their plots. We plot the responses by the source of risk. We also plot the fraction who fear a loss from any of the sources. **Panel b:** We plot the fraction who fear a loss from any source conditional on whether it has recently had land reallocated or had a land dispute, and whether it is a "large" landholder (in the top decile of the distribution).

Fig. 1. Perceptions of tenure insecurity.

These fears are in part driven by experience. Fig. 1b plots the fraction of households who, conditional on a past event, fear a loss of land to any source. The first 2 bars show that households who have had some of their land reallocated in the past 5 years are more likely to fear a future loss (62 percent versus 45 percent). The difference is even larger between households who have or have not had a land dispute in the past 3 years (69 percent versus 40 percent). The last set of bars confirms that, as alluded to in Section 2.2, households with large landholdings (those in the top decile) perceive greater risk of a loss (57 percent versus 45 percent). This pattern confirms results from our survey of village heads, which suggest 63 percent of heads would give priority to poor households when allocating land (as compared to 2 percent who give priority to rich households and 34 percent who treat them equally).

Yet the extent of the fear is likely overblown. Though nearly 25 percent of households think it likely the chief will reallocate their land in the next 3 years, barely 2 percent have actually had their land reallocated. That said, Fig. 1b suggests the variation in households' perceptions of risk is at least partly grounded in reality. Households who are at greater risk (large landholders) or have experienced encroachment are more likely to feel at risk. Moreover, as shown by Jensen (2010), it is ultimately people's perceptions, regardless of how unrealistic, that govern their actions. Fig. 1 gives little evidence to suggest Zambian farmers feel more secure in their tenure than those in other developing countries.

3. Study design

3.1. Intervention

The intervention was rolled out across four chiefdoms in Chipata District of Eastern Province, Zambia over two and a half years beginning in the third quarter of 2014. The intervention cross-randomized two treatments, each done with support from the U.S. Agency for International Development by a separate local organization:

1. A village-level land tenure intervention implemented by the Chipata District Land Alliance (CDLA).[5]
2. A village-level agroforestry extension implemented by an NGO called Community Markets for Conservation (COMACO).

CDLA began its intervention by holding a community workshop in each village to form a Village Land Committee (VLC). Each VLC was trained in land management, conflict resolution, customary land certificates, and the customary land certification process. Under the guidance of CDLA, each VLC worked with individual households to demarcate their land and any natural resources held in common. The result was a widely accepted village map that could be used by the village headman when mediating land disputes. During this process each household was informed about its rights under the law, and in particular the rights that come with the customary land certificate they were issued at the end of the process. These certificates are signed by the chief and confirm in writing the right to use the land in perpetuity. These land-use rights could not be sold or transferred except through inheritance, typically passing first to the widow(er) and then to children. By giving households an explicit endorsement of their rights by the chief, the certificates clarified the chief's intention (much as firms in the U.S. rely on guidance from regulators) and were meant to reduce the perceived risk of reallocation. These documents, together with the boundary registry, would also be the sources used by the VLC and headman to adjudicate private disputes. The VLC and registry were meant to protect a household's land from encroachment by private actors.

COMACO's intervention began by conducting awareness meetings with chiefs and headmen before forming farmer groups in each village. Each group was trained in agroforestry, and lead farmers were chosen to help disseminate best practices to the rest of the village. COMACO distributed high-quality seedlings and established nurseries for two tree species (Gliricidia and Msangu). Households typically received and planted 3 to 5 seedlings per field.[6] COMACO led additional trainings on how farmer groups could manage their nurseries, how to prepare fields, how to successfully intercrop, and when to plant. COMACO gave con-

[5] DLAs, such as the CDLA, are community-based organizations founded under the broader umbrella consortium of the national Zambia Land Alliance. They promote greater security and ownership of land through advocacy and community outreach.

[6] On the open market these seedlings would each typically cost $0.40 to $1.00 USD.

tinuous support (such as groundnut seeds[7] or wells[8]) as necessary and monitored planting, survival, and threats. In short the extension paid all direct financial costs for getting a seedling and learning how to care for it. The farmer only had to invest time in being trained and in caring for the seedlings.[9]

The two treatments were cross-randomized across villages in the four chiefdoms of Mnukwa, Mkanda, Mshawa, and Maguya. Field teams obtained from each chief an official list of the villages in his or her chiefdom.[10] The treatments were randomized within chiefdom to ensure treatment was orthogonal to chiefdom-level characteristics, but the randomization was otherwise unstratified. The original intent was to have 75 villages in each treatment arm (Control, Agroforestry, Land Tenure, or Agroforestry + Land Tenure) for a total sample of 300. But the official lists contained only 276 villages, and after randomization the field teams discovered inaccuracies in the official lists, either because some clusters of households were incorrectly recorded as separate villages, or because the official list recorded some villages incorrectly as belonging to a study chiefdom when they were in fact part of a neighboring chiefdom.[11] In neither case was the village included in the study. In cases where it was discovered that a cluster of villages was a single village, all households were assigned the treatment status of the primary cluster. Throughout what follows we use treatment assignment as our "treatment" variable. Since treatment assignment was randomized there is no reason to expect these logistical problems to bias the results.[12] The ultimate consequence is that the final sample is 244 villages.[13]

3.2. Data and baseline balance

The baseline data were collected in June of 2014 before the intervention.[14] Three years later the study team resurveyed the same households, as well as a randomly selected set of new households to maintain sample size in the face of attrition (which was roughly 13 percent). The survey asks households whether and why they practice agroforestry, which fields they have left fallow, which investments they have made, their perceptions of tenure security, their experiences of land-related conflict, and about the physical characteristics of their land.

The certification part of the tenure intervention—the formation of VLCs, the boundary walks, the participatory mapping—was done by early 2016. At the time of the endline survey, in mid 2017, households had had one full agricultural year after certification. Though paper land certificates were still being distributed at the time of the endline survey, the other parts of the certification process—especially the map marking each household's boundaries—would also directly improve perceptions of tenure security. We show in Section 4.1 that the tenure intervention does make households feel more secure.

Table 1 shows baseline summary statistics in the control group and shows how each treatment group differs from the control group. The column labeled "P-value" tests for whether the means in the treatment groups are jointly different from that of the control group. There is balance across most variables, though it appears that households in the control group farm significantly fewer fields than the treated groups.[15] Since treatment status is randomized, this difference must have arisen by chance. The balance in other outcomes suggests it is not a sign of any fundamental difference between the groups.

However, it may mechanically induce imbalance for other outcomes measured by field (because households with more fields must take a longer survey). To avoid any bias that may arise as a direct result of the difference in the number of fields, all field-level results are validated by re-running the tests on only the first field. As shown in Table 1, field-level results that might otherwise be rendered imbalanced—for example, the perceived likelihood of encroachment or reallocation—are generally balanced for the first field. Enumerators were instructed to ask first about the household's largest field (and thus the most important). There is marginal imbalance in the household's perception of likelihood of encroachment by other households or reallocation by the chief. Given the number of outcomes tested (and the fact that we are not correcting for multiple inference) it is not surprising that one set of related outcomes would be imbalanced by chance.[16] We also re-run all tests after aggregating outcomes by household.

The last row of Table 1 tests for differences in attrition from the baseline to the endline survey. Average attrition is 13 percent, not atypical for a three-year follow-up survey. Most important is that the attrition is similar across treatment groups, suggesting treatment did not trigger any sample selection.

3.3. Estimation

We confirm the results are robust by estimating the treatment effects with three different specifications. The most basic simply compares outcomes between treated and control groups in the post-intervention data

$$Y_{vi,t=1} = \alpha + \sum^{j} \beta^{j}_{post} T^{j}_{v} + \epsilon_{vi,t=1} \tag{1}$$

where $Y_{vi,t=1}$ is the endline outcome for i, which may be either a plot or a household, in village v at endline ($t = 1$). $\{T^j_i\}$ are dummies that equal 1 if i received the tenure, agroforestry, or combined treatment (all three equal zero if i is in the control group). Since this specification requires only endline data, it yields the largest estimation sample

[7] In year 2, every village was given access to a groundnut "seed fund" to provide groundnut seeds to households who wished to intercrop their trees with groundnuts. This was not part of the original intervention design, but developed organically from community needs.

[8] As a result of severe water shortages that threatened seedling survival, 47 communities were provided with a well as part of the agroforestry intervention.

[9] Note that in the Zambian context, fruit-producing trees (as studied in Berry, 1988) and "life-trees" serve the traditional role of signaling claim to a plot of land. Agroforestry trees do not have the same symbolic role, making the decision to plant a tree not directly comparable to that studied in Besley (1995). Indeed, the results reported in Table 2 confirm that the agroforestry intervention did not improve perceptions of tenure security.

[10] USAID mandated that the sample must be chosen using the official lists rather than an enumeration constructed by the study team itself.

[11] USAID was unwilling to consider any adjustments to the study sample at this stage. As a result we were not able to expand the sample or re-randomize with a manually verified list.

[12] In practice, actual treatment status deviates from assignment for only 4 villages.

[13] After corrections the final list contained 245 villages. Only one village refused to participate, yielding a total of 244 villages.

[14] Within each village the target sample was 13 households. In many cases the village had fewer than 13 households, in which case every household was surveyed. In larger villages the survey team and head person partitioned the village into 4 strata and randomly sampled within each stratum. The strata (with number of observations) are female-headed households (3), younger heads of household (3), wealthy households (3), and all others (4). Younger households are defined as those younger than 35, and "wealthy" households are defined as those with metal roofs on their homes (as opposed to grass roofs). Our final sample contains 27% female-headed households and 37% young households, as compared to 23% and 38% in the 2010 Census. Given that there is no clear evidence of heterogeneous impacts by these categories (see Online Appendix A.4.2), the sample composition is unlikely to be driving our estimates.

[15] Two areas of land are marked as distinct fields if either there is a natural geographic separation between them or they are contiguous but planted with different crops.

[16] We would expect at least 1 rejection of the null, and since responses to the "likelihood" questions are highly correlated it is not surprising to find a second rejection conditional on the first. Correcting for multiple inference would show that the effects are not significant, but for the sake of transparency we report uncorrected tests.

Table 1
Balance at baseline.

	Control Mean	Difference from Control			P-value
		Tenure	Agro	Both	
Agroforestry on Any Field	0.10	−0.00	0.00	0.04	0.340
	(0.02)	(0.02)	(0.02)	(0.02)	
Left Any Field Fallow	0.10	0.00	−0.02	0.02	0.256
	(0.01)	(0.02)	(0.02)	(0.02)	
Number of Fields Farmed	2.32	0.36	0.20	0.25	0.000
	(0.06)	(0.08)	(0.08)	(0.09)	
Have At Least 1 Field	0.99	−0.00	−0.00	−0.01	0.366
	(0.00)	(0.00)	(0.01)	(0.01)	
Female-Headed HH	0.25	0.04	0.03	−0.00	0.113
	(0.02)	(0.03)	(0.03)	(0.02)	
Total Area Owned (ha)	2.56	−0.13	−0.61	0.58	0.127
	(0.53)	(0.62)	(0.54)	(0.85)	
Had Land Reallocated?	0.02	−0.00	−0.00	−0.00	0.923
	(0.01)	(0.01)	(0.01)	(0.01)	
Can write name?	0.65	−0.02	0.01	−0.01	0.840
	(0.02)	(0.03)	(0.03)	(0.03)	
Can read newspaper?	0.52	−0.04	−0.02	−0.02	0.663
	(0.02)	(0.03)	(0.03)	(0.03)	
Considered local?	0.98	−0.01	−0.01	−0.00	0.678
	(0.01)	(0.01)	(0.01)	(0.01)	
Have Paper Document for Field [a]	0.01	0.00	−0.00	0.00	0.875
	(0.00)	(0.01)	(0.01)	(0.01)	
Year acquired (if known) [a]	1999.80	2.03	1.35	0.87	0.180
	(0.69)	(0.94)	(0.96)	(0.98)	
Likelihood Other HHs Encroach [a]	1.87	0.10	0.25	0.13	0.060
	(0.06)	(0.09)	(0.09)	(0.08)	
Likelihood Elites Encroach [a]	1.66	−0.05	0.08	−0.00	0.416
	(0.05)	(0.07)	(0.07)	(0.07)	
Likelihood Neighboring Villages Encroach [a]	1.60	0.10	0.13	0.06	0.401
	(0.06)	(0.08)	(0.08)	(0.08)	
Likelihood Family Encroachs [a]	1.71	−0.02	−0.06	−0.13	0.297
	(0.06)	(0.08)	(0.08)	(0.07)	
Likelihood Chief Reallocates [a]	2.03	−0.22	−0.18	−0.12	0.069
	(0.06)	(0.09)	(0.09)	(0.08)	
Likelihood Head Reallocates [a]	1.61	−0.10	−0.11	−0.13	0.269
	(0.05)	(0.07)	(0.07)	(0.07)	
Attrit from Baseline to Endline	0.13	0.02	0.05	0.03	0.330
	(0.02)	(0.02)	(0.02)	(0.03)	
Households at Baseline	664	721	713	770	

Note: Each cell shows the estimate and the standard error (in parentheses). The p-value reports a test on the hypothesis that the means in the three treatment groups are jointly different from the control and each other. Inference is clustered by village (the unit of randomization). For likelihood questions households were asked to use a Likert scale (1 is impossible, 6 is currently occurring). Households who refused to respond were coded as a 7. For this table we include the non-responders to test for differential non-response by treatment.

[a] Outcome is household's raw categorical response for the first field reported by the household (see text for explanation).

(roughly 7200 fields farmed by 2700 households).

Our second specification controls for the baseline outcome (the outcome at $t = 0$):

$$Y_{vi,t=1} = \alpha + \rho Y_{vi,t=0} + \sum^{j} \beta^{j}_{base} T^{j}_{v} + \varepsilon_{vi,t=1} \quad (2)$$

Our last specification measures the effect of treatment on the first-difference:

$$\Delta Y_{vi,t=1} = \alpha + \sum^{j} \beta^{j}_{fe} T^{j}_{v} + \Delta\varepsilon_{vi,t=1} \quad (3)$$

Since we observe only two periods of data, the first-difference estimates $\{\beta^{j}_{fe}\}$ are algebraically identical to those that would be produced by controlling for plot or household fixed-effects.[17] As the subscript *fe* implies, in the tables that follow we refer to these as the fixed-effects estimates. Since Specifications 2 and 3 require the outcome to be observed at both baseline and endline, they must be run on a smaller estimation sample (roughly 4200 fields farmed by 2300 households).

Since treatment is randomized, an ordinary least squares estimate of these three equations yields consistent estimates of the treatment effects (indeed, they should be asymptotically equivalent). We cluster the standard errors by village, the unit of randomization.

Finally, when measuring the treatment effect on perceptions of insecurity, which households report on a scale from 1 to 6, in some specifications we estimate ordered logistic regressions. In the main text we report the results of regressions that take (1) as the index function. In the appendix we also report estimates using an index similar to (2), except we nonparametrically control for the lagged value using dummies for every possible response to $[Outcome]_{vi,t=0}$. As with the ordinary least squares regressions we cluster inference in the logistic regressions by village.

Our primary outcomes for measuring investment are dummies for practicing agroforestry and fallowing land. These outcomes were registered in the American Economic Association RCT Registry before end-

[17] To be precise they are equivalent to those estimated from the regression $Y_{vi,t} = \alpha_{vi} + \gamma \mathbb{I}(t=1) + \sum^{j} \beta^{j}_{base} T^{j}_{v} \times \mathbb{I}(t=1) + \varepsilon_{vi,t}$ where $\mathbb{I}(t=1)$ is a dummy for the post-intervention period.

line data had been compiled and analyzed.[18] Households are asked whether they have planted any of the 4 major nitrogen-fixing agroforestry trees common in the region (two of which were distributed in the intervention plus the other two common varieties). The dummy equals 1 if such a tree is planted on either the field (for field-level regressions) or on any field (for household-level regressions). We define two measures of fallowing, each based on a different survey question. The first is a dummy for whether at endline the household reported having fallowed its field in the previous 3 years. This measure is imperfect for two reasons: it partly reflects the household's actions in the two years before certification was completed for all villages, and because the question was asked slightly differently at baseline, which could widen the confidence intervals of Specifications 2 and 3.[19] We thus also measure the impact on a dummy for whether the household is currently fallowing the field (or any field, for household-level regressions).

4. Impact of the intervention

4.1. First stage: the intervention strengthened land rights

As noted in Section 3.1 the land tenure intervention had many parts. Panels A and B of Table 2 show that households in treated villages are many times more likely to have been exposed to each part. We estimate Equation (1) at the household level, testing for simple differences between treated and control households in the endline data.[20]

Column 1 of Panel A shows that households in the tenure and combined treatment groups were roughly 50 percentage points more likely to report having paper documentation for at least one of their fields (over an average of 8 percent in the control group). As expected, households who received only the agroforestry treatment are not statistically different from the control group. This pattern is repeated in the treatment effects on the other certification-related outcomes. Columns 2 and 3 show that households who got the tenure intervention are far more likely to have heard of customary land certificates and to be able to describe them accurately. Column 4 shows that treated households are more likely to report a village land committee (VLC) was formed in their village. Column 5 shows that these households are more likely to report that CDLA had visited the village, suggesting households were sufficiently involved to remember the name of the organization.

Columns 1–4 of Panel B report treatment effects on the likelihood a household participated in different parts of the certification. Column 1 shows that treated households are far more likely to have attended village meetings where maps of the village were made or presented. Columns 2, 3, and 4 show that treated households are more likely to have done a boundary walk, to have entered their name in the village land registry, and to have examined the village map and registry.

Finally, Column 5 of Panel B tests for differences in whether households report participating in COMACO's agroforestry extension program. As expected, households given only the tenure treatment are statistically no different from the control group, whereas those given the agroforestry treatment (either alone or together with the tenure treatment) are some 35 percentage points more likely to have participated. Taken with the preceding results, this estimate suggests both interventions were successfully rolled out to the households who were supposed to receive them.

Panel C of Table 2 suggests that these tenure certification activities succeeded in making households feel more secure. We estimate ordered logistic regressions to test for whether households that received the tenure intervention are more likely to report feeling secure in their tenure. As described in Section 2.3, households were asked to rate on a scale from 1 to 6 the likelihood of a loss of land to each of several agents. We test for differences in the post-intervention data between treatment and control households. We report the marginal effect on the probability the household reports the chance of a loss as "impossible."

Columns 1–6 suggest that the tenure intervention made households 5 to 7 percentage points more likely to consider a loss impossible. Households feel more secure against all threats, regardless of whether they are private or public. Most notably, households think reallocation by the chief or headman 7 percentage points less likely. Though they are the ones to issue and enforce the land use certificates, households evidently consider them credible in their promise not to take the land. The combined intervention had slightly smaller but still positive and generally significant effects. In no case can we reject equality in the sizes of the two treatment effects. Households given only the agroforestry intervention are statistically indistinguishable from the control group.[21]

Together these estimates imply a 5 to 7 percent increase in the percentage who believe encroachment impossible. The prior literature is largely silent on whether these estimates are large or small. To our knowledge Ali et al. (2014) is the only prior study to estimate the impact of improved land rights on both investment and perceptions of tenure security.[22] Their observational estimates imply that tenure regularization on average increases the proportion who perceive no risk by 2–6 percent while also increasing investment in soil conservation measures.[23] Our estimates are comparable or perhaps even slightly larger than theirs.[24] Based on the what evidence we have, the prior literature suggests our estimated reduction in perceived tenure insecurity is at least as large as that found in observational studies that do find an impact on investment.

4.2. Second stage: the tenure intervention had no effect on pre-registered measures of investment

Table 3 reports the impact of treatment on three measures of the pre-registered outcomes. Panel A reports field-level regressions, Panel B restricts to the first field only, and Panel C reports household-level regressions. In each panel and for each outcome we estimate the treatment effects using each of the three specifications.

All of the estimates imply the tenure intervention had no impact. Column 1 of Panel A, which estimates Specification 1 on field-level outcomes, suggests the tenure intervention had zero impact on whether the household planted an agroforestry tree on the field. Columns 2 and 3, which estimate Specifications 2 and 3, yield slightly different point

[18] See https://www.socialscienceregistry.org/trials/2315. The outcomes were registered roughly 1 week before the endline household survey was completed and roughly 10 weeks before Shenoy received the data for analysis.

[19] At baseline households were asked if they had fallowed the field in the previous 5 years while at endline they were asked if they had fallowed in the previous 3 years. This difference will not bias our estimates because the randomized assignment to treatment is uncorrelated with the measurement error. But it may increase the standard errors of the estimates.

[20] We cannot estimate the other specifications for most of these outcomes because they were only collected at endline.

[21] This result may appear to contradict Besley (1995), who argues that planting trees on customary land establishes some customary claim to the land. But as noted in Section 3.1, in Zambia an agroforestry tree is not viewed as legitimizing claim to a plot of land.

[22] They compare households on either side of the boundary of an area designated by the Rwandan government for a pilot program that regularizes household land tenure. They find that treated households increase investment in soil conservation measures, but also test for changes in a binary measure for whether the household perceives any chance of land expropriation in the next 5 years. This measure is roughly the complement of our measure for whether the household perceives encroachment to be impossible.

[23] They do not detect any statistically significant additional impact on female-headed households, though they do find much a much bigger impact on investment among these households.

[24] OLS regressions of the impact of the intervention on a dummy for whether the household reports no fear of any agent imply an impact of 4–9 percent. See the appendix for regression results.

Table 2
First-stage effects.

	(1)	(2)	(3)	(4)	(5)
Panel A: Program Participation					
	Any Documentation?	Heard of CLCs?	Can Describe CLC?	VLC Ever Formed?	CDLA in Village?
Tenure	0.512***	0.338***	0.288***	0.433***	0.640***
	(0.042)	(0.042)	(0.038)	(0.040)	(0.035)
Agroforestry	0.033	0.046	0.049	0.007	0.069*
	(0.031)	(0.040)	(0.031)	(0.033)	(0.040)
Both	0.487***	0.327***	0.317***	0.486***	0.637***
	(0.043)	(0.041)	(0.036)	(0.033)	(0.037)
Control Mean	0.08	0.20	0.12	0.15	0.14
Test: T = AT	0.654	0.798	0.505	0.174	0.945
Test: A = AT	0.000	0.000	0.000	0.000	0.000
Households	2755	2809	2809	2809	2809
Villages	244	244	244	244	244
Panel B: Program Participation, cont.					
	Mapping?	Boundary Walk?	Registered Land?	Examine Map?	Did Agro Program?
Tenure	0.541***	0.530***	0.576***	0.540***	0.055
	(0.033)	(0.033)	(0.031)	(0.031)	(0.042)
Agroforestry	0.029	0.032	0.029	0.035	0.345***
	(0.032)	(0.031)	(0.031)	(0.027)	(0.038)
Both	0.537***	0.549***	0.581***	0.522***	0.379***
	(0.032)	(0.034)	(0.033)	(0.034)	(0.035)
Control Mean	0.08	0.08	0.10	0.07	0.22
Test: T = AT	0.915	0.609	0.877	0.658	0.000
Test: A = AT	0.000	0.000	0.000	0.000	0.340
Households	2809	2809	2809	2809	2809
Villages	244	244	244	244	244

	(1)	(2)	(3)	(4)	(5)	(6)
Panel C: Perceptions of Tenure Security (Field-Level, First Field Only)						
	Other HHs	Elites	Neighbor Vill.	Family	Chief	Vill. Headman
Tenure	0.065**	0.047*	0.054**	0.060*	0.067**	0.071***
	(0.028)	(0.027)	(0.025)	(0.031)	(0.029)	(0.026)
Agroforestry	−0.007	−0.023	0.003	−0.006	0.013	0.000
	(0.025)	(0.026)	(0.023)	(0.028)	(0.029)	(0.023)
Both	0.046*	0.015	0.034	0.048*	0.043*	0.042*
	(0.026)	(0.025)	(0.024)	(0.029)	(0.026)	(0.024)
Control Mean[a]	0.78	0.80	0.78	0.75	0.72	0.77
Test: T = AT	0.520	0.248	0.467	0.725	0.396	0.297
Test: A = AT	0.052	0.144	0.211	0.067	0.269	0.087
Fields	2760	2760	2760	2759	2759	2760
Villages	244	244	244	244	244	244

Note: "CLC" stands for customary land certificate, the paper documentation of customary rights distributed through the intervention. "VLC" stands for village land committee, a committee empowered to mediate land disputes. "CDLA" stands for Chipata District Land Alliance, the organization that did the land tenure intervention. The first outcome, a dummy for whether the household reports having paper documentation for any of its fields, excludes 54 households that rent all of their fields. For Panel C we estimate ordered logit regressions and report the effect of treatment on the marginal probability the household reports encroachment by the actor to be "impossible." The row "control mean" gives the mean in the control group at endline. The tests are for whether the Tenure coefficient and the Agroforestry coefficient differ from the combined treatment. All standard errors are clustered by village (the unit of randomization).
[a] Fraction of households in the control group reporting that encroachment is "impossible."

estimates, but all are small and statistically insignificant. By contrast, the agroforestry intervention made households 13 to 16 percentage points more likely to plant a tree, depending on the specification. And households that received both the agroforestry and tenure intervention planted trees at a similar rate to those who received the agroforestry intervention alone. The row labeled "p(Both = Agroforestry)" gives the p-value of the test for equality between the two treatment effects. In no case can we reject that they are identical. Taken together these results suggest the tenure intervention had no impact either by itself or when combined with the agroforestry intervention.

Columns 4–9 suggest that it likewise had no significant impact on whether the household fallowed its field in the prior 3 years or is currently fallowing the field. And although the combined intervention seems to have a significant effect on whether the household is currently fallowing (Columns 7–9), this is likely a statistical fluke driven by uncorrected multiple inference. The combined intervention has no comparable statistically significant estimate in Columns 7–9 of Panel C. It likewise has no effect in any specification on the dummy for whether the field was fallowed in the past 3 years. And there is no obvious explanation for why land tenure only increases fallowing when combined with agroforestry training.

Panels B and C run similar regressions restricting the sample to the household's first field and aggregating all outcomes by household (see Section 3.2 for an explanation of why we verify the results after mak-

Table 3
Treatment effect on key outcomes.

	Agroforestry on Field			Has Fallowed Field			Currently Fallowing Field		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Field-Level									
Tenure	−0.000	0.002	0.012	−0.017	0.001	−0.001	−0.007	0.002	0.017
	(0.016)	(0.022)	(0.020)	(0.013)	(0.015)	(0.017)	(0.009)	(0.001)	(0.012)
Agroforestry	0.132***	0.164***	0.166***	−0.028**	−0.014	−0.004	−0.008	0.002	0.014
	(0.017)	(0.023)	(0.024)	(0.012)	(0.012)	(0.016)	(0.010)	(0.001)	(0.011)
Both	0.139***	0.151***	0.142***	−0.020	−0.000	−0.007	0.003	0.003**	0.020*
	(0.016)	(0.022)	(0.022)	(0.013)	(0.013)	(0.018)	(0.011)	(0.002)	(0.012)
Baseline Outcome		X			X			X	
Field FEs			X			X			X
Baseline Mean	.047	.047	.047	.051	.051	.051	.045	.045	.045
p(Both = Agroforestry)	.706	.563	.342	.452	.224	.875	.276	.443	.337
p(Both = Tenure)	0.000	0.000	0.000	0.798	0.946	0.752	0.300	0.441	0.623
Fields	7258	4210	4210	6694	4079	4079	7173	4250	4250
Clusters	244	242	242	244	241	241	244	242	242
B. Field-Level (First Field Only)									
Tenure	−0.005	0.007	0.022	0.000	0.021	0.017			
	(0.031)	(0.035)	(0.034)	(0.015)	(0.020)	(0.024)			
Agroforestry	0.245***	0.285***	0.279***	−0.015	−0.008	−0.006			
	(0.036)	(0.046)	(0.047)	(0.013)	(0.015)	(0.023)			
Both	0.254***	0.242***	0.223***	0.011	0.019	0.004			
	(0.032)	(0.038)	(0.040)	(0.015)	(0.018)	(0.027)			
Baseline Outcome		X			X				
Field FEs			X			X			
Baseline Mean	.078	.078	.078	.073	.073	.073			
p(Both = Agroforestry)	.799	.337	.238	.0521	.103	.721			
p(Both = Tenure)	0.000	0.000	0.000	0.503	0.916	0.645			
Fields	2613	1806	1806	2569	1786	1786			
Clusters	244	239	239	244	239	239			
C. Household-Level									
Tenure	−0.007	0.003	0.009	−0.020	−0.006	−0.015	−0.017	−0.017	−0.034
	(0.040)	(0.038)	(0.035)	(0.022)	(0.024)	(0.028)	(0.022)	(0.023)	(0.032)
Agroforestry	0.305***	0.303***	0.304***	−0.043**	−0.040**	−0.029	−0.013	−0.008	−0.021
	(0.042)	(0.042)	(0.042)	(0.018)	(0.019)	(0.025)	(0.022)	(0.023)	(0.030)
Both	0.319***	0.305***	0.287***	−0.029	−0.022	−0.041	0.002	0.008	−0.007
	(0.038)	(0.038)	(0.040)	(0.019)	(0.021)	(0.029)	(0.024)	(0.024)	(0.030)
Baseline Outcome		X			X			X	
Field FEs			X			X			X
Baseline Mean	.11	.11	.11	.098	.098	.098	.1	.1	.1
p(Both = Agroforestry)	.737	.973	.699	.365	.292	.677	.493	.489	.603
p(Both = Tenure)	0.000	0.000	0.000	0.673	0.481	0.402	0.393	0.275	0.368
Households	2760	2342	2342	2732	2297	2297	2761	2356	2356
Clusters	244	242	242	244	242	242	244	242	242

Note: The row "baseline mean" gives the mean across the entire pre-intervention sample. As the "currently fallowing" variable cannot be defined for the first field (see text) Columns 7–9 of Panel B are left blank. The sample sizes vary across some specifications because either baseline data are not available for all households (those randomly added at endline), or because some outcomes are missing while others are not. It is not possible to define a single sample that works for all outcomes because those who report "Currently Fallowing Field" are not asked whether they have fallowed the field in the past 3 years. Standard errors are clustered by village (the unit of randomization).

ing this restriction). Since enumerators generally did not ask first about currently fallow fields there is too little variation in the dependent variable to estimate first-field regressions for that outcome. None of these variations in sample definition or specification makes much difference in the results.

It is hard to look at the broad pattern of estimates in Table 3 and infer that the tenure intervention had the sort of transformative impact found in observational studies.[25] The 95 percent confidence intervals rule out a greater than 1 percentage point increase in fallowing (based on field-level estimates) and a 2 to 3 percentage point increase at the household-level. We can likewise rule out anything greater than a 3.2 percentage point increase in agroforestry in the field-level estimates, far below our estimates of the impact of the agroforestry intervention. Even if the true impact of tenure security is slightly positive, it is small compared to the direct impact of relaxing constraints.[26]

4.3. There is no evidence of larger effects on fearful households

One might wonder if the estimates of Section 4.2 measure the treatment effect on the wrong subpopulation. A land tenure intervention might mainly affect households who felt insecure at baseline. It is possible that the average effects in Table 3 drown out a large heterogeneous effect.

[25] In some specifications the agroforestry intervention has a negative effect on one measure of fallowing. Given that the effect is absent in other specifications and when tested on the alternative measure of fallowing, this too is likely a statistical fluke.

[26] Appendix A.3 presents 95% confidence intervals for all of the estimates of Table 3.

Table 4
No Evidence of Larger Effects on Households Fearful at Baseline (The Household is the Unit of Analysis).

	Agroforestry, Any Field			Has Fallowed Any Field			Currently Fallowing Any Field		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Tenure	0.013	0.017	0.022	−0.000	0.002	0.017	−0.015	−0.018	−0.038
	(0.046)	(0.041)	(0.041)	(0.030)	(0.030)	(0.036)	(0.027)	(0.028)	(0.036)
Agroforestry	0.315***	0.316***	0.316***	−0.046*	−0.045*	−0.026	−0.003	−0.005	−0.020
	(0.051)	(0.048)	(0.049)	(0.024)	(0.024)	(0.031)	(0.029)	(0.029)	(0.035)
Both	0.335***	0.316***	0.287***	−0.006	−0.003	−0.017	0.026	0.022	−0.016
	(0.046)	(0.043)	(0.048)	(0.026)	(0.026)	(0.037)	(0.031)	(0.031)	(0.036)
Fear (Baseline)	0.066*	0.073*	0.046	0.005	0.001	−0.004	0.011	0.010	−0.048
	(0.038)	(0.039)	(0.044)	(0.024)	(0.024)	(0.030)	(0.029)	(0.030)	(0.044)
Tenure × Fear	−0.028	−0.028	−0.026	−0.017	−0.021	−0.080	0.001	0.003	0.006
	(0.060)	(0.059)	(0.062)	(0.038)	(0.038)	(0.050)	(0.040)	(0.040)	(0.055)
Agroforestry × Fear	−0.030	−0.031	−0.027	0.013	0.010	−0.006	−0.008	−0.007	0.000
	(0.052)	(0.053)	(0.061)	(0.034)	(0.033)	(0.041)	(0.039)	(0.039)	(0.059)
Both × Fear	−0.049	−0.030	−0.002	−0.037	−0.040	−0.050	−0.037	−0.032	0.021
	(0.057)	(0.058)	(0.067)	(0.034)	(0.034)	(0.046)	(0.043)	(0.043)	(0.057)
Baseline Outcome		X			X			X	
Household FEs			X			X			X
Baseline Mean	.11	.11	.11	.098	.098	.098	.1	.1	.1
p(Both = Agroforestry)	.701	.999	.605	.0918	.0854	.81	.332	.367	.894
Households	2355	2342	2342	2330	2297	2297	2356	2356	2356
Clusters	242	242	242	242	242	242	242	242	242

Note: "Fear" is a dummy for whether the household thought encroachment on any field likely at baseline. The row "baseline mean" gives the mean in the entire pre-intervention sample. See the note of Table 3 for an explanation of why samples vary across specification. Standard errors are clustered by village (the unit of randomization).

Table 4, which adds interaction terms to the household-level specifications in Panel C of Table 3, do not support this hypothesis. We test for whether the interaction of the treatment effects and a dummy for whether the household thought encroachment on any of its fields by any agent to be likely. None of the interaction terms are significant, and the signs of the point estimate imply the tenure intervention had a smaller (or even a negative) effect on some outcomes. In unreported regressions we confirm that these results are no different when we test them at the level of the field.

We also run these tests for a host of alternative definitions of baseline insecurity: dummies for reporting encroachment likely for each of the six sources of risk, the index from ordered logit regressions of baseline perceptions on baseline characteristics (e.g. tribe, previous land disputes), the predicted insecurity from those ordered logit regressions, and several aggregations of these measures. We find that the resulting interaction coefficients are significant at essentially the rate expected by chance (and where coefficients are significant they are generally of the wrong sign).

4.4. There is no impact on other measures of investment or labor allocations

One might also wonder if the null results of Section 4.2 are driven by the type of investment we study. Fenske (2011) finds in a meta-analysis that studies of tenure and investment that focus on discrete investments generally have larger p-values. Column 1 of Table 5.A estimates Equation (1) on the fraction of fields that the household has fallowed. This continuous measure of fallowing still shows no effect.[27] Another possible critique is that fallowing and agroforestry are large investments unlikely to be affected by any intervention. Though it is precisely such large, long-term investments that tenure insecurity most strongly discourages, we also test whether a smaller investment—fertilizer use—is affected by the intervention. Columns 2 and 3 of Table 5.A suggest neither a dummy for positive fertilizer use nor the total kilograms of fertilizer used are affected by any of the treatments.[28]

As noted in Section 2.1, several prior observational studies have focused on the effect of tenure on labor allocation rather than investment. Field (2007) finds that urban households granted deeds to their homes allocate more labor to market work and less to guarding their homes. De Janvry et al. (2015) and Valsecchi (2014) find that households exposed to a certification program in Mexico were more likely to migrate. But Columns 4 and 5 of Table 5.A suggest there is no evidence that the tenure treatment induced household members to hire out as casual labor (ganyu) or migrate.

Finally, Panel B of Table 5 shows that there is no effect on a set of other investments of varying size and baseline prevalence. Households are no more likely to attempt ambitious investments like zero tillage farming than mundane ones like spreading manure.[29] The non-effect on fencing (Column 4) is of special interest given Hornbeck's (2010) conclusion that the diffusion of barbed wire fencing throughout the United States in the late 1800s had a transformative impact on agriculture. Though the tenure intervention created maps that set clear and commonly acknowledged boundaries to everyone's land, it does not induce farmers to build fences on those boundaries.[30]

5. Why do our results differ from those of the prior literature?

5.1. Differences in identification assumptions

The key difference between our study and what came before is that it combines an experimental research design with precise measures of

[27] The other specifications, which show similar results, are in Online Appendix A.2.

[28] This variable is defined to include any type of fertilizer, organic or inorganic, including manure. Column 5 of Panel B is based on a separate question that asks specifically about manure.

[29] The negative impact on Zero Tillage is likely a fluke that arises from not correcting for multiple inference. The other specifications reported in Online Appendix A.2 do not corroborate this estimate, which in any case has the wrong sign.

[30] One caveat is that the culture of private property and exclusion from private land is much stronger in the U.S. than in Zambia (or other sub-Saharan African countries like Ghana). It may not be culturally salient for Zambian farmers to fence off their land. But in the same vein, that may suggest why tenure insecurity is less salient in Zambia than Westerners might expect.

Table 5
Treatment has No Effect on Other Measures of Investment or Labor Allocation (The Household is the Unit of Analysis).

	(1)	(2)	(3)	(4)	(5)
Panel A					
	Fraction Fallow	Any Fertilizer?	Total Fertilizer	Casual Labor?	Any Migration?
Tenure	−0.008	−0.015	−4.964	0.028	−0.022
	(0.008)	(0.041)	(21.561)	(0.034)	(0.020)
Agroforestry	−0.004	0.009	24.919	0.019	−0.014
	(0.009)	(0.035)	(23.510)	(0.032)	(0.018)
Both	0.000	−0.034	−3.464	0.014	−0.007
	(0.009)	(0.038)	(20.602)	(0.032)	(0.017)
Baseline Outcome					
Baseline Mean	0.04	0.75	160.40	0.44	0.09
Households	2748	2748	2748	2748	2748
Villages	244	244	244	244	244
Panel B					
	Plant Basin	Zero Tillage	Ridging	Fencing	Manure
Tenure	0.004	−0.048**	−0.028	0.003	0.011
	(0.031)	(0.024)	(0.032)	(0.007)	(0.034)
Agroforestry	0.024	−0.040	0.000	0.006	0.044
	(0.035)	(0.026)	(0.034)	(0.007)	(0.033)
Both	0.023	−0.030	−0.039	0.006	0.061*
	(0.032)	(0.025)	(0.031)	(0.008)	(0.033)
Baseline Outcome					
Baseline Mean	0.19	0.13	0.91	0.02	0.29
Households	2748	2748	2748	2748	2748
Villages	244	244	244	244	244

Note: The row "baseline mean" gives the mean in the entire pre-intervention sample. Standard errors are clustered by village (the unit of randomization).

property rights. Besley and Ghatak (2010) write that it is unusually difficult to measure the causal effect of land rights using an observational research design because such rights are not allocated at random. Any observational study must answer why some households are granted rights to their land sooner than others, and whether the reasons are orthogonal to the many other constraints and incentives that govern investment.

Compounding this challenge is that some studies cannot directly measure land rights, relying instead on proxies that partly determine land rights (e.g. political connections). That imposes a further burden on the research design beyond having exogenous variation in the proxy. It is also necessary that the proxy *only* affects investment through its effect on property rights (not unlike the assumptions needed for a valid instrument).

The vast majority of studies use cross-time variation, sometimes with proxies and sometimes with direct measures of land rights. Studies that compare within-household changes in rights (Besley, 1995) or political power (Markussen and Tarp, 2014) must assume whatever triggers these changes does not also allow the household to invest more. Studies that use difference-in-differences to study the roll-out of titling or certification programs (Field, 2007; Valsecchi, 2014; De Janvry et al., 2015, e.g.) must assume areas that received the program sooner were not also changing in other ways that made market work or out-migration more attractive.[31] Studies that measure the differential impact of new technology for securing rights (e.g. Hornbeck, 2010) must make a similar assumption.

Other studies exploit some form of cross-sectional variation. Studies that rely on spatial discontinuities must assume only the strength of land rights changes at the boundary—that the boundary was not drawn to isolate a group that invests more (Markussen and Tarp, 2014, e.g.), or that the authorities in a long-established administrative boundary do not enact policies other than tenure regularization (Besley et al., 2016). Studies that exploit variation induced by institutional quirks must assume the differences arose truly by chance. Galiani and Schargrodsky (2010), who exploit variation induced by a legal dispute against a tenure regularization program, must assume households on disputed plots are comparable to other households. Goldstein and Udry (2008), whose main specification compares plots within a household based on the gender and political power of the plot's cultivator, requires that differences between these plots arises solely through the owner's heightened tenure security.[32]

Though each assumption listed above could be plausible in certain contexts, the benefit of a randomized controlled trial is that no such assumption is necessary. The experimental estimates are unlikely to be contaminated by selection bias. They also measure the effect of a well-defined, precisely measured change in land rights. The treatment strengthened each household's right to hold and use its land, but did not give the household any right to transfer the land or use it as collateral. There is no accompanying change in power, wealth, or financial access that may alter the household's ability to invest for reasons unrelated to tenure security. The risk of expropriation is reduced without any ancillary benefit, leaving the estimates relatively pure.

[31] Field (2007) actually studies the differential impact of Peru's titling program on titled households versus those who had previously been squatters. Her identifying assumption is that the behavior of these two groups is not diverging in places that got the program sooner for reasons unrelated to land tenure.

[32] In Table 8 of their paper they relax this assumption for political power by holding the cultivator fixed and comparing plots that differ only in whether the fields are matrilineal. This specification requires that the matrilineal land of the politically powerful is comparable to their non-matrilineal land (or at least that the difference is similar to the difference between matrilineal and non-matrilineal land of less powerful cultivators). The authors report that the politically powerful invest more in matrilineal land relative to less powerful cultivators. They do not report a similar specification for gender.

5.2. Differences in context: is Zambia comparable to the setting of prior studies?

Is it possible that the difference in results stems not from research design but context? One may wonder if our sample of Zambian farmers is unusual in a way that makes it uniquely unlikely a tenure intervention would have any effect. Any study context is unique; Zambian farmers live in a culture and under institutions that differ from those in, say, Ghana (studied in Besley, 1995; Goldstein and Udry, 2008). But it is hard to imagine the difference is greater than that between Ghana and Communist Vietnam (Markussen and Tarp, 2014) or urban Peru (Field, 2007). Like Ghana, Zambia is a poor sub-Saharan country where farmers have little access to inputs and finance. Their chiefs (at least in the district studied here) are hereditary, have the power to reallocate land, and enforce their decisions through a network of village headmen.[33]

It is hard to claim this situation leaves households feeling far more secure in their tenure than in other studies. As explained in Section 2.2, the 1995 Land Act has amplified farmers' vulnerability. And as shown in Section 2.3, at baseline some 45 percent of households think it likely they will face encroachment in the next 3 years (either by the authorities or private actors). Though expropriation by the chief and headman is rare—at baseline roughly 2.6 percent of households report having land reallocated in the previous 3 years—it is not drastically rarer than in other contexts. For example, the numbers reported in Markussen and Tarp (2014) imply that the comparable number in their sample is 5.9 percent. While this number is certainly higher, it is hard to argue the difference is so large that Vietnamese households face a drastically higher risk of official expropriation. And although Ali et al. (2014) do not report the incidence of expropriation in their Rwandan sample, we compute using their data that roughly 2 percent of households report ever being affected by land expropriation. Only 1.7 percent of households report having a land dispute in the past year, implying at most a 5 percent chance of a dispute over 3 years.[34] In our sample the number is above 20 percent, suggesting that farmers in our sample are if anything less secure than those in Rwanda.

One might also wonder if we focus on the wrong outcomes. Should security of tenure really matter for the planting of trees or the fallowing of land? In fact our primary outcomes were chosen not only because they make sense in this context (see Section 2.2) but because they are comparable to the outcomes measured in the two observational studies done in Ghana (Besley, 1995; Goldstein and Udry, 2008), the context most similar to ours. In contrast to their studies we do not find that granting tenure security induces either investment. Section 4.4 shows that outcomes used in other prior studies are equally unaffected by tenure security. Unlike Field (2007) we find no impact on out-of-home labor. Unlike Valsecchi (2014) and De Janvry et al. (2015) we find no impact on migration. Unlike Ali et al. (2014) we find no impact on the uptake of any soil conservation measures (e.g. ridging or zero tillage planting). And though Hornbeck (2010) studies the impact rather than the uptake of barbed wire fencing, his results suggest households would derive huge benefits from fencing off their land. Yet creating commonly acknowledged boundaries in a village does not induce households to put fences around their land.

One final concern is that strengthening tenure has no impact in our study because households face substantial other constraints to investment. As suggested in Section 2.1, our design addresses this concern by cross-randomizing tenure security with the agroforestry extension. The extension by itself has a large and significant effect on the take-up of agroforestry. That suggests the training and seedlings provided by COMACO alleviated all binding financial and technical constraints for some 30 percent of households (see Table 3.C). But tenure certification did not induce any additional households to take up agroforestry, suggesting tenure insecurity was never a barrier to investment.

5.3. Applying the research designs of observational studies yields estimates similar to the prior literature

What would we have concluded about the effect of tenure security on investment had we used an observational research design naively similar to those discussed in Section 5.1? Suppose we discard the experimental variation by restricting our sample to villages assigned to receive the tenure and combined treatments. As noted in Sections 3.1 and 4.1, by the time of the endline survey all treated villages had undergone certification but not all households had received paper certificates. Define two new measures of land rights: a field-level dummy for whether the household has paper documentation for the field, and a village-level dummy for whether at least half of the fields in the village have documentation. Table 6 shows that even after controlling for household and year fixed-effects, several of our key outcomes are positively correlated with the two certification dummies. The odd-numbered columns show that applying a design based on within-household changes in rights yields estimates orders of magnitude larger than those estimated using experimental variation. The even-numbered columns, meant to mimic a phased certification program, yield similar estimates. The observational estimates for "Has Fallowed" in the past 5 years are particularly large, lying well outside the confidence intervals of the household-level experimental estimates in Table 3.

Rather than studying the phased introduction of a certification program, consider studying changes in the household's political connections. To purge the experimental variation we restrict the sample to villages in the control group. But since this yields a small sample, we also use households surveyed in the non-study chiefdom of Saili. All of these households received the agroforestry extension, but there was no tenure certification program. We define dummies for whether the household is related to the chief or the village head. We test for whether, after controlling for village-year fixed effects, within-household changes in relationship status significantly predict household-level changes in investment. Table 7 shows that being related to the village head is positively correlated with two measures of fallowing.[35] The observational estimates for "Currently Fallowing" lie outside the confidence intervals of the household-level estimates in Table 3. And as we show in Online Appendix A.4.2, there is no evidence that any treatment had a differential impact on households with political connections. That suggests changes in political connections can be correlated with changes in investment even when these connections have no bearing on how a household reacts to a randomized tenure intervention.

Unfortunately our survey does not separately ask each member of the household about political connections, making it impossible to test whether, after controlling for household fixed-effects, variation in the political power of an individual predicts differences in how they manage their field. But we can run such a test on gender. Since this research design does not rely on cross-time variation we use baseline data for the entire study sample. We define dummies for whether the plot is managed by a woman and whether it is jointly managed by the head of household and his or her spouse. We regress the field-level outcomes studied earlier on these two dummies, controlling for household fixed-

[33] At the time of writing there is one unpublished observational study specifically on Zambia. Dillon and Voena (2018) use ethnicity as an instrument for the inheritance rights of widows. Their research design shows results similar to those of observational studies in other contexts. As the study is unpublished we do not discuss it at length, but the similarity of its results to prior work suggests there is nothing about the Zambian context that would drive our results.

[34] The upper bound arises from assuming the chance of a dispute in any year is independent of whether there was a dispute in the previous year. Since there is almost certainly a positive serial correlation, the true number would likely be smaller.

[35] In unreported regressions we show that breaking out immediate and non-immediate relations, as in Markussen and Tarp (2014), makes little difference.

Table 6
Replicating observational designs: Difference-in-Differences.

	Currently Fallow		Has Fallowed		Agroforestry	
	(1)	(2)	(3)	(4)	(5)	(6)
Household Documented	0.014*		0.045***		0.049**	
	(0.007)		(0.015)		(0.020)	
Village Documented		0.016*		0.050***		0.038
		(0.008)		(0.018)		(0.024)
Household FEs	X	X	X	X	X	X
Year FEs	X	X	X	X	X	X
Fields	7158	7372	6703	6884	7137	7382
Households	489	500	458	467	484	495
Villages	123	123	123	123	123	123

Note: Standard errors are clustered by village. Sample restricted to villages that received the tenure certification.

Table 7
Replicating observational designs: Political power.

	(1) Currently Fallowing	(2) Fraction Fallow	(3) Has Fallowed	(4) Agroforestry
Related to Village Head	0.044*	0.025**	0.029	−0.002
	(0.025)	(0.011)	(0.028)	(0.035)
Related to Chief	−0.013	0.001	0.023	0.008
	(0.026)	(0.012)	(0.030)	(0.039)
Household FEs	X	X	X	X
Village-Time FEs	X	X	X	X
Households	2526	2526	2493	2517
Villages	105	105	105	105

Note: Standard errors are clustered by village. Sample restricted to control group and adjacent, non-experimental chiefdom.

Table 8
Replicating observational designs: Plot manager within a household.

	(1) Currently Fallow	(2) Has Fallowed	(3) Agroforestry
Female-Managed	−0.063**	−0.015	−0.037
	(0.028)	(0.028)	(0.033)
Jointly-Managed	−0.005	0.028	−0.070**
	(0.019)	(0.028)	(0.031)
Household FEs	X	X	X
Fields	7145	6696	7065
Households	2825	2714	2747
Villages	244	244	244

Note: Sample restricted to baseline data. Standard errors are clustered by village.

effects.[36] Column 1 of Table 8 shows that, by one measure, fields managed by women are significantly less likely fallowed. Column 3 implies that fields jointly managed by the head and spouse are significantly less likely to be planted with agroforestry trees than plots managed by a man alone. The absolute values of these estimates lie well outside the confidence intervals of our experimental field-level estimates.

These regressions suggest that when we apply several common observational designs to our study sample we find results similar to the prior literature. Households that receive certification sooner or are closer to political leaders are more likely to invest. Plots managed by women (singly or jointly with their spouses) are less likely to invest.

These results by themselves cannot be taken as proof that the prior studies are biased. It is possible the policies studied previously, for example, are rolled out differently. Rather, these regressions show that our study sample is similar enough to those studied previously to exhibit the same key correlations. That makes it more likely our experimental results are not a fluke driven by an unusual study context. It also shows that patterns like these—more investment by men and by the politically powerful—can arise even in a context where an exogenous improvement in land rights has no impact on investment.

5.4. Other recent experimental work

As noted in the introduction, the only prior experimental test of the tenure hypothesis is a recently published study of a randomized program in Benin (Goldstein et al., 2018). The authors find small but statistically significant effects on two outcomes: a 1.7 percentage point increase in the planting of trees, and a 2.4 percentage point increase in the planting of perennial plants. They find no effect on land fallowing, labor, or the use of inputs.[37] We cannot test for an effect on their perennial crop indicator, as the study does not specify which crops are perennial or what additional costs make these crops unappealing when land tenure is insecure. On other outcomes their study largely agrees with ours, with the only difference being the significant impact on tree planting.

It is difficult to pinpoint why Goldstein et al. (2018) find significant effects on tree planting but we do not.[38] The study does not discuss the pre-existing source of tenure insecurity in Benin or the impact of the intervention on perceived tenure security. It is difficult to gauge whether farmers in Benin or Zambia face a more acute expropriation risk, or whether the Benin intervention had a bigger effect on perceptions of tenure security.

[36] By construction we cannot define the fraction of fields left fallow in a field-level regression.

[37] The effect sizes on these are 0.4 percentage points for fallowing, 4.5 days per hectare (compared to a control mean of 108) for labor, and 1.8 percentage points for use of fertilizers or high-yielding seeds.

[38] The closest analog to their 1.7 percentage point effect is our Specification 1 in Panels A and B of Table 3, which show points estimates of 0 or -0.5 percentage points.

One difference between the interventions is the authority under which tenure is recognized. The Benin intervention certified tenure under the authority of the central government, whereas the intervention studied here certified tenure under the authority of chiefs. The type of certifying authority may affect how households perceive the intervention or whether households think themselves better protected against some threats than others. But Table 2.C shows similar improvements in perceived security against all threats, cutting against this latter hypothesis. Still, we cannot rule out some unmeasured difference in how the intervention is perceived.

Another difference is the selection of the study sample of villages. Our sample frame was the set of all villages within four chiefdoms that were willing to participate and accessible to survey enumerators. Villages within this frame were randomly selected for the study (see Section 3). The government intervention studied by Goldstein et al. (2018) was randomized across a national sample chosen based on a "poverty index, potential for commercial activities, regional market integration, local interest in promoting gender equality," and a few other criteria. If some households value tenure security more than others, the difference in samples might induce different results.

One final difference is that unlike the intervention we study, which confers only tenure security, the Benin intervention provides fully transferable land rights that could be upgraded to formal land titles. Though the transferable land certificates had not yet been distributed at the time of their intervention, it is possible that the prospect of getting a transferable right makes households more willing to invest and better able to get loans.

That said, the most important lesson from this comparison is that the estimates of Goldstein et al. (2018) are in magnitude somewhat closer to ours than to much of the observational literature. Though they find statistically significant impacts on tree planting, the point estimates imply treated households are 1.7 percentage points more likely to plant. These impacts are an order of magnitude smaller than those of the observational work that preceded it.

6. Discussion and conclusion

6.1. Why does tenure security have no impact?

A model whose prediction goes unmet is probably based on a questionable assumption. But the tenure hypothesis seems so basic that it may be hard to spot any assumptions beyond the two we have shown to hold in our context. Households do fear expropriation, and land certificates do make them less fearful.[39]

But the tenure hypothesis makes one implicit assumption that often goes unquestioned: that households weigh expropriation risk when deciding whether to invest. Basic though it may seem, this assumption is at odds with what households report in focus groups held after the endline survey. The facilitators of these groups write that although households given land certificates acknowledge that "the threat of encroachment from various actors has decreased," they seem puzzled when asked about how this might affect their decision to invest. The households "viewed these decisions as entirely independent." That would explain why despite widespread perceptions of insecurity at baseline, nearly 93 percent of households report that a lack of formal documentation does not deter them from making investments. But why would households deciding whether to invest see expropriation risk as, at best, a second-order consideration?

The key to the puzzle may lie in what the household loses when expropriated. Though it does lose its investment, that loss is trivial compared to the loss of its land. By itself this combined loss does not resolve the puzzle. Expected utility theory predicts that a risk-averse household left impoverished by the loss of its land will have a very high marginal utility of consumption, making the lost investment even more painful. But many theorists have argued that expected utility theory is a flawed model of how people perceive risk. Under Prospect Theory (Kahneman and Tversky, 1979), for example, agents do not make choices based on their final payoff, just the gain or loss they may face. Prospect Theory also proposes that there is diminishing marginal cost to a loss much as there is diminishing benefit from a gain—the distinctive "S-shaped" utility function.

Under Prospect Theory it may not seem surprising that households ignore expropriation risk when planning investments. A farmer who is expropriated loses her land regardless of whether she invests. That loss is catastrophic enough to put her in a region of the utility function where additional losses have little marginal impact on her utility. Whatever time or effort she puts towards planting a tree on the land might seem trivial. Since the pain of being expropriated is similar regardless of whether she invests, the farmer might not find it useful to consider expropriation risk when deciding whether to invest.

Our claim is not that the null result arises because households behave as per Prospect Theory or any other specific theory of decision-making under uncertainty. We offer this example to only to show that the tenure hypothesis, straightforward though it may seem, rests on implicit assumptions that are not universally accepted.

6.2. Directions for future work

This study tests for whether a randomized intervention to make land tenure more secure induced households to invest in their farms. Though the intervention successfully distributed documentation to most households and improved perceptions of tenure security, it had no significant impact on investment. Our results are at odds with many observational studies that precede it, which find significant or even transformational effects. Our results suggest it is far from settled whether insecurity of land tenure is a key barrier to investment.

The most obvious direction for future work is to assess whether long-term impacts differ from those in the short-term.[40] Although Goldstein et al. (2018) find effects within a time-frame comparable to ours, it is possible that households are waiting to confirm their new rights will be honored, or that markets need time to adjust. Further work may also compare use rights with formal titling, as the most important effects of tenure security may come from being able to sell the land or use it as collateral. Finally, future work might test for whether the source of the authority—the national government, elected leaders, or traditional authorities—affects its impact on investment. This agenda, though incomplete, should give policymakers the tools they need to assess whether and how scarce resources are best spent in documenting who holds a piece of land.

Authorship

Heather Huntington: Conceptualization, Investigation, Writing – review & editing, Supervision, Project administration, Funding acquisition. Ajay Shenoy: Conceptualization, Formal analysis, Writing – original draft, Visualization, Supervision.

Data availability

All data required to replicate this study is included in the Supplementary Materials.

[39] We illustrate the predictions of the canonical model in Online Appendix B.

[40] Results from a long-run follow-up of this intervention may be available in 2022.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jdeveco.2021.102632.

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