Titling indigenous communities protects forests in the Peruvian Amazon

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Developing countries are increasingly decentralizing forest governance by granting indigenous groups and other local communities formal legal title to land. However, the effects of titling on forest cover are unclear. Rigorous analyses of titling campaigns are rare, and related theoretical and empirical research suggests that they could either stem or spur forest damage. We analyze such a campaign in the Peruvian Amazon, where more than 1,200 indigenous communities comprising some 11 million ha have been titled since the mid-1970s. We use community-level longitudinal data derived from high-resolution satellite images to estimate the effect of titling between 2002 and 2005 on contemporaneous forest clearing and disturbance. Our results indicate that titling reduces clearing by more than three-quarters and forest disturbance by roughly two-thirds in a 2-y window spanning the year title is awarded and the year afterward. These results suggest that awarding formal land titles to local communities can advance forest conservation.

tenure reform | decentralization | deforestation | degradation | REDD

Over the last three decades, dozens of developing countries have decentralized forest governance, a phenomenon driven by fiscal and administrative constraints, local community demands for participation, and external pressure from donors (1–3). By one estimate, almost a third of all developing country forests are now managed by local communities, well over twice the share currently found in protected areas (1, 4). Granting indigenous groups and other local communities formal legal title to forests is a leading mechanism being used to implement decentralization, particularly in Latin America (5, 6). For example, by the end of 2000, Latin American and Caribbean countries had awarded local communities formal title to at least 100 million ha of forest (7).

Even as forest tenure reform has gained momentum, however, forest clearing and degradation in developing countries have persisted (8, 9). According to the United Nations Food and Agriculture Organization, the overall rate of deforestation in these countries remains "alarmingly high." For example, in both Latin America and Africa, deforestation averaged one-half of 1% per year in the first decade of the 21st century, five times the global rate (9). Forest clearing and degradation have contributed to a host of global and local environmental problems, including climate change, biodiversity loss, soil erosion, and flooding (8, 10–12).

Given these two concurrent trends, it is important to understand the effect of community titling on forest cover change in developing countries. Previous theoretical and empirical research suggests that it can either stem or spur forest damage. [This paragraph and the next draw from the publication by Blackman et al. (13).] It has long been known that ill-defined property rights can, in principle, create incentives for agents to overexploit natural resources (14, 15). Research focusing specifically on tropical forests has shown that weak property rights can spur forest damage in a variety of ways: by enabling landless migrants to colonize frontier areas (16, 17), by strengthening land managers' preferences for productive activities that provide quick but unsustainable returns (18, 19), by creating incentives for squatters to clear forests to establish use rights or block competing claims (20, 21), and by preventing land managers from participating in payments for environmental services and reducing emissions from deforestation and degradation initiatives (22, 23). In principle, granting title to indigenous communities could mitigate each of these problems.

Previous research also suggests that titling can increase forest cover change, however. Giving title to entire communities instead of individual households can recreate common-pool resource problems on a local level, which the communities may or may not be willing and able to address (24, 25). Community forest management can be undermined or co-opted by powerful private and public sector actors (26–28). Finally, by improving communities' access to credit and extending their planning horizons, titling can raise the returns on agriculture relative to forests, thereby encouraging extensification (29, 30).

Hence, the net effect on forest clearing and degradation of granting title to communities is an empirical matter. However, as discussed in the next section, we know little about it. Although numerous studies have examined the effect of preexisting tenure type on forest cover change, rigorous studies of the effect of titling initiatives that change the tenure status of indigenous communities are rare. The latter studies are more relevant to policy decisions about future titling and are better positioned to disentangle tenure's effect from the effect of confounding factors.

We analyze the effect of granting title to indigenous communities on forest cover change in the Peruvian Amazon. One of

Significance

Developing countries are increasingly granting local communities legal title to forests. Almost a third of forests in the global south are now managed by local communities, more than twice the share currently found in protected areas. However, we know little about the effects of titling on forest clearing and disturbance, which remain urgent problems. We use community-level longitudinal data derived from high-resolution satellite images, along with statistical techniques that control for confounding factors, to measure the effect of titling indigenous communities in the Peruvian Amazon. Results indicate that titling significantly reduces both clearing and disturbance, at least in the short term. The implication is that awarding formal land titles to local communities can protect forests.

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the world's largest remaining contiguous primary forests, the Peruvian Amazon is increasingly threatened by forest clearing and degradation. Titling there has been extensive: More than 1,200 indigenous communities accounting for more than 11 million ha received title between 1975 and 2008 (31). We focus on the effect of titling from 2002 to 2005 on forest cover change from 2000 to 2005. *Supporting Information* provides additional background on our study area, including forest cover change and governance, indigenous communities, and the titling process. To identify the effect of titling on forest cover change, we use high-resolution remotely sensed data on both forest clearing and forest disturbance, along with statistical techniques that aim to control for confounding factors (32).

Evidence Base

More than 100 published papers explore the relationship between preexisting tenure security and/or tenure type on the one hand and forest cover on the other (20, 21, 33–35). Two recent meta-analyses conclude that, in general, preexisting tenure security is associated with lower rates of forest cover change regardless of the form of tenure (36, 37).

Although these studies are valuable, they do not directly address the policy questions with which we are concerned: Does changing land tenure affect forest cover? If so, how? Studies of policies and programs that have changed land tenure are likely to provide more informative answers to those questions. One reason is that they are better positioned to disentangle tenure's effects from the effects of confounding factors. As one of the meta-analyses cited above makes clear, preexisting tenure type is often correlated with unobservable confounding factors because the historical processes that assign tenure types typically are not random (37). For example, in many Latin American countries, over hundreds of years, powerful private landowners acquired the forestlands that would be most productive when converted to agriculture (e.g., because they were most accessible or had the most fertile soil), leaving other forest lands for peasant communities and the state. In such countries, tenure type is strongly correlated with confounding factors. Unfortunately, studies of the effect of preexisting tenure on forest cover generally do not try to control for these factors. The meta-analysis noted above examined 150 peer-reviewed publications and found only 36 studies that attempted to control for confounding factors and only two studies (other than of protected areas) that used quasi-experimental methods for that purpose (37). Controlling for confounding factors is more straightforward when tenure has changed relatively recently as a result of a titling initiative. In such cases, it is easier to observe, and design an empirical strategy to control for, the treatment assignment process.

To our knowledge, only two published studies use quasi-experimental methods to evaluate the effects of changes in land tenure on forest cover: the studies by Buntaine et al. (38) and Liscow (29). However, neither focuses on the effects of a national-level campaign aimed at indigenous communities. Buntaine et al. (38) use satellite data, along with matched difference-in-difference models, to measure the effect on forest loss of an initiative that titled indigenous communities in a single province of Ecuador (Morona-Santiago) during the early 2000s. They find no evidence that titling reduced forest loss in the 5 y after title was awarded. Liscow (29) exploits a natural experiment, the massive land reform associated with the Sandinista revolution, to identify the effects of changes in tenure security on forest cover in Nicaragua. Using landholder-level cross-sectional data, along with instrumental variables models, he finds that all other things being equal, properties with relatively secure title, including properties held by individuals, cooperatives, and indigenous communities, had less forest cover per hectare than properties without such title. He hypothesizes that this correlation reflects the effect of tenure security on credit access, agricultural productivity, and, ultimately, the return to deforestation. However, the study does not focus exclusively on indigenous communities. *SI Comparison with Previous Evaluations* discusses possible reasons for differences between our results and the results of Buntaine et al. (38) and Liscow (29).

In addition to studies of the effects of preexisting tenure on forest cover, two other strands of literature are relevant. One examines the effect of community forestry on a range of social and environmental outcomes (e.g., refs. 39–41). Meta-analyses of this literature suggest that generalizing about such effects on the basis of these studies is risky, given that some have methodological limitations and that idiosyncratic factors tend to be important (42, 43).

The other strand of related literature focuses on titling properties held by individuals (versus communities) in Peru. As discussed below in *SI Background*, Peruvian government campaigns aimed at titling indigenous communities fit into larger contemporaneous efforts to title all manner of rural and urban properties with insecure informal land rights. Although, to our knowledge, no studies have examined the effects of such titling on forest cover, several aim to identify socioeconomic effects. The findings are mixed. For example, Field (44) and Fort (45) find that in urban areas, titling led to an increase in labor supply outside the home, and that in rural areas, it increased on-farm investment. However, Zegarra et al. (46) find that rural titling had few positive effects.

Empirical Approach

To identify the effect of titling on forest cover change, we use indigenous community-level longitudinal data. The principal challenge we face is the usual one in program evaluation: The treatment, titling in our case, was not randomly assigned. As a result, it could be spatially and/or temporally correlated with observed and unobserved confounding factors that affect the outcome, forest cover change in our case. For example, in principle, titling could be correlated across space with proximity to rivers used for transporting logs, a time-invariant community characteristic that likely spurs forest cover change. In addition, in principle, titling could be correlated over time with changes in national timber prices, which, in turn, affect forest cover change. Unless we control for them, such confounding factors can bias our treatment effect estimates.

To that end, we rely on fixed effects, along with a set of control variables that vary both over time and across space. We estimate

$$Y_{nit} = \gamma_i + \delta_t + D'_{it\text{-}z}\beta_{n1} + X'_{it\text{-}z}\beta_{n2} + \varepsilon_{nit} \quad (n = 1,2,3), \qquad [1]$$

where n indexes the type of forest cover change (clearing, disturbance, or both), i indexes communities, t indexes years, z indexes temporal lags, Y is the percentage of the community's forest changed, γ are community-fixed effects, δ are year-fixed effects, D is a vector of contemporaneous and lagged dichotomous dummy variables indicating titling, X is a vector of time-varying control variables, β_1 and β_2 are vectors of parameters to be estimated, and ε is an error term. The parameters in β_1 measure titling's effect on forest cover change. The community-fixed effects control for observed and unobserved time-invariant community heterogeneity, and the year-fixed effects control for observed and unobserved location-invariant temporal effects. We omit the time-invariant control variables like travel time to population centers because they are perfectly correlated with the community-fixed effects. We estimate Eq. **1** using ordinary least squares and cluster SEs at the community level.

A potential concern is that our treatment effect estimates may be biased by unobserved time-varying confounding factors (i.e., factors correlated temporally and spatially with both titling and forest cover change) for which our fixed effects models would not control. However, we believe such factors are unlikely to drive our results for at least two reasons. First, as discussed in *SI Robustness Checks*, lagged dependent variable models that control for at least some time-varying confounding factors generate results that are quite similar to the results from our main models (Table S1).

Second, the location and timing of titling in our study area and period were largely determined by plausibly exogenous factors. Because titling is costly and complex, few if any indigenous communities have undertaken it independently. Rather, the vast majority of titles have been awarded via large-scale titling campaigns led by government agencies and nongovernmental organizations (NGOs). Because almost all communities are situated along rivers, which are the main transportation arteries in the region, each titling campaign started at one end of a watershed and swept toward the other. Much of the funding for these campaigns was obtained from external sources and was allocated piecemeal over time. As a result, multiple campaigns were conducted over the course of several decades. For a variety of reasons, including a border dispute with Ecuador and expanded oil exploration, many of the early titling campaigns were in the northern part of our study area.

We find no evidence in our data of a correlation between the location of titling campaigns and contemporaneous rates of forest cover change (i.e., nonrandom treatment assignment) that could, in principle, confound our efforts to identify the effect of titling on forest cover change. To address that concern, we created a region-level (regions are first-level administrative units in Peru) 2000–2005 panel dataset and then regressed the number of titles awarded to indigenous communities in each region onto the average rates of forest cover change in each region. We fit separate regressions for deforestation, disturbance, and deforestation or disturbance. In all of these regressions, we are unable to reject the null hypothesis that titling is not correlated with forest cover change. These results are robust to inclusion of region- and year-fixed effects and to the clustering of SEs at the region level (Table S2).

In addition to estimating average treatment effects (Eq. **1**), we use interaction effects to examine potential treatment effect heterogeneity across community type (i.e., to test whether community characteristics moderate the effect of titling on forest cover change). This analysis is limited by a lack of comprehensive baseline (pretreatment) data on community characteristics, and is therefore speculative. The goal is to generate hypotheses that can be tested in future research. For simplicity's sake, we add interaction effects to a model that uses the percentage of the community's forest either cleared or disturbed as a dependent variable and features a single cumulative 1-y lagged titling dummy variable. We interact this titling dummy variable with three time-invariant community characteristic variables: *area*, the size of the community in hectares; *distance to city*, the distance in kilometers to the nearest population center with more than 10,000 residents; and *Pucallpa*, a binary indicator of whether the community is in the region (Ucayali) hosting the city that is the hub of both the logging industry and regulatory monitoring and enforcement in the Peruvian Amazon (Table S3). The choice of these variables was dictated more by data availability than by theory. We estimate

$$Y_{it} = \gamma_i + \delta_t + D^{'}_{it\text{-}z}\beta_1 + D^{'}_{it\text{-}z}C_m\beta_2 + X^{'}_{it\text{-}z}\beta_3 + \varepsilon_{it}, \qquad [2]$$

where C is a vector of community characteristic variables and m is an index of these variables. As a robustness check, we also estimate three separate regressions, each with a single interaction term. To control for multiple hypothesis testing, we adjust probability values on the interaction term coefficients to maintain a constant family-wise type 1 error rate (47).

Data

We created the dataset used in the empirical analysis by compiling indigenous community-level longitudinal data on forest clearing and disturbance (our outcome variables), titling (our treatment variable), and climatological and agronomic drivers of forest cover change (our control variables) for a 750,000-ha study area (Fig. 1 and Table S4). Based on Landsat satellite images, our forest cover change data measure forest clearing and disturbance at a resolution of 30 m × 30 m each year from 2000 to 2005 (a detailed description of these data is provided in ref. 48). Our titling data are derived from records maintained by the Instituto de Bien Común (IBC), one of Peru's leading indigenous community NGOs (31, 49). [For each community, we derive the date title was awarded as reported by the IBC (31) and geolocator information as reported by the IBC-World Resources Institute (WRI) (49). An early version of this paper incorrectly used the date recorded by the IBC-WRI (49), which marks data entry or release, as the date title was awarded, and therefore generated qualitatively different results.] As for our control variables, we use 2000–2005 temperature and precipitation data derived from Moderate Resolution Imaging Spectroradiometer (MODIS) and Tropical Rainfall Measuring Mission (TRMM) satellite products and crop price data derived from district-level agricultural statistics. *SI Variables* details the variables used in our regression analysis. Our regression sample comprises communities in the IBC database that received title during the period spanned by our forest cover change data: specifically, all 51 communities that received title after 2001 (Table S5). A scarcity of pre-2000 satellite data on forest cover change, along with our fixed effects empirical design, limits our ability to include communities titled earlier.*

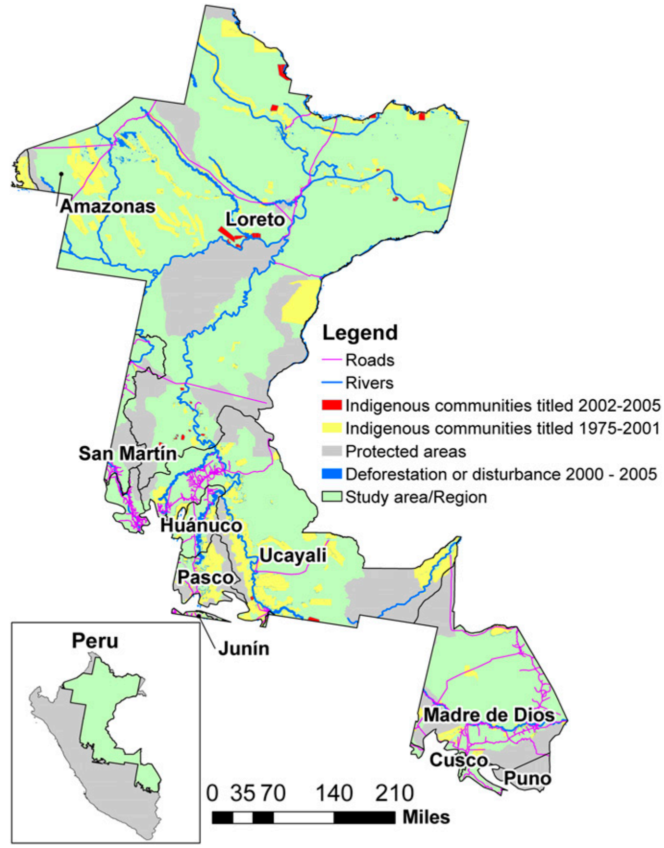

Fig. 1. Study area.

*Prior to 2000, Landsat data needed to generate fine-scale annual maps of forest loss and disturbance are sporadic. As a result, our forest cover change panel begins in 2000. Our fixed effects design requires that for every community in our regression sample, we have at least 1 y of pretitle data on forest cover change. That requirement (along with our inclusion of a 1-y lagged titling dummy variable) implies that we can estimate treatment effects only for communities titled after 2001.

Model Specification

Unlike most land cover change data, ours measure both forest clearing and disturbance and distinguish between them. To exploit that feature, we present results using three dependent variables: *cleared* (clearing in year *t*), *disturbed* (disturbance in year *t*), and *forest cover change* (either clearing or disturbance in year *t*).

We specify our fixed effects models to examine the effect of titling on forest cover change in a 2-y window spanning the year title is awarded and the year afterward. Our relatively short 6-y forest cover change panel limits our ability to estimate longer lived effects. Our main models include two single-year titling dummy variables (*title_0, title_1*) in one specification (A) and a 1-y cumulative lag variable (*title_1c*) in a second specification (B). Estimated coefficients for our treatment variables have straightforward interpretations. For specification A, the coefficient on *title_0* can be interpreted as the marginal effect of titling on the percentage of forest cover change in an average indigenous community in the year title is awarded, and the coefficient on *title_1* can be interpreted as the marginal effect in the first year afterward. For specification B, the coefficient on *title_1c* can be interpreted as the average marginal effect over our 2-y study window.

Results

Main Results. Estimates of Eq. **1** indicate that titling has a statistically and economically significant negative effect on forest cover change within our 2-y study window (Fig. 2 and Table S6). In model 1A, which uses *forest cover change* as the dependent variable and includes single-year titling dummy variables, *title_0* and *title_1* are negative and statistically significant at the 1% and 10% levels, respectively. The implication is that titling reduces forest cover change in the year title is awarded and in the following year. These effects are economically significant. Expressed as percentage reductions from the counterfactual average annual rate of forest cover change of 0.37 percentage points per year (the value of forest cover change predicted by our model when treatment variables are set equal to zero), they imply that titling reduces the percentage of the community deforested or disturbed by 81% in the year in which titling occurs and by 56% in the first year afterward. Although we are unable to reject the null hypothesis that coefficients on *title_0* and *title_1* are the same, these results hint that the effect of titling attenuates over time. Model 1B indicates that the average annual effect over both years is substantial: a 71% reduction.

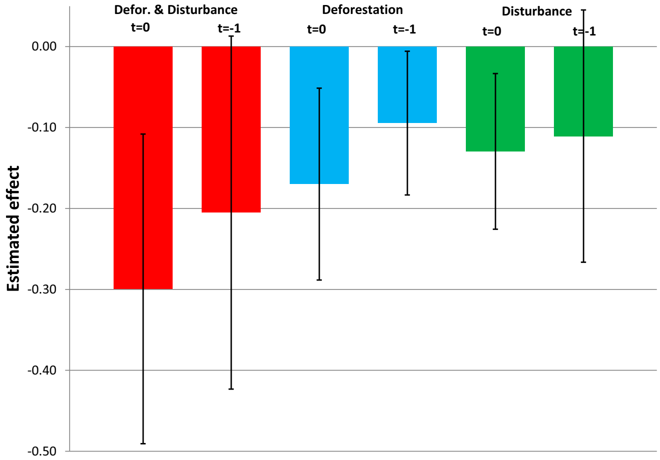

Fig. 2. Titling reduces deforestation and/or disturbance in the year of award and the year after; estimated coefficients on titling variables (models 1A, 2A, and 3A) and 95% confidence intervals.

In model 2A, which uses *clearing* as the dependent variable and includes single-year titling dummy variables, *title_0* and *title_1* are negative and statistically significant at the 1% and 5% levels, respectively. These results imply that titling reduces the percentage of the community deforested by 97% in the year in which titling occurs and by 54% in the first year afterward. In this case, we are able to reject at the 5% level the null hypothesis that coefficients on *title_0* and *title_1* are the same. Model 1B indicates that, on average, titling reduces deforestation by 80% per year during our 2-y study window.

In model 3A, which uses *disturbance* as the dependent variable and includes single-year titling dummy variables, *title_0* is negative and statistically significant at the 1% level and *title_1* is insignificant. Again, this result is economically significant. It implies that titling reduces the percentage of the community disturbed by 67% in the year title is awarded.

Robustness Checks. As detailed in *SI Robustness Checks*, to check the robustness of our results, we estimate models that include lagged dependent and lead treatment variables and that control for spillover. Overall, the results suggest that our results are, in fact, robust (Tables S1, S7, and S8).

Community Characteristics. Turning to our analysis of treatment effect heterogeneity across community type, a Wald test rejects at the 1% level the null hypothesis that coefficients on the three interaction terms included in Eq. **2** are jointly equal to zero. All three coefficients are statistically significant (Table S3, model 14). However, in specifications that include interaction terms one at a time, only *area* and *distance to city* are statistically significant, implying that *Pucallpa* only affects forest cover change conditional on the other two community-level characteristics (Table S3, models 15–17). The signs of *area* and *distance to city* indicate that the negative association between titling and forest cover change is more pronounced in communities that are smaller and communities that are closer to population centers. These effects are economically meaningful. The estimated coefficients in Table S3 (model 14) imply that a 100-ha reduction in the size of an indigenous community reduces forest cover change in the 2-y window spanning the year title is awarded and the year afterward by 1.2 percentage points, and that a 100-km reduction in the distance from the community to the nearest sizable population center reduces it by 0.4 percentage points. We discuss the implications of these findings in the next section.

Discussion and Hypotheses

To identify the effect of land titling on forest cover change in the Peruvian Amazon, we use fine-scale community-level longitudinal data on forest clearing and disturbance, along with fixed effects models that aim to control for confounding factors. We find that, on average, titling reduces forest clearing by more than three-quarters and forest disturbance by roughly two-thirds in a 2-y window spanning the year title is awarded and the year afterward. A preliminary analysis of treatment effect heterogeneity suggests that these effects may be more pronounced in communities that are smaller and closer to sizable population centers. As discussed in *SI Comparison with Previous Evaluations*, our results differ from the results of other published quasi-experimental studies that examine the effect on forest cover of changes in land tenure. These differences may be due to a variety of contextual and methodological factors.

Our analysis of the effect of titling on forest cover change in the Peruvian Amazon has several limitations. First, our 2000–2005 annual data on forest cover change span only 6 y of the 40-y period during which indigenous communities have been receiving land titling. A longer panel that covered the 1990s, when almost half of the titling in our study region occurred, would be preferable. Unfortunately, however, the Landsat data needed to generate annual maps of forest loss and disturbance are sporadic for the years before 2000. Second, without a valid instrument or discontinuity, we are not able to control fully for time-varying confounding factors. Third, we lack comprehensive baseline (pretreatment) data on community characteristics that would allow us to draw firm conclusions about treatment effect heterogeneity. Finally, our analysis does not identify the causal mechanisms that drive the negative correlation between titling and forest cover change. Pinpointing causal mechanisms is beyond the scope of our paper, and thus a focus of future research.

Toward that end, we conjecture about these mechanisms. We emphasize that this discussion, like our analysis of treatment effect heterogeneity, is speculative and aimed at generating hypotheses for future study. Although, as discussed below, the temporal pattern of our results provides some hints, we have no hard evidence to support our hypotheses. To underpin this discussion, we develop a theory of change (detailed in *SI Theory of Change* and Fig. S1) that reflects findings from the empirical and theoretical literature (summarized above); the historical and institutional context of titling indigenous communities in Peru (summarized in *SI Background*); and discussions with regulatory agencies, NGOs, and funders of titling of indigenous communities in Peru. It describes six possible mechanisms by which titling could reduce forest cover change in an indigenous community: (*i*) ratcheting up formal regulatory pressure applied by regulatory agencies and other state entities, (*ii*) strengthening informal regulatory pressure exerted by nonstate entities such as NGOs, (*iii*) improving the community's internal forest cover change governance, (*iv*) boosting the community's interactions with public sector entities such as government technical extension and educational programs, (*v*) augmenting the community's interactions with private sector entities such as creditors and input providers, and (*vi*) improving community livelihoods.

We hypothesize that two of those mechanisms, enhanced formal regulatory pressure and enhanced informal regulatory pressure, drove our findings. The main reasons concern the temporal pattern of our results. We find that titling has an effect instantaneously (i.e., in the year it occurs). In addition, as discussed above, our results suggest that this effect attenuates over time. Formal and informal regulatory pressure could have effects that fit this temporal pattern: They could be ratcheted up in the year title is awarded but could quickly dissipate afterward because of limited human and political resources. Three other potential causal mechanisms described in our theory of change seem less likely to have instantaneous, short-lived effects. Any effects that titling has by boosting the community's public sector interactions, augmenting its private sector interactions, or improving livelihoods would likely occur with a lag: It takes time for community members to enroll in public sector programs, contract with banks and input suppliers, and improve their livelihoods enough to affect forest cover change. Finally, enhancing internal community governance would likely have effects that persist beyond a single year. The hypothesis that titling reduces forest cover change soon after title is awarded by ratcheting up formal and informal regulatory pressure is consistent with our (admittedly speculative) findings that the communities where titling has a more pronounced effect tend to be smaller and closer to sizable population centers. It is plausible that in small communities close to centers of regulatory activity, the costs of monitoring by regulatory agencies, NGOs, and other stakeholders are relatively low.

Here, we have shown that titling indigenous communities in the lowland Peruvian Amazon basin reduces forest clearing and disturbance soon after title is awarded, and we have hypothesized that it does so by ratcheting up formal and informal regulatory pressure. This study is among the first spatially explicit analyses of its kind, and the findings strongly support the notion that awarding land title to indigenous and/or local communities can, at least in the short term, help protect forests. The cascading effects include biodiversity protection, carbon sequestration, water resource provisioning, and a host of other ecosystem services considered vital at local to global ecological scales.

Future research can build on this study in at least two ways. Field research combining qualitative and quantitative survey-based methods is needed both to identify the causal mechanisms driving the effects of titling on forest cover change that we have described and to understand better how these effects are moderated by community characteristics. In addition, remote sensing studies like ours can be applied to other countries, and over longer time periods, to monitor and quantify the effects of community land titles on forest governance and conservation.

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Supporting Information

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SI Background

Peruvian Amazon. The 66 million-ha Peruvian Amazon accounts for 60% of Peru's land mass, 90% of its forests, and 15% of the entire Amazon (50) (Fig. 1). Consisting of large tracts of contiguous primary forest, it provides a wealth of global and local ecological services, including biodiversity conservation and carbon storage (51). Forest clearing and disturbance are serious concerns. Using the same high-resolution 2000–2005 forest cover data on which our analysis relies and the same 750,000-ha study area, Oliveira et al. (48) estimated an average of 64,500 ha of deforestation and 63,200 ha of disturbance per year, mostly concentrated in the Ucayali and Madre de Dios regions. Major drivers of this forest damage include illegal logging, gold mining, commercial oil palm cultivation, and agricultural expansion, particularly by migrants from the western highland regions (50, 52–54).

Indigenous Communities. The Peruvian Amazon is home to at least 1,200 indigenous communities, defined as collections of families linked by indigenous language or culture and common use of a single territory (31, 55). Collectively, these communities inhabit more than 11 million ha and count 330,000 people (56). The average indigenous community in our regression sample (defined below) spans 6,268 ha and supports 26 families comprising 131 people, although some communities are much larger in terms of both area and population (Table S4). Almost half have a bilingual school (i.e., instruction in Spanish as well as the indigenous language), and a fifth have a health clinic. The communities specialize in a wide variety of economic goods, with the most common being fish (18% of communities), corn (12%), rice (8%), and wood (8%). Socioeconomic indicators for indigenous communities are well below national averages (56, 57).

The Peruvian government began awarding land title to indigenous communities in the mid-1970s and has continued doing so intermittently until the present day. Although the government's goals for titling have no doubt evolved over this 40-y period, tenure security and economic development have been persistent themes. A major institutional driver was pervasive tenure insecurity in both urban and rural areas created by massive land reforms that began in the late 1960s. These reforms entailed expropriating large estates; redistributing them to land-poor families, mostly as communal holdings; and restricting land sales. Although most indigenous communities were not directly affected by these reforms, the government's subsequent community titling campaigns were part of its broader effort to formalize and regularize land rights.

Three laws have guided titling of indigenous communities in Peru. The first was the 1974 Law of the Agricultural Development of Indigenous Communities of the Rainforest and Rainforest Border (D.L. 20653), which established a legal basis for granting indigenous communities rights to land. Its stated objective was to "establish an agrarian structure that contributes to the development of the [region], so that its population reaches living levels compatible with human dignity [authors' translation]." It recognized a broad range of criteria for delimiting community territory, including use of land for hunting, gathering, and fishing, and committed the state to guaranteeing the integrity of indigenous community tenure, generating a land cadaster, and granting communities land title (Article 9).

The 1978 Law of Indigenous Communities (D.L. 22175) set out detailed procedures for granting communities legal title. It organized these processes into two broad stages: recognizing the community as a legal entity and awarding it title. Each stage involves numerous legal, bureaucratic, and technical steps, and each is complex, costly, and lengthy. For example, the titling stage involves three sets of steps: (*i*) a desk phase that entails compiling technical and legal documents, forming a working group of representatives of the responsible entities and agencies, and formally notifying local stakeholders of the process; (*ii*) a fieldwork phase that entails face-to-face meetings with the community and local stakeholders, demarcating the community's territory with stone markers, and classifying parcels of land within the territory as suitable for agriculture, forestry, and forest protection; and (*iii*) a processing phase that involves preparing maps and field reports, obtaining approval of these documents by a general assembly of the community, and issuing formal reports by the Regional Agrarian Agency [known by its initials in Spanish (DRA)] of the Agriculture Ministry and other agencies.

A third legal pillar of community titling in Peru has been the 1989 International Labor Organization's (ILO's) Convention 169 on Indigenous and Tribal Peoples in Independent Countries, which Peru ratified in 1994. As an international human rights treaty, this agreement has constitutional standing in Peru. Among other things, it states that indigenous peoples have a special relationship with the lands they occupy and use; that the state must recognize their ownership of these lands; that they have rights to participate in the use, management, and conservation of natural resources on their lands; and that they must be consulted about the use of state resources on their lands. During the 1990s, as energy exploration in the Amazon region expanded, the Peruvian government was under considerable pressure to grant indigenous communities formal title to their lands so as to comply with the ILO Convention 169.

Although titling generally enhances Peruvian indigenous communities' tenure security, that security is by no means absolute (54, 58). Article 89 of Peru's 1993 constitution recognizes indigenous lands as imprescriptible but not untransferable and inalienable. Furthermore, by law, only part of indigenous community land is owned by the community itself. As noted above, in the process of titling, community land is categorized as land suitable for agriculture and ranching and land suitable for forestry. The community owns only the former outright. The state owns the latter but cedes rights to the community.

Given the cost and complexity of the legal procedures for obtaining land title, most communities are not able to secure titles on their own (54). As a result, outside organizations have provided technical and financial assistance, including NGOs such as the Interethnic Association for the Development of the Peruvian Rainforest and the IBC, government agencies such as the Special Project for Land Titling and (later) the Organization for the Formalization of Informal Property, and multilateral and bilateral international cooperation agencies such as the Inter-American Development Bank and the US Agency for International Development.

A Peruvian indigenous community first received title in 1975. By the end of 2008, 1,223 communities, 752 of which fall either completely or partly within our study area, had been granted land title (31, 49) (Fig. 1). As discussed in the main text, in general, the order in which indigenous communities in our study area obtained title was dictated by events outside their control.

To our knowledge, the drivers of forest cover change in indigenous communities in the entire Peruvian Amazon have not been rigorously assessed. However, case studies and other evidence point to a variety of factors (54). One is shifting cultivation, a leading mode of small-scale agriculture in the Peruvian Amazon, where soils tend to be shallow, acidic, and quickly depleted (54, 59). Practitioners may rotate production among a limited number of cleared and regenerating fallow plots so that over the long run, the total area of forest cover change is limited (60, 61). A second important driver of forest cover change in indigenous communities is illegal logging conducted by small- and medium-scale independent contractors (52–54, 62). Such contractors have incentives to harvest timber in indigenous communities. Compared with forest concessions, indigenous communities tend to be significantly closer to rivers used to transport timber, to have more valuable standing timber (because many forest concessions have been high-graded by selective logging over the past 50 y), and to have less economic and political leverage in contract negotiations (62–64). Because most logging in the Peruvian Amazon is selective, its direct effects on deforestation are mostly limited to associated roads, skid trails, landings, and tree-fall gaps. However, it can have very significant indirect effects by lowering the costs of transportation and subsequent land-use change (54). Finally, commercial agriculture, gold mining, and oil and gas exploration are increasingly important drivers of forest cover change in indigenous communities (54).

Forest Governance. The reforms ushered in by the 2000 Forest and Wildlife Law (D.L. 27308) defined Peruvian forest governance during the period spanned by our forest cover change data. The law aimed to promote sustainable timber extraction by designating new and existing institutions as forest management units, requiring these institutions (and the loggers with whom they contract) to obtain permits and authorizations defining the scope and nature of their extractive activities, and setting up a system for enforcing compliance with these permits and authorizations.

Specifically, the law focuses on two types of forest management units: new, large (5,000–40,000 ha), long-term (up to 40 y) forest concessions awarded through a public tender process and smaller existing institutions with title, including indigenous communities, small private landholders, and (nonindigenous) river communities. (In addition to these two sets of institutions, the law provided a mechanism for one-time extraction by logging companies in special circumstances.) Both types of units are required to draw up and obtain approval for a general forest management plan, which identifies the total quantity and type of trees to be harvested over a 5-y period, and more specific annual operating plans identifying the exact location, size, and type of individual trees to be extracted each year. In addition, those entities actually removing the timber, whether the units themselves or independent logging companies, must have permits for extraction that comport with the annual operating plan, as well as permits for transporting timber to market (53, 62).

Two institutions are responsible for monitoring and enforcing these mandates: the General Directorate of Forestry and Wildlife, which has 29 local offices called Technical Administrations for Forestry and Wildlife Control, and the semi-independent Organization for the Supervision of Forest Resources and Wildlife. Because the Peruvian Amazon is expansive, transportation is difficult, and regulatory resources are scarce, these institutions generally do not monitor timber extraction sites. Rather, they mainly inspect loads of timber being transported by river barge to Pucallpa, the region's main processing and export center, to ensure they are accompanied by requisite permits. These inspections are conducted both in Pucallpa and at checkpoints along rivers (53, 62).

SI Variables

We use three dependent variables: *forest cover change* is the percentage of the total area of the indigenous community either deforested or degraded each year from 2000 to 2005, *cleared* is the percentage deforested each year, and *disturbed* is the percentage degraded each year. On average, roughly one-third of 1% was either deforested or disturbed each year between 2000 and 2005, one-seventh of 1% was deforested, and one-sixth of 1% was disturbed (Table S4).

Our treatment variables are lagged dichotomous titling dummies: *title_n* are single-year lags indicating whether the community was titled *n* years before the current year, and *title_nc* is a cumulative lag indicating whether a plot was titled zero to *n* years before the current year. For reasons discussed in the main text, our main models use the two single-year lags (*title_0*, *title_1*) in one specification and a 1-y cumulative lag (*title_1c*) in a second specification. On average, in each year between 2002 and 2005, 14% of the communities in our sample obtained title and about a quarter obtained title in the current or previous years.

Among the control variables, *rainfall* is the total annual precipitation in each year from 2000 to 2005, *temperature* is the average annual temperature in each year, and *crop price index* is a district-level Paasche index of the prices of the 10 agricultural crops that account for the most area planted in our study area.

Finally, as discussed in the main text, we use several indigenous community-level variables to analyze treatment effect heterogeneity across community type (Table S4). These variables include *area*, the size of the community in thousands of hectares; *distance to city*, the distance in kilometers from the community to the nearest population center with at least 10,000 residents; and *Pucallpa*, a binary dummy variable that indicates whether an indigenous community is in the Ucayali region, which hosts the city that is the hub of both the logging industry and regulatory monitoring and enforcement in the Peruvian Amazon. The choice of variables used for our analysis of heterogeneous treatment effects was dictated by data availability. Only a limited number of characteristics are observed for the communities in our data.

SI Specification Tests

We conduct a series of tests to determine whether fixed effects, pooled ordinary least squares (OLS), or random effects models are called for. They confirm that a fixed effects model is indeed appropriate. We use Breusch and Pagan's (65) Lagrange multiplier test to check whether a pooled OLS model will generate unbiased treatment effects estimates: formally, a test of whether the variance of α_i, (the intercept component of a composite error term $\alpha_i + \varepsilon_{it}$ derived from Eq. **1**) is zero, which is a necessary condition for OLS to be consistent. The test rejects the null hypothesis of zero variance at the 1% level, implying that an individual effects model, either random effects or fixed effects, and not OLS, is appropriate. To inform our choice of whether to use a random effects model or fixed effects model, we use a Hausman (66) test of whether coefficients from the two models are systematically different. We reject the null of no systematic differences at the 1% level. Finally, we use a Chow (67) test to determine whether year-fixed effects, in addition to community-fixed effects, are needed: formally, a test of whether year-fixed effects are jointly equal to zero. We reject the null hypothesis at the 1% level. For simplicity, these tests use specification B.

SI Robustness Checks

This section presents results from regressions aimed at checking the robustness of our results: specifically, regressions that include lagged dependent and lead treatment variables and that control for spillover.

Lagged Dependent Variables. A limitation of the fixed effects models used to generate our main results is that they do not control for time-varying confounding factors. A common concern is that such factors include lagged outcomes and unobserved variables correlated with them. The best-known example comes from analyses of the effect of job training programs on unemployment. Estimates of these effects are biased when they fail to account for "preprogram dips," spells of unemployment that cause workers to enroll in training programs (68). In our case, the issue is that past forest cover change could, in principle, be correlated with both our treatment (titling) and our outcome (current forest cover change). For example, it could be that indigenous communities experiencing relatively high rates of forest cover change due to encroachment by outsiders may disproportionately seek land title as a means of stemming that encroachment. Such communities may be likely to take measures to reduce encroachment whether or not they are awarded title. If so, our fixed effects model would conflate this self-selection effect with the effect of titling.

To control for such problems, we estimate a dynamic model that includes lagged forest cover change as a dependent variable (69). We omit community-fixed effects because the assumptions needed for a dynamic model with fixed effects to be consistent are quite strong (70). Hence, we estimate

$$Y_{nit} = Y_{nit\text{-}h} + \delta_t + D'_{it\text{-}z}\beta_{n1} + X'_{it\text{-}z}\beta_{n2} + \varepsilon_{nit} \quad (n = 1,2,3). \qquad \textbf{[S1]}$$

Of course, omitting fixed effects would bias our results if unobserved time-invariant confounders are important.* In principle, we could include multiple lagged dependent variables (e.g., $Y_{nit\text{-}1}$, $Y_{nit\text{-}2}$, $Y_{nit\text{-}3}$) in the dynamic model. However, each such variable requires dropping the first full year (i.e., one-sixth) of our community-year observations. [The same issue prevents us from using estimators that use one lagged dependent variable as an instrument for a second such variable (e.g., ref. 72).] Therefore, to preserve degrees of freedom and facilitate an apples-to-apples comparison with our fixed effects model results, we use a single 1-y lagged dependent variable. Even though sample sizes are significantly smaller, results from models with two and three lagged dependent variables are similar.

Results from the lagged dependent variable model are similar to results from the fixed effects model (Table S1). They indicate that titling reduces forest clearing and disturbance in the year title is awarded and, in some cases, in the year afterward. The magnitudes of these effects are comparable to the magnitudes derived from the fixed effects model. The main difference is that in the dynamic model, the 1-y lagged effect of titling (*title_1*) is consistently smaller in magnitude and is statistically insignificant in the models of deforestation and disturbance (models 4A and 6A).

Anticipatory Effects. Related to the concern that the results from our fixed effects model may reflect self-selection on the basis of unobserved confounding variables is the worry that they may reflect communities' anticipatory behavior. For example, it could be that knowing the award of title makes communities legally culpable for forest cover change not sanctioned by regulators, communities harvest timber or clear forest in advance of the actual award of title. Alternatively, if titling somehow improves forest governance, that improvement may begin to happen in advance of the formal award of title. In these cases, our fixed effects model would either overestimate or underestimate the negative effect of titling on forest cover change.

To address that concern, we estimate a model that includes two lead treatment effects. We drop eight communities titled in 2004 and 2005 so that outcomes for the 2 y preceding title are observed for every community in the regression sample. The results suggest that anticipatory effects are negligible (Table S7). Both lead titling dummy variables are statistically insignificant in all six models estimated.

Note that the fixed effects regressions that include lead treatment variables can be interpreted as placebo tests (i.e., tests of whether "fake" treatments that occur before actual treatments have an effect on the outcome). Our inability to reject the null hypothesis that these hypothetical treatments have no effect provides reassurance that results for actual treatments are robust.

Spillover. Our estimates of the effect of titling on forest cover change within indigenous communities could be problematic if titling causes forest cover change in neighboring indigenous communities. Such spillover could happen for a variety of reasons. For example, titling a given community could cause independent logging companies to shy away from that community and instead target not-yet-titled neighboring communities. That scenario, in turn, could cause a pretitling spike in forest cover change in these communities, which could bias our treatment effects estimates downward (larger negative effect). Alternatively, titling a given community could cause independent logging companies to shy away from that community and instead target less recently titled neighboring communities. That scenario, in turn, could cause a posttitling spike in forest cover change, which would bias our treatment effect estimates upward (smaller negative effect).

To check the robustness of our results to such spillover, we estimate our main fixed effects models using a regression sample that omits at least one of any two neighboring communities, which we define as communities with borders less than 10 km apart. In the resulting subsample, we expect spillover to be significantly reduced. A limitation of this approach is that dropping neighbors reduces our sample size to just 168 (28 communities × 6 y = 168) community-year observations.

Not surprisingly, given this limited sample, significance levels for treatment effect estimates are lower (Table S8). The treatment dummy variable *title_0* is significant at the 5% level in the model of forest cover change (model 10A) and at the 10% level in models of deforestation (model 11A) and disturbance (model 12A). The variable *title_1* is not significant in any models, and *title_1c* is significant at the 10% level in models of forest cover change (model 10B), deforestation (model 11B), and disturbance (model 12B). That said, however, the signs and magnitudes of our treatment effect estimates are comparable to the signs and magnitudes from our main models (models 1–3). These results provide some reassurance that spillover does not drive our broad finding that titling reduces forest cover change in our 2-y study window.

SI Comparison with Previous Evaluations

As noted in the main text section, *Evidence Base*, to our knowledge, only two published studies use quasi-experimental methods to examine the effect of a change in tenure on forest cover: the studies by Buntaine et al. (38) and Liscow (29). The broad findings of each study differ markedly from ours. Buntaine et al. (38) conclude that titling indigenous communities in Morona-Santiago, Ecuador, had no discernible effect on forest cover change, whereas Liscow (29) concludes that secure title was associated with less forest cover in northwestern Nicaragua. A variety of contextual and methodological factors may explain these differences.

The institutional context of Buntaine et al.'s study (38) is closer to ours. Like us, they examine the effect on forest cover of policy interventions that titled indigenous communities in Amazonia. However, we obviously analyze different indigenous groups in a different country. In addition, we use a different empirical strategy. We use community-level panel data, along with fixed effects models. As a result, we identify the effect of titling using "within" variation (i.e., variation over time) for our study communities.

*However, estimates from lagged dependent variable dynamic models that omit fixed effects (Eq. **S1**) and fixed effects models that omit lagged dependent variables (Eq. **1**) can be thought of as bounding the "true" causal effect, given certain assumptions about the nature of the selection bias: Lagged dependent variable models will have an upward bias when time-invariant confounders are important, and fixed effects models will have a downward bias when time-varying confounders are important (69, 71). Hence, taken together, our fixed effects results and our lagged dependent variable model results shed light on the robustness of our results to a range of unobserved confounders.

By contrast, Buntaine et al. (38) use cell-level cross-sectional data, along with matched difference-in-difference models. As a result, they identify the effect of titling using "between" variation (i.e., the variation across space) in before and after changes for cells in titled communities and similar cells outside these communities.

The institutional context of Liscow's study (29) is quite different from ours. Whereas we examine the effect of an intervention aimed at enhancing tenure security for indigenous communities in the Amazon tropical rainforest, Liscow (29) examines the effect of a set of interventions (Sandinista land reforms) that had the effect of undermining tenure security for a wide range of landholders, most of whom did not belong to indigenous communities, in a part of Central America that is geographically diverse. In addition, we use a different empirical strategy. As just noted, we use community-level panel data, along with fixed effects models, and therefore identify the effect of titling using within variation over time. By contrast, Liscow (29) uses landholder-level cross-sectional data, along with instrumental variable methods.

SI Theory of Change

This section presents a theory of change that describes hypothesized causal pathways between the intervention in question, titling of indigenous communities, and the outcome, forest cover change. It reflects findings from the empirical and theoretical literature (summarized in the main text); the historical and institutional context of titling indigenous communities in Peru (summarized above); and discussions with regulatory agencies, NGOs, and funders of titling of indigenous communities in Peru.

Fig. S1 summarizes the theory of change. It represents (*i*) three inputs into the intervention, (*ii*) the intervention itself, (*iii*) six intermediate outcomes, (*iv*) final impacts, and (*v*) assumptions that inform these links. Below, we discuss each of these components. To save space, however, we do not discuss all of the assumptions. Although, in principle, titling indigenous communities could either reduce or exacerbate forest cover change, given our empirical findings, we frame the theory of change as a hypothesis that titling reduces these losses.

i) Inputs. Awarding land title to indigenous communities in Peru entails three main inputs. First, the community must convene internal meetings to decide whether and how to participate and be represented in the titling process. Second, the community must meet with external stakeholders, including representatives of government titling agencies and NGOs that assist in the titling process. Finally, the boundaries of the community must be physically and digitally demarcated.

ii) Intervention. The actual award of land title is marked by bureaucratic actions, including the production and registration of legal documents.

iii) Intermediate outcomes. In principle, titling can have six intermediate outcomes that, in turn, can reduce forest cover change. First, titling can enhance formal regulatory pressure on agents engaging in illegal deforestation. As discussed in *SI Background*, although written regulations prohibit harvesting timber and clearing forest without permits, regulatory monitoring and enforcement are weak. Illegal logging and clearing are particularly common in areas where land tenure is ill-defined. One reason is that in such areas, regulators cannot easily assign culpability for illegal activities in a given forest to the managers of that forest. A second reason is that managers of forests without legal title do not have the standing to register formal complaints with regulators about illegal encroachments on their lands. In principle, titling of indigenous communities can address both problems. That said, this hypothesized intermediate outcome depends on the assumption that ill-defined land tenure, and not other factors like a lack of regulatory capacity or political will, is a binding constraint on stronger formal regulatory pressure.

Second, titling indigenous communities can enhance informal regulatory pressure exerted by nonstate entities, such as NGOs and the press, on agents engaging in illegal forest cover change. Titling could enhance informal regulatory pressure in much the same way as it enhances formal regulatory pressure: by allowing NGOs and other stakeholders to assign responsibility for illegal forest cover change on community land to community leaders and by giving community representatives standing to complain to NGOs, the press, and other nonstate stakeholders about illegal encroachment. That said, this hypothesized intermediate outcome depends on the assumption that ill-defined land tenure, and not other factors like the absence of capable environmental NGOs, is a critical barrier to stronger informal regulatory pressure.

Third, titling can enhance community governance, including governance of forest resources. It can have that effect as a result of all three inputs described above: Territorial demarcation can focus scarce community governance resources on a particular forest area, meetings of communities' members can help build the internal community dialogue and consensus needed for more effective forest management, and meetings between community representatives and external stakeholders can help build community governance capacity. For example, the titling process entails meetings with NGOs specifically aimed at teaching community representatives how to interact with government and regulatory agencies. These hypothesized causal links depend on assumptions that titling helps focus community management resources, improves internal community communication and consensus, and builds governance capacity, and that all of these changes improve the management of forest resources.

Fourth, titling indigenous communities can boost their interactions with public sector entities other than regulatory agencies, which, in turn, can reduce forest cover change. For example, titling can make communities eligible to participate in government programs that provide technical assistance to forest managers. This hypothesized intermediate outcome depends on the assumption that lack of legal title to forests, and not other factors like lack of government resources or political biases, is the binding constraint on the provision of such assistance to communities.

Fifth, titling indigenous communities can facilitate their interactions with private sector entities such as creditors and input providers, which, in turn, can reduce forest cover change. For example, tilting can make it easier for communities to obtain credit for investments in intensive agriculture or improved silviculture. This hypothesized intermediate outcome depends on the assumption that ill-defined land tenure, and not other factors like thin credit markets, is the binding constraint on enhanced interactions between communities and private sector entities.

Finally, titling can enhance indigenous communities' livelihoods, which, in turn, can reduce forest cover change. Titling can boost livelihoods via three of the intermediate outcomes discussed above: internal governance, public sector interactions, and private sector interactions. Each can have both a direct effect on forest cover change and an indirect effect via livelihoods. For example, titling can enhance internal governance, which can directly reduce forest cover change. Titling can also cut forest cover change indirectly by boosting communities' livelihoods (e.g., by more efficiently allocating public investment resources), which, in turn, can cut forest cover change (e.g., by reducing communities' reliance on shifting agriculture and timber).

iv) Final impacts. The six intermediate outcomes discussed above can, in principle, stem forest cover change.

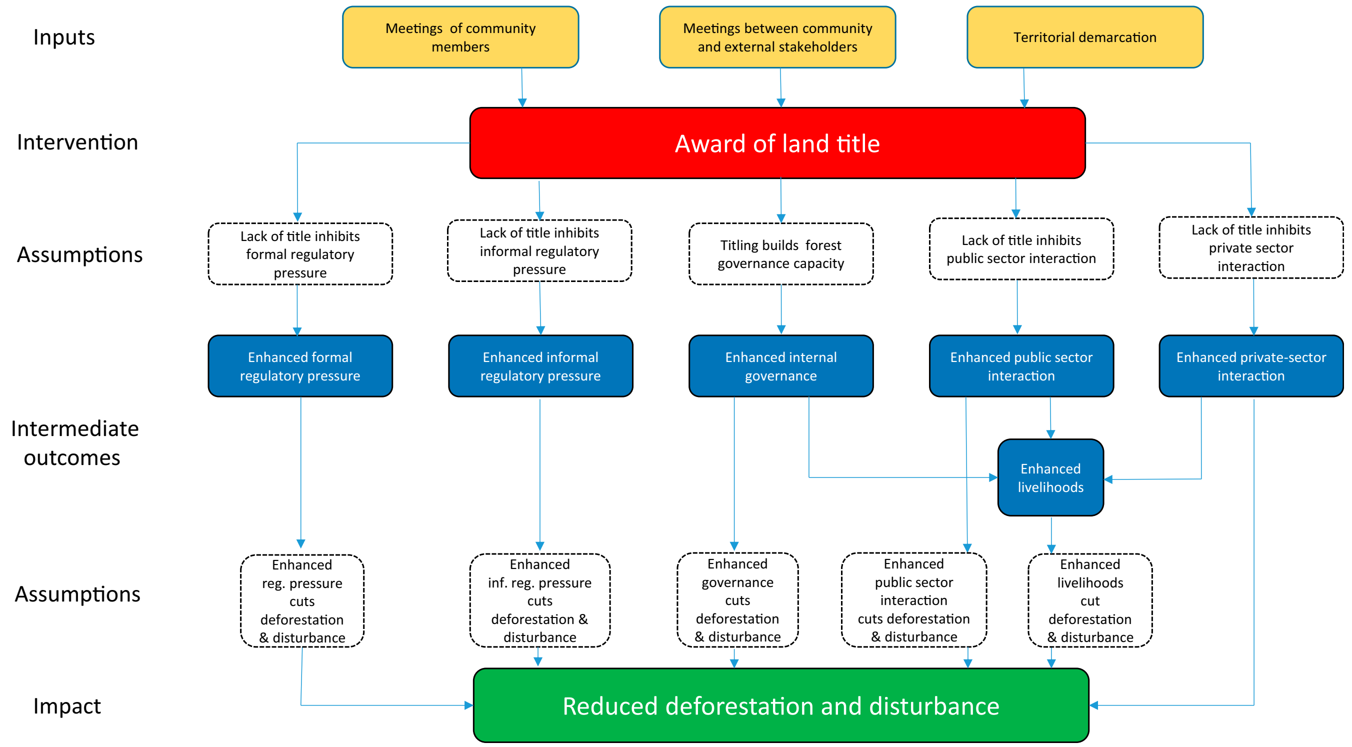

Fig. S1. Theory of change. inf., informal; reg., regulatory.

Table S1. Robustness check: Pooled OLS regression with 1-y lagged dependent variable

Treatment variable	Model					
	Deforestation + disturbance		Deforestation		Disturbance	
Outcome	4A	4B	5A	5B	6A	6B
title_0	−0.2752*** (0.0871)		−0.1385*** (0.0490)		−0.1221*** (0.0431)	
title_1	−0.1377 (0.1168)		−0.0605* (0.0338)		−0.0853 (0.0939)	
title_1c		−0.2180*** (0.0843)		−0.1059*** (0.0383)		−0.1068* (0.0556)
Lagged dependent var.?	Yes	Yes	Yes	Yes	Yes	Yes
Com.-fixed effects?	No	No	No	No	No	No
Year-fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Control variables?	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SEs?	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.1289	0.1283	0.0667	0.0660	0.1325	0.1326
$P > \chi^2$	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
Test statistic†	0.2092	n/a	0.0503	n/a	0.6526	n/a

Dependent variable is percentage of community deforested or disturbed in year $t = 2002–2005$; treatment is award of formal title (SE) ($n = 51$ communities × 5 y = 255 community-years). Com., Community; n/a, not applicable.
†Probability value from χ^2 test of null hypothesis that coefficients on *title_0* and *title_1* are equal.
***Significant at 1% level; *significant at 10% level.

Table S2. Treatment assignment process: Region-year-level pooled OLS and fixed effects regressions

Treatment variable	Model					
	Deforestation + disturbance		Deforestation		Disturbance	
Outcome	17	18	19	20	21	22
Titles	−0.4145 (0.9947)	0.3738 (0.3916)	−0.6091 (1.6096)	0.3473 (0.4025)	−1.1369 (2.4874)	1.4983 (1.5012)
Region-fixed effects?	No	Yes	No	Yes	No	Yes
Year-fixed effects?	No	Yes	No	Yes	No	Yes
Clustered SEs?	No	Yes	No	Yes	No	Yes
Within R^2	0.0001	0.1316	0.0000	0.1303	0.0001	0.1344
$P > \chi^2$	0.6769	n/a	0.7051	n/a	0.6476	n/a
$P > F$	n/a	0.7081	n/a	0.6590	n/a	0.8198

Dependent variable, Titles, is number of indigenous communities awarded in year t = 2002–2005; independent variable is percentage of region deforested or disturbed (SE) (n = 10 regions × 4 y = 40 region-years).

Table S3. Treatment effect heterogeneity: Fixed effects regressions

	Model			
Treatment variable	14	15	16	17
title_1c	−0.7477*** (0.2284)	−0.4419*** (0.1342)	−0.7580*** (0.2296)	−0.2599*** (0.0932)
*title_1c * area*†	0.0118* (0.0054)	0.0229** (0.0086)		
*title_1c * distance to city*	0.0035** (0.0015)		0.0042*** (0.0013)	
*title_1c * Pucallpa*	−0.3874*** (0.0879)			−0.0393 (0.2605)
Counterfactual‡	0.4070*** (0.0343)	0.3843*** (0.0276)	0.4069*** (0.0347)	0.3708*** (0.0243)
Com.-fixed effects?	Yes	Yes	Yes	Yes
Year-fixed effects?	Yes	Yes	Yes	Yes
Control variables?	Yes	Yes	Yes	Yes
Clustered SEs?	Yes	Yes	Yes	Yes
Within R^2	0.1869	0.1699	0.1837	0.1551
$P > F$	0.0000	0.0035	0.0042	0.0102

Dependent variable is forest clearing and disturbance in year t = 2000–2005; treatment is award of formal title (SE) (n = 51 communities × 6 y = 306 community-years). To control for multiple hypothesis testing, P values are adjusted to maintain a constant family-wise type 1 error rate [Sankoh et al. (47)].

†1,000 ha.

‡Predicted outcome with all treatment variables set equal to zero; computed using delta-method.

***Significant at 1% level; **significant at 5% level; *significant at 10% level.

Table S4. Variables

Variable	Description	Units	Source	Scale	Years	Mean*	SD*
Outcome							
Forest cover change	Forest cleared or disturbed in year t	%	CIS	30 m	2000–2005	0.30	0.80
Cleared	Forest cleared in year t	%	CIS	30 m	2000–2005	0.14	0.41
Disturbed	Forest disturbed in year t	%	CIS	30 m	2000–2005	0.16	0.48
Treatment							
title_n	Titled in year $t - n$	0/1	IBC-WRI	1:1,000,000	2002–2008	n/a	n/a
title_nc	Titled year $t - n$ through $t - n$	0/1	IBC-WRI	1:1,000,000	2002–2008	n/a	n/a
Control							
Rainfall	Total annual rainfall in year t	mm	NASA	25 km	2000–2005	2,698.60	637.18
Temperature	Average annual temperature in year t	K/ρ†	NASA/USGS	1 km	2000–2005	14,921.03	30.26
Crop price index	Crop price index in year t	PEN	SIEA	District	2000–2005	3,199.98	3,330.34
Interaction‡							
Area	Area	ha	IBC-WRI	1:1,000,000	2002–2008	6,268.24	8,174.92
Distance to city	Distance city >10,000 people	km	INEI-MTC	n/a	2007	101.70	73.53
Pucallpa	In Ucayali region	0/1	IBC-WRI	1:1,000,000	2002–2008	0.06	0.24

CIS, Carnegie Institution for Science; IBC/SICNA, Instituto del Bien Común, Sistema de Información sobre Comunidades Nativas de la Amazonia Peruana; INEI/MTC, Instituto Nacional de Estadística e Informática, Ministerio de Transportes y Comunicaciones; NASA, National Aeronautics and Space Administration; PEN, Peruvian Sol; SIEA, Sistema Integrado de Estadisticas Agrarias; USGS, US Geological Survey. The NASA rainfall data are available at https://pmm.nasa.gov/data-access/downloads/trmm and are described by Huffman et al. (73). The NASA/USGS temperature data are available at https://lpdaac.usgs.gov/dataset_discovery/modis/modis_products_table/mod11a2_v006 and are described by NASA (74).

*n = 306 community-years (51 communities × 6 y).

†ρ = 50.

‡Time-invariant.

Table S5. Titles awarded to indigenous communities in study area, by year, 2002–2008

Year	Titles per year	Total titles
2002	5	5
2003	31	36
2004	2	38
2005	6	44
2006	6	50
2007	1	51
2008	0	51

Source: IBC (31).

Table S6. Main results: Fixed effects regressions

Treatment variable	Model					
	Deforestation + disturbance		Deforestation		Disturbance	
Outcome	1A	1B	2A	2B	3A	3B
title_0	−0.2994*** (0.0953)		−0.1699*** (0.0590)		−0.1295*** (0.0479)	
title_1	−0.2051* (0.1086)		−0.0945** (0.0442)		−0.1106 (0.0776)	
title_1c		−0.2621*** (0.0904)		−0.1401*** (0.0504)		−0.1220** (0.0521)
Counterfactual[†]	0.3692*** (0.0245)	0.3709*** (0.0242)	0.1745*** (0.0132)	0.1759*** (0.0135)	0.1946*** (0.0145)	0.1950*** (0.0140)
Com.-fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Year-fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Control variables?	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SEs?	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.1559	0.1551	0.1295	0.1276	0.1417	0.1416
$P > F$	0.0094	0.0070	0.0294	0.0205	0.0080	0.0053
Test statistic[‡]	0.2926	n/a	0.0458	n/a	0.7756	n/a

Dependent variable is the percentage of community deforested or disturbed in year t = 2000–2005; treatment is award of formal title (SE) (n = 51 communities × 6 y = 306 community-years).

[†]Predicted outcome with all treatment variables set equal to zero; computed using delta-method.

[‡]P value from F-test of null hypothesis that coefficients on *title_0* and *title_1* are equal.

***Significant at 1% level; **significant at 5% level; *significant at 10% level.

Table S7. Robustness check: Fixed effects regressions with 1-y and 2-y lead treatment variables (*title_+1* and *title_+2*)

Treatment variable	Model					
	Deforestation + disturbance		Deforestation		Disturbance	
Outcome	7A	7B	8A	8B	9A	9B
title_+2	0.0384 (0.1648)	0.0163 (0.1500)	0.0014 (0.0699)	−0.0151 (0.0681)	0.0370 (0.1100)	0.0314 (0.0975)
title_+1	0.0501 (0.1040)	0.0658 (0.0989)	0.0235 (0.0562)	0.0352 (0.0533)	0.0267 (0.0553)	0.0306 (0.0537)
title_0	−0.5350*** (0.1704)		−0.2755*** (0.0897)		−0.2594*** (0.098)	
title_1	−0.3282 (0.2048)		−0.1206** (0.0606)		−0.2076 (0.1539)	
title_1c		−0.4301** (0.1693)		−0.1970*** (0.0702)		−0.2331** (0.1140)
Com.-fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Year-fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Control variables?	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SEs?	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.1816	0.1790	0.1564	0.1512	0.1631	0.1626
$P > F$	0.0303	0.0252	0.0723	0.0560	0.0214	0.0177

Dependent variable is percentage of community deforested or disturbed in year t = 2000–2005; treatment is award of formal title (SE) (n = 43 communities × 6 y = 258 community-years).

***Significant at 1% level; **significant at 5% level.

Table S8. Robustness check: Fixed effects regressions using a sample that drops neighboring communities

Treatment variable	Model					
	Deforestation + disturbance		Deforestation		Disturbance	
Outcome	10A	10B	11A	11B	12A	12B
title_0	−0.2607** (0.1265)		−0.1244* (0.0659)		−0.1363* (0.0687)	
title_1	−0.2190(0.1562)		−0.0741 (0.0519)		−0.1450 (0.1186)	
title_1c		−0.2442* (0.1234)		−0.1044* (0.0579)		−0.1398* (0.0763)
Counterfactual†	0.4112*** (0.0343)	0.4119*** (0.0338)	0.1568*** (0.0156)	0.1576*** (0.0158)	0.2544*** (0.0218)	0.2543*** (0.0209)
Com.-fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Year-fixed effects?	Yes	Yes	Yes	Yes	Yes	Yes
Control variables?	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SEs?	Yes	Yes	Yes	Yes	Yes	Yes
Within R^2	0.1544	0.1543	0.1278	0.1263	0.1352	0.1352
$P > F$	0.0109	0.0078	0.0067	0.0052	0.0530	0.0348

Communities are considered neighbors if their borders are with 10 km of each other; the dependent variable is the percentage of community deforested or disturbed in year t = 2000–2005; treatment is award of formal title (SE) (n = 28 communities × 6 y = 168 community-years).

†Predicted outcome with all treatment variables is set equal to zero; computed using the delta-method.

***Significant at 1% level; **significant at 5% level; *significant at 10% level.