Who Secures Property Rights Under Contested Authority?

Armed Groups, the State, and Tenure Security in the Colombian Amazon

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Abstract

A large literature shows that armed groups can win civilian support by supplying order when the state does not. Yet whether armed groups can substitute for the state as enforcers of property rights is less clear. We argue that an armed group can produce order while remaining a threat to the land it governs. We study this question in the Colombian Amazon, where armed groups govern: nearly three in ten households we surveyed say armed groups make, monitor, or enforce local rules governing land and resource use, about as many as say the same of the national state. Using an experiment embedded in a survey of 2,024 households, we find that civilians judge land 30 percentage points less secure under armed-group protection than under state protection. The gap narrows among households exposed to armed rule but does not close. Even the most state-skeptical households prefer state protection by nearly 25 points, and nearly three in four state-skeptical households with an undocumented field would pay for a state title. Observational data support these patterns: households under greater armed governance report more fear that armed groups will take their land and are more likely to have lost access to forest they once used. These findings distinguish governing capacity from the ability to credibly protect property rights.

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1 Introduction

In settings where the state is absent, weak, or predatory, armed groups often fill the vacuum by setting social and economic rules, settling disputes, and maintaining order (Arjona, 2016; Breslawski, 2021; Liu, 2022; Mampilly, 2011; Loyle et al., 2023). A growing literature shows that this governance can produce civilian support. Peasant communities in Peru welcomed the Shining Path where it punished wrongdoers whom local authorities failed to sanction (La Serna, 2012); Afghan communities under Taliban rule describe the group's courts as faster and less corrupt than the state's (Jackson and Weigand, 2020); and Iraqis who viewed the Islamic State's governance as superior to the Iraqi state's were more likely to remain under its rule than to migrate (Revkin, 2021). Similar dynamics appear among criminal gangs in Medellín (Blattman et al., 2025) and in Colombia's rural periphery, where multiple armed groups overlap (Arjona, 2016; De Bruin et al., 2025). Armed groups can, and often do, appear to substitute for the state.

Yet whether armed groups can substitute for the state in every domain is less clear. Property rights are a hard case. Protecting property requires not only the capacity to make and enforce rules, but also a credible commitment not to violate them (Frye, 2004; Haber, Razo and Maurer, 2003; North and Weingast, 1989). Armed groups face two obstacles to making that commitment. First, rebels on extraction frontiers profit from the land and resources they control, which may make them the actor most likely to take a household's land (International Crisis Group, 2021). Second, rebel rule is often short-lived: it rises and falls with territorial control, group fragmentation, and peace processes. Civilians therefore face uncertainty about how long rebel order will last. Our argument, in short, is that an armed group can enforce rules while remaining a threat to the land it governs. Civilians may thus accept armed groups as rulers while still trusting the state more as a guarantor of property rights.

In this research note, we test several expectations that this argument yields. If armed groups cannot credibly commit to protecting property rights, civilians should judge land as less secure under armed than under state protection. Formal documentation should not close this gap, because

a title is only as secure as the actor who enforces it. The gap should also persist among the civilians most inclined to accept armed rule, and among those most disillusioned with the state. Finally, if armed groups themselves threaten land rights, households under armed-group governance should fear armed groups in particular and be more likely to lose access to land and forest they use.

We test these expectations in the Colombian Amazon, where state agencies, rebel groups, and local civilian institutions create and enforce overlapping rules governing land use. Data from settings like this are rare. Armed groups tend to govern a country's remote margins, and the presence of armed groups makes these places difficult and risky to survey. *De facto* governance also leaves few administrative traces, so learning who governs often requires asking the people who live under armed rule.

Between February and April 2023, we surveyed 2,024 households on Colombia's agricultural frontier, nearly a third of whom report that armed groups help govern their community. Embedded in the survey is a conjoint experiment in which respondents chose between two hypothetical scenarios, selecting the one where they would feel more confident that their rights to land were safe. The scenarios vary on several attributes; our focus is whether the state or an armed group enforces property rights. The design offers a key advantage in this setting. Asking respondents directly whether they trust the armed groups that govern them likely invites social desirability bias or preference falsification (Matanock and García-Sánchez, 2018). Because several attributes vary at once, the design partly masks views of armed groups, which can reduce that bias (Horiuchi, Markovich and Yamamoto, 2022).

We find that replacing state enforcement with armed-group enforcement lowers perceived land tenure security by 30 percentage points. This enforcer penalty is substantial and outweighs the value of a legal title for land: on average, a titled plot under armed protection is seen as less secure than an untitled plot under state protection. This penalty for armed-group enforcement survives among those most inclined to accept armed rule or reject the state. While the armed-group penalty shrinks with exposure to armed governance, it remains 22 points among the most

exposed households, and 20 points among households that name armed groups as the enforcers of local forest rules. The state's advantage also survives among its critics: the most state-skeptical third of households still prefer state protection by nearly 25 points.

Survey responses outside the experiment point in the same direction. Among the most state-skeptical households, only 32 percent agree that the authorities protect land rights, yet 72 percent of those with an undocumented field say they would pay for a state-issued title. Households that report more armed-group governance are also less likely to consider their land safe from armed groups or criminals, and more likely to have lost access to forest they once used: a one-standard-deviation increase in armed governance is associated with a 2.3-point higher probability of losing access, against a base rate of 6.5 percent. These patterns do not identify causal effects of governance, but they corroborate the distinction revealed in the experiment between governing authority and credible protection.

Our findings make two contributions. First, they contribute to research on rebel governance by shifting the focus from whether civilians generally accept armed rule to the domains in which different governing actors hold advantages. Much of this work asks whether civilians accept armed rule or prefer it to state rule (Arjona, 2016; Mampilly, 2011; Loyle et al., 2023; De Bruin et al., 2025). But authority can vary across domains (Baldwin, Kao and Lust, 2024), and recent work shows rebel governance can harm wealth and human capital (Bandiera et al., 2022) and has mixed effects on environmental outcomes (Balán, Huntington and Tellez, 2026). Mapping these domain-specific advantages offers a richer account of what rebel governance can and cannot deliver.

Second, our results identify a scope condition for theories of land formalization and legibility. Recent evidence suggests that what matters for tenure security is formal documentation rather than the enforcer: citizens view land rights as similarly secure whether the state or a customary authority stands behind them (Ferree et al., 2023). We show that this claim breaks down when the enforcer may itself pose a threat to the claim. In that case, who enforces property rights matters independently of whether land rights are formally documented.

2 Setting and Data

2.1 State and Armed Group Governance on the Amazon Frontier

Land governance in the Colombian Amazon is divided among armed groups, national state agencies, environmental authorities, municipal governments, and local civilian organizations. These actors make and enforce overlapping rules over land use, settlement, forest access, and resource extraction. We study the agricultural frontier of southern Meta, Guaviare, and Caquetá, around Chiribiquete National Park, where state authority is uneven and armed groups remain involved in local governance. Armed groups play a substantial governance role in these communities: in our data, they perform at least one governance role for 22 percent of households in the municipal veredas and 36 percent in the frontier polygons (described below).

Here, armed groups both govern land and threaten it. The FARC controlled much of the region for decades, granting or denying permission for citizens to develop settlements, clear forests for agriculture and cattle, and otherwise use resources (International Crisis Group, 2021). After the 2016 peace agreement, successor groups expanded into parts of the frontier and became involved in taxation, land transactions, cattle, coca, and forest clearing (International Crisis Group, 2021; Foundation for Conservation and Sustainable Development, 2023). These groups also regulate land sales and settle disputes. At the same time, they can threaten residents' land claims. In our survey, 24 percent of households report that armed groups or criminals are somewhat or very likely to take one of their fields within the next five years. Notably, the government follows closely at 20 percent, which speaks to the general tenure insecurity of the region.[1]

Moreover, while the state is the source of legally recognized land rights, it is an imperfect guarantor. Colombian land reform has historically served as a counterinsurgency tool (Albertus and Kaplan, 2013). The National Land Agency (*Agencia Nacional de Tierras*, ANT) and municipal institutions administer land formalization, and the 2016 peace agreement expanded commitments

[1]The survey does not identify specific armed groups.

to rural land access and titling (Gobierno de Colombia and FARC-EP, 2016). At the same time, state intervention on the frontier includes coca eradication and environmental restrictions on settlement and forest use (International Crisis Group, 2021). PNIS (*Programa Nacional Integral de Sustitución de Cultivos de Uso Ilícito*), the coca-substitution program created by the 2016 peace agreement, illustrates this tension. Participating households agreed to eradicate coca in exchange for state payments and alternative livelihoods programs, but implementation was often delayed or incomplete (Acero and Machuca, 2021; International Crisis Group, 2021). The result is that households are effectively comparing two imperfect protectors—the state and armed groups—rather than a protector and a predator.

Formal land rights are also uncommon. Among households in our data with at least one field, only 10 percent hold a formal title on any field. Another 8 percent hold a public deed or land-agency resolution, 33 percent hold private sale papers, and 8 percent hold another document; 40 percent hold no document at all (Appendix Figure A2). Much frontier land remains public land or falls within forest reserves and protected areas, where formalization is restricted. Formal documentation and the identity of the authority enforcing a claim are therefore separate dimensions of tenure security. Our experiment varies both.

2.2 Data

Our data come from the baseline survey of a planned impact evaluation of the Land for Prosperity (LfP)—Southern Meta and the Vicinity of Chiribiquete National Park (SMVC) project, a USAID program[2] supporting land formalization and conservation on Colombia's Amazon frontier (Huntington et al., 2023). Between February and April 2023, we surveyed 2,024 households in eight

[2]Because the survey is a baseline, it predates LfP's land interventions in the study area. The parcel sweep in Puerto Rico and the community formalization pilots near Chiribiquete began after our fieldwork. Our measures of governance and tenure security therefore capture conditions before the program could alter them. One smaller component, a cocoa public–private partnership in southern Meta, began in 2022. It supports alternative livelihoods rather than land rights.

municipalities across the program's two geographic focus areas.[3] In the first area, we surveyed rural veredas—Colombia's smallest rural administrative units—in and around Puerto Rico, Meta, a former FARC stronghold, where LfP planned a municipal parcel sweep. We refer to these locations as "municipal veredas." The second area encompasses unincorporated frontier settlements near Chiribiquete National Park in Guaviare and Caquetá, which we refer to as "frontier polygons," where LfP planned community formalization pilots and where cattle ranching, coca cultivation, and forest clearing have expanded in recent years. Appendix Figure A1 shows the study area and sampling locations.

The survey measures local governance by asking respondents which actors make rules, monitor compliance, and impose sanctions in their community. Respondents could name six types of actors: armed groups, the national state, environmental authorities, municipal governments, community action boards (JACs), and other local organizations. For each actor, we count the number of governance roles it performs and standardize this measure across households.

We measure tenure security by asking respondents how likely different actors are to take one of their fields in the next five years. The potential threats include armed groups or criminals, the government, previous or absent owners, outside investors, neighboring communities, other members of the community, and extended family. We code a household as secure against a given threat when it reports that losing land to that actor is "very unlikely." We also measure whether households lost access during the previous four years to forest they had previously used and the reason for that loss.

Finally, the survey records the documentation households hold for ownership or use rights to each field and their investment in land, including how they divide spending between short-horizon inputs, such as fertilizer, seed, and pesticide, and longer-horizon assets.

[3]The sample was designed to cover the program's future treatment and comparison areas, not rural Colombia as a whole, so our ability to generalize is limited.

2.3 Land tenure conjoint experiment

Our argument implies that civilians separate an actor's capacity to govern from the credibility of its promise to protect property. Observational data cannot easily test this distinction. Armed groups govern in particular places, so households under armed rule systematically differ from others. We therefore embedded a paired forced-choice experiment in the survey that holds a household's plot of land fixed and varies who promises to protect it (Hainmueller, Hopkins and Yamamoto, 2014). Because the vignette describes enforcement as a promise, respondents must judge whose commitment is more credible, the core of our argument. The design also randomizes whether the household holds formal documentation of land ownership, which lets us compare the value of documentation with the value of the enforcer, the comparison at the center of recent work on formalization (Ferree et al., 2023).

Each respondent saw two hypothetical plots and chose the one where they would feel more confident that their property was "safe and respected." Respondents first read:

> "In some communities, people's land is safe and clearly respected by others, while in other communities, people's land is unsafe and others may disrespect it. Imagine the following two hypothetical scenarios..."

Each hypothetical plot then read:

> "Your plot is in an area that is of [high/low] interest to agro-businesses from outside the community. You [have/do not have] formal papers showing you own this land. In addition, [the state/armed groups] have promised to enforce people's property rights in this area."

The experiment independently[4] randomized the three binary attributes for each plot: (1) whether property rights were enforced by the state or by armed groups, (2) whether the plot had a formal ti-

[4]When the two randomized profiles were identical, the survey omitted the choice question so as not to confuse respondents. This occurred for 256 households.

tle, and (3) whether the area attracted high or low interest from outside agribusinesses. Our interest is in the first attribute. Respondents then answered: "In which case would you feel more confident that your property is safe and respected?" Each respondent evaluated one pair of profiles.

To estimate the effect of who protects property on respondents' perception of land tenure security, we use the following model:

$$\text{Chosen}_{ij} = \alpha + \beta_1 \text{Armed}_{ij} + \beta_2 \text{Title}_{ij} + \beta_3 \text{HighInterest}_{ij} + \varepsilon_{ij},$$

where i indexes respondents and $j \in \{1,2\}$ the two plots shown to each respondent. Armed_{ij}, Title_{ij}, and HighInterest_{ij} are indicators for armed-group enforcement, formal papers, and high agribusiness interest; the omitted categories are state enforcement, no papers, and low interest. We estimate a linear probability model with standard errors clustered by vereda. The coefficient β_1 captures the average effect of armed-group rather than state enforcement on the probability that a plot is chosen as more secure.

3 Results

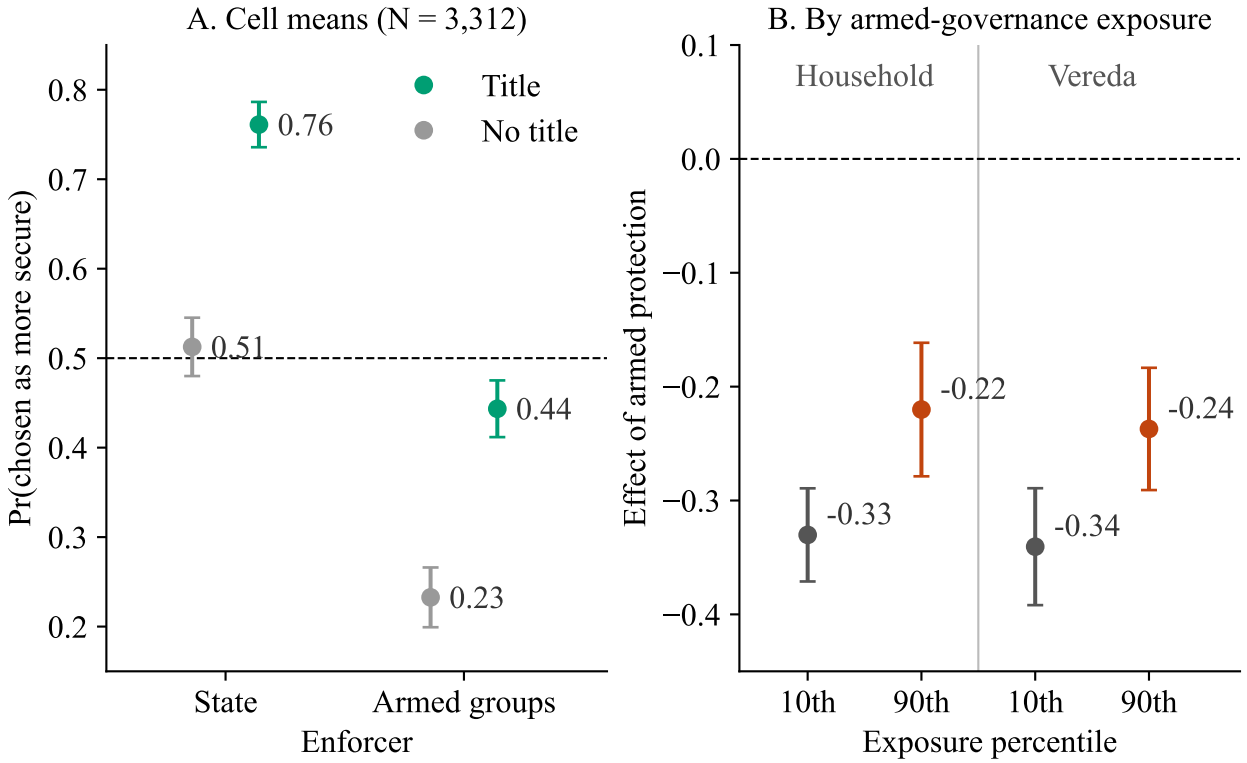
3.1 Experimental evidence

Appendix Table A1 reports the full experimental results. Replacing the state with an armed group as the enforcer lowers the probability that a plot is chosen as more secure by 30 percentage points ($p < .001$). Because enforcer identity is randomized independently of the other attributes, this penalty captures the causal effect of replacing state with armed-group enforcement on perceived tenure security. We interpret this as evidence that respondents find armed groups less credible guarantors of property rights. It is also the largest effect in the design: a formal title raises perceived security by 23 points, while outside agribusiness interest has no clear effect (-2.3 points, $p = .25$).

Panel A of Figure 1 extends the main specification by testing to what extent a legal title changes

the penalty on the identity of the enforcer. A title raises perceived security by similar amounts under both enforcers, from 51 to 76 percent under the state and from 23 to 44 percent under armed groups (24.8 vs. 21.1 points; difference $p = .23$). But a title does not close the gap between enforcers. A titled plot under armed enforcement is chosen less often than an untitled plot under state enforcement (44 vs. 51 percent, $p = .014$). Documentation and enforcement are thus separate sources of tenure security, and the enforcer matters more. This contrasts with evidence that the authority behind a land claim matters little when the alternative to the state is a customary authority (Ferree et al., 2023).

Figure 1: Perceived security by enforcer, title, and exposure to armed governance



Notes: Panel A: probability that a profile is chosen as more secure, by enforcer and title, 95% confidence intervals clustered by vereda. Panel B: effect of armed enforcement at the 10th and 90th percentiles of armed-governance exposure. Household: the number of governance roles the respondent assigns to armed groups. Vereda: its mean across respondents in the vereda. Appendix Table A3 reports the estimates. Attribute estimates in Appendix Table A1.

If governing builds trust, the armed-group penalty should shrink among households that live under armed rule, and perhaps disappear among those who experience it most directly. Panel B

of Figure 1 shows that the armed-group penalty shrinks but does not disappear. Between the 10th and 90th percentiles of household exposure to armed governance, the penalty falls from 33 to 22 percentage points (interaction $p = .003$); across veredas, it falls from 34 to 24 points ($p = .014$). Even at high exposure, the penalty remains large (-22 points, $p < .001$).

The penalty persists among households most familiar with armed enforcement. Households that name armed groups as enforcers of forest rules still penalize armed protection by 20.0 points, compared with 33.8 points among other households (Appendix Table A4; difference $p < .001$). Among these households, the penalty does not vary with how highly they rate local enforcement (-21.1 vs. -20.0, $p = .89$) or with their satisfaction with forest management ($p = .29$).[5] Even households that know armed enforcement firsthand, and rate local enforcement highly, do not treat armed groups as credible protectors of property.

Similarly, if the state's advantage as a guarantor of property depends on residents holding positive views of the state, then it should disappear among households that the state has failed or that distrust it. We instead find that negative experiences with or views of the state do not reverse the preference for the state over armed groups. Preference for a state enforcer of property rights is similar among PNIS participants (whom the state promised payments that often arrived late or not at all) and nonparticipants (33.0 vs. 29.2 points; difference $p = .41$) and among households that do and do not fear losing their land to the government (30.5 vs. 30.0 points; $p = .87$). In both cases, the confidence intervals rule out reductions larger than about five to six points (Appendix Table A6). Indeed, the most state-skeptical third still prefer state protection by 24.5 points ($p < .001$). This premium is smaller than among other households (32.6 points, $p = .029$), although the difference does not survive correction for seven comparisons (Holm $p = .15$). Moreover, households that distrust all four state institutions operating in the area still show a 20.0-point premium ($p = .003$). Skepticism of the state thus narrows its advantage but does not eliminate it.

[5]The same pattern appears when neighbors supply the rating: the penalty is -20.5 points where neighbors' ratings are low and -19.0 where they are high (Appendix Table A5; slope $p = .86$).

3.2 Observational evidence

Survey responses outside the experiment echo these patterns. Among the most state-skeptical third of households, only 32 percent agree that the authorities protect land rights, yet 72 percent of those with an undocumented field would pay for a state-issued title (Appendix Table A7). These responses show that residents distinguish between confidence in state institutions and demand for state-backed property rights.

We next ask whether households that actually live under armed governance report more insecurity, and from whom. We estimate:

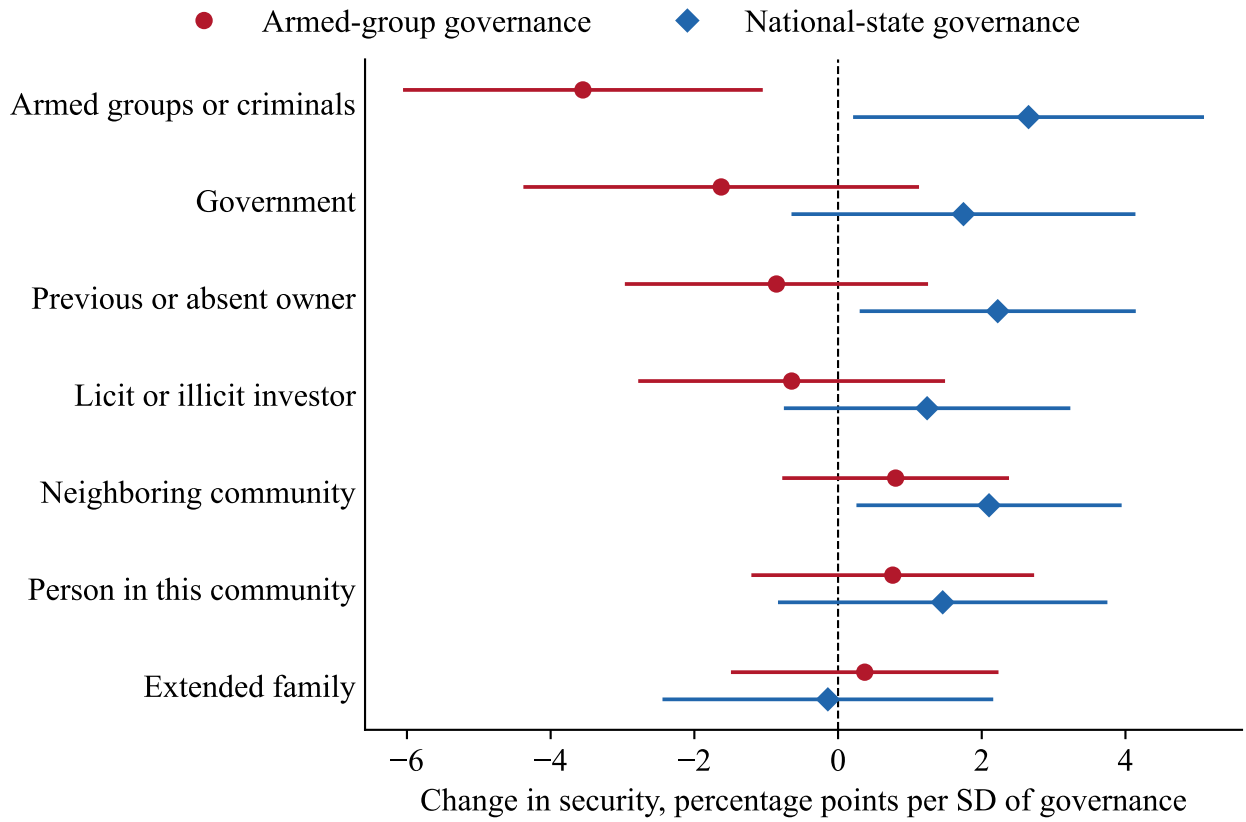
$$Y_{iv} = \alpha + \sum_{a=1}^{6} \delta_a G_{a,iv} + \mathbf{X}'_{iv}\gamma + \varepsilon_{iv}, \tag{1}$$

where Y_{iv} is the outcome for household i in *vereda* v. Depending on the specification, the outcome is perceived security from a particular source of land loss or reported loss of forest access. $G_{a,iv}$ measures the number of governance roles—rule-making, monitoring, and enforcement—that household i assigns to actor a, standardized to have mean zero and standard deviation one. We include measures for six actors: armed groups, the national state, environmental authorities, community action boards (JACs), other local organizations, and municipal government. Our coefficients of interest are those for armed groups and the national state; the remaining actors enter jointly in every specification. $\mathbf{X}_{iv}$ includes 11 household and sampling controls, listed in the notes to Table 1. Standard errors are clustered by vereda.

We find that households under armed governance feel less secure, and the insecurity they report concerns the armed groups themselves (Figure 2). Respondents rate how likely each of seven actors is to take their land in the next five years. A one-standard-deviation increase in armed-group governance is associated with 3.6 percentage points lower perceived security from armed groups or criminals ($p = .005$), while none of the other six threats is associated with armed governance. By contrast, state governance is associated with greater security from armed groups or criminals,

previous or absent owners, and neighboring communities, with associations ranging from 2.1 to 2.7 percentage points.

Figure 2: Governance and security from specific threats of land loss

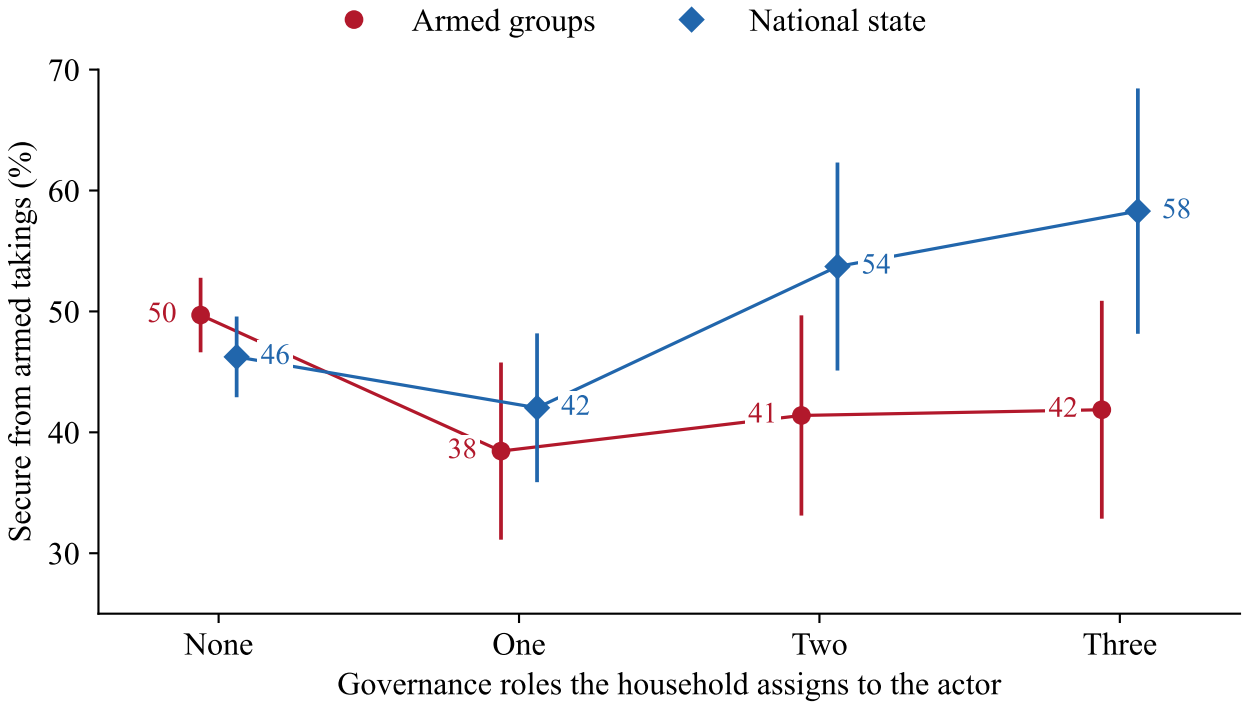


Notes: Coefficients per standard deviation of armed-group and national-state governance roles, with 95% confidence intervals clustered by vereda. Each row estimates equation (1) using a linear probability model (1,717 households for armed groups or criminals; 1,884–1,915 for the other threats). Outcome: 1 if the household rates its land very unlikely to be taken by the named actor in the next five years. All six governance measures and 11 household and sampling controls enter jointly. Appendix Table A8 reports all coefficients.

Figure 3 further distinguishes the extensive and intensive margins of governance. Households that assign armed groups even one governance role report substantially lower security from armed takings: 38 percent, compared with 50 percent among households assigning them no role ($p = .005$). Households assigning armed groups additional roles report similar security, near 41–42 percent. The state shows the opposite pattern. Households assigning the state one governance role are no more secure (42 percent, $p = .22$), but security is 54 percent where the state performs

two roles and 58 percent where it performs all three, compared with 46 percent where it performs none ($p = .020$ for three roles). Armed groups are thus associated with lower security as soon as they govern at all, whereas greater security under the state appears only where state governance is extensive.

Figure 3: Security from armed takings by number of governance roles



Notes: Predicted percentage of households that rate their land very unlikely to be taken by armed groups or criminals in the next five years, by the number of governance roles (rule-making, monitoring, and enforcement) the household assigns to each actor. One linear probability model: for each of the six governance actors, separate terms for naming it for one, two or three roles, all actors jointly, with 11 household and sampling controls. Each line sets one actor's roles from zero to three and leaves all other variables at their observed values. 95% confidence intervals clustered by vereda. 1,717 households in 97 veredas. Households per point, from none to three roles: armed groups 1,221, 198, 155, 143; national state 1,173, 277, 163, 104. Appendix Table A9 reports all coefficients.

The *perceived* insecurity reported in Figure 2 is also reflected in households' reported experience. Table 1 shows that armed governance is associated with reported loss of access to the forest, which residents depend on for their livelihoods. A one-standard-deviation increase in armed-group governance is associated with a 2.3 percentage-point higher probability of losing access for any

reason, relative to a mean of 6.5 percent.

Most reported access loss is due to restrictions imposed by environmental authorities. Armed-group governance is associated with a 1.2-percentage-point higher probability of conservation-related loss, while loss attributed directly to armed groups or crime is only 0.6 points higher. National-state governance predicts none of these outcomes.

Panel B shows a similar pattern for perceived insecurity from armed groups. Households that fear armed takings are 3.4 percentage points more likely to report losing forest access. They are also more likely to report losses attributed to armed groups or crime (+1.6 points) and to conservation restrictions (+1.9 points). Thus, perceived insecurity tracks reported loss, although most reported losses concern access rather than ownership and are attributed to conservation restrictions rather than armed groups directly.

Finally, we provide exploratory evidence on an additional observable implication of tenure insecurity: households under greater armed governance, and those who fear armed takings, are more likely to buy short-horizon inputs (Appendix Table A11).

Table 1: Governance, fear of armed takings, and loss of forest access

	Lost access to forest in the past four years because of…		
	Any reason	Armed groups or crime	Conservation restrictions
Panel A: governance roles (per SD)			
Armed groups	2.3***	0.6*	1.2**
	(0.7)	(0.3)	(0.6)
National state	0.0	−0.3	0.3
	(0.5)	(0.2)	(0.5)
Households	1,914	1,914	1,914
Panel B: fear of armed takings (0/1)			
Fears armed takings	3.4***	1.6**	1.9**
	(1.2)	(0.6)	(0.9)
Households	1,714	1,714	1,714
Households with the loss	124	24	78
Outcome mean (%)	6.5	1.3	4.1
Veredas (clusters)	103	103	103

Notes: Linear probability models; coefficients in percentage points. Outcome: 1 if the household lost access to forest it used in the past four years for the stated reason, 0 if it reported no loss; a household may cite more than one reason. Conservation restrictions: access restricted by environmental authorities. Panel A: number of governance roles (rule-making, monitoring, and enforcement), standardized; all six governance actors enter jointly. Panel B: the household does not rate its land very unlikely to be taken by armed groups or criminals in the next five years (Figure 2). All models include 11 household and sampling controls: respondent born in the municipality, respondent age, female respondent, household size (top-coded at seven), top quintile of land owned, top quintile of durable assets, owns livestock, household went without enough food in the past 12 months, household depends heavily on the forest for basic needs, any field in a protected area, and an indicator for the frontier polygons. Panel B models include the controls but not the governance measures. Outcome mean and households with the loss refer to the Panel A sample. Appendix Table A10 reports the other four actors and the enforcers of forest rules. Standard errors clustered by vereda in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4 Conclusion

Armed groups fulfill a wide range of governance roles, including managing environmental resources, administering justice, and sustaining local institutions. Yet whether they can substitute for state governance across domains is an open empirical question. Property rights are a hard case

since they require not only governance but also the capacity to credibly commit to respecting rights (North and Weingast, 1989; Haber, Razo and Maurer, 2003; Frye, 2004).

In this note, we provided experimental evidence that replacing the state with an armed group enforcer sharply lowers perceived tenure security, even in a setting where armed-group governance is common and state presence is weak. The armed-group penalty is large enough that a titled plot under armed protection is, on average, considered less secure than an untitled plot under state protection. The penalty becomes smaller with exposure to armed governance but remains substantial, and the state premium persists even among households that distrust or fear the state. Survey data point in the same direction: households under greater armed governance are more likely to see armed groups themselves as a threat to their land and more likely to report loss of forest access.

These results suggest that armed-group and state governance are not substitutes across all domains (Baldwin, Kao and Lust, 2024). Governing capacity and credible commitment are distinct functions. Legitimacy or effective rule enforcement need not make a governing actor a credible guarantor of property rights. The results thus identify a limit of rebel governance: civilians may accept armed groups as rulers without trusting them as guarantors of property. Future work should examine how this commitment problem varies with the identity and durability of governing actors and with the characteristics of the assets and economic activities in the territories they govern.

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Supplementary Information:

Who Secures Property Rights Under Contested Authority?
Armed Groups, the State, and Tenure Security in the Colombian Amazon

Pablo Balán

Heather Huntington

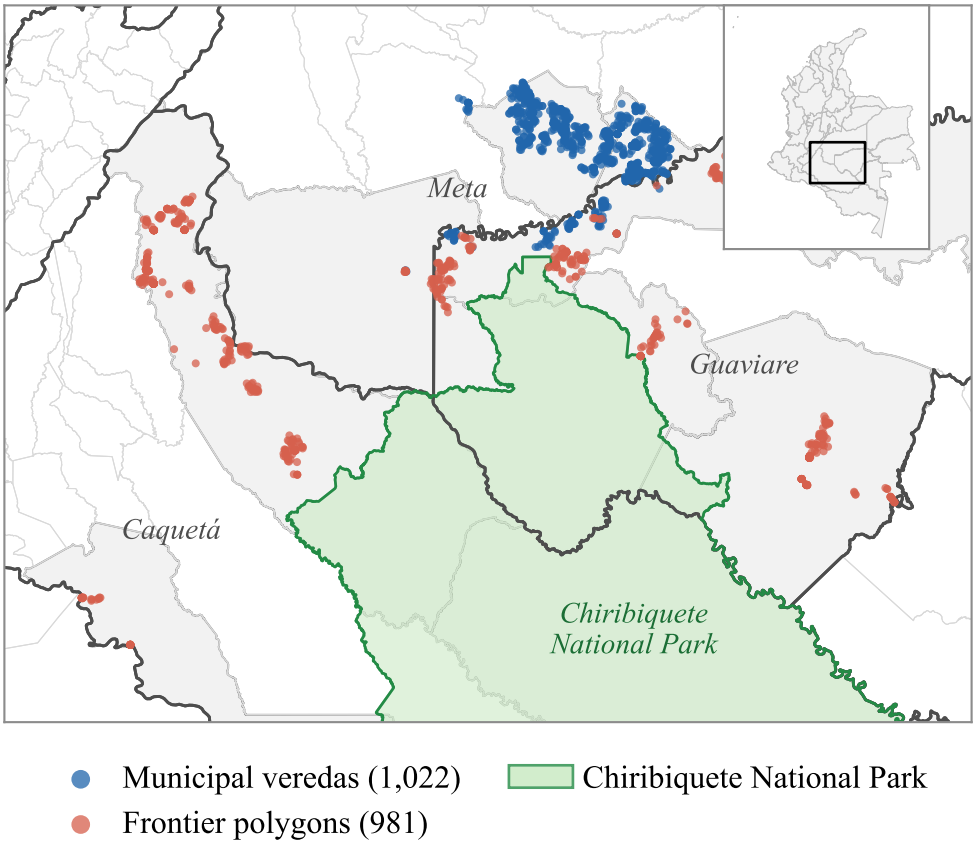
Juan Tellez

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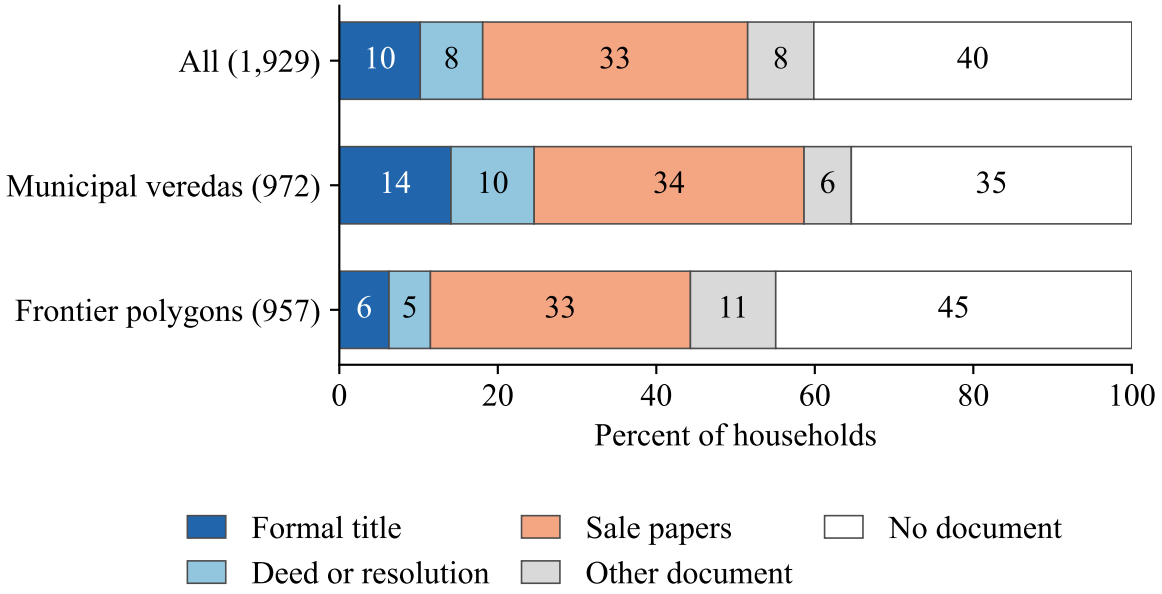
A Setting and sample

Figure A1: Study area and surveyed households



Notes: Each point is a surveyed household, by sample: the 2,003 validated households of the 2,024 surveyed. Shaded: the eight study municipalities. Thick lines: department boundaries. Green: Chiribiquete National Park, with the boundary set by its 2018 expansion. Boundaries: geoBoundaries (municipalities and departments) and OpenStreetMap (park). A few households geocode just outside the shaded municipalities. Inset: location of the study area in Colombia.

Figure A2: Land documents held by surveyed households



Notes: Percent of the 1,929 surveyed households with at least one field, by sample. Each household is counted once, according to the form of land-rights documentation it reports holding for any of its fields that ranks highest under the statutory system. Formal title: titled, registered or unregistered. Deed or resolution: public deed or a resolution of the former national land agency (INCORA/INCODER). Sale papers: sales letter, promise of sale, or document signed by the seller. Other document: succession, out-of-court statement, verbal contract, request for formalization, or other.

B Survey data and methods

Soluciones Estratégicas en Información (SEI), a Colombian research firm, conducted the quantitative and qualitative data collection. Household data collection took place from February 26 to April 11, 2023. Enumerators recorded responses directly on Android smartphones using SurveyCTO, a cloud-based mobile data-collection platform.

The University of Pennsylvania Institutional Review Board approved the study in January 2023 (protocol 852855). Before administering the survey, enumerators read each prospective participant a standardized consent statement describing the purpose and content of the research, its anticipated risks and benefits, the expected time commitment, and the measures used to protect confidentiality. Participants provided verbal informed consent, which enumerators recorded in the electronic survey form. Data-quality procedures included direct observation of enumerators, field presence by the SEI field manager, daily remote checks by the research team, and audits and reinterviews of selected respondents.

The household survey took about 60 minutes to administer. It measured perceived tenure security, land rights and conflicts; livelihoods, income, and well-being; productive and housing investments and land clearing; and households' knowledge, experiences, and perceptions of land and

forest governance.

The study focuses on communities along Colombia's expanding agricultural frontier in Meta, Guaviare, and Caquetá. The sampling frame was originally constructed around two USAID Land for Prosperity activities. In Puerto Rico, Meta, the evaluation team randomly selected 21 *veredas* from communities eligible for the municipal parcel sweep and added 25 communities in the neighboring municipalities of Puerto Concordia, La Macarena, and San José del Guaviare. For the community-level formalization component, the team sampled communities within priority polygons defined by the overlap of deforestation hotspots, ecological-connectivity corridors, settlements with high tenure informality, and areas prioritized for right-of-use contracts. Because the original polygons varied greatly in size and population, 71 polygons were consolidated into 30 "macropolygons." Within each macropolygon, one or more *veredas* were selected based on their geographic coverage of the macropolygon, population, rural character, and forest presence. Thus, the sample was designed to capture communities at the intersection of forest pressure, informal settlement, and overlapping environmental and land-governance institutions. It was designed to represent treatment and control areas for a future program evaluation of the Land for Prosperity activities, not rural Colombia as a whole.

Within the selected *veredas* and polygons, field teams used randomized procedures to select resident households and confirmed household locations by GPS. The design generally targeted at least 20 households per selected *vereda* or macropolygon, with larger samples in the original program areas and more populous or geographically extensive polygons. The resulting sample comprised 2,024 households across Puerto Rico and surrounding areas in Meta, Guaviare, and Caquetá. Because armed-group activity prevented access to 14 initially selected *veredas* and to Cartagena del Chairá, inaccessible communities were replaced, where possible, with the nearest accessible *veredas*; the sample therefore does not represent communities that could not safely be reached during fieldwork.

C Survey experiment

Table A1: Effects of randomized plot attributes on perceived security

	Pooled estimator, by sample			Alternative estimators, all respondents		
	(1) All	(2) Municipal veredas	(3) Frontier polygons	(4) Position control	(5) Support-standardized	(6) Respondent FE
Armed enforcement (vs. state)	-30.0^{***}	-32.5^{***}	-27.5^{***}	-29.9^{***}	-30.0^{***}	-52.0^{***}
	(1.8)	(2.3)	(2.7)	(1.8)	(1.7)	(4.1)
Plot has a formal title	23.1^{***}	24.3^{***}	21.7^{***}	23.1^{***}	22.1^{***}	38.7^{***}
	(1.9)	(2.8)	(2.8)	(1.9)	(1.7)	(4.2)
High agribusiness interest	-2.3	-6.3^{**}	1.8	-2.2	-2.3	-3.1
	(2.0)	(2.9)	(2.5)	(2.0)	(1.9)	(4.7)
Profile choices	3,312	1,684	1,628	3,312	3,312	3,312
Respondents	1,656	842	814	1,656	1,656	1,656
Veredas (clusters)	103	46	59	103	103	103

Notes: Sample: 1,656 respondents who recorded a choice (Appendix Table A2); two veredas appear in both samples. Linear probability models; the outcome is 1 if the respondent chose the profile as the one where the property would be safer and respected. Coefficients in percentage points. (1)–(3) pooled estimator. (4) adds an indicator for the first-listed profile. (5) differences in means over the 56 own-by-opponent profile cells with uniform weights, which removes the dependence on the instrument's skipping of identical pairs. (6) respondent fixed effects and a position indicator; the estimate is a within-pair contrast, not an average marginal component effect. Standard errors clustered by vereda in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A1 reports the attribute effects behind Figure 1. Armed enforcement lowers the probability that a plot is chosen as more secure by 30.0 percentage points, and a formal title raises it by 23.1 points; agribusiness interest has no effect in the pooled sample (−2.3 points). The enforcer and title effects hold in both settings: −32.5 and 24.3 points in the municipal veredas, −27.5 and 21.7 points in the frontier polygons. Agribusiness interest lowers security only in the municipal veredas (−6.3 points). Controlling for the order in which the two plots appeared, or weighting the 56 attribute combinations equally, leaves the estimates unchanged. Respondent fixed effects yield larger contrasts (−52.0 and 38.7 points), as expected for a comparison within each pair.

Table A2 shows that the randomized attributes are balanced on the 11 household and sampling controls. Respondents shown more armed-protected plots were more likely to record no choice; if every such non-chooser had chosen the armed-protected plot, the armed-enforcement penalty would be 26.0 points instead of 30.0.

Table A2: Randomized attributes: balance and missing choices

	Armed groups enforce	Formal papers	High agribusiness interest
Panel A: balance on household characteristics (plots)			
Share of plots (%)	47.5	52.2	50.0
Joint test of 11 controls, p	0.36	0.77	0.13
Plots	3,182	3,182	3,182
Panel B: no choice recorded (respondents shown two different plots)			
Plots shown with the attribute (0–2)	2.5***	−1.7*	0.9
	(0.7)	(1.0)	(0.7)
Respondents		1,747	
No choice recorded (%)		5.2	
Joint test, p		0.001	

Notes: Panel A: each randomized attribute regressed on the 11 household and sampling controls, one observation per plot shown to the 1,656 respondents who made a choice; plots with a missing control are dropped. Panel B: linear probability model of recording no choice on the number of the two plots that carried each attribute, among the 1,747 respondents shown two different plots (256 respondents shown identical plots were not asked to choose). No choice was recorded for 2.9% of respondents shown no armed-protected plot, 5.3% shown one, and 7.8% shown two. If all 52 non-choosers shown one armed-protected and one state-protected plot had chosen the armed-protected plot, the armed-enforcement penalty would be 26.0 points (SE 1.8) instead of 30.0. Coefficients in percentage points; standard errors clustered by vereda (103 veredas) in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Armed-enforcement effect by exposure to armed governance

	Exposure measure	
	Household	Vereda
Effect of armed enforcement at low exposure (10th percentile)	-33.0^{***}	-34.1^{***}
	(2.1)	(2.6)
Effect of armed enforcement at high exposure (90th percentile)	-22.0^{***}	-23.7^{***}
	(3.0)	(2.7)
Difference, high minus low	11.0^{***}	10.3^{**}
	(3.6)	(4.1)
Respondents	1,656	1,656
Veredas (clusters)	103	103

Notes: The model of Figure 1, Panel B: the outcome of Table A1 regressed on the three randomized attributes, each interacted with armed-governance exposure. Household exposure: the number of governance roles (rule-making, monitoring, enforcement) the respondent assigns to armed groups, standardized (10th and 90th percentiles: -0.57 and 1.52). Vereda exposure: the mean of the household measure across respondents in the vereda, the respondent included (-0.57 and 0.85). Effects in percentage points. Standard errors clustered by vereda in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Armed-protection penalty by exposure to armed enforcement and rating of local enforcement

Group	Penalty (pp)	Respondents
Panel A: armed groups named as an enforcer of forest rules		
Not named	-33.8^{***}	1,067
	(2.1)	
Named	-20.0^{***}	412
	(3.1)	
Difference	13.8^{***}	
	(3.7)	
Panel B: enforcement strength, households naming armed groups		
Strong	-21.1^{***}	190
	(5.2)	
Weak	-20.0^{***}	183
	(5.1)	
Difference	-1.1	
	(8.0)	
Panel C: satisfaction with forest management, households naming armed groups		
Satisfied	-22.4^{***}	249
	(3.8)	
Not satisfied	-15.6^{***}	162
	(5.3)	
Difference	-6.9	
	(6.5)	
Panel D: monitoring effectiveness, households naming armed groups		
Effective	-12.7^{**}	239
	(4.8)	
Not effective	-30.5^{***}	137
	(5.8)	
Difference	17.9^{**}	
	(8.1)	

Notes: Effect of armed-group protection relative to state protection on choosing a plot as more secure, in percentage points, within each group of respondents; the full-sample effect is -30.0. Estimates from forced-choice linear probability models of profile choice on the three randomized attributes, with the enforcer attribute interacted with group indicators. Difference: the named or favorable group minus the other group; a positive difference means a smaller penalty in the named or favorable group. Named: the respondent lists armed groups in answer to "Who are the three most important actors for enforcing rules about forest use and access in this vereda?" Ratings answer "How effectively are rules about forest use and access enforced in this area?" (Panel B), "Overall, how satisfied are you with how forests are managed in this area?" (Panel C), and "How effectively are rules about forest use and access monitored in this area?" (Panel D), on five-point scales: the two most favorable categories versus the other three; responses of not applicable or do not know are excluded. The ratings cover forest rules in the area, not armed groups as such. Exploratory: specified after the main results. Standard errors clustered by vereda in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4 asks whether the armed-protection penalty reflects reluctance to speak against armed groups. Respondents who name armed groups among the enforcers of forest rules in their vereda penalize armed protection less (-20.0 vs. -33.8 points), but the penalty remains large. Among these respondents, the penalty does not depend on how highly they rate local enforcement (-21.1 vs. -20.0 points, $p = .894$) or on their satisfaction with forest management (-22.4 vs. -15.6, $p = .291$). Respondents who rate local monitoring effective show a smaller penalty (-12.7 vs. -30.5, $p = .030$). Respondents who live under armed enforcement and rate local enforcement highly have little reason to feign a preference for the state, yet they still judge armed-protected land less secure.

Table A5: Armed-protection penalty by neighbors' rating of enforcement

	Penalty (pp) by vereda rating		Change per SD	
Neighbors' rating of…	Low (-1 SD)	High ($+1$ SD)	of rating	Respondents
Enforcement strength	-20.5^{***}	-19.0^{***}	0.7	396
	(6.7)	(4.0)	(4.1)	
Forest management	-20.1^{***}	-19.5^{***}	0.3	403
	(4.3)	(5.0)	(3.4)	
Monitoring effectiveness	-20.6^{***}	-19.3^{***}	0.7	401
	(4.6)	(4.2)	(3.1)	

Notes: Households that name armed groups among the enforcers of forest rules (Table A4); ratings as in that table. The vereda rating is the share of the respondent's vereda giving a favorable rating (the two most favorable of five categories), computed without the respondent and standardized; veredas with fewer than five other raters are excluded. Estimates from a forced-choice linear probability model with the enforcer attribute interacted with the vereda rating. Exploratory: specified after the main results. Standard errors clustered by vereda in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In Table A4, the same respondent supplies the rating and the choice. Table A5 replaces each respondent's rating with the share of other households in the vereda that rate local enforcement favorably. The penalty is -20.5 points where neighbors' ratings are low and -19.0 points where they are high (slope $p = .862$). Neighbors' ratings of monitoring ($p = .822$) and of forest management ($p = .935$) give the same result. The monitoring difference in Table A4 does not appear when neighbors supply the rating.

Table A6: State advantage by respondent group

	State advantage (pp)			p-value of difference			
Respondents	Yes	No	Difference	Unadjusted	Holm	BH	Yes / No
In crop-substitution program (PNIS)	33.0*** (4.5)	29.2*** (1.8)	3.8 (4.6)	.414	1.000	.580	317 / 1,339
Fear government taking their land	30.5*** (2.6)	30.0*** (2.3)	0.5 (3.3)	.872	1.000	.872	741 / 885
Most state-skeptical third	24.5*** (3.2)	32.6*** (2.1)	−8.1 (3.7)	.029	.145	.068	555 / 1,098
Distrust all four state institutions	20.0*** (6.6)	30.8*** (2.3)	−10.8 (6.3)	.089	.356	.156	140 / 939
Both JACs and armed groups govern	22.0*** (3.2)	32.1*** (1.9)	−10.1 (3.5)	.004	.031	.026	350 / 1,306
Protected land, municipal veredas	12.3 (8.0)	35.3*** (2.4)	−23.0 (8.2)	.007	.045	.026	114 / 707
Protected land, frontier polygons	25.9*** (6.5)	27.4*** (3.0)	−1.5 (6.9)	.831	1.000	.872	128 / 663

Notes: State advantage: the effect of state rather than armed-group enforcement on the probability that a plot is chosen as more secure, in percentage points, within each group. Each row is one linear probability model of the experiment (Figure 1) in which the group indicator interacts with all three randomized attributes; no controls. Difference: Yes minus No. Holm and BH (Benjamini–Hochberg) adjust the difference p-values for the seven comparisons. Crop-substitution program: the household took part in PNIS, the coca-substitution program created by the 2016 peace agreement. Fear government taking their land: the household does not rate its land very unlikely to be taken by the government in the next five years. Most state-skeptical third: bottom third of an index of trust in the municipal hall, the police, and the national land agency (standardized items, averaged). Distrust all four: distrusts greatly or somewhat the municipal hall, the police, the national land agency, and the municipal land office. Both JACs and armed groups govern: the household names community action boards and armed groups for at least one governance role. Protected land: any field in a protected area, as reported by the household. Yes / No (last column): respondents in each group; groups exclude respondents missing the moderator. Standard errors clustered by vereda in parentheses; stars on the Yes and No columns test the state advantage against zero. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6 reports the state advantage—the armed-protection penalty with the sign reversed—within seven groups. The advantage is positive in every group and significant in all but one. It is similar among PNIS participants and nonparticipants (33.0 vs. 29.2 points) and among respondents who do and do not fear government takings (30.5 vs. 30.0 points). It is smaller among the most state-skeptical third (24.5 vs. 32.6 points, $p = .029$) and among respondents who distrust all four state institutions (20.0 vs. 30.8 points, $p = .089$); neither difference survives correction for seven comparisons. Two differences survive: households where both JACs and armed groups govern (22.0 vs. 32.1 points, Holm $p = .031$) and households reporting protected land in the municipal

veredas (12.3 vs. 35.3 points, Holm $p = .045$). The joint-governance difference depends on the cutoff: 22.9 vs. 31.3 points ($p = .035$) when both actors exceed their mean number of roles, and 25.9 vs. 30.5 points ($p = .42$) when both hold two or more roles. Reports of protected land may reflect awareness of restrictions rather than legal designation.

Table A7: Demand for state-issued titles and confidence in state protection of land

	All	Municipal veredas	Frontier polygons	Most state-skeptical third	Distrust all four institutions
Willing to pay for a state-issued title (households with an undocumented field)					
Yes (%)	74	72	75	72	64
Households	950	438	512	318	78
Land rights are well protected by the authorities (all households)					
Agree (%)	43	49	36	32	34
Disagree (%)	42	35	50	58	59
Households	1,966	1,003	963	684	166

Notes: Shares of households in the analysis sample. Willing to pay: "Would you be willing to pay for a formal state-issued document proving your ownership of this field?" asked for each field with no legal document of any kind; a household counts as willing if it answers yes for at least one such field. The median amount offered is 100,000 pesos. Land rights: "Citizens' rights to land are well protected by the Colombian authorities," on a five-point scale; Agree combines agree and strongly agree, Disagree combines disagree and strongly disagree, and the remainder neither agree nor disagree; "don't know" answers are excluded. State skeptics and distrust of all four institutions as defined in Table A6.

Table A7 reports two survey items on the state's role in land tenure. Among households with a field that has no document, 74 percent would pay for a state-issued title, and the share is similar among the most state-skeptical third (72 percent) and smaller among households that distrust all four state institutions (64 percent). Confidence in state protection is lower and divided: 43 percent agree that the authorities protect citizens' land rights and 42 percent disagree; among state skeptics, 32 percent agree. Willingness to pay is stated, not revealed, and the median offer is 100,000 pesos.

D Governance and security from specific threats

Table A8: Governance and security from specific threats of land loss: all six governance measures

	Governance roles (per SD) of							
Threat of losing a field to…	Armed	Nat. state	Env. auth.	JAC	Other local	Municipal	Mean (%)	Households
Armed groups or criminals	−3.6***	2.7**	−0.8	0.5	0.0	0.8	47.0	1,717
	(1.2)	(1.2)	(1.2)	(1.3)	(1.3)	(1.3)		
Government	−1.6	1.7	0.1	0.5	−2.0	0.4	55.0	1,884
	(1.4)	(1.2)	(1.4)	(1.4)	(1.7)	(1.3)		
Previous or absent owner	−0.9	2.2**	2.0*	1.0	0.4	−1.6	67.5	1,915
	(1.1)	(1.0)	(1.1)	(1.2)	(1.3)	(1.2)		
Licit or illicit investor	−0.6	1.2	0.3	1.2	−0.2	−1.2	70.9	1,907
	(1.1)	(1.0)	(1.1)	(1.1)	(1.1)	(1.3)		
Neighboring community	0.8	2.1**	1.4	1.0	−0.6	−1.7	76.5	1,913
	(0.8)	(0.9)	(1.0)	(1.1)	(1.1)	(1.2)		
Person in this community	0.8	1.5	1.7*	0.7	0.8	−2.9**	75.9	1,913
	(1.0)	(1.1)	(1.0)	(1.1)	(1.0)	(1.2)		
Extended family	0.4	−0.1	0.8	0.9	0.4	−0.6	72.8	1,915
	(0.9)	(1.1)	(1.1)	(1.2)	(1.2)	(1.4)		
Any involuntary loss	−1.0	1.9	−0.3	0.6	0.0	−2.6**	61.4	1,918
	(1.1)	(1.2)	(1.1)	(1.1)	(1.4)	(1.2)		

Notes: Each row is one linear probability model. Outcome: 1 if the household rates its land very unlikely to be taken by the named actor in the next five years. Coefficients and means in percentage points. All six governance measures and 11 household and sampling controls enter jointly. The armed/criminal item was not fielded in two late form versions, hence its smaller sample. Standard errors clustered by vereda (97–103 clusters) in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8 reports all coefficients behind Figure 2, including the four governance actors the figure omits and an eighth outcome, any involuntary loss of the land. Environmental authorities, JACs, and other local organizations predict none of the eight outcomes at the 5 percent level. Municipal governance predicts less security from members of the household's own community (-2.9 points, $p = .021$) and against any involuntary loss (-2.6, $p = .032$). With 48 coefficients, a few significant cells are expected by chance; we did not predict the municipal pattern and do not interpret it.

E Number of governance roles

Table A9: Security from armed/criminal land takings, by number of governance roles: all six actors

Roles	Armed groups	National state	Environmental authorities	JAC	Other local	Municipal government
One role	-11.3^{***}	-4.2	-0.1	-2.6	2.5	1.9
	(3.9)	(3.4)	(2.7)	(2.9)	(3.5)	(3.5)
	[198]	[277]	[324]	[284]	[228]	[255]
Two roles	-8.3^{**}	7.5^{*}	-5.6^{*}	0.1	2.4	-4.8
	(4.1)	(4.4)	(3.2)	(4.0)	(4.7)	(4.9)
	[155]	[163]	[248]	[334]	[116]	[130]
Three roles	-7.8^{*}	12.1^{**}	1.6	1.4	-2.5	7.5
	(4.6)	(5.1)	(3.4)	(3.3)	(5.8)	(5.2)
	[143]	[104]	[248]	[396]	[98]	[117]
Share secure, no role (%)	49.9	46.6	48.5	48.1	46.6	46.4
p, one = three roles	.547	.010	.655	.300	.441	.369
p, all three = 0	.008	.035	.192	.740	.805	.241

Notes: The model of Figure 3, all coefficients shown (1,717 households, 97 veredas). Coefficients and shares in percentage points. Households in each cell in brackets. Standard errors clustered by vereda in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9 reports the model behind Figure 3 for all six actors. Relative to naming them for no role, naming armed groups for one role predicts 11.3 points less security from armed takings ($p = .005$); two and three roles predict similar declines (one vs. three roles, $p = .547$). Naming the national state for one role predicts no change (-4.2, $p = .222$); naming it for three roles predicts $+12.1$ points ($p = .020$). No other actor's roles jointly predict security at the 5 percent level.

F Loss of forest access

Table A10: Loss of forest access: all governance actors, enforcers of forest rules, and fear of armed takings

	Lost access to forest in the past four years because of…	
	Any reason	Armed groups or crime
Panel A: governance roles (per SD), six actors jointly		
Armed groups	2.3***	0.6*
	(0.7)	(0.3)
National state	0.0	−0.3
	(0.5)	(0.2)
Environmental authorities	0.6	−0.2
	(0.8)	(0.3)
Community action boards	0.5	0.0
	(0.6)	(0.3)
Other local organizations	0.4	−0.6***
	(0.6)	(0.2)
Municipal government	0.2	0.2
	(0.7)	(0.2)
Households	1,914	1,914
Panel B: named as enforcer of forest rules (0/1), jointly		
Armed groups	3.7***	1.6**
	(1.4)	(0.7)
Environmental authorities	3.1*	0.8
	(1.8)	(0.6)
Households	1,699	1,699
Panel C: fear of armed takings (0/1)		
Fears armed takings	3.4***	1.6**
	(1.2)	(0.6)
Households	1,714	1,714
Households with the loss	124	24
Outcome mean (%)	6.5	1.3
Veredas (clusters)	103	103

Notes: Models, outcomes, and controls as in Table 1; Panels A and C repeat its two panels and add the other four governance actors. Panel B: the household names the actor among the three most important for enforcing rules on forest use and access in the vereda; environmental authorities are the departmental protected-areas system, the three regional environmental agencies, and forest rangers. Standard errors clustered by vereda in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10 adds the other four governance actors and the enforcers of forest rules to Table 1. Governance by environmental authorities predicts no access loss. Other local organizations predict less armed-attributed loss; we did not predict it and do not interpret it. Households that name armed groups among the enforcers of forest rules report more access loss overall (3.7 points, $p = .008$) and more loss to armed groups or crime (1.6 points, $p = .026$). Naming environmental authorities predicts a similar increase in overall loss (3.1 points, $p = .086$).

G Investment horizon

As a final, exploratory analysis, we test whether tenure insecurity is associated with shorter investment horizons. The evidence here is mixed. Insecurity is not associated with less investment overall: households that fear armed takings and those under greater armed governance are no less likely to invest than their counterparts. However, the composition of investment differs. Households under greater armed governance are more likely to buy short-horizon inputs such as fertilizer, seed, and pesticide (+3.0 percentage points per SD, $p = .018$), as do households that fear armed takings (+5.5 points, $p = .030$).

About half of investment spending goes to long-horizon assets. Holding total investment spending fixed, this share falls from 50 percent to 45 percent between the 10th and 90th percentiles of armed governance (-5.5 points, $p = .024$). It also falls from 51 percent to 47 percent among households that fear armed takings, although this difference is not statistically significant (-4.1 points, $p = .12$). With vereda fixed effects, the fear gradient becomes larger and statistically significant (-5.7 points, $p = .045$), while the armed-governance estimate weakens ($p = .12$). The evidence suggests that households keep investing under insecurity but are more likely to buy short-horizon inputs.

Table A11: Insecurity and the time horizon of investment: full estimates

	Investment, past 12 months			Current land use	
	Any long-horizon	Any short-horizon	Long-horizon share of spending	Permanent crops	Planted forest
	(1)	(2)	(3)	(4)	(5)
Panel A: armed-group governance (per SD), six actors jointly					
Controls	−0.3	3.0**	−2.7**	−1.2	−0.3
	(1.2)	(1.3)	(1.2)	(1.3)	(1.3)
Vereda fixed effects	−0.5	2.4*	−2.3	−1.8	0.9
	(1.3)	(1.4)	(1.5)	(1.1)	(1.3)
Households	1,918	1,918	1,152	1,918	1,428
Panel B: fears armed takings (0/1)					
Controls	2.8	5.5**	−4.1	5.7**	−7.7**
	(2.7)	(2.5)	(2.6)	(2.5)	(3.3)
Vereda fixed effects	3.3	5.7**	−5.7**	5.8**	−5.3
	(2.9)	(2.5)	(2.8)	(2.5)	(3.4)
Households	1,717	1,717	1,037	1,717	1,278
Outcome mean (%)	43.4	45.3	48.7	54.0	28.0

Notes: Linear models; coefficients in percentage points. (1) Any investment in irrigation, permanent structures, or planted fruit, coffee, timber, or other commercial trees. (2) Any spending on fertilizer, seed, or pesticide. (3) Share of investment spending that went to the items in (1), among households that invested; holds log total spending fixed. (4) Any field under permanent or mixed crops. (5) Any field with planted forest. Columns (1)–(3) hold farm type fixed (any livestock or pasture field, any field with seasonal crops, number of fields); columns (4)–(5) hold fixed any livestock or pasture field and the number of fields. Panel A: all six governance measures enter jointly; the armed-group coefficient is shown. Panel B: the household does not rate its land very unlikely to be taken by armed groups or criminals in the next five years. All models include 11 household and sampling controls; vereda fixed effects rows add vereda indicators. Outcome mean refers to the Panel A sample. Standard errors clustered by vereda in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11 reports the investment models. Neither armed governance nor fear of armed takings predicts whether households make any long-horizon investment (column 1). Both predict spending on short-horizon inputs: 3.0 points per standard deviation of armed governance and 5.5 points for households that fear armed takings (column 2). Holding total spending fixed, armed governance predicts a smaller long-horizon share of spending (−2.7 points, $p = .024$); fear has a larger but less precise estimate (−4.1 points, $p = .12$; column 3). With vereda fixed effects, the fear estimate grows (−5.7 points, $p = .045$) and the armed estimate shrinks (−2.3 points, $p = .12$). Current land use gives mixed evidence: households that fear armed takings are more likely to hold permanent crops (5.7 points) and less likely to have planted forest (−7.7 points).