Revenue or Legitimacy?

Rebel Governance and Forest Extraction in the Philippines

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Abstract

Tropical forests are disappearing at alarming rates. Nearly 40 percent of what is left lies in regions affected by violent conflict, yet armed groups' role in forest loss remains poorly understood. We argue that rebels face a tradeoff between extracting resources from the forest and protecting it as a form of rebel governance. They can profit by shielding illegal logging, but the marginalized populations they rely on for support are often the most harmed, as deforestation erodes livelihoods, degrades ancestral lands, and exacerbates environmental hazards. Using a difference-in-differences design in the Philippines pairing village-level intelligence reports on rebel presence with satellite imagery on forest cover, we find support for this view. Rebels drive deforestation, but only in areas without Indigenous communities and far from recent landslides. For rebels, protecting the forest is profit deliberately left on the table, a price they pay only where civilians have the most to lose.

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Introduction

Deforestation poses a critical challenge to environmental sustainability, with far-reaching consequences for climate stability, biodiversity, and human livelihoods. Forest clearing releases vast amounts of stored carbon into the atmosphere, contributing significantly to greenhouse gas emissions (Alkama and Cescatti, 2016), and drives habitat destruction that threatens biodiversity (Gibson et al., 2011). Tropical forests, which harbor the majority of Earth's aboveground biodiversity and biomass, are particularly vulnerable (Vancutsem et al., 2021). Beyond these environmental impacts, deforestation accelerates soil erosion, depletes water resources, and heightens the human toll of natural disasters like floods and landslides (Bradshaw et al., 2007).

Protecting the forest is especially challenging in conflict-affected regions. Nearly 40 percent of the world's tropical forests are located in areas experiencing violent conflict (CIFOR, 2005), complicating efforts to curb deforestation. Yet the relationship between conflict and deforestation remains poorly understood. While many armed groups exploit natural resources to fund their operations (Fergusson, Romero, and Vargas, 2014; Walsh et al., 2018), other evidence suggests they may actively deter deforestation as a form of "rebel governance" (Arjona, 2017; Prem, Saavedra, and Vargas, 2020; Gilmore et al., 2025). Existing studies document both patterns but offer little guidance on when and why each occurs.

In this paper, rather than casting rebel groups as either purely extractive actors or conservationists, we argue that rebel environmental management is shaped by a tradeoff between resource extraction and maintaining legitimacy with local populations. Armed groups need both revenue and cooperation from civilians, who provide shelter, information, and social cover (Mao, 1938; Giap, 1961; U.S. Army and Marine Corps, 2007). Forests offer a lucrative source of revenue, especially in the remote, mountainous terrain where insurgents tend to operate. Rebels can capture this revenue directly, by logging or clearing forests for mining and illicit crops, or indirectly, by taxing the firms and farmers who do, all while shielding these activities from government enforcement. But the same civilians whom rebels rely on for support are often harmed by extractive forest loss, which may disrupt livelihoods, undermine community governance structures and land management practices, and heighten exposure to environmental hazards. As a result, rebels may face *legitimacy costs* from facilitating forest extraction. We predict that where legitimacy costs from deforestation are low, the revenue motive prevails and rebel control accelerates deforestation. Where they are high, rebels conserve forests.

Our argument primarily applies to population-centric asymmetric conflicts, where ground forces compete for civilian cooperation (U.S. Army and Marine Corps, 2007). It does not extend to wars where the mode of combat directly destroys the forest, as with mass defoliation and bombing in Vietnam, nor to settings where forests matter to rebels chiefly as cover from aerial surveillance (Feuer, 2023).

To evaluate the argument, we estimate a difference-in-differences model that leverages spatial and temporal variation in the presence of the New People's Army (NPA) across more than 37,000 villages in the Philippines from 2009 to 2015. Our measure of insurgent presence comes from internal military intelligence reports compiled by the Armed Forces of the Philippines (AFP) (Crost, Felter, and Johnston, 2016). We pair this with high-resolution satellite data on forest cover from Vancutsem et al. (2021) to estimate the relationship between NPA presence and forest disturbances. Overall, we find that NPA presence is associated with a measurable *increase* in forest disturbance rates. Villages the NPA enters experience a 44 percent increase in forest disturbance relative to similar villages without rebels, while those it leaves see a 54 percent reduction. We then examine two settings where forest loss is especially costly to civilians, and where our theory therefore predicts rebels will exercise restraint. The first are areas inhabited by Indigenous communities, who often maintain livelihoods, local governing institutions, and collective identities tied to forested land.[1] The second are areas near recent landslides, where the acute environmental risks of deforestation are especially salient to surrounding communities. Consistent with the legitimacy logic, we find that the deforestation effect of rebel presence disappears entirely in both settings.

This paper makes two contributions to the study of conflict and environmental governance. First, we provide a framework for understanding rebel environmental governance that parallels the political economy of deforestation literature from the state's perspective. Where that work highlights how incumbent politicians allow forest extraction in exchange for financial or political support (Sanford, 2023), this paper shows that, from a rebel perspective, the same choice creates a tradeoff between financial and community support. However, detecting this tradeoff demands a measure of *territorial control*. The few existing studies of rebel conflict and the environment instead draw largely on measures of *violent incidents* that systematically miss areas of sustained rebel presence, precisely where rebel-facilitated resource extraction is possible. In our tests, the disparity between violence-based measures and rebel presence data is stark. Fewer than one percent of barangay-years with NPA presence had a violent incident recorded in the Uppsala Conflict Data Program (UCDP) dataset, and we find no relationship between conflict events and deforestation.

Second, we advance a theory of rebel resource governance that treats extraction and restraint as competing uses of the same asset. Research on rebel rule has largely treated the services rebels provide to win civilian support and the resources they extract to finance the fight as distinct activities (Weinstein, 2007; Mampilly, 2011; Arjona, 2017). When governance and extraction are considered together, they are typically understood as complements under a stationary-bandit logic: by maintaining order, rebels raise the value of what they can tax (Olson, 1993). Recent work characterizes rebel environmental protection as a governance service that builds legitimacy and preserves the value of rebel territory, leaving open how

[1] We identify Indigenous communities using data from the National Commission on Indigenous Peoples (NCIP) and the Philippine Census (McMurry, 2022).

rebels weigh it against the revenue the forest can supply (Gilmore et al., 2025; Cunningham et al., 2025). We argue that forests break this complementarity.[2] What civilians need protected is precisely what rebels can sell, creating a tradeoff between governance and revenue. Thus, rebels limit forest loss only where it is likely to impose the greatest legitimacy costs.

A Theory of Conflict and Deforestation

Understanding how armed conflict shapes environmental outcomes requires taking seriously the strategic behavior of rebels and the local political economy in which they operate. The argument we develop is informed by fieldwork in the Philippines, including interviews with former insurgent leaders and residents of forest-dependent communities, which brought to light the forest-governance tradeoff faced by insurgents. We begin by describing that tradeoff, laying out the competing incentives that drive rebels to either exploit the forest for profit or protect it as a form of governance, and situating our account in research on rebel rule. We then argue that detecting this tradeoff requires measuring rebels' territorial *presence* rather than discrete acts of violence, reconciling the contradictory findings of existing research. Finally, we discuss conditions under which legitimacy costs constrain rebel-facilitated deforestation in this setting.

Rebel Strategy and Environmental Tradeoffs

The Profits of Forest Exploitation. Armed groups require material resources to sustain their operations and achieve their political goals. Natural resources, especially those that are easily extractable and portable, are central to rebel financing (Ross, 2004; Berman et al., 2017; Manzano and Gutiérrez, 2019). This is especially true in asymmetric insurgencies,

[2] Feuer (2023) also models forest protection as a tradeoff, but one between revenue and concealment rather than revenue and legitimacy. As noted above and discussed further in the theory section, the concealment motive is largely inoperative in the setting we study.

where rebels rarely control resources like oil fields but instead operate in remote, mountainous terrain rich in timber, minerals, and illicit crops. According to a global dataset on rebel financing, timber is the second most common source of funding among armed groups engaged in contraband markets, and more than 80 percent of rebel groups rely on logging, mining, and illicit crop production in (previously) forested areas (Walsh et al., 2018; Conrad, Reyes, and Stewart, 2022). Among asymmetric insurgencies, every group in the dataset relies on extractive activity in forested regions.

Many rebel groups do not conduct logging or mining themselves. Instead, they impose informal taxes on the industries in the territory where they operate. Insurgents may extort logging firms or establish long-term protection rackets in which they offer security in exchange for a share of the profits. In some cases, they exert control over access routes or smuggling corridors, extracting tolls at checkpoints or facilitating the illicit flow of goods. These relationships allow rebel groups to benefit from environmental exploitation without directly acquiring the capacity to engage in extractive work.

Firms involved in extractive industries may also have incentives to collaborate with non-state armed groups. First, rebels may deter state or international actors from enforcing environmental regulations. In exchange for a cut of the profits, insurgents can use the threat of violence to block inspections, sabotage enforcement campaigns, or intimidate local officials. This is particularly important when the state imposes environmental regulations that limit the scope of legal extraction available to firms. Second, rebel networks can facilitate the smuggling of timber, minerals, or other resources across borders or into black markets, reducing transaction costs and lowering the risk of confiscation. Third, when armed groups are embedded in the local social fabric, they may help firms navigate tense relationships with nearby communities by mediating disputes, co-opting local opposition to extractive activity, and drawing on their authority or local legitimacy to smooth over resistance. These arrangements can create a mutually beneficial, if sometimes tense, alliance between extractive firms and armed groups that comes at the expense of environmental preservation.

One such arrangement was described to us by Eddie Quitoriano, a former NPA front commander. Several NPA commanders had for years been independently taxing what turned out to be subsidiaries of a single conglomerate, Alcantara, until a lawyer for the firm convened the rebels and proposed a consolidated arrangement: a flat percentage of its logging revenue across all of its holdings in NPA-controlled areas, in exchange for the rebels managing local resistance, assisting with smuggling, and deterring regulators and rival firms. Far from a reluctant accommodation, the alliance was one the firm actively sought.

The Political Costs of Deforestation. Armed groups also face potential costs associated with extractive activities, because they often depend on the cooperation of local civilians in the areas where they operate. This basic logic is central both to practitioner accounts of insurgency (Mao, 1938; Giap, 1961; U.S. Army and Marine Corps, 2007) and to scholarly work emphasizing how civilians, especially through their role in information provision, shape armed actors' behavior (Kalyvas, 2006; Berman and Matanock, 2015; Sexton, 2016; Lyall, Zhou, and Imai, 2020). Although rebels may gain economically from taxing or protecting environmentally harmful industries, doing so may produce a backlash among the same local communities they depend on for support.

Forest exploitation can impose real and immediate harm on civilian populations. It accelerates soil erosion, disrupts water systems, decreases access to non-timber forest products, and increases vulnerability to disasters such as floods and landslides (Bradshaw et al., 2007). These harms fall disproportionately on poor and marginalized communities, the very populations on which asymmetric insurgencies tend to rely for shelter and social cover. In some areas, forest destruction may also violate long-standing cultural practices and land management traditions, for example, among Indigenous groups (Camacho et al., 2016; Baragwanath and Bayi, 2020). These cultural and material harms raise the political costs of extractive activity for rebels that depend on local legitimacy. Meanwhile, direct economic benefits to these communities are typically limited. Logging tends to produce few, short-lived, and low-paid jobs in extractive areas, instead benefiting those in more distant timber-processing zones (Jung et al., 2019; Ribeiro, Azevedo-Ramos, and Nascimento dos Santos, 2020).

In such contexts, rebels frequently position themselves as protectors of local populations against exploitative firms and a state they portray as complicit in the damage. Environmental protection, in this sense, becomes a deliberate form of rebel governance. However, it is a form of governance that existing theories of rebel rule are not well-equipped to explain (Loyle et al., 2023). Prior research treats governance and extraction either as separate activities or as complements, with extraction funding governance, order expanding the tax base, and taxation itself serving as a claim to authority (Arjona, 2017; Olson, 1993; Sánchez de la Sierra, 2020; Revkin, 2020; Mampilly and Thakur, 2025).[3] With forests, the two cannot be separated: governance takes the form of restraint rather than provision, placing it in direct competition with revenue. Recent work on rebel *environmental* governance has raised several of the same considerations, showing that rebels across many contemporary conflicts protect the environment to build legitimacy and preserve the value of their territory, at the expense of potential economic gains from extraction (Cunningham et al., 2025; Gilmore et al., 2025). How rebels resolve that tradeoff remains an open question.

Answering that question calls for a different emphasis from most work on rebel governance. Much of that work takes the group as the unit of analysis and explains how and why rebels govern as a function of their aims, resources, and ties to constituencies (Weinstein, 2007; Staniland, 2014; Jo, 2015; Stewart, 2018, 2021; Huang and Sullivan, 2021). This approach is well-suited to the types of governance the field has studied most closely, namely the provision of services like clinics, schools, and courts, which require substantial resources and administrative capacity. Forest preservation, by contrast, is a relatively "cheap" form

[3] Arjona (2017), for example, counts regulation of fishing, hunting, and logging as governance but excludes the regulation of coca and illegal mining because it involves taxation, leaving no category for a resource that does both.

of governance: groups that lack the capacity to provide services at scale can still restrict logging through coercion. But this protection comes at the direct cost of forgone revenue, which may be particularly constraining for groups with few alternative sources of financing. Such groups may be able to protect the forest in some places but not in all of them. Thus, forest protection poses an allocation problem within rebel territory, necessitating a focus on local-level variation.

Existing studies of variation within a single group's territory tend to focus on features like the civilian community's capacity to resist or demand governance (Arjona, 2017; Kaplan, 2017; Rubin, 2020; van Baalen, 2021; Breslawski, 2021; Mampilly and Stewart, 2021). Again, this focus on community capacity makes sense for conventional service provision, which is demanded by communities more or less everywhere. In the case of forests, however, communities also vary in how much they value protection relative to extraction. The question our account must answer, therefore, is where civilian support for rebels most depends on what happens to the forest. We take that up in the discussion of legitimacy costs below.

Before turning there, we address a second cost of deforestation, featured prominently in existing accounts: the loss of forest cover that conceals insurgents from government forces (Fearon and Laitin, 2003; Feuer, 2023). In some settings, forest canopy matters as cover where insurgents fear detection from the air, as the Taliban did in Afghanistan or the FARC did in Colombia (Morgan, 2021; Feuer, 2023), or where industrial-scale clearing would fully displace rebel operating areas. Neither describes the NPA conflict. During the period we study, the AFP relied on ground-centric counterinsurgency rather than extensive aerial bombardment or defoliation, as is typical of many irregular wars (Kalyvas and Balcells, 2010; Burgess, Miguel, and Stanton, 2015). Concealment thus depended more on blending into the population than on canopy cover. Dense forests may still offer some tactical advantages in this type of setting, but this does not preclude legitimacy concerns from independently shaping rebels' decisions regarding forest extraction, nor do we expect the benefits of concealment to vary systematically with our measure of legitimacy costs.[4]

Detecting the Tradeoff: Rebel Presence, Not Violence

The tradeoff we have described, together with the distinction between violence and rebel presence, may help explain why the few existing studies of conflict and deforestation reach opposite conclusions. Studies by Fergusson, Romero, and Vargas (2014), Butsic et al. (2015), and Liévano-Latorre, Brum, and Loyola (2021) find that conflict *intensifies* deforestation in Colombia and the DRC. Meanwhile, Prem, Saavedra, and Vargas (2020) and Burgess, Miguel, and Stanton (2015) find that forest loss is *reduced* in areas experiencing conflict in Colombia and Sierra Leone.

These contradictory findings have several possible sources. First, studies that theorize the activities of armed non-state actors tend to foreground one direction of the relationship. Some argue that armed groups intensify deforestation by taxing logging operations, facilitating smuggling routes, or deterring state regulation (Fergusson, Romero, and Vargas, 2014; Butsic et al., 2015), while others highlight their role in displacing land-intensive economic activity that would otherwise result in deforestation (Prem, Saavedra, and Vargas, 2020; Burgess, Miguel, and Stanton, 2015). However, few directly consider how rebels navigate competing incentives differently across local contexts within the same conflict.

Another issue is how conflict is conceptualized and measured. Some studies emphasize violence, insecurity, and disruption as mechanisms through which conflict affects forest cover (Butsic et al., 2015; Burgess, Miguel, and Stanton, 2015), while others focus on rebel territorial influence (Fergusson, Romero, and Vargas, 2014; Prem, Saavedra, and Vargas, 2020; Liévano-Latorre, Brum, and Loyola, 2021). However, even studies in the second group

[4] The other explanation for lower deforestation under conflict, that fighting itself displaces farmers, loggers, and investors (Burgess, Miguel, and Stanton, 2015), concerns violence rather than rebel governance, a distinction we discuss below.

typically measure rebel presence using recorded incidents of *violence*, such as battle events or attacks on civilians.[5] While violence is a central feature of conflict, it is often rare in areas where rebel presence is strongest (Kalyvas, 2006). These are precisely the areas where armed groups may shift from active combat toward taxation, resource extraction, and attempts to govern civilian populations. The mechanisms we theorize suggest that rebel *presence*, not just violence, drives environmental outcomes. By relying on incident-based measures, existing research may misclassify areas with higher levels of rebel control or influence, obscuring important ways in which rebel groups shape forest loss.

The tradeoff we describe yields no single directional prediction. Whether revenue motives or legitimacy costs prevail on average is an open question, and the competing accounts above effectively stake out the two answers. Because we measure rebel presence directly rather than inferring it from violence, our data can put the two accounts on equal footing. We therefore begin by examining the average relationship between rebel presence and deforestation:

H1a: Rebel presence increases deforestation.

H1b: Rebel presence decreases deforestation.

When Legitimacy Costs Deter Rebel Exploitation of Forests

If rebel-facilitated deforestation reflects a tradeoff between resource extraction and civilian legitimacy, then observed patterns of forest loss should vary depending on how local communities experience the consequences of environmental degradation. In our account, civilians need not uniformly oppose forest exploitation. Indeed, where the state heavily regulates forest use, communities may welcome extraction that brings employment or access to informal economies (Peluso, 1992), and rebels attentive to legitimacy may permit some extraction

[5] Liévano-Latorre, Brum, and Loyola (2021) use a measure of FARC presence, but not in a way that captures variation over time.

while limiting its most severe harms. But where deforestation generates serious harm to civilians, the legitimacy costs are more likely to outweigh the financial gains, and armed groups will face stronger incentives to preserve forest cover. Two factors that increase the legitimacy costs of deforestation are particularly important: the degree to which forest loss disrupts existing community institutions and livelihoods, and the extent to which it exposes civilians to acute environmental harm.

Indigenous Land and Environmental Protection. The legitimacy costs of facilitating deforestation may be especially high in Indigenous communities, where forests are often deeply embedded in livelihoods, territorial claims, community governance systems, and cultural practices (Camacho et al., 2016). In the Philippines, many Indigenous communities view large-scale logging, mining, and other extractive activities that result in deforestation not only as economic threats but as direct violations of their customary laws, norms, and territorial claims. These concerns are exacerbated by long histories of marginalization and exploitation by states and extractive firms in Indigenous territories. As in many conflict-affected countries, Indigenous territories in the Philippines have been targeted for commercial logging and mining, often with little or no compensation for affected communities (Human Rights Watch, 1996). Accordingly, Indigenous Peoples (IP) communities have been at the forefront of environmental activism, including mobilization against large-scale extractive and infrastructure projects, and these efforts have often been met with violent responses by the military (Delina, 2020).[6]

These experiences generate grievances that rebels may seek to exploit by positioning themselves as protectors of Indigenous land and autonomy. In some cases, including the NPA in the Philippines, the Zapatista Army of National Liberation (EZLN) in Mexico, and the Naxalites in India, such protection has been explicitly incorporated into broader

[6] In the Philippines, Indigenous communities are often collectively referred to as IPs or IPs/ICCs, acronyms for Indigenous Peoples (IPs) and Indigenous Cultural Communities (ICCs).

Maoist liberation movements (Feuer, 2023; Mongaya, 2025). Illustrating this, the National Democratic Front of the Philippines, the NPA's political wing, includes "upholding the rights of indigenous peoples to self-determination and democracy" among the twelve points of its political platform and regularly issues statements condemning state violence against Indigenous environmental defenders (National Democratic Front of the Philippines, n.d., 2025). This explicit support for Indigenous self-determination is particularly striking given the tensions between territorially bounded Indigenous land rights and important elements of classical Marxist ideology and agrarian reform proposals. Its adoption by the NDFP and similar Maoist movements may therefore reflect strategic adaptation to secure support in remote Indigenous territories (Abinales, 2000; International Crisis Group, 2011).

Quitoriano, in the interview introduced on page 6, also drew this contrast. Having described the consolidated logging deal with Alcantara as a major source of revenue, he recounted a very different relationship with the same firm in an area where the Higaonon Indigenous community lived. There, the NPA seized on the community's longstanding grievances generated by Alcantara's operations, which violated local land governance practices. In this case, the NPA backed the community's grassroots resistance movement against the firm before brokering an arrangement that scaled back the operations and delivered a share of the proceeds for the community.

We therefore expect the NPA to facilitate deforestation to a lesser extent in Indigenous areas. Even when rebels do not fully block resource extraction, they may limit its scale. In this way, forest preservation, or at least restraint in its exploitation, can serve as a strategy of legitimacy-building and governance.

> **H2:** Rebel presence results in less deforestation in Indigenous areas compared to non-Indigenous areas.

Disaster Salience and Civilian Safety. A second constraint on rebel-backed deforestation arises from the tangible risks that forest loss poses to civilian safety and welfare. Where forests are cleared, landslides, floods, and soil erosion often follow, and these dangers fall disproportionately on poor and geographically isolated communities, the same populations on which insurgents often rely for support.

Whether these risks translate into legitimacy costs for rebels depends on whether communities connect forest loss to the harms they experience. That connection is clearest where a community has lived through a disaster. A landslide makes the consequences of deforestation concrete and locally visible, and it makes blame possible. In the Philippines, major landslides have heightened public awareness of the environmental consequences of deforestation. Where that awareness is acute, even rebel groups primarily motivated by profit face reputational costs from being blamed for the next disaster, and the legitimacy costs of facilitating environmental harm are more likely to outweigh the financial benefits of extraction.

> **H3:** Rebel presence results in less deforestation in communities where the environmental harms of forest loss are salient compared to communities where they are not.

Context and Data

The Philippines offers an informative case for understanding how rebel presence shapes forest loss. Like other countries in Southeast Asia, the Philippines has lost significant forest cover in recent decades (Food and Agriculture Organization of the United Nations, 2020; Estoque et al., 2019). The Philippines is also home to one of the world's longest-running Maoist insurgencies, the New People's Army (NPA). Our combined dataset assembles annual, village-level measures across 1,263 municipalities and 37,694 rural barangays (villages) for the 2009–2015 period.

Outcome Data: Forest Loss

Forest loss in the Philippines is driven primarily by small-scale activity, in contrast to the large-scale commercial conversion that dominates elsewhere in Southeast Asia. In Indonesia and Malaysia, for example, conversion of moist forests to oil palm and rubber plantations accounts for the bulk of forest loss (Vancutsem et al., 2021). In the Philippines, forest loss instead occurs primarily through localized commercial logging operations, forest clearing for mining and infrastructure expansion, and *kaingin*, a traditional form of slash-and-burn agriculture (Gabriel et al., 2025). A nationwide logging ban took effect in 2011, but illegal logging and mining have continued at high rates (Gabriel et al., 2025; Barit, Choi, and Ko, 2022).

Our main outcome is *forest disturbance*, measured using annual classifications of tropical moist forest cover recorded at 30-meter pixels from the Landsat satellite archive (Vancutsem et al., 2021).[7] Because satellite imagery does not rely on self-reported statistics, it is resistant to manipulation by actors with incentives to obscure forest loss. The dataset distinguishes two types of forest loss by duration. *Degradation*, canopy disruptions lasting 2.5 years or less, corresponds in the Philippines mainly to selective logging and charcoal production.[8] *Deforestation* captures permanent land-use conversion to facilitate activities like mining, agriculture, or infrastructure construction. Our main outcome pools the two, because activities that degrade the forest (like selective logging) or permanently convert it to other uses (like mining) can both generate revenue for the NPA and impose the civilian harms central to our theory.[9]

[7] We use the Vancutsem dataset rather than Hansen et al. (2013) because it is specifically designed to capture disturbances in closed-canopy humid tropical forests like those found in the Philippines (Vancutsem et al., 2021; Bourgoin et al., 2024).

[8] Degradation also captures natural disruptions like wildfires or storm damage. Because Philippine counterinsurgency does not involve intentional forest burning to expose insurgents, we treat this as background noise.

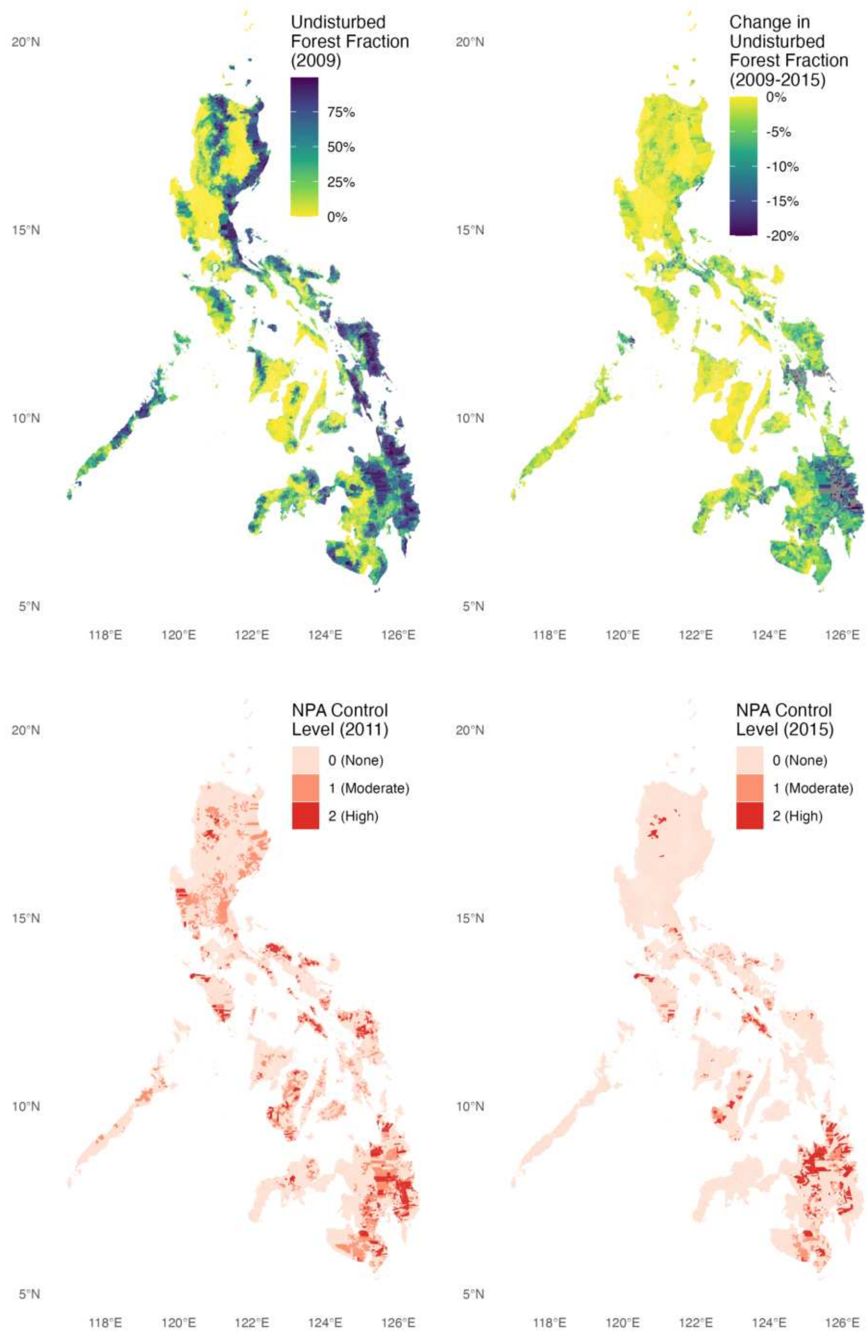
[9] The classification of forest disturbance as degradation vs. deforestation is retrospective: a pixel that ends up deforested is coded that way from the start of the event rather than being coded as degraded for the first 2.5 years. In the results section, we show results disaggregated by

A pixel-year is coded as 1 if it transitions from undisturbed to disturbed and 0 otherwise.[10] The pixel-level binary outcome can then be aggregated to larger spatial units like barangays, where the annual rate of forest disturbance is measured as the share of pixels transitioning from undisturbed to disturbed in a given year. The top panel of Figure 1 visualizes baseline forest cover and forest disturbance rates during our study period. Additional descriptive statistics, maps, and treatment-timing visualizations appear in Appendix A1.

type of disturbance and address alternative explanations related to state-driven sources of forest disturbances in conflict zones like infrastructure development.

[10]The 0 category includes pixels that remain undisturbed and those already disturbed. Using year-to-year *change* rather than the *level* of forest cover is standard in this literature, though it raises concerns about differences in baseline forest cover and mechanical floor effects, which we address in our robustness analyses below.

Figure 1. Forest cover, forest disturbance, and NPA presence.



Note: The top left panel shows undisturbed forest as a percent of barangay area in 2009, the year in which our analysis begins, from Vancutsem et al. (2021). The top right panel shows rates of forest disturbance during our panel. The bottom two panels show NPA presence in 2011 and 2015. These years represent the height and nadir of NPA presence during our study period.

Main Explanatory Variable: Rebel Presence

This study focuses on the New People's Army (NPA), one of two long-running insurgencies that began in the Philippines in the late 1960s.[11] A traditional Maoist insurgency, the NPA operates primarily in remote, forested, and economically marginalized areas. The period covered by our study, 2009–2015, includes the height of the group's post–Cold War resurgence and its subsequent decline. In 2011, the Armed Forces of the Philippines (AFP) assessed that the NPA was active in nearly 5,500 villages, roughly 15 percent of the country's rural barangays, with operations spanning all major island groups. By 2015, its territorial reach had declined sharply, to approximately 2,000 villages.

The NPA is precisely the type of insurgency our theory describes, one that depends heavily on locally extracted revenue and lacks the resources to govern through public goods provision. Receiving little foreign support, the group relies on revolutionary taxes levied on businesses operating in its territory, particularly logging, mining, and construction firms.[12] Rather than delivering services at scale, the NPA signals responsiveness to local grievances through coercive means: mediating disputes between landlords and tenants, operating alternative justice mechanisms, and intimidating firms involved in resource extraction.[13]

We measure rebel presence using village-level data from internal military intelligence reports provided by the AFP. Each year, designated military intelligence officers at AFP bases score each barangay in their area of operations on a three-point scale: 0 (clear), 1 (threatened), and 2 (influenced) (Crost, Felter, and Johnston, 2016; Haim, Ravanilla, and Sexton, 2021). Our data cover each year from 2009 through 2015. Barangays coded as

[11]The other involves Moro separatist groups that operate primarily in five provinces of what is now the Bangsamoro Autonomous Region in Muslim Mindanao (BARMM). These provinces are excluded from our analysis because they are not included in the AFP intelligence data.

[12]For example, Human Rights Watch documented loggers who refused the NPA's taxes finding "their equipment burned, employees harassed, and executives threatened or killed" (Human Rights Watch, 1996).

[13]In some areas, the NPA also provides limited medical services or livelihood support (Rubin, 2020).

"influenced" are those where the NPA operates regularly and maintains an active political organization.[14] "Threatened" barangays are areas where rebels travel and interact with civilians without establishing permanent structures. Our presence-based measure addresses the limitation of the violence-based proxies used in most prior work. In our dataset, fewer than one percent of barangay-years with NPA presence recorded a UCDP incident, so violence captures almost none of the territory where the NPA operates.

In our main analyses, we collapse the three-point scale into a binary indicator for any NPA presence (influenced or threatened) versus none, primarily because several of our difference-in-differences specifications require a binary treatment. Supplementary analyses using the three-level coding point in the same direction at both levels of NPA presence, but the effects are larger where NPA presence is stronger (Appendix A4.6, Table A4.7). This is what the revenue logic predicts, because in influenced villages the NPA can tax firms with greater impunity. In threatened villages, it can still credibly threaten firms that defy it, but must operate more carefully.

Two factors mitigate concerns about the reliability of military-coded data. First, the assessments are intended for internal planning rather than public dissemination, reducing incentives for officers to systematically over- or under-state rebel activity. Second, historical trends in these data reflect both gains and losses for the military, weighing against the idea that they are systematically biased toward flattering AFP performance. From 1995 to 2008, for instance, the total number of NPA-affected villages steadily increased even as the AFP conducted active suppression campaigns (Felter, 2006).[15] The pattern between 2009 and 2015, the period covered by our barangay-level data, also reflects both expansion and

[14]The NPA conflict remains highly asymmetric, and even "influenced" villages, the highest level of the scale, are not areas of exclusive rebel control as in conventional civil wars (Kalyvas and Balcells, 2010). Civilian officials do travel to these areas, but often require military escorts to operate, and residents rarely interact with government personnel (Haim, Ravanilla, and Sexton, 2021).

[15]This analysis in Felter (2006) uses nationally aggregated AFP data.

contraction of NPA territory across regions, providing the spatial and temporal variation that our identification strategy requires.

The bottom panels of Figure 1 display NPA presence in 2011, the height of the group's resurgence in our window, and in 2015, its nadir. During this period, the AFP successfully expelled the NPA from many villages while the rebels gained ground in others, generating within-unit changes in treatment status in both directions. Cross-sectionally, NPA presence is concentrated in mountainous, forested areas, where rugged terrain offers protection from military operations and poorer populations are more receptive to NPA mobilization (see Appendix Figure A1.1).

Moderating Variables: Legitimacy Costs of Deforestation

As discussed above, Indigenous communities in the Philippines have disproportionately high stakes in preserving forests, due to the important role forests play in livelihoods, customary governance institutions, and collective identity, and a history of land dispossession and extractive targeting. Our main measure of Indigenous presence is a barangay-level binary variable, coded 1 if a community recognized as Indigenous Peoples or Indigenous Cultural Communities (IPs/ICCs) by the government is located in a given barangay. More specifically, it captures whether a barangay falls within the territory of a Certificate of Ancestral Domain Title (CADT), contains a community that is part of an in-process CADT application, or contains a community that is considered by the government to be eligible to apply for a CADT. CADTs are collective titles to Indigenous ancestral lands, granted under the 1997 Indigenous Peoples' Rights Act (IPRA). While the opportunity for collective titling applies to IPs/ICCs throughout the country, its implementation to date has been uneven.[16] The underlying data are compiled by McMurry (2022) using records from the Philippines' National Commission

[16]To secure a CADT, communities must provide extensive evidence of historical occupancy, customary law, and governance institutions, followed by a government-verified land survey. As of 2018, more than 220 CADTs had been granted, covering roughly 18 percent of national land area.

on Indigenous Peoples. We use this broader eligibility criterion because our interest is in the presence of Indigenous communities rather than state-recognized collective property rights.[17] In supplementary analyses, we separately examine the narrower CADT-titled subset and use data from the 2010 Census to capture the share of each barangay's population identifying as a member of an Indigenous ethnic group. The results hold across these different measures.

A second source of legitimacy costs comes from the physical risks that forest loss poses to civilian safety in disaster-prone areas. The Philippines is among the world's most disaster-prone countries: vulnerability to typhoons, combined with deforestation and steep terrain, makes deadly floods and landslides a recurring feature of rural life. Major disasters like the 2011 Compostela Valley landslide and the 2006 Southern Leyte landslide, which killed more than a thousand people, have heightened public awareness of the link between forest loss and catastrophic outcomes. Landslide prevention was a central concern cited in the 2011 national logging ban (Office of the President of the Philippines, 2011; The Philippine Star, 2011).

In communities that have experienced landslides directly or are in the vicinity of locations where these events occurred, the risks of facilitating forest loss are salient and concrete. We capture this variation using data on historical landslide occurrence from the NASA Global Landslide Catalog, which has compiled landslide events since 2007 (Kirschbaum et al., 2010; Kirschbaum, Stanley, and Zhou, 2015). The catalog combines media reports, disaster databases, scientific papers, and other text sources with automated detection from remote sensing imagery. Our main landslide measure is the distance to the nearest large recorded landslide event in the Kirschbaum, Stanley, and Zhou (2015) data. Barangays closer to

[17] There is a growing literature on the relationship between Indigenous property rights and deforestation (BenYishay et al., 2017; Baragwanath and Bayi, 2020; Camino et al., 2023). Because we theorize the relationship between non-state armed actors and IP communities, our analysis does not focus on state-guaranteed property rights. By definition, areas of NPA influence are areas where the state does not exert full territorial control, meaning enforcement of these rights may be limited. However, property rights may shape deforestation and rebel interactions in other ways, a point we return to in the conclusion.

past landslide events face both higher landslide risk and heightened public sensitivity to forest-related disasters, raising the costs of facilitating forest disturbance in those areas.

Research Design

We test our hypotheses with a difference-in-differences (DiD) design that uses within-village changes in NPA presence over time. The design addresses a key inferential challenge in studying the relationship between rebel presence and deforestation: villages where rebels operate differ systematically from other villages on dimensions such as mountainous terrain, land area, and distance from city centers. These same characteristics shape forest cover and the potential for forest disturbances. Thus, a simple comparison across villages would risk conflating the effect of rebel presence with these underlying differences. By focusing on within-village changes, we hold these static factors constant and ask what happens to a village's forests after the NPA gains or loses territorial control there. Moreover, the DiD approach addresses concerns about reverse causation. Existing studies argue that forest cover influences rebel presence by providing concealment and resources critical to sustaining insurgencies (Fearon and Laitin, 2003; Chow and Han, 2024). Our design allows us to observe the temporal sequencing of NPA entry and exit and forest loss. Treatment moves in both directions over the panel: more villages lost NPA presence than gained it, but more than 1,000 barangay-years saw NPA entry.

We conduct our primary analysis at the point level, using a regular grid of about 102,000 points spaced roughly 1.6 km apart across the AFP coverage area. We use points rather than barangays in the main specifications because forest disturbance is a land-area phenomenon, and points weight areas equally while barangays vary substantially in size. Each point inherits the NPA presence value of the encompassing barangay and the forest-disturbance value of the pixel it falls within. A barangay-level panel, which weights each barangay equally regardless of area, answers the complementary question of what happens to village-wide forest cover under NPA presence. We report it alongside the point-level results in the main table, and all main results are robust to barangay-level estimation in the appendix.

Our main analyses use a two-way fixed effects (TWFE) regression. The treatment is a binary indicator of NPA presence: D_{it} equals 1 if the AFP coded unit i's barangay as either threatened or influenced in year t, and 0 if the barangay was coded as clear. The estimating equation is:

$$Y_{it} = \tau \, D_{it} + \alpha_i + \xi_t + \mathbf{X}'_{it}\beta + \varepsilon_{it}, \tag{1}$$

where Y_{it} is new forest disturbance at unit i in year t in percentage points (an indicator that the point's pixel is newly disturbed at the point level; the share of barangay area newly disturbed at the barangay level), α_i and ξ_t are unit and year fixed effects, $\mathbf{X}_{it}$ is a vector of time-varying covariates, and ε_{it} is an error term. τ captures the average within-unit change associated with NPA presence, pooling entry and exit effects. We cluster standard errors at the barangay level. For the legitimacy-cost hypotheses, we extend the same specification with an interaction between D_{it} and a moderator Z_i, where Z_i is either a binary indicator for whether the encompassing barangay contains an Indigenous community, or the mean-centered log distance from the point to the nearest recorded large landslide event. The interaction coefficient captures how the NPA effect on forest disturbance varies with the moderator.

TWFE has known limitations in settings like ours. With staggered treatment timing and treatment reversal, TWFE can recover biased average treatment effects when treatment effects are heterogeneous across units or over time (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020). These concerns are especially salient here because treatment is non-absorbing: the panel contains both staggered entries into and exits from NPA presence, and the processes generating entry may differ from those generating departure. As an alternative specification, we re-estimate the main effect using the FEct estimator proposed by Liu, Wang, and Xu (2024), which is numerically equivalent to the imputation estimator described by Borusyak, Jaravel, and Spiess (2024). FEct avoids "forbidden comparisons" by estimating the untreated potential-outcome model using only untreated observations and then imputing counterfactual untreated outcomes for treated observations (Liu et al., 2025). This construction also avoids the negative-weight problem associated with conventional TWFE and naturally accommodates non-absorbing treatment patterns in which units may enter and subsequently exit treatment. FEct therefore allows us to estimate treatment effects separately around transitions into and out of NPA presence, which we report alongside the pooled TWFE estimate in the main table. We interpret these estimates as the effects associated with NPA *entry* and NPA *departure*, respectively. We have no strong theoretical reason to expect these transitions to differ in magnitude, although their signs should reverse if the effect of NPA presence is symmetric. Reporting them separately, therefore, provides a useful diagnostic for whether the pooled estimate is driven disproportionately by one type of treatment transition.

We nonetheless adopt TWFE as our main specification for two reasons. First, in all the specifications where we can produce both TWFE and FEct estimates, the results converge in direction and magnitude, indicating that the modeling strategy is not driving the substantive conclusions. Second, FEct does not currently accommodate interaction terms. The imputation step estimates $Y_{it}(0)$ from not-yet-treated units, which works for a binary treatment but breaks down when potential outcomes are indexed by a moderator with multiple or continuous values.[18] Using TWFE throughout, therefore, keeps our main heterogeneity and robustness estimates on the same footing and simplifies interpretation.

Our design rests on two identifying assumptions. Parallel trends requires that, absent a change in NPA presence, treated units would have followed the same disturbance trends as the comparison units; the main threat is strategic timing, where NPA entry or exit

[18]As an informal test, we subset our data based on binary versions of our moderating variables and estimate separate FEct specifications in each subset. The results of these analyses are consistent with those we obtain using the TWFE with heterogeneous treatment effects.

responds to something that independently affects deforestation. SUTVA requires that a unit's outcome not depend on other units' treatment, and it would fail if NPA presence in neighboring barangays shifted logging or state activity into a focal barangay. We assess both below, with event-study estimates and matched samples for the first, and direct estimates of neighboring NPA changes for the second, and we examine state responses to NPA entry as an alternative mechanism in the same section.

Results

Consistent with H1a, when averaging across all barangays, NPA presence *increases* forest disturbance. The baseline TWFE specification at the point level (Table 1, Model 1) estimates that NPA presence raises annual forest disturbance by 0.2 percentage points on average, a 34 percent increase over the 0.59 percent baseline disturbance rate.[19] The barangay-level analysis, which weights each village equally regardless of area, recovers the same pattern at slightly smaller magnitude: an additional 0.16 percentage points, or 31 percent above the 0.52 percent barangay baseline (Table 1, Model 4).

Separating NPA entry and exit using the FEct estimator confirms that NPA presence is associated with increased forest disturbance on both sides of the transition, with a moderate asymmetry in magnitude (Table 1, Models 2 and 3; Figure 2). NPA entry is associated with an additional 0.25 percentage points of forest disturbance per year, a 44 percent increase over the 0.57 percent baseline in the entry sample, while NPA exit is associated with a 0.38 percentage-point reduction in disturbance per year, a 54 percent reduction from the 0.70 percent baseline in the exit sample. Figure 2 depicts dynamic effects of both NPA entry (blue circles) and NPA exit (red triangles), showing the acceleration of effects in the years following both entry and exit.

[19] This estimate combines the average effect of NPA arrival and the average effect of NPA departure into a single coefficient, since the binary indicator changes in both directions over the panel.

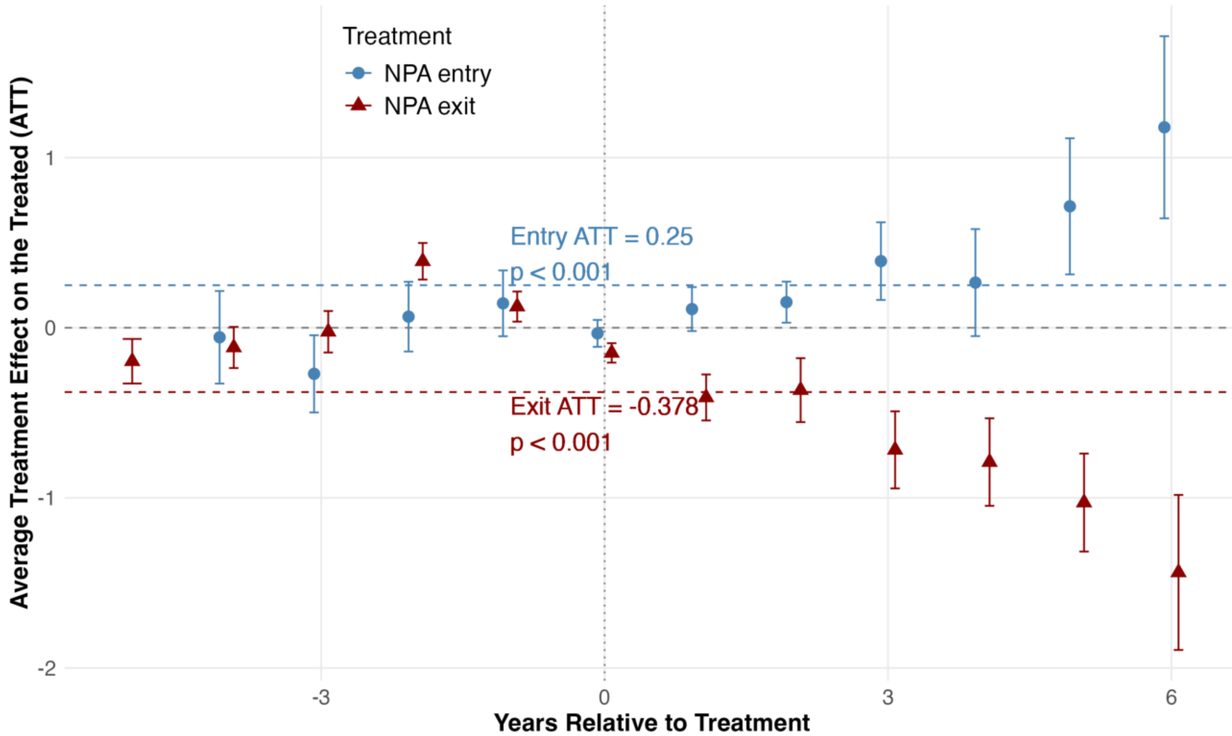
Table 1. NPA entry raises annual forest disturbance; NPA exit reduces it.

	Dependent variable: new forest disturbance in year t (percentage points)					
	Point level			Barangay level		
	(1) Presence	(2) Entry	(3) Exit	(4) Presence	(5) Entry	(6) Exit
	$\mathbf{H_1}$			$\mathbf{H_1}$		
NPA Effect	0.202***	0.250***	−0.378***	0.165***	0.143***	−0.331***
	(0.047)	(0.054)	(0.058)	(0.016)	(0.022)	(0.025)
Outcome Mean	0.588	0.569	0.701	0.525	0.519	0.603
Observations	712,341	686,266	247,506	263,858	259,966	48,153
Treated Units	35,358	31,633	31,633	6,879	6,323	6,323
Estimator	TWFE	FEct	FEct	TWFE	FEct	FEct

Note: At the point level the outcome is an indicator that the point's pixel is newly disturbed that year, scaled to percentage points; at the barangay level it is the share of barangay area newly disturbed. Presence columns report the TWFE coefficient on a binary NPA-presence indicator, which pools the effects of NPA arrival and departure, with treated units being those ever under NPA presence; Entry and Exit columns report the FEct average treatment effect of each transition separately, with treated units being those that experience it. All models include unit and year fixed effects. Standard errors clustered at the barangay level in parentheses. $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Figure 3 presents results for different types of forest disturbances: shorter-term forest *degradation* and more permanent *deforestation*. NPA presence appears to drive forest loss along both margins of extraction, rather than operating through a single dominant mode. NPA entry is associated with an additional 0.15 percentage points of *deforestation* per year (Figure 3, top panel, $p < 0.01$) and 0.10 percentage points of *degradation* (bottom panel, marginal at $p = 0.08$). On the exit side, the larger effect is on degradation (−0.30 percentage points per year, $p < 0.01$), with a smaller but still significant effect on deforestation (−0.09 percentage points, $p = 0.02$). While several individual years in the deforestation panel are not distinguishable from zero on their own, the reported ATTs pool across all post-treatment years, and the pooled estimates are significant. Substantively, we interpret this pattern as consistent with the NPA facilitating both the small-scale illegal logging and charcoaling that drive degradation and the larger-scale conversions, particularly to mining, that drive permanent deforestation.

Figure 2. NPA entry increases forest disturbance; NPA exit reduces it.

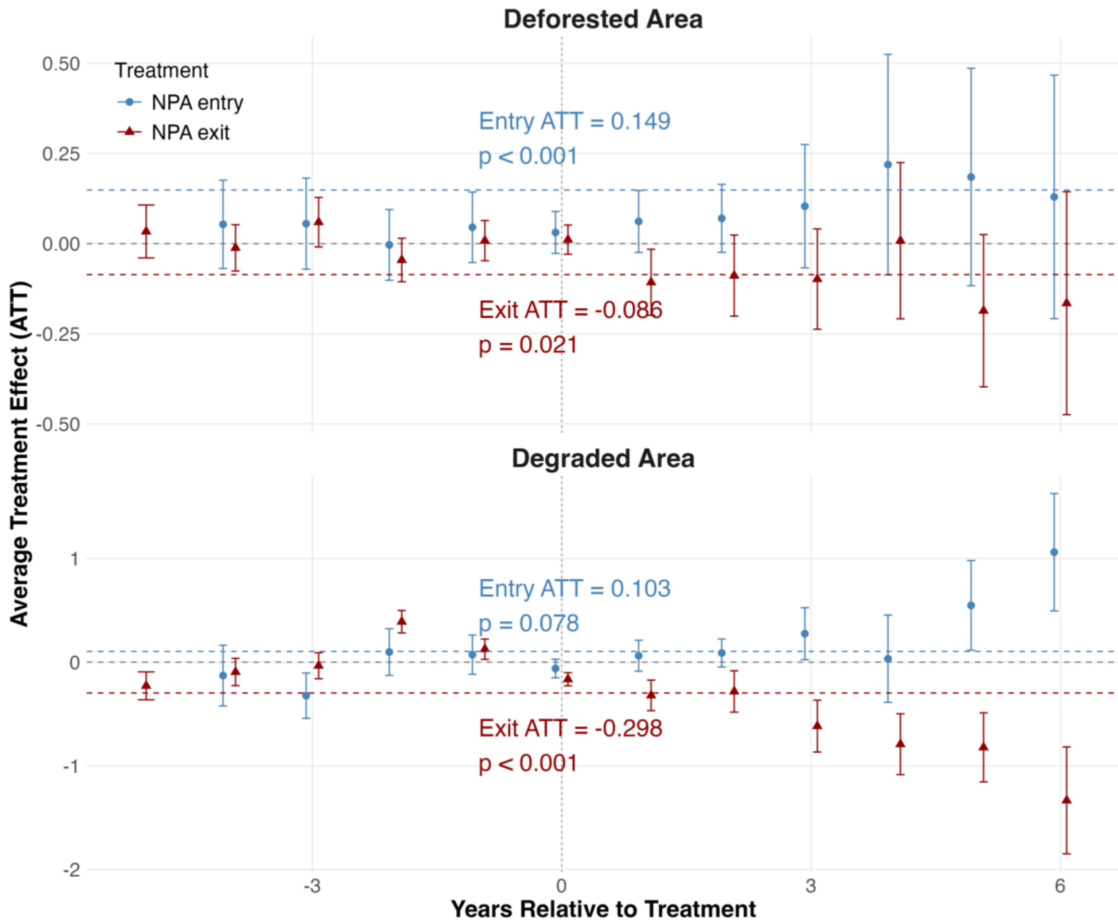


Note: Event-study estimates from the FEct estimator. The x-axis is years relative to treatment; the y-axis is the average treatment effect on annual forest disturbance, in percentage points. Blue circles show the effect of NPA entry; red triangles show the effect of NPA exit. The vertical dashed line marks the year of transition. The pooled ATTs annotated on the plot are the Entry and Exit estimates in Table 1, Models 2 and 3.

The dynamic estimates also raise concerns about parallel trends for NPA exit specifically: forest disturbance rises in the two years before exit, with no comparable pre-trend for entry. As shown in Figure 3, this is driven by short-term degradation rather than more permanent deforestation. We consider the implications of this finding for our analysis in greater detail in the next section.

These main-effect patterns survive alternative estimators and operationalizations. A traditional TWFE event-study specification recovers the same dynamic pattern (Appendix A2.1). The results also hold at the barangay level (Appendix A4.7), and when using the three-category NPA presence variable in the TWFE estimation (Appendix A4.6). To account for spatial correlation, we repeat our analyses using Conley standard errors;

Figure 3. NPA presence affects both permanent deforestation (top) and shorter-duration degradation (bottom).



Note: Event-study estimates by disturbance type. Top panel: deforestation (permanent conversion). Bottom panel: degradation (selective logging, charcoaling, and other short-duration disruptions). Conventions as in Figure 2.

the coefficient on NPA presence remains highly statistically significant (Appendix A3).[20] A spatial autoregressive model estimated on a random subsample of points, which models spatial dependence in forest loss directly, yields a similar estimated effect of NPA presence to that from a TWFE model estimated on the same subsample (Appendix A3.3). Finally, we

[20]We choose an optimal bandwidth for Conley standard errors using a spatial variogram and report these results in Table A3.1 (Lehner, 2026). Our main effects and heterogeneity analyses (described below) both remain robust. We also report Conley standard errors for a variety of other bandwidths in Table A3.2.

conduct a randomization inference procedure that reassigns observed NPA presence histories across barangays while preserving the timing and duration of treatment episodes. The observed estimate lies at the extreme tail end of the placebo distribution, suggesting that the relationship we estimate is unlikely to arise from the particular spatial configuration of treatment histories in the sample (Appendix A2.6).

The choice of how to measure conflict, on the other hand, matters enormously. If we operationalize NPA activity the way most prior work does, through recorded violent incidents rather than territorial presence, the relationship to forest loss disappears. Replacing NPA presence measured in AFP intelligence reports with the occurrence of a violent incident in the UCDP dataset yields null effects on forest disturbance, with or without lags of the incident measure (Table A4.1). We find similar null results using data from the Bangsamoro Conflict Monitoring System (BCMS) on violent incidents relating to the NPA conflict (Table A4.2).[21] Sustained NPA presence captures the rebel behavior that drives forest disturbance in our context, while incident-based measures capture moments of active conflict that are largely uncorrelated with the forest loss we observe.

Main Effects: Robustness and Alternative Explanations

Before turning to the legitimacy-cost hypotheses (H2 and H3), we address several threats to inference in our analysis of the main effects. We begin with two concerns about what the outcome measure captures, then consider whether strategic targeting by the NPA or state responses to NPA presence could be driving the pooled estimate. We then take up the pre-exit anticipation pattern, which is both a threat to the exit ATT and, on closer inspection, evidence for the legitimacy mechanism.

[21] BCMS covers only the Davao Region, but draws from both police reports and local news sources, so it records many more incidents in a single region than UCDP records nationwide during our panel.

The first concern is that the NPA tends to operate in heavily forested areas, so our estimates could reflect differences in how much forest a unit has available to lose rather than rebel behavior. However, unit fixed effects hold time-invariant differences in forest cover constant, and our results hold when we match treated and untreated units on 2009 forest cover (described below). The second is a possible floor effect: because our outcome captures change rather than levels of forest cover, a reduction in disturbance upon NPA exit could reflect the NPA leaving areas that are no longer forested and where no further disturbance is possible. As Figure A1.4 shows, however, levels of forest cover in years of NPA exit vary widely, and the distribution is similar in exit and entry years.

A different alternative explanation is that the NPA strategically chooses areas to operate based on extractive potential. The static version of this story, that the NPA targets locations with high natural-resource potential, is largely addressed by our unit fixed effects, which hold constant time-invariant features like terrain, baseline forest cover, or access to rivers for moving logs. One dynamic version is that the NPA observes logging once it begins and moves in to extract revenue. However, this would likely generate rising pre-trends in the entry event study, which we do not see in our data. A second version is harder to dismiss: the NPA arrives synchronously with a planned logging operation, either anticipating extractive opportunity in the area or coordinating directly with a firm, such that disturbances from logging and NPA presence both begin in the same year. If the logging activity would have occurred absent NPA entry, this would represent a violation of the parallel trends assumption.

As an indirect test, we repeat our analysis in matched subsets of the data, pairing treated and untreated units with similar pre-treatment characteristics that may predict logging dynamics. Specifically, we match units within the same municipality, with similar access to cities (measured using travel time), with similar 2009 forest cover, and jointly on both access and forest cover. We find similar results within these matched subsets (see Appendices A2.3 and A2.4). By comparing units with similar logging potential, this strategy makes parallel trends more plausible, but it neither tests the assumption directly nor rules out contemporaneous NPA entry and logging activity. However, such co-occurrence would not necessarily reflect logging that would have occurred in the NPA's absence. In the context of the national logging ban, firms may collaborate directly with the NPA to keep state enforcement at bay, allowing them to engage in extraction that would not otherwise be possible.

Another set of possibilities concerns state responses to NPA activity. NPA presence may be associated with forest disturbance because state activity targeting NPA-affected areas drives deforestation, not because the NPA itself does. Counterinsurgency campaigns, for example, may directly result in forest destruction. In this context, however, this particular alternative explanation is unlikely. First, as discussed above, Philippine counterinsurgency in this period relied on ground operations, with little use of aerial bombardment or intentional defoliation (Armed Forces of the Philippines, 2010; Yuson, 2021; Hall, 2016). Even the most critical human rights monitoring documents report only a few dozen airstrikes nationwide across 2010 to 2015 (Karapatan, 2016). Systematic use of defoliation and forest clearing to expose insurgents, the tactics behind the widespread forest loss of the Vietnam War (Feuer, 2023), appears neither in published accounts nor in our own years of fieldwork on this conflict. Second, if military operations in NPA areas were driving disturbance, we would expect the strongest effects at the front lines of active conflict. However, violent incidents do not predict forest disturbance in our data.

The most consequential alternative in this category is that deforestation in conflict areas is driven by infrastructure construction that the state uses both to gain access to remote areas and to win civilian "hearts and minds." This type of infrastructure development in the Philippines happens primarily through two programs: PAMANA, the government's flagship development program that explicitly targets conflict zones, and Kalahi-CIDSS, the nationwide community-driven development (CDD) program targeting poorer areas.[22] These

[22]PAMANA is an umbrella program that pools the conflict-area spending of the government's thirteen main line agencies, including the Department of Public Works and Highways (DPWH)

programs do produce measurable forest disturbance (Figure A4.5), so the question is whether their allocation relates to changes in NPA presence in a way that could confound our main results.

Overall, our findings are inconsistent with this story. One possibility we investigate is that the state initiates infrastructure projects in direct response to the NPA's arrival in a given area, so that the disturbances we observe following NPA entry are driven by infrastructure construction rather than NPA-facilitated resource extraction. If this were true, we would expect NPA entry to predict infrastructure construction; however, we do not find evidence of this in our data (Tables A4.12 and A4.13). Another possibility relates to the exit side: state-led infrastructure construction may increase forest disturbance while it is underway, help push the NPA out, and then wind down, so that the reduction in disturbance we attribute to NPA exit actually reflects the end of construction. Again, we do not find evidence consistent with this interpretation. In the aggregate, the initiation of state-funded infrastructure does not predict NPA exit (Table A4.14). The one project type that does—road construction funded by Kalahi-CIDSS—produces no detectable increase in either type of forest disturbance (see Tables A4.15 and A4.16 and Figures A4.6 and A4.7).[23]

Because the deforestation effects of infrastructure projects build over several years (Figure A4.5), we also re-estimate our main models including lagged indicators of infrastructure initiation in each of the three preceding years, and find the estimated effects of NPA presence essentially unchanged (Table A4.17). Finally, state infrastructure projects and the NPA leave different footprints across places. As we show below, the effect of NPA presence on forest disturbance disappears in Indigenous areas and near landslide scars, while the infrastructure clears the forest there as much as anywhere else (Table A4.11).

and the Department of the Interior and Local Government (DILG), so our records capture that spending in one place (Haim, Fernandez, and Cruz, 2019). The bulk of funds under both PAMANA and Kalahi-CIDSS go to rural infrastructure, especially roads.

[23]The aggregate effect estimate of Kalahi-CIDSS roads is not significant for either deforestation or degradation; however, there is a detectable decrease in deforestation three years following project initiation.

Interpreting Pre-Exit Anticipation

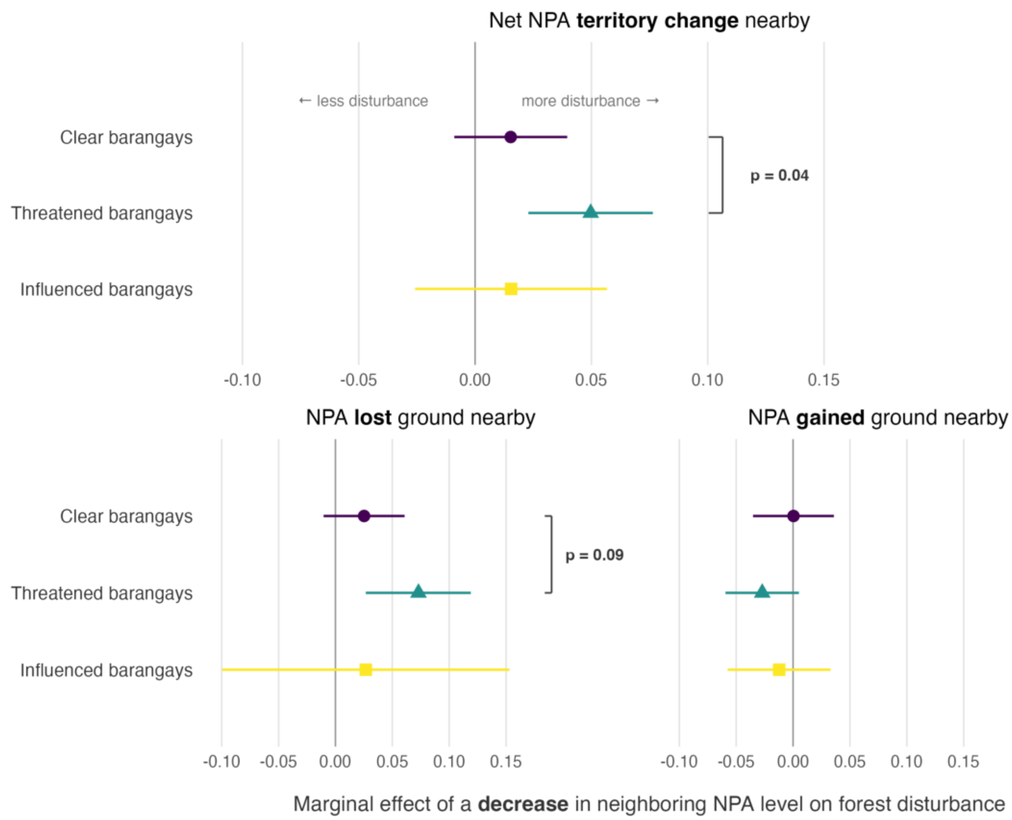
Turning to anticipation effects: in the two years before an NPA exit, forest disturbance rises before dropping sharply at the moment of exit (Figure 2, red triangles). The uptick is visible in degradation (Figure 3, bottom panel) but not in deforestation (Figure 3, top panel), suggesting that whatever drives the pre-exit pattern operates through logging-style extraction rather than permanent land conversion.

The pre-trend complicates the interpretation of the exit ATT. If disturbance is rising before the NPA leaves for reasons unrelated to NPA behavior, the post-exit drop may be partly due to mean-reversion or another underlying trend rather than a treatment effect. The infrastructure results above rule out the most concrete version of this story, in which the post-exit drop is caused by a state infrastructure project winding down rather than the NPA's departure. In addition, the anticipation effects are mitigated substantially within the matched samples described above, while the post-exit effects remain similar in significance and magnitude.[24]

However, the anticipation pattern is also consistent with a prediction of civilian-legitimacy theory that we did not initially set out to test. When the NPA expects to retain its presence in a village, civilian legitimacy matters and may discipline logging. When it expects to lose the village, its time horizon shortens, the long-run cost of harming civilians falls, and short-run revenue considerations dominate, shifting the rebel calculus from the restraint of a stationary bandit toward the extraction of a roving one (Olson, 1993; Arjona, 2017). Rebels whose hold on a village is slipping extract as much as they can before they go. This implies that disturbance should rise in the years before exit, specifically in villages where nearby events in those years signal that the NPA is more likely to lose control.

[24]See Appendix A2.4. Matching on municipality, pre-treatment forest cover, and access eliminates the anticipation effect two years prior to NPA exit. Degradation is still higher one year before exit, but the magnitude of the coefficient shrinks considerably, compared to the full sample and to the post-exit effects in the matched samples.

Figure 4. Anticipation of NPA exit: disturbance rises in threatened barangays after neighboring losses of NPA control.



Note: Marginal effects, with 95 percent confidence intervals, of the prior year's net change in NPA control across adjacent barangays on annual forest disturbance, by the barangay's own NPA control level (clear, threatened, influenced, in increasing order of control). Estimates come from TWFE specifications with point and year fixed effects and standard errors clustered by barangay. The top panel uses net neighbor change (Table A5.2, Model 2), and the bottom panels separately estimate the effects of neighboring losses and gains (Table A5.2, Model 3). The top and bottom-left panels show the effect of a one-unit loss of NPA control nearby, and the bottom-right panel shows the effect of a one-unit gain. Brackets indicate the p-value for a test of differential effects between threatened and clear barangays.

To test this, we need a proxy for whether the NPA in a given village expects to lose control in the coming year. We use the sum of NPA-control changes in adjacent barangays in the prior year, since counterinsurgency campaigns tend to roll across neighboring villages. Consistent with this, we find that one-, two-, and three-year lags of this variable – net neighbor change – predict NPA control in the present year (Table A5.1). The logic linking time horizons to accelerated deforestation further predicts that these effects should be concentrated in "threatened" barangays, the middle level of the scale, where the rebels are active but their hold is fragile. There, a nearby loss is a strong signal that they may be pushed out next. The response should be absent in "clear" barangays, which have no NPA presence and so no rebels to anticipate losing territory. It should also be weaker in "influenced" barangays, where rebels are secure enough that a neighboring loss may not create expectations of losing the village. There, civilian legitimacy should continue to restrain logging.

As shown in Figure 4, the empirical pattern matches this prediction. In threatened barangays, forest disturbance rises significantly in the year after NPA control declines across neighboring barangays (Figure 4, top panel). We find no such effect in clear and influenced barangays (Figure 4, top panel), and the effects differ significantly between threatened and clear barangays ($p = 0.04$; Table A5.2, Model 2). The effect is concentrated in degradation rather than deforestation (Appendix A5), reinforcing the selective logging interpretation.

The bottom panels of Figure 4 disaggregate our net change variable into neighboring gains and losses. In threatened barangays, disturbance rises significantly after nearby losses (bottom left panel), and this rise is larger than in clear barangays ($p = 0.09$; Table A5.2, Model 3). Nearby gains result in no such increase (bottom right panel), suggesting that disturbance increases in response to the NPA losing momentum, not to nearby territorial contestation more generally. Taken together, we read these results as evidence of the NPA accelerating logging when it expects to lose the village.

Legitimacy Costs

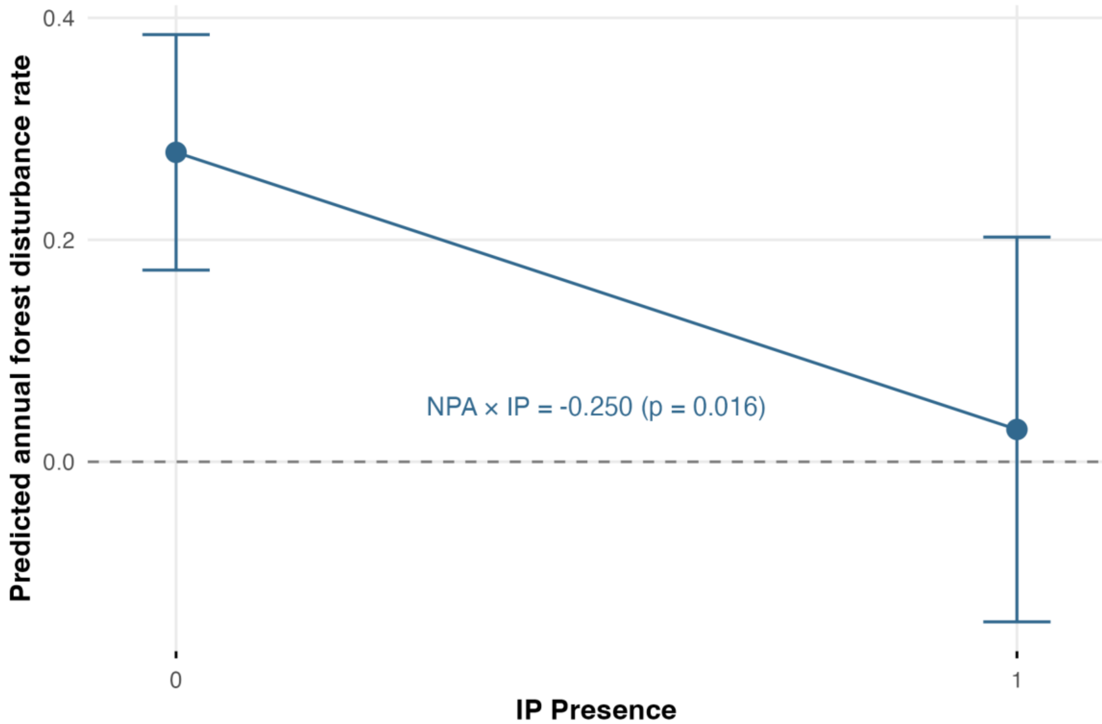
The neighbor-change result provides suggestive evidence that civilian-legitimacy considerations vary with the NPA's expected time horizon. In this section we test whether they also vary across communities, as our remaining two hypotheses predict: that the deforestation effect of NPA presence is mitigated in Indigenous communities, where forests are embedded in livelihoods and customary governance (H2), and near recent landslide scars, where forest loss has acute, locally salient consequences (H3).

Consistent with H2, NPA presence increases forest disturbance only in non-Indigenous barangays (Figure 5, Table 2 Model 1). In barangays without an Indigenous community, NPA presence is associated with an additional 0.28 percentage points of disturbance per year, roughly a 47 percent increase over the 0.59 percent point-level baseline. In barangays containing an Indigenous community, the conditional NPA effect collapses to 0.03 percentage points and is statistically indistinguishable from zero. The interaction coefficient (NPA $\times$ Indigenous community $= -0.250$, $p < 0.05$) captures the differential. The IP moderation is robust to alternative measures of Indigenous community presence, including barangays containing approved CADT-titled land and IP population share.[25]

Consistent with H3, proximity to recent landslides also moderates the NPA effect on forest disturbance (Figure 6, Table 2 Model 2). Because the moderator is distance, larger values mean less exposure to landslides. Points close to a recent landslide, where civilians have the most direct experience of forest-related disaster, take small values on this measure. The estimates line up with this reading. Far from any recent landslide, NPA presence is associated with more forest disturbance, in line with the pooled main effect. Closer to landslide scars, the conditional NPA effect attenuates and, at points around 500 m from a

[25]Subsetting the FEct event study by Indigenous community presence shows the same heterogeneity dynamically: the NPA entry effect is essentially zero in barangays with an Indigenous community (Figure A4.2), while it is large and significant in barangays without one (Figure A4.1). The exit effect is similar in the two subsamples.

Figure 5. The NPA effect on forest disturbance disappears in Indigenous communities.



Note: Predicted annual forest disturbance from a TWFE specification interacting NPA presence with an indicator for an Indigenous community located in the barangay. Points show the estimated effect of NPA presence on annual forest disturbance, in percentage points, for barangays without (0) and with (1) an Indigenous community, with 95 percent confidence intervals. Estimates are from Table 2, Model 1.

scar, the point estimate turns negative (roughly −0.4 percentage points relative to the no-NPA counterfactual), though the confidence interval at that end of the range is wide. The positive interaction coefficient captures the underlying gradient: moving away from landslide scars shifts the NPA effect upward, toward more disturbance. When both moderators are estimated jointly (Table 2 Model 3), the IP and landslide interactions retain their signs and significance, indicating they capture distinct variation.[26]

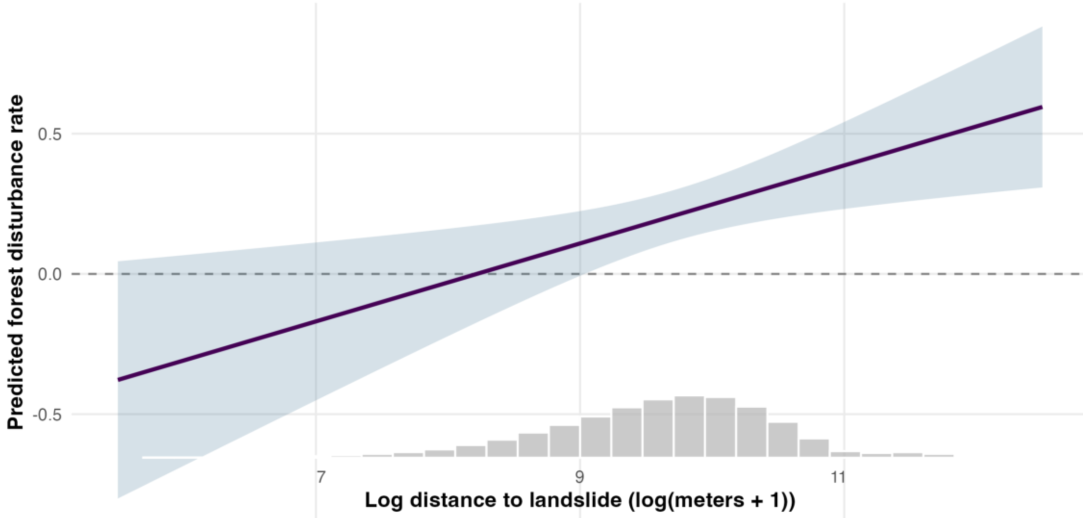
[26]The landslide moderation is robust to alternative specifications of landslide proximity, including raw rather than logged distance and alternative codings of NPA presence. In addition, we conduct the FEct analysis in subsets of the data defined by landslide distance. The results are generally consistent with the pattern in the main TWFE analysis: in areas closest to landslides (within the bottom quintile of distance), the effect of NPA entry is no longer significant, while it remains significant in areas in the second through fifth quintiles. The average effects of exit are similar in

Table 2. The NPA effect on forest disturbance attenuates in IP communities and near landslides.

Dependent variable: new forest disturbance in year t (percentage points)	(1)	(2)	(3)	(4)	(5)
		Moderators		With Controls	
	IP	Landslide	Both	City Access	Infra.
NPA × IP Community (**H2**)	-0.250^{*}		-0.251^{*}	-0.277^{**}	-0.229^{*}
	(0.103)		(0.102)	(0.106)	(0.104)
NPA × Landslide Dist. (**H3**)		0.139^{**}	0.140^{**}	0.123^{*}	0.132^{**}
		(0.050)	(0.050)	(0.049)	(0.049)
NPA Presence	0.279^{***}	0.196^{***}	0.273^{***}	0.268^{***}	0.214^{***}
	(0.054)	(0.047)	(0.054)	(0.052)	(0.053)
NPA × City Access				0.024^{+}	0.020
				(0.013)	(0.013)
NPA × Infra.					0.370^{**}
					(0.135)
Any Infra.					−0.043
					(0.055)
Infra. × IP Community					0.526^{***}
					(0.140)
Infra. × Landslide Dist.					−0.056
					(0.048)
Infra. × City Access					0.005
					(0.017)
Outcome Mean	0.588	0.588	0.588	0.588	0.588
Unit of Analysis	Point-year	Point-year	Point-year	Point-year	Point-year
Observations	712,341	712,341	712,341	712,341	712,341
R^2	0.140	0.140	0.140	0.140	0.140
Estimator	TWFE	TWFE	TWFE	TWFE	TWFE

Note: All models include point and year fixed effects. NPA presence is a binary indicator of any NPA control, so its coefficient pools the effects of entry and exit. IP community is an indicator that the barangay contains an Indigenous community. Landslide dist. is the natural log of one plus the distance in meters from the point to the nearest recorded landslide, mean-centered; larger values are farther from any landslide, so a positive interaction means the NPA effect grows with distance from a scar. Models 4 and 5 add city access and infrastructure (Infra.) controls, respectively. Any infrastructure indicates a Kalahi-CIDSS or PAMANA project active in the barangay that year; city access is travel time in hours to the nearest city in 2015, mean-centered. Time-invariant main effects are absorbed by point fixed effects. Shaded cells are the tests of H2 and H3. Standard errors clustered at the barangay level in parentheses. Appendix A4.9 reports FEct entry and exit event studies within the Indigenous and landslide subsamples. Appendix A2.4 reports FEct event studies on samples matched on city access and forest cover. $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Figure 6. The NPA effect on forest disturbance attenuates near recent landslide scars.



Note: Predicted effect of NPA presence on annual forest disturbance, in percentage points, from a TWFE specification interacting NPA presence with log(1 + distance to nearest recorded landslide), distance in meters, with a 95 percent confidence band. Smaller values are points closer to a recent landslide scar; larger values are points farther from any recent landslide. The histogram shows the distribution of points along this range. Estimates are from Table 2, Model 2.

Additional sources of potential confounding threaten inference in the interaction models. In particular, Indigenous communities tend to occupy mountainous interiors, where landslides are also more likely and which may be harder for logging and mining firms to reach. This raises the possibility that the heterogeneous effects of NPA presence reflect variation in the feasibility or profitability of forest extraction, or of state enforcement, rather than NPA restraint where deforestation is especially costly to civilians. Unlike the main analysis, where the use of within-village variation accounts for static unobserved differences in accessibility, the heterogeneity tests rely on cross-sectional variation in the moderators, which may be correlated with other factors that separately shape forest disturbance. To address this, we use travel time from each point to the nearest city, which captures both the limits of routine state monitoring and the cost of moving extracted products to buyers

both subsets, but the effect appears earlier in more distant barangays (see Figures A4.3 and A4.4 in the appendix).

(Table A4.4).[27] The NPA × city-access interaction is generally positive (Table 2 Models 3 and 4; Tables A4.4 and A4.5), consistent with NPA-driven disturbance being larger in less accessible areas, but including it does not meaningfully change either the NPA × IP or the NPA × log-landslide interaction. In addition, the results in Table A4.11 Model 1 and Figure A1.2 indicate that levels of forest disturbance are *higher* on average in areas with Indigenous communities and near landslides, weighing against the idea that these areas are generally less feasible or attractive to firms.

A second concern is that the heterogeneous effects could be capturing how the state allocates infrastructure programs in Indigenous areas and areas near recent landslides, rather than NPA responsiveness to civilian legitimacy concerns. To address this, we estimate NPA presence and infrastructure timing jointly with their full interactions with IP presence and landslide distance (Table 2 Model 5). The NPA × IP and NPA × log-landslide interactions retain their signs and significance; the corresponding infrastructure interactions run in the opposite direction. Infrastructure × IP community is +0.526 ($p < 0.001$): government projects are associated with *more* disturbance in Indigenous barangays, the opposite of the NPA pattern. Infrastructure × log landslide distance is roughly zero, while the NPA's analogous interaction remains positive and strongly significant. Government development accelerates disturbance where the NPA restrains it, which would be hard to reconcile with the IP and landslide patterns being artifacts of state programming.

Taken together, the empirical pattern is what a legitimacy-constrained extractor should produce. The pooled NPA effect on forest disturbance is large and operates through both selective logging and permanent conversion. It intensifies as exit approaches and the time horizon for civilian payoffs collapses. It disappears or reverses where forest loss imposes the heaviest civilian costs, and it runs opposite to the state's pattern over the same dimensions.

[27] This measure relies on data on geographic distance, road networks, and other terrain features.

Conclusion

Our findings speak to ongoing debates in the study of rebel governance and environmental politics. Rather than asking only whether rebels destroy or protect forests on average, we show that the answer depends on the legitimacy costs they face, which vary with how much forest loss harms the communities on which they rely. In the Philippines, NPA presence accelerates forest loss on average but not in Indigenous communities or areas near recent landslides. Empirically, we measure rebel presence directly rather than inferring it from violence. The two measures give different answers, suggesting that the environmental footprint of civil war is not only a byproduct of fighting, but also a choice armed groups make. Rebels weigh the revenue the forest can yield against the support of the civilians who depend on it.

How far these findings travel depends on a few conditions. The revenue-legitimacy tradeoff should be sharpest where forests are an important source of rebel revenue and civilian support is the main constraint on extraction. Where rebels draw on other revenue sources, such as foreign sponsors or drug trafficking, or where they rival the state in coercive power, forest extraction and civilian legitimacy both matter less. Where forests are critical for strategic cover, rebels may protect them for reasons unrelated to civilian preferences. And where clearing forests generates profits for local populations that outweigh the harms, responsiveness to civilians may instead *encourage* deforestation. Still, the case is far from unique. Like the NPA, the majority of asymmetric insurgencies draw revenue from forested land and depend heavily on civilian support. Many such insurgencies also operate in disaster-prone regions with historically marginalized populations, where we find the legitimacy costs of deforestation are highest. Because the conditions that shape legitimacy costs vary across conflicts as well as within them, the tradeoff we identify may help explain why rebel presence accelerates forest loss in some conflicts and slows it in others.

Our theory centers on rebel incentives, a tractable starting point for a problem that also involves other actors, including government agencies and politicians. Deforestation continues in the Philippines despite the national logging ban, not only due to the NPA conflict but also because government enforcement is selective. Like rebels, local politicians use forest resources for political ends, especially in decentralized settings. Recent research from a variety of contexts finds that deforestation rises in election years, as politicians raise campaign funds, reward contractors, or deliver benefits to voters (Sanford, 2023; Harding et al., 2024; Pailler, 2018). Our design absorbs common shocks like election cycles, and we show that state infrastructure programs do not explain the effects we attribute to rebels. What it does not capture is the strategic interaction between rebels and politicians, in which rebel presence shapes what politicians can offer and extract, and politicians' choices in turn shape what rebels can protect or sell. We see the interplay between enforcement agencies, national and local politicians, and rebels as a natural next question.

A second open question concerns the strategic dynamics between Indigenous communities, rebels, and the state. We interpret our findings as evidence that Indigenous presence, on average, constrains NPA-facilitated deforestation. This does not mean that Indigenous communities necessarily oppose all resource extraction; they may support forms perceived as less harmful and expected to yield material benefits. Understanding the determinants of these preferences and expectations is critical for gaining a comprehensive picture of how rebel-community interactions shape deforestation. One potentially important factor is the extent of Indigenous communities' territorial autonomy, often linked to state-recognized collective land rights (McMurry, 2022; Albertus, 2026). Here, we find little evidence of a role for formal property rights: our results are similar when we measure Indigenous presence using approved ancestral domain titles rather than eligibility or population share. This may reflect the state's limited authority in rebel-influenced areas, but could also mask important community-level variation or countervailing effects on community capacity, bargaining leverage, and extractive preferences, especially in the context of a national logging ban. Whether, and under what conditions, state recognition shapes community-rebel interactions over forest use remains to be seen. This question is also relevant for the growing literature examining the relationship between Indigenous collective property rights and deforestation (BenYishay et al., 2017; Baragwanath and Bayi, 2020; Camino et al., 2023), which has rarely considered contexts where armed non-state actors limit state authority or where state policy restricts logging.

Finally, our findings have implications for conservation policy. Conservation efforts, even in conflict-affected regions, focus primarily on state regulations and enforcement. We contribute to a growing body of research showing that forest governance in conflict areas depends on armed non-state actors, not only the state (Cunningham et al., 2025; Gilmore et al., 2025). Those groups' incentives run through communities, so programs that give communities a stake in preserving forests and the agency to defend them may also raise the cost to rebels of allowing their loss.

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Table of Contents

A1 Descriptives, maps, and treatment timing

A1.1 Descriptive statistics

Table A1.1 reports summary statistics for the NPA analysis sample.

Table A1.1. Summary statistics for the NPA analysis sample (point-years, 2009–2015).

	Mean	SD	Min	Max	N
Undisturbed change	0.588	7.648	0.000	100.000	712,341
NPA presence	0.206	0.405	0.000	1.000	712,341
Distance to landslide (m)	22,224.194	24,249.601	16.887	297,744.834	712,341
Indigenous community (indicator)	0.212	0.409	0.000	1.000	712,341

Note: Rows report annual forest disturbance (sign-flipped so positive values indicate loss), binary NPA presence, distance to the nearest landslide (meters), and Indigenous community (indicator).

A1.2 Undisturbed change by NPA control

Figures A1.1 and A1.3 summarize point-level forest disturbance in the NPA analysis sample. The outcome is the average decrease in undisturbed forest cover, so higher values indicate greater loss of undisturbed forest cover.

Figure A1.1 has two panels: (A) the cross-sectional mean disturbance rate by NPA control intensity across all point-years, and (B) mean 2009 undisturbed forest cover by 2009 NPA control, both with 95% confidence intervals. Figure A1.3 plots the national mean disturbance rate over 2009–2015 (bold black line) alongside regional means (grey lines).

Figure A1.2 shows how baseline forest stock and average disturbance vary by binary Indigenous Peoples (IP) presence and across quartiles of log distance to landslide-prone areas.

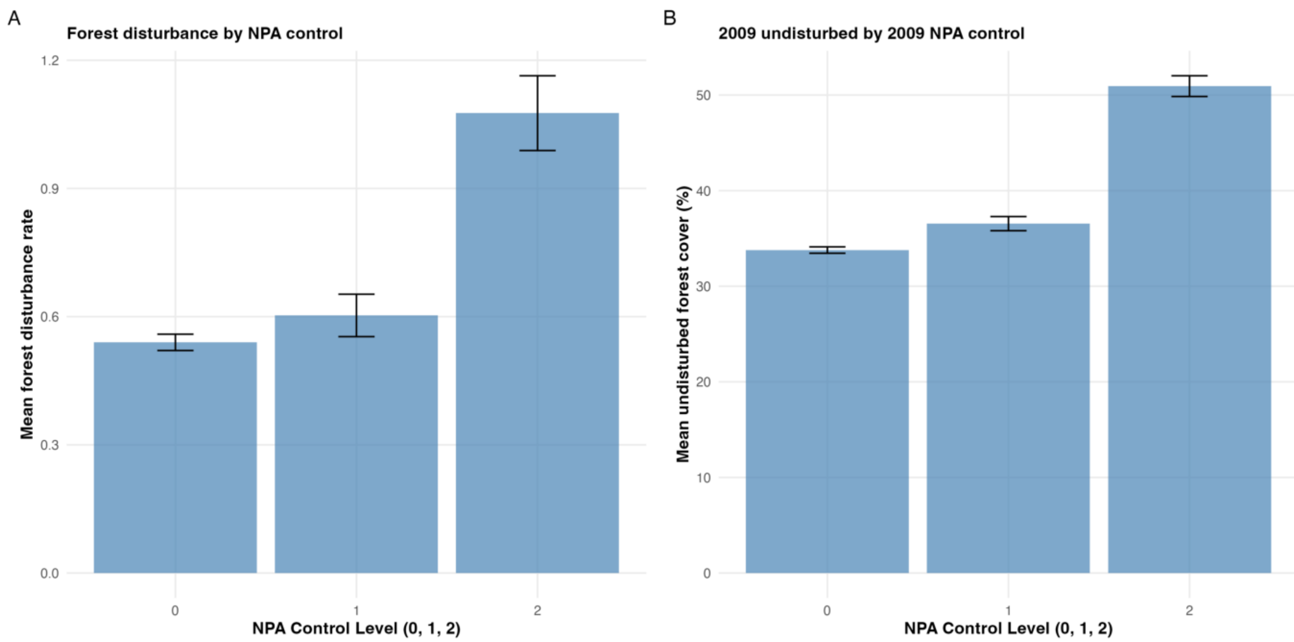
A1.3 NPA entry, exit, and remaining forest cover

Figure A1.4 plots undisturbed forest cover in the transition year for barangay-level NPA entry (no presence to presence) and exit (presence to absence). The exit panel addresses whether negative exit effects in the event studies (Appendix A2.1, Figure A2.2) reflect a mechanical floor effect. The distribution of forest cover is similar where the NPA enters and where it exits, so the rebels are unlikely to be driving forest cover to zero and then leaving.

A1.4 Treatment timing visualization

Figure A1.5 displays barangay-level NPA treatment timing in a heatmap. Each row is a barangay sorted by when NPA presence begins or ends; columns are years. Dark blue cells indicate NPA presence. During the study period the NPA is in national decline, so more barangays lose NPA presence than gain it.

Figure A1.1. (A) Cross-sectional mean forest disturbance rate by NPA control level across point-years (0 = clear, 1 = threatened, 2 = influenced, the AFP categories used in the main text). (B) Mean 2009 undisturbed forest cover (%) by 2009 NPA control level. Both panels show 95% confidence intervals.



A1.5 Maps

Figure A1.6 maps barangays that ever experienced NPA presence during the panel. Light blue barangays had NPA presence at some point; dark blue barangays additionally exhibit time-varying NPA presence (both years with and without NPA).

Figure A1.7 maps the two main heterogeneity covariates: binary Indigenous Peoples (IP) presence and barangay-level mean log distance to landslide-prone areas.

Figure A1.2. (A)–(B) Mean 2009 undisturbed forest cover (%) and mean forest disturbance rate by binary IP presence. (C)–(D) Same outcomes by quartile of log distance to landslide (higher quartile = farther from recorded landslides). Points are sample means with 95% confidence intervals.

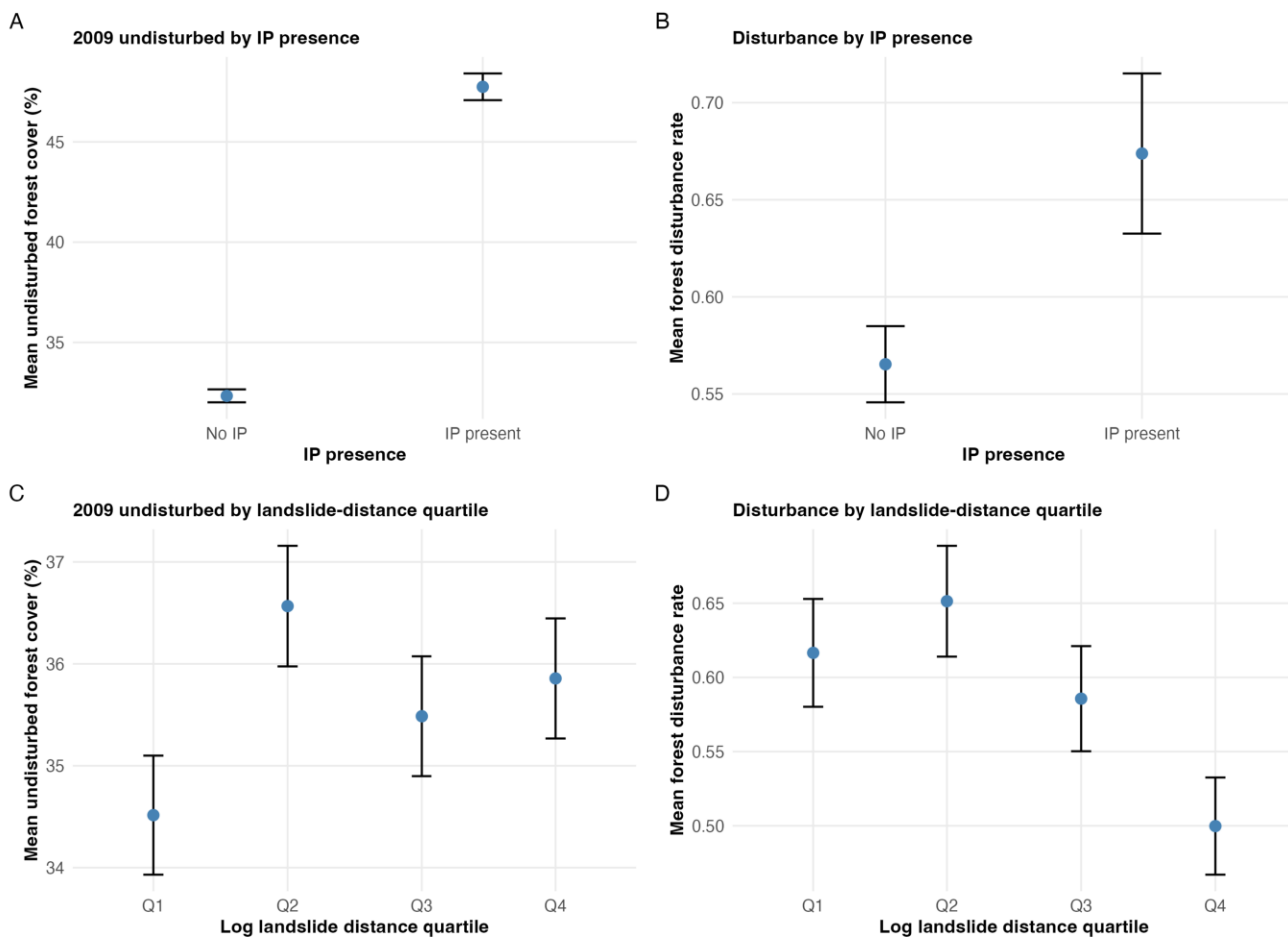


Figure A1.3. Mean forest disturbance rate by year, 2009–2015: national average (bold) and regional averages (grey).

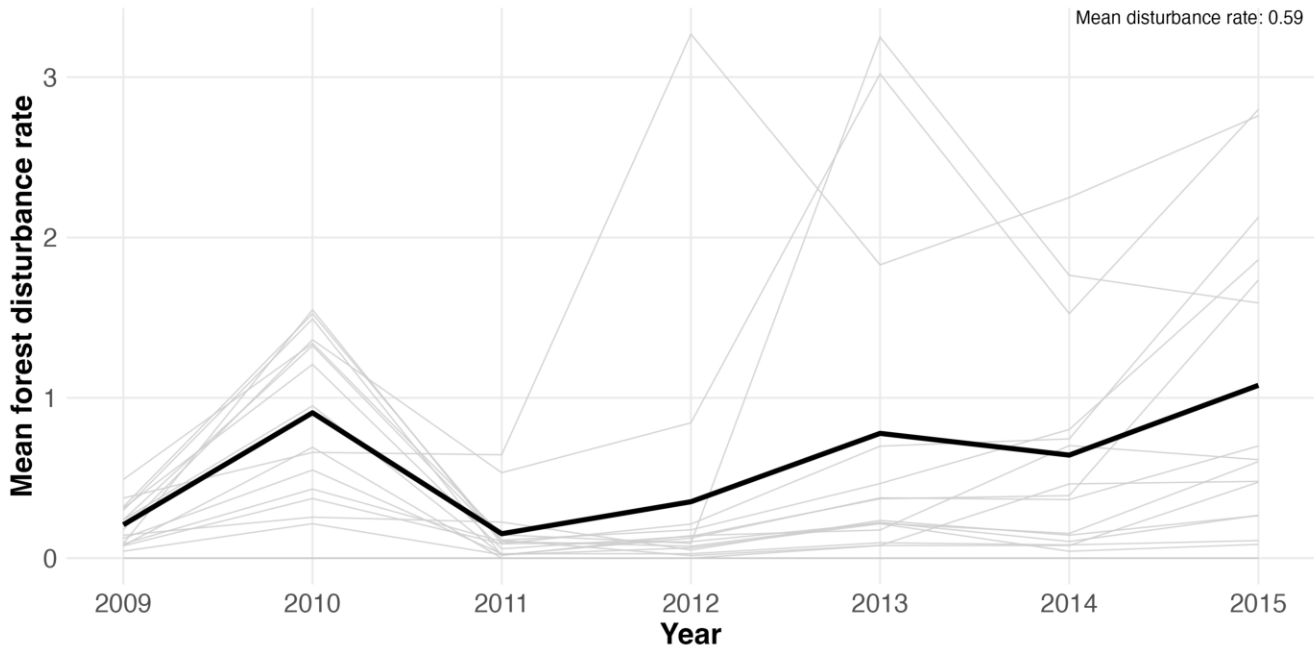


Figure A1.4. Distribution of undisturbed forest cover at barangay-level NPA entry and exit events (share of barangay area in the transition year). Left: entry (NPA presence rises from zero to positive). Right: exit (NPA presence drops from positive to zero).

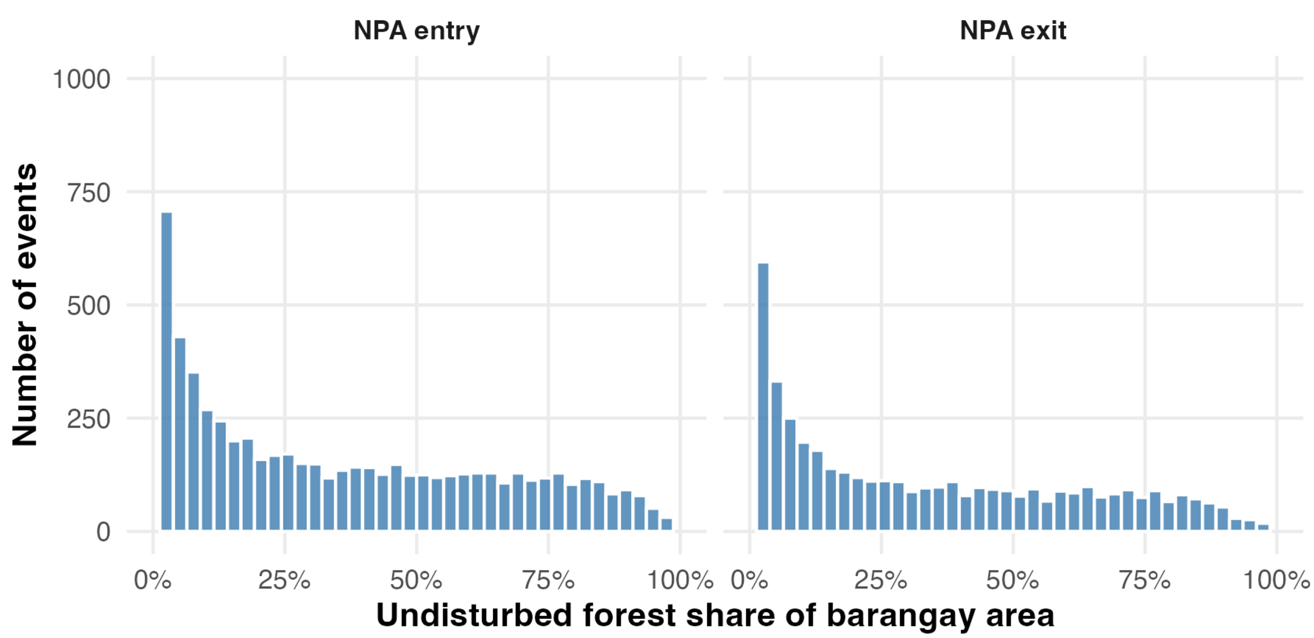


Figure A1.5. Barangay-level NPA presence over time (rows sorted by treatment timing). Treatment: NPA presence.

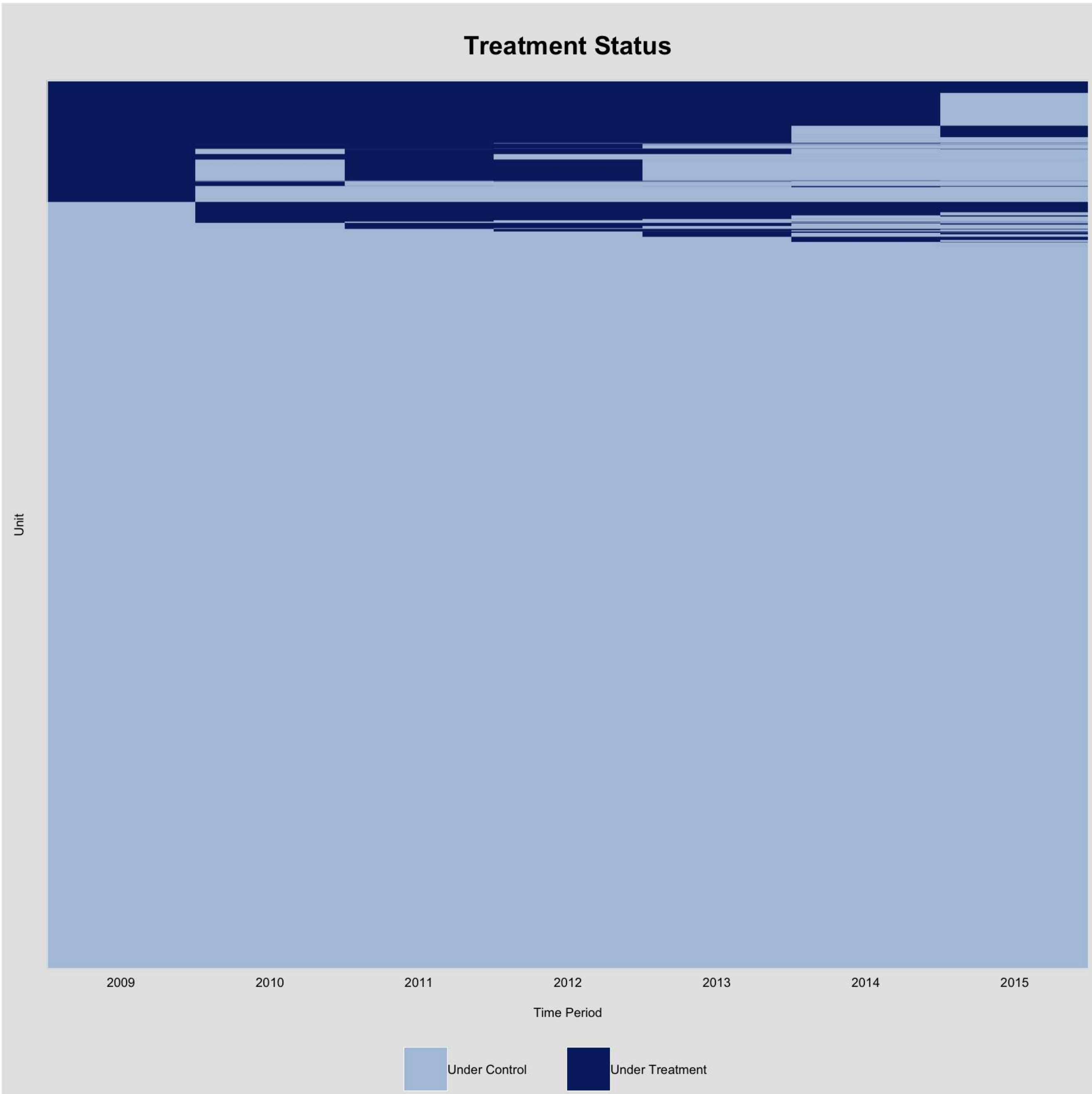


Figure A1.6. Barangays with any NPA presence (light blue) and barangays with time-varying NPA presence (dark blue).

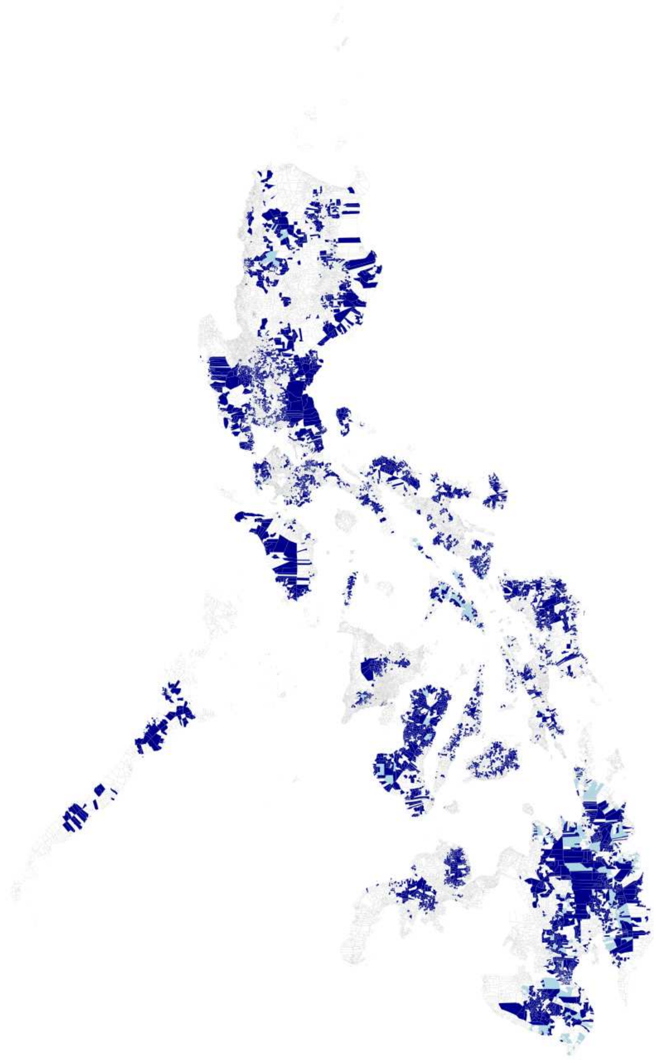
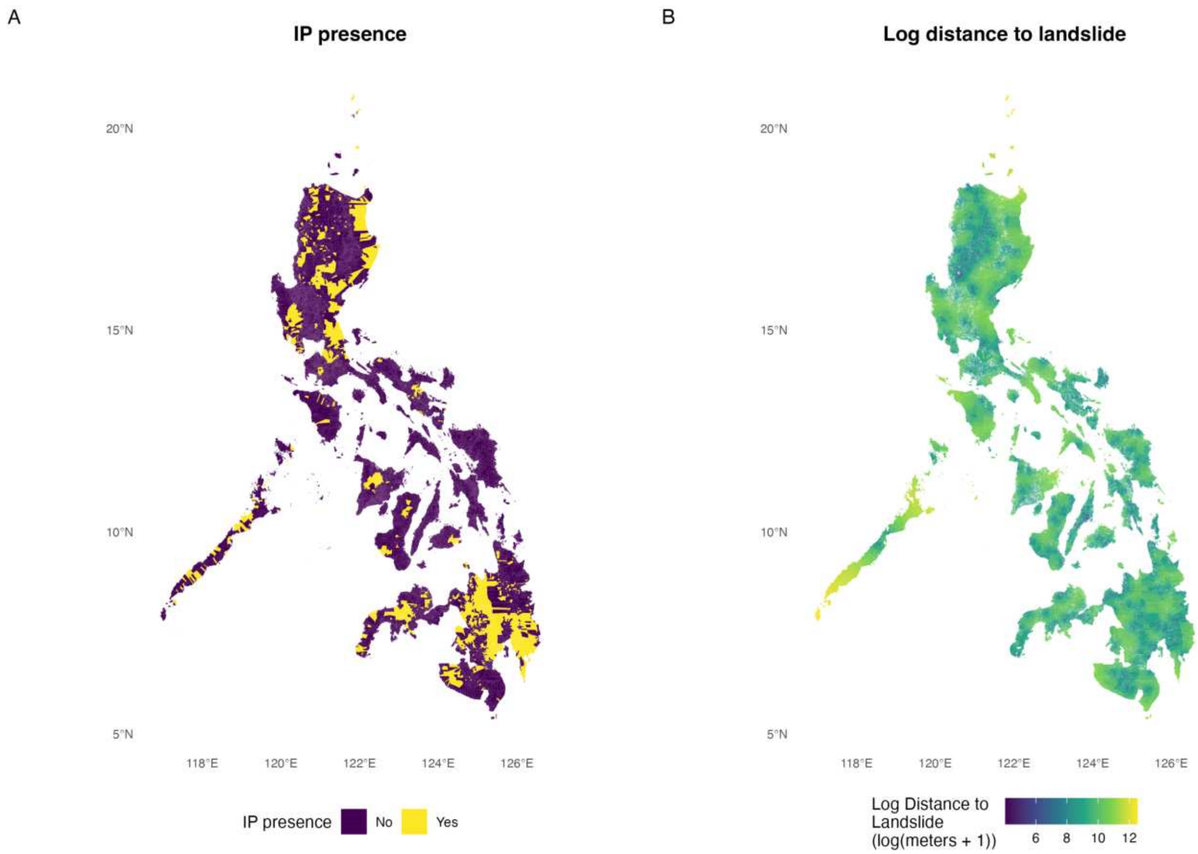


Figure A1.7. (A) Barangay-level IP presence. (B) Barangay-level mean log distance to the nearest recorded landslide (log(meters + 1)).



A2 Panel methods

This appendix reports TWFE event studies, nearest-neighbor matching combined with TWFE, matching-based FEct event studies, a robustness check excluding points with no undisturbed forest in 2008, and barangay-level randomization inference.

A2.1 Event studies

We estimate two-way fixed effects (TWFE) event studies. The outcome is the annual forest disturbance rate (loss of undisturbed forest cover, in percentage points). Entry event time is years relative to the first year a point gains NPA presence; exit event time is years relative to the first transition from NPA presence to absence. The omitted reference period is one year before treatment ($k = -1$). All models include point and year fixed effects with standard errors clustered by barangay.

$$Y_{it} = \alpha_i + \alpha_t + \sum_{k \neq -1} \gamma_k \, D_{it}^k + \varepsilon_{it}, \tag{2}$$

where $D_{it}^k = \mathbf{1}\{t - G_i = k\}$ for event time k relative to the first treatment year G_i, omitting $k = -1$ as the reference period. Figures A2.1 and A2.2 report entry and exit dynamics. We estimate exit by using only observations that had NPA presence for at least three periods before losing NPA presence.

Figure A2.1. TWFE event study: effect of first NPA entry on forest disturbance. Reference period: one year before entry ($k = -1$).

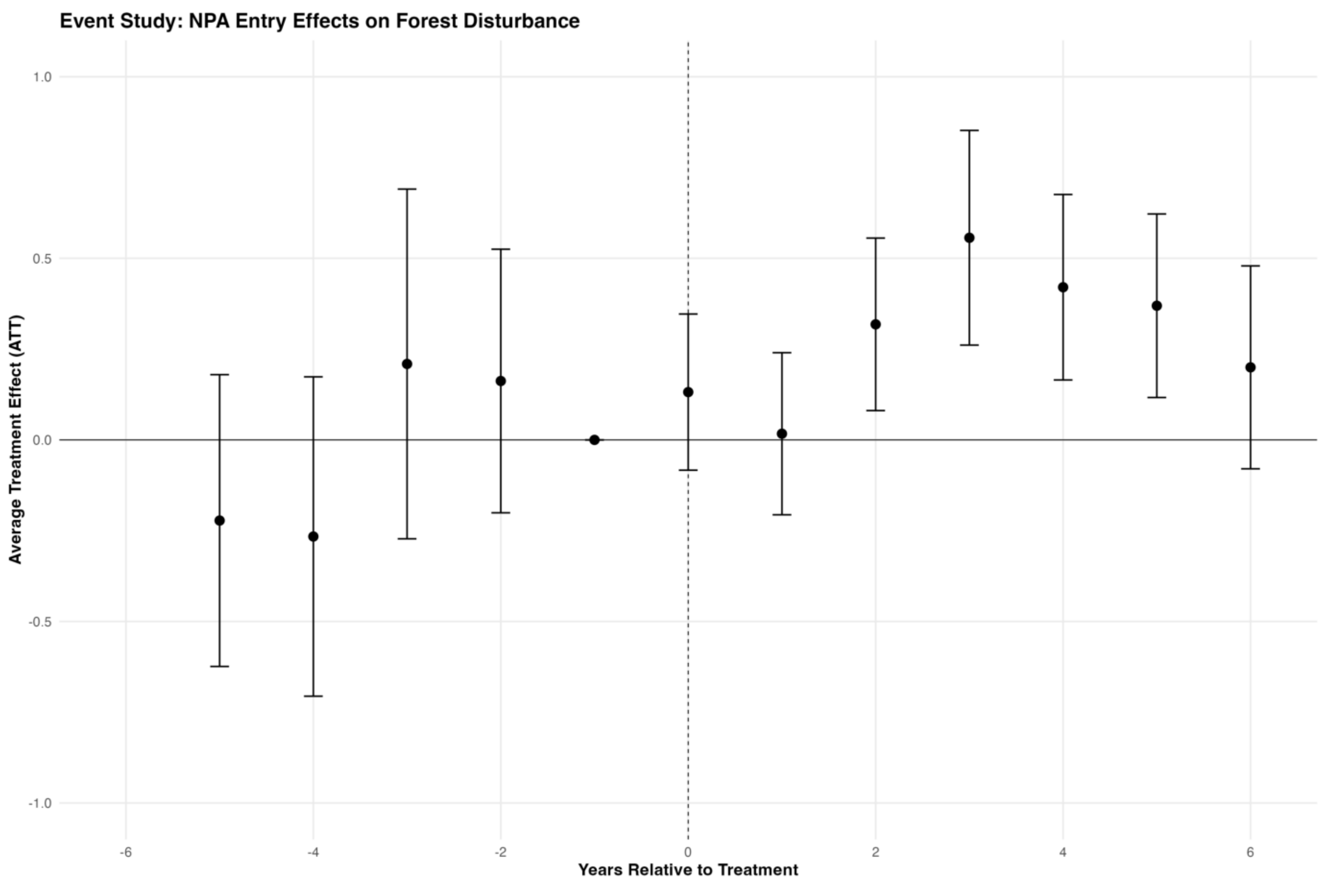
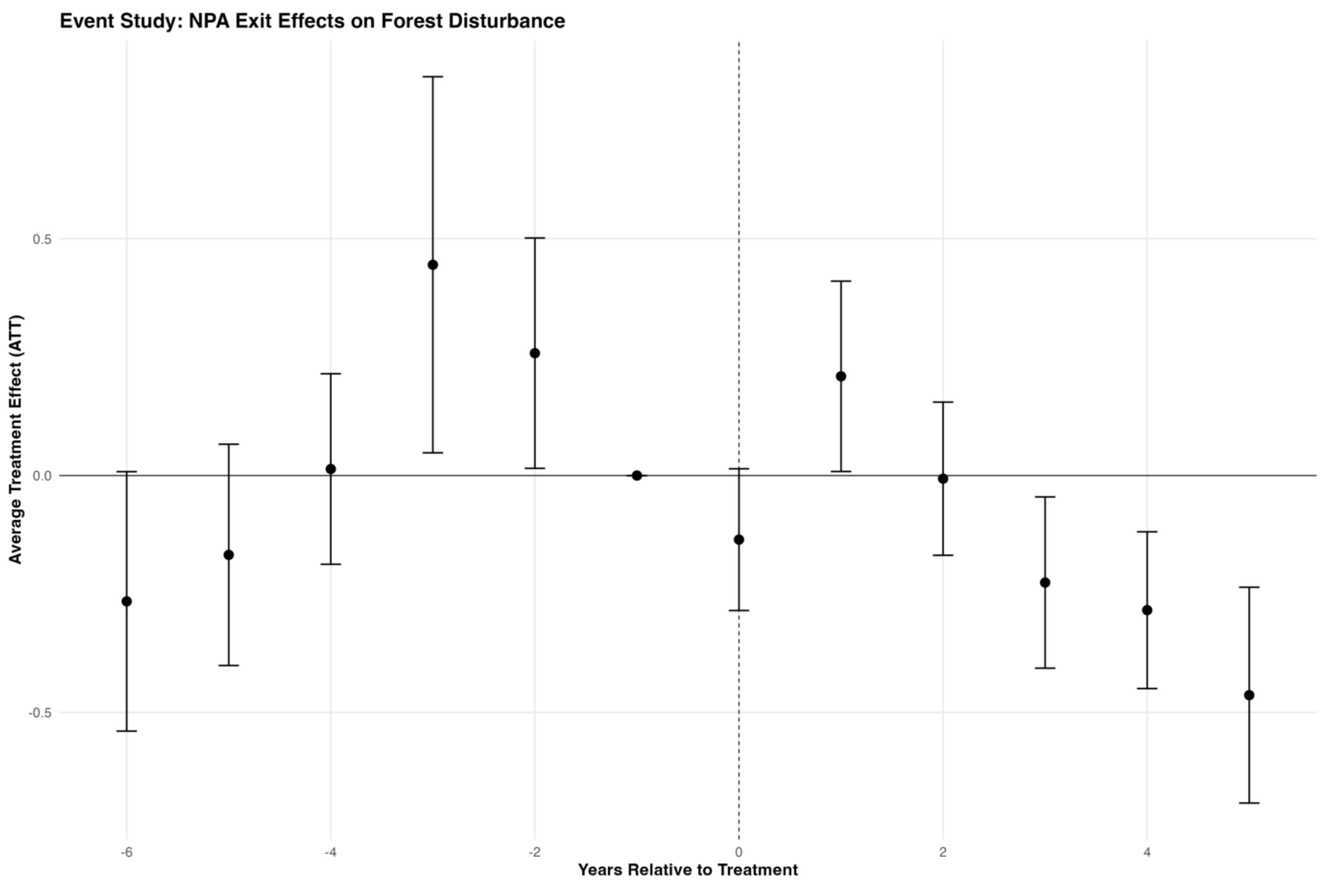


Figure A2.2. TWFE event study: effect of first NPA exit on forest disturbance. Reference period: one year before exit ($k = -1$).



A2.2 Matching and TWFE coefficients

Table A2.1 shows the ATT estimates of NPA entry and exit from the FEct model displayed in Figure 2 along with the TWFE estimates of NPA presence for both degradation and deforestation.

Table A2.1. NPA presence and annual rates of forest degradation and deforestation: TWFE (OLS with point and year fixed effects, standard errors clustered by barangay) vs FEct (two-way FE). Point-level estimates corresponding to Figure 4. Columns (1)–(3): degraded area; columns (4)–(6): deforested area. Columns (1) and (4): TWFE, NPA presence; (2) and (5): FEct, NPA entry; (3) and (6): FEct, NPA exit.

Metric	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient / ATT	0.0936	0.1032	-0.2975	0.1142	0.1486	-0.0859
Std. Error	0.0515	0.0585	0.0610	0.0289	0.0327	0.0373
p value	0.069	0.078	¡0.001	¡0.001	¡0.001	0.021
Observations	712,341	686,266	247,506	712,341	686,266	247,506
Treated units	35,358	31,633	31,633	35,358	31,633	31,633
Mean outcome	0.2635	0.2470	0.4073	0.2942	0.2916	0.2667
Outcome	Degraded	Degraded	Degraded	Deforested	Deforested	Deforested
Model	TWFE	FEct	FEct	TWFE	FEct	FEct
Treatment	Presence	Entry	Exit	Presence	Entry	Exit

Note:
Mean outcome is the mean annual change in degraded or deforested area (*degraded_diff* or *deforested_diff*, percentage points) in each column's estimation sample.

A2.3 Matching and TWFE coefficients

We perform matching as sample trimming to help resolve overlap issues and non-parametrically adjust for known possible confounders. In Tables A2.2 through A2.4 we report five specifications. Column (1) reports the estimate on the full sample. Columns (2) to (5) use 1:1 nearest-neighbor matching without replacement at the 2009 cross-section, always within the same municipality: column (2) matches on municipality alone, and columns (3) to (5) additionally match on travel time to the nearest city, the fraction of undisturbed forest in the containing barangay, and both.

Table A2.2 reports TWFE estimates on matched and full samples (see table caption for matching designs by column).

Table A2.2. TWFE estimates of NPA presence on forest disturbance (percentage points).

	Dependent variable: new forest disturbance in year t (percentage points)				
	(1)	(2)	(3)	(4)	(5)
NPA presence	0.2018***	0.1523**	0.1776**	0.1893**	0.1752**
	(0.0469)	(0.0573)	(0.0588)	(0.0578)	(0.0588)
Unit of analysis	Point-year	Point-year	Point-year	Point-year	Point-year
Observations	712,341	262,850	262,850	262,850	262,850
R^2	0.140	0.140	0.140	0.140	0.140
Estimator	TWFE	TWFE	TWFE	TWFE	TWFE

Note: Columns: (1) full sample; (2) within municipality only (no additional covariates); (3) match on 2015 city access (hours); (4) match on 2009 barangay undisturbed forest fraction; (5) match on access and forest. Point and year fixed effects; standard errors clustered by barangay. Matching at the 2009 cross-section (1:1 nearest neighbor, no replacement, exact municipality). $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

A2.4 Matching FEct entry/exit event studies

For each matching design above, we estimate combined NPA entry and exit dynamic treatment effects on the same sample using the FEct two-way fixed effects estimator. Treatments are first-year NPA entry and first-year NPA exit; the outcome is the annual forest disturbance rate. Event-time periods with fewer than 100 treated observations are dropped. Figures A2.3–A2.7 show results for the five samples.

Figure A2.3. FEct entry/exit event study on the full sample (no matching).

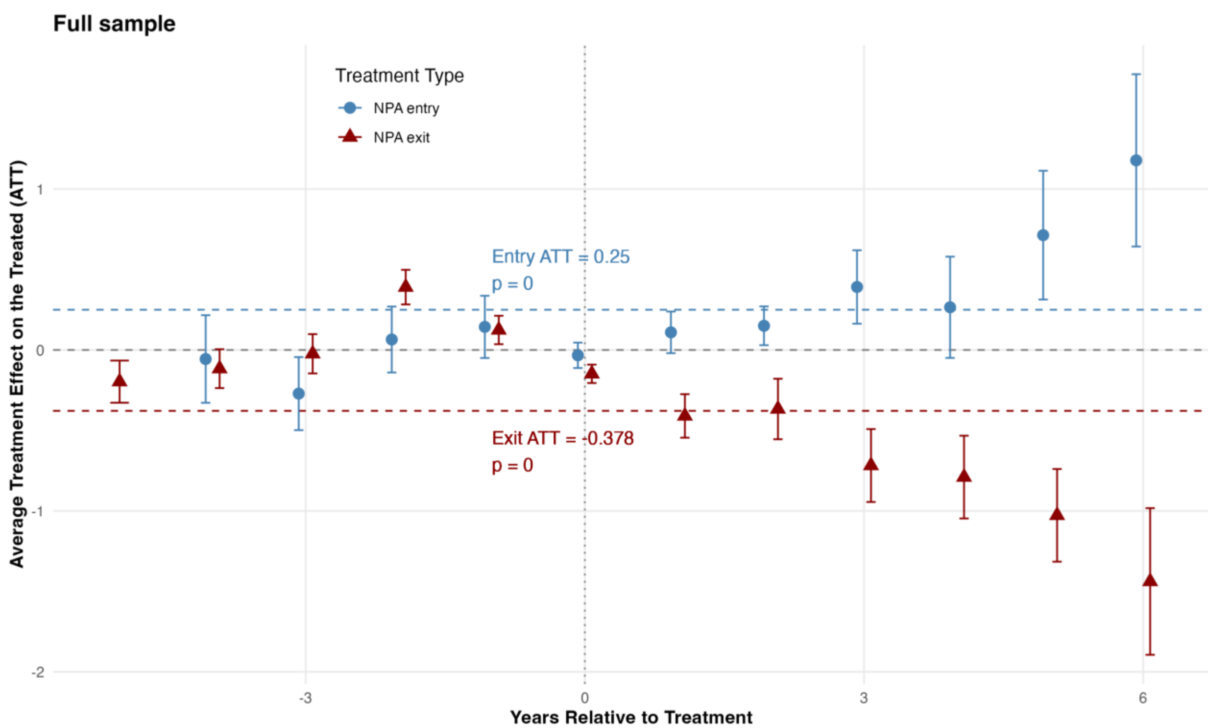


Figure A2.4. FEct entry/exit event study for treated and control points matched within municipality (exact match on municipality; no additional covariates).

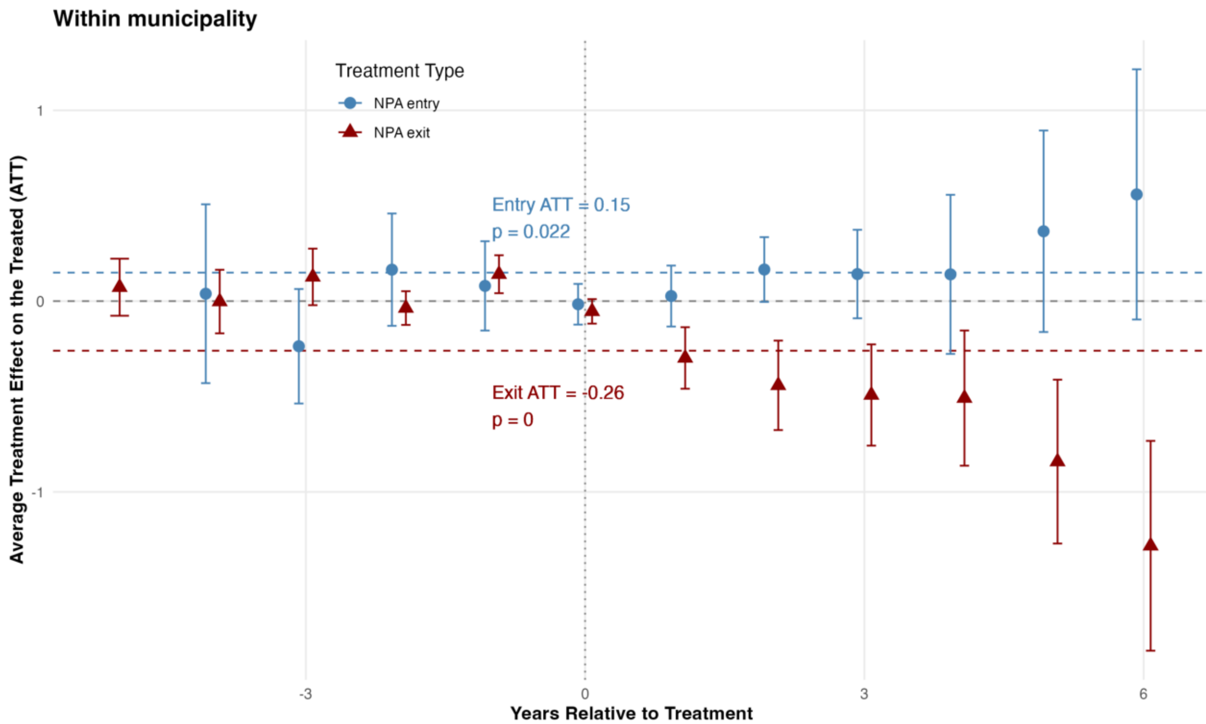


Figure A2.5. FEct entry/exit event study after matching on access to cities (2015 travel time).

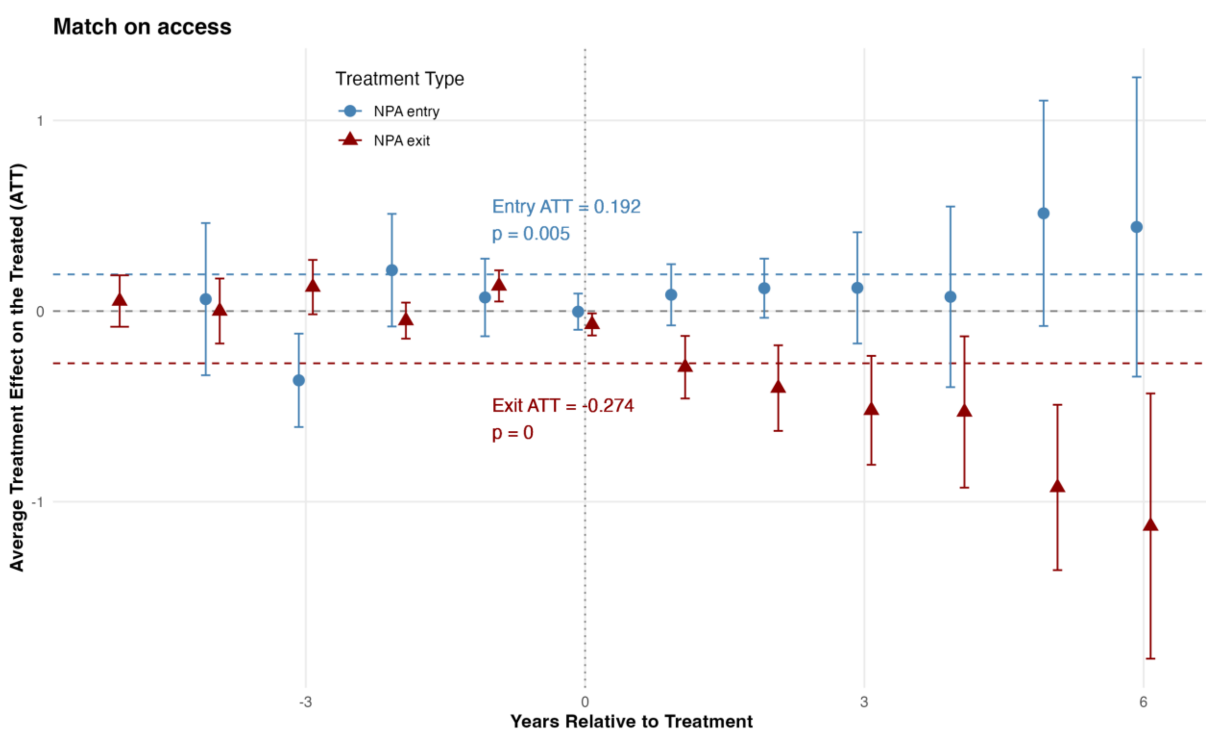


Figure A2.6. FEct entry/exit event study after matching on barangay undisturbed forest fraction in 2009.

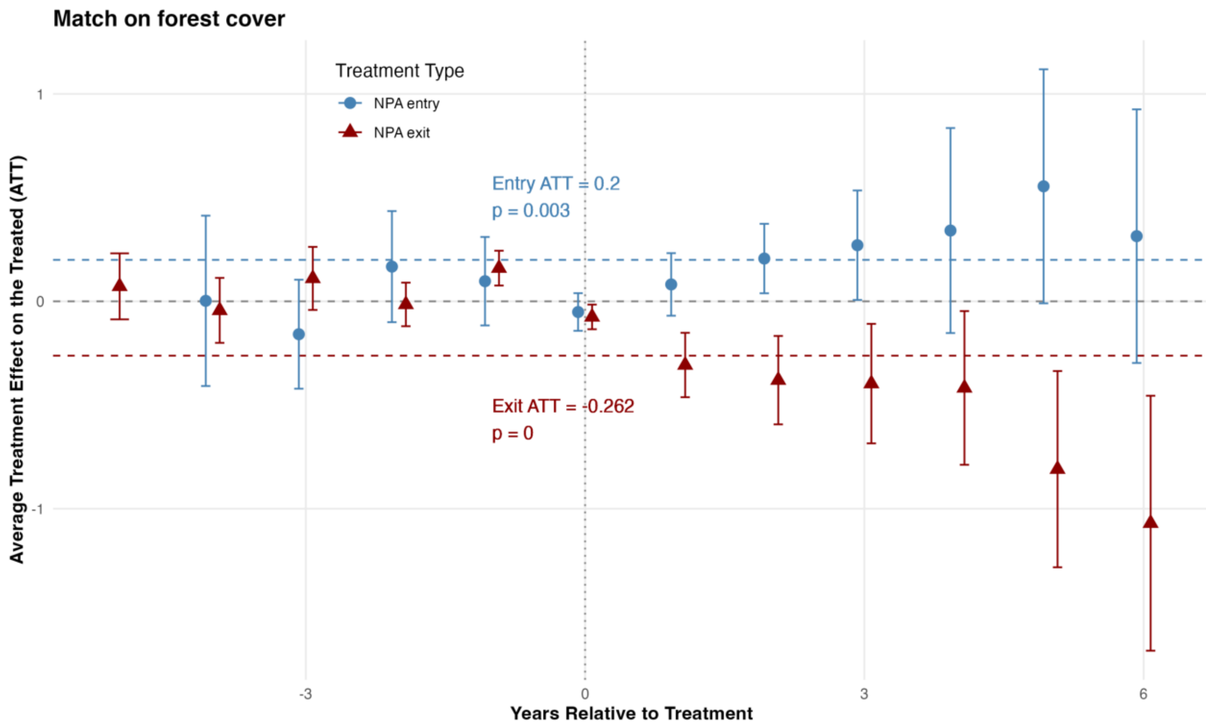
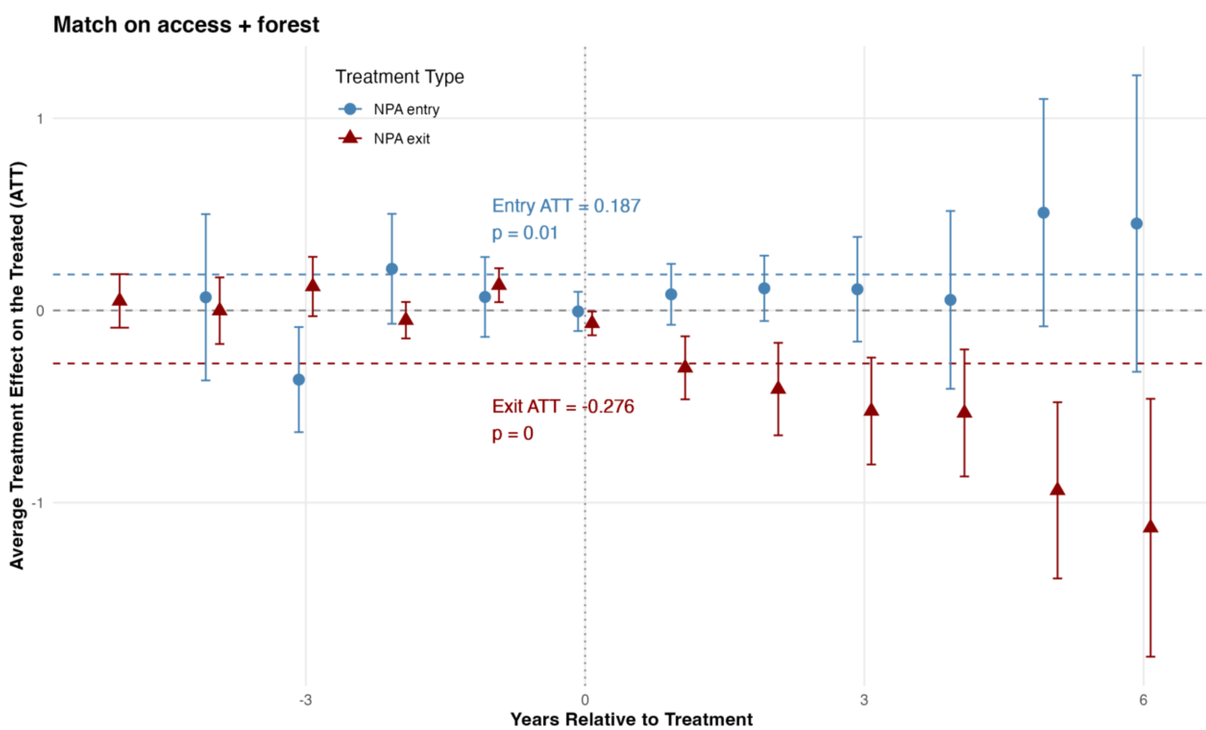


Figure A2.7. FEct entry/exit event study after matching on access and 2009 forest cover.



A2.5 Robustness: exclude zero undisturbed forest in 2008

Table A2.3 and Table A2.4 repeat these designs after dropping points with no undisturbed forest in 2008.

Table A2.3. Robustness: same matching TWFE designs as Table A2.2, excluding points with zero undisturbed forest in 2008.

	Dependent variable: new forest disturbance in year t (percentage points)				
	(1)	(2)	(3)	(4)	(5)
NPA presence	0.5189***	0.3789*	0.3906*	0.3958*	0.3582*
	(0.1108)	(0.1659)	(0.1566)	(0.1626)	(0.1577)
Unit of analysis	Point-year	Point-year	Point-year	Point-year	Point-year
Observations	255,122	86,534	86,534	86,534	86,534
R^2	0.134	0.134	0.135	0.135	0.135
Estimator	TWFE	TWFE	TWFE	TWFE	TWFE

Note: TWFE estimates of NPA presence on forest disturbance (percentage points). Columns: (1) full sample; (2) within municipality only (no additional covariates); (3) match on 2015 city access (hours); (4) match on 2009 barangay undisturbed forest fraction; (5) match on access and forest. Point and year fixed effects; standard errors clustered by barangay. Matching at the 2009 cross-section (1:1 nearest neighbor, no replacement, exact municipality). $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A2.4. Robustness: same matching FEct designs as Figure A2.3, excluding points with zero undisturbed forest in 2008.

	Dependent variable: new forest disturbance in year t (percentage points)				
	(1)	(2)	(3)	(4)	(5)
NPA entry	0.7000***	0.5105**	0.6303***	0.5717**	0.4884*
	(0.1315)	(0.1864)	(0.1768)	(0.2067)	(0.1917)
NPA exit	−0.7966***	−0.5779**	−0.5311**	−0.5155**	−0.5795**
	(0.1259)	(0.2085)	(0.1767)	(0.1809)	(0.1804)
Unit of analysis	Point-year	Point-year	Point-year	Point-year	Point-year
Observations	243,432	82,586	82,250	82,418	82,159
Treated units	13,799	5,617	5,569	5,593	5,556
Estimator	FEct	FEct	FEct	FEct	FEct

Note: FEct average treatment effects of NPA entry and exit on forest disturbance (percentage points). Columns: (1) full sample; (2) within municipality only (no additional covariates); (3) match on 2015 city access (hours); (4) match on 2009 barangay undisturbed forest fraction; (5) match on access and forest. Two-way fixed effects (`fect` method `fe`). Matching at the 2009 cross-section (1:1 nearest neighbor, no replacement, exact municipality). $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

A2.6 Randomization inference

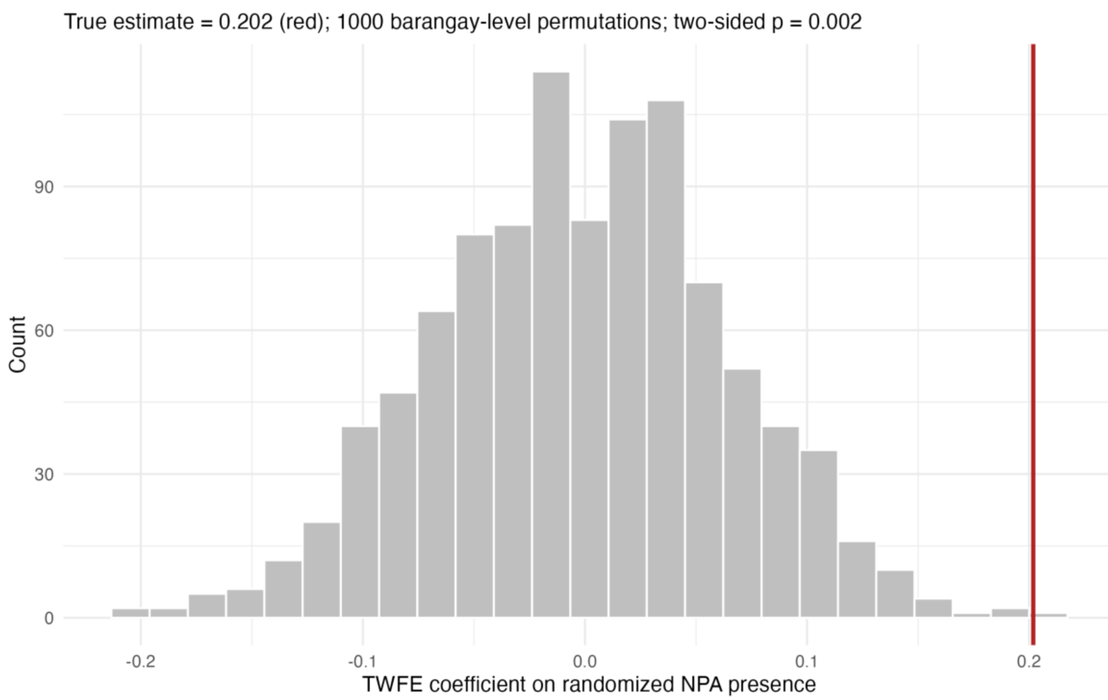
Table A2.5 and Figure A2.8 report barangay-level randomization inference for the main TWFE (see table caption).

Table A2.5. Randomization inference for the main TWFE of NPA presence on forest disturbance.

Dependent variable: new forest disturbance in year t (percentage points)	Estimate
True TWFE (NPA presence)	0.2018
Mean placebo coefficient	−0.0016
SD placebo coefficients	0.0654
Share $\lvert\text{placebo}\rvert \geq \lvert\text{true}\rvert$	0.002
Permutations	1,000

Note: The two-sided p-value is the share of placebo coefficients with absolute value at least as large as the true estimate. Sample: point-years with non-missing NPA control, 2009–2015. Barangay-level NPA presence paths are randomly reassigned across barangays (1,000 permutations, seed 20260601); each draw re-estimates the point-year TWFE with point and year fixed effects.

Figure A2.8. Randomization inference: histogram of placebo TWFE coefficients from barangay-level treatment-path permutations; red line is the true estimate.



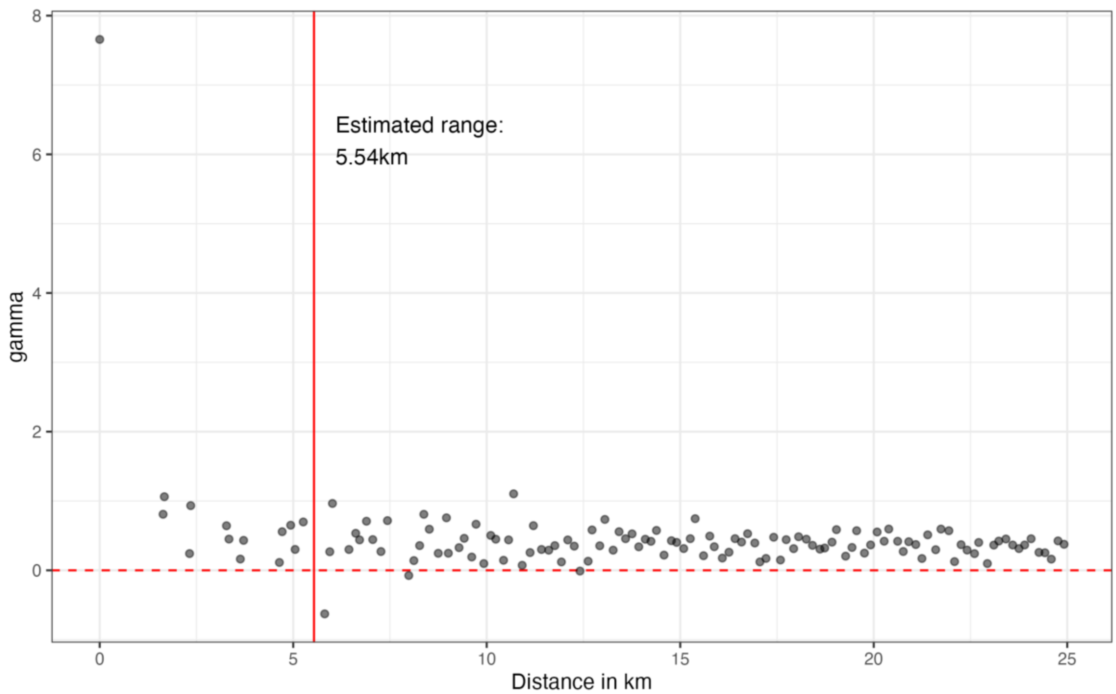
A3 Spatial diagnostics

This appendix reports the covariogram used to choose the Conley cutoff, the main specifications with Conley standard errors at that and alternative cutoffs, and a spatial autoregressive model estimated on a random subsample.

A3.1 Covariogram range

Figure A3.1 plots the empirical covariogram of the TWFE residuals for annual forest disturbance (2015 subsample of up to 20,000 points; distances up to 25 km). The estimated correlation range is 5.5 km, which is why we use a 5 km cutoff for the Conley standard errors below.

Figure A3.1. Empirical covariogram of the TWFE residuals for annual forest disturbance, 2015 subsample of up to 20,000 points (distances $\leq$ 25 km). The vertical line marks the estimated correlation range (5.5 km), which motivates the 5 km Conley cutoff.



A3.2 Conley standard errors

Table A3.1 reports point-year TWFE estimates with Conley standard errors using a 5 km cutoff (see table caption for specifications by column). This reflects the appropriate bandwidth for Conley standard errors according to Lehner (2026). Table A3.2 reports Conley standard errors for the NPA presence and interaction coefficients at alternative cutoffs (10–25 km).

Table A3.1. Point-year TWFE of forest disturbance with Conley standard errors.

Dependent variable: new forest disturbance in year t (percentage points)			
	(1)	(2)	(3)
NPA presence	0.2018***	0.2788***	0.1959***
	(0.0477)	(0.0548)	(0.0479)
NPA × IP		−0.2497*	
		(0.1028)	
NPA × ln(landslide dist.)			0.1390**
			(0.0536)
Outcome mean	0.588	0.588	0.588
Unit of analysis	Point-year	Point-year	Point-year
Observations	712,341	712,341	712,341
R^2	0.13970	0.13972	0.13972
Within R^2	4.38×10^{-5}	5.87×10^{-5}	5.92×10^{-5}
Estimator	TWFE	TWFE	TWFE

Note: Columns: (1) NPA presence; (2) NPA presence interacted with IP presence; (3) NPA presence interacted with log distance to landslide. Point and year fixed effects. Parentheses report Conley standard errors (spherical distance; 5 km cutoff, on average 25 neighboring points). $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

A3.3 Spatial autoregressive model

To assess whether spatial dependence in forest disturbance changes our inferences about NPA presence, we estimate a static spatial autoregressive (SAR) panel model with two-way fixed effects:

$$Y_{it} = \rho(WY)_{it} + \beta\,\mathrm{NPA}_{it} + \alpha_i + \alpha_t + \varepsilon_{it}, \tag{3}$$

where Y_{it} is forest disturbance, NPA_{it} is binary NPA presence, W is a row-standardized spatial weights matrix based on distance-band adjacency within 5 km (EPSG:32651), with $k = 8$ minimum nearest-neighbor threshold for isolated points, and ρ is the spatial autoregressive parameter. Models are estimated by maximum likelihood with a within transformation using `splm::spml`. Because full-sample SAR estimation would require over 2 TB of RAM and over 7 days to fit, we use a balanced random subsample of 75,000 forest points (525,000 point-years, 2009–2015). Because the models are fit on a subsample, the table also reports the non-spatial TWFE estimate on the same subsample, so that the two can be compared directly.

Table A3.3 reports the SAR estimate with NPA × IP presence and NPA × log landslide distance interactions (landslide distance mean-centered), alongside a TWFE estimate on the same subsample. Although ρ is positive and significant, the NPA presence and interaction coefficients are essentially unchanged relative to TWFE estimates on the same subsample, suggesting that accounting for a spatial lag in the outcome does not alter the main substantive conclusions.

Table A3.2. Conley standard errors at alternative spherical-distance cutoffs for coefficients from the point-year TWFE specifications in Table A3.1.

	Dependent variable: new forest disturbance in year t (percentage points)			
Conley cutoff (km)	Coefficient	Estimate	Conley SE	Barangay-clustered SE
Main TWFE (Table A3.1, column (1))				
10	NPA presence	0.2018	0.0566	0.0469
15	NPA presence	0.2018	0.0648	0.0469
20	NPA presence	0.2018	0.0698	0.0469
25	NPA presence	0.2018	0.0739	0.0469
NPA × IP (Table A3.1, column (2))				
10	NPA presence	0.2788	0.0647	0.0541
10	NPA × IP	−0.2497	0.1174	0.1033
15	NPA presence	0.2788	0.0731	0.0541
15	NPA × IP	−0.2497	0.1324	0.1033
20	NPA presence	0.2788	0.0790	0.0541
20	NPA × IP	−0.2497	0.1435	0.1033
25	NPA presence	0.2788	0.0829	0.0541
25	NPA × IP	−0.2497	0.1540	0.1033
NPA × log landslide (Table A3.1, column (3))				
10	NPA presence	−1.1426	0.6201	0.4874
10	NPA × ln(landslide dist.)	0.1390	0.0633	0.0499
15	NPA presence	−1.1426	0.7171	0.4874
15	NPA × ln(landslide dist.)	0.1390	0.0730	0.0499
20	NPA presence	−1.1426	0.7827	0.4874
20	NPA × ln(landslide dist.)	0.1390	0.0799	0.0499
25	NPA presence	−1.1426	0.8544	0.4874
25	NPA × ln(landslide dist.)	0.1390	0.0871	0.0499

Note: The main table uses a 5 km cutoff; rows report 10, 15, 20, 25 km cutoffs. Barangay-clustered standard errors are reported for comparison.

Table A3.3. Spatial autoregressive (SAR) panel model of NPA presence on forest disturbance, estimated with `splm::spml` (maximum likelihood, `within` transformation, two-way fixed effects).

Dependent variable: new forest disturbance in year t (percentage points)		
	Estimate	(SE)
Spatial lag ρ	0.226	(0.003)
NPA presence	0.201	(0.050)
NPA × IP	−0.241	(0.088)
NPA × ln(landslide dist.)	0.107	(0.048)
TWFE: NPA presence	0.256	(0.058)
TWFE: NPA × IP	−0.281	(0.108)
TWFE: NPA × ln(landslide dist.)	0.131	(0.054)

Note: Spatial weights are row-standardized distance-band adjacency (0–5.0 km, EPSG:32651) with k=8 nearest-neighbor minimum for isolated points, regardless of distance. Balanced random subsample of 75,000 points (525,000 point-years, 2009–2015). The TWFE rows report the non-spatial estimates on the same subsample with barangay-clustered standard errors.

A4 Alternative specifications

This appendix reports alternative conflict measures (UCDP and BCMS violent incidents), the main specifications with an NPA × city-access control, with barangay fixed effects, with the three-level NPA control coding, and at the barangay unit, robustness checks excluding points with no undisturbed forest in 2008, FEct event studies by subsample, and the infrastructure analyses.

A4.1 UCDP violent incidents

Table A4.1 estimates point-year TWFE with progressively added UCDP violent-incident lags (see table caption).

Table A4.1. Point-year TWFE of forest disturbance on UCDP violent-incident indicators with progressively added lags.

Dependent variable: new forest disturbance in year t (percentage points)				
	(1)	(2)	(3)	(4)
Violent incident (t)	−0.0629	−0.0578	−0.0076	0.0121
	(0.1960)	(0.2078)	(0.2087)	(0.2100)
Violent incident (t−1)		0.0525	0.0941	0.1231
		(0.1974)	(0.1979)	(0.2013)
Violent incident (t−2)			0.4374	0.4569
			(0.4041)	(0.4001)
Violent incident (t−3)				0.2056
				(0.2449)
Outcome mean	0.588	0.588	0.588	0.588
Unit of analysis	Point-year	Point-year	Point-year	Point-year
Observations	712,341	712,341	712,341	712,341
R^2	0.140	0.140	0.140	0.140
Estimator	TWFE	TWFE	TWFE	TWFE

Note: Columns: (1) violent incident in year t only; (2) t and $t-1$; (3) t through $t-2$; (4) t through $t-3$. Point and year fixed effects; standard errors clustered by barangay.

A4.2 BCMS violent incidents

Table A4.2 estimates point-year TWFE with progressively added BCMS violent-incident lags (see table caption).

Table A4.2. Point-year TWFE of forest disturbance on BCMS NPA indicators with progressively added lags.

Dependent variable:	new forest disturbance in year t (percentage points)			
	(1)	(2)	(3)	(4)
BCMS NPA (t)	−0.0681	0.0371	−0.3670	−0.7878
	(0.3720)	(0.4462)	(0.6741)	(1.1449)
BCMS NPA (t−1)		0.5479	0.3540	−1.1401
		(0.4916)	(0.6845)	(0.8935)
BCMS NPA (t−2)			−0.6509	−0.9633
			(0.5138)	(0.8911)
BCMS NPA (t−3)				−0.2035
				(0.7917)
Unit of analysis	Point-year	Point-year	Point-year	Point-year
Observations	45,185	36,148	27,111	18,074
R^2	0.193	0.240	0.321	0.492
Estimator	TWFE	TWFE	TWFE	TWFE

Note: Columns: (1) BCMS NPA in year t only; (2) t and $t-1$; (3) t through $t-2$; (4) t through $t-3$. Point and year fixed effects; standard errors clustered by barangay.

A4.3 NPA presence and violent incidents

Table A4.3 compares NPA presence and UCDP and BCMS violent incidents (see table caption). Note that BCMS data covers a fraction of total area, so N is much smaller for those specifications.

A4.4 Main paper TWFE with NPA × city access (point FE)

Table A4.4 reports the main four NPA presence specifications with point and year fixed effects, controlling for NPA × city access in every column. Table A4.5 repeats the same designs after dropping points with zero undisturbed forest in 2008.

A4.5 Main paper TWFE with barangay FE (point unit)

Table A4.6 replicates the main paper point-level TWFE with barangay fixed effects. This is to mitigate the possible error introduced by a forest disturbance acting as an absorbing state in the outcome variable (see Garcia and Heilmayr, 2024).

Table A4.3. Point-year TWFE of forest disturbance: NPA presence, UCDP violent incidents, and BCMS NPA (contemporaneous measures).

	Dependent variable: new forest disturbance in year t (percentage points)				
	(1)	(2)	(3)	(4)	(5)
NPA presence	0.2018***		0.2028***		0.1772
	(0.0469)		(0.0470)		(0.2772)
UCDP violent incident		−0.0629	−0.0865		
		(0.1960)	(0.1955)		
BCMS violent incident				0.1923	0.1943
				(0.1914)	(0.1897)
Unit of analysis	Point-year	Point-year	Point-year	Point-year	Point-year
Observations	712,341	712,341	712,341	45,245	45,245
R^2	0.140	0.140	0.140	0.193	0.193
Estimator	TWFE	TWFE	TWFE	TWFE	TWFE

Note: Columns: (1) NPA presence only; (2) UCDP violent incident only; (3) NPA and UCDP jointly; (4) BCMS violent incidents only; (5) BCMS violent incidents and NPA presence jointly. Point and year fixed effects; standard errors clustered by barangay. Because the BCMS data only covers 6.4 percent of the total sample, the NPA coefficient loses significance but is qualitatively similar to the coefficient from the whole sample. $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A4.4. Point-level TWFE of forest disturbance with point and year fixed effects.

	Dependent variable: new forest disturbance in year t (percentage points)			
	(1)	(2)	(3)	(4)
NPA presence	0.1879***	0.2710***	0.1863***	0.2679***
	(0.0460)	(0.0524)	(0.0459)	(0.0524)
NPA × city access	0.0237+	0.0306*	0.0172	0.0241+
	(0.0128)	(0.0130)	(0.0127)	(0.0128)
NPA × IP		−0.2828**		−0.2773**
		(0.1068)		(0.1057)
NPA × ln(landslide dist.)			0.1272*	0.1235*
			(0.0495)	(0.0488)
Outcome mean	0.588	0.588	0.588	0.588
Unit of analysis	Point-year	Point-year	Point-year	Point-year
Observations	712,341	712,341	712,341	712,341
R^2	0.140	0.140	0.140	0.140
Estimator	TWFE	TWFE	TWFE	TWFE

Note: Columns: (1) NPA presence with NPA × city access; (2) add NPA × IP; (3) add NPA × log landslide distance; (4) both IP and landslide interactions (with NPA × city access). All columns control for NPA × city access (main effects of time-invariant covariates are absorbed by point FE). Mean-centered 2015 city access (hours) and log landslide distance; standard errors clustered by barangay. $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A4.5. Robustness: same designs as Table A4.4, excluding points with zero undisturbed forest in 2008.

	Dependent variable: new forest disturbance in year t (percentage points)			
	(1)	(2)	(3)	(4)
NPA presence	0.4590***	0.6502***	0.4563***	0.6481***
	(0.1129)	(0.1374)	(0.1123)	(0.1371)
NPA × city access	0.0918***	0.1002***	0.0761***	0.0845***
	(0.0214)	(0.0218)	(0.0216)	(0.0220)
NPA × IP		−0.5069*		−0.5087*
		(0.2110)		(0.2086)
NPA × ln(landslide dist.)			0.2833*	0.2844*
			(0.1196)	(0.1181)
Unit of analysis	Point-year	Point-year	Point-year	Point-year
Observations	255,122	255,122	255,122	255,122
R^2	0.134	0.134	0.134	0.134
Estimator	TWFE	TWFE	TWFE	TWFE

Note: Columns: (1) NPA presence with NPA × city access; (2) add NPA × IP; (3) add NPA × log landslide distance; (4) both IP and landslide interactions (with NPA × city access). Point-level TWFE of forest disturbance with point and year fixed effects. All columns control for NPA × city access (main effects of time-invariant covariates are absorbed by point FE). Mean-centered 2015 city access (hours) and log landslide distance; standard errors clustered by barangay. $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

A4.6 NPA control categories (point + year FE)

Table A4.7 repeats the main interaction specifications using the ordered three-level NPA control coding (clear/threatened/influenced) with point and year fixed effects (same sample as Table A4.6).

A4.7 Main paper TWFE at barangay unit

Table A4.8 estimates barangay-year TWFE at the barangay unit with progressively richer NPA presence specifications (see table caption).

A4.8 Robustness: exclude zero undisturbed forest in 2008

Tables A4.9 and A4.10 repeat the point-level barangay-FE and barangay-unit designs from Sections A4.5–A4.7 after dropping points (or barangays) with no undisturbed forest in 2008.

Table A4.6. Main paper point-level TWFE of forest disturbance with barangay and year fixed effects.

	Dependent variable: new forest disturbance in year t (percentage points)			
	(1)	(2)	(3)	(4)
NPA presence	0.2018***	0.2788***	0.1944***	0.2720***
	(0.0469)	(0.0541)	(0.0466)	(0.0536)
NPA × IP		−0.2496*		−0.2518*
		(0.1033)		(0.1019)
NPA × ln(landslide dist.)			0.1727**	0.1735**
			(0.0582)	(0.0580)
City access (hrs)	−0.1925***	−0.1925***	−0.1935***	−0.1935***
	(0.0112)	(0.0112)	(0.0113)	(0.0113)
2008 forest stock (m^2)	2.1378***	2.1378***	2.1373***	2.1373***
	(0.0497)	(0.0497)	(0.0497)	(0.0497)
Outcome mean	0.588	0.588	0.588	0.588
Unit of analysis	Point-year	Point-year	Point-year	Point-year
Observations	712,341	712,341	712,341	712,341
R^2	0.052	0.052	0.052	0.052
Estimator	TWFE	TWFE	TWFE	TWFE

Note: Columns: (1) NPA presence with access and forest controls; (2) NPA presence with NPA × IP presence; (3) NPA presence with NPA × log landslide distance; (4) NPA presence with both IP and landslide interactions. Mean-centered 2015 city access (hours) and 2008 undisturbed forest stock (m^2); standard errors clustered by barangay. $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A4.7. Point-level TWFE of forest disturbance with point and year fixed effects, using ordered NPA control categories (0 = clear; 1 = threatened; 2 = influenced) with clear as the omitted baseline.

	Dependent variable: new forest disturbance in year t (percentage points)			
	(1)	(2)	(3)	(4)
NPA threatened	0.1807***	0.2082***	0.1750***	0.2057***
	(0.0522)	(0.0622)	(0.0519)	(0.0618)
NPA influenced	0.2387**	0.4246***	0.2323**	0.4119***
	(0.0887)	(0.1041)	(0.0888)	(0.1040)
Threatened × IP		−0.0894		−0.1007
		(0.1119)		(0.1112)
Influenced × IP		−0.5226**		−0.5033**
		(0.1913)		(0.1887)
Threatened × ln(landslide dist.)			0.1236*	0.1221*
			(0.0513)	(0.0518)
Influenced × ln(landslide dist.)			0.1759	0.1539
			(0.1115)	(0.1089)
Outcome mean	0.588	0.588	0.588	0.588
Unit of analysis	Point-year	Point-year	Point-year	Point-year
Observations	712,341	712,341	712,341	712,341
R^2	0.140	0.140	0.140	0.140
Estimator	TWFE	TWFE	TWFE	TWFE

Note: Columns: (1) NPA control categories only; (2) NPA × IP presence interactions; (3) NPA × log landslide distance interactions; (4) both interactions. Standard errors clustered by barangay. Same sample and mean-centered covariates as Table A4.6 (2015 city access in hours; 2008 undisturbed forest stock in m^2). $^+p < 0.1$, $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A4.8. Barangay-year TWFE of forest disturbance.

	Dependent variable: new forest disturbance in year t (percentage points)			
	(1)	(2)	(3)	(4)
NPA presence	0.1704***	0.2089***	0.1695***	0.2071***
	(0.0185)	(0.0195)	(0.0185)	(0.0195)
NPA × city access	−0.0175	−0.0060	−0.0218*	−0.0104
	(0.0108)	(0.0112)	(0.0108)	(0.0112)
NPA × IP		−0.2734***		−0.2663***
		(0.0544)		(0.0540)
NPA × ln(landslide dist.)			0.0901***	0.0861***
			(0.0183)	(0.0181)
Unit of analysis	Barangay-year	Barangay-year	Barangay-year	Barangay-year
Observations	189,182	189,182	189,182	189,182
R^2	0.262	0.262	0.262	0.262
Estimator	TWFE	TWFE	TWFE	TWFE

Note: Columns: (1) NPA presence with NPA × city access; (2) add NPA × IP; (3) add NPA × log landslide distance; (4) both IP and landslide interactions (with NPA × city access). Barangay and year fixed effects; standard errors clustered by barangay. All columns control for NPA × city access (main effects of time-invariant covariates are absorbed by barangay FE). $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A4.9. Robustness: same designs as Table A4.6, excluding points with zero undisturbed forest in 2008.

	Dependent variable: new forest disturbance in year t (percentage points)			
	(1)	(2)	(3)	(4)
NPA presence	0.5189***	0.6792***	0.4991***	0.6690***
	(0.1108)	(0.1422)	(0.1085)	(0.1395)
NPA × IP		−0.4131+		−0.4386*
		(0.2194)		(0.2142)
NPA × ln(landslide dist.)			0.4348***	0.4418***
			(0.1294)	(0.1288)
City access (hrs)	−0.1890***	−0.1890***	−0.1917***	−0.1917***
	(0.0157)	(0.0157)	(0.0159)	(0.0158)
Unit of analysis	Point-year	Point-year	Point-year	Point-year
Observations	255,122	255,122	255,122	255,122
R^2	0.065	0.065	0.065	0.065
Estimator	TWFE	TWFE	TWFE	TWFE

Note: Columns: (1) NPA presence with access and forest controls; (2) NPA presence with NPA × IP presence; (3) NPA presence with NPA × log landslide distance; (4) NPA presence with both IP and landslide interactions. Main paper point-level TWFE of forest disturbance with barangay and year fixed effects. Mean-centered 2015 city access (hours) and 2008 undisturbed forest stock (m^2); standard errors clustered by barangay. $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A4.10. Robustness: same designs as Table A4.8, restricting to barangays with at least one sample point that had positive undisturbed forest in 2008.

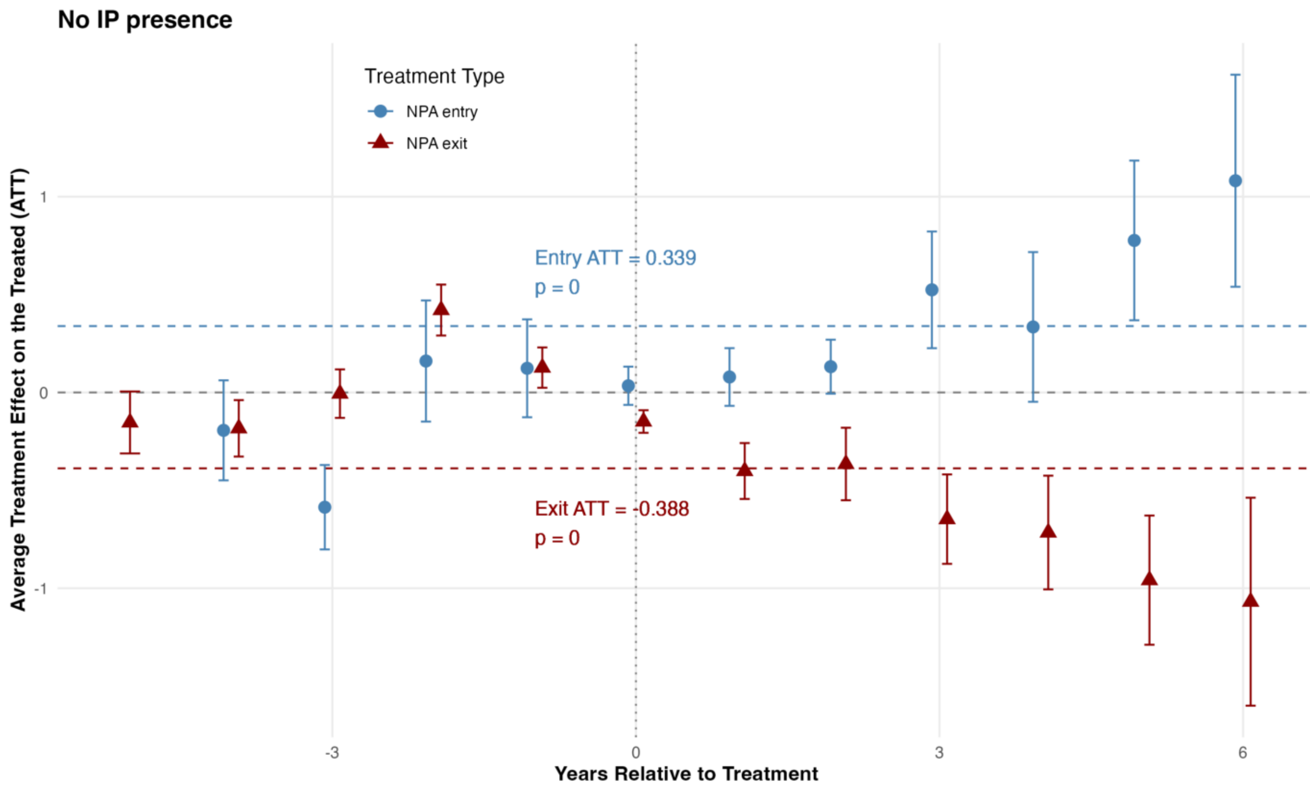
	Dependent variable: new forest disturbance in year t (percentage points)			
	(1)	(2)	(3)	(4)
NPA presence	0.1017*	0.1614***	0.1048*	0.1638***
	(0.0446)	(0.0486)	(0.0447)	(0.0488)
NPA × city access	0.0662***	0.0722***	0.0640***	0.0703***
	(0.0167)	(0.0168)	(0.0170)	(0.0171)
NPA × IP		−0.2874***		−0.2864***
		(0.0786)		(0.0784)
NPA × ln(landslide dist.)			0.0263	0.0229
			(0.0335)	(0.0332)
Unit of analysis	Barangay-year	Barangay-year	Barangay-year	Barangay-year
Observations	67,949	67,949	67,949	67,949
R^2	0.232	0.232	0.232	0.232
Estimator	TWFE	TWFE	TWFE	TWFE

Note: Columns: (1) NPA presence with NPA × city access; (2) add NPA × IP; (3) add NPA × log landslide distance; (4) both IP and landslide interactions (with NPA × city access). Barangay-year TWFE of forest disturbance. Barangay and year fixed effects; standard errors clustered by barangay. All columns control for NPA × city access (main effects of time-invariant covariates are absorbed by barangay FE). $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

A4.9 FEct entry/exit by subsample

Figures A4.1–A4.4 report combined NPA entry/exit dynamic treatment effects estimated with FEct (two-way fixed effects) on point-level subsamples. Treatments are first-year NPA entry and first-year NPA exit; the outcome is the annual forest disturbance rate. Subsamples are defined by whether the point lies in a barangay containing an Indigenous community and by quintiles of distance to the nearest landslide. Event-time periods with fewer than 100 treated observations are dropped.

Figure A4.1. FEct entry/exit event study: points in barangays without an Indigenous community.



A4.10 Infrastructure and forest disturbance

Table A4.11 column 1 shows the raw relationships between the IP community, landslide distance, and city access variables to deforestation without colinear unit fixed effects. IP communities experience higher rates of forest disturbance, areas distant from landslides experience lower forest disturbance, and city access is not significantly associated with disturbance. Column 2 shows the spatial patterns of where infrastructure development as part of Kalahi-CIDSS or PAMANA is associated with forest disturbance. These infrastructure projects are associated with higher rates of disturbance in areas with IP communities and closer to landslides, the opposite pattern from what we find with NPA-associated disturbances.

Figures A4.5 through A4.7 report FEct event studies of infrastructure on forest disturbance, using the same estimator and point-year panel as Figure 2 of the main text. Treatment is an indicator that a PAMANA or Kalahi-CIDSS project is active in the point's barangay in that year

Figure A4.2. FEct entry/exit event study: points in barangays with an Indigenous community.

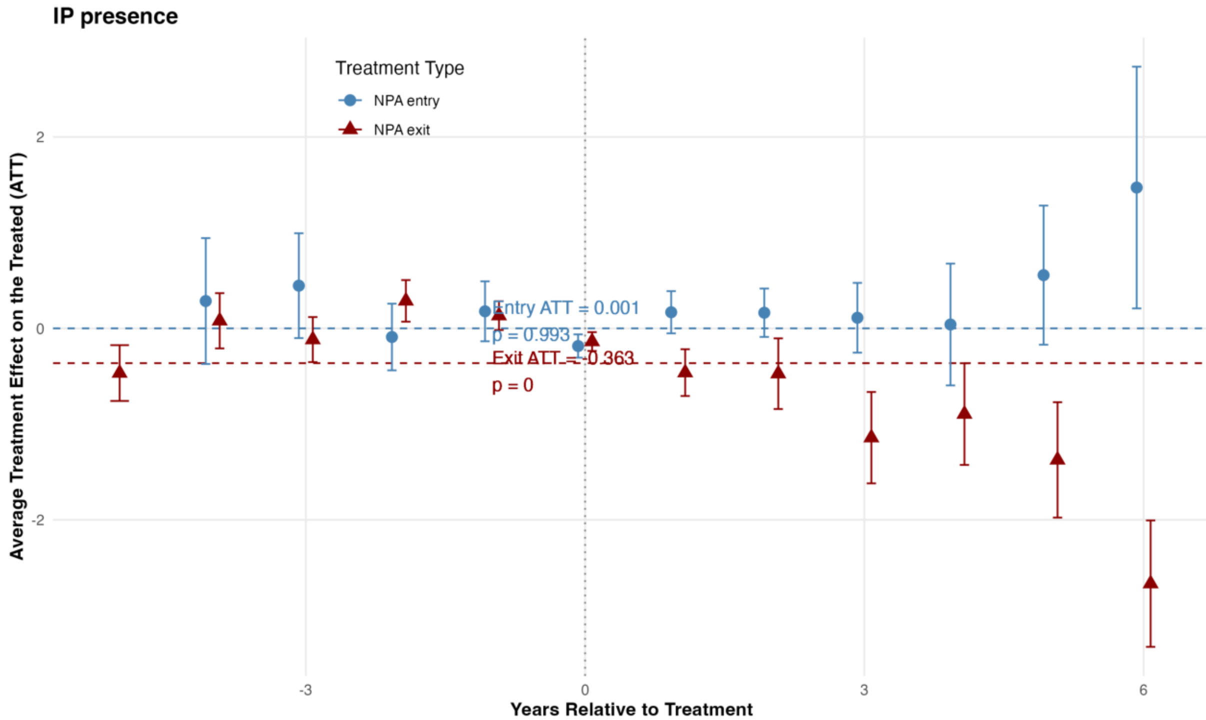


Figure A4.3. FEct entry/exit event study: points in the bottom quintile of distance to the nearest landslide (closest areas).

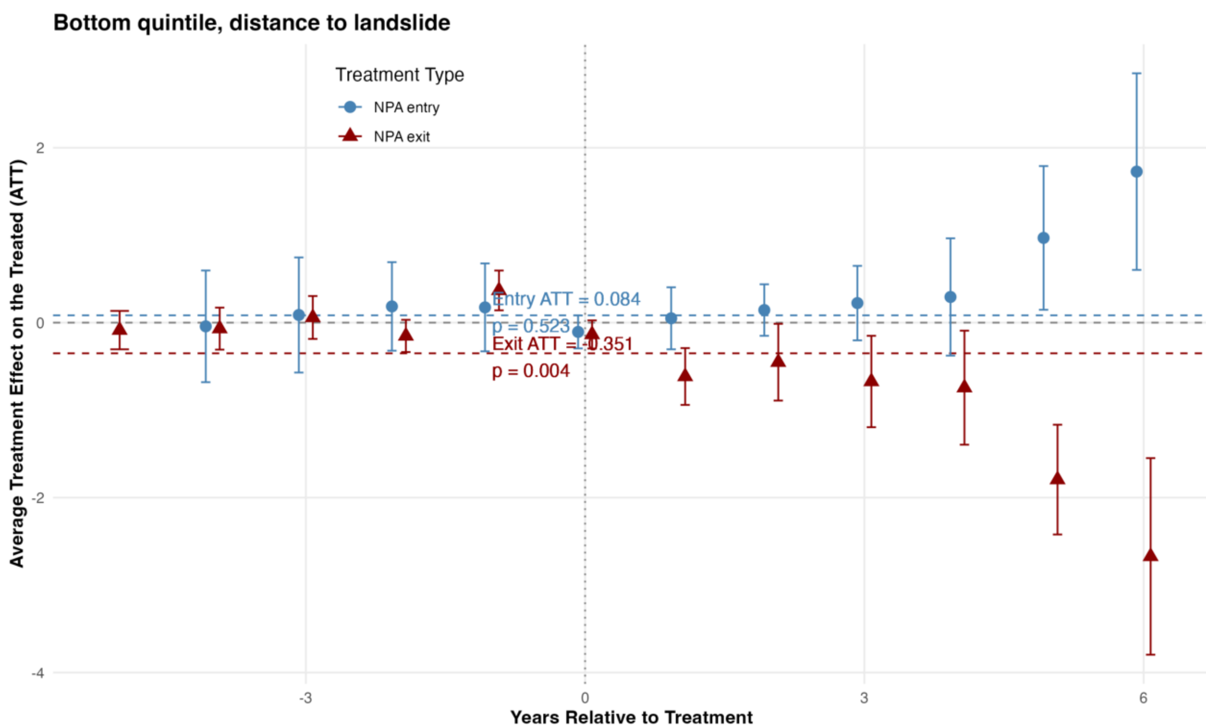
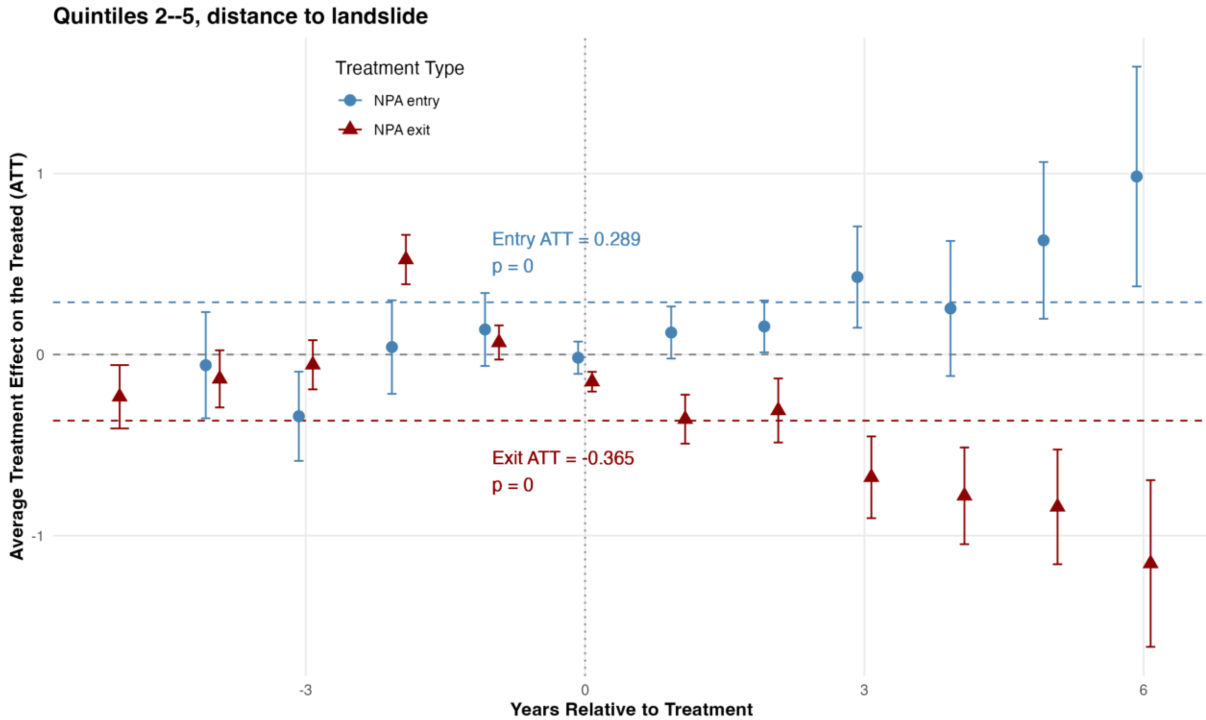


Figure A4.4. FEct entry/exit event study: points in quintiles 2–5 of distance to the nearest landslide (farther areas).



(Figure A4.5), or a Kalahi-CIDSS road project specifically (Figures A4.6 and A4.7), with event time measured from the first treated year. Infrastructure effects on disturbance are small in the first two years after a project begins and grow thereafter, while Kalahi-CIDSS roads show no detectable effect on either deforestation or degradation.

Table A4.11. Forest disturbance is higher in Indigenous communities and near landslides, where state infrastructure projects also have larger effects.

	Dependent variable: new forest disturbance in year t (percentage points)	
	(1) Descriptives: by area	(2) State comparison: infrastructure
Infrastructure × IP community		0.571^{***}
		(0.144)
Infrastructure × Landslide dist.		-0.087^{+}
		(0.047)
Infrastructure × City access		0.015
		(0.018)
Any infrastructure		0.035
		(0.051)
Indigenous (IP) community	0.121^{**}	
	(0.042)	
Landslide distance (log)	-0.041^{***}	
	(0.012)	
City access (hours, 2015)	−0.010	
	(0.007)	
Outcome mean	0.588	0.588
Unit of analysis	Point-year	Point-year
Observations	712,341	712,341
R^2	0.002	0.140
Estimator	OLS (year FE)	TWFE

Note: Model 1 is descriptive: with year fixed effects only, it shows that disturbance is higher on average in Indigenous barangays and near landslides. These characteristics are fixed within a point, so Model 2 absorbs their main effects in point fixed effects; they enter only through interactions with infrastructure. Model 2 estimates larger effects of state infrastructure projects on forest disturbance in Indigenous communities and, with weaker statistical evidence, nearer landslides. Any infrastructure indicates a Kalahi-CIDSS or PAMANA project active in the barangay that year; city access is travel time in hours to the nearest city in 2015, mean-centered. Standard errors clustered at the barangay level in parentheses. $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Figure A4.5. Event study: All infrastructure effects on forest disturbance rate.

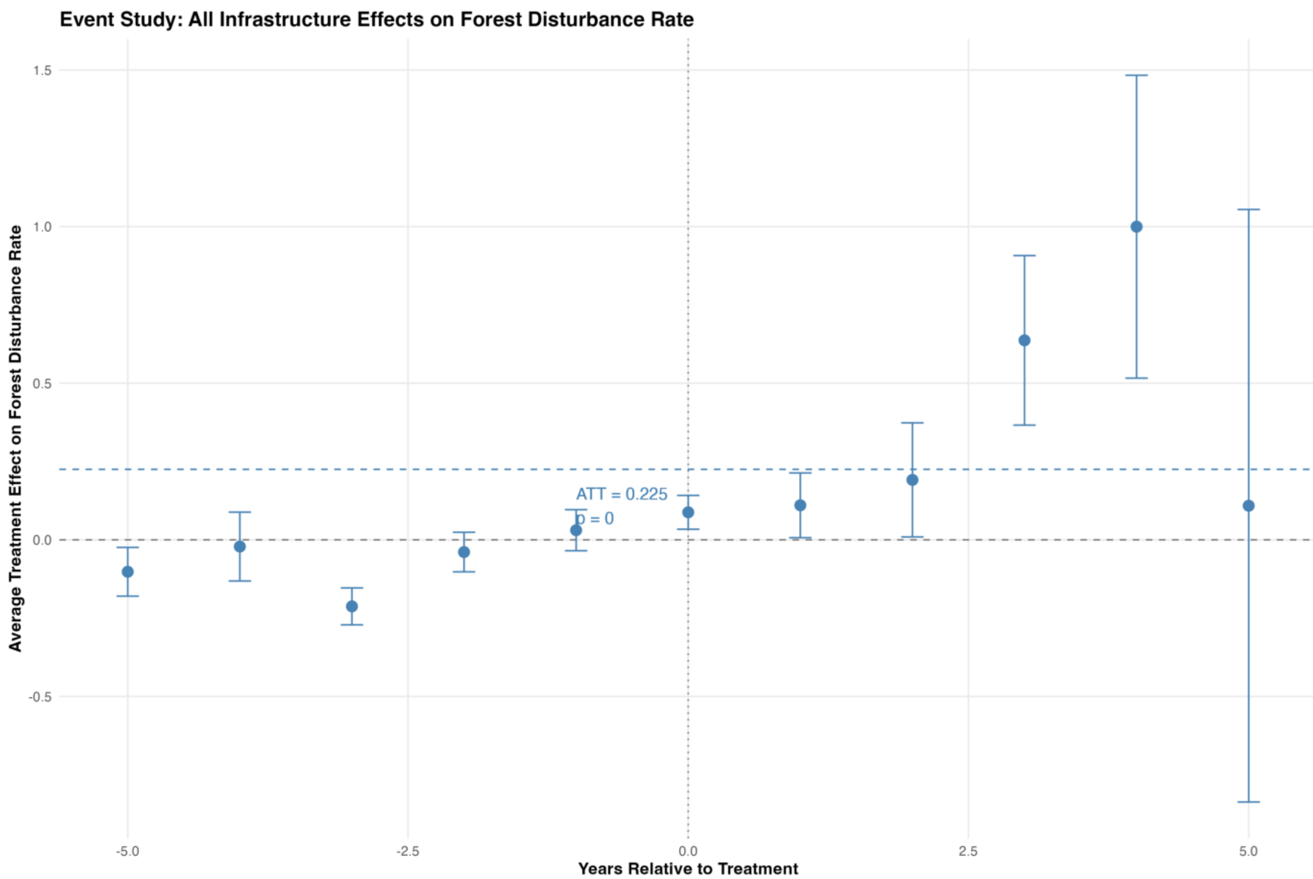


Figure A4.6. Event study: Kalahi-CIDSS roads effects on deforestation rate.

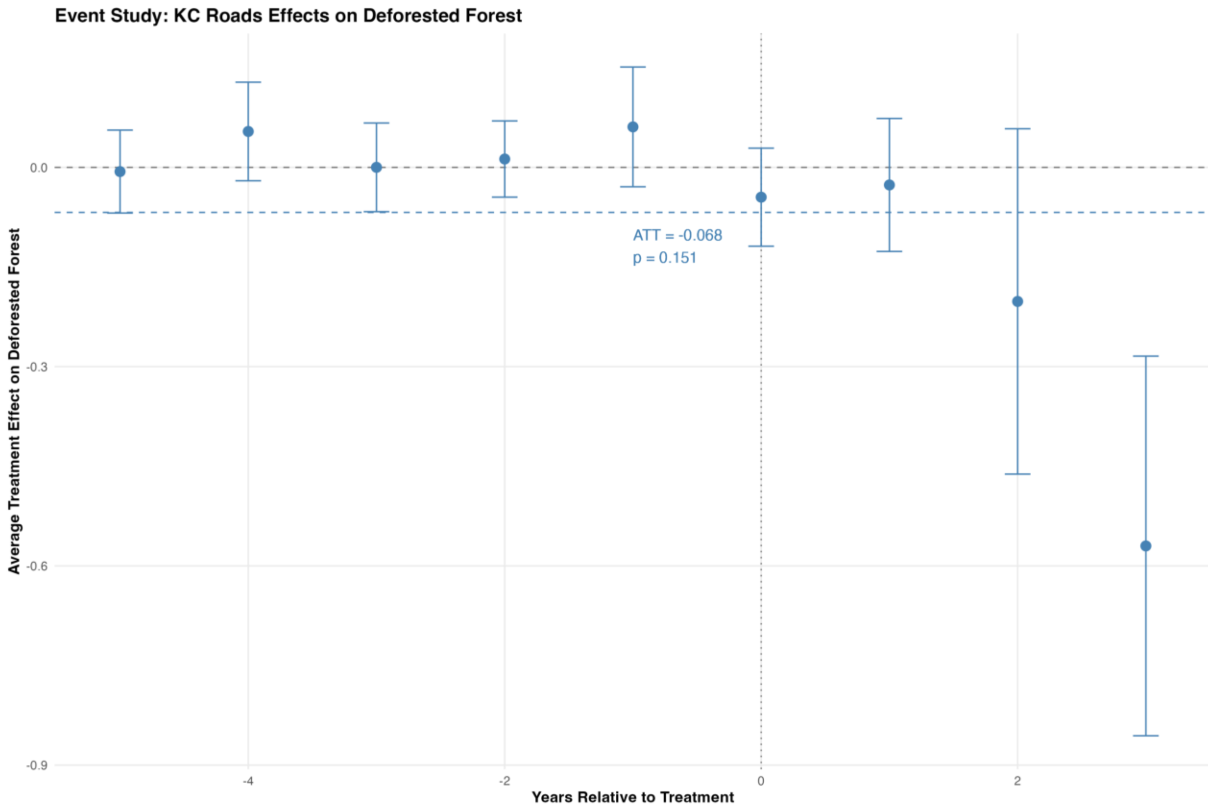
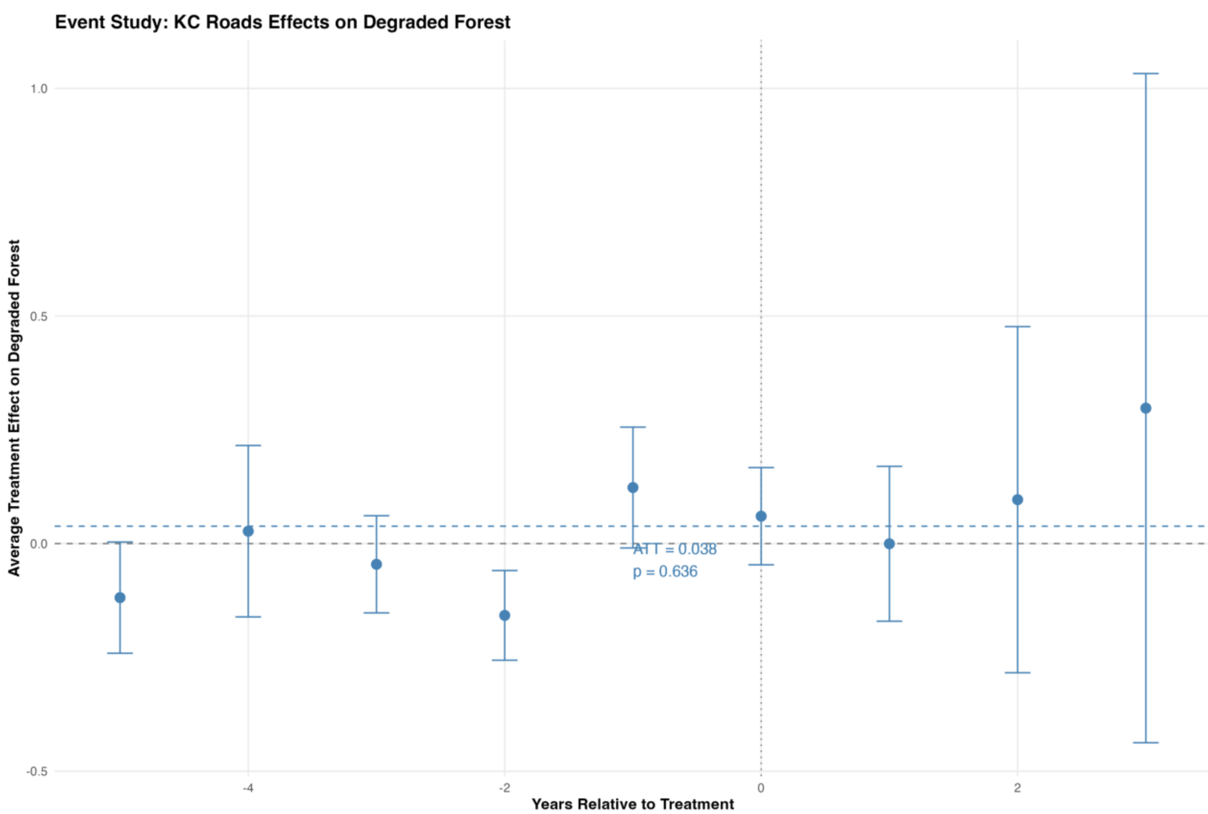


Figure A4.7. Event study: Kalahi-CIDSS roads effects on degradation rate.



A4.11 NPA presence and infrastructure outcomes

Tables A4.12 and A4.13 test whether NPA control predicts infrastructure outcomes using TWFE and FEct (see table captions).

Table A4.12. Point-year TWFE of NPA control on binary infrastructure and road project indicators (2009–2015).

	Dependent variable: infrastructure or road project in year t (indicator; one outcome per column)				
	(1)	(2)	(3)	(4)	(5)
NPA presence	−0.0024	−0.0073	−0.0043	−0.0122**	−0.0082
	(0.0056)	(0.0045)	(0.0070)	(0.0039)	(0.0073)
Unit of analysis	Point-year	Point-year	Point-year	Point-year	Point-year
Observations	712,341	712,341	712,341	712,341	712,341
R^2	0.383	0.243	0.338	0.257	0.394
Estimator	TWFE	TWFE	TWFE	TWFE	TWFE

Note: Each column is a separate outcome model. Columns: (1) PAMANA (PAM) infrastructure; (2) PAMANA roads; (3) Kalahi-CIDSS (KC) infrastructure; (4) Kalahi-CIDSS roads; (5) any PAMANA or Kalahi-CIDSS infrastructure. Point and year fixed effects; standard errors clustered by barangay. $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A4.13. FEct average treatment effect of NPA entry on infrastructure outcomes (two-way fixed effects, `fect` method `fe`).

	Dependent variable: infrastructure or road project in year t (indicator; one outcome per row)				
Outcome	ATT	Std. error	p-value	Observations	Treated units
PAM infrastructure	−0.0085	0.0016	0.0000	686,266	31,633
PAM road	−0.0185	0.0011	0.0000	686,266	31,633
KC infrastructure	−0.0295	0.0023	0.0000	686,266	31,633
KC road	−0.0294	0.0015	0.0000	686,266	31,633
Any infrastructure	−0.0316	0.0021	0.0000	686,266	31,633

Note: Point-year panel, 2009–2015. Columns report ATT, standard error, p-value, observations, and treated units. Outcomes match the TWFE table: PAM infrastructure, PAM roads, KC infrastructure, KC roads, and any PAM or KC infrastructure.

Table A4.14 tests whether infrastructure indicators predict NPA exit (point-year TWFE; see table caption). Table A4.15 adds lagged Kalahi-CIDSS road projects to the main specifications, and Table A4.16 adds all lagged infrastructure projects by type; Table A4.17 pools them into a single any-infrastructure indicator with one-, two-, and three-year lags.

Table A4.14. Point-year TWFE of infrastructure on NPA exit.

	Dependent variable: NPA exit (indicator for no NPA presence)				
	(1)	(2)	(3)	(4)	(5)
PAM infrastructure	-3.517×10^{-4}				
	(0.0117)				
PAM road		0.0607			
		(0.0375)			
KC infrastructure			0.0031		
			(0.0079)		
KC road				0.0356***	
				(0.0103)	
Any infrastructure					0.0055
					(0.0069)
Unit of analysis	Point-year	Point-year	Point-year	Point-year	Point-year
Observations	712,341	712,341	712,341	712,341	712,341
R^2	0.669	0.669	0.669	0.669	0.669
Estimator	TWFE	TWFE	TWFE	TWFE	TWFE

Note: Outcome is an indicator for no NPA presence (exit state). Each column is a separate model with one infrastructure regressor. Columns: (1) PAM infrastructure; (2) PAM roads; (3) KC infrastructure; (4) KC roads; (5) any PAM or KC infrastructure. Point and year fixed effects; standard errors clustered by barangay. $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A4.15. Point-level TWFE of forest disturbance with a one-year lagged KC road indicator.

	Dependent variable: new forest disturbance in year t (percentage points)			
	(1)	(2)	(3)	(4)
NPA presence	0.1340*	0.2096**	0.1224*	0.1994**
	(0.0598)	(0.0709)	(0.0594)	(0.0702)
NPA × IP		−0.2352+		−0.2399+
		(0.1282)		(0.1260)
NPA × ln(landslide dist.)			0.2172***	0.2188***
			(0.0621)	(0.0618)
KC road (t−1)	0.0958	0.0976	0.0961	0.0980
	(0.1085)	(0.1084)	(0.1084)	(0.1083)
Unit of analysis	Point-year	Point-year	Point-year	Point-year
Observations	610,578	610,578	610,578	610,578
R^2	0.163	0.163	0.163	0.163
Estimator	TWFE	TWFE	TWFE	TWFE

Note: Mean-centered 2015 city access (hours) and 2008 undisturbed forest stock (m^2). Columns: (1) NPA presence; (2) NPA presence with NPA × IP presence; (3) NPA presence with NPA × log landslide distance; (4) NPA presence with both interactions. Point and year fixed effects; standard errors clustered by barangay. $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A4.16. Point-level TWFE of forest disturbance with one-year lagged infrastructure indicators (PAM infrastructure, PAM road, KC infrastructure, and KC road).

	Dependent variable: new forest disturbance in year t (percentage points)				
	(1)	(2)	(3)	(4)	(5)
NPA presence		0.1407*	0.2094**	0.1297*	0.2000**
		(0.0589)	(0.0707)	(0.0586)	(0.0701)
NPA × IP			−0.2141+		−0.2192+
			(0.1250)		(0.1235)
NPA × ln(landslide dist.)				0.2039***	0.2055***
				(0.0619)	(0.0617)
PAM infrastructure (t−1)	0.3746**	0.3784**	0.3725**	0.3688**	0.3628**
	(0.1231)	(0.1231)	(0.1229)	(0.1235)	(0.1234)
PAM road (t−1)	−0.1060	−0.1000	−0.0920	−0.0916	−0.0834
	(0.2038)	(0.2045)	(0.2039)	(0.2041)	(0.2035)
KC infrastructure (t−1)	0.0941	0.0926	0.0928	0.0908	0.0910
	(0.0895)	(0.0897)	(0.0895)	(0.0894)	(0.0893)
KC road (t−1)	−0.0450	−0.0416	−0.0396	−0.0385	−0.0364
	(0.1381)	(0.1383)	(0.1379)	(0.1379)	(0.1375)
Unit of analysis	Point-year	Point-year	Point-year	Point-year	Point-year
Observations	610,578	610,578	610,578	610,578	610,578
R^2	0.163	0.163	0.163	0.163	0.163
Estimator	TWFE	TWFE	TWFE	TWFE	TWFE

Note: Mean-centered 2015 city access (hours) and 2008 undisturbed forest stock (m^2). Columns: (1) lagged infrastructure only (no NPA); (2) NPA presence; (3) NPA presence with NPA × IP presence; (4) NPA presence with NPA × log landslide distance; (5) NPA presence with both interactions. Point and year fixed effects; standard errors clustered by barangay. $^+p < 0.1$, $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A4.17. Point-level TWFE of forest disturbance with one-, two-, and three-year lags of any PAM or KC infrastructure or road (a pooled indicator, not broken out by type).

	Dependent variable: new forest disturbance in year t (percentage points)				
	(1)	(2)	(3)	(4)	(5)
NPA presence		0.2051***	0.2773***	0.1998***	0.2725***
		(0.0463)	(0.0541)	(0.0463)	(0.0538)
NPA × IP			−0.2344*		−0.2361*
			(0.1001)		(0.0995)
NPA × ln(landslide dist.)				0.1239*	0.1249*
				(0.0497)	(0.0495)
Any infrastructure (t−1)	0.2148***	0.2143***	0.2132***	0.2123***	0.2112***
	(0.0611)	(0.0612)	(0.0609)	(0.0612)	(0.0609)
Any infrastructure (t−2)	0.2419**	0.2442***	0.2433**	0.2419**	0.2410**
	(0.0741)	(0.0741)	(0.0740)	(0.0741)	(0.0739)
Any infrastructure (t−3)	0.2085*	0.2110**	0.2077*	0.2072*	0.2038*
	(0.0814)	(0.0814)	(0.0812)	(0.0815)	(0.0813)
Outcome mean	0.588	0.588	0.588	0.588	0.588
Unit of analysis	Point-year	Point-year	Point-year	Point-year	Point-year
Observations	712,341	712,341	712,341	712,341	712,341
R^2	0.140	0.140	0.140	0.140	0.140
Estimator	TWFE	TWFE	TWFE	TWFE	TWFE

Note: Lags are calendar-year lags using the full point history, so 2009 can use 2006–2008 infrastructure. Columns: (1) lagged any-infrastructure only (no NPA); (2) NPA presence; (3) NPA presence with NPA × IP presence; (4) NPA presence with NPA × log landslide distance; (5) NPA presence with both interactions. Point and year fixed effects; standard errors clustered by barangay. $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

A5 Anticipation

Table A5.1 shows that 1-, 2-, and 3-year lags of the neighbor change variable predict NPA control, though the 1-year lag is by far the most predictive. This indicates that it is reasonable for the NPA to expect that loss of control in adjacent barangays will lead to them being pushed out of their current position.

Table A5.2 reports the regression results that correspond to Figure 4 in columns (2) and (3). In column (1) it shows the coefficients for NPA control scale controlling for changes in NPA control in adjacent barangay without interacting those with neighbor changes, while column (3) shows the coefficients after separating neighbor NPA gain from neighbor NPA loss. NPA change in the year prior does modify the effect of NPA control, but only in threatened areas (not influenced areas) where an increase (decrease) in nearby NPA control is associated with less (more) forest disturbance. This effect is concentrated in areas where the NPA is losing ground rather than in places where they are gaining ground.

Table A5.3 reports a fuller set of specifications than Table A5.2. Model 1 shows the main effects of threatened and influenced NPA control (the middle and highest levels of the AFP scale). Model 2 shows the effects of neighbor changes along with two lags of neighbor changes, with neighbor changes calculated as the sum of year-on-year changes in NPA control across adjacent barangays. Model 3 includes NPA control and neighbor changes in the same regression. Model 4 includes NPA control interacted with a 1-year lag of neighbor change. Model 5 includes NPA control interacted with contemporary neighbor change and a 1-year lag. Model 6 shows interactions with contemporary, 1-year, and 2-year lags.

On average, neighbor changes have only a weak direct relationship with forest disturbance: the contemporaneous net neighbor change is small and positive (0.018 in Model 2, about a tenth of the NPA main effect), and the lagged terms are close to zero. The pattern the theory predicts appears in the interactions. In every specification, the interaction of threatened NPA control with

Table A5.1: Lagged Neighbor NPA Change Predicting Own NPA Change

	(1)	(2)	(3)
Net Neighbor Change (t-1)	−0.038***	−0.049***	−0.060***
	(0.001)	(0.001)	(0.002)
Net Neighbor Change (t-2)		−0.046***	−0.060***
		(0.001)	(0.002)
Net Neighbor Change (t-3)			−0.038***
			(0.002)
Num.Obs.	188,470	188,470	188,470
R2	0.117	0.140	0.151
R2 Adj.	−0.104	−0.074	−0.061

* p <0.1, ** p <0.05, *** p <0.01
Barangay-year TWFE. Outcome is own NPA change. Same Predictors are lags of net neighbor NPA change in presence. Point and year fixed effects, SEs clustered by barangay.

lagged net neighbor change is negative and significant, meaning that threatened barangays whose neighbors lost NPA control in the prior year see more forest disturbance. This holds for two lags of neighbor change (Model 6). Where the NPA is influenced, its most consolidated level of control, the interaction is close to zero.

Table A5.4 runs the same interactions in four specifications as Table A5.3 but divides neighbor change into areas with neighbor change less than 0 and those with neighbor change greater than or equal to 0. This allows us to distinguish whether the anticipation is occurring via areas with decreasing neighbor NPA presence (as hypothesized) or increasing neighbor NPA presence. We find most support for the hypothesis that as neighboring areas lose NPA presence, continued NPA presence at the threatened level in a barangay leads to more forest disturbances.

Figure A5.1 shows net neighbor change for each barangay in 2010, the peak of NPA power across the Philippines.

We further test whether this effect operates through degradation or deforestation. If the NPA is primarily allowing increased illegal logging, the interaction of threatened NPA control with lagged net neighbor change should predict degradation but not deforestation. Our findings are consistent with this: Figure A5.2 shows the deforestation outcome and Figure A5.3 shows the degradation outcome.

Table A5.2. Anticipation of NPA exit: disturbance rises in threatened barangays after neighboring losses of NPA control.

	Dependent variable: new forest disturbance in year t (percentage points)		
	(1) Lag timing	(2) Anticipation: by control level	(3) Anticipation: losses vs. gains
NPA threatened × Net neighbor change (t−1)		−0.034*	
		(0.017)	
NPA influenced × Net neighbor change (t−1)		−0.000	
		(0.023)	
NPA threatened × Neighbor loss (t−1)			−0.048+
			(0.028)
NPA threatened × Neighbor gain (t−1)			−0.027
			(0.023)
NPA influenced × Neighbor loss (t−1)			−0.002
			(0.066)
NPA influenced × Neighbor gain (t−1)			−0.012
			(0.030)
NPA threatened	0.179**	0.191***	0.173**
	(0.059)	(0.053)	(0.056)
NPA influenced	0.253**	0.261**	0.268**
	(0.094)	(0.092)	(0.095)
Net neighbor change (t)	0.006		
	(0.010)		
Net neighbor change (t−1)	−0.021+	−0.015	
	(0.012)	(0.012)	
Net neighbor change (t−2)	0.001		
	(0.015)		
Neighbor loss (t−1)			−0.025
			(0.018)
Neighbor gain (t−1)			0.000
			(0.018)
Outcome mean	0.588	0.588	0.588
Unit of analysis	Point-year	Point-year	Point-year
Observations	712,341	712,341	712,341
R^2	0.140	0.140	0.140
Estimator	TWFE	TWFE	TWFE

Note: NPA threatened and NPA influenced are the middle and top levels of the AFP's three-point control scale (clear < threatened < influenced), relative to clear as the omitted group. Net neighbor change is the sum of year-on-year changes in NPA control across adjacent barangays; neighbor loss equals that sum in years when it fell and zero otherwise, and neighbor gain equals it in years when it rose and zero otherwise. The interactions use last year's neighbor change, so a negative coefficient means that threatened barangays whose neighbors lost NPA control in the previous year see more disturbance. Shaded cells are the anticipation estimates discussed in the text. Standard errors clustered at the barangay level in parentheses. Appendix A5 reports fuller specifications with additional lags and the same models by disturbance type. $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Figure A5.1. Net neighbor NPA change in 2010. Positive values indicate net increases among adjacent barangays.

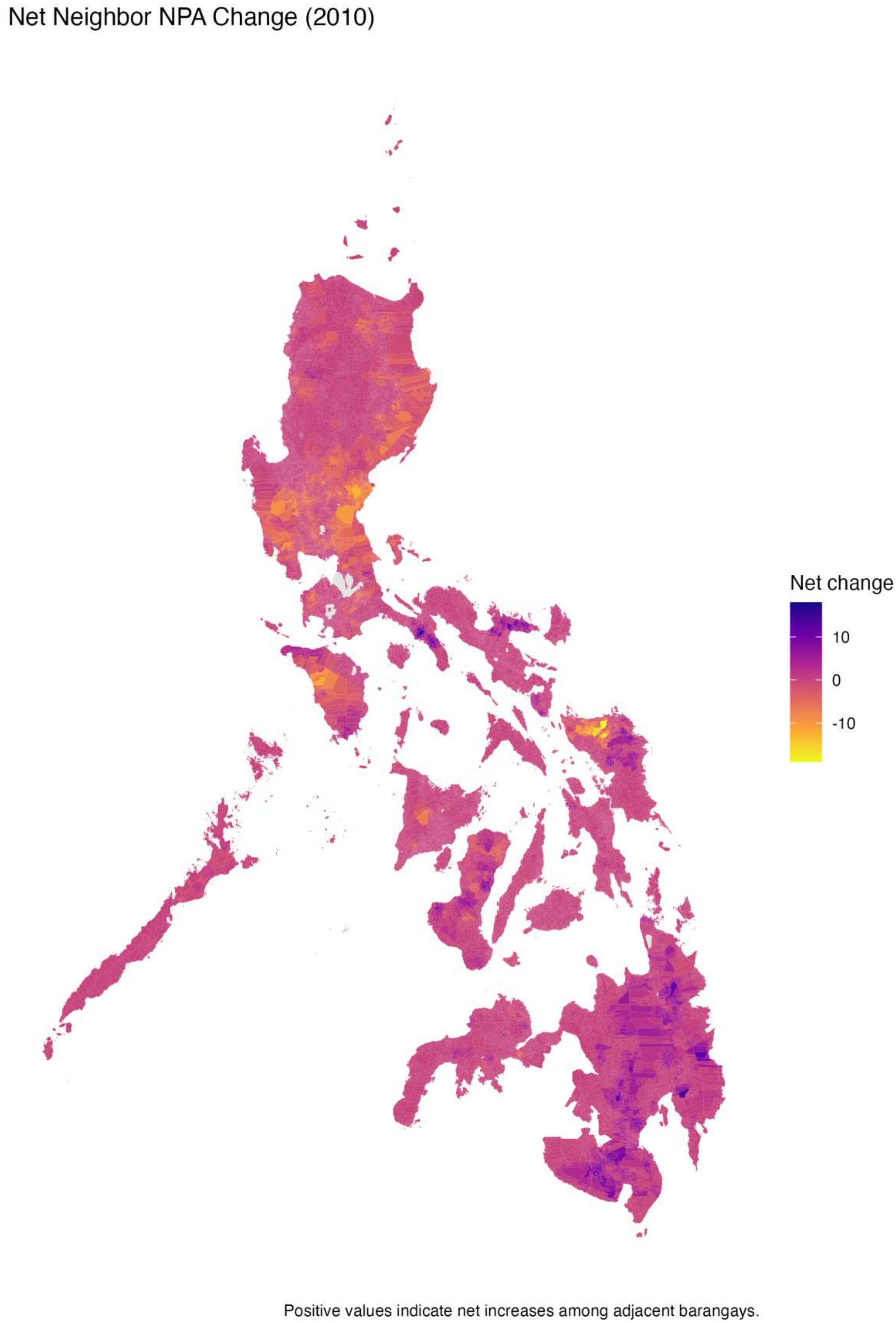


Table A5.3. Anticipation effects: NPA control and neighbor change interactions.

	Dependent variable: new forest disturbance in year t (percentage points)					
	(1)	(2)	(3)	(4)	(5)	(6)
NPA threatened	0.181***		0.179**	0.191***	0.168**	0.155**
	(0.052)		(0.059)	(0.053)	(0.058)	(0.057)
NPA influenced	0.240**		0.255**	0.263**	0.258**	0.192*
	(0.089)		(0.094)	(0.092)	(0.095)	(0.095)
Net neighbor change (t)		0.018*	0.006		0.019+	0.020+
		(0.009)	(0.010)		(0.011)	(0.011)
Net neighbor change (t−1)		−0.012	−0.021+	−0.015	−0.009	−0.008
		(0.012)	(0.012)	(0.012)	(0.013)	(0.014)
Net neighbor change (t−2)		0.008	0.001			0.009
		(0.014)	(0.015)			(0.012)
Threatened × neighbor (t)					−0.042	−0.063*
					(0.025)	(0.028)
Threatened × neighbor (t−1)				−0.034*	−0.049*	−0.087**
				(0.017)	(0.020)	(0.032)
Threatened × neighbor (t−2)						−0.090*
						(0.045)
Influenced × neighbor (t)					−0.034	−0.024
					(0.024)	(0.023)
Influenced × neighbor (t−1)				0.000	−0.011	0.002
				(0.023)	(0.025)	(0.025)
Influenced × neighbor (t−2)						0.071
						(0.044)
Unit of analysis	Point-year	Point-year	Point-year	Point-year	Point-year	Point-year
Observations	711,907	711,907	711,907	711,907	711,907	711,907
R^2	0.140	0.140	0.140	0.140	0.140	0.140
Estimator	TWFE	TWFE	TWFE	TWFE	TWFE	TWFE

Note: This table reports the effects of NPA control levels and neighbor NPA changes on forest disturbance. Models include point and year fixed effects with standard errors clustered by barangay. $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A5.4. Anticipation effects: positive and negative neighbor changes on forest disturbance.

	Dependent variable: new forest disturbance in year t (percentage points)			
	(1)	(2)	(3)	(4)
NPA threatened		0.131+	0.173**	0.085
		(0.068)	(0.056)	(0.064)
NPA influenced		0.213*	0.270**	0.134
		(0.105)	(0.095)	(0.107)
Net neighbor change (t, +)	0.009	0.005		0.000
	(0.015)	(0.021)		(0.022)
Net neighbor change (t, −)	0.026*	0.027*		0.025+
	(0.012)	(0.013)		(0.014)
Net neighbor change (t−1, +)	0.002		0.000	0.009
	(0.013)		(0.018)	(0.018)
Net neighbor change (t−1, −)	−0.028+		−0.025	−0.018
	(0.017)		(0.018)	(0.019)
Net neighbor change (t−2, +)	0.040*			0.005
	(0.018)			(0.019)
Net neighbor change (t−2, −)	−0.019			0.014
	(0.024)			(0.016)
Threatened × neighbor (t, +)		0.005		−0.038
		(0.037)		(0.042)
Threatened × neighbor (t, −)		−0.018		−0.070+
		(0.045)		(0.037)
Influenced × neighbor (t, +)		−0.015		0.002
		(0.033)		(0.033)
Influenced × neighbor (t, −)		−0.031		−0.031
		(0.043)		(0.043)
Threatened × neighbor (t−1, +)			−0.028	−0.110**
			(0.023)	(0.038)
Threatened × neighbor (t−1, −)			−0.048+	−0.089*
			(0.028)	(0.041)
Influenced × neighbor (t−1, +)			−0.013	−0.014
			(0.030)	(0.030)
Influenced × neighbor (t−1, −)			−0.002	0.006
			(0.066)	(0.071)
Threatened × neighbor (t−2, +)				0.008
				(0.042)
Threatened × neighbor (t−2, −)				−0.165*
				(0.066)
Influenced × neighbor (t−2, +)				0.118+
				(0.061)
Influenced × neighbor (t−2, −)				−0.069
				(0.083)
Unit of analysis	Point-year	Point-year	Point-year	Point-year
Observations	711,907	711,907	711,907	711,907
R^2	0.140	0.140	0.140	0.140
Estimator	TWFE	TWFE	TWFE	TWFE

Note: This table reports the effects of positive and negative neighbor NPA changes on forest disturbance, with and without interactions with NPA control levels. Models include point and year fixed effects with standard errors clustered by barangay. $^{+}p < 0.1$, $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Figure A5.2. Interaction of lagged net neighbor changes with NPA control with deforestation as the outcome.

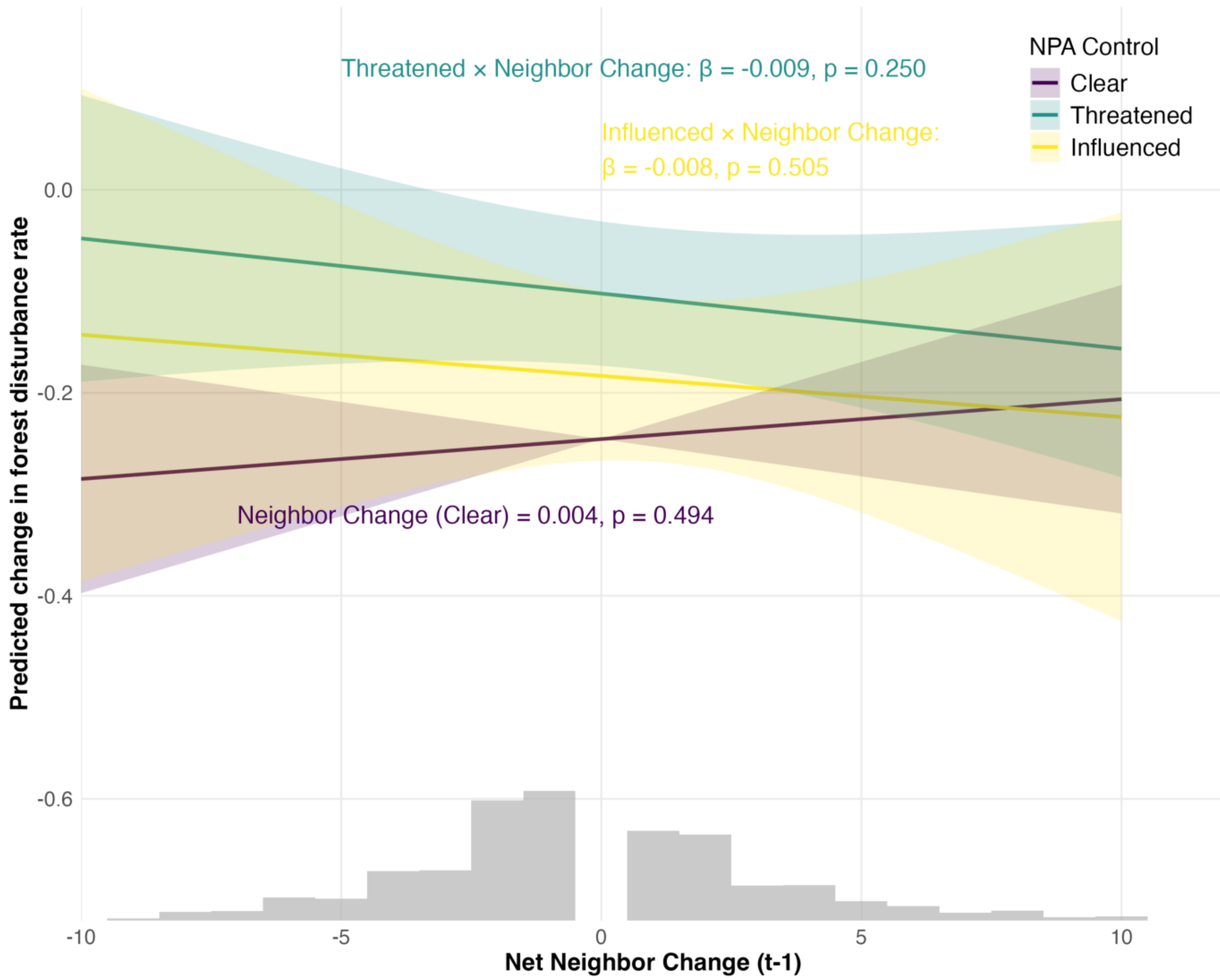


Figure A5.3. Interaction of lagged net neighbor changes with NPA control with degradation as the outcome.