LAND WITHOUT MASTERS: LOCAL POLITICAL EFFECTS OF PERU'S LAND REFORM

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Local Political Effects of Peru's Land Reform*

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Abstract

Can historically excluded groups develop capacities to compete for political power under autocracy? Can these capacities outlive the regime? This study examines Peru's 1969 agrarian reform, which under military rule expropriated large estates from landed elites and transferred the land to newly formed cooperatives of predominantly indigenous peasants, long excluded from politics. I estimate the reform's effects on local political competition, electoral participation, and candidate selection once elections resumed, focusing on the erosion of landed elites' electoral control and the rise of collective organization as potential mechanisms. Identification exploits regional office locations and reform-zone boundaries established a decade earlier for other purposes. Three designs combine office proximity, within-zone border discontinuities, and district-by-election variation. Beginning with the return to democracy in 1980, the reform raised the number of parties contesting local elections, narrowed winning margins, and increased indigenous representation among elected mayors. To interpret these findings, I extend a model of electoral control in which expropriation weakens landlord power and persistent agrarian organization gives beneficiaries political weight. Nonparametric mediation tests support this reading. The share of farmers in agrarian organizations and left voting in the 1980s can each account for the entire effect on the number of parties, while turnout responds only where the exit of incumbent elites combines with local agrarian organization. The economic changes the reform set off cannot fully explain either response.

Keywords: land reform, political competition, voting, Peru
JEL Classification: P16, N46, Q15, D72

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"From this day on, the peasants of Peru will no longer be pariahs or the disinherited, living in poverty from birth to death and viewing themselves as powerless in the face of a future that appears equally dismal for their children. From the time of this fortuitous day, June 24th, the peasants of Peru will truly be free citizens."

— General Velasco, June 1969
(translation by Seligmann, 1995)

1. Introduction

Who gets to compete for political power may crucially depend on groups' control over economic resources and their capacity for collective organization (Olson, 1965; Moore, 1966; North, 1981). In many historical settings, concentrated economic power gave elites both the incentives and the means to convert economic dependence into political control, making economic and political power mutually reinforcing (Sokoloff and Engerman, 2000; Acemoglu and Robinson, 2008; Galor, Moav and Vollrath, 2009). This logic underlies canonical theories of democratization, in which political change occurs only when elites can no longer contain redistributive demands from the masses (Acemoglu and Robinson, 2001, 2006; Boix, 2003). Yet political change may rest on economic and organizational capacities that begin to form long before formal democratization, allowing such demands to become actionable, and broadening who can effectively compete for political power.

A natural question is how such capacities can emerge in settings where political competition is absent or deliberately constrained. Redistribution under autocracy provides one such channel, with land reform as a canonical case.[1] Existing accounts largely explain why autocrats redistribute and how land transfers can serve as political instruments to weaken rival elites, secure rural support, and sustain their rule (Albertus, 2015). However, land reform often did more than transfer assets: beneficiaries were also organized into collective enterprises, serving both economic and political purposes (e.g. Montero, 2022). While the economic consequences of land redistribution and collective organization have received considerable attention, these reforms may also reshape key conditions underlying political competition by weakening the economic dependence that sustains elite control and building collective capacity among beneficiaries. Whether such capacities can form under autocracy and outlive it remains much less understood, despite extensive evidence on how effective political competition can shape economic development, particularly where other checks on political power are weak.[2]

[1] A large literature casts autocratic redistribution as a commitment device to forestall revolt (Acemoglu and Robinson, 2001, 2006; Boix, 2003), as a tool of cooptation and elite power-sharing to buy off potential rivals (Bueno de Mesquita et al., 2003; Gandhi and Przeworski, 2006), or as a weapon against rival elites to prolong autocratic tenure (Albertus and Menaldo, 2012).

[2] See, for example, Besley, Persson and Sturm (2010), Ferraz, Finan and Martinez-Bravo (2024), or Marx, Pons and Rollet (2025).

This paper studies whether such political legacies emerged from Peru's 1969 agrarian reform, one of the largest land expropriation programs in Latin American history. Implemented under military rule, the reform combined the redistribution of nearly half of the country's agricultural land with the collective organization of beneficiaries into cooperatives. For many rural workers, predominantly indigenous and excluded from formal voting by a literacy requirement, elections within these cooperatives provided their first experience of choosing representatives (Cant, 2021). The reform thus weakened a key economic basis of elite control, with nearly every large private landholding gone by 1985, and built organizational capacity among formerly excluded rural workers, all while parties and elections were suspended. When competitive local elections resumed in 1980, with illiterates now enfranchised, these changes could shape both entry into politics and the political weight of previously marginalized voters. I therefore study the reform's effects on local political competition, electoral participation, and candidate selection: how many parties compete and how closely, who turns out to vote, and who runs for and wins local office.

Identification exploits the reform's administrative geography. Expropriations were run through Agrarian Reform Zones (ARZ) whose boundaries had been drawn nearly a decade earlier for different purposes, and through regional offices placed in each zone's high-priority reform areas. As suggested by historical accounts, districts farther from these offices received less territorial attention and became the reform's *de facto* periphery, even though core and peripheral districts were similar in local characteristics and in the presence of *haciendas*, the large estates that controlled both land and resident labor until the reform (Echevarría, 1978; Matos Mar and Mejía, 1980). The analysis combines digitized expropriation records covering the full reform period (1969–1980) with district-level results from every direct municipal election Peru has held, from the first in 1963 to 2022, and candidate-level data. Three complementary designs build on this structure: (i) an instrumental variable design based on the distance to the nearest ARZ office, (ii) a regression discontinuity design exploiting within-ARZ core-periphery boundaries, and (iii) a difference-in-differences specification leveraging local elections before and after the reform.[3]

Across the three designs, the results are consistent with a shift toward a more competitive local political environment in reform-affected districts. In the instrumental variable estimates, a one-standard-deviation increase in land reform exposure raises the number of effective parties contesting local elections by 20 to 26 percent relative to the mean and narrows victory margins by 4.8 to 6.8 percentage points. The other two designs replicate

[3]The boundary design builds on Albertus (2020), who first exploited this core-periphery geography to study land reform and civil conflict.

the effect on the number of parties. At within-zone boundaries, core districts field 0.33 to 0.56 more effective parties than peripheral ones, 10 to 17 percent of the periphery mean, and in the election panel each standard deviation of exposure adds 0.07 to 0.10 parties, an effect that builds through the 1980s and 1990s, peaks in 2002 and recedes thereafter. The 1980 enfranchisement of illiterates and compulsory voting raised turnout nationwide, so although positive effects on turnout are also identified, they are harder to disentangle from gradients of remoteness. The effects of the reform on participation operate instead through the supply side of local politics, changing who runs and who is elected, and they run through what the reform left organized. The cooperatives did not last, but the agrarian organizations that outlived them, measured by membership in the 1994 agricultural census, carry the effect on the number of parties.

If a more open contest lets more parties compete, it should also change who runs for office and who wins it. Consistent with parties fielding stronger candidates when races tighten, a one-standard-deviation increase in exposure raises the average education of contesting candidates by 1.1 to 1.5 years and their party experience by 0.15 to 0.31 years. Beyond candidate quality, the reform also reshapes representation along the ethnic divide it made salient: the effect concentrates at the winning margin, with the share of elected mayors carrying indigenous surnames rising robustly across designs while the response of the candidate pool is smaller and less precise. At the same time, contesting candidates are on average older and less associated with traditional politics, entering more often through parties that compete nationally rather than through regional ones.

To interpret these findings, I extend a model of electoral control originally developed by Baland and Robinson (2008), in which landlords' labor-market power translates into vote buying when voting is observable or punishable. At baseline, political capture operates on two margins: through the labor contract, as in their model, where landlords condition employment on electoral compliance, and through the franchise, the margin I add, as landlords restrict schooling where only the literate can vote. Expropriation breaks the first margin and the 1980 enfranchisement of illiterates removes the second, but a freed voter counts in what parties offer only if she can be reached and coordinated with others like her. That takes organization, the second element I add: associations that let parties reach these voters, let the voters act as a bloc, and supply candidates of their own. The model therefore predicts more parties competing, narrower winning margins, and a shift toward candidates with attributes that voters value and parties find cheaper to recruit, each effect larger the more organized the freed voters are. The prediction on turnout is instead conditional, since the loss of landlord-driven mobilization works against the participation that organization adds.

The evidence on mechanisms follows this structure. On elite capture, incumbent landed families lose about 10 percentage points of local office, and they do so by withdrawing from competition rather than losing it. On organization, agrarian associations outlived the cooperatives that created them: in the 1994 agricultural census, exposed districts had 11.7 percentage points more producers in some organization, well after the cooperatives were gone. That capacity also took root in the cohorts at the age of political activation: membership in local organizations is higher among those who were between 12 and 25 when the reform reached their district, and flat among those exposed as children. On preferences, revealed demand shifts toward redistribution, with the broad left gaining 11.9 points of vote share in the 1980s while stated attitudes toward public institutions, democracy, or political parties barely move, a re-weighting of given preferences rather than their conversion. The nonparametric mediation tests of Kwon and Roth (2026) then weigh these channels against each other: the share of farmers in agrarian organizations in 1994, or the left vote share in the 1980s, could each carry the entire effect on competition while the exit of incumbent elite families alone cannot, and turnout responds only where the exit of incumbent elites combines with local organization. Adding an economic channel to the same framework rejects full mediation through local economic conditions, measured by the two structures the reform rewrote, land inequality and the share of farms hiring labor, suggesting the reform moved local politics through the power it dismantled and the organization it built, not through the economic trajectories it set off.

I conduct important validity checks for each of the three empirical strategies. For the IV approach, the key concern is that proximity to ARZ offices may capture not only greater exposure to the reform but also districts with a different set of voters and parties. A falsification test cuts against this reading: proximity predicts exposure only in central ARZs, those where haciendas were effectively targeted, and has no effect in peripheral areas where reform officials entered only marginally. Since placebos mimic only specific confounding channels, I follow Dell and Olken (2020) and treat the office placement itself as the object of inference: a thousand counterfactual office networks drawn from the original placement criteria show that the effect on political competition is specific to the true offices. Recentering the instrument on its expectation across those configurations leaves the effect intact and, if anything, larger (Borusyak and Hull, 2023). Since the IV leans on cross-district comparisons, the regression discontinuity and difference-in-differences designs are built to net that out, each with its own validity checks. At the boundary, covariates are balanced, the pre-reform elections of 1963 and 1966 show no discontinuity, and alternative cutoffs and placebo boundaries localize the effect at the actual cutoff. In the difference-in-differences design, the pre-reform elections show no

differential trends. Because Peru's first direct municipal elections date from 1963, only two precede the reform. Since the pre-trend test is low-powered, I complement it with Rambachan and Roth (2023) bounds.

The paper speaks to several literatures. First, it contributes to studies on regime transitions and the interplay between economic and political inequality (Acemoglu et al., 2008; Acemoglu, Reed and Robinson, 2014; Anderson, Francois and Kotwal, 2015; Fergusson, Larreguy and Riaño, 2022; Ferraz, Finan and Martinez-Bravo, 2024). The Peruvian experience complements canonical theories of democratization by shifting attention upstream, to the economic and organizational capacities that allow redistributive demands to become politically consequential. Although imposed from above to sustain authoritarian rule, the reform weakened elite control and organized rural workers excluded from the franchise through cooperatives, creating capacities that later shifted *de facto* political power and fostered competition once democracy returned. The results trace the political effects of the reform first to the agrarian organizations it recognized and later to broader forms of local organization, a capacity built under autocracy and long before formal democratization. Political change, in this sense, can be shaped not only by the redistributive demands that autocratic elites cannot contain, but also by the unintended mobilizing consequences of the policies they adopt to survive.

Second, the results contribute to work on how historical elites shape political competition and development, and on the consequences of dismantling their power (Galor, Moav and Vollrath, 2009; Cinnirella and Hornung, 2016; Markevich and Zhuravskaya, 2018; Eapen, 2019; Goñi, 2023; Ferraz, Finan and Martinez-Bravo, 2024; Bisin and Verdier, 2024; Ashraf et al., 2024). Where unconstrained, elites can shape civil society and local organization to preserve their power and suppress competition (Acemoglu, Reed and Robinson, 2014; Anderson, Francois and Kotwal, 2015). The Peruvian experience shows the reverse possibility: organization imposed from above to weaken landed elites, extended to a large excluded population, and left behind by a collapsing regime can later broaden political competition. This opening, however, did not translate into unambiguous welfare gains. Some short-run dynamism was followed by land reconcentration, mixed effects on agricultural productivity, and no reduction in poverty. The cohort evidence shows both sides at once: those exposed as children (ages 0–11) carry a lasting economic cost, with lower consumption, more agricultural work, and less out-migration as adults, while those exposed at the age of political activation (12–25) are the ones who organize, plausibly the same cohorts behind the entry of new and, in particular, indigenous candidates documented above. While a prominent literature asks whether democracy promotes development, these results highlight that broader political repre-

sentation may itself emerge through redistributive reforms that carry lasting economic costs, even when the political rights they create are valuable in their own right.[4]

Finally, the paper speaks to a long tradition of studies on land as a political device (de Janvry, 1981; Bardhan and Mookherjee, 2010; Carter and Morrow, 2014; de Janvry, Gonzalez-Navarro and Sadoulet, 2014; Albertus, 2015; Bhattacharya, Mitra and Ulubaşoğlu, 2019; López-Uribe, 2022; Caprettini, Casaburi and Venturini, 2025; Moshrif, Saleh and Hartnett, 2026). Much of this work focuses on the political economy of reform enactment, rather than their downstream effects, or on land redistribution as a tool of clientelism and elite control. In Mexico, gradual land allocation sustained ruling-party clientelist networks and shaped local state capacity (de Janvry, Gonzalez-Navarro and Sadoulet, 2014; Fergusson, Larreguy and Riaño, 2022), while in Paraguay the autocracy used land distribution to consolidate social control through politically connected elite families (Bandiera, Larreguy and Mangonnet, 2026). Although not concerned with autocratic redistribution, Caprettini, Casaburi and Venturini (2025) and Rouanet and Tallec (2026) document persistent electoral returns of land redistribution in postwar Italy and revolutionary France, where redistribution generated durable political support for the reforming side. The Peruvian case highlights a different mechanism. The regime that implemented the reform collapsed before it could convert redistribution into durable partisan support, yet the reform continued to shape politics by weakening landed elites' control and building organizational capacity among previously excluded rural workers. Redistribution, in this sense, can create political capacity, not only political loyalty. Its long-run political effects may operate not only by binding beneficiaries to the reforming side, but also by changing who is able to enter and compete once political competition opens.

The remainder of the paper is organized as follows. Section 2 presents the historical background and Section 3 the data. Section 4 estimates the effects on local political competition, and Section 5 those on candidate selection. Section 6 develops a model to interpret them and Section 7 examines mechanisms. Section 8 concludes.

[4]The economic literature on land reform centers on the equity-efficiency trade-off and reports mixed effects that depend on reform design and implementation constraints (e.g. Besley and Burgess, 2000; Banerjee and Iyer, 2005; Adamopoulos and Restuccia, 2020; Montero, 2022; Gachet, 2024; Kim and Wang, 2025; Deseau, 2025; Mitra et al., 2026).

2. Background

2.1. The pre-reform scenario

The *hacienda*, the predominant system of large agrarian estates with coerced labor and roots in the colonial period, remained a central institution of rural Peru as late as the 1960s.[5] Weak central oversight and limited spatial mobility allowed landowners to secure labor through coercive and dependent arrangements that bound a predominantly indigenous workforce to the estates and to exercise de facto authority over land access, local services, and justice (Handelman, 1975; Cotler, 1976; Favre, 1976; Thorp and Bertram, 1978). By the mid-twentieth century, this system had entrenched landownership as a central source of economic inequality and social hierarchy.[6] In 1965, half of the economically active population worked in agriculture. The top 1% of landowners held 80% of private land, while 83% of farmers operated holdings of five hectares or less, accounting for only 6% of agricultural land.

Previous attempts of implementing even mild versions of an agrarian reform under democracy were continuously blocked by the influence of the preexisting landowning elite over the military until the election of Fernando Belaunde Terry in 1963 (Albertus, 2013). One of the main promises of Belaunde's campaign was the implementation of an agrarian reform. Unsurprisingly, only a few months after him running office, peasant-based mobilizations started a wide spreading in the country (Eguren, 2006*b*). In response to this social upheaval, Belaunde's administration promoted an agrarian reform law that passed through the Congress in 1964 but due to the political influence of landlords over the two main opposition parties in Congress –the 'Union Nacional Odriista' and the 'Alianza Popular Revolucionaria Americana APRA'– the project was severely modified until the point that became useless for its initial purpose (Eguren, 2006*a*; Cleaves and Scurrah, 1980). The scope of application of this reform, in consequence, was extremely limited. The upcoming of economic crisis, social unrest associated to land invasions and leftist rebellions in the highlands, and a broad public perception of an oligarquic and corrupted state characterized the last period of Belaunde's presidency.

[5] These estates, owned almost exclusively by Spaniards and criollos, emerged from the decline of sixteenth-century Crown grants conferring rights to extract tribute and forced labor from indigenous populations (Lockhart, 1969; Gill, 2018; Yeager, 1995).

[6] Montoya and Gorman (1978), for instance, characterized this regime as generating "a class of sierra landlords, seignorial rentiers appropriating for themselves rent in the form of labor, principally, from serfs, semiserfs and, eventually, parcelarios (smallholders) in semi-feudal agricultural and livestock units." Palmer (1973) similarly linked Peru's relative social and economic stagnation in the 1960s to the persistence of this archaic agrarian structure.

2.2. The military coup and the "revolution from above"

In October 1968, denouncing the obsolescence of the oligarchic state and corruption of partisan politics, military forces led by the General Juan Velasco Alvarado established a junta-led "revolutionary government", setting the beginning of a set of radical reform processes known as the "revolution from above" (Puente, 2019; Albertus, 2013). Figure 1 places this transition within the sequence of regimes, party systems and municipal elections used in the analysis. Under military rule, these reforms explicitly aimed to bring about a "structural transformation" of the Peruvian economy and society along nationalist lines. The government expropriated foreign and privately owned firms in sectors including industry, banking, mining, and petroleum, reorganized the public administration, and created public enterprises with monopolistic privileges in strategic sectors such as energy and key agricultural exports. It also introduced labor legislation granting workers participation in profit distribution, shareholding, and company management (Thorp and Bertram, 1978).[7]

Among these reforms, the agrarian one had particular importance for the military. Despite the lack of a cohesive ideological orientation within Velasco's support coalition, the imperative consensus was the unity against economic elites in the country, in agriculture but also in industry, finance and export sectors, where redistributive initiatives were also promoted (Albertus, 2013). Authors as Kruijt (1994) have suggested the origin of the military administration as a determinant of their inclination towards the implementation of a land reform. As stated by Albertus (2013) *'threats to political stability in Peru in the late 1950s and 1960s brought the military in direct contact with Peru's backward agrarian structure and the landed elites that dominated it, deepening its sense that landed elites must be eliminated and its resentment of manipulation at the hand of these same elites'*. In the Southern Highlands, Hugo Blanco's revolution required army intervention, and this implied the revelation of the land problematic in the country. In fact, the rise of a rural guerrilla in 1965 and the lack of government response from Belaunde's administration solidified the ideas of officers surrounding Velasco regarding land reform. The experience of the army as it dealt with the waves of mobilization and the guerrillas of the 1960s, particularly in Cusco, persuaded the military that a serious land reform was needed to guarantee national security (Thorp and Bertram, 1978; Lowenthal, 1974).

[7] As stated by Schydlowsky and Wicht (1978): *"From 1969 to 1974 the military enjoyed strong political power, as the old political parties (including [Belaunde's] Accion Popular and the APRA) were discredited as 'derechistas' [right-wing] and widely viewed as incapable of carrying out the necessary reforms that their leaders had promised for so many years"*.

2.3. Land redistribution under military rule and the transition towards democracy

The Agrarian Reform Law of 1969 stipulated that all landholdings above 150 hectares in coastal regions and between 15 to 55 hectares in the Highlands were subject to expropriation. In addition, those under violation of labor laws were subject to expropriation regardless of property size and all the expropriations included the redistribution of productive assets: agricultural equipment, animals, etc. (Albertus, 2013). For the Coast, the *Cooperativas Agrarias del Peru* (*CAPs*) were designed as units of self-managed production under the control of workers, while for the traditional haciendas of the Highlands –concentrating a higher number of indigenous communities–, conditional to terrain feasibility, a set of large-scale groupings called *Sociedades Agrarias de Interés Social* (*SAIS*) were created including the workers of the ex-hacienda (Thorp and Bertram, 1978). The expropriation process included a compensation scheme based on the land value previously declared for tax purposes –typically below market value– and long-term government bonds that lost almost all their value after the inflationary process of the following years. Mayer (2009) estimates that land was compensated approximately at 10% of its 1967 market value, 73% through the emission of government bonds.

The implementation of the reform was diligent: the same day of its proclamation, the massive agro-industrial conglomerates in the northern coast, whose influential owners were called 'sugar barons', were among the first being expropriated (Eguren, 2006*a*). The areas most affected by the reform were the northern coast and the southern highlands of the country, while the eastern side remained practically untouched. Figure 4 shows the agro-ecological conditions and transport infrastructure of the Agrarian Reform Zones through which the reform was administered. As shown in Figure 2 (panels a and b), the reform unfolded rapidly: expropriations accelerated after 1971 and had essentially run their course by 1976, shortly after Velasco's ouster, reaching 9.3 million hectares, over 45% of the country's 1972 agricultural surface. Panels (c) and (d) show that the Sierra concentrated the bulk of expropriated land in absolute terms, while relative to its own agricultural surface the reform hit the Costa hardest. As stated by McClintock (1983), by the time the expropriation rate declined, *'all coastal landholdings of more than fifty hectares and highland holdings of more than thirty hectares had been expropriated'*.

During the implementation process, to ensure political support, the government dismantled the National Agrarian Society, a guild of large-scale landowners created by the end of the XIXth century with a strong political influence against previous efforts of agrarian reform under democracy. At the same time, Velasco created the National Agrarian Confederation (CNA), an agricultural-based organization that joined the reform beneficiaries to support the regime.

As Thorp and Bertram (1978) state, by 1974, the implementation of the reform in addition to Velasco's policies in the agrarian cooperatives diminished the rural support to the government among non-beneficiaries. Hacienda land went mostly to the permanent workforce of the expropriated estates, organized in SAIS and CAPs, while independent peasants, temporary laborers, and the surrounding peasant communities received little.[8] A context of social discontent, the upcoming of economic crisis, a bilateral dispute with Chile and Velasco's health deterioration signed the fractionalization of the military and the push of Velasco out of office by the General Morales Bermudez in 1975 with the support of many insiders (McClintock, 1983; Albertus, 2013). While Morales at first continued the agrarian reform, it lowered its pace substantially and was unable to control the strikes against the implemented agrarian policies. At the same time, the reversal of economic policies in other sectors returned some power to the elites and pressured Morales against the maintenance of progressive members of the military coalition. In 1977, with a highly fractionalized military, Morales announced upcoming elections.

The military regime collapsed, but the reform had already transformed the structure of rural power. Nearly every large private holding had disappeared by 1985, and about 400,000 families had been incorporated into cooperatives and associations (Albertus, 2013; Espinoza and Escobal, 2025). By dismantling the hacienda as a labor regime, expropriation weakened a major source of workers' economic dependence on landlords across much of the countryside. At the same time, the institutions created to manage the transferred land became new spaces of collective organization. Although their autonomy varied across regions and organizational forms (Puente, 2022), cooperatives brought rural workers into assemblies, elected councils, and other forms of collective decision-making. Case studies from the 1960s and 1970s agree on the direction of the change. On the eve of the reform, resident indigenous laborers in traditional highland haciendas were cut off from even the most elementary forms of political participation, and dealt with public authorities only through the landlord (Cotler, 1976, p. 337). In the former haciendas that McClintock (1981, pp. 159–162) resurveyed, 39 percent of respondents held a voting card in 1969, while by 1974, 71 percent reported taking part in the General Assembly of their cooperative. For most of them, these were the first elections they had ever voted in (Cant, 2021).

When democracy returned in 1980, these changes met a broader opening of local political competition. Peru had held direct municipal elections only in 1963 and 1966 before the military regime restored executive appointments (Muñoz, 2005; Tuesta Sol-

[8] By 1974, *'only 9% of the lands were distributed among indigenous peasant communities, and 13% among other groups in the countryside, while the newly created SAIS received 43%'* (Thorp and Paredes, 2010).

devilla, 1995). The 1979 Constitution established universal suffrage and restored elected local governments, bringing previously excluded rural citizens into national and municipal elections (Muñoz, 2005). Historical accounts describe the countryside entering this transition with a substantially altered political structure: landed elites had long dominated local office, while the reform had weakened their economic base and created new leadership roles and organizational arenas among peasants (Seligmann, 1995; Cant, 2021). Case studies point the same way. In ten districts of Ayacucho, the mayors appointed under the military were often of indigenous origin and return migrants with a record of social leadership, and those elected in 1980 were schooled peasants of the same profile (Remy, 2005, pp. 116–117). In Huanoquite, Cusco, where the mayoralty had belonged to the landed elite for decades, Juan Bautista Quispe Antitupa, a peasant running on the United Left slate, became in 1980 the first peasant ever elected district mayor (Seligmann, 1995). The reform did not itself create the democratic opening, but it appears to have shaped who was positioned to enter and compete within it once elections resumed.

3. Data

This section describes the main variables and their sources. Appendix A gives the full detail on construction, validation and coverage. The analysis combines four sets of data. District exposure to the 1969 land reform is built from the universe of expropriation decrees (Albertus, Espinoza and Fort, 2020). Electoral outcomes come from a district-level panel of the fifteen municipal elections held between 1963 and 2022. Candidate-level records cover the 2002–2022 elections, complemented by a surname-based classification available for the full 1963–2022 span. Pre-reform district characteristics come from the historical statistical record.

3.1. Land reform exposure

Land reform exposure is built from the universe of expropriation decrees issued under the 1969 Agrarian Reform Law and published in the official daily *El Peruano*, originally compiled by Albertus, Espinoza and Fort (2020). Approximately 15 thousand decrees cover 10 million hectares of land in active private use, abandoned or long-fallowed private land, and agriculturally unproductive land. The paper works at the level of the individual expropriation event: records are validated, assigned to the 1972 census geography, dated by decree and classified by land type (Appendix A, Note A2).

The land reform exposure measure the paper uses accounts for the total extension of expropriated hectares of land on each district via the main land reform legislation

from 1969 to 1980 as a share of each district's total area. On average, 14% of a district's total area was expropriated, accounting for an average of 41% of total agricultural land. Figure 3, panel (b), shows the spatial distribution of the land reform intensity employed by Albertus, Espinoza and Fort (2020)[9]. As shown, the intensity of the reform is more pronounced in coastal and highland (sierra) regions. In the average district, the reform started in 1973 and ended in 1975, with a median duration of 2 years.

The implementation of the reform was based on the demarcation of 13 Agrarian Reform Zones (ARZ) and the installation of regional offices for local execution located in high-priority reform areas within each ARZ. Both the zones and their offices predate the reform. They were delimited in 1960 by Peru's Agrarian Research and Promotion Service (SIPA) for agricultural planning purposes, and the 1969 reform was later grafted onto them (Figure 3, panel a). The distance from each district centroid to the nearest office is computed from their geocoded locations. Section 4 returns to this feature and uses that distance as an instrument.

3.2. Electoral competition

This paper compiles district-level results for all fifteen municipal elections held between 1963 and 2022 from the official records of the National Elections Board (JNE), published through the INFOGOB observatory. The panel covers the two pre-reform elections under civilian rule (1963, 1966) and the thirteen post-reform elections (1980–2022). The 1963 contest was the first direct municipal election after more than four decades in which the central government appointed local authorities (Muñoz, 2005). Turnout, the Number of Effective Parties (NEP), the victory margin between the top two parties, and the voting shares Herfindahl-Hirschman (HH), Total Volatility (TV) and Hyperfragmentation (HP) indexes are computed from party-level vote counts in each district-election.[10]

Figure 5 summarizes the resulting series. Three patterns stand out. First, the average number of effective parties is essentially flat at around two parties per district throughout the national-party era (1963–1989), doubles after the collapse of the traditional party system in the early 1990s, peaks in 2002, and declines gradually thereafter. Second, victory margins mirror this trajectory, compressing from roughly 30–40 percentage points before 1990 to under 10 points from 2002 onward. Third, turnout climbs steadily after redemocratization, from about 61% in 1980 to 87% in 2006. The bottom panel shows

[9]The authors restrict their measure of land reform exposure to actively used private agricultural land as a share of total agricultural land on each district.

[10]These indicators are computed from raw vote counts rather than taken from published tables. Appendix A, Note A1, reports the internal and external validation and the coverage of the registered-voter counts that determine when district-level turnout can be computed.

that these aggregates mask regional convergence: in 1980, Costa districts voted at rates 17 percentage points above Sierra districts, a gap that closes fully by 2006, while the number of effective parties converges across regions in the 1990s.

3.3. Candidates' characteristics

Candidate-level information comes from the candidate profiles (*hojas de vida*) that every candidate files with the JNE, which report schooling, work history and party service. The compilation for 2002–2014 follows Artiles, Kleine-Rueschkamp and León-Ciliotta (2021). This paper extends it to the 2018 and 2022 elections by scraping the JNE candidacy registries directly (Appendix A, Note A1). Based on this information, a series of consistent variables related to the candidates' schooling are computed (educational dummies) to construct an estimate of years of education. Additionally, the dataset includes information on the candidates' work and political experience as well as political party service from where is possible to compute the number of years of experience in (i) elected public office (mayor, councilor or regional counselor), (ii) the position of mayor, (iii) service in party office, as well as (iv) whether a candidate is a member of a national political party, (v) has work experience in the public sector or (vi) private sector.

In addition, based on text analysis of all candidates' surnames, it's possible to recover the authors' ethnicity classification of candidates as indigenous (Quechua or Aymara) or other (Spanish or foreign) by matching the linguistic roots of all surnames with a set of dictionaries containing native roots[11]. Because surnames appear in the electoral registries themselves, this classification extends to the full 1963–2022 panel, well beyond the years for which candidate profiles are available. The same registries support a second surname-based measure built for this paper, a list of 48 traditional landed-elite family names curated from the historiography of the Peruvian hacienda system (Matos Mar, 1976; Miller, 1982; Quiroz, 1988; Gonzales, 1991), matched to elected officials within each family's home department. Figure 18 describes the matching rule and the placebo list used to validate it.

3.4. Pre-reform controls

Several specifications include five sets of pre-reform controls, drawn primarily from the 1961 Census. *Geographic controls* include district elevation, slope, and centroid longitude and latitude, from the FAO's Global Agro-Ecological Zones database. *Agriculture and labor force controls* include the agricultural labor share, GAEZ crop suitability and the

[11]This classification is based on the procedure and data in Artiles (2022)

size of the labor force. *Institutional controls* include whether the district lies inside a mita zone, the forced mining labor system in effect in Peru and Bolivia between 1573 and 1812 (Dell, 2010), the number of conflictive social movements and autonomous communal uprisings prior to the late 1960s, and a measure of "caudillo" presence based on episodes of local authorities forcibly rebuffing central authority (Kammann, 1982; Albertus, 2020). *State capacity controls* include log state personnel and illiteracy rates. *Hacienda and ethnicity controls* include the number of haciendas and the indigenous mother-tongue share. When the full set of controls is included, an additional specification selects among the non-geographic controls by post-double-selection lasso (Belloni, Chernozhukov and Hansen, 2014), with the geographic controls and the fixed effects partialled out. For candidate-level specifications, *sociodemographic controls* include age, gender and indigenous ethnicity. Pre-reform district characteristics draw on the historical statistical record, including the population censuses of 1876, 1940, 1961 and 1972, the 1961 agricultural census and the 1961 directory of populated centers. Part of this material was digitized for this project and for a companion paper (Espinoza and Paredes, 2026), which assembles district-level measures of hacienda presence from colonial and republican sources (Appendix A, Note A3).

4. Did Exposure to Land Reform Impact Local Political Competition?

This section estimates the impact of the reform on local political competition, measured by the number of effective parties, the election victory margin, and turnout. Treatment is district exposure to the reform, the share of district area expropriated between 1969 and 1980, standardized so that coefficients read per standard deviation. Because expropriation may respond to district characteristics that also shape local politics, the analysis uses three complementary designs that progressively shift the comparison from across districts to within districts. First, an instrumental variable strategy exploits distance to the offices that administered the reform, checked against 1,000 placebo configurations and recentering (Borusyak and Hull, 2023). Second, a discontinuity design exploits the mismatch between the ARZ demarcation and Peru's regional administrative borders, comparing nearby districts on either side of the core-periphery boundary within each ARZ. Third, a difference-in-differences design compares changes across pre- and post-reform elections within districts with different levels of exposure. Each design changes the comparison set and, with it, the main threat to identification. The IV relies on cross-district variation in administrative reach and remains exposed to spatial confounding; the discontinuity design makes that comparison local, at the cost of a narrower estimand and lower statistical power; and the difference-in-differences

design absorbs time-invariant district differences with only two pre-reform elections. Their assumptions therefore differ, so the three estimands, read together, strengthen the causal interpretation.

4.1. The Reform: geography and implementation

The reform was administered through a system of 13 Agrarian Reform Zones and their regional offices. Its demarcation traces back to 1960, to Peru's Agrarian Research and Promotion Service (SIPA), a convention to promote agricultural development with technical support from the Organization of American States and USAID. SIPA delineated these zones on *"ecological conditions, social conditions, transportation routes, and access to markets"* (SIPA, 1967), as noted by Albertus (2020) and shown in Figure 3, panel (a). Almost a decade later Velasco's reform was grafted onto these existing zones and their regional offices, for three strategic reasons: the need for a rapid rollout to prevent landowner resistance (Albertus, 2015), the planning advantages of a small number of zones for the goal of promoting peasant-based organization through cooperatives (García, 1970), and the mandate of top-down control by regional managers (Cleaves and Scurrah, 1980).

Implementation through this pre-existing infrastructure produced a *de facto* distinction between core and peripheral districts. Districts less proximate to the regional offices received less territorial attention even when they did not differ markedly in local characteristics or in the intensity of the hacienda system (Echevarría, 1978; Matos Mar and Mejía, 1980). Internal documentation from the Ministry of Agriculture recognized this and justified it by budget constraints on the training and allocation of reform officials in peripheral zones (MINAG, 1971; WB, 1975).[12] In this sense, distance to an office can plausibly capture the lower intensity of the reform in districts further away, without capturing local differences beyond that reduced administrative capacity.

4.2. Cross-sectional estimates

The estimation starts from the correlation between land reform exposure and district-level measures of political competition over the pool of 13 post-reform municipal elections between 1980 and 2022:

$$y_{dt} = \gamma_1 LandReform_d + X_d' B + \delta_z + \delta_t + \epsilon_{dt} \tag{1}$$

[12]The online appendix of Albertus (2020) reviews these internal records and provides further evidence on the budget and personnel allocation constraints.

where y_{dt} is the outcome for district d in election t, number of effective parties, the victory margin, or turnout. $LandReform_d$ is standardized exposure, and δ_z and δ_t are Agrarian Reform Zone and election fixed effects. The vector X_d includes the five sets of pre-reform controls described earlier: geographic, agriculture and labor force, institutional, state capacity, and hacienda and ethnicity. Unless otherwise specified, an additional specification is reported where the full set of controls is selected by post-double-selection lasso (Belloni, Chernozhukov and Hansen, 2014). Standard errors are clustered at the district level. Because spatially autocorrelated residuals inflate conventional t statistics in regressions of this structure (Conley and Kelly, 2025), each table also reports Conley spatial-HAC errors that let residuals covary within 100 and 200 km. Oster δ-bounds are reported to assess the sensitivity of the estimates to selection on unobservables (Oster, 2019).

4.2.1. OLS estimates.

Table 1 reports the results on the Number of Effective Parties. In the specification with the full set of controls and both sets of fixed effects, a one standard deviation increase in exposure is associated with 0.079 additional effective parties, about 2 percent of the district-election mean of 3.61. The estimate is stable across the four columns. Consistent with this direction, districts with higher exposure to the reform have 0.80 percentage points of higher turnout (against a mean of 75.3, Table 2) and their elections exhibit between 0.25 and 0.73 percentage points lower victory margins (Table 3). All three outcomes remain significant under Conley errors at both bandwidths in the fully controlled specifications. For the Number of Effective Parties and the victory margin, the Oster δ-bounds suggest that selection on unobservables would have to be at least four times as strong as selection on observables to explain away the estimates[13].

4.2.2. Instrumental variable estimates.

The results in Tables 1 to 3 suggest a robust association between historical exposure to the reform and contemporary measures of political competition and electoral participation. However, this is not enough to identify a causal effect on any of those outcomes, since an omitted variable may determine both exposure to the reform and local conditions shaping political outcomes. To address this concern, an ideal instrument would predict a district's exposure to the reform without affecting the political outcomes of interest through any other channel. The administrative geography described at the beginning of the section supplies a candidate: the distance from each district centroid

[13]The bound is negative for turnout because adding the full control set makes the coefficient larger rather than smaller.

to the nearest Agrarian Reform Zone office, standardized across districts. The paper leads with this design for two reasons. First, it exploits broad cross-district variation in the full distribution of reform exposure and can be carried consistently through the candidate and mechanism analyses. Second, its main threat, that office location may capture other spatial determinants of local politics, can be examined directly through the counterfactual-office and recentering exercises below.

As mentioned earlier, the Agrarian Reform Zones were not designed or drawn for the reform. SIPA delimited them to group districts sharing agro-ecological conditions and access to markets, so that agricultural research and extension could be organized around a small number of homogeneous units. Figure 4 shows what that grouping tracked: Mantilla's 1940 synoptic map of natural resources and rural activity in panel (a), and in panel (b) the crop with the highest FAO-GAEZ suitability in each cell together with the road and rail network of 1929. Offices were located in high-priority areas within each zone, but the assignment of further resources or personnel was limited. A decade later, the reform was deployed through that same infrastructure. It is the resulting geography that the instrument exploits: panel (c) overlays on the exposure map a line from each district centroid to its closest office, and panel (d) colours centroids by that distance.

First-stage estimates. Table 4 corroborates the relevance of the instrument: a one standard deviation increase in distance lowers exposure to the reform by 4.3 percentage points of district area against a mean of 14.5, the estimate is stable across the specifications and it survives Conley errors at both bandwidths. The excluded-instrument F, which in this just-identified setting coincides with the effective F of Montiel Olea and Pflueger (2013), runs from 26.4 to 62.8 across the four columns, clearing both the conventional rule of thumb of 10 (Staiger and Stock, 1997) and their stricter threshold of 23.1 for a bias of at most 10 percent of the worst case. In the district-year panel the effective first stage is stronger still, between 31.5 and 77.4 across the three outcomes and the four specifications. While these ranges are reassuring, Table B1 recomputes the F implied by Conley errors at both bandwidths and all second stage results also report weak-instrument-robust inference alongside conventional standard errors, as Lal et al. (2024) recommend for observational instruments. Furthermore, Table B2 reports two falsification exercises that further support a strength of the first stage linked to the roll-out of the reform: the instrument only carries predictive power in central agrozones (in peripheral ones, most of them in the Amazon, expropriations of haciendas did not happen) and it does not explain pre-reform political outcomes, such as social movements, number of effective parties and victory margins in 1963.

Regardless of the strength of the instrument, if the zones and offices were drawn

for agronomy rather than for politics, it should be possible to check if pre-reform characteristics are correlated with how far a district sits from an office. Figure B1 checks that covariate by covariate (42 in total). Each row regresses one standardized pre-reform characteristic on exposure in the left panel and on the instrument in the right panel, across geographic, agricultural, demographic, economic and connectivity, political and colonial groups, with Agrarian Reform Zone fixed effects. Rows mark the 15 covariates we include as controls in all specifications. Against exposure, the gradients track the targeting of the reform towards the hacienda system, with imbalances led by the number of haciendas in 1961 and the share of the population living on haciendas in 1940. Against the instrument, pre-reform electoral outcomes are balanced, with the 1966 number of effective parties as the single exception. Remaining imbalances are either controlled for in every specification, as with the 1961 agricultural labour share, or belong to the remoteness channel (e.g. distance to province capitals) that the randomization inference and recentering exercises below model explicitly, drawing counterfactual configurations from the criteria that governed the ARZ offices' original placement.

Second-stage estimates. Tables 5 to 7 report the second stage. A one standard deviation increase in exposure raises the Number of Effective Parties by 0.71 to 0.94, between 20 and 26 percent of the mean, narrows the victory margin by 4.8 to 6.8 percentage points, and raises turnout by 3.5 to 6.5 percentage points (Table 6). Weak-instrument-robust inference is consistent with these results: Anderson–Rubin confidence sets exclude zero for the three outcomes in all four specifications, and so do the tF intervals of Lee et al. (2022). Following Ishimaru (2024), although the IV-OLS gap is sizable, the decomposition presented in Table 9 argues against an identified effect on a selected set of compliers or one driven by the intensity of treatment[14].

Randomization inference. Since the instrument is a function of the realized office locations, it is possible to ask whether the estimated gradient is specific to those offices or whether any configuration satisfying the same placement criteria would have produced it. Following Dell and Olken (2020), I draw counterfactual office locations from the criteria that governed the original placement based on the available documentation from the Ministry of Agriculture (land suitable for agriculture, road networks, and proximity to cities) and recompute the instrument for a thousand possible configurations (Figure

[14]The method takes up the gap between the instrumental variable and the least squares estimates and divides it into differences in covariate weights, differences in the weights placed on treatment levels, and differences in marginal effects. As Table 9 shows, the instrument does tilt the estimand toward sierra region and indigenous-majority districts, but neither the difference in the weights the two estimators place on covariates nor the difference in their weights across treatment levels accounts for any part of the gap, which loads almost entirely on the difference between the identified marginal effects.

6)[15]. The actual coefficient is then compared against the resulting placebo distribution. Figure 7 and Table B3 report the outcome. For the Number of Effective Parties the placebo distribution is centered near zero, at -0.029 against an actual reduced form of -0.184, so almost the entire gradient survives the comparison. The same holds for the broad-left vote share of the 1980s, and more sharply within 1961 indigenous-majority districts (Figure 8). In the full sample, placebo offices reproduce part of the left gradient, since remote districts are sierra districts and the sierra voted left in the 1980s. Within indigenous-majority districts, concentrated in the sierra, that proxy disappears: the placebo distribution centers at zero while the actual offices still move the left vote, which is where the argument predicts the reform's effect to concentrate. For turnout, the placebo distribution reproduces the actual gradient and the test does not reject, coming closest in the Spanish-speaking-majority subdivision of the sierra. The victory margin sits between the two, with roughly half of the actual coefficient surviving. In practice, this suggests that, for these latter outcomes, the instrument tracks a component of remoteness that this design cannot isolate.

Recentering. The recentering exercise of Borusyak and Hull (2023) makes the same comparison within a single specification. Because the instrument is a formula that combines the realized office locations with each district's predetermined position relative to the network, its expectation across counterfactual configurations, μ_i, isolates the part of distance that any placement of offices would have produced (Note 8). Table 8 shows that μ_i on its own does not predict the Number of Effective Parties, with a reduced-form coefficient of -0.052, while it does predict the victory margin and turnout. Instrumenting with the recentered instrument $z_i - \mu_i$ leaves the effect on the Number of Effective Parties intact and larger, at 1.556, and the effect on the victory margin at -7.06, while the effect on turnout falls to 0.155 and is indistinguishable from zero, only recovered from population weighted draws. Controlling for μ_i leaves the effects on the three outcomes significant.

Taken together, these results support a robust relation between exposure to the reform and political competition as measured by the Number of Effective Parties, but the cross-sectional nature of the design leaves remoteness as a potential confounder for turnout and the victory margin. The next two designs are built to net that out.

4.3. Regression Discontinuity Design: ARZ Core vs. Periphery

The instrumental variable design draws its power from a distance gradient that runs across the whole country, at the cost of the remoteness component that the exercises

[15] Note 8 details how the placebo placements are generated.

above isolate for the Number of Effective Parties but not fully for turnout and the victory margin. The regression discontinuity design gives up most of that power in exchange for a comparison that is local by construction, between neighboring districts on either side of a boundary within the same zone. As mentioned at the beginning of the section, the Agrarian Reform Zones were drawn by SIPA for agronomic reasons and do not map onto Peru's regional administrative borders. The location of the office inside a given ARZ can therefore define a core and a periphery, based on the plausibly stronger coordination capacities each ARZ office could have with the administrative region that contains it, as opposed to the rest of the area inside the same ARZ. The design exploits the resulting discontinuity in the intensity of the reform across this boundary (Albertus, 2020). Figure 9 shows the zones, the cores and the offices, and the intensity of the reform on either side of that boundary. Panel (b) zooms into the region of Cusco, where it is possible to note how the administrative boundary that divides ARZ 11 defines different district levels of exposure to land reform.

Let X_j be the signed distance in kilometres from district j's centroid to the nearest core/periphery boundary, negative in the periphery and positive inside the core, and let C_j indicate that the district lies inside the core. The estimated effect is the difference between two regression intercepts,

$$Y_j = \alpha + \tau\, C_j + \beta_1 Z_j + \beta_2 X_j + \beta_3 C_j \cdot X_j + \varepsilon_j \tag{2}$$

estimated by local linear regression on districts within a bandwidth b of the threshold, with bandwidths chosen to minimise mean squared error separately on each side, robust bias-corrected inference, and nearest-neighbor clustering by department. Fixed bandwidths of 25 and 50 kilometres are reported alongside the data-driven ones. The vector Z_j follows Albertus (2020): a second-order polynomial in the district centroid coordinates, the coordinates of the nearest point on the boundary, and boundary-segment fixed effects, so that districts are compared with others facing the same stretch of boundary rather than across the country. Two additional specifications add slope and the hacienda variables, and one adds the average of the 1963 and 1966 number of effective parties as a precision covariate. The design sample starts from the 1,570 districts of the reform era and drops the zones outside the scope of the 1969 Decree Law and the seventeen districts that were already urban in 1972, which leaves 1,002 districts, 519 in cores and 483 in peripheries (Table B6). Provincial capitals are excluded from estimation. Within 50 kilometres of the boundary the sample is 417 districts and the raw contrast in reform intensity is 21.7 percent of district area in cores against 11.9 in peripheries.

Continuity and first-stage estimates. Following Albertus (2020), the design is validated by testing for differences in predetermined district characteristics at the optimal bandwidth on either side of the cutoff (Figure 11, Table 10). Out of 26 covariates, only terrain slope, district population and the share of rural population living on haciendas in 1961 reject continuity at the ten percent level. Slope and the hacienda share are included as controls. The reform dose does jump at the boundary, by about 8 percentage points of district area in the specifications with MSE-optimal and 50-kilometre bandwidths (Figure 10, Table B7). Nearly every district near the boundary received some reform, so what shifts at the cutoff is the intensity of exposure rather than its receipt, and the first-stage effective F never exceeds seven. With this first stage, conventional fuzzy inference is unreliable (Feir, Lemieux and Marmer, 2016), so I read τ as an intention-to-treat effect throughout. The rescaled estimate, τ divided by the dose jump, appears in a single row as an effect per percentage point of exposure: with a continuous dose it is a weighted average of dose effects among districts whose exposure shifts at the cutoff (Angrist and Imbens, 1995; Dong, Lee and Gou, 2023), not a local average treatment effect on takers.

RDD estimates. Pooling the thirteen post-reform elections between 1980 and 2022, the boundary effect on the number of effective parties is 0.33 to 0.56 parties across the six specifications of Table B8, against a periphery mean of 3.23. The victory margin shows no pooled effect, and turnout has negative point estimates of between 1.5 and 2.6 percentage points. Figures 12 and 13 estimate the same discontinuity election by election at a common bandwidth, which holds the comparison window fixed and lets the trajectory rather than the pooled average carry the interpretation. Three patterns follow. First, the design has a placebo built into it, because the electoral record begins before the reform: the number of effective parties and the victory margin of the 1963 and 1966 elections are continuous at the boundary (panel (a) of Figure 12, Table 11). Second, the discontinuity in the number of effective parties is clear and robust, absent in the 1980s, appearing in the 1990s and peaking in the 2000s at about one full party before receding (Table 12). Third, the other two outcomes move only once each. The victory margin tightens in a single window, 1993 to 1998, and turnout falls in the transition elections of 1980 and 1983, by 4.2 points at the common bandwidth and 9.3 at the data-driven one, and is a precise null from 2002 onward. That early deficit sits on a periphery base of 56 percent and vanishes as participation converges toward the 84 percent of the 2000s. Although sample sizes within the bandwidth are small once the sample is split, the effect on the number of effective parties appears concentrated in Spanish-speaking-majority districts, a pattern consistent with differential mobilization across districts.

Robustness. Three exercises support reading the result on the number of effective parties as a feature of the boundary rather than of the specification, and a fourth locates where the design is more fragile. The design has few effective clusters, and Table B10 recomputes inference with clustering by boundary segment and with a wild cluster bootstrap: the effect of the 2000s rejects at every bandwidth and under every variance and the effect of the 1990s rejects at ten percent, while the pooled estimate weakens. Figure B2 sweeps fixed bandwidths from 15 to 75 kilometres and three kernels, and the point estimates barely move. Table B12 shifts the cutoff by ten and twenty kilometres to either side without finding a systematic effect on the number of effective parties, with one exception ten kilometres into the periphery that carries the opposite sign. Finally, Table B11 asks what else changes at the boundary and finds that departmental presidential turnout does jump, in both 1963 and 1980, while district electoral registration in 1933 is smooth, which is a reason to treat the turnout results with more caution than those on the number of effective parties.

4.4. Difference-in-differences

The third design exploits the timing of the reform against the two pre-reform elections of 1963 and 1966. I estimate an event study with the following structure:

$$y_{dt} = \sum_{s \neq 1966} \beta_s \left(LandReform_d \times \mathbf{1}\{t = s\} \right) + \mu_d + \tau_t + \varepsilon_{dt} \tag{3}$$

where y_{dt} is the outcome for district d in election t, μ_d and τ_t are district and election fixed effects, and ε_{dt} an idiosyncratic error, with standard errors clustered at the district level. The coefficients β_s measure the effect of exposure to the reform on the outcome in each election year relative to 1966, which is omitted. As before, exposure is standardized, so a coefficient reads per standard deviation of dose, which is 21.1 percentage points of district area. The main specification interacts each baseline covariate with the full set of election dummies, so that every predetermined characteristic is allowed its own free time path rather than a single level shift. I summarize the post-reform coefficients by their average rather than by a pooled regression.

Identification. Three features of this structure are worth noting. First, the measure of exposure to the reform comprises a single shock that aggregates accumulated expropriations during the decade of military rule, under which municipal elections were not held until 1980. In this sense, negative-weighting problems linked to variation in the timing of the treatment do not apply (Roth et al., 2023). Second, the treatment is a continuous dose rather than a binary indicator, the case analysed by Callaway, Goodman-Bacon and

Sant'Anna (2024). The average effect at a given dose is identified under the standard parallel-trends assumption, but reading a linear coefficient as the response to the dose requires a stronger one, so dose-response estimates are also reported. Third, the first municipal elections were in 1963, leaving only two elections before the reform. This implies that the placebo coefficient on 1963, which asks whether dose predicts the change between them, is a low-powered test (Rambachan and Roth, 2023). To mitigate this concern, I reestimate the event study with distance to the nearest office in place of dose and then recompute it for the thousand placebo office configurations, which tests the whole trajectory rather than a single coefficient. Turnout cannot be estimated in this design, because the municipal voter registry begins in 1980. In its place I report event-study estimates for votes cast per capita, which is available for 1963 and 1966.

Event study estimates. Figure 14 shows the event study for the number of effective parties. As shown in panel (a), the single pre-reform coefficient is flat, the post-reform path rises through the 1980s and 1990s, peaks in 2002 and then recedes, and the shape is unchanged when each covariate is given its own time path. The average post-reform effect is 0.07 to 0.10 additional parties per standard deviation of dose against a mean of 3.60, significant in all four specifications of Table B13 and robust to a departmental wild bootstrap. Panels (b) and (c) of the same figure show that the gradient picked up by the event study is independent of placebo configurations (randomization inference). Votes cast per capita as a proxy for turnout rise by 2.1 to 2.3 points of population per standard deviation of dose, the effect is present in both the 1980s and the 2000s, the 1963 placebo is flat, and the result survives a departmental wild bootstrap (Table B16). It does not, however, survive the placebo-office comparison. The victory margin shows similarly unstable results, aligned with a significant 1963 placebo (the dose already predicted the change in margins between the two pre-reform elections; Table B13, Panel B). It is negative and marginally significant without covariates, at about 1.2 points, and it collapses to zero once the covariates receive their own time paths.

Dose-response. Following Callaway, Goodman-Bacon and Sant'Anna (2024), Table B14 splits districts into quartiles of dose and estimates each against the untreated. This gives a better sense of what the single coefficient per standard deviation of dose summarizes. As shown, the pattern is monotone but concave. The lowest quartile, with a mean dose below one percent of district area, is indistinguishable from zero, while the second quartile at four percent already reaches 0.20 parties and the top quartile at fifty-one percent reaches 0.29. Most of the effect therefore appears between negligible and modest exposure rather than between modest and heavy exposure. Reassuringly, in the check proposed by Callaway, Goodman-Bacon and Sant'Anna (2024), dropping the zero-dose

districts and comparing each quartile against the lowest one instead leaves the estimates only between eight and twelve percent smaller. This evidence mitigates concerns linked to the sensitivity of the two-way fixed-effects coefficient with a continuous treatment to the size of the untreated group.

What kind of parties. If the reform widened competition, the question is which organizations filled the space. Peru's registry has distinguished national parties from regional movements only since 2002, so I classify each party by its footprint in that election: national in five departments or more, regional in two to four, local in a few districts of one department. Exposure moves about two points of the vote toward national parties, against a mean of 59.7, and three points away from regional ones, against a mean of 32.1, while the local share does not move (Figure B3, Table B15). The national gain grows across eras. This need not mean that national parties moved into exposed districts. A local race is cheapest to enter under an existing registration, and nationally registered parties routinely lend their label to local candidates, so new entrants show up under a national label. The organizations that lose ground, those confined to a few departments, are the usual vehicles of regional notables, the base the reform weakened. Before 1968 that meant local office rotating among the notables of each district (Muñoz, 2005; Remy, 2005). Since the 1990s it has meant coalitions of independents, assembled for one election around a candidate with a firm, a media outlet or a personal following (Levitsky and Cameron, 2003; Zavaleta, 2022).

Sensitivity to parallel trends. The pre-reform elections test parallel trends only up to 1966. For the decades after, the question is how large a departure from them the estimate can absorb. The Rambachan and Roth (2023) bounds let exposed and unexposed districts drift apart after the reform by up to a chosen multiple of the drift observed between 1963 and 1966, and report the confidence set for each multiple. Table B17 reports these bounds and the breakdown value at which the effect can no longer be distinguished from zero. The result survives only when post-reform violations are held below a quarter of the single pre-reform violation of 1963 to 1966. But the restriction caps drift between consecutive elections and lets it accumulate, so one pre-reform step anchors thirteen post-reform steps running to 2022, and Rambachan and Roth (2023) propose their $\bar{M} = 1$ benchmark for designs where the two counts are comparable. The smoothness restriction fails for the same reason. The test therefore cannot separate a fragile effect from a pre-period too short to discipline forty years of post-treatment drift.

Taken together, the results so far suggest that exposure to the reform left local elections with a higher number of effective parties, and the result holds whether identification comes from office proximity, from a boundary inside a zone, or from the timing of the

reform within a district. The evidence on turnout and margins of victory is design-dependent and harder to isolate from confounders such as remoteness gradients. If this supply-side account holds, the effects of the reform should also be visible in the characteristics of the candidates themselves. The next section takes up that possibility.

5. Land Reform and Candidate Selection

Where the electorate changes and elections become harder to win, the calculation of whom to field changes with it. Two things move at once. The attributes that win votes are not the ones that won them before, and the people who carry those attributes become easier to recruit or to place strategically. Shaukat (2019) formalizes the second case, showing that parties place higher-valence candidates where the race is close.[16] The candidate registries of the National Jury of Elections make the prediction testable, since they record the education, career and origin of everyone who stood for local office. I estimate

$$y_{ct} = \gamma_1 LandReform_d + X_c'\Gamma + X_d'B + \delta_z + \delta_t + \epsilon_{ct} \tag{4}$$

where y_{ct} is a characteristic of candidate c in election t, X_c collects candidate-level controls and X_d the district-level controls of Section 4.2, with Agrarian Reform Zone and election fixed effects. The endogeneity problem is the one already discussed, since exposure may be correlated with local conditions that independently shape who becomes a politician, so the instrument of Section 4.2.2 and the panel design of Section 4.4 are carried over unchanged. What changes is the unit of observation. Exposure and the instrument vary at the district level while the outcomes are measured on individual candidates, so standard errors are clustered by district throughout. The registries also set the window. Education, career and demographic outcomes are observed from 2002, when the candidate microdata begin, while the indigenous-surname measure can be built from the historical records back to 1963, which is why only the latter carries a pre-reform placebo.

Candidate quality. Table 13 reports years of education. The instrumented estimates are positive and significant throughout, at 1.07 to 1.52 years per standard deviation of exposure against a mean of 14.2, or roughly 7 to 11 percent. Table C1 shows that the gain is entirely tertiary. The share of candidates with university education rises by 29 percentage points against a mean of 48, while the shares whose highest attainment is

[16] Valence is a function of observable characteristics such as education, past experience or criminal record, valued positively regardless of a candidate's ideology.

secondary or technical fall. Field studies describe the same shift. By the 1990s being a professional had become a source of legitimacy in local races, and in the ten Ayacucho districts followed by Degregori, Coronel and Del Pino six of ten elected mayors had university or higher studies where in 1966 none had, not even the landowners (Remy, 2005, p. 117). Party experience moves in the same direction from a much lower base (Table 14). The instrumented estimates are 0.15 to 0.31 years against a mean of 0.61, although they are not significant in every column.

What the reform did not do is favor incumbents. Table C2 finds no effect on years already served in elected office or as mayor. The additional experience is partisan and it arrives from outside the state, since prior public-sector experience falls by 7 percentage points while private-sector experience rises by 20. It also arrives under a particular kind of label: candidates in exposed districts are 9.5 percentage points more likely to run for a party that competes nationally, against a mean of 47. This is the same displacement the aggregate vote shares showed in Section 4.4, where exposure moved the vote toward national organizations and away from regional ones.

Indigenous political entry. A more competitive arena rewards not only the attributes that make a candidate effective in office but also those that make voters feel represented. The reform touched one of these directly: it was presented as a vindication of the indigenous peasantry, with the passage from *indígena* to *campesino*, a peasant with title to land, at the center of the government's own account. Table 15 tests whether that symbolic vindication translated into actual indigenous entry, using the surname-matching procedure of Artiles (2022) to identify candidates carrying two indigenous surnames. The design has a placebo built in, since the same estimate can be run on the pre-reform elections of 1963 and 1966, and there both the candidate pool and the elected are flat. After the reform the two margins separate. Pooling 1980 to 2022, exposure raises the probability that the elected mayor carries both surnames by 4.2 percentage points against a mean of 6.0, while the effect on the candidate pool is 2.8 and not significant. Restricting to 2002 onward the elected effect nearly doubles, to 7.4 points (Table 15). Entry into the pool arrives late and only in the last era, at 5.4 points in 2014 to 2022 against 10.6 for elected mayors (Figure 16, Table C3). This evidence suggests the reform did not remove the barriers to standing for office so much as it changed what happened to those who stood.

Figure 17 and Table 16 repeat the exercise with the event study of Section 4.4, so that identification comes from the timing of the reform within a district rather than from office proximity. The pre-reform coefficient sits at zero. The elected margin turns on in 2002, at 2.7 to 3.2 points, and the candidate pool follows from 2006, peaking in 2018 at

1.5 points against 3.0 for elected mayors. Two designs that draw on different sources of variation therefore produce the same sequence, in which the winning margin moves first and the pool of candidates moves later.[17]

Who else runs. Two further characteristics have a direct reading in this setting. Women are historically excluded from the governance positions that require land tenure, and rural districts lose their young to intermediate cities. Table C4 is consistent with both. Female candidacies rise by 4.6 percentage points against a mean of 6.8, although the effect does not carry to elected mayors, and both candidates and elected mayors are older, by 2.5 and 3.2 years against means near 45. Candidates in exposed districts are also considerably less likely to have been born in the district where they run (Table C5).

Taken together, these results suggest the reform changed who competes for local office and not only how many compete. Candidates in exposed districts are more educated, more often outsiders to the district and the local state, and more often run under a national label. The indigenous result deserves emphasis. The reform targeted the group that the literacy franchise kept out of politics until 1980, and in exposed districts indigenous candidates reach office before they enter the pool in larger numbers, a sign that voters changed before candidates did. Ethnographies from Ayacucho, Cusco and Piura describe that entry as a contest between members of peasant communities (comuneros), who begin to appear on council lists, and the descendants of landowners, now merchants (Remy, 2005, pp. 116–117). The next section develops a theoretical interpretation of these empirical patterns, linking the breakdown of landed elites' electoral control to the changes in political competition and candidate selection documented above.

6. Interpretation: capture and its breakdown

The two previous sections describe districts that became more contested and began to field different candidates, and they leave open why expropriating land should have produced either. This section sets out a stylized model of local electoral capture and its breakdown to interpret those results and to organize the mechanism tests that follow. I state the objects that carry the argument and the predictions they generate, and leave the derivations and the auxiliary assumptions to Appendix E. The model is static, and it is meant to interpret the pattern of results rather than to be calibrated or structurally estimated.

[17] The discontinuity design of Section 4.3 is not extended to candidates. The jump in exposure it isolates is about eight percentage points of district area, well under the 21 that one standard deviation of the instrument moves, and its first stage is weak. An effect of the size found here, 4.2 points on elected mayors per standard deviation, would amount to about 1.6 points at the boundary, below what the local sample can detect. In addition, most candidate outcomes are observed only from 2002 onwards.

6.1. Two margins of capture

Before the reform a landlord governed local politics through two instruments, and they worked on different groups. The setting is a district with landlords, dependent workers employed on the estates and self-employed peasants, and two parties: R, which protects land rents and is the landlords' party, and L, which favors redistribution and is the party workers and peasants prefer. Voting is observable or punishable, and parties pay for votes.

The first instrument is the one Baland and Robinson (2008) model, and it runs through the labor contract. Production on the estate is subject to moral hazard, so the landlord pays an efficiency wage and the worker earns an employment rent r above what she would earn on her own. Voting for R costs her an ideological loss s_L. Conditioning continued employment on that vote is profitable exactly when the rent exceeds the loss, $r > s_L$, because a deviation is observed and costs her the rent. The same arrangement makes the landlord the cheapest route to the vote: R obtains a dependent worker's vote through the landlord at v_L, the value party L places on a vote, while an independent peasant's vote costs more, since R must also compensate her ideological loss, so parties buy votes through landlords rather than directly (Baland and Robinson, 2008, Propositions 1 and 2). I call this the *freedom margin*, and it decides how those who vote cast their ballot.

The second instrument is the extension of this paper, and it runs through the franchise. In Peru, until the 1979 Constitution, only literate adults could register, and in a rural district a school required the estate's land, its buildings, its tax base and the release of children from work. Let i^0 be the district's illiterate mass. The margin borrows an idea documented in other agrarian settings, that landed elites hold back mass schooling because they gain little from a literate workforce and lose political control to it (Bourguignon and Verdier, 2000; Galor, Moav and Vollrath, 2009). Here the literate franchise gives that choice an electoral edge: landlords withhold schooling whenever the political cost of enfranchising the newly literate exceeds the productivity gain, and the whole of i^0 then stays outside the electorate. I call this the *eligibility margin*, and it decides who votes at all. Total capture is the sum of the two,

$$\Gamma^0 = \underbrace{m^0}_{\substack{\text{captured votes:}\\ \text{literate dependents}}} + \underbrace{i^0}_{\substack{\text{excluded voters:}\\ \text{illiterate dependents and self-employed}}} = D^0 + E^0, \tag{5}$$

where m^0 is the mass of literate dependents whose vote the landlord captures, D^0 the dependent mass as a whole and E^0 the illiterate dependents and self-employed. The

two terms leave different traces. Captured votes shape the outcomes of the pre-reform elections of 1963 and 1966, where the landed families' hold on office is visible (Section 7.2), while the excluded cast no votes at all and can only be counted through the 1961 illiteracy rate (Appendix E.2).

6.2. What the reform breaks: released voters and their political weight

Expropriation removes the freedom margin at its root. On an expropriated estate the employment relationship no longer exists, the rent is zero, condition $r > s_L$ fails and the worker votes sincerely. The eligibility margin is removed nationally rather than locally, since the 1979 Constitution enfranchises the illiterate everywhere at once. What varies across districts is therefore not who may vote but whether the released voters count.

Let α denote exposure, the share of district area expropriated, and let $B_d(\alpha) = \alpha D^0 + E^0$ be the mass that is both enfranchised and free of landlord control, with D^0 and E^0 as in equation (5). A citizen in that mass carries weight in what parties offer only if she is reachable and coordinated with others like her, and that has to be built. Adjudication required beneficiaries to receive land through cooperatives, recognized communities, irrigation boards and unions, so exposure raised local organizational capacity Org_d. Letting $h(Org_d) \in (0, 1]$ be the share of the mass that capacity makes responsive, the effective political weight of the released bloc is

$$\omega_d(\alpha) = h\big(Org_d(\alpha)\big) \cdot B_d(\alpha), \qquad \frac{d\omega_d}{d\alpha} = \underbrace{h'\big(Org_d\big)\frac{dOrg_d}{d\alpha}\, B_d(\alpha)}_{\text{organization}} + \underbrace{h\big(Org_d\big)\, D^0}_{\text{liberation}} > 0. \quad (6)$$

The two factors enter as a product, so neither a large released mass without organization nor dense organization over a small released mass produces much. That Org_d rises with α is an assumption grounded in the adjudication rule rather than a result, and it is the hinge of the argument.

One more feature of the break matters for how exposure maps into outcomes. Control is not lost in proportion to the land taken. An estate can direct votes only while it retains enough land to pay the employment rent and to make the threat of dismissal credible, so once expropriation pushes it below that size, control fails altogether rather than shrinking (Anderson, Francois and Kotwal, 2015). This has a testable shape. If the estates of a district cross that threshold at similar levels of exposure, the district's response jumps at a modest dose and then flattens, and if they cross it at different levels it builds gradually. The dose-response estimates of Section 4.4 (Table B14), where most of the effect appears between negligible and modest exposure, are closer to the first

case.

Everything the model says about the reform runs through ω_d. A heavier organized bloc raises the vote a challenger can expect, so entry pays and more parties compete for a narrower margin. It also changes who is worth fielding, since candidates with the attributes the bloc values gain support and organization lowers the cost of finding them. On turnout it cuts both ways, adding autonomous participation while the participation landlords used to enforce disappears. The next subsection states these implications and what each leaves to be tested.

6.3. Implications: political competition, representation and participation

Four predictions follow. The first and fourth concern the competition outcomes of Section 4, the third the candidate outcomes of Section 5, and the second the vote for parties representing peasant interests, whose evidence sits with the mechanisms in Section 7.4. The first three run through ω_d, and the fourth the model leaves ambiguous by construction.

Implication 1: more parties. The larger the share of area expropriated in a district (α), the more a challenger stood to gain from entering, on three counts (Appendix E.4). First, the released bloc grows with exposure and, where organized, votes together, so the vote a challenger can expect rises with it. Second, expropriated landlords lose the means to punish those who run against them, so the cost of entering falls. Third, the organizations the reform induced make it cheaper to field a candidate the bloc prefers. The result: more challengers find entry worthwhile, so districts with more exposure should show more effective parties, the outcome of Section 4.[18]

Implication 2: more support for parties representing peasant interests. The same exposure moves the policy platform a party has to offer to win the district (Appendix E.5). As the share of area expropriated rises, the votes landlords direct fall away and the released bloc gains weight, so that platform shifts toward the peasant ideal point, keeping preferences fixed. The result: districts with more exposure should give more votes to pro-redistribution parties, those representing peasant interests, the 1980s broad-left share of Section 7.4, with no change in stated attitudes.

Implication 3: candidates with the attributes the released voters value. Exposure also changes which candidates parties choose to put forward (Appendix E.6). As the released

[18]Entry means a party running under an existing label or a new one. Since party law makes the first route cheaper, new entrants should appear under established labels, which is how Section 4.4 reads the shift of the vote toward parties of national reach. The margin of victory is not derived, but the same movement, directed votes falling while challengers' shares rise, works to compress it.

bloc gains weight, the attributes it values bring in more votes, and the organizations the reform induced make the people who carry them easier to recruit, for example leaders of those organizations or candidates who share the bloc's identity, the profile that case studies describe among post-reform mayors (Remy, 2005, pp. 116–117). The result: such candidates become both more valuable to field and cheaper to find, so districts with more exposure should field and elect candidates with those attributes, the quality (education) and representation (indigenous surnames) traits of Section 5.

Implication 4: turnout rises only where organization compensates for lost mobilization. Turnout has two parts in the model. Captured workers always vote, since a directed vote is worth something to the landlord only if it is cast and he sees to it that it is. A free citizen votes when the payoff from the best party on offer covers a participation cost that organization lowers, net of the sanction for abstaining under compulsory voting (Appendix E.7). Expropriation removes the first part and organization and entry raise the second, so the effect on turnout is ambiguous, positive only where the participation organization adds outweighs the mobilization that capture enforced.

The model offers an account of the results that rests on three claims, for each of which empirical support can be provided. First, the pre-reform condition $r > s_L$ requires that landlords converted labor-market power into political control, a statement about the period before any of the outcomes above are observed. Second, the break of the freedom margin requires that expropriated elites lost both the dependent votes and the punishment capacity that sustained them. Third, because ω_d is a product, the argument turns on exposure having left organizational capacity behind, which the adjudication rule makes plausible. Section 7 takes these claims in turn, providing empirical evidence for each, and then runs a mediation exercise to test how much of the effects each channel carries.

7. Mechanisms

The results so far show districts more exposed to the reform becoming more competitive and fielding different candidates. Section 6 offers one account of why. This section provides a set of empirical exercises supporting that account. First, it asks whether landlords' electoral control was in place before the reform and whether expropriation ended it. Second, it turns to the hinge of the argument: whether organization outlived the cooperatives the reform created, and whether it can be traced back to the cohorts of political activation. It then asks whether that organization came with more support for parties representing peasant interests. Third, it shows that this political activation came

at a welfare cost, borne by the cohorts exposed at a different age. The section closes with a mediation exercise that asks how much of the effects each channel carries.

7.1. Landlords' electoral control before the reform

The two margins of Section 6 leave different traces in the historical record. No source of the period records how hacienda workers voted, so the evidence on the freedom margin is indirect. What the record does show is that landowners were the political class. Sugar planters rose to national office under the Partido Civil, which governed from 1899 to 1919 on a program that included a restricted franchise: two of them held the presidency and almost all sat in Congress (Gonzales, 2020, pp. 22, 55 and 79), while the highland departments, over-represented in Congress, were represented by their own landowners (Miller, 1982, pp. 116–117). Where local office was elective it was so on the landowners' terms. Votes in the irrigation boards were weighted by land and water, so the large estates elected themselves and their allies (Gonzales, 2020, p. 83), and municipal office rotated among landowners and the notables close to them until the 1960s (Muñoz, 2005, p. 16). Section 7.2 shows those families holding local office before the reform and leaving it afterward.

The eligibility margin can be observed, because under a literate franchise keeping workers out of the electorate meant keeping schools out of the district, and the supply of schooling is documented at least since 1918. In a companion paper we instrument the share of a district's population living on *haciendas* in 1876 and estimate its effect on local public goods through the first half of the twentieth century (Espinoza and Paredes, 2026). Districts with more hacienda population had fewer schools and fewer teachers per 10,000 inhabitants in 1918 and 1930: in the instrumented estimates, one standard deviation more hacienda population in 1876 meant roughly eight to nine fewer of each, against means of 13 to 21, a gap that fades by 1950. There is no comparable shortfall in distance to the road network, in land titling or in irrigation. The contrast across public goods separates the withholding of schooling from general backwardness.

Figure D1 closes the link from schooling to the franchise. The 1933 Electoral Census is the earliest source that reports registered voters by ethnicity. Across provinces, conditional on geography and on the population, urbanization, indigenous share and economic structure of 1876, a higher share of the population living on *haciendas* in 1876 predicts a lower share of the indigenous population registered to vote six decades later. The relation is steepest in the highlands, where the indigenous population was concentrated and the literacy requirement bound hardest.

7.2. The erosion of elite capture

If the freedom margin broke, the families that governed through it should have left office. The model is specific about why the break need not be gradual. The rent that sustains control rests on the outside option behind it and on the punishment that enforces it, both of which scale with the land the estate retains, so below some threshold control fails rather than weakens. Figure 18 provides a first piece of evidence: it shows the raw composition of elected local office across the reform.[19] Surnames of documented landed-elite families fall by 31 percent between the pre-reform elections and 1980–2002 (panel a), while a placebo list built from the post-reform economic elite of around 2000, purged of families with pre-reform presence, moves by one percent under the identical matching rule (panel b). Historical case studies describe the same turnover, from landowners and their allies to merchants, professionals and comuneros, under way by 1966 and completed in the 1980s. In Huari, for instance, the new groups reached office through the United Left, which won the provincial council three times in a row in the 1980s as the old elite withdrew (Remy, 2005, pp. 116–118).

Table 17 supplies the causal counterpart. On a cross-section of 1,378 districts I define incumbent families as within-district surname networks over officials elected in 1963 or 1966, and take as outcome the share of local office those families hold afterwards, instrumenting exposure with distance to the nearest Agrarian Reform Zone office as in Section 4.2. A one standard deviation increase in exposure lowers the incumbent families' share of elected offices by 10.8 percentage points in 1980–83 and by 9.6 points over 1980–2002, against bases near one half. The candidacy margin falls at least as much, by 12.7 and 9.9 points, suggesting incumbent families withdrew from competition rather than ran and lost, and their advantage in winning conditional on running does not respond to exposure at all. Three appendix tables qualify the result. Old families have not disappeared, since 37 percent of candidacies and 41 percent of elected mayors still come from them four decades later (Table D1). The change in family concentration is a precise zero (Table D2), so the reform rotated the families exercising local power rather than dispersing it, and survival rates confirm the same reading (Table D3).

Two further checks bear on the reading. The reform did not target districts where a few families held more of the pre-reform seats, so the exit of incumbent families is an effect of exposure rather than of selection (Table D5). Furthermore, the effects on the number of parties and on turnout are no larger where those families were more entrenched (Table D4), which suggests that competition opened through the released voters and their organizations rather than through the removal of elites as such, the

[19] Construction and the exclusion rules are in Note D1 of Appendix D.

point the mediation tests of Section 7.6 take up.

Taken together, the families that had governed local politics through the labor contract left office where the reform hit, and they left by not running rather than by losing. That is what the model predicts once the employment rent and the punishment behind it are gone: control does not weaken gradually, it stops working. Whether the vacated space was filled by an organized electorate, as the model requires, is the next question.

7.3. The organizational legacy of the reform

Releasing voters is not the same as giving them weight. In the model, a large released mass without organization produces little, and this is why the franchise reform of 1979 on its own would not have delivered competition.[20] The adjudication rule is what makes the hinge plausible. Land went to collective enterprises rather than to individuals, production cooperatives (CAPs) on the coast and SAIS in the highlands, and the government meant them as instruments of collective action as much as of production (Mayer, 2009; Thorp and Bertram, 1978; Schirmer, 1977). Case studies describe the political mobilization and learning that followed inside them (McClintock, 1981; Cant, 2021). Most failed within two decades (Thorp and Paredes, 2010). The question is therefore whether the organizational capacity they created outlived them. Three exercises trace that capacity once the cooperatives had dissolved: in membership in agrarian organizations in 1994, in membership in local organizations of any kind among the cohorts that came of age during the reform, and in the places where membership clusters today.[21]

The first uses the 1994 agricultural census. Table 18 instruments exposure as in Section 4.2 and finds that in 1994, well after the cooperatives were gone, exposed districts still had 11.7 more percentage points of agricultural producers in some organization against a base of 34.6, with the effect concentrated in irrigation users, the institutional heirs of the 1969 water law passed in the same legislative package as the reform itself.

The second exercise changes the source of variation. Following Albertus, Espinoza and Fort (2020), I compare adults born in the same district who were of different ages when the reform reached it, using the citizen-participation module of the Peruvian national household survey (ENAHO) for 2004 to 2017 and membership in at least one local organization as the outcome. The design is a cohort difference-in-differences: within

[20] For the muted effect of a franchise reform where landed elites kept de facto power, see Chapman (2026).

[21] The first link, from expropriation to the cooperatives themselves, is not measured district by district, because the records are of expropriation rather than adjudication. It rests on the law: under the military government land could be adjudicated only to collective units, so exposure and the formation of cooperatives were close to one and the same. The one organizational form created by the reform that can be observed directly, the recognition of peasant communities, enters the third exercise.

district of birth, and net of birth-cohort and survey-year effects, it asks whether the gap in membership between those who were young when the reform arrived and those who were already adults widens with the district's exposure (Note D2). Figure 19 shows the result. For those aged 12 to 25 when the reform arrived, membership today is 2.3 points higher on a base of 30 percent under full exposure, and it is flat for those exposed as young children. Notably, the window that responds is the one of political activation (12–25) rather than the school years (0–11).

The third exercise asks whether the organizational footprint is tied to the places the reform reached. Peasant communities (*comunidades campesinas*), rural communities granted legal status by the state, carry the year in which this recognition was granted, so I ask whether membership among households surveyed in the ENAHO declines with distance to the nearest community recognized during the reform, and compare that gradient with the one around pre-existing communities, within district of residence and survey year (Note D3). Figure 20 shows a gradient in irrigation-users membership around the reform-era communities and a flat profile around the historical ones: what predicts organization is recognition by the reform, not the historical presence of a community. Figure 21 splits the gradient by whether the district had recorded peasant mobilization before the reform. The effect is about four times stronger where it had, while the gradient around the historical communities stays at zero, which favors activation of an existing capacity over creation of a new one and matches the regional historical accounts of Cant (2021). Robustness is in Figure D2 and Tables D6 and D7.

The three exercises approach the same question from complementary angles, what producers belonged to in 1994, which cohorts joined organizations later in life, and where membership clusters today. As shown, the reform left organizational capacity behind, it did so among those old enough to use it politically, and it did so most where something was already there to activate. What the model still needs is that this capacity changed what politics delivered.

7.4. The freed vote: support for redistribution or a shift in broader preferences?

The reform could change what politics delivered in two ways. Implication 2 is the first: as the released bloc gains political weight through organization, support for parties representing peasant interests rises. The second is that the reform changed not only the political weight of the released voters, but broader political preferences, such as trust in political institutions or support for democracy. The historical context makes that version worth testing, because the regime did not only transfer land: it tried to shape beliefs through a nation-building narrative. It framed expropriation as the vindication of the

indigenous peasantry and as a first step toward other social rights, which could have changed what beneficiaries expected from the state. It built a propaganda apparatus around the reform (Cant, 2012, 2017) and created SINAMOS to organize the population behind it while parties were suspended, an explicit attempt at political socialization that could have shaped trust in parties and institutions. The cooperatives gave many beneficiaries their first experience of electing representatives, which could have shaped how they valued democratic procedure. Furthermore, the memory of the reform remains salient in electoral campaigns to this day (Cant, 2021; Aguirre and Drinot, 2017), so any imprint could have lasted. The distinction matters for what follows: the shift of the freed vote toward parties representing peasant interests is one of the channels Section 7.6 tests as carriers of the effect on political competition.

The two accounts can be told apart in the data: a stronger bloc with fixed preferences predicts a shift in the vote without a shift in attitudes, while a change in preferences predicts both. The vote shifted. Table 19 takes vote shares of the left in the municipal elections of 1980 to 1989, the period in which party labels are still classifiable, and finds that a one standard deviation increase in exposure raises the broad left's vote share by 11.9 percentage points against a mean of 21.1, with candidacy and elected shares moving by similar amounts and the Marxist left alone accounting for about half. Revealed demand in exposed districts shifted toward redistribution, as the re-weighting predicts, consistent with the rural councils the United Left won through peasant organizations in the 1980s (Remy, 2005; Seligmann, 1995).

Stated attitudes, by contrast, did not shift. Figure 22 applies the cohort comparison of Section 7.3, adults born in the same district who were of different ages when the reform arrived, to indices of trust in political institutions and of support for democracy, and returns precise zeros across every exposure window, including the formative one in which organization itself responds (Note D2). The contrast is informative. The same design that finds organization among cohorts aged 12 to 25 finds no change in their trust in institutions or in their support for democracy. That is what the model predicts, and what an account in which the reform changed broader political preferences does not. Preferences enter as a channel only in this sense: what changed was not what the released voters wanted but how much their preferences counted, and the left vote of the 1980s is where that change is observed.

7.5. Economic change alongside political change

The economic impacts of land reforms have motivated a large literature. Across contexts and research designs, these studies report mixed effects on agricultural productivity,

structural transformation, growth and rural welfare, with results that depend on how the reforms were designed and on the implementation constraints they faced (Besley and Burgess, 2000; Banerjee and Iyer, 2005; Adamopoulos and Restuccia, 2020; Montero, 2022; Kim and Wang, 2025; Mitra et al., 2026). However, even studies that causally identify economic costs of land reform, namely productivity losses or slower structural transformation, explicitly abstract from weighing them against the political effects, while acknowledging that these were not only a central motivation for these reforms but also a necessary part of a broader welfare assessment (Adamopoulos and Restuccia, 2020; Kim and Wang, 2025). This section takes the opposite route and reads the political trajectories identified above alongside the economic changes the reform set off, as an approximation to specific dimensions of these trade-offs.

The Peruvian evidence shows the same mixed pattern of economic effects, varying over time and across regions. In the short run the reform correlates with economic dynamism: instrumenting exposure as in Section 4.2, exposed districts in the 1981 census have higher household income per capita and less child labor (Table 20). However, these gains are not tied to the reform's organizational footprint. The community-distance design of Section 7.3 finds no gradient in them around reform-recognized communities, while it detects the historical gradient sharply, so the null is not a matter of power (Figure 23).

Over the long run the initial fall in land inequality reverses. At the 1961 baseline the top one percent of owners held 80 percent of agricultural land, and the reform redistributed about half of that area, but between the agricultural censuses of 1994 and 2012 land inequality in exposed districts rises back by enough to undo the fall measured in 1994, most strongly in the highlands, while the technification indicators move in different directions (Table 21). The 1994 baseline is itself a limitation, since it is the first agricultural census after the reform and already sits nearly a decade after the last expropriation, so the inequality it records is a resting point rather than an immediate effect. Four decades in, the welfare picture is mixed at best: poverty shows no effect in the full sample and expenditure inequality is higher (Table 22).

The cohort design makes it possible to weigh the economic and the political legacies of the reform against each other, by showing both sides within a single specification. On the economic side, the cohorts exposed as young children (0–11) carry a lasting cost, with lower consumption, more agricultural work and less out-migration as adults, concentrated among the indigenous, for whom poverty rises by more than five percentage points and the share working in agriculture by seven (Figures 24 and D7). On the political side, the cohorts exposed at the age of political activation (12–25) are the ones who organize, plausibly the same cohorts behind the entry of new and indigenous can-

didates documented above. The reform's long-run economic incidence and its long-run political effect therefore fall on different cohorts.

7.6. How much do these channels explain?

The four preceding subsections follow the structure of the model. Capture rested on two margins and both were operating. The freedom margin broke and the families that used it left office and stopped contesting. Organization survived the collapse of the bodies that created it. The freed vote shifted toward parties representing peasant interests while stated attitudes did not. The economic record was mixed, with early gains, a later reversal in land inequality and lasting costs for those exposed as children. Each of these is an effect of the reform on an intermediate outcome, and the step that usually follows is to assert that these intermediate outcomes explain the effect on political competition. That step has a limitation: exposure is instrumented, but none of the intermediate outcomes is, so a channel that responds to the reform and correlates with competition need not carry any of the effect.

Kwon and Roth (2026) make that step testable. Under the sharp null that the entire effect of the instrument on the outcome runs through a stated mediator, the instrument is a valid instrument for that mediator, so the null inherits the testable implications of instrument validity, and a rejection comes with a lower bound on the share of the effect that must operate outside the stated channel. I take five mediators from the preceding subsections, each measured before the outcome window and binarized at its within-zone median: elite replacement, the share of local office held in 1980–83 by the families that governed before the reform; organization, the share of agricultural producers who belonged to an organization in 1994; left voting, the broad-left vote share in the municipal elections of 1980–89; and two economic conditions in 1994, the incidence of hired labor and land inequality. The outcomes are the number of effective parties and turnout in the elections of 2002 to 2010. Figure 25 sets out the decomposition and Note D5 the implementation.

Figure 26 and Table 23 show the results, and they differ by outcome. For the number of effective parties, full mediation through organization alone cannot be rejected, nor through left voting alone, so either could carry the entire effect on this outcome. Elite replacement alone is rejected, and so is land inequality, while hired labor is marginal. For turnout, every single channel is rejected, with lower bounds between five and nine percent of always-takers moved. Taken in pairs the picture resolves: organization and elite replacement jointly cannot be rejected for either outcome, and adding left voting drives the unexplained mass for turnout to zero.

Three readings follow. First, the channels measured are jointly sufficient to account for both effects, but no single one accounts for turnout, which matches a participation margin the model leaves ambiguous. Second, elite replacement is the one channel that cannot carry the effect on its own: removing the incumbents was not what made elections competitive, it worked alongside an organized electorate. Monge's reading of the rural vote through 1993 points the same way: absenteeism was highest in rural provinces except where peasant communities were dense, which he took as communities acting as channels of political integration (Remy, 2005, p. 117). Third, the economic conditions the reform rewrote do not carry the political effects on their own: land redistribution opened local politics through organization, not fully through land inequality or rural wage labor.

8. Conclusion

The notion of *political legacy* from a historical perspective relates to how collective memories are constructed. The empirical analysis in this study aims to recover broad patterns from a set of complex historical relationships, highlighting how changes in economic power and collective organization can reshape who is able to contest political power. From this perspective, the mixed or even negative economic consequences of the Peruvian agrarian reform are only one dimension of its legacy. The political changes it set in motion are another.

The empirical strategy exploits the reform's administrative geography. The expropriations were implemented through regional offices and Agrarian Reform Zones whose locations and boundaries had been established nearly a decade earlier for different purposes. I combine digitized records covering the territorial deployment of the reform with district-level results from every direct municipal election from 1963 to 2022 and candidate-level records, providing broad spatial and temporal coverage of local political change. I then exploit variation in exposure generated by proximity to reform offices, discontinuities at within-zone boundaries, and elections before and after the reform to study its effects on political competition, electoral participation, and candidate selection.

The results show a persistent increase in local political competition. Across the three designs, greater exposure to the reform raises the number of parties contesting local elections. Turnout also rises with exposure, although these estimates are harder to isolate from remoteness in a context of nationwide enfranchisement and compulsory voting after 1980. The reform also reshaped the supply side of local politics, changing who runs and who is elected: candidates became more educated and more experienced

in party politics, while indigenous representation among elected mayors increased. The mechanism evidence points to three linked changes. Incumbent landed families withdrew from local office, while agrarian organization outlived the cooperatives and revealed political demand shifted toward the left. Mediation tests are consistent with persistent organization or early left voting carrying the effect on political competition. Elite exit alone cannot account for it, and turnout responds only where this exit combines with organization. The economic changes the reform set off cannot fully explain the observed responses in competition and participation.

Read against the questions that open the paper, the results suggest that historically excluded groups can develop the capacity to compete for political power under autocracy, when redistribution weakens their economic dependence and organizes them, and that this capacity can outlive the regime that built it. Peru's reform dismantled an economic basis of landlord control and organized politically excluded rural workers, and part of that organizational capacity survived both the cooperatives and the regime that created them. In this sense, the Peruvian experience shifts attention upstream from the moment of democratization itself, toward capacities that can begin to form while political competition is still constrained and become politically consequential once it opens. The results also qualify the idea that dismantling elite economic power is sufficient to open politics: formerly excluded citizens gained political weight where weakened landlord control was accompanied by enfranchisement and durable local organization. More broadly, the results illustrate how the political and economic legacies of redistribution need not move together, so neither dimension alone provides a complete assessment of what such historical transformations accomplish.

Although it is difficult to offer a definitive interpretation of the reform's political legacy, historical accounts and work from other social sciences consistently highlight the local political changes documented here (Seligmann, 1995; Mayer, 2009; Cant, 2021). This leaves scope for further research, particularly on a more precise historical understanding of landed elites' and beneficiaries' economic and political strategies in response to the reform, as well as on its persistent effects on a broader set of social preferences and beliefs, including norms of cooperation and the conditions under which the reform's memory is politically activated. As suggested by Mayer (2009), no land reform has been conceived without an image of what its political future would be. The Peruvian case is perhaps a good example.

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List of Figures and Tables

I. Historical background 54

Figure 1 Timeline: political regimes, party systems, and municipal elections in Peru 54

Figure 2 Evolution of Land Reform Expropriations 1969–1980 55

Figure 3 Agrarian Reform Zones and District-level Land Reform Exposure 56

Figure 4 The Agrarian Reform Zones: Agro-ecological Conditions, Infrastructure, and Access to ARZ Offices 57

Figure 5 Municipal Electoral Outcomes 1963–2022 58

II. Political competition 59

Instrumental Variables 59

Figure 6 Randomization inference: inputs, counterfactual configurations, and construction example 59

Figure 7 Randomization inference: placebo distributions and distance profiles . . 60

Figure 8 Randomization inference - broad-left vote share, 1980–89 61

Table 1 OLS: Land-reform exposure and the Number of Effective Parties (1980–2022) 62

Table 2 OLS: Land-reform exposure and Turnout (1980–2022) 63

Table 3 OLS: Land-reform exposure and the Victory Margin (1980–2022) 64

Table 4 First stage: Distance to Agrarian Reform Zone offices and land-reform exposure (1980 cross-section) 65

Table 5 IV: Land-reform exposure and the Number of Effective Parties (1980–2022) 66

Table 6 IV: Land-reform exposure and Turnout (1980–2022) 67

Table 7 IV: Land-reform exposure and the Victory Margin (1980–2022) 68

Table 8 Recentered IV (Borusyak–Hull): expected office distance from counterfactual configurations 69

Table 9 Decomposition of the IV–OLS gap (Ishimaru 2024) 70

Regression Discontinuity Design 71

Figure 9 Agrarian Reform Zones: cores, peripheries, and reform intensity at the core/periphery boundary 71

Figure 10 RDD first stage: land-reform dose at the core/periphery boundary 72

Figure 11 RDD balance at the core/periphery boundary: standardized estimates for 26 predetermined characteristics 73

Figure 12 NEP and victory margin at the core/periphery boundary, 1963–2022 . . . 74

Figure 13 Turnout at the core/periphery boundary, 1980–2022 75

Table 10 Smoothness of predetermined characteristics at the core/periphery boundary . . . 76
Table 11 Pre-reform electoral competition (1963 and 1966) at the core/periphery boundary . . . 77
Table 12 The boundary effects by era: rise in the number of effective parties, competitive tightening, and transitional demobilization . . . 78
Differences in Differences . . . 79
Figure 14 NEP in the panel design: event study and randomization inference, 1963–2022 . . . 79
Figure 15 Votes cast per capita in the panel design: event study and randomization inference, 1963–2022 . . . 80
III. Candidate selection . . . 81
Instrumental Variables . . . 81
Figure 16 IV effect of land-reform exposure on indigenous representation, by election, 1963–2022 . . . 81
Table 13 Land-reform exposure and candidates' years of education (2002–2022) . . . 82
Table 14 Land-reform exposure and candidates' party experience (2002–2022) . . . 83
Table 15 Land-reform exposure and indigenous political entry: IV estimates by window, 1963–2022 . . . 84
Differences in Differences . . . 85
Figure 17 Indigenous political entry election by election: candidates and elected mayors with both surnames Quechua or Aymara . . . 85
Table 16 Land-reform dose and indigenous political entry: mayoral candidates and elected mayors with both surnames Quechua or Aymara . . . 86
IV. Mechanisms . . . 87
Local political capture . . . 87
Figure 18 Elite exit, indigenous entry: the changing composition of elected local office 87
Table 17 Land-reform exposure and the replacement of incumbent political families 88
Social organization . . . 89
Figure 19 Organizational membership and land reform: cohort evidence . . . 89
Figure 20 The reform's organizational footprint in space: irrigation-users membership and distance to reform-recognized communities . . . 90
Figure 21 Activation vs. amplification: where the organizational footprint bites . . . 91
Table 18 Land-reform exposure and organizational membership of agricultural producers, 1994 . . . 92

Political preferences 93
Figure 22 Reported attitudes and land reform: precise zeros across cohort-exposure windows 93
Table 19 Land-reform exposure and left voting in municipal elections, 1980–1989 . 94
Economic vis-à-vis political changes 95
Figure 23 Distance to reform-formalized communities and the 1981 economic outcomes 95
Figure 24 Economic and political outcomes by cohort exposure window 96
Table 20 Short-run economic effects of the land reform: the 1981 census 97
Table 21 Land reconcentration 1994–2012 and technification: the 2012 agricultural census 98
Table 22 Poverty and expenditure inequality four decades in: the 2007 district poverty map 99
Mediation Analysis 100
Figure 25 The mechanism decomposition in one diagram 100
Figure 26 Kwon–Roth full-mediation tests: lower bounds and verdicts, by mechanism and jointly 101
Table 23 Kwon–Roth tests of full mediation, by mechanism and jointly 102

Figures and Tables

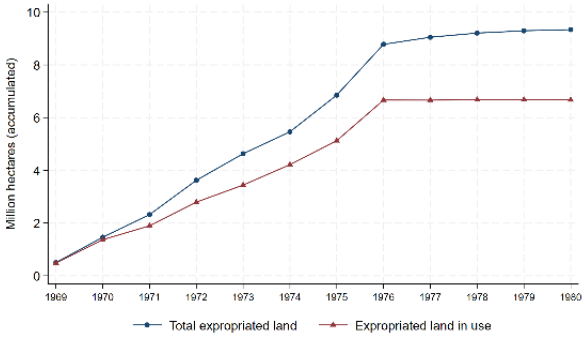
Historical background

Figure 1: Timeline: political regimes, party systems, and municipal elections in Peru

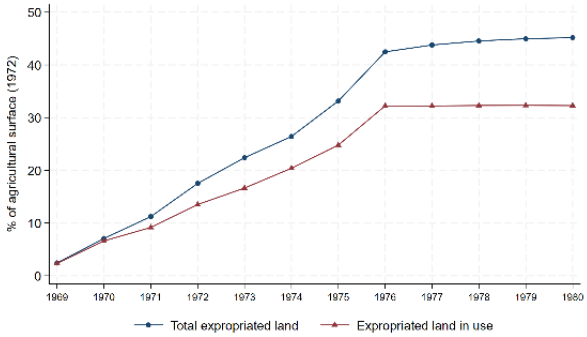
Notes: Blue blocks denote periods of civilian rule, dark gray blocks military rule (the 1962–1963 junta and the 1968–1980 military government), and the hatched block the autocratic phase of the Fujimori presidency. Green vertical lines mark the fifteen municipal elections analyzed. The lower band summarizes party-system eras; the traditional national parties (AP, APRA, PPC, IU) jointly received 74–82% of municipal valid votes in 1980–1986, 40% in 1989, 23% in 1993 and 4% in 1995.

Figure 2: Evolution of Land Reform Expropriations 1969–1980

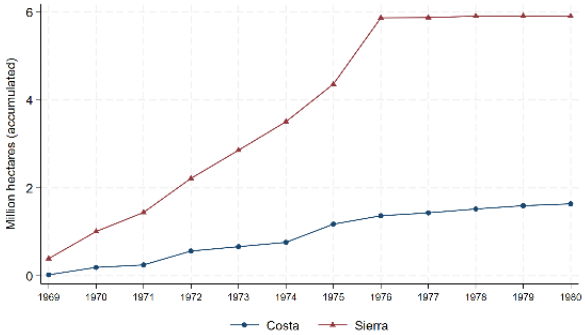
(a) Million hectares (accumulated)



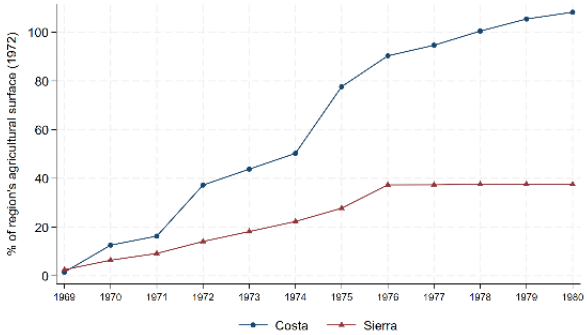
(b) % of agricultural surface (1972)



(c) By natural region (million has)



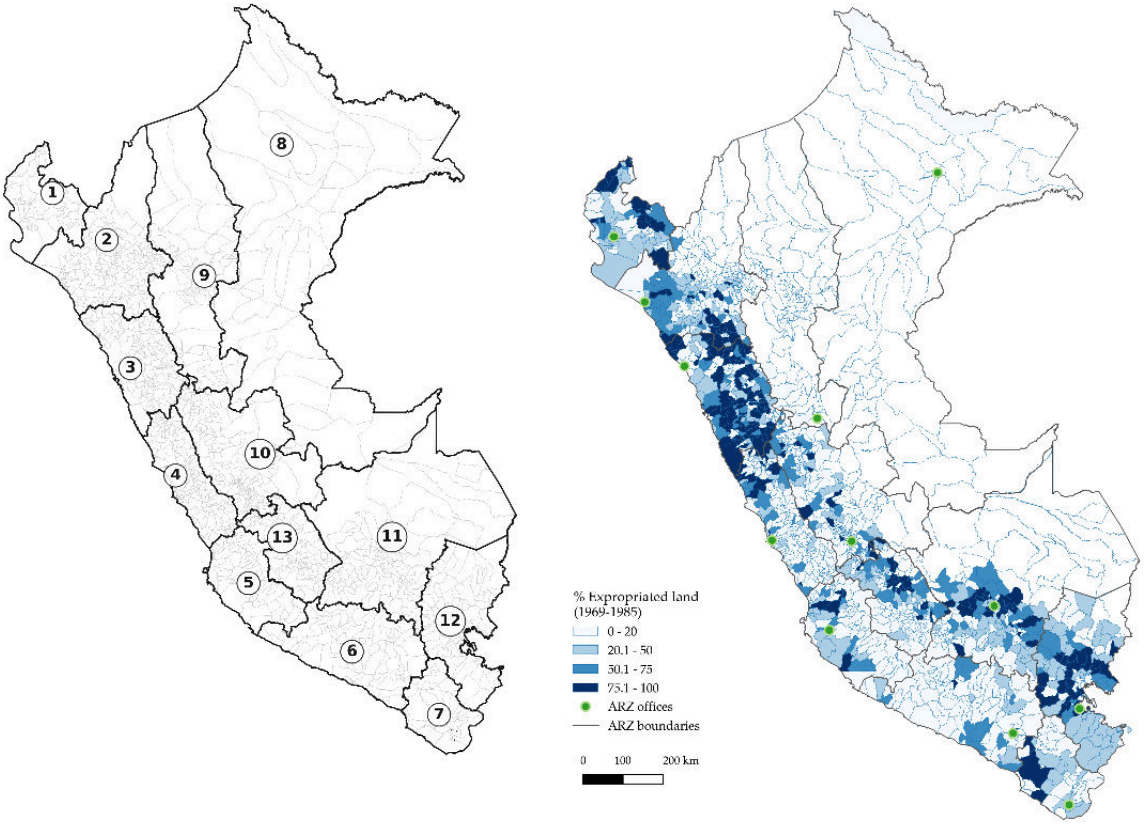
(d) By natural region (% of ag. surface 1972)



Notes: Accumulated expropriated hectares under the 1969–1980 land reform, for the 1,570 districts in the analysis sample. Panels (a) and (b) plot total expropriated land and expropriated land in active private use. Panels (c) and (d) plot total expropriated land for Costa and Sierra districts (natural regions assigned by largest area share); the Selva is excluded, as the 1969 Decree Law did not target private landholdings there. Agricultural surface from the 1972 Agricultural Census. Source: own elaboration based on the land reform expropriation decrees database.

Figure 3: Agrarian Reform Zones and District-level Land Reform Exposure

(a) Agrarian Reform Zones (b) Land reform exposure



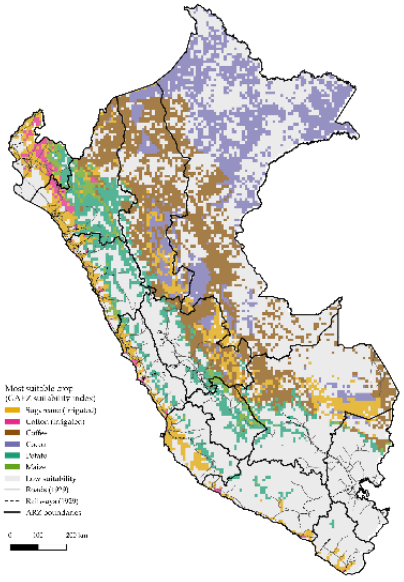
Notes: Panel (a) displays the boundaries of the 13 Agrarian Reform Zones, as mapped by Albertus (2020), with district contours in light gray (census-1972 geography). Panel (b) displays each district's expropriated land in active private use as a share of its 1972 agricultural surface (1969–1980) and the location of the ARZ regional offices for local execution. Selva districts are left unfilled, as the 1969 Decree Law did not target private landholdings in the Amazonia.

Figure 4: The Agrarian Reform Zones: Agro-ecological Conditions, Infrastructure, and Access to ARZ Offices

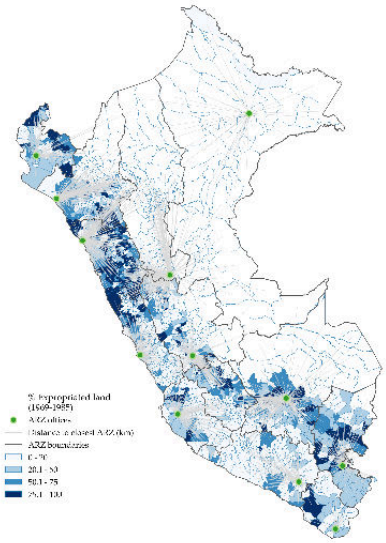
(a) Mantilla's synoptic map (1940)



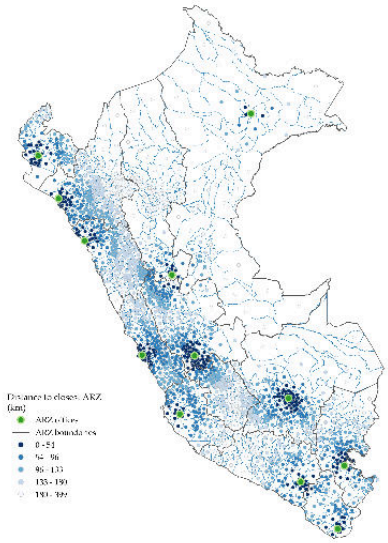
(b) Crop suitability and 1929 roads



(c) Exposure and distance to ARZ offices



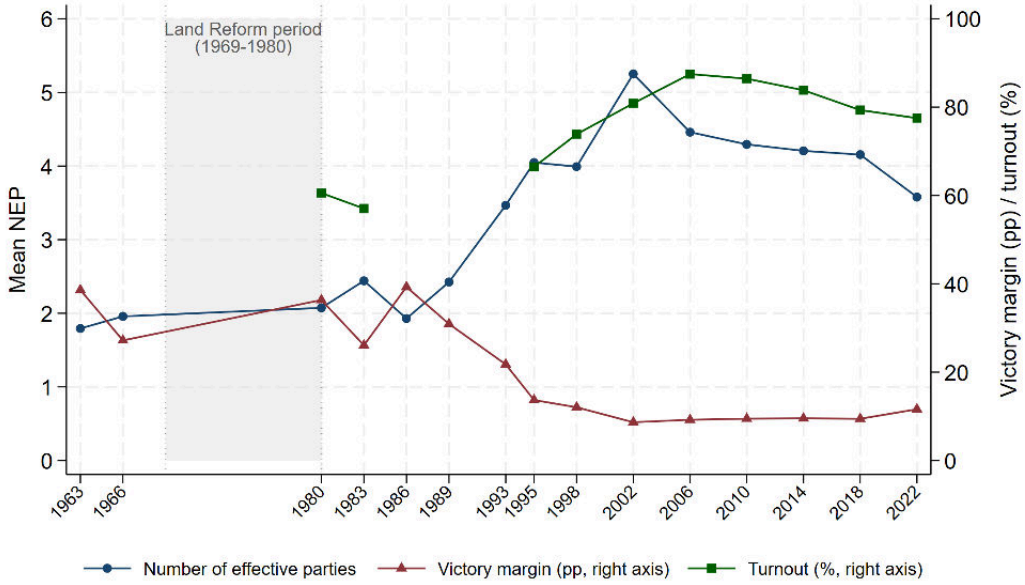
(d) Distance to the closest ARZ office



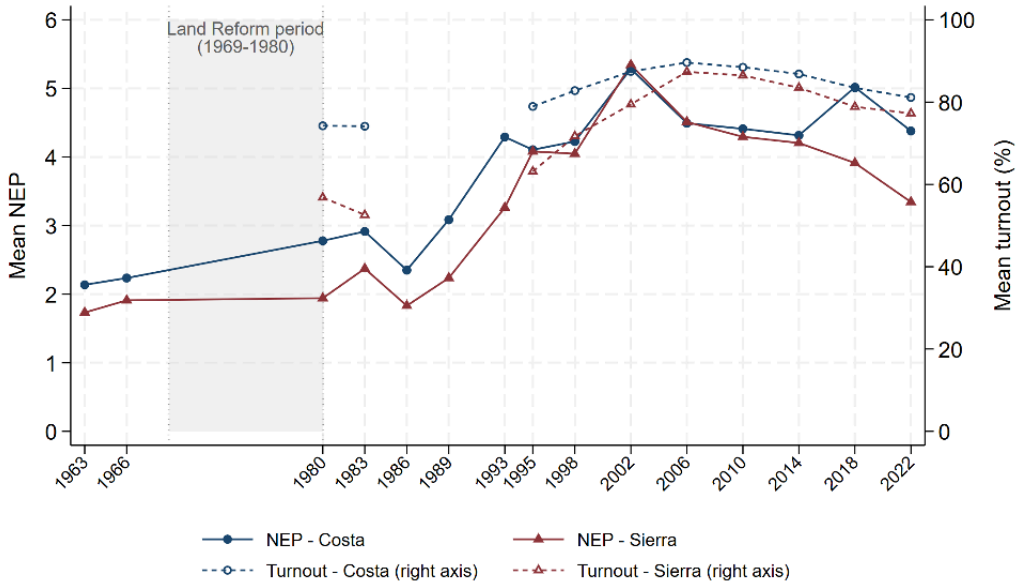
Notes: Panel (a) reproduces the Mapa Sinóptico del Perú by Belisario E. Mantilla (circa 1940), displaying natural resources, agricultural and ranching activity, from Albertus (2020)'s replication materials. Panel (b) displays, for each 5 arc-minute cell, the crop with the highest FAO GAEZ v4 agro-ecological suitability index among six emblematic crops (sugarcane and cotton under high-input irrigated conditions; coffee, cocoa, potato and maize under high-input rain-fed conditions), cells where no crop reaches 10% of the maximum suitability index in light gray, and the 1929 network of roads and railways. Panel (c) overlays on the land reform exposure map a line from each district centroid to the closest ARZ office. Panel (d) displays district centroids colored by their distance to the closest ARZ office, the basis of the instrument used in the IV strategy. ARZ boundaries as mapped by Albertus (2020). SIPA delineated these zones in 1960 based on "ecological conditions, social conditions, transportation routes, and access to markets" (SIPA, 1967).

Figure 5: Municipal Electoral Outcomes 1963–2022

(a) National trends: effective parties, victory margin and turnout

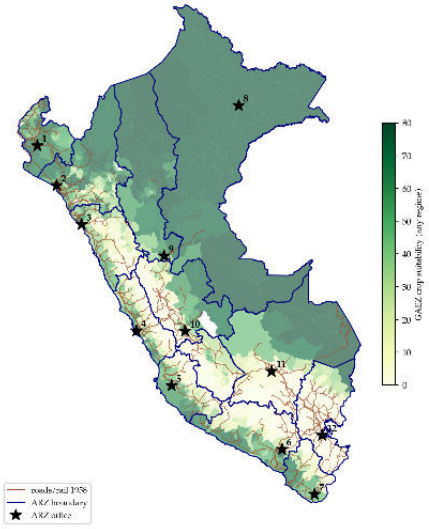


(b) Costa and Sierra: effective parties and turnout

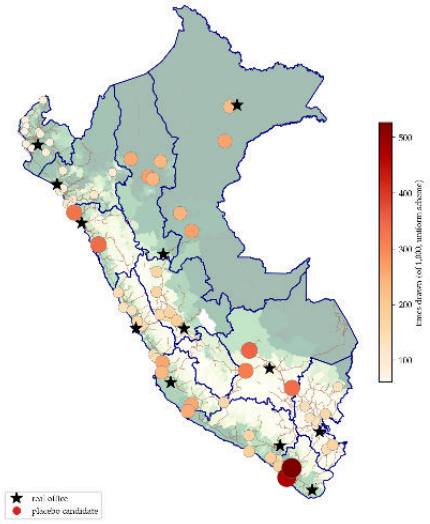


Notes: Panel (a) displays, for each municipal election between 1963 and 2022, the district-level mean of the number of effective parties (left axis) and, on the right axis, the victory margin (difference in vote shares between the top-two parties, in percentage points) and turnout. Panel (b) displays the mean number of effective parties (solid lines) and turnout (dashed lines, right axis) separately for Costa and Sierra districts. The shaded band marks the land reform period (1969–1980), during which no municipal elections were held. The turnout series is interrupted in the elections for which the district-level electoral roll is not available (1966, 1986, 1989 and 1993); the 1963 value is omitted (available for 30 districts only). Source: own compilation based on JNE/INFOGOB electoral records.

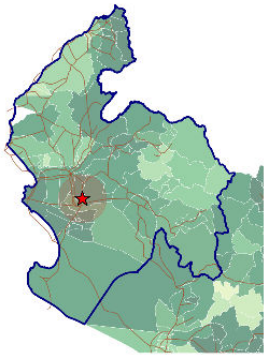
Figure 6: Randomization inference: inputs, counterfactual configurations, and construction example



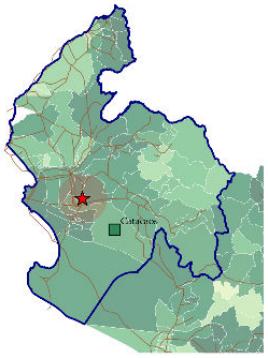
(a) Exercise inputs: crop suitability, 1958 roads, ARZ boundaries and offices



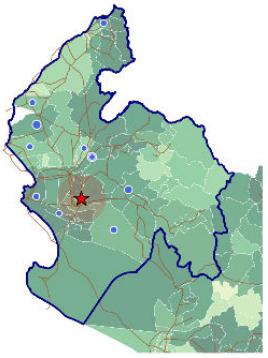
(b) Candidate placebo sites and draw frequency



(c) ARZ 1 (Piura): actual office



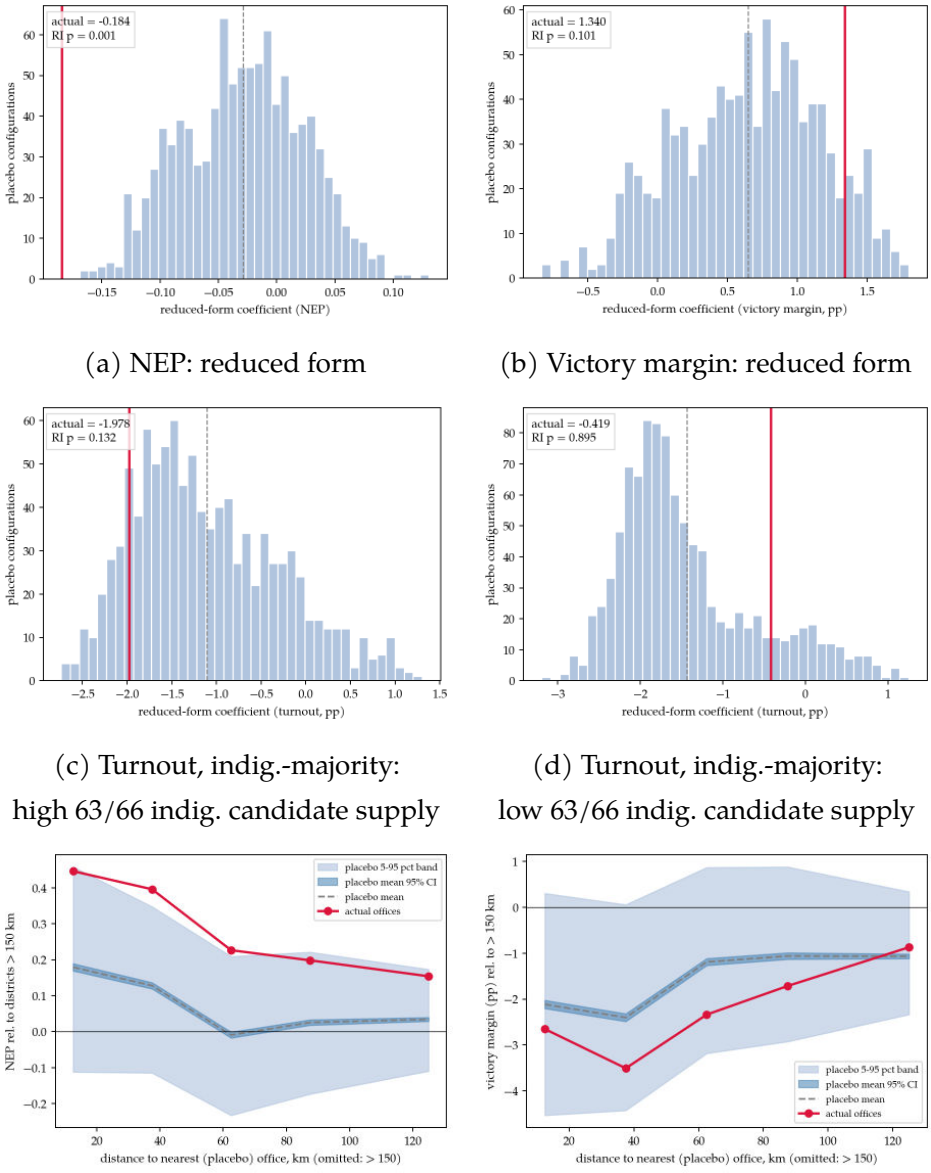
(d) One feasible counterfactual



(e) All feasible counterfactuals

Notes: Panel (a): inputs of the randomization-inference exercise — district crop suitability (GAEZ v4; green scale), the 1958 road and railway network (sienna), the 13 Agrarian Reform Zone boundaries (navy) and the 12 ARZ offices (stars; zone 13 had no office). Panel (b): the 68 candidate placebo sites, sized and colored by the number of times each is drawn across the 1,000 uniform-scheme configurations. Panels (c)–(e): construction of counterfactual offices for ARZ 1 (Piura) — the actual office with its 25 km anti-contamination buffer (red), one feasible counterfactual site (green square), and the full feasible set (blue circles, sized by 1961 population); gray crosses mark settlements failing the suitability, connectivity or buffer screens. Feasibility criteria and thresholds are described in Appendix B.

Figure 7: Randomization inference: placebo distributions and distance profiles



(a) NEP: reduced form

(b) Victory margin: reduced form

(c) Turnout, indig.-majority: high 63/66 indig. candidate supply

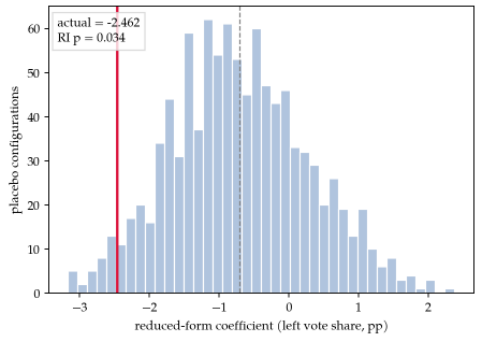
(d) Turnout, indig.-majority: low 63/66 indig. candidate supply

(e) NEP: distance profile

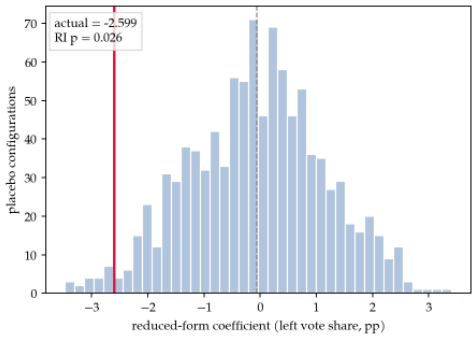
(f) Victory margin: distance profile

Notes: Panels (a)–(d): distribution of reduced-form coefficients across the 1,000 uniform-scheme placebo configurations (the column-(3) specification of Tables 5–7); the red line marks the coefficient of the actual offices, the dashed gray line the placebo mean, and the box reports the actual coefficient and the randomization-inference p-value (share of placebo configurations with an absolute coefficient at least as large). Panels (c)–(d) split the 1961 indigenous-majority districts by their *pre-reform supply of indigenous candidates* — the median share of 1963/66 municipal candidacies with an indigenous surname (Artiles et al. dictionary); the measure predates the reform and the direction was specified before estimation. Panels (e)–(f): reduced form on distance-to-office bins (omitted: districts more than 150 km away), with the same controls and fixed effects; the red line is the actual-office profile, the light band the 5–95 percentile range of the placebo profiles, and the darker band the 95% Monte Carlo confidence interval of the placebo mean. The turnout histograms use the ten elections with an available voter registry.

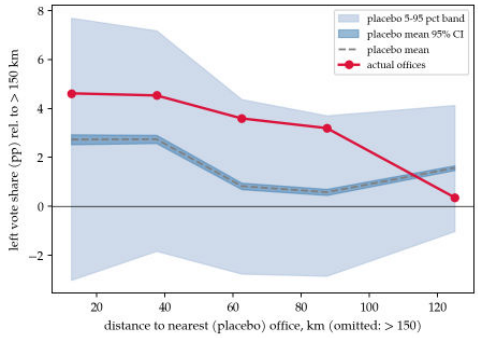
Figure 8: Randomization inference - broad-left vote share, 1980–89



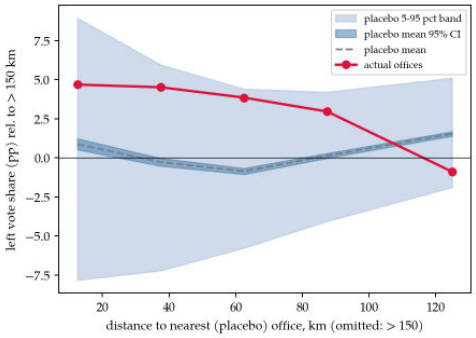
(a) Broad-left vote share: reduced form



(b) Broad left, 1961 indigenous-majority districts: reduced form



(c) Broad left: distance profile



(d) Broad left, indigenous-majority: distance profile

Notes: Reduced form of the broad-left vote share (Izquierda Unida and member parties, non-IU socialist parties and FRENATRACA, in pp) on the standardized distance to the nearest office, municipal elections 1980–89, against the 1,000 uniform-scheme placebo office configurations; same specification, controls and fixed effects as the previous figure. Panels (a)–(b): placebo distributions; the red line marks the actual-office coefficient, the dashed gray line the placebo mean. Unlike turnout (RI p = 0.54), the left gradient is office-specific: the full-sample coefficient survives the placebo test (panel (a)), and in 1961 indigenous-majority districts the actual coefficient sits in the far tail with essentially nothing explained by generic geography (panel (b): purged −2.5 of −2.6). These are the same districts where the boundary design finds the transitional turnout loss of the 1980s: fewer votes were cast there, and the votes that were cast shifted left where the reform hit — the liberated-vote composition margin, while the mobilization margin was depressed. Panels (c)–(d): reduced form on distance-to-office bins (omitted: districts more than 150 km away); the red line is the actual-office profile, the light shaded area the 5–95 percentile band of individual placebo profiles (the randomization acceptance band), the darker inner band the 95% Monte Carlo CI of the placebo mean.

Table 1: OLS: Land-reform exposure and the Number of Effective Parties (1980–2022)

	Number of Effective Parties (1980–2022)			
	(1)	(2)	(3)	(4)
Land reform exposure (1 SD)	0.105***	0.075***	0.079***	0.076***
Clustered S.E. (district)	(0.019)	(0.022)	(0.022)	(0.022)
Conley S.E. (100 Km.)	[0.026]***	[0.027]***	[0.027]***	[0.027]***
Conley S.E. (200 Km.)	[0.029]***	[0.026]***	[0.026]***	[0.027]***
Oster δ-bounds	63.58	12.67	4.96	4.74
Geographic controls	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes
Institutional controls	No	Yes	Yes	Yes
State capacity controls	No	No	Yes	Yes
Hacienda and ethnicity controls	No	No	Yes	Yes
Lasso-selected controls	No	No	No	Yes
Agrarian Reform Zone FE	No	Yes	Yes	Yes
Election FE	No	No	Yes	Yes
Observations	17,063	17,063	15,085	15,085
Clusters	1,401	1,401	1,235	1,235
Mean Dep. Var.	3.616	3.616	3.613	3.613
R-squared	0.045	0.067	0.437	0.435

Notes: District-year panel pooling all 13 post-reform municipal elections (1980–2022). The dependent variable is the effective number of parties in municipal district elections, elector-weighted across all district contests within each unit's 1975 boundaries (provincial capitals excluded). Land-reform exposure is standardized (1 SD = 21.1 pp of district area reformed). Controls are pre-reform, primarily from the 1961 Census: geography (elevation, slope, longitude and latitude), agriculture and labor force (agricultural labor share, GAEZ crop suitability, labor force), institutions (mita zone, prior social movements, communal uprisings, caudillo presence), state capacity (log state personnel, illiteracy), and haciendas and ethnicity (number of haciendas, indigenous mother-tongue share). Col (4): post-double-selection lasso over the non-geographic control groups (Belloni–Chernozhukov–Hansen 2014), with the geographic controls and the ARZ and election FE partialled out. Inference: standard errors clustered by district in parentheses; Conley spatial-HAC standard errors (Bartlett kernel, 100/200 km) in brackets; Oster δ: how many times stronger selection on unobservables, relative to the included controls, would be needed to drive the coefficient to zero (Oster 2019). *** p<0.01, ** p<0.05, * p<0.1.

Table 2: OLS: Land-reform exposure and Turnout (1980–2022)

	Turnout (1980–2022)			
	(1)	(2)	(3)	(4)
Land reform exposure (1 SD)	0.842***	0.468***	0.804***	0.791***
Clustered S.E. (district)	(0.132)	(0.128)	(0.135)	(0.134)
Conley S.E. (100 Km.)	[0.248]***	[0.169]***	[0.165]***	[0.167]***
Conley S.E. (200 Km.)	[0.307]***	[0.202]**	[0.192]***	[0.202]***
Oster δ-bounds	-12.59	-334.57	-18.14	-18.37
Geographic controls	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes
Institutional controls	No	Yes	Yes	Yes
State capacity controls	No	No	Yes	Yes
Hacienda and ethnicity controls	No	No	Yes	Yes
Lasso-selected controls	No	No	No	Yes
Agrarian Reform Zone FE	No	Yes	Yes	Yes
Election FE	No	No	Yes	Yes
Observations	13,509	13,509	11,922	11,922
Clusters	1,401	1,401	1,235	1,235
Mean Dep. Var.	75.588	75.588	75.275	75.275
R-squared	0.057	0.083	0.605	0.604

Notes: District-year panel of post-reform municipal elections. The dependent variable is turnout (votes cast as % of registered voters) in municipal district elections, elector-weighted across all district contests. The voter registry is unavailable for the 1986, 1989 and 1993 elections, so the panel covers the remaining ten post-reform elections. Control groups, fixed effects, the standardized exposure measure (1 SD = 21.1 pp of district area), the lasso protocol of col (4) (selection re-run for turnout) and the inference sub-rows (clustered SEs at the district level in parentheses; Conley spatial-HAC SEs with a Bartlett kernel at 100/200 km in brackets; Oster δ) are identical to the previous table. The δ in cols (3)–(4) is negative because adding the full control set makes the coefficient *larger*, not smaller (col (2) vs. col (3)): more-reformed districts have characteristics that depress turnout, so omitted factors resembling the controls would be hiding part of the effect rather than producing it. The extreme magnitude in col (2) ($\delta \approx -335$) arises because that column's controls leave the coefficient nearly unchanged. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: OLS: Land-reform exposure and the Victory Margin (1980–2022)

	Victory Margin (1980–2022)			
	(1)	(2)	(3)	(4)
Land reform exposure (1 SD)	-0.725***	-0.250	-0.385**	-0.384**
Clustered S.E. (district)	(0.159)	(0.174)	(0.184)	(0.180)
Conley S.E. (100 Km.)	[0.200]***	[0.184]	[0.190]**	[0.187]**
Conley S.E. (200 Km.)	[0.194]***	[0.160]	[0.175]**	[0.173]**
Oster δ-bounds	12.94	5.47	4.86	5.10
Geographic controls	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes
Institutional controls	No	Yes	Yes	Yes
State capacity controls	No	No	Yes	Yes
Hacienda and ethnicity controls	No	No	Yes	Yes
Lasso-selected controls	No	No	No	Yes
Agrarian Reform Zone FE	No	Yes	Yes	Yes
Election FE	No	No	Yes	Yes
Observations	17,063	17,063	15,085	15,085
Clusters	1,401	1,401	1,235	1,235
Mean Dep. Var.	17.609	17.609	17.690	17.690
R-squared	0.012	0.017	0.279	0.279

Notes: District-year panel pooling all 13 post-reform municipal elections (1980–2022). The dependent variable is the victory margin (winner minus runner-up vote share, in percentage points) in municipal district elections, elector-weighted across all district contests within each unit's 1975 boundaries (provincial capitals excluded). Control groups, fixed effects, the standardized exposure measure (1 SD = 21.1 pp of district area), the lasso protocol of col (4) (selection re-run for the margin) and the inference sub-rows (clustered SEs at the district level in parentheses; Conley spatial-HAC SEs with a Bartlett kernel at 100/200 km in brackets; Oster δ) are identical to Tables 1–2. *** p<0.01, ** p<0.05, * p<0.1.

Table 4: First stage: Distance to Agrarian Reform Zone offices and land-reform exposure (1980 cross-section)

	Land-reform exposure (share of district area)			
	(1)	(2)	(3)	(4)
Distance to nearest ARZ office (1 SD)	-0.059***	-0.041***	-0.043***	-0.043***
Robust S.E.	(0.007)	(0.007)	(0.008)	(0.008)
Conley S.E. (100 Km.)	[0.016]***	[0.012]***	[0.013]***	[0.013]***
Conley S.E. (200 Km.)	[0.019]***	[0.015]***	[0.015]***	[0.016]***
Excluded-instrument F (robust)	62.8	30.1	27.3	26.4
Geographic controls	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes
Institutional controls	No	Yes	Yes	Yes
State capacity controls	No	No	Yes	Yes
Hacienda and ethnicity controls	No	No	Yes	Yes
Lasso-selected controls	No	No	No	Yes
Agrarian Reform Zone FE	No	Yes	Yes	Yes
Observations (districts)	1,191	1,191	1,059	1,059
Mean Dep. Var.	0.139	0.139	0.145	0.145
R-squared	0.105	0.270	0.318	0.312

Notes: Cross-sectional first stage on the 1980 estimation sample. The dependent variable is land-reform exposure (share of district area adjudicated). The instrument is the distance from the district to the nearest Agrarian Reform Zone office (standardized). Heteroskedasticity-robust standard errors in parentheses. Conley spatial-HAC standard errors (Bartlett kernel, 100/200 km) in brackets. The F row reports the robust F on the excluded instrument. Control groups and the col (4) lasso protocol (selection re-run for this equation, Agrarian Reform Zone FE partialled out) are as in Table 1, with ARZ FE only. Results are insensitive to widening the sample to the full universe of districts with non-missing instrument and controls (1,506 districts): coefficients range from -0.043 to -0.052 with F between 40 and 60. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: IV: Land-reform exposure and the Number of Effective Parties (1980–2022)

	Number of Effective Parties (1980–2022)			
	(1)	(2)	(3)	(4)
Panel A. Reduced form				
Distance to nearest ARZ office (1 SD)	-0.199***	-0.137***	-0.184***	-0.172***
	(0.021)	(0.024)	(0.026)	(0.025)
Panel B. Second stage (2SLS)				
Land reform exposure (1 SD)	0.712***	0.720***	0.939***	0.809***
Clustered S.E. (district)	(0.107)	(0.171)	(0.205)	(0.174)
Anderson–Rubin 95% CI	[0.529, 0.966]	[0.438, 1.167]	[0.616, 1.509]	[0.529, 1.271]
Anderson–Rubin p-value	<0.001	<0.001	<0.001	<0.001
VtF 95% CI	[0.494, 0.929]	[0.332, 1.107]	[0.463, 1.415]	[0.419, 1.200]
Panel C. First stage				
Distance to nearest ARZ office (1 SD)	-0.279***	-0.190***	-0.196***	-0.212***
	(0.032)	(0.032)	(0.035)	(0.035)
Effective first-stage F	76.4	36.0	31.6	37.4
Geographic controls	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes
Institutional controls	No	Yes	Yes	Yes
State capacity controls	No	No	Yes	Yes
Hacienda and ethnicity controls	No	No	Yes	Yes
Lasso-selected controls (IV-PDS)	No	No	No	Yes
Agrarian Reform Zone FE	No	Yes	Yes	Yes
Election FE	No	No	Yes	Yes
Observations	17,063	17,063	15,085	15,085
Clusters	1,401	1,401	1,235	1,235
Mean Dep. Var.	3.616	3.616	3.613	3.613

Notes: District-year panel; outcome, sample, column structure, control groups and fixed effects as in Table 1. Panel B reports 2SLS estimates of standardized land-reform exposure instrumented with the standardized distance to the nearest Agrarian Reform Zone office; Panels A and C report the reduced form and the first stage estimated on each column's 2SLS sample. Standard errors clustered at the district level in parentheses throughout. Anderson–Rubin rows: 95% confidence set and p-value (for the null of no effect) obtained by inverting the cluster-robust AR test over a grid of null values — the weak-instrument-robust procedure recommended for just-identified models, where AR, CLR and LM coincide (Andrews, Stock and Sun 2019). VtF row: 95% confidence interval $\hat{\beta} \pm c(\hat{F})\times$SE, with the critical value $c(\hat{F})$ from Lee, McCrary, Moreira and Porter (2022) and $\hat{F}$ the cluster-robust first-stage F of Panel C (equal to the Kleibergen–Paap Wald F in the just-identified case). Col (4): IV post-double-selection lasso, same protocol as the OLS tables: selection over the four non-geographic control groups with the geographic controls and the FE partialled out and cluster-robust penalty loadings; the post-selection 2SLS re-includes geo and FE on the selection's sample. *** p<0.01, ** p<0.05, * p<0.1.

Table 6: IV: Land-reform exposure and Turnout (1980–2022)

	Turnout (1980–2022)			
	(1)	(2)	(3)	(4)
Panel A. Reduced form				
Distance to nearest ARZ office (1 SD)	-0.966***	-1.194***	-1.257***	-1.281***
	(0.184)	(0.223)	(0.218)	(0.219)
Panel B. Second stage (2SLS)				
Land reform exposure (1 SD)	3.494***	6.318***	6.456***	6.357***
Clustered S.E. (district)	(0.725)	(1.487)	(1.425)	(1.378)
Anderson–Rubin 95% CI	[2.168, 5.103]	[3.817, 10.133]	[4.099, 10.247]	[4.065, 9.979]
Anderson–Rubin p-value	<0.001	<0.001	<0.001	<0.001
VtF 95% CI	[2.025, 4.964]	[2.950, 9.687]	[3.138, 9.774]	[3.180, 9.533]
Panel C. First stage				
Distance to nearest ARZ office (1 SD)	-0.276***	-0.189***	-0.195***	-0.201***
	(0.031)	(0.031)	(0.035)	(0.035)
Effective first-stage F	77.4	36.1	31.5	33.1
Geographic controls	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes
Institutional controls	No	Yes	Yes	Yes
State capacity controls	No	No	Yes	Yes
Hacienda and ethnicity controls	No	No	Yes	Yes
Lasso-selected controls (IV-PDS)	No	No	No	Yes
Agrarian Reform Zone FE	No	Yes	Yes	Yes
Election FE	No	No	Yes	Yes
Observations	13,509	13,509	11,922	11,922
Clusters	1,401	1,401	1,235	1,235
Mean Dep. Var.	75.588	75.588	75.275	75.275

Notes: Same design, estimators and inference as the previous table for turnout (votes cast as % of registered voters; the padrón is unavailable in 1986, 1989 and 1993, so the panel covers ten post-reform elections). *** p<0.01, ** p<0.05, * p<0.1.

Table 7: IV: Land-reform exposure and the Victory Margin (1980–2022)

	Victory Margin (1980–2022)			
	(1)	(2)	(3)	(4)
Panel A. Reduced form				
Distance to nearest ARZ office (1 SD)	1.391***	0.905***	1.342***	1.286***
	(0.200)	(0.234)	(0.252)	(0.248)
Panel B. Second stage (2SLS)				
Land reform exposure (1 SD)	-4.979***	-4.770***	-6.849***	-6.062***
Clustered S.E. (district)	(0.861)	(1.435)	(1.675)	(1.462)
Anderson–Rubin 95% CI	[-6.953, -3.454]	[-8.328, -2.271]	[-11.310, -4.081]	[-9.783, -3.589]
Anderson–Rubin p-value	<0.001	<0.001	<0.001	<0.001
VtF 95% CI	[-6.726, -3.231]	[-8.025, -1.515]	[-10.748, -2.949]	[-9.352, -2.772]
Panel C. First stage				
Distance to nearest ARZ office (1 SD)	-0.279***	-0.190***	-0.196***	-0.212***
	(0.032)	(0.032)	(0.035)	(0.035)
Effective first-stage F	76.4	36.0	31.6	37.4
Geographic controls	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes
Institutional controls	No	Yes	Yes	Yes
State capacity controls	No	No	Yes	Yes
Hacienda and ethnicity controls	No	No	Yes	Yes
Lasso-selected controls (IV-PDS)	No	No	No	Yes
Agrarian Reform Zone FE	No	Yes	Yes	Yes
Election FE	No	No	Yes	Yes
Observations	17,063	17,063	15,085	15,085
Clusters	1,401	1,401	1,235	1,235
Mean Dep. Var.	17.609	17.609	17.690	17.690

Notes: Same design, estimators and inference as Table 5 for the victory margin (winner minus runner-up vote share, in percentage points; all 13 post-reform elections). *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Recentered IV (Borusyak–Hull): expected office distance from counterfactual configurations

	NEP	Victory margin	Turnout
	(1)	(2)	(3)
2SLS, actual distance (baseline)	0.934***	-6.800***	6.459***
	(0.204)	(1.670)	(1.424)
First-stage F	31.5	31.5	31.5
2SLS, controlling expected distance μ_i	1.070***	-6.857***	5.075***
	(0.256)	(1.952)	(1.468)
First-stage F	25.1	25.1	24.9
Expected distance μ_i (reduced form)	-0.052	2.160***	-4.761***
	(0.059)	(0.541)	(0.402)
2SLS, recentered instrument $z_i - \mu_i$	1.556***	-7.062**	0.155
	(0.555)	(3.204)	(2.448)
First-stage F	9.2	9.2	8.9
2SLS, μ_i from population-weighted draws	1.120***	-6.070***	3.332**
	(0.311)	(2.129)	(1.431)
First-stage F	18.2	18.2	18.1
Full col. (3) controls	Yes	Yes	Yes
Agrarian Reform Zone FE	Yes	Yes	Yes
Election FE	Yes	Yes	Yes
Observations	15,085	15,085	11,922
BH specification test, permutation p		0.211	

Notes: For each district, the expected instrument μ_i is the average distance to the nearest office across the 1,000 counterfactual office configurations of the randomization-inference exercise (Borusyak and Hull, 2023); the recentered instrument is $z_i - \mu_i$. All distances euclidean, in cross-district SD units of the actual distance; μ_i from the uniform draw scheme unless noted. Column-(3) controls, Agrarian Reform Zone and election FE, SEs clustered by district; first-stage F of the excluded instrument below each estimate. The recentered-instrument row differs from the μ_i-control row because $z_i - \mu_i$ and μ_i are correlated in sample (corr $= 0.36$). The specification test regresses the recentered instrument on the full control set and ARZ FE at the district level and compares its R^2 against the 1,000 counterfactual configurations (permutation p-value). *** p<0.01, ** p<0.05, * p<0.1.

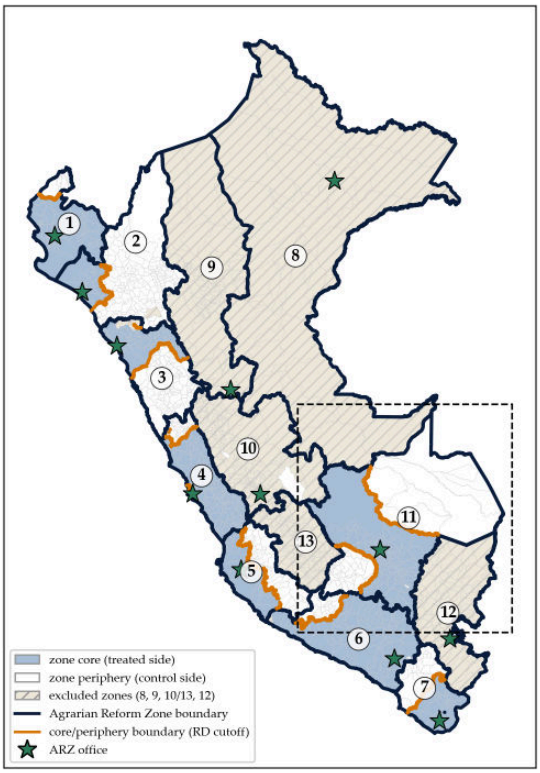
Table 9: Decomposition of the IV–OLS gap (Ishimaru 2024)

	NEP	Victory margin	Turnout
	(1)	(2)	(3)
Panel A. Decomposition of the IV–OLS gap			
OLS coefficient	0.079***	-0.385**	0.804***
	(0.022)	(0.184)	(0.135)
IV (2SLS) coefficient	0.939***	-6.849***	6.456***
	(0.205)	(1.675)	(1.425)
IV–OLS gap	0.860***	-6.464***	5.652***
	(0.205)	(1.675)	(1.437)
(i) Covariate-weight difference	0.004	-0.126	0.286
	(0.046)	(0.395)	(0.367)
(ii) Treatment-level-weight difference	-0.015	0.173	-0.050
	(0.013)	(0.140)	(0.066)
(iii) Marginal-effect difference	0.871***	-6.511***	5.416***
	(0.203)	(1.668)	(1.388)
Panel B. OLS and IV weights on covariate groups			
	Share	OLS weight	IV weight
Sierra (elev. $\geq$ 2,000 m)	0.713	0.725	0.816
		(0.031)	(0.089)
Non-sierra	0.287	0.275	0.184
		(0.031)	(0.089)
Indigenous majority, 1961	0.509	0.576	0.793
		(0.034)	(0.095)
Non-indigenous majority	0.491	0.424	0.207
		(0.034)	(0.095)
Inside mita zone	0.335	0.362	0.426
		(0.034)	(0.083)
Outside mita zone	0.665	0.638	0.574
		(0.034)	(0.083)
Above-median N of haciendas	0.517	0.756	0.718
		(0.024)	(0.073)
Below-median	0.483	0.244	0.282
		(0.024)	(0.073)
Full col. (3) controls	Yes	Yes	Yes
Agrarian Reform Zone FE	Yes	Yes	Yes
Election FE	Yes	Yes	Yes
Observations	15,085	15,085	11,922

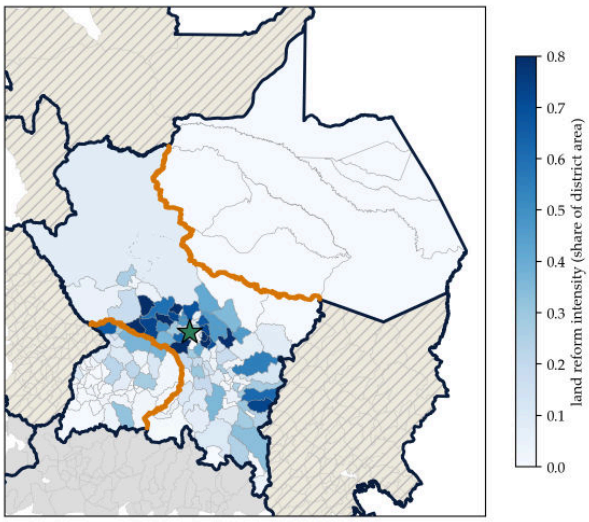
Notes: Decomposition of the gap between the OLS and 2SLS coefficients of Tables 1–3 and 5–7, col. (3), following Ishimaru (2024): the gap is split into (i) the difference in the weights that IV and OLS place on the covariates (observed heterogeneity), (ii) the difference in their weights across treatment levels (nonlinearity), and (iii) the difference between the IV- and OLS-identified marginal effects. Component (iii) bundles endogeneity bias, unobserved effect heterogeneity (LATE vs. ATE) and measurement error in the treatment, which are not separable (Ishimaru 2024). Auxiliary regressions use a cubic basis in the treatment and the linear control set as covariate basis; column-(3) controls with Agrarian Reform Zone and election FE; SEs clustered by district (delta method for the components). Panel B reports the sample share of four binary partitions derived from the control set (sierra, indigenous-majority in 1961, mita catchment, above-median number of haciendas in 1961) next to the weights that the OLS and IV estimands place on each group (Ishimaru 2024); weights sum to one within each pair and depend only on the treatment, instrument and controls, so a single computation serves all columns. *** p<0.01, ** p<0.05, * p<0.1.

Figure 9: Agrarian Reform Zones: cores, peripheries, and reform intensity at the core/periphery boundary

(a) Agrarian Reform Zones, cores and offices

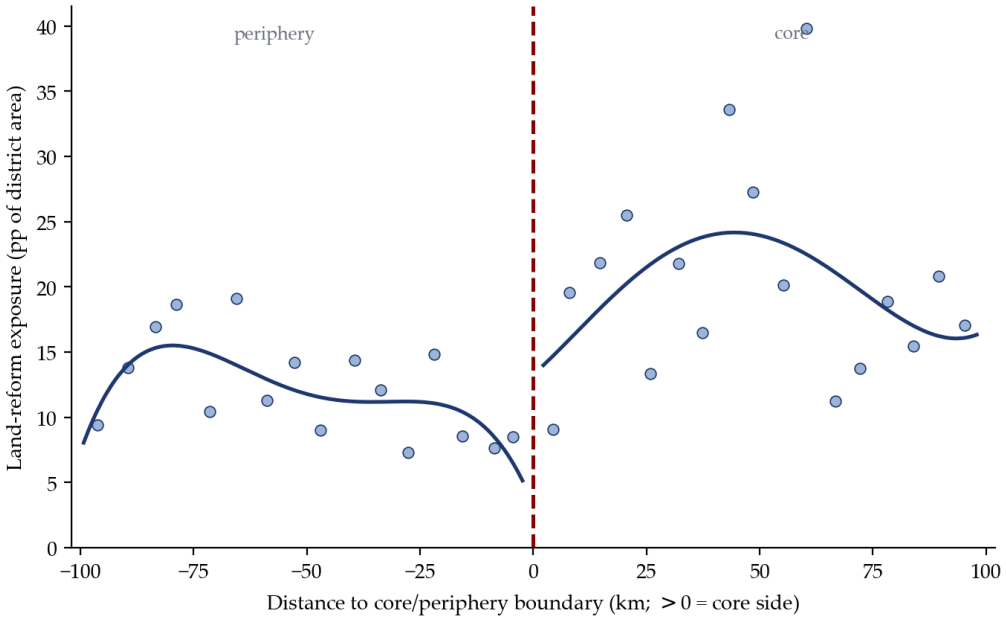


(b) Zoom: ARZ 11, reform intensity at the boundary



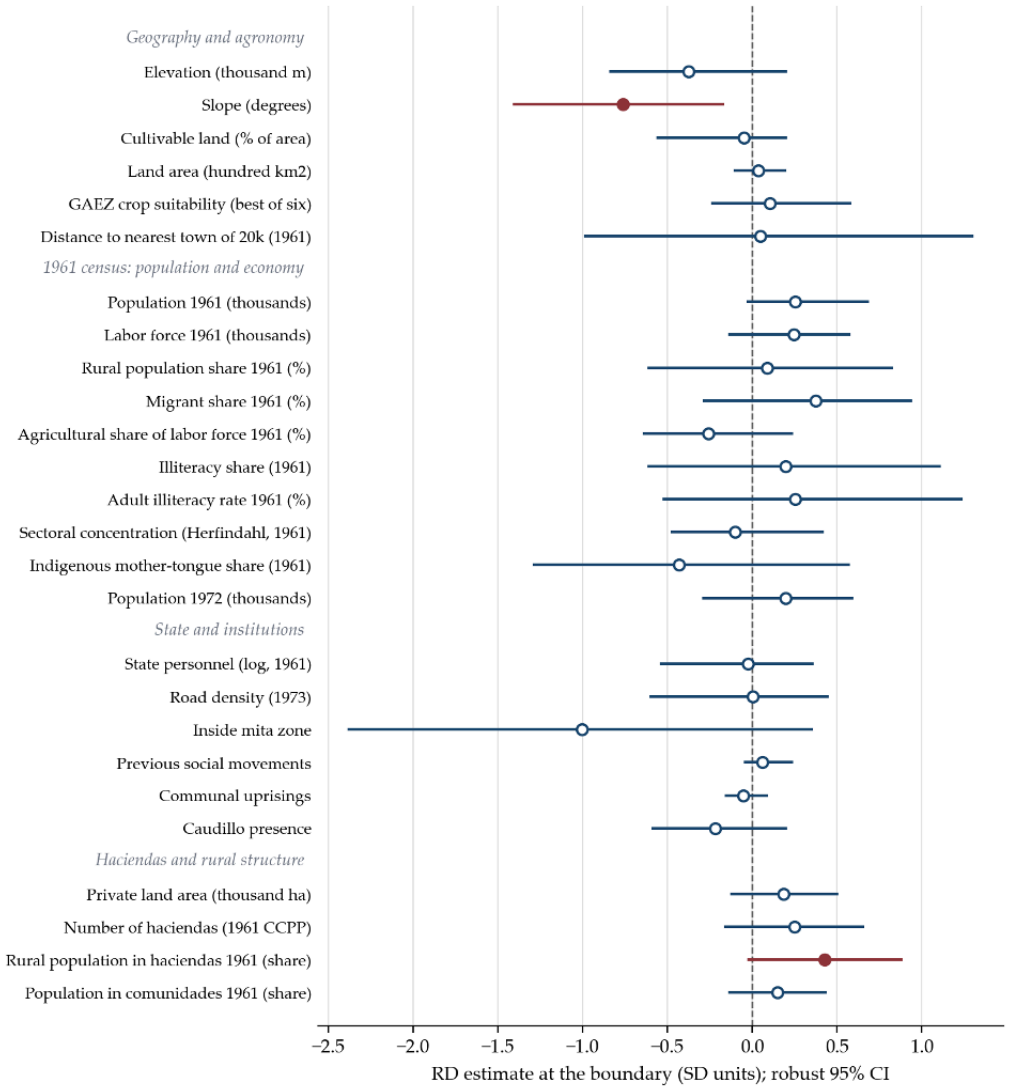
Notes: Panel (a): the eleven Agrarian Reform Zones (navy outlines, numbered) over the 1975 district map. Within the zones eligible for the design (1–7 and 11), gray districts form the zone core, the departments hosting the zonal office (blue stars), and white districts the periphery. The magenta line is the core/periphery interface, the RD cutoff (98% of its 2,528 km runs along 1975 departmental boundaries). Hatched beige zones are excluded from the design, mirroring the agro-zone falsification of the IV: the Amazonian zones 8–9, zone 10/13, and Puno's zone 12. The dashed rectangle marks the panel (b) window. Panel (b): zoom to ARZ 11, the eligible zone with the most districts within 50 km of the cutoff. Districts of the zone are colored by land-reform intensity (share of district area adjudicated, 1969–80). Neighboring eligible zones are flat gray and excluded zones keep the beige hatch. Intensity concentrates on the core side of the cutoff (crude means within 25 km: 0.19 core vs. 0.12 periphery), the visual first stage. The design sample excludes urban districts (1972 population of 75,000 or more).

Figure 10: RDD first stage: land-reform dose at the core/periphery boundary



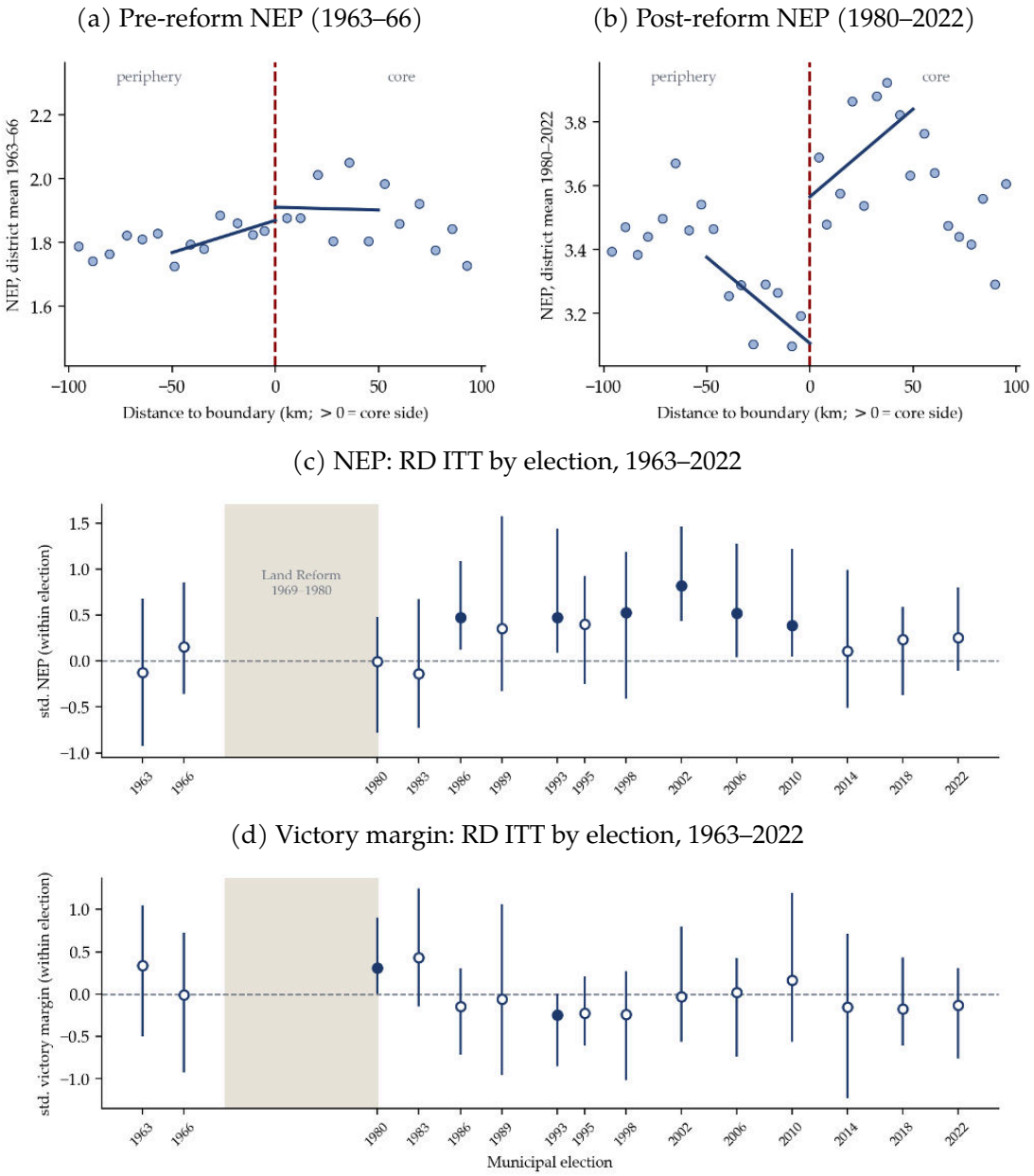
Notes: RD plot of land-reform exposure (expropriated area in use as pp of district area, the paper's headline measure) against the signed distance to the core/periphery boundary, on the design sample within 100 km of the cutoff (dashed line). Dots are evenly-spaced mimicking-variance bins (33 bins). Solid lines are fourth-order polynomial fits estimated separately on each side over the plotted window. The level shift at the cutoff, with the periphery approaching the boundary at roughly 5–9pp and the core starting at roughly 14pp, is the visual counterpart of the local-linear first-stage estimates of Table B7 (+8–9pp with boundary-segment covariates).

Figure 11: RDD balance at the core/periphery boundary: standardized estimates for 26 predetermined characteristics



Notes: Each marker is the conventional local-linear RD estimate of a predetermined district characteristic at the core/periphery boundary, divided by the characteristic's standard deviation in the design sample. Whiskers are robust bias-corrected 95% confidence intervals on the same scale. Specifications: no covariates, MSE-optimal bandwidths selected separately on each side and per covariate, nearest-neighbor clustering by department. Filled dark-red markers flag conventional $p < 0.05$ in two of the 26 characteristics. The first is slope, the imbalance reported by Albertus (2020), absorbed by the boundary-segment fixed effects and geographic controls of the covariate-adjusted specifications. The second is the 1961 share of rural population living in haciendas: the core side had denser pre-reform hacienda settlement, part of how the boundary generates the dose discontinuity. All remaining 24 intervals cover zero.

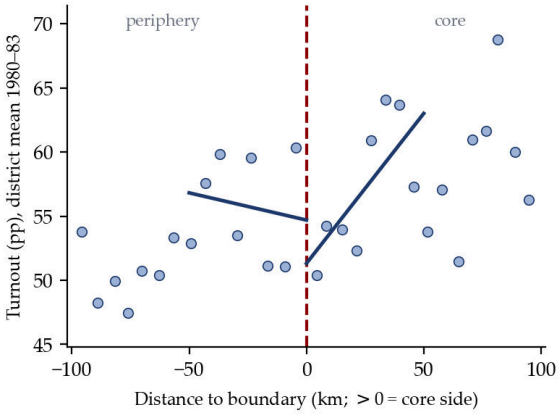
Figure 12: NEP and victory margin at the core/periphery boundary, 1963–2022



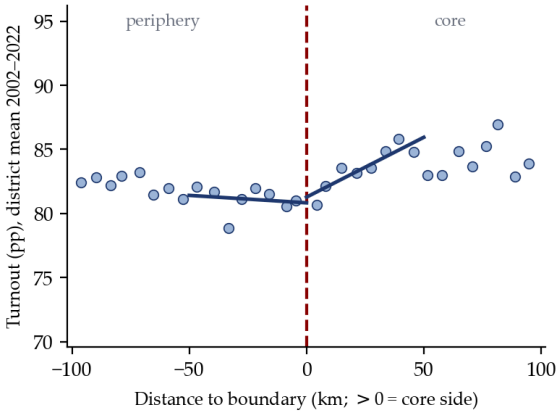
Notes: Panels (a)–(b): RD plots of the district mean number of effective parties against the signed distance to the core/periphery boundary, on the design sample without provincial capitals (807 and 891 districts). Dots are evenly-spaced mimicking-variance bins over the ±100 km window. Solid lines are linear fits estimated separately on each side within 50 km of the cutoff, the bandwidth of the fixed-bandwidth specifications, with no covariates and outcomes in native units (parties). The vertical axes of the two panels use the same span so that the discontinuities are visually comparable. Panel (a) averages the two pre-reform elections (1963 and 1966). The local-linear placebo estimates at the cutoff are +0.03 to +0.06 parties on a periphery base of 1.8 and statistically indistinguishable from zero. Panel (b) averages the thirteen post-reform elections (1980–2022). The corresponding local-linear estimates are +0.42 to +0.65 parties on a periphery mean of 3.2, roughly 13 to 20%, significant and stable across bandwidths and covariate sets. Panels (c)–(d): RD ITT estimates by election for the number of effective parties and the victory margin, standardized within election on the design sample. Every regression uses a common fixed bandwidth of 50 km, the boundary-segment covariates and nearest-neighbor clustering by department. The common bandwidth holds the comparison window constant across elections, since per-election MSE-optimal selection would confound temporal variation with bandwidth variation. Hollow markers are conventional estimates and filled markers have conventional $p < 0.05$. Whiskers are robust bias-corrected 95% confidence intervals. The shaded band marks the Land Reform (1969–1980).

Figure 13: Turnout at the core/periphery boundary, 1980–2022

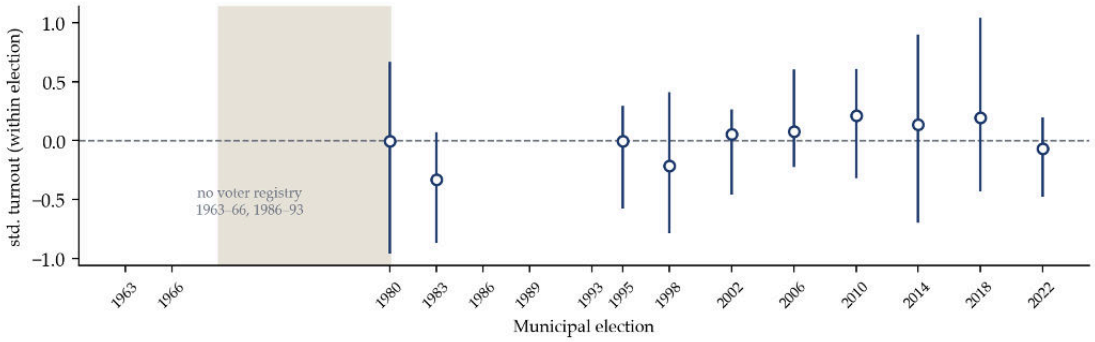
(a) Transition: turnout 1980–83



(b) Later elections: turnout 2002–2022



(c) Turnout: RD ITT by election, 1980–2022



Notes: Panels (a)–(b): RD plots of the district mean turnout (votes cast as % of registered voters) against the signed distance to the core/periphery boundary, on the design sample without provincial capitals (821 and 891 districts). Dots are evenly-spaced mimicking-variance bins over the ±100 km window. Solid lines are linear fits estimated separately on each side within 50 km of the cutoff, with no covariates and outcomes in percentage points. The vertical axes of the two panels use the same span so that the discontinuities are visually comparable. Panel (a) averages the two transition elections (1980 and 1983), where the local-linear estimates are negative and sizable (−9.3pp at MSE bandwidths and −4.2pp at 50 km, $p \leq 0.008$). Panel (b) averages the 2002–2022 elections, where the boundary effect is a precise null. Pooling all ten elections with a voter registry yields a net negative that is entirely driven by the early decades. The early drop is behavioral rather than mechanical: the voter registry itself is smooth at the boundary in 1980/83 (RD on log registered voters of +0.20 to +0.25, $p > 0.24$) and so is its subsequent growth. Panel (c): RD ITT estimates by election, standardized within election on the design sample, with the same specification as the trajectory panels of the previous figure (common 50 km bandwidth, boundary-segment covariates, nearest-neighbor clustering by department). Turnout has no pre-reform counterpart: the municipal voter registry starts in 1980 and is also unavailable for 1986, 1989 and 1993. Hollow markers are conventional estimates and filled markers have conventional $p < 0.05$. Whiskers are robust bias-corrected 95% confidence intervals. The shaded band marks the Land Reform (1969–1980).

Table 10: Smoothness of predetermined characteristics at the core/periphery boundary

	Periphery mean	RD estimate	Conv. SE	Robust p-value	Bandwidth l / r (km)	Eff. N l / r
Geography and agronomy						
Elevation (thousand m)	2.95	-0.530	(0.348)	0.23	61 / 48	253 / 196
Slope (degrees)	8.16	-2.758***	(0.926)	0.01	25 / 42	121 / 168
Cultivable land (% of area)	4.78	-0.484	(1.681)	0.35	39 / 39	179 / 158
Land area (hundred km^2)	3.97	0.588	(1.056)	0.55	36 / 37	162 / 149
GAEZ crop suitability (best of six)	19.98	2.479	(5.127)	0.41	65 / 56	259 / 215
Distance to nearest town of 20k (1961)	47.71	1.526	(15.917)	0.79	87 / 55	324 / 214
1961 census: population and economy						
Population 1961 (thousands)	4.24	1.745	(1.178)	0.07	55 / 62	203 / 214
Labor force 1961 (thousands)	1.15	0.495	(0.312)	0.23	63 / 48	258 / 184
Rural population share 1961 (%)	70.77	2.218	(8.430)	0.77	41 / 52	186 / 206
Migrant share 1961 (%)	9.82	5.981	(4.125)	0.29	37 / 76	166 / 269
Agricultural share of labor force 1961 (%)	79.66	-5.301	(3.957)	0.36	31 / 48	144 / 193
Illiteracy share (1961)	0.51	0.043	(0.092)	0.57	44 / 63	195 / 234
Adult illiteracy rate 1961 (%)	50.78	5.541	(9.123)	0.43	44 / 57	195 / 216
Sectoral concentration (Herfindahl, 1961)	0.67	-0.021	(0.039)	0.90	32 / 34	147 / 136
Indigenous mother-tongue share (1961)	0.57	-0.181	(0.179)	0.45	66 / 59	265 / 225
Population 1972 (thousands)	4.49	1.710	(1.639)	0.50	64 / 50	259 / 198
State and institutions						
State personnel (log, 1961)	2.58	-0.036	(0.278)	0.69	60 / 64	253 / 235
Road density (1973)	20.10	0.259	(13.593)	0.77	36 / 41	162 / 166
Inside mita zone	0.27	-0.386*	(0.232)	0.14	107 / 69	369 / 248
Previous social movements	0.05	0.036	(0.036)	0.18	27 / 24	132 / 90
Communal uprisings	0.03	-0.016	(0.011)	0.56	34 / 37	161 / 147
Caudillo presence	0.03	-0.027	(0.020)	0.33	34 / 58	161 / 223
Haciendas and rural structure						
Private land area (thousand ha)	7.58	16.000	(13.023)	0.23	43 / 158	193 / 401
Number of haciendas (1961 CCPP)	3.48	2.836	(2.113)	0.23	46 / 53	183 / 191
Rural population in haciendas 1961 (share)	0.08	0.078**	(0.035)	0.06	34 / 59	157 / 225
Population in comunidades 1961 (share)	0.02	0.017	(0.019)	0.30	36 / 47	147 / 173

Notes: Each row is a separate local-linear RD of a predetermined district characteristic on the signed distance to the core/periphery boundary, with no covariates and MSE-optimal bandwidths selected separately on each side and per covariate, following the balance design of Albertus (2020), with nearest-neighbor clustering by department. Stars on the RD estimate use the conventional p-value. The Robust column reports the bias-corrected robust p-value. The periphery mean is computed within the left bandwidth. Of the 26 characteristics, covering geography and agronomy, the 1961 population census, state presence and institutions, and the hacienda structure, 24 are smooth at the boundary. The two exceptions are slope (-2.76, conventional $p = 0.003$), the same imbalance that Albertus (2020) reports and absorbs, which motivates the boundary-segment fixed effects and geographic controls of the covariate-adjusted specifications, and the 1961 share of rural population living in haciendas ($+0.078$ on a periphery mean of 0.08). The second imbalance is substantive rather than incidental. Offices were placed where there was more hacienda to expropriate, so it is part of how the boundary generates the dose discontinuity, while the count of haciendas and total private land area are smooth. Outcome specifications accordingly report robustness to hacienda controls. A McCrary-style density test does not reject smoothness of the running variable at the cutoff (robust $p = 0.15$), consistent with no sorting of districts across the boundary, as expected for administrative units fixed in 1975. Population sorting during implementation is also checked directly. While census levels echo the geography above (1972 population $+0.51$ log points, $p = 0.05$), the changes over the reform decade are clean zeros at the boundary (log population growth 1961–72 of -0.02 to $+0.01$, $p \geq 0.74$, and the change in illiteracy of -0.3 to $+0.3$pp, $p \geq 0.79$), so the reform did not differentially move people across the boundary while it was being implemented. Figure 11 plots the full set of estimates in standard-deviation units. *** p<0.01, ** p<0.05, * p<0.1.

Table 11: Pre-reform electoral competition (1963 and 1966) at the core/periphery boundary

	1963		1966		Pooled 1963–66	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: effective number of parties (NEP)						
RD estimate (conventional)	-0.043	-0.072	0.057	0.064	0.041	0.055
	(0.159)	(0.111)	(0.094)	(0.052)	(0.099)	(0.071)
Robust bias-corrected	-0.023	-0.095	0.033	0.047	0.033	0.035
	[0.186]	[0.139]	[0.104]	[0.070]	[0.112]	[0.097]
Mean dep. var. (periphery)	1.78	1.79	1.87	1.85	1.82	1.84
Bandwidth left / right (km)	63 / 60	26 / 43	79 / 56	52 / 49	71 / 67	45 / 47
Eff. N left / right	169 / 137	73 / 102	222 / 175	163 / 163	204 / 196	140 / 158
Panel B: victory margin (pp)						
RD estimate (conventional)	11.293	8.915	3.296	-0.731	2.951	-0.047
	(9.444)	(7.447)	(4.810)	(5.255)	(5.917)	(5.775)
Robust bias-corrected	10.509	10.750	3.112	-1.379	2.519	-0.518
	[11.022]	[9.535]	[5.795]	[6.811]	[7.369]	[7.432]
Mean dep. var. (periphery)	33.97	31.88	26.39	26.43	30.55	28.97
Bandwidth left / right (km)	65 / 39	24 / 39	66 / 67	49 / 40	66 / 41	46 / 42
Eff. N left / right	171 / 92	66 / 92	196 / 196	157 / 135	196 / 137	144 / 139
Segment covariates	No	Yes	No	Yes	No	Yes

Notes: Local-linear RD estimates of pre-reform electoral outcomes at the core/periphery boundary, the design's key validity asset. The boundary was drawn in 1969 for reform administration, so the municipal elections of 1963 and 1966 (literate franchise) predate it. Both outcomes follow the paper's aggregation rule (elector-weighted across the unit's district contests, provincial capitals excluded). Panel A is the effective number of parties and Panel B the victory margin (first- minus second-place vote share, in pp). Pre-reform turnout is unavailable. The voter registry survives for 28 districts in 1963 and none in 1966. Odd columns use no covariates and even columns add the boundary-segment covariates of the first stage, both with MSE-optimal two-sided bandwidths and nearest-neighbor clustering by department. Stars follow each row's own p-value. All twelve estimates are statistically indistinguishable from zero. The same holds for pre-reform preferences over the reform itself: the 1963/66 vote share of the reform-promising bloc (Acción Popular–DC against the APRA–UNO coalition, jointly 83% of the valid vote) is smooth at the boundary (+3.5pp, $p = 0.62$ at MSE bandwidths, on a periphery mean of 50%). *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

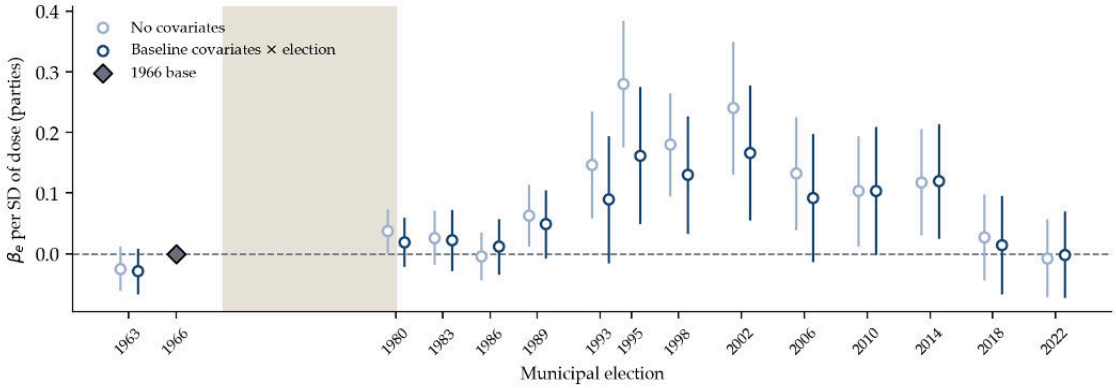
Table 12: The boundary effects by era: rise in the number of effective parties, competitive tightening, and transitional demobilization

	1980–89	1993–98	2002–10	2014–22
Panel A: effective number of parties				
RD ITT (MSE-two)	0.088	0.664***	1.032***	0.334*
	(0.107)	(0.228)	(0.230)	(0.193)
Robust bias-corrected	0.069	0.729***	1.120***	0.336
	[0.115]	[0.240]	[0.245]	[0.219]
RD ITT (h = 50 km)	0.124	0.664***	0.868***	0.287
	(0.116)	(0.230)	(0.207)	(0.223)
Mean dep. var. (periphery, 50 km)	2.00	3.38	4.04	3.48
Bandwidth left / right (km)	29 / 45	43 / 48	26 / 48	30 / 64
Eff. N left / right	110 / 156	173 / 169	110 / 169	123 / 204
Panel B: victory margin (pp)				
RD ITT (MSE-two)	4.513	-4.175***	0.230	-1.869
	(3.902)	(1.106)	(0.954)	(1.143)
Robust bias-corrected	3.986	-4.983***	-0.022	-1.941
	[3.997]	[1.294]	[1.131]	[1.254]
RD ITT (h = 50 km)	4.140	-3.512***	0.513	-1.789
	(4.062)	(1.148)	(0.838)	(1.344)
Mean dep. var. (periphery, 50 km)	36.49	18.55	10.32	12.42
Bandwidth left / right (km)	48 / 42	45 / 39	33 / 51	82 / 68
Eff. N left / right	177 / 148	180 / 142	136 / 178	272 / 215
Panel C: turnout (%)				
RD ITT (MSE-two)	-9.277***	-1.094	0.239	0.466
	(2.360)	(1.664)	(0.718)	(1.082)
Robust bias-corrected	-9.675***	-2.107	0.292	0.230
	[2.958]	[1.898]	[0.862]	[1.328]
RD ITT (h = 50 km)	-4.177***	-1.068	0.603	0.489
	(1.564)	(1.444)	(0.608)	(1.012)
Mean dep. var. (periphery, 50 km)	55.77	67.02	83.75	78.46
Bandwidth left / right (km)	24 / 35	45 / 37	56 / 33	43 / 46
Eff. N left / right	73 / 116	175 / 133	208 / 114	172 / 162

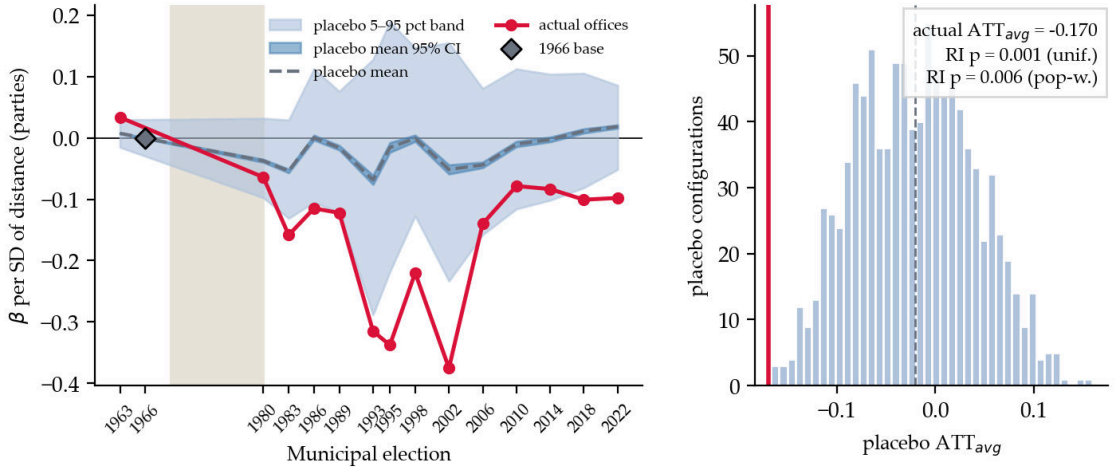
Notes: Era-pooled companion of the trajectory figures. Within each era the outcome is the district mean over its municipal elections, and each cell is a separate local-linear RD with the boundary-segment covariates, MSE-two and common 50 km bandwidths, and nearest-neighbor clustering by department. Stars follow each row's own p-value. Eras: 1980–89 (return to democracy; turnout covers 1980/83 only, with no voter registry in 1986/89), 1993–98 (party-system collapse; turnout covers 1995/98), 2002–10, and 2014–22. The early turnout drop is behavioral rather than mechanical. The voter registry itself is smooth at the boundary in 1980/83 (RD on log registered voters of $+0.20$ to $+0.25$, $p > 0.24$) and so is its subsequent growth, ruling out a denominator artifact from differential registration of the newly enfranchised. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure 14: NEP in the panel design: event study and randomization inference, 1963–2022

(a) Event-study estimates, 1966 base



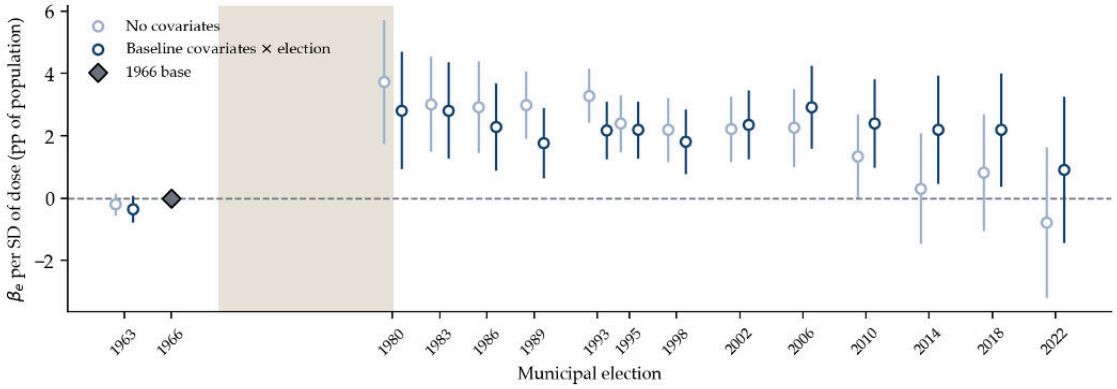
(b) Randomization inference on the event study: actual offices vs. 1,000 placebo configurations



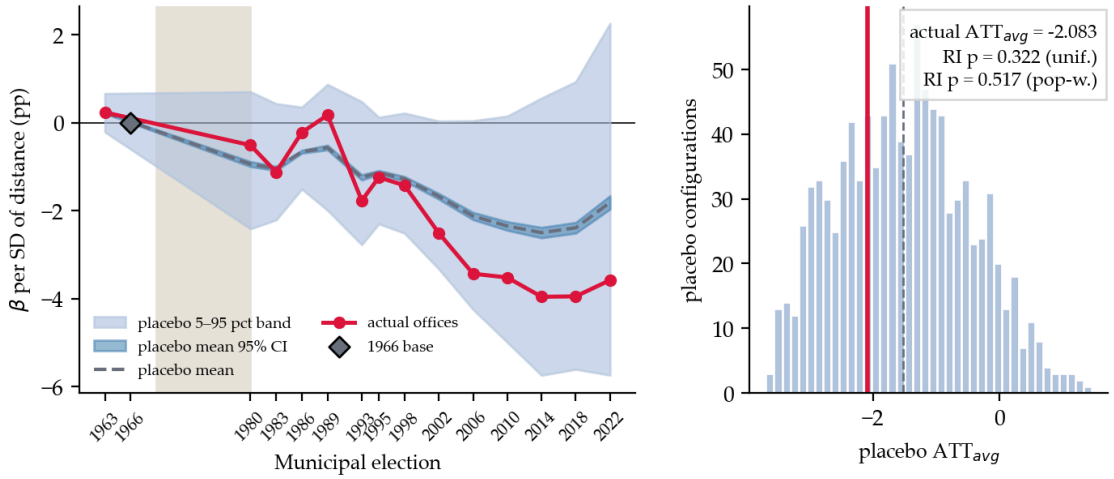
Notes: Panel (a): event-study coefficients β_e on standardized land-reform dose (1 SD = 21.1pp of district area) for all 15 municipal elections, with 1966 normalized to zero (diamond). Both series include district and election fixed effects. Light markers use fixed effects only and dark markers add the baseline covariates interacted with election dummies, so each predetermined characteristic follows its own free time path. Whiskers are 95% confidence intervals with district-clustered standard errors. The shaded band marks the reform period (1969–80). Panel (b): the same event-study specification re-estimated with the treatment measured as the standardized distance to the nearest Agrarian Reform Zone office, the reduced form of the instrument, and then re-estimated 1,000 times relocating the twelve offices to equally suitable cities of their zones (the placebo configurations of Note B1 in Appendix B). Left: the actual trajectory against the 5–95 percentile band of the placebo trajectories and the placebo mean with its Monte Carlo confidence interval. Right: the distribution of placebo ATT$_{avg}$, the average of the 13 post-reform coefficients, with the actual value in red. The RI p-value is the share of configurations with an ATT$_{avg}$ at least as large in absolute value, under the uniform and the population-weighted sampling schemes.

Figure 15: Votes cast per capita in the panel design: event study and randomization inference, 1963–2022

(a) Event-study estimates, 1966 base



(b) Randomization inference on the event study: actual offices vs. 1,000 placebo configurations

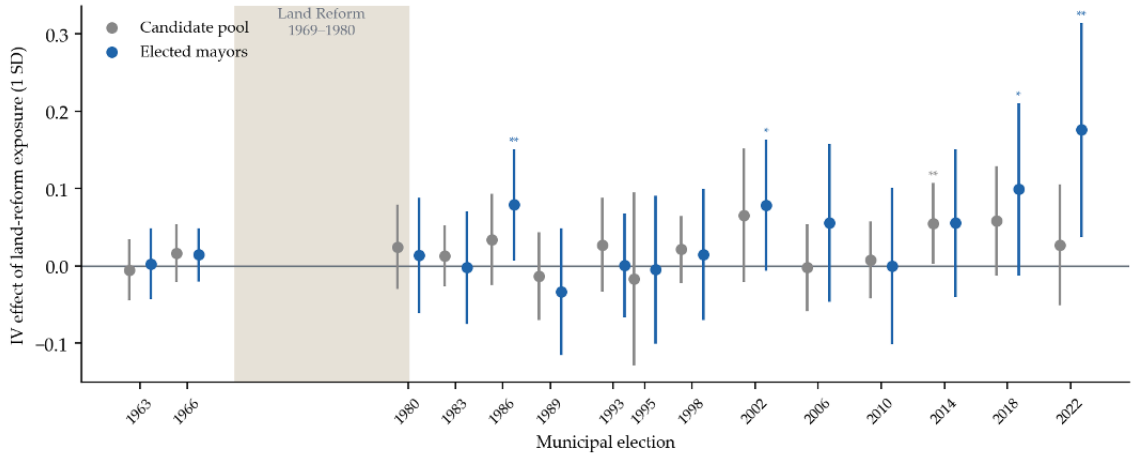


Notes: Panels as in the previous figure for votes cast per capita, the participation proxy: votes cast (party votes plus blank and null ballots) over log-linearly interpolated census population, on the panel of 490 stable 1876-parent territorial units without provincial capitals. Table B16 documents the construction. Panel (a) reports estimates in pp of population per SD of dose, with light and dark markers as in the previous figure. Panel (b) applies the randomization-inference exercise of the previous figure to this outcome.

Candidate selection

Instrumental Variables

Figure 16: IV effect of land-reform exposure on indigenous representation, by election, 1963–2022



Notes: Each point is the 2SLS coefficient on standardized land-reform exposure from a separate cross-sectional regression for that municipal election, on the probability that the candidate (gray) or the elected mayor (blue) has both Quechua or Aymara surnames (*ind2*). Col (3) specification without election fixed effects and without the age and gender controls, which do not exist before 2002. Bars are 95% confidence intervals with standard errors clustered at the district level. Sources as in Table 15: the historical JNE candidate panel for 1963–1998 and the modern candidate file for 2002–2022. The 1963 and 1966 elections predate the reform and serve as placebos. Stars mark conventional significance: * p<0.1, ** p<0.05, *** p<0.01.

Table 13: Land-reform exposure and candidates' years of education (2002–2022)

	Years of Education (2002–2022)			
	(1)	(2)	(3)	(4)
Panel A: OLS estimates				
Land reform exposure (SD)	0.192***	0.113***	0.075**	0.065**
	(0.030)	(0.032)	(0.032)	(0.031)
Panel B: Second-stage 2SLS estimates				
Land reform exposure (SD)	1.065***	1.479***	1.520***	1.155***
	(0.184)	(0.376)	(0.390)	(0.306)
Anderson–Rubin p-value	0.000	0.000	0.000	0.000
F-statistic of excluded instrument	69.14	26.24	23.92	29.67
Sociodemographic controls	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Lasso
Institutional controls	No	Yes	Yes	Lasso
State capacity controls	No	No	Yes	Lasso
Hacienda and ethnicity controls	No	No	Yes	Lasso
Agrarian Reform Zone FE	No	Yes	Yes	Yes
Election FE	No	No	Yes	Yes
Observations	46111	46111	40976	40976
Clusters	1542	1542	1375	1375
Mean Dep. Var.	14.221	14.221	14.241	14.241

Notes: Candidate-level panel of district-mayor candidates in the estimation sample (1,383–1,385 districts per election), pooling the six municipal elections 2002–2022. The 2002–2014 records come from the validated panel of Artiles, Kleine-Rueschkamp and León-Ciliotta (2021) and the 2018–2022 records from the JNE sworn-CV platform, coded with the same rules. Land-reform exposure is standardized (1 SD = 21.1pp of district area) and instrumented with the standardized distance to the nearest Agrarian Reform Zone office. Column structure, control groups and fixed effects as in Tables 5–7, adding sociodemographic controls (candidate age, gender and indigenous surname, with the outcome excluded from this set where applicable). Standard errors clustered at the district level. The Anderson–Rubin p-value is the cluster-robust reduced-form test of the instrument on the 2SLS sample. Col (4): post-double-selection lasso over the non-geographic control groups, with geographic and sociodemographic controls and both fixed effects partialled out. Two caveats: the 2010 education and experience variables carry a coding anomaly inherited from the source panel (results are robust to excluding 2010), and the work and national-party variables change measurement in 2018 (election fixed effects absorb level shifts). Dependent variable: years of education. *** p<0.01, ** p<0.05, * p<0.1.

Table 14: Land-reform exposure and candidates' party experience (2002–2022)

	Years of Party Experience (2002–2022)			
	(1)	(2)	(3)	(4)
Panel A: OLS estimates				
Land reform exposure (SD)	0.016	-0.000	0.002	-0.006
	(0.015)	(0.018)	(0.019)	(0.019)
Panel B: Second-stage 2SLS estimates				
Land reform exposure (SD)	0.150**	0.235	0.305**	0.201*
	(0.076)	(0.145)	(0.144)	(0.121)
Anderson–Rubin p-value	0.041	0.087	0.021	0.085
F-statistic of excluded instrument	71.13	27.15	24.64	32.69
Sociodemographic controls	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Lasso
Institutional controls	No	Yes	Yes	Lasso
State capacity controls	No	No	Yes	Lasso
Hacienda and ethnicity controls	No	No	Yes	Lasso
Agrarian Reform Zone FE	No	Yes	Yes	Yes
Election FE	No	No	Yes	Yes
Observations	48859	48859	43430	43430
Clusters	1542	1542	1375	1375
Mean Dep. Var.	0.620	0.620	0.614	0.614

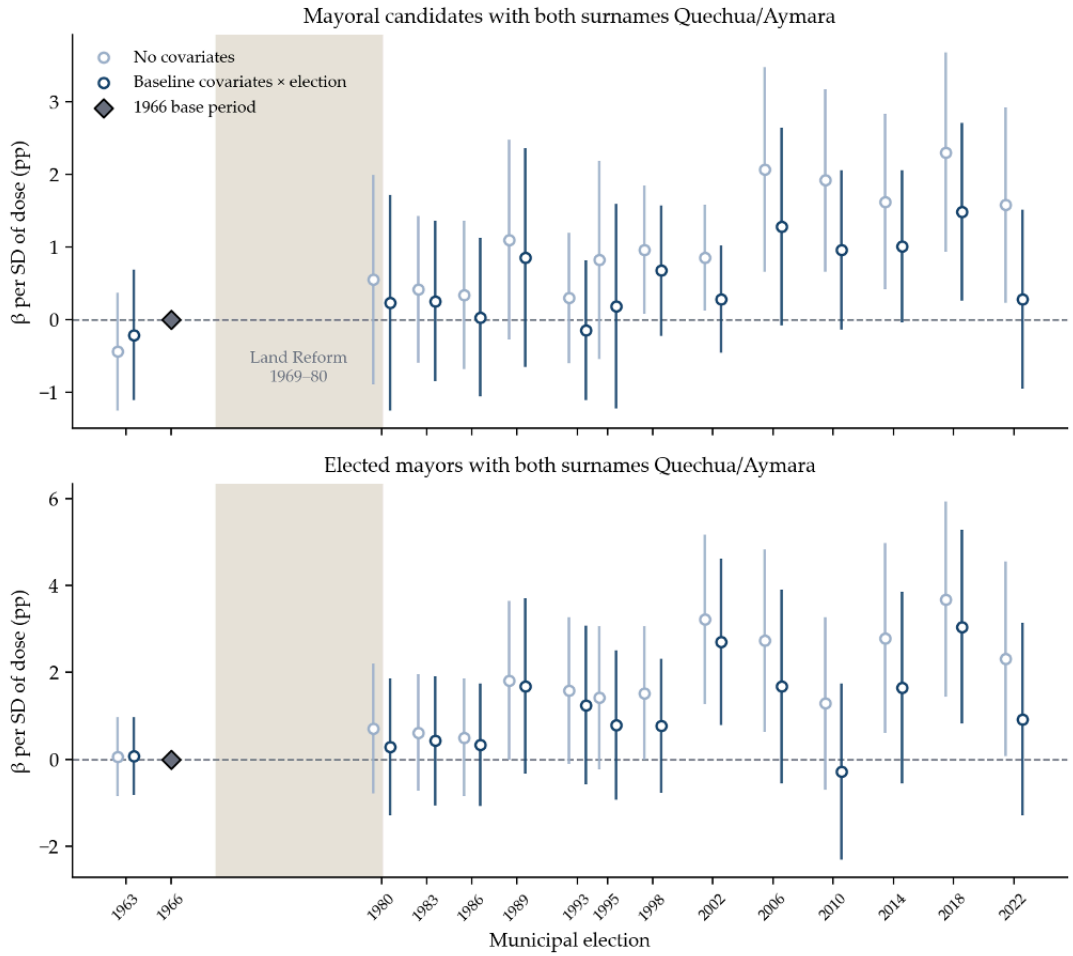
Notes: Panel construction, specification, inference and caveats as in Table 13. Dependent variable: years of party-office experience declared in the sworn CV (longest-tenure rule of the source coding). *** p<0.01, ** p<0.05, * p<0.1.

Table 15: Land-reform exposure and indigenous political entry: IV estimates by window, 1963–2022

	Pre-reform 1963–66		1980–2022		2002–2022	
	Candidates	Elected	Candidates	Elected	Candidates	Elected
2SLS: land-reform exposure (1 SD)	0.94	1.00	2.81	4.17**	3.80*	7.43**
	(1.42)	(1.42)	(1.77)	(2.05)	(2.23)	(2.96)
First-stage F	19.9	26.6	24.1	33.7	24.8	35.5
Mean dep. var. (%)	1.8	1.6	6.9	6.0	8.3	8.2
Observations	3,804	2,007	66,531	14,489	44,470	7,252

Notes: Candidate-level 2SLS of the *ind2* indicator (both paternal and maternal surnames Quechua or Aymara, following the dictionaries of Artiles (2022)) on standardized land-reform exposure, instrumented with the standardized distance to the nearest Agrarian Reform Zone office. Candidates columns use the full pool of district-mayor candidates and Elected columns restrict the sample to election winners. Sources: the historical JNE candidate panel for 1963–1998 and the modern candidate file for 2002–2022. Each window pools its elections with election fixed effects. The specification is column (3) of the IV tables (all district control groups, Agrarian Reform Zone fixed effects), without the age and gender controls, which do not exist before 2002. In 2002–2022, adding them leaves the estimates essentially unchanged. Coefficients in percentage points. Standard errors clustered by district. The pre-reform window is a placebo: exposure does not predict indigenous candidacies or victories in the two elections held before the reform. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure 17: Indigenous political entry election by election: candidates and elected mayors with both surnames Quechua or Aymara



Notes: Event-study coefficients from the specification of Table 16, plotted for all 15 municipal elections with 1966 normalized to zero (diamond). Shaded band, the reform period 1969–80. Whiskers, 95% confidence intervals with district-clustered standard errors. Light markers: district and election fixed effects only. Dark markers: adding baseline covariates interacted with election dummies.

Table 16: Land-reform dose and indigenous political entry: mayoral candidates and elected mayors with both surnames Quechua or Aymara

	Mayoral candidates		Elected mayors	
	(1)	(2)	(3)	(4)
ATT_{avg}, 1980–2022	1.144***	0.572	1.860***	1.172**
	(0.428)	(0.397)	(0.533)	(0.483)
ATT, 2006–2022		1.007**		1.402**
		(0.473)		(0.651)
2002 election	0.855**	0.288	3.223***	2.703***
	(0.370)	(0.376)	(0.995)	(0.978)
2018 election	2.307***	1.490**	3.682***	3.046***
	(0.701)	(0.623)	(1.146)	(1.137)
Pre-reform placebo (1963)	-0.438	-0.210	0.050	0.078
	(0.414)	(0.458)	(0.464)	(0.457)
Wild bootstrap p (department)		0.089		0.132
Mean dep. var. (post-1980)	5.93		5.83	
Observations (post-1980)	14,595		14,042	
Baseline covariates × election	No	Yes	No	Yes
District and election fixed effects	Yes	Yes	Yes	Yes

Notes: Event-study estimates on the district × election panel of mayoral contests, 1963–2022, with the same specification as Table B13: standardized land-reform dose interacted with election dummies, 1966 base period, district and election fixed effects, and (even columns) the baseline covariate set interacted with election dummies. Standard errors clustered by district. The outcome is the share of mayoral candidates (columns 1–2) and of elected mayors (columns 3–4) whose paternal and maternal surnames are both Quechua or Aymara, following the dictionaries of Artiles (2022). This is the strict marker of the paper's candidate-level IV evidence. The broad any-surname marker (mean 27%, diluted by mestizo surname mixing) shows no effect. Sources: the historical JNE candidate panel for 1963–2002 and the modern JNE candidate file for 2006–2022. The two agree in the overlapping election (indigenous share of 27.9 vs. 28.2% in 2002), and the level break of the source change (about 5pp, common to all districts) is absorbed by the election fixed effects. ATT_{avg} averages the 13 post-reform coefficients. The 2006–2022 row averages the five modern elections. *** p<0.01, ** p<0.05, * p<0.1.

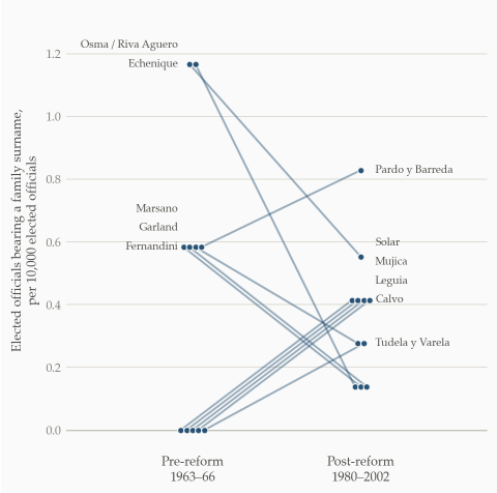
Mechanisms

Local political capture

Figure 18: Elite exit, indigenous entry: the changing composition of elected local office

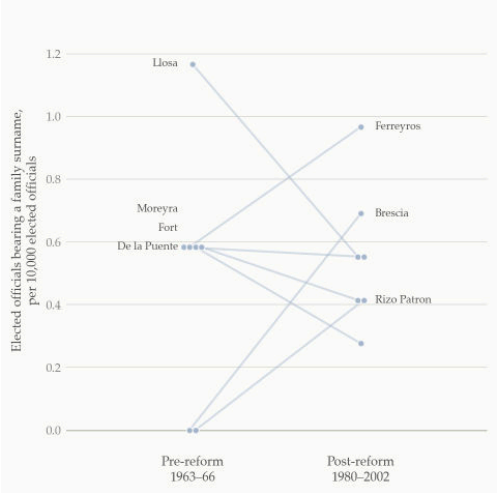
(a) Traditional landed elite (List A)

List total: 6.4 → 4.4 per 10k (−31%)



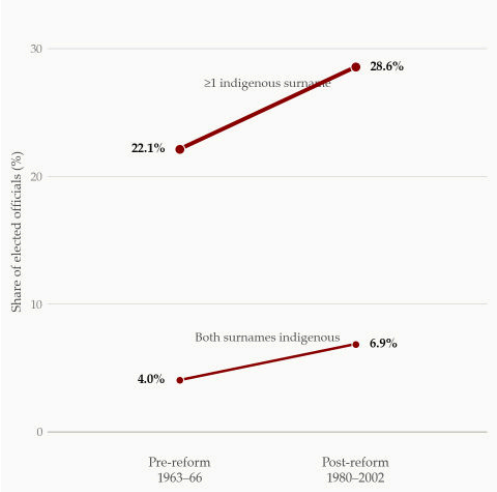
(b) Placebo: new economic elite (List B)

List total: 4.1 → 4.1 per 10k (+1%)



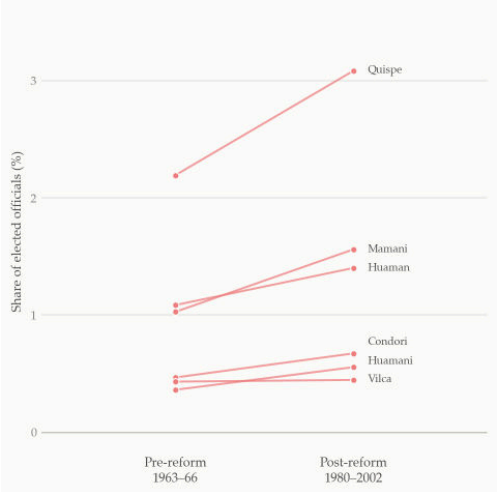
(c) Indigenous surnames

≥1 indigenous surname: 22.1% → 28.6% (+29%)



(d) Indigenous surnames: examples

Most frequent surnames, own scale



Notes: Elected district mayors and councilors from the historical JNE records of the 1963–2002 municipal elections: 17,149 elected officials in the pre-reform elections (1963, 1966) and 72,426 in the post-reform elections (1980–2002). Panel (a): officials bearing a surname of a documented traditional landed-elite family (List A, 48 surnames curated from the historiography, Matos Mar, 1976; Miller, 1982; Quiroz, 1988; Gonzales, 1991), per 10,000 elected officials. Matching is strict: a surname counts only within the family's home department and is excluded when its frequency exceeds 25 per 10,000 candidates nationally or 2.5 per 10,000 within the home department. Panel (b): the placebo List B, the post-reform economic elite of ca. 2000 (Figueroa, 2008) purged of families with pre-reform presence, under the same matching rule and on the same scale. The placebo moves +1% against −31% for List A. In both panels thin lines trace individual families with at least two appearances. Panel (c): share of elected officials with at least one surname and with both surnames classified as Quechua or Aymara with the dictionaries of Artiles (2022). Panel (d): the most frequent indigenous surnames, on their own scale. The franchise was restricted to literates until 1980, so the pre/post contrast conflates the land reform with enfranchisement. Causal estimates come from the exposure design of Table 17. Construction details in Note D1 of Appendix D.

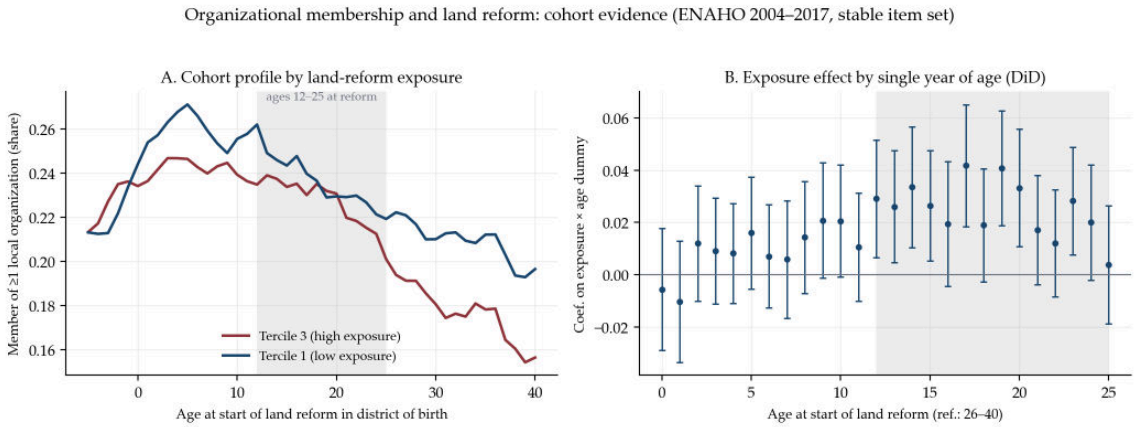
Table 17: Land-reform exposure and the replacement of incumbent political families

	(1)	(2)	(3)	(4)
	Incumbent share, 1980–83		Incumbent share, 1980–2002	
	OLS	2SLS	OLS	2SLS
Panel A: elected offices held by incumbent families				
Land-reform exposure (std)	-0.017**	-0.108*	-0.017**	-0.096*
	(0.008)	(0.065)	(0.007)	(0.055)
Mean dep. var.	0.511	0.511	0.455	0.455
First-stage F (KP)		13.4		13.5
Observations	1043	1043	1053	1053
Panel B: candidacies by incumbent families				
Land-reform exposure (std)	-0.017**	-0.127**	-0.017***	-0.099*
	(0.007)	(0.061)	(0.006)	(0.054)
Mean dep. var.	0.484	0.484	0.411	0.411
First-stage F (KP)		13.5		13.5
Observations	1051	1051	1053	1053
Geographic controls	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes
State capacity controls	Yes	Yes	Yes	Yes
Hacienda and ethnicity controls	Yes	Yes	Yes	Yes
Agrarian Reform Zone FE	Yes	Yes	Yes	Yes

Notes: District cross-section: 1,378 districts of the reform-era sample matched to the 1963–2002 JNE electoral panel. Incumbent families are within-district surname networks over elected officials, in families holding at least one elected seat in the 1963/66 municipal elections (construction and exclusion rules in Note D1 of Appendix D). Panel A: share of elected district offices (mayor and councilors) held by incumbent families in 1980–83 (columns (1)–(2), the two-election window symmetric to the pre-reform baseline) or 1980–2002 (columns (3)–(4)). Panel B: share of candidacies from incumbent families in the same windows, where non-elected candidates inherit a family when they share a surname with the district's elected network. The candidacy margin falls at least as much as the seat margin: incumbent families withdraw from competition rather than run and lose. The effect on the share of non-elected candidacies alone, which shares no observations with Panel A, is at least as large (2SLS -0.145, se 0.053), while the incumbents' win-rate advantage (mean $+0.03$) does not respond to exposure (2SLS $+0.02$, se 0.02). Land-reform exposure is standardized (1 SD = 21.1 pp of district area) and instrumented with the standardized distance to the nearest Agrarian Reform Zone office. All columns include the column (3) control set of the main tables and Agrarian Reform Zone fixed effects. Standard errors clustered at the province level (about 190 clusters). First-stage F is the cluster-robust Kleibergen–Paap statistic. *** p<0.01, ** p<0.05, * p<0.1.

Social organization

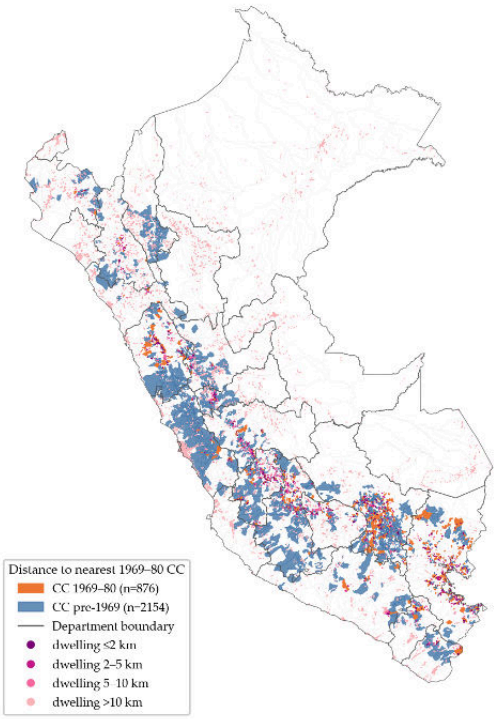
Figure 19: Organizational membership and land reform: cohort evidence



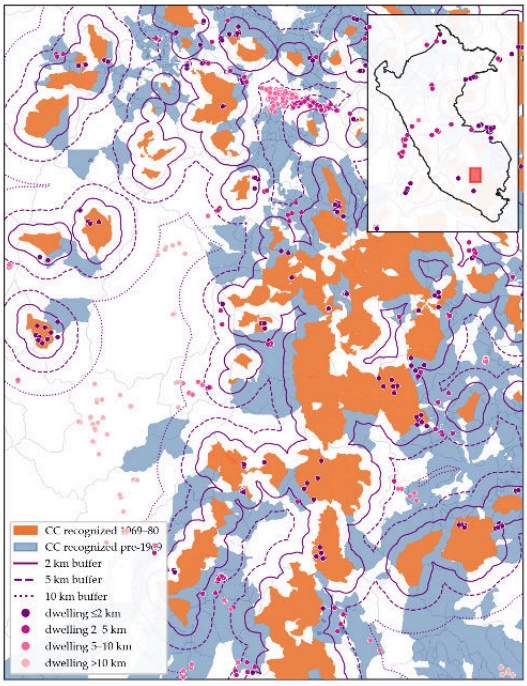
Notes: Adults born in coast and highland districts where some land reform took place, ENAHO 2004–2017 (about 312,000 person-year observations from the citizen-participation module, which records membership person by person). The outcome is membership in at least one local organization, restricted to the twelve organization types surveyed in every wave (sports and cultural clubs, political party, neighborhood association, rondas campesinas, irrigation users, professional association, union, mothers' club, APAFA, vaso de leche, comedor popular). Panel (a): mean membership by single year of age at the start of land reform in the district of birth (3-year centered moving average), top vs. bottom tercile of land-reform intensity. The shaded band marks ages 12–25. Panel (b): coefficients on land-reform exposure interacted with single-year age-at-reform dummies (reference: ages 26–40) from the cohort difference-in-differences specification with district-of-birth, birth-cohort and survey-year fixed effects, province-of-birth cubic cohort trends, and gender and native-mother-tongue controls. Whiskers are 95% confidence intervals with standard errors clustered by district of birth (1,146 clusters). The binned estimate for ages 12–25 is $+0.023$ (se 0.007) per unit of exposure, about $+2.3$pp on a 30% base for full exposure. Ages 0–11: $+0.009$ (n.s.). The estimate strengthens among non-movers ($+0.035$), survives sampling weights, a Romano–Wolf familywise correction over the six index-outcome hypotheses ($p = 0.006$), and replicates in pre-2012 waves alone. Reported attitudes show precise zeros in the same design. Design, data construction and the estimating equation in Note D2 of Appendix D.

Figure 20: The reform's organizational footprint in space: irrigation-users membership and distance to reform-recognized communities

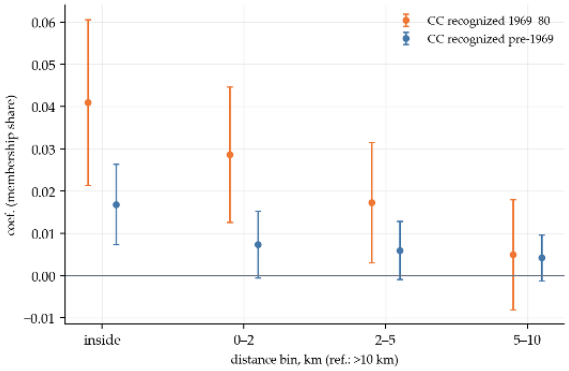
(a) Setting and treatment



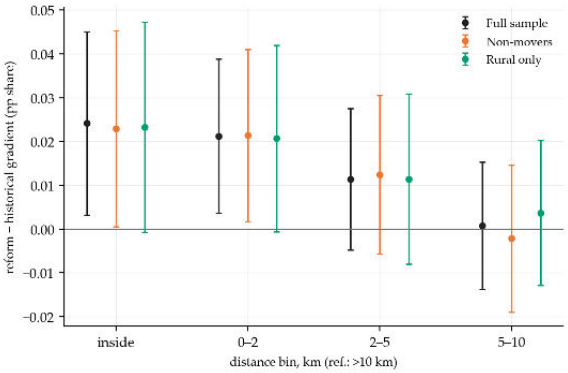
(b) Zoom: southern Cusco



(c) Irrigation-users membership: proximity gradient



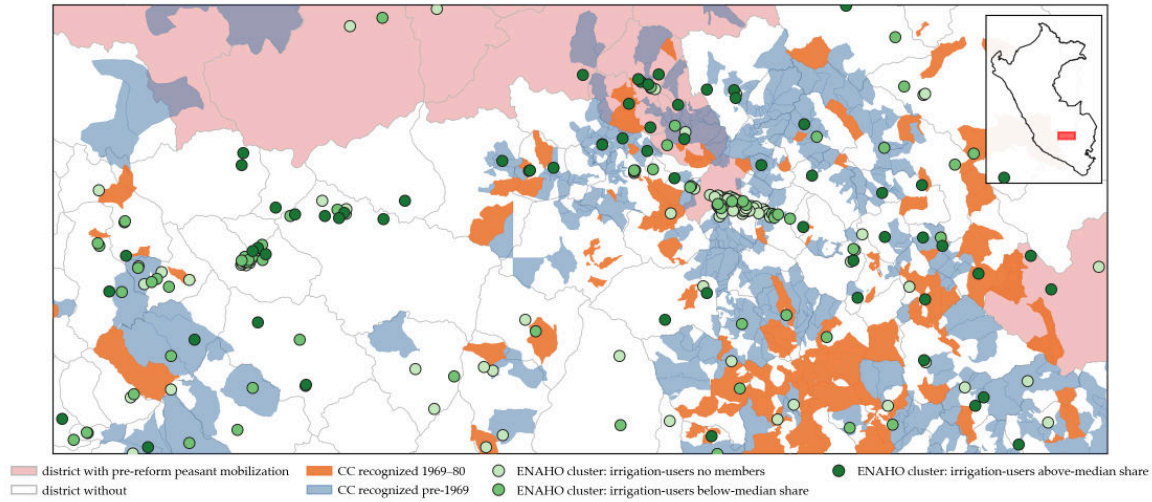
(d) Identification: reform − historical differential



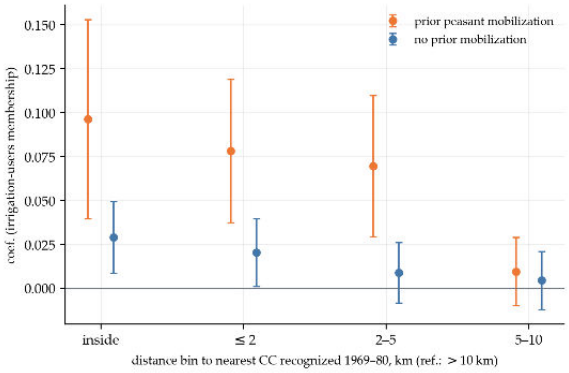
Notes: Panel (a): comunidades campesinas by recognition epoch (IBC polygons, recognition year from the 2009 COFOPRI directory validated against the SICCAM 2016 directory). Orange marks communities recognized in 1969–80 (n = 876) and blue those recognized before 1969 (n = 2,154). Dots are 66,625 georeferenced ENAHO dwellings (2007–17) colored by distance to the nearest 1969–80 community. Gray lines are department boundaries. Panel (b): representative window in southern Cusco (locator inset), with 2/5/10 km buffers around reform-recognized communities. Panel (c): coefficients on distance bins to the nearest 1969–80 (orange) and pre-1969 (blue) community, from the same regression (reference >10 km). The outcome is membership in an irrigation-users association (mean 2.2%). District-of-residence and survey-year fixed effects, individual controls, 95% confidence intervals, standard errors clustered by primary sampling unit (N = 733,272). Panel (d): the reform-minus-historical differential by bin, for the full, non-mover and rural samples. Construction and specification in Note D3 of Appendix D, robustness in Figure D2 and Table D6.

Figure 21: Activation vs. amplification: where the organizational footprint bites

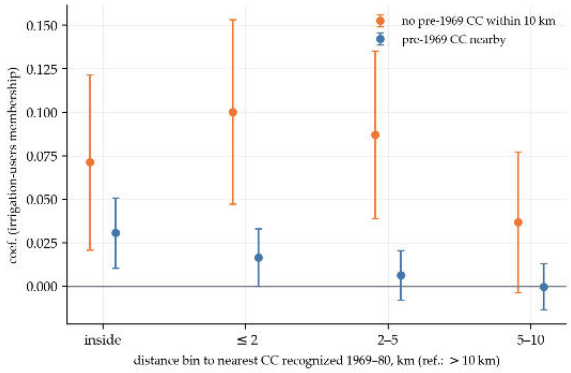
(a) Setting: recognition-era communities and pre-reform peasant mobilization (southern Cusco–Apurímac window)



(b) Amplification: gradient by pre-reform mobilization



(c) Activation: gradient by pre-1969 communal presence



Notes: Panel (a): representative window (locator inset). The large mobilized area at the top is La Convención, the canonical case of pre-reform peasant mobilization in the regional histories of Cant (2021). Districts are shaded by whether they recorded community-based peasant mobilization before the reform (pink, 92 of 1,570 districts, 4.3% of ENAHO persons), with community polygons by recognition epoch: orange recognized in 1969–80, blue before 1969. The reform-era polygons sit in both district types, which is the variation the panels exploit. Dots are the 288 georeferenced ENAHO clusters of the window (2007–17), colored by their share of adults in an irrigation-users association (none, below or above the median of positive shares). The darker clusters concentrate around the mobilized belt and the reform-era polygons of the north-center. The dense pale strip at the center is metropolitan Cusco, where irrigation membership is naturally absent. Panels (b) and (c): coefficients on distance bins to the nearest 1969–80-recognized community (reference >10 km) from the spatial-gradient specification of Note D3 (district-of-residence and survey-year fixed effects, individual controls, standard errors clustered by primary sampling unit, 95% confidence intervals), estimated separately by cell. The outcome is irrigation-users membership (base 2.2%). Panel (b): the gradient is roughly four times stronger in districts with prior mobilization, and the pre-1969 placebo is a precise zero in those same cells. Panel (c): the gradient concentrates where recognition arrived on communal vacuum, with no pre-1969 community within 10 km of the dwelling (52% of the sample). Numbers and the participation-index versions in Table D7.

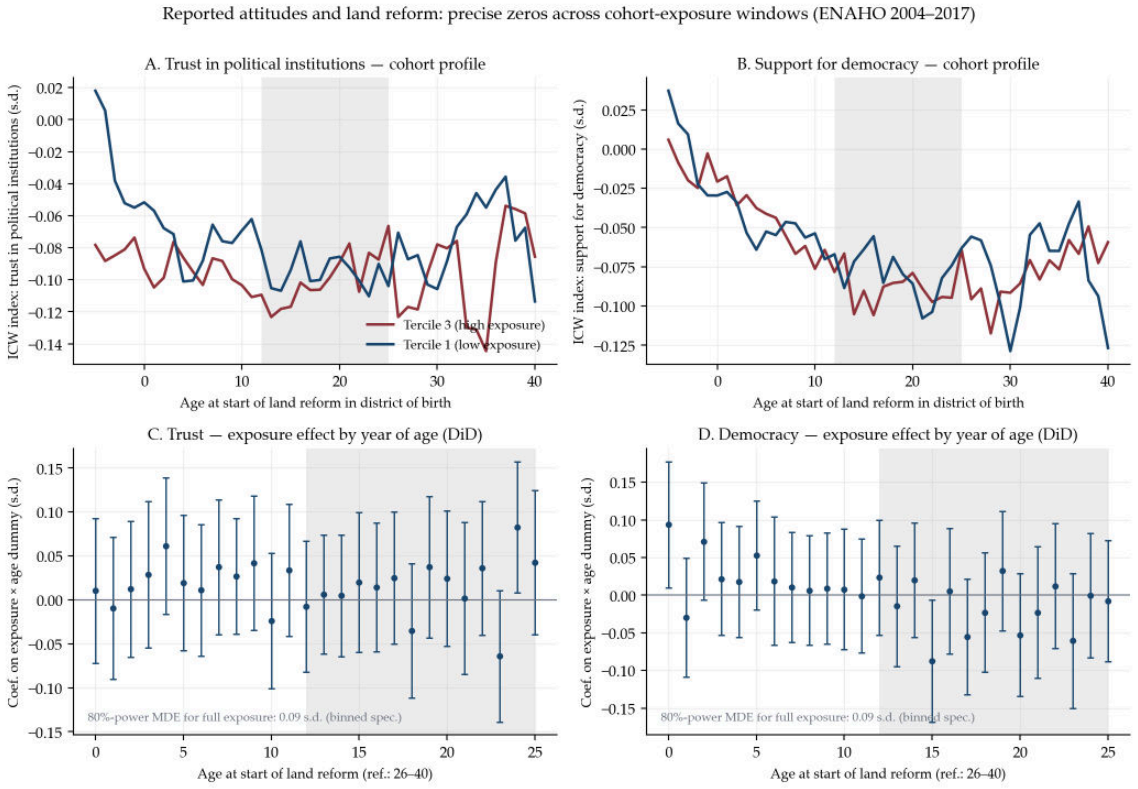
Table 18: Land-reform exposure and organizational membership of agricultural producers, 1994

	(1)	(2)	(3)	(4)	(5)	(6)
	Any organization		Producer organizations		Irrigation-users assoc.	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Land-reform exposure (std)	1.03	11.68**	0.54*	5.06*	0.17	10.46**
	(0.76)	(5.58)	(0.29)	(2.65)	(0.54)	(4.68)
Mean dep. var.	34.62	34.62	4.66	4.66	17.09	17.09
First-stage F (KP)		35.0		35.0		35.0
Observations	1370	1370	1370	1370	1370	1370
Geographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes	Yes	Yes
State capacity controls	Yes	Yes	Yes	Yes	Yes	Yes
Hacienda and ethnicity controls	Yes	Yes	Yes	Yes	Yes	Yes
Agrarian Reform Zone FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: District cross-section: 1,370 districts of the reform-era sample with the 1994 agricultural-census membership breakdown, producer-level shares reconstructed from the census microdata and aggregated to 1975-boundary districts. Dependent variables are shares of agricultural producers in percent (0–100), so coefficients read directly as percentage-point effects per SD of exposure: membership in any organization (columns (1)–(2)), in a producers' committee or farmers' association (columns (3)–(4)), and in a junta de usuarios de riego (columns (5)–(6)), the institutional heirs of the 1969 Ley General de Aguas, the same legislative package as the land-reform decree, and the organizational layer independently flagged by the spatial gradient of Figure 20. Land-reform exposure is standardized (1 SD = 21.1 pp of district area) and instrumented with the standardized distance to the nearest Agrarian Reform Zone office, with the column (3) controls and Agrarian Reform Zone fixed effects. Standard errors are clustered at the district level, with the cluster-robust Kleibergen–Paap first-stage F. Under province clustering the 2SLS t-statistics are 1.28, 1.58 and 1.37 (Kleibergen–Paap F 16.5). These estimates are read jointly with the spatial gradient and the cohort evidence as triangulation on the same organizational layer rather than as stand-alone results. *** p<0.01, ** p<0.05, * p<0.1.

Political preferences

Figure 22: Reported attitudes and land reform: precise zeros across cohort-exposure windows



Notes: Top row: mean inverse-covariance-weighted index by single year of age at the start of land reform in the district of birth (3-year centered moving average), for the top and bottom terciles of land-reform intensity, on the estimation sample of adults born in reform districts, ENAHO 2004–2017. Bottom row: coefficients on exposure interacted with single-year age-at-reform dummies (reference: ages 26–40), from the cohort difference-in-differences specification of Note D2 in Appendix D, with district-of-birth, birth-cohort and survey-year fixed effects, province-of-birth cubic cohort trends and individual controls. Whiskers are 95% confidence intervals with standard errors clustered by district of birth. The shaded band marks the politically formative window (ages 12–25). The 80%-power minimum detectable effect for full exposure in the binned specification is about 0.09 s.d. for both indices. Robustness (non-movers, sampling weights, Romano–Wolf correction) in Table D8.

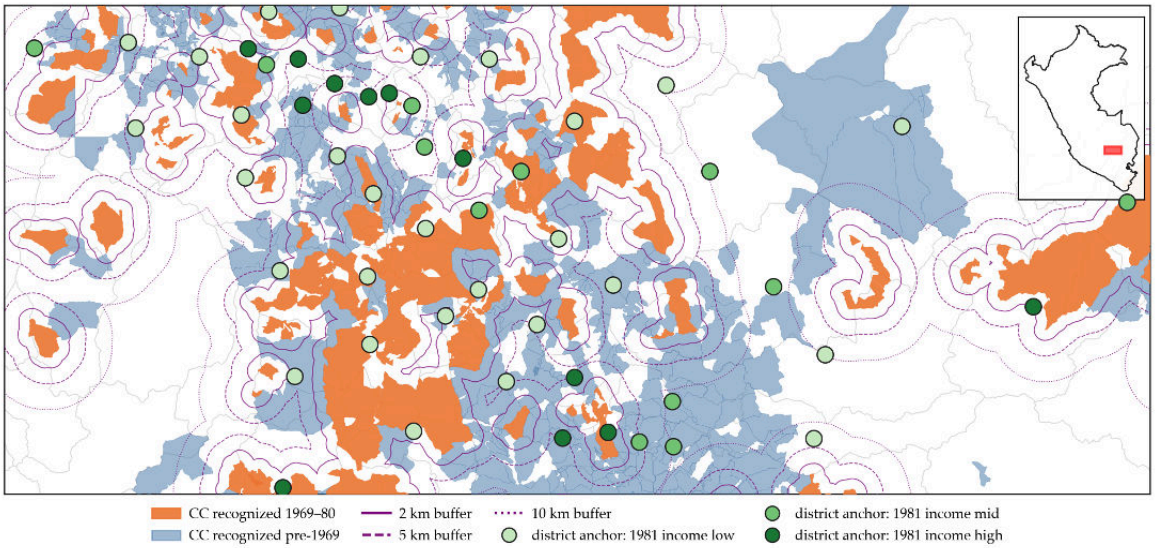
Table 19: Land-reform exposure and left voting in municipal elections, 1980–1989

	(1)	(2)	(3)	(4)	(5)	(6)
	Vote share		Candidacy share		Elected share	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Panel A: broad left (izquierda amplia)						
Land-reform exposure (std)	0.004	0.119***	0.011***	0.125***	0.005	0.103***
	(0.004)	(0.034)	(0.004)	(0.032)	(0.005)	(0.036)
Mean dep. var.	0.211	0.211	0.218	0.218	0.202	0.202
First-stage F (KP)		29.2		29.5		30.6
Observations	4030	4030	3995	3995	3857	3857
Panel B: Marxist left (IU and member parties)						
Land-reform exposure (std)	0.002	0.065**	0.009**	0.068**	0.004	0.042
	(0.004)	(0.029)	(0.004)	(0.027)	(0.005)	(0.032)
Mean dep. var.	0.190	0.190	0.194	0.194	0.180	0.180
First-stage F (KP)		29.2		29.5		30.6
Observations	4030	4030	3995	3995	3857	3857
Geographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes	Yes	Yes
State capacity controls	Yes	Yes	Yes	Yes	Yes	Yes
Hacienda and ethnicity controls	Yes	Yes	Yes	Yes	Yes	Yes
Agrarian Reform Zone FE	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes

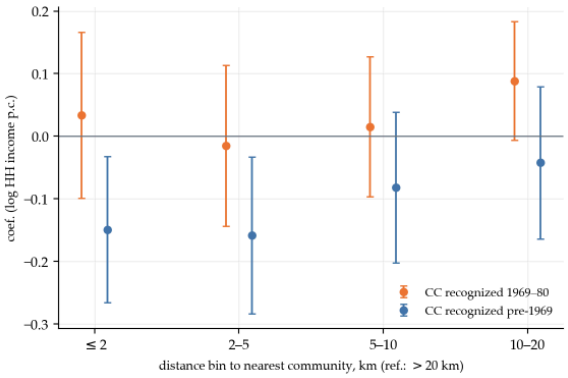
Notes: District-election panel of the municipal elections of 1980, 1983, 1986 and 1989 (about 1,000 districts of the reform-era sample per election, matched to the exposure dataset through the JNE–INEI ubigeo crosswalk). Parties are classified with a dictionary of about 50 organizations coded from Tuesta Soldevilla (1995), the JNE electoral history, Van Cott (2005) and Madrid (2012), with year-specific aliases. In 1980–89 the dictionary covers 87–95% of valid votes (78–90% of candidacies). Classification coverage collapses with the proliferation of independents after 1993, so the post-1993 elections are excluded here. Pooled 1980–2002 estimates have the same signs at roughly half the magnitude. Panel A: broad left, Izquierda Unida and its member parties, plus the non-IU socialist parties and the peasant-based FRENATRACA. Panel B: Marxist left only (IU and member parties: PUM, PCP, UNIR, PSR, FOCEP, UDP, PRT and others). Outcomes: share of valid votes (blank and null votes excluded), share of candidacies, and share of elected offices (mayor and councilors) won by listed parties in the district-election. Land-reform exposure is standardized (1 SD = 21.1 pp of district area) and instrumented with the standardized distance to the nearest Agrarian Reform Zone office, with the column (3) controls and Agrarian Reform Zone and election fixed effects. Standard errors are clustered at the district level. First-stage F is the cluster-robust Kleibergen–Paap statistic. A randomization-inference check against the 1,000 placebo office configurations of Note B1 in Appendix B supports a reform-specific reading: the broad-left distance gradient survives the office placebo (conventional −2.5pp per SD of distance, purged −1.8, RI p = 0.034 uniform and 0.074 population-weighted) and is strongest and almost fully office-specific in indigenous-majority districts (purged −2.5 of −2.6, RI p = 0.026/0.010), the same districts where the boundary design finds the transitional turnout loss, so the votes cast there shifted left even as fewer were cast. The gradient weakens to marginal for the Marxist-only definition (RI p ≈ 0.16). *** p<0.01, ** p<0.05, * p<0.1.

Figure 23: Distance to reform-formalized communities and the 1981 economic outcomes

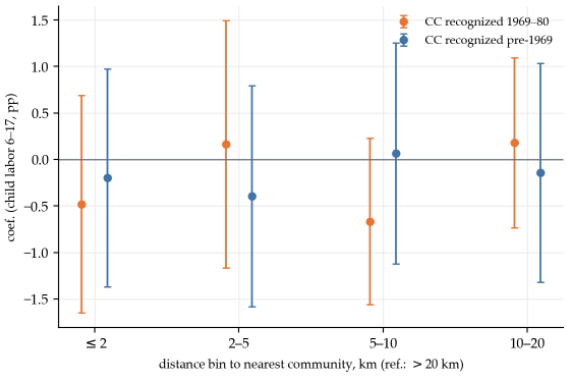
(a) The exercise in space: communities by recognition epoch, distance buffers, and district anchors shaded by 1981 income



(b) Household income per capita, 1981

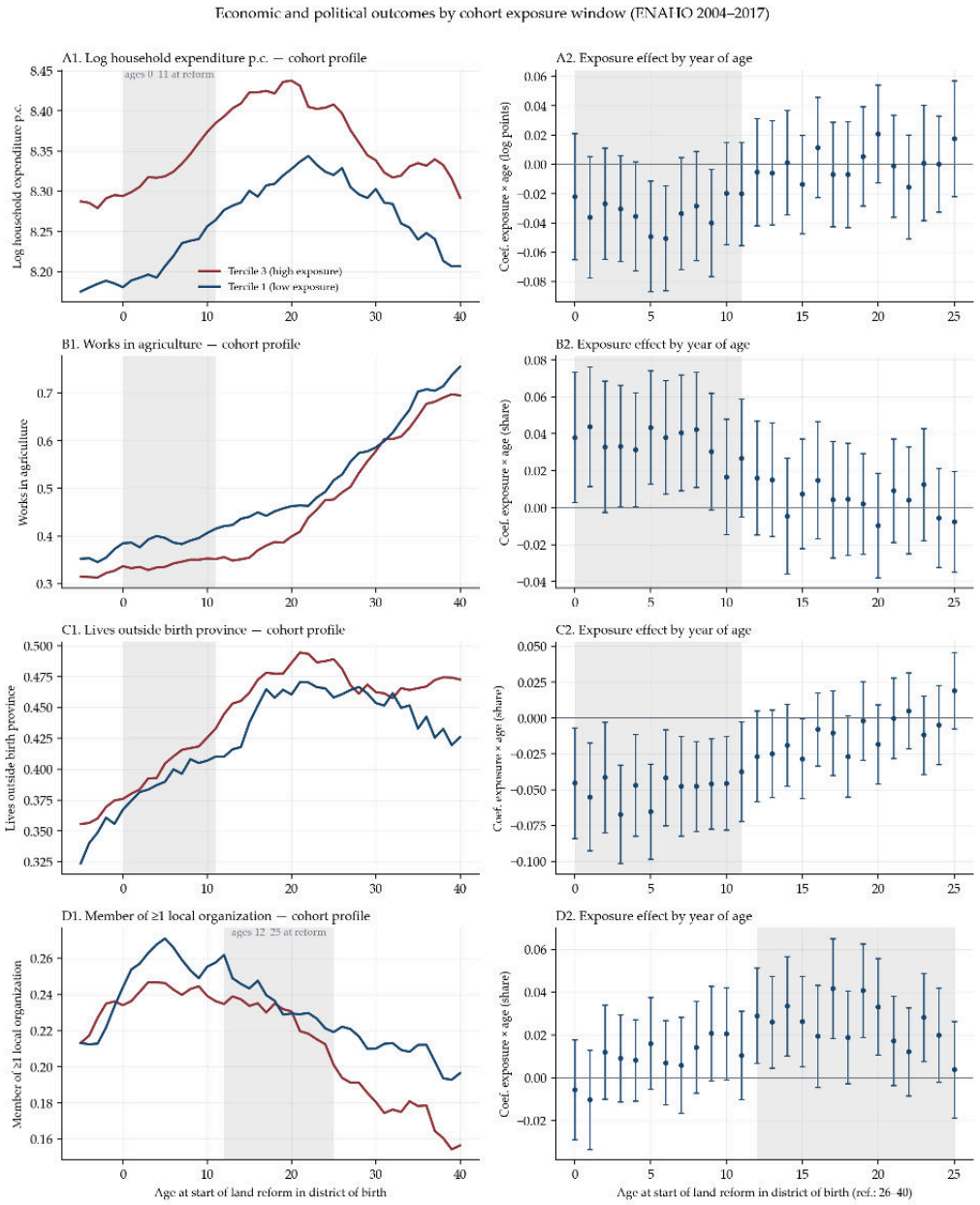


(c) Child labor 6–17, 1981



Notes: Panel (a): representative window in the southern sierra (locator inset; 52 district anchors, 155 communities recognized in 1969–80 and 383 recognized before 1969). Orange marks communities recognized during the reform and blue those recognized before 1969 (IBC polygons, recognition year from the 2009 COFOPRI directory). Purple contours are the 2, 5 and 10 km buffers around reform-formalized communities, the bins of the specification in Note D4 of Appendix D. Circles are the district anchor points from which distances are measured, shaded by terciles of mean household income per capita in the 1981 census. The visual claim is that dark and light circles are not sorted by their position relative to the orange polygons and their buffers. Panels (b) and (c): coefficients on the reform-era (orange) and pre-1969 (blue) distance bins from the same regression, with 95% confidence intervals and district-clustered standard errors (N = 1,107 districts; the 1981 sample excludes the Amazon, Apurímac and metropolitan Lima, see Table 20). Distance to reform-formalized communities carries no gradient in either outcome: every reform-era coefficient is statistically indistinguishable from zero and the joint tests fail to reject (income p = 0.39, child labor p = 0.42). This is not a matter of power. The same regressions detect the historical gradient sharply, with districts nearer a pre-1969 community showing substantially lower income in 1981 (about 15 log points within 5 km), so the design has enough precision to find a gradient where one exists. The early economic gains are therefore not organized around the reform's organizational footprint. The community gradient appears instead in the 1994 outcomes that are organizational in nature (Table D10).

Figure 24: Economic and political outcomes by cohort exposure window



Notes: Adults born in coast and highland districts where some land reform took place, ENAHO 2004–2017, economic outcomes assigned by district of birth (about 326,000 person-year observations; the birth-district question leaves the ENAHO questionnaire after 2017, which fixes the survey window). Rows A–C: log household expenditure per capita, works in agriculture (employed 14+), and lives outside the birth province. Row D: membership in at least one local organization (the stable item set of Figure 19). Left panels: mean outcome by single year of age at the start of land reform in the district of birth (3-year centered moving average), top vs. bottom tercile of land-reform intensity, with shaded bands marking ages 0–11 (rows A–C) and 12–25 (row D). Right panels: coefficients on exposure interacted with single-year age-at-reform dummies (reference: ages 26–40) from the cohort difference-in-differences of Note D2 in Appendix D, with 95% confidence intervals and standard errors clustered by district of birth. Binned estimates for ages 0–11 at full exposure: expenditure -2.8% ($t = 2.1$), agricultural work $+2.8$pp ($t = 2.5$), out-migration -4.3pp, with ages 12–25 indistinguishable from zero in all three. The political row shows the mirror image: $+2.3$pp on a 30% base at ages 12–25 ($t = 3.5$, Romano–Wolf $p = 0.006$) and zero at 0–11. The economic core survives a Romano–Wolf familywise correction over the 15 outcome-window hypotheses. The design replicates the household-income and migration estimates of Albertus, Espinoza and Fort (2020) on this independently reconstructed base (-6.4% vs. their -5.7%, -5.3pp vs. their -4.7pp). Full outcome battery, indigenous subsample and construction in Appendix D.

Table 20: Short-run economic effects of the land reform: the 1981 census

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log HH income per capita		Child labor 6–17 (%)		Electricity (%)		Running water (%)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Land-reform exposure (std)	0.02	0.31**	−0.20	−4.03***	0.91*	16.81***	−0.03	14.92***
Clustered S.E. (district)	(0.02)	(0.13)	(0.14)	(1.39)	(0.48)	(5.00)	(0.47)	(5.18)
Conley S.E. (100 Km.)	[0.02]	[0.15]**	[0.14]	[1.67]**	[0.49]*	[6.49]***	[0.57]	[6.43]**
Conley S.E. (200 Km.)	[0.02]	[0.17]*	[0.16]	[1.57]**	[0.45]**	[6.05]***	[0.63]	[6.76]**
Excl. Amazon (*ARZ zones 8–9*), 2SLS		0.31**		−3.89***		16.97***		15.04***
		(0.13)		(1.38)		(5.04)		(5.21)
Mean dep. var.	1.74	1.74	7.46	7.46	16.43	16.43	17.32	17.32
First-stage F (KP)		23.2		23.2		23.2		23.2
Observations	1107	1107	1107	1107	1107	1107	1107	1107
Geographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State capacity controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hacienda and ethnicity controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agrarian Reform Zone FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: District cross-section: district-level aggregates of the 1981 population census mapped to 1975-boundary districts through the 1981→1993 census correspondence and the district unifier (1,241 districts merge and 1,107 enter the regressions once the column (3) controls bind). Sample coverage: the 1981 electronic census microdata permanently lack Apurímac, Loreto and San Martín. INEI's own recovery from the magnetic tapes failed for those departments after the storage disks were damaged, an archival rather than economic loss. The selva is excluded by design and metropolitan Lima and Callao lack a historical-district match. Tacna, missing from the census correspondence, is recovered through a manual 1981→1993 name mapping (26 districts, with post-1981 districts inheriting their parent's values), though most Tacna districts lack the 1961 labor-force control and only two enter the estimation sample (coefficients move by at most 0.08). The 1981 census was fielded as the reform's main phase ended, so these are effects roughly a decade after it began, the short-run datapoint that Albertus, Espinoza and Fort (2020) note is unavailable in their household-survey data. Dependent variables: log average household income per capita, share of children 6–17 working (percent), and shares of dwellings with electricity and with running water (percent). Land-reform exposure is standardized (1 SD = 21.1 pp of district area) and instrumented with the standardized distance to the nearest Agrarian Reform Zone office, with the column (3) controls and Agrarian Reform Zone fixed effects. Standard errors clustered at the district level in parentheses. Conley spatial-HAC standard errors (Bartlett kernel, 100 and 200 km cutoffs) in brackets with stars from their own p-values. All four 2SLS effects survive the spatial correction (income at 5%/10%, the rest at 5% or better). First-stage F is the cluster-robust Kleibergen–Paap statistic. The memo row excluding the Amazon is uninformative here by construction: the census correspondence covers only the non-Amazonian country, so just four districts leave the sample. Exposed districts show substantially higher income, half the child labor, and roughly twice the electricity and water coverage a decade in. The early aggregate effect is positive and the child-labor decline signals improvement at the bottom. The cohort estimates of Albertus, Espinoza and Fort (2020) find lower household income in 2004–17, so the joint temporal pattern is an early boom followed by relative erosion. The night-lights trajectory of Appendix D covers the intervening decades. *** p<0.01, ** p<0.05, * p<0.1.

Table 21: Land reconcentration 1994–2012 and technification: the 2012 agricultural census

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: land reconcentration, 1994→2012 (changes)	Δ Land Gini (0–1)		Δ Top-10% land share (pp)		Δ Units ≥50 ha (pp)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Land-reform exposure (std)	−0.000	0.087***	0.06	10.38**	0.21	0.02
Clustered S.E. (district)	(0.004)	(0.034)	(0.56)	(4.28)	(0.22)	(1.71)
Conley S.E. (100 Km.)	[0.004]	[0.045]*	[0.57]	[5.66]*	[0.20]	[2.01]
Conley S.E. (200 Km.)	[0.004]	[0.050]*	[0.53]	[6.21]*	[0.17]	[1.84]
Sierra only, 2SLS		0.119***		13.84***		−1.24
		(0.037)		(4.60)		(1.44)
Excl. Amazon (ARZ zones 8–9), 2SLS		0.076***		9.55**		−0.84
		(0.029)		(3.79)		(1.49)
Mean dep. var.	0.012	0.012	1.15	1.15	−0.70	−0.70
First-stage F (KP)		36.2		35.6		35.6
Observations	1355	1355	1364	1364	1364	1364
Panel B: farm technology and services, 1994→2012 (changes)	Δ Tractor use (pp)		Δ Technical assistance (pp)		Δ Sought credit (pp)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Land-reform exposure (std)	1.67***	15.15***	−1.02***	−8.76***	0.86***	4.77***
Clustered S.E. (district)	(0.46)	(3.30)	(0.35)	(2.62)	(0.22)	(1.68)
Conley S.E. (100 Km.)	[0.55]***	[4.28]***	[0.38]***	[2.91]***	[0.22]***	[1.97]**
Conley S.E. (200 Km.)	[0.64]***	[4.82]***	[0.41]**	[3.15]***	[0.24]***	[2.16]**
Excl. Amazon (ARZ zones 8–9), 2SLS		14.73***		−9.90***		4.54***
		(3.15)		(2.34)		(1.49)
Mean dep. var.	6.41	6.41	−4.47	−4.47	0.05	0.05
First-stage F (KP)		35.5		35.5		35.5
Observations	1365	1365	1365	1365	1365	1365
Geographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes	Yes	Yes
State capacity controls	Yes	Yes	Yes	Yes	Yes	Yes
Hacienda and ethnicity controls	Yes	Yes	Yes	Yes	Yes	Yes
Agrarian Reform Zone FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: District cross-section (about 1,365 districts; CENAGRO-2012 microdata aggregated from the unit records, with roughly 6% of unit records lost to file corruption, approximately at random within districts). An agricultural unit's landholding is its declared total surface in the district. Panel A is the late-period test that the 1994 census table of Appendix D calls for: the change in the district land distribution between the two agricultural censuses. Where the reform hit, the land Gini and the top-decile share re-concentrate, and the magnitude essentially undoes the 1994 dispersion (-0.070 in 1994, $+0.088$ in changes, with a 2012 level gap that is a statistical zero). Two qualifications sharpen the reading. First, the share of units of 50 hectares or more does not move and remains 8pp lower in 2012 levels: this is not the return of the latifundio but concentration within the sub-50-hectare distribution, a local small-to-medium-scale acquiring class. Second, the memo rows show the change is driven by the sierra. In the coastal subsample the instrument has no power (Kleibergen–Paap F = 1.8–2.1, N = 182–188), so the coastal agro-export margin is untestable with this design rather than zero, and the result is not an Amazonian artifact (excluding zones 8–9 leaves the Gini change at $+0.076$). Under Conley spatial-HAC standard errors both changes hold at the 10% level, and at 5% in the sierra-only cut where the effect concentrates (Gini $+0.119$, Conley se 0.058). The reconcentration is stable across every sample variant but is a 10%-level result once spatial correlation is allowed for. Panel B reports changes in farm technology and services between the two censuses. Levels are uninformative here: exposure already predicted tractor use and fertilizer in the 1994 levels at nearly the 2012 magnitudes, so the level gradient prices pre-existing geography rather than an effect, and differencing removes it. The 1994 analogues are built from the corresponding items of the 1994 questionnaire under the same coding rules as the 2012 ones. For credit the 1994 counterpart is whether the unit applied for a loan, matching the 2012 item. Mechanization grows faster where the reform hit ($+15$pp against a mean change of $+6.4$pp) and credit applications rise ($+4.8$pp, from a 1994 level gradient that was a precise zero). The timing brackets the PETT titling wave, consistent with a collateral channel opening once the property-rights gap closed. Technical assistance moves the other way and sharply: it falls 8.8pp more in exposed districts, on a national decline of 4.5pp. The state extension services that accompanied the reform's first decades withdrew disproportionately from the districts the reform had created. All three hold under Conley standard errors at both cutoffs and under the Amazon exclusion. Fertilizer (-2.9, se 3.3) and certified seed ($+4.6$, se 3.7) are indistinguishable from zero once differenced. Panel A is the late-period verdict: the pro-poor land margin of 1994 eroded through the land market over 1994–2012, consistent with the higher 2007 expenditure Gini of Table 22. Panel B adds that the erosion ran alongside a retreat of the state from the reformed sector, with private capital and credit only partly filling the gap. Detail on every technology, credit and services margin in Table D12. *** p<0.01, ** p<0.05, * p<0.1.

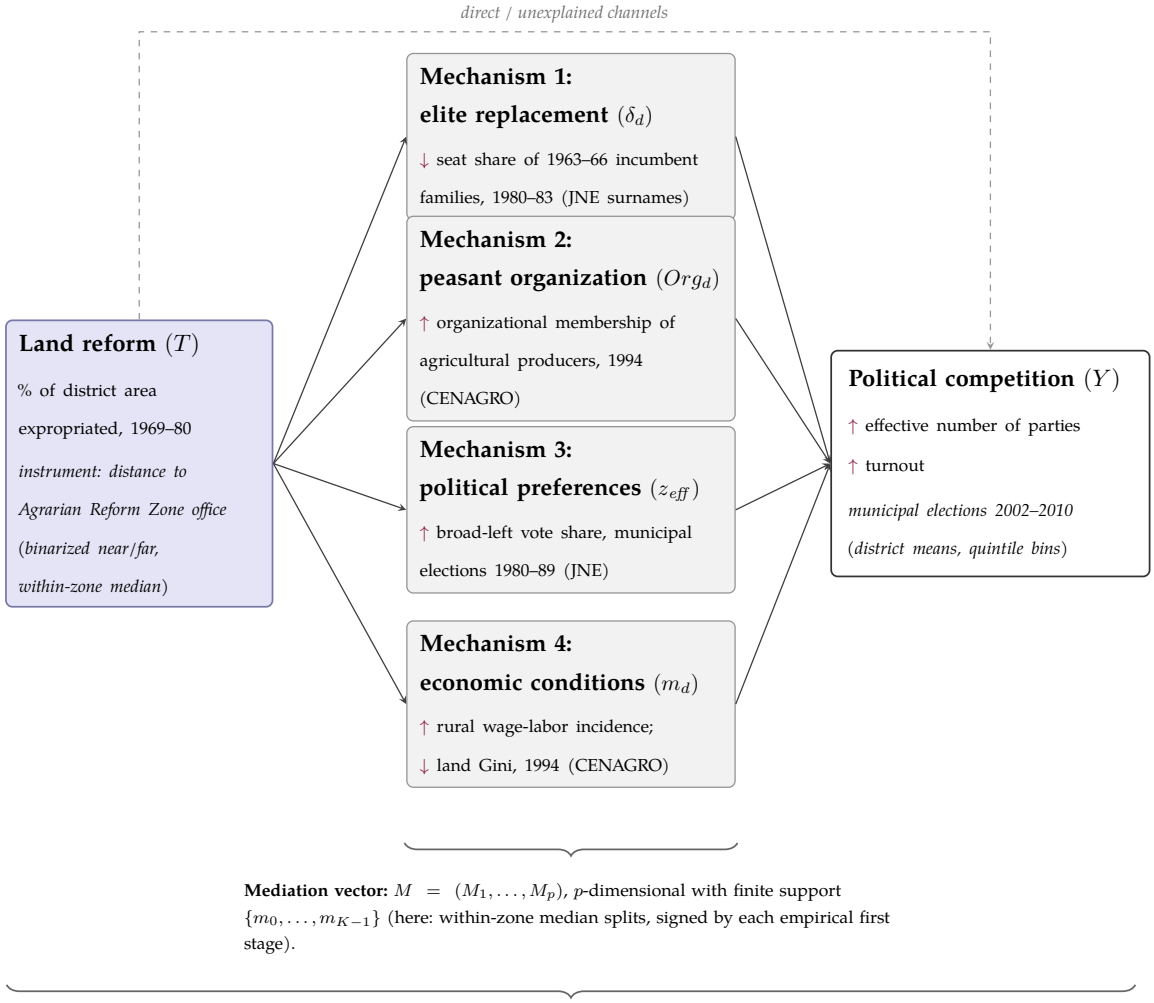
Table 22: Poverty and expenditure inequality four decades in: the 2007 district poverty map

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Poverty rate FGT0 (%)		Extreme poverty FGT0 (%)		Poverty gap FGT1 (%)		Expenditure Gini (0–1)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Land-reform exposure (std)	−0.11	1.64	−0.64	1.36	−0.22	2.37	0.001	0.016**
Clustered S.E. (district)	(0.37)	(2.43)	(0.41)	(2.52)	(0.24)	(1.56)	(0.001)	(0.007)
Conley S.E. (100 Km.)	[0.45]	[3.67]	[0.47]	[4.82]	[0.28]	[2.88]	[0.002]	[0.014]
Conley S.E. (200 Km.)	[0.47]	[3.34]	[0.48]	[5.35]	[0.28]	[3.17]	[0.002]	[0.017]
Excl. Amazon (ARZ zones 8–9), 2SLS		4.26**		5.83**		4.64***		0.027***
		(2.11)		(2.36)		(1.49)		(0.007)
Mean dep. var.	59.17	59.17	27.57	27.57	21.59	21.59	0.289	0.289
First-stage F (KP)		34.5		34.5		34.5		34.5
Observations	1374	1374	1374	1374	1374	1374	1374	1374
Geographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State capacity controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hacienda and ethnicity controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agrarian Reform Zone FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: District cross-section (1,568 districts; INEI's 2007 provincial and district poverty map, statistical annexes, matched on the 2007 ubigeo). Dependent variables: the district poverty headcount (FGT0), the extreme-poverty headcount, the poverty gap (FGT1), all in percent, and the Gini coefficient of household expenditure per capita. These indicators are small-area estimates (ELL method combining the 2007 census with the ENAHO expenditure survey), so the dependent variable is predicted rather than directly observed welfare. Specification as in the previous tables. Two results, and inference matters for both. First, poverty levels show no effect in the full sample. The 2SLS confidence intervals rule out reductions larger than about 3 points of headcount per SD of exposure, so the early income gains of 1981 have not persisted as lower poverty four decades in. Excluding the Amazon (memo row) turns all three poverty margins positive and significant under clustered errors (headcount $+4.3$, extreme poverty $+5.8$, poverty gap $+4.6$), which would say that where the reform actually expropriated haciendas exposed districts are poorer four decades on, but none of the three survives the Conley correction (all $t < 1.6$). The honest statement about poverty levels is that they are not lower, and possibly higher, with the positive reading resting on inference that does not account for spatial correlation. Second, the expenditure Gini is higher where the reform hit ($+0.016$ in the full sample and $+0.027$ excluding the Amazon, about 0.4 and 0.7 SD of the cross-district distribution). The sample restriction carries the result: the full-sample estimate does not survive Conley, while the Amazon-excluded estimate does at 5% and 10% for the two cutoffs. Consumption inequality is the one 2007 margin that holds up under spatial-robust inference, and only in the subsample where the treatment is well defined. *** p<0.01, ** p<0.05, * p<0.1.

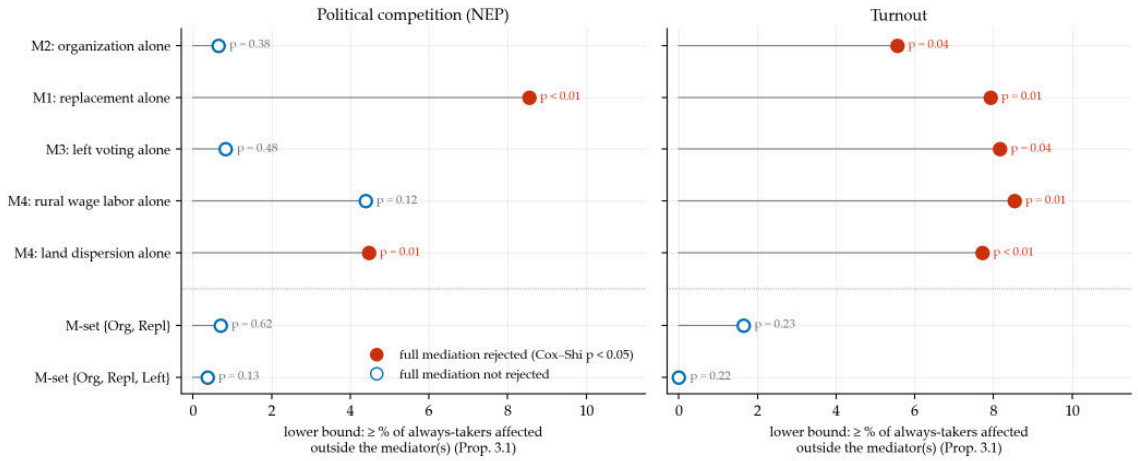
Mediation Analysis

Figure 25: The mechanism decomposition in one diagram



Notes: Each mechanism enters as a district-level mediator measured before the outcome window (1980–94 vs. elections 2002–10), binarized at the within-zone median in the direction of its empirical first stage. The dashed arrow is what the Kwon–Roth statistic bounds: the share of the effect not explained by the mediator(s) under test. Method and implementation in Note D5 of Appendix D.

Figure 26: Kwon–Roth full-mediation tests: lower bounds and verdicts, by mechanism and jointly



Notes: Each dot is the lower bound on the fraction of always-takers, units whose mediator does not respond to the binarized instrument, whose outcome is nevertheless moved, that is, the mass of the effect that must operate outside the stated mediator(s) (Kwon–Roth Proposition 3.1, linear programming over the full set of implications). Filled red: the sharp null of full mediation is rejected (Cox–Shi $p < 0.05$, annotated). Hollow blue: not rejected. Rows above the dotted line test each mechanism alone on its maximal sample. Rows below test mechanism sets on their intersection ($N = 1{,}182$). The visual summary of the decomposition: for NEP, organization alone or left voting alone could carry the entire effect. For turnout, every single mechanism is rejected but the set {Org, Repl} is jointly sufficient, and adding left voting drives the unexplained mass essentially to zero. Full estimates, Andrews–Roth–Pakes p-values and defier breakdowns in Table 23. Method and implementation in Note D5 of Appendix D.

Table 23: Kwon–Roth tests of full mediation, by mechanism and jointly

	NEP				Turnout			
Full mediation through:	$\geq$% ATs affected	CS p-val	ARP p-val	defier breakdown	$\geq$% ATs affected	CS p-val	ARP p-val	defier breakdown
Mechanism 2: organization alone	0.6%	0.384	0.45	0.5%	5.5%	**0.039**	0.04	3.1%
N = 1,418								
Mechanism 1: replacement alone	8.5%	**0.002**	0.02	5.1%	7.9%	**0.010**	0.05	4.3%
N = 1,188								
Mechanism 3: left voting alone	0.8%	0.481	0.55	0.7%	8.2%	**0.036**	0.15	4.0%
N = 1,315								
Mechanism 4: rural wage labor alone	4.4%	0.121	0.08	2.9%	8.5%	**0.010**	0.65	3.8%
N = 1,418								
Mechanism 4: land dispersion alone	4.5%	**0.007**	0.01	3.9%	7.7%	**0.003**	0.01	4.6%
N = 1,406								
M-set {Org, Repl}	0.7%	0.619	0.65	0.6%	1.6%	0.227	0.25	1.3%
N = 1,182								
M-set {Org, Repl, Left}	0.4%	0.129	0.25	0.3%	0.0%	0.216	0.25	0.0%
N = 1,182								

Notes: Each row tests the sharp null that the entire effect of the binarized instrument (near vs. far from an Agrarian Reform Zone office, within-zone median) on the outcome operates through the stated mediator(s), with single-mediator rows on each mediator's maximal sample and mediator sets on their intersection. "$\geq$% ATs affected" is the lower bound on the fraction of always-takers, units whose mediator does not respond to treatment, whose outcome is nevertheless moved (Kwon–Roth Proposition 3.1, linear programming over the full set of implications). CS and ARP are Cox–Shi and Andrews–Roth–Pakes p-values with analytic variance (**bold**: CS < 0.05). The defier breakdown is the smallest defier share at which the rejection is overturned. Mediators are measured before the outcome window (1980–94 vs. elections 2002–10) and binarized at within-zone medians in the direction of their empirical first stage (first stages: organization $+0.18$, replacement $+0.08$, left voting $+0.13$, hired labor $+0.10$, Gini dispersion $+0.12$). Three readings. First, for NEP, full mediation through organization alone (CS $= 0.38$) or left voting alone (0.48) cannot be rejected, while replacement (0.002) and land dispersion (0.007) are rejected and rural wage labor is marginal (0.12). Second, no single mechanism fully mediates turnout, with every candidate rejected at 5% (organization 0.039, replacement 0.010, left voting 0.036, wage labor 0.010, Gini 0.003, and at least 5.5–8.5% of always-takers affected), but the pair {Org, Repl} is jointly sufficient for both outcomes (CS $= 0.62$ and 0.23) and adding left voting drives the bounds essentially to zero. Third, the economic mediators are rejected nearly everywhere despite healthy first stages: the path from reform to political competition does not run through the 1994 economic states, the formalization of the age-window dissociation of the cohort evidence. The turnout rejections are fragile to modest monotonicity violations (breakdown at 3–5% defiers), the relevant caveat. An independent reimplementation with cluster-bootstrap inference agrees on every verdict except rural wage labor on NEP, where Cox–Shi is more conservative. Method and implementation in Note D5 of Appendix D.

Appendix for:

Land Without Masters
Local Political Effects of Peru's Land Reform

Author:

Héctor Paredes

Paris School of Economics (*PSE*)

October 30, 2026

Contents

Appendix A — Data Appendix . . . **107**

Note A1 Electoral Data . . . 107

Note A2 Land Reform Exposure . . . 108

Note A3 Historical Variables . . . 108

Appendix B — Political Competition . . . **110**

Instrumental Variables . . . 110

Figure B1 Balance of Pre-reform Covariates . . . 110

Table B1 First Stage — Full Inference (Conley-implied F) . . . 112

Table B2 Placebo IVs and Falsification Tests . . . 113

Note B1 Randomization Inference: Placebo ARZ Offices — Procedure . . . 114

Table B3 Randomization Inference: Actual Offices vs. Placebo Configurations . . . 116

Note B2 Recentered IV: The Expected Instrument . . . 117

Table B4 Spatial Unit-root Diagnostics: Raw vs. Recentered Instrument . . . 118

Table B5 IV: Other Measures of Political Competition . . . 119

Regression Discontinuity Design . . . 120

Table B6 Sample Definition: Eligible Zones, Exclusions and Boundary Bands . . . 120

Table B7 First Stage: Local-linear Estimates of the Dose Discontinuity . . . 121

Table B8 Main Electoral Results, Pooled 1980–2022 (ITT) . . . 122

Table B9 Heterogeneity: Language, Candidate Supply and the 1933 Electorate . . . 123

Table B10 Few-cluster Robust Inference (Wild Bootstrap) . . . 124

Figure B2 Bandwidth and Kernel Sensitivity . . . 125

Table B11 Compound Treatments at the Departmental Boundary . . . 126

Table B12 Placebo Cutoffs . . . 127

Differences in Differences . . . 128

Table B13 Event-study Estimates, 1963–2022 . . . 128

Table B14 Dose-response of the Effect on the Number of Effective Parties (CGBS) . . . 129

Figure B3 Party Breadth Event Studies . . . 130

Table B15 Party Breadth: National, Regional and Local Vote Shares . . . 131

Table B16 Votes Cast per Capita (Participation Proxy) . . . 132

Table B17 HonestDiD Sensitivity (Rambachan–Roth) . . . 133

Appendix C — Candidate Selection . . . **134**

Table C1 Education Levels . . . 134

Table C2 Other Experience Measures and Party Type . . . 135

Table C3 Indigenous Entry: IV Estimates by Era . . . 136

Table C4 Female and Age . . . 137

Table C5 Other Representation Characteristics (2018–2022) . . . 138

Appendix D — Mechanisms . . . 139

Pre-reform electoral control . . . 139

Figure D1 Hacienda Exposure (1876) and Indigenous Registered Voters (1933) . . . 139

Local political capture . . . 140

Note D1 Data Construction: Incumbent Families and Elite Surnames . . . 140

Table D1 Incumbent Families in the Modern Era . . . 143

Table D2 No Deconcentration (ΔH) . . . 144

Table D3 Family Survival . . . 145

Table D4 Heterogeneity by Pre-reform Capture . . . 146

Table D5 Targeting of Pre-reform Capture . . . 147

Social organization . . . 148

Note D2 The Cohort Difference-in-Differences: Design and Data . . . 148

Note D3 The Spatial Gradient: Construction and Specification . . . 150

Figure D2 Spatial Gradient Robustness . . . 152

Table D6 Spatial Gradient: Levels, Differentials, ORAMS Split . . . 153

Table D7 Footprint by Prior Mobilization and Communal Vacuum . . . 154

Political preferences . . . 155

Table D8 Attitudes: Cohort DiD Point Estimates . . . 155

Economic vis-à-vis political changes . . . 156

Note D4 Economic Outcomes: Conventions, Community Gradients, First-Stage Decomposition . . . 156

Table D9 Land Distribution and Rural Labor, 1994 . . . 158

Table D10 Distance-to-Community Gradients . . . 159

Table D11 First-Stage Decomposition . . . 160

Table D12 Technology, Credit and Services, 1994–2012 . . . 161

Table D13 Land Titling and the PETT Window . . . 162

Table D14 Night Lights: Level and Long Differences . . . 163

Figure D3 Night-Lights Trajectory 1992–2022 . . . 164

Figure D4 Cohort Evidence: Household Outcomes . . . 165

Figure D5 Cohort Evidence: Employment and Migration . . . 166

Figure D6 Cohort Evidence: Additional Labor Outcomes . . . 167

Figure D7 Cohort Evidence: Indigenous Subsample . . . 168

Mediation Analysis . . . 169

Note D5 The Kwon–Roth Mediation Test: Implementation and Logic . . . 169

Appendix E — Conceptual Framework . . . 171

E.1 Setting . . . 171

E.2 Two margins of political control . . . 172
E.3 Land expropriation: the break of both margins . . . 175
E.4 Party entry . . . 177
E.5 Whose preferences count . . . 180
E.6 Candidate selection . . . 181
E.7 Turnout: demobilization against autonomous participation . . . 182
E.8 Summary of predictions . . . 183
E.9 The economic channel . . . 184

Appendix A — Data Appendix

Note A1. Electoral data

District-level electoral outcomes are compiled from the records of Peru's National Elections Board (Jurado Nacional de Elecciones, JNE), covering the fifteen municipal district elections held between 1963 and 2022. For the 1963–2002 elections, results by party and district were assembled from the JNE historical archive and its governance observatory INFOGOB, digitized and harmonized into a district-level panel. Results for 2002–2022 come from the JNE's official electoral results platform. Candidate-level information for 2002–2022 was assembled by web-scraping the JNE candidacy registries and the candidates' sworn résumés (*hojas de vida*), building on the INFOGOB-based compilation of Artiles, Kleine-Rueschkamp and León-Ciliotta (2021). All series are aggregated within each unit's 1975 district boundaries, weighting each contest by registered electors. Provincial capitals are excluded throughout. Election-level totals were cross-validated against the compendium *Perú Político en Cifras* (Tuesta Soldevilla, 2001).

The main district-level outcomes are:

- **Number of effective parties (NEP)**: the inverse Herfindahl index of party vote shares, $1/\sum_j s_j^2$, in the district contest.
- **Turnout**: votes cast as a share of registered voters. The voter registry is unavailable for 1963 (about 30 districts), 1986, 1989 and 1993.
- **Victory margin**: the difference in vote shares between the two most-voted parties, in percentage points.
- **Broad-left vote share** (1980–89): party-dictionary classification covering Izquierda Unida and its member parties, non-IU socialist parties and FRENATRACA.

The main candidate-level outcomes (2002–2022) are:

- **Education**: years of schooling and completed education levels, reconstructed from the sworn résumés.
- **Partisan experience**: years of service in elected office and in party office, and membership in a national political party.
- **Indigenous background**: surname-based classification matching the linguistic roots of candidates' surnames against Quechua and Aymara dictionaries, following Artiles (2022). The baseline measure flags candidates with at least one native surname and the strict measure requires both surnames.

- **Elected**: an indicator for winning the mayoral contest.

Note A2. Land reform exposure

District exposure to the reform is built from the universe of expropriation records generated under the 1969 Agrarian Reform Law (D.L. 17716), originally compiled from Ministry of Agriculture archives by Albertus (2020) and Albertus, Espinoza and Fort (2020). Rather than taking the replication files as given, we work at the level of individual expropriation events. Records are validated, cleaned and assigned to the districts of the 1972 census geography, dated by decree, and classified by land type, separating in particular productive land from expropriations of uncultivable (*eriazo*) land, which we exploit in placebo tests. The resulting treatment variable is the accumulated extension of expropriated hectares during the reform as a share of the district's total area. In the average district, 14% of total area (41% of agricultural land) was expropriated. The standardized version used throughout normalizes one standard deviation to 21.1 percentage points of district area. From the same event-level database we construct the share of the 1972 agricultural surface expropriated, exposure restricted to land in active private use (the measure of Albertus, Espinoza and Fort (2020)), the start year and duration of the reform in the district (median two years), and the *eriazo* share used as placebo treatment.

Note A3. Historical variables

Pre-reform district characteristics draw on the historical statistical record. The population censuses of 1876, 1940, 1961 and 1972 provide district-level demographics, labor-force composition, literacy and mother tongue, as well as the settlement history of the hacienda system. The 1961 agricultural census adds irrigation and land-tenure structure. The 1961 directory of populated centers (CCPP) identifies hacienda and *fundo* settlements together with settlement populations, the input for the feasibility screens of the placebo-office exercise. Agro-ecological potential comes from the FAO-GAEZ v4 crop-suitability grids. Historical connectivity is measured from the road and railway networks digitized from historical cartography (the 1920s network and the 1958 network). The district-level institutional and state-capacity measures (the mita catchment, prior uprisings and social movements, caudillo presence and public personnel in 1961) follow Albertus (2020).

A second group of sources supports the pre-reform electorate margins and the mechanism analysis. We digitized the 1933 Electoral Census (the earliest national register disaggregated by ethnicity), which records registered voters by province and their indigenous share, together with district-level registration rolls. These are the basis for the

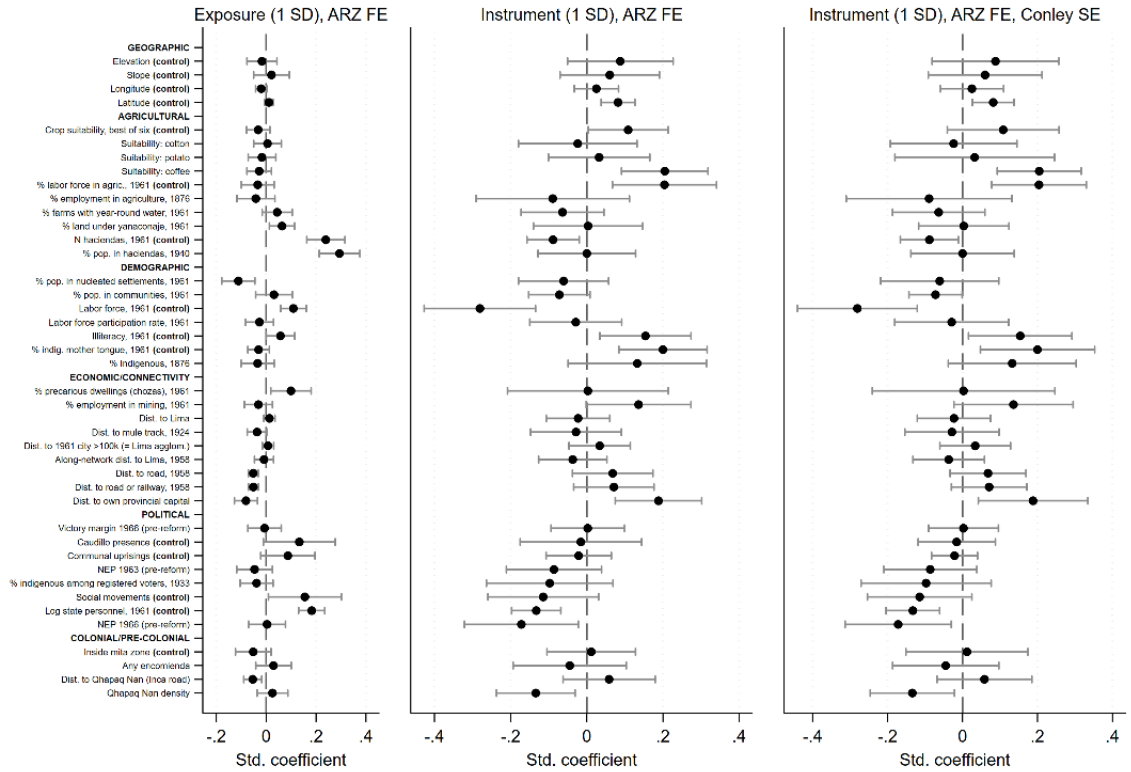
1933-electorate heterogeneity splits and for documenting the exclusionary pre-reform franchise. For the organizational and economic mechanisms we use the 1994 agricultural census (CENAGRO), from which we reconstruct district-level membership in agrarian organizations, and its 2012 wave for the evolution of the land distribution. We also use georeferenced household-survey modules (ENAHO) on participation in local organizations. Finally, the administrative registry of peasant-community recognition resolutions identifies the communities formalized during the reform and the involvement of its corporatist mobilization agencies.

Appendix B — Political Competition

Instrumental Variables

Figures

Figure B1: Balance of pre-reform covariates: land-reform exposure and the distance-to-ARZ instrument



Notes: Each row is a separate district-level regression of the standardized covariate on (left panel) standardized land-reform exposure and (middle and right panels) the standardized distance to the nearest Agrarian Reform Zone office, with Agrarian Reform Zone fixed effects (13 zones) in all panels. Bars are 95% confidence intervals: standard errors clustered at the province level (∼190 clusters) in the left and middle panels, and Conley spatial-HAC (Bartlett kernel, 200 km) in the right panel. Rows marked **(control)** enter the OLS/IV regressions; the rest are figure-only covariates. Each row uses the covariate's own available sample (N ranges from ∼1,050 to 1,570; sources differ). Sources. Agricultural: FAO-GAEZ v4 crop suitabilities (1961–90 climate, rain-fed, low inputs); the 1961 population census (labor force in agriculture); the 1961 agricultural census (irrigation and land tenure); the 1961 CCPP directory (haciendas); and the 1876 and 1940 population censuses. Demographic: the 1961 population census, 1876 census. Economic/connectivity: the 1961 population censuses (mining employment, precarious dwellings) and roads and railways of historical maps (1924, 1958). Political: municipal election results of 1963 and 1966 (JNE records); the 1933 electoral census (% indigenous among registered voters); and the district-level institutional and state-capacity measures (uprisings, social movements, caudillos, state personnel) of Albertus (2020).

Tables

Table B1: First stage with Conley-implied excluded-instrument F

	Land-reform exposure (share of district area)			
	(1)	(2)	(3)	(4)
Distance to nearest ARZ office (1 SD)	-0.059***	-0.041***	-0.043***	-0.043***
Robust S.E.	(0.007)	(0.007)	(0.008)	(0.008)
Conley S.E. (100 Km.)	[0.016]***	[0.012]***	[0.013]***	[0.013]***
Conley S.E. (200 Km.)	[0.019]***	[0.015]***	[0.015]***	[0.016]***
Excluded-instrument F (robust)	62.8	30.1	27.3	26.4
F (Conley 100 Km.)	13.8	11.0	11.9	11.4
F (Conley 200 Km.)	9.8	7.6	7.9	7.5
Geographic controls	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes
Institutional controls	No	Yes	Yes	Yes
State capacity controls	No	No	Yes	Yes
Hacienda and ethnicity controls	No	No	Yes	Yes
Lasso-selected controls	No	No	No	Yes
Agrarian Reform Zone FE	No	Yes	Yes	Yes
Observations (districts)	1,191	1,191	1,059	1,059
Mean Dep. Var.	0.139	0.139	0.145	0.145
R-squared	0.105	0.270	0.318	0.312

Notes: Identical to Table 4 in the main text, adding the excluded-instrument F implied by each Conley variance, $(\hat{\beta}/\mathrm{se}_{\mathrm{Conley}})^2$. Given the spatial smoothness of the distance instrument, the robust F, which runs from 26.4 to 62.8 across columns, overstates its effective strength. Under Bartlett spatial-HAC the implied F falls to between 11.0 and 13.8 at 100 km and to between 7.5 and 9.8 at 200 km, and in both bandwidths the highest value is column (1), the specification without Agrarian Reform Zone fixed effects. This motivates reporting weak-identification-robust (Anderson–Rubin) inference alongside conventional t-based inference in the IV estimates. *** p<0.01, ** p<0.05, * p<0.1.

Table B2: Placebo IVs and falsification tests

	Pre-reform political outcomes			1980–2022		
	Social movements	NEP 1963	Victory margin 1963	NEP	Victory margin	Turnout
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. 2SLS: land-reform exposure, instrumented by distance to ARZ office						
LR exposure (1 SD)	-0.084	0.121	5.326	0.939***	-6.849***	6.456***
	(0.082)	(0.127)	(7.948)	(0.205)	(1.675)	(1.425)
Panel B. 2SLS: placebo treatment, same instrument						
Uncultivable-land exposure (1 SD)	–	–	–	-6.106	44.531	-44.232
				(10.199)	(74.951)	(77.986)
Panel C. 2SLS: actual treatment, placebo instruments						
Dist. hacienda-dense district (1 SD)	–	–	–	0.167	-0.972	0.316
				(0.172)	(1.485)	(1.218)
Dist. neighboring district capital (1 SD)	–	–	–	0.276	-5.992***	13.018***
				(0.225)	(2.247)	(3.089)
Dist. provincial capital (1 SD)	–	–	–	0.336	-5.230**	15.427***
				(0.243)	(2.421)	(3.932)
Panel D. First stages (*coefficient, clustered s.e., robust F of the excluded instrument*)						
ARZ distance → exposure			-0.196 (0.035) [F = 31.6]			
ARZ distance → uncultivable-land exposure			0.030 (0.050) [F = 0.4]			
Hacienda-dense district dist. → exposure			-0.218 (0.050) [F = 19.3]			
Neighboring district capital dist. → exposure			-0.139 (0.030) [F = 21.0]			
Provincial capital distance → exposure			-0.094 (0.023) [F = 16.8]			
ARZ distance → exposure, central zones			-0.255 (0.042) [F = 37.8]			
ARZ distance → exposure, peripheral zones			0.051 (0.036) [F = 2.0]			
Panel E. Falsification: reduced form of ARZ distance, by region						
Central agrozones	–	–	–	-0.175***	1.602***	-1.645***
				(0.028)	(0.288)	(0.196)
Peripheral agrozones (AZ 8–9)	–	–	–	-0.250***	0.569	0.314
				(0.070)	(0.552)	(0.565)
Full col. (3) controls	Yes	Yes	Yes	Yes	Yes	Yes
Agrarian Reform Zone FE	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	No	No	No	Yes	Yes	Yes
Observations (Panel A)	1,059	999	999	15,085	15,085	11,922

Notes: All treatments and instruments in cross-district SD units; full col. (3) controls, Agrarian Reform Zone FE (election FE in the 1980–2022 columns), SEs clustered by district. *Panel A*: the actual design on pre-reform outcomes — social movements (dropped from the controls in that column; 1980 cross-section) and the NEP and victory margin of the 1963 municipal election — and the post-reform benchmarks of Tables 5–7, col. (3). *Panel B*: placebo treatment = share of district area expropriated as uncultivable (*eriazo*) land, same instrument. *Panel C*: placebo distance instruments with the actual treatment: nearest hacienda-dense district (top decile of the 1961 share of hacienda/fundo settlements); nearest neighboring district capital; nearest provincial capital. *Panel D*: first stages of each design. *Panel E*: reduced form of the actual instrument by region; peripheral = Agrarian Reform Zones 8–9 (Amazon, outside the scope of the 1969 Decree Law), cf. Nunn and Wantchekon (2011), Lowes and Montero (2021). *** p<0.01, ** p<0.05, * p<0.1.

Note B1. Randomization inference: placebo ARZ offices — procedure

The distance-to-ARZ-office instrument could violate the exclusion restriction if proximity to an office merely proxies access to a significant urban center: offices were located in cities chosen for their agricultural surroundings and market connectivity. Following Dell and Olken (2020), I compare the actual office configuration against placebo configurations located at *equally suitable* sites. If any comparable city generates the same reduced-form patterns, the instrument reflects generic geography; if the actual offices are an outlier among plausible configurations, the effect operates through the reform administration itself.

Feasible placebo sites. A candidate site is a nucleated settlement (1961 CCPP directory) satisfying: (i) population $\geq$ the 10th percentile of the 12 actual office host cities (5,647 inhabitants); (ii) district crop suitability $\geq$ the 10th percentile of the office host districts (GAEZ v4, 1961–90 climate, taking the maximum over the rain-fed low-input and irrigated regimes, so that the criterion binds in both costa and sierra); (iii) district distance to the 1958 road and railway network $\leq$ the 90th percentile of office hosts (31 km); and (iv) located in the same Agrarian Reform Zone as the office it replaces, at least 25 km away from the actual office (anti-contamination buffer, cf. Dell–Olken). The percentile thresholds follow Dell and Olken's convention, whose counterfactual factory sites must be at least as suitable as the 10th percentile of the actual factory distribution: intuitively, a feasible placebo site must be at least as populous, suitable and connected as the least-demanding location that actually received an office, with the percentile (rather than the minimum) guarding against measurement artifacts in the extreme host. Where fewer than three candidates survive in a zone, filters are relaxed in a documented sequence. This yields 68 candidate sites across the 12 zones with offices; zone 13, which had no office, is left without one, as in reality.

Placebo configurations and estimation. A placebo configuration c draws one site per zone; I generate 1,000 configurations under two sampling schemes (uniform and population-weighted, fixed seed). For the actual and each placebo configuration I re-estimate the full column-(3) specification of Tables 5–7,

$$y_{it} \;=\; \beta_c\, z_i^{(c)} \;+\; X_i'\gamma \;+\; \mu_{a(i)} \;+\; \lambda_t \;+\; \varepsilon_{it},$$

where $z_i^{(c)}$ is the standardized distance from district i's centroid to the nearest office of configuration c, X_i collects the full control set, and $\mu_{a(i)}$ and λ_t are Agrarian Reform Zone and election fixed effects. Inference is by randomization: the RI p-value is the rank

of the actual coefficient in the placebo distribution,

$$p_{RI} \;=\; \frac{1 + \#\{\,|\beta_c| \geq |\beta_{\text{actual}}|\,\}}{C+1},$$

and the *purged* estimate $\beta_{\text{actual}} - \bar{\beta}_{\text{placebo}}$ removes any confound common to suitable-city locations. I report the reduced form as the primary object — the placebo IV is an unstable ratio whenever the placebo first stage is near zero — together with the first stage. The actual configuration is computed with the identical (euclidean) pipeline and reproduces the haversine-based col. (3) estimates of Tables 5–7 to the third decimal.

Table B3: Randomization inference: actual offices vs. placebo configurations

	N	Actual coefficient	Placebo mean	Placebo s.d.	Purged estimate	RI p (uniform)	RI p (pop-weight)
Panel A. Reduced forms: outcome on distance to nearest office (1 SD)							
NEP	15,085	-0.184***	-0.029	0.052	-0.155	0.001	0.001
		(0.026)					
Victory margin (pp)	15,085	1.340***	0.649	0.523	0.692	0.101	0.197
		(0.254)					
Turnout (pp)	11,922	-1.265***	-1.294	0.492	0.028	0.543	0.684
		(0.219)					
Turnout — sierra, 1961 Spanish-speaking majority	2,773	-2.972***	-1.696	1.038	-1.276	0.125	0.078
		(0.647)					
Turnout — sierra, 1961 indigenous majority	5,775	-1.347***	-0.876	0.643	-0.471	0.265	0.363
		(0.301)					
Turnout — indig. majority, high 63/66 indig. candidate supply	3,532	-1.978***	-1.105	0.812	-0.873	0.132	0.208
		(0.323)					
Turnout — indig. majority, low 63/66 indig. candidate supply	1,790	-0.419	-1.433	0.838	1.014	0.895	0.876
		(0.890)					
Turnout — above-median illiteracy, 1961	6,201	-1.622***	-1.582	0.582	-0.040	0.491	0.634
		(0.305)					
Turnout — below-median illiteracy, 1961	5,721	-0.776***	-1.081	0.503	0.305	0.724	0.812
		(0.295)					
Turnout — above-median 1933 indig. electorate	5,492	-1.665***	-0.745	0.805	-0.920	0.138	0.235
		(0.336)					
Left vote share (broad, pp), 1980–89	4,030	-2.462***	-0.697	0.982	-1.765	0.034	0.074
		(0.524)					
Left vote share — 1961 indigenous majority	1,995	-2.599***	-0.057	1.201	-2.542	0.026	0.010
		(0.900)					
Panel B. First stage: exposure on distance to nearest office (1 SD)							
Land-reform exposure (1 SD)	15,085	-0.197***	-0.101	0.059	-0.096	0.036	0.042
		(0.035)					

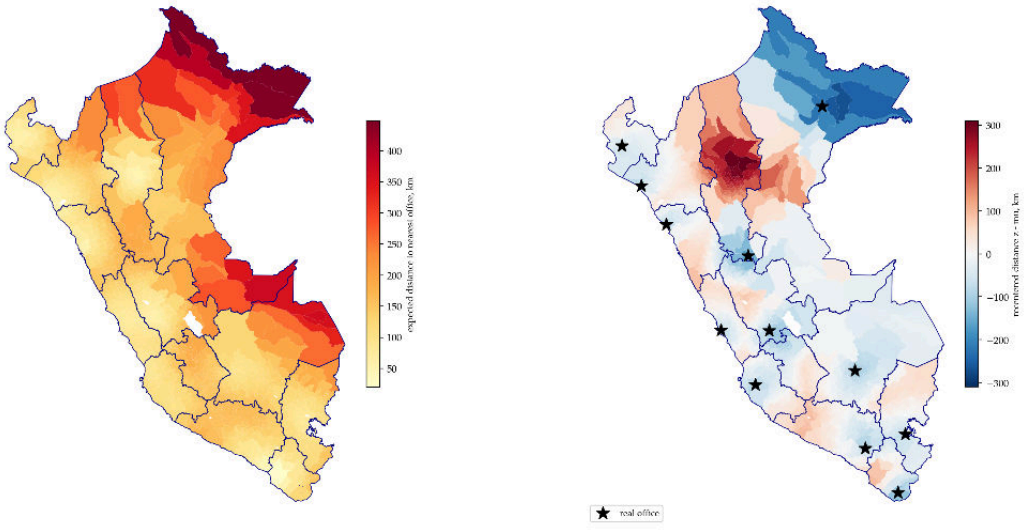
Notes: Each row is the coefficient on the standardized distance to the nearest office in the column-(3) specification (full controls, Agrarian Reform Zone and election FE). *Actual coefficient*: real office configuration, computed with the same euclidean pipeline as the placebos; standard errors clustered at the district level in parentheses, with conventional significance stars. *Placebo mean* and *s.d.*: moments of the 1,000 uniform-scheme placebo-configuration coefficients. *Purged estimate*: actual minus placebo mean — the component attributable specifically to the actual offices, net of what any equally suitable city location generates. *RI p*: share of placebo configurations with an absolute coefficient at least as large as the actual one, under the uniform and the population-weighted sampling schemes. Turnout subsamples: the two sierra rows restrict to districts above 2,000 m elevation and split them by the 1961 mother-tongue majority; the two *candidate-supply* rows split the 1961 indigenous-majority districts at the median share of 1963/66 municipal candidacies with an indigenous surname (Artiles et al. dictionary; pre-reform, so not conditioned on a reform outcome; direction specified before estimation); the two *illiteracy* rows split the full sample at the median 1961 illiteracy share; the 1933 row restricts to districts above the median share of indigenous among registered voters in the 1933 electoral census; the two *left vote* rows are the broad-left vote share (Izquierda Unida and member parties, non-IU socialist parties and FRENATRACA; municipal elections 1980–89 only, in pp), full sample and restricted to 1961 indigenous-majority districts (Figure 8). The first stage is estimated on the NEP sample. Splits are exploratory. The construction of the placebo configurations is described in the preceding section. *** p<0.01, ** p<0.05, * p<0.1 (conventional, clustered).

Note B2. Recentered IV: the expected instrument

The distance instrument is a formula. It combines the realized office locations with each district's predetermined position relative to the network of feasible office sites. Borusyak and Hull (2023) show that formula instruments can be confounded even when the underlying shocks are as good as random. Districts in regions dense with office-suitable cities are mechanically close to any plausible office configuration, and this nonrandom exposure may correlate with unobserved determinants of political outcomes. Their solution is to specify the assignment process of the shocks, here the 1,000 counterfactual office configurations of the randomization-inference exercise, which relocate each office to an equally suitable city of its zone, and to compute each district's expected instrument,

$$\mu_i \;=\; \frac{1}{C}\sum_{c=1}^{C} z_i^{(c)},$$

the average distance to the nearest office across configurations. Controlling for μ_i, or recentering the instrument to $z_i - \mu_i$, removes the exposure component, so that identification relies only on the deviation of realized office locations from their expectation within the feasible sets. Borusyak and Hull (2026) show that recentered formula instruments of this kind are valid and can be more powerful than instruments built from the shocks alone. The reduced-form coefficient on μ_i measures the exposure confound directly, the analog of the expected market-access effects in the high-speed-rail application of Borusyak and Hull (2023). The specification test checks that the recentered instrument is not systematically related to the controls, benchmarked against the counterfactual configurations. The estimates are reported in Table 8 in the main text.



(a) Expected distance μ_i (b) Recentered distance $z_i - \mu_i$

Table B4: Spatial unit-root diagnostics: raw vs. recentered instrument

	H0: spatial unit root (I(1), p-value)	H0: stationarity (I(0), p-value)	Spatial half-life (95% CI, km)
Raw instrument z_i	0.162	0.216	$[81, \infty)$
Recentered instrument $z_i - \mu_i$	0.014	0.536	[2, 613]

Notes: Müller and Watson (2024) diagnostics computed with the `spur` suite of Becker, Boll and Voth (2026), on the district-level cross-section (1,570 districts, latitude/longitude coordinates; 20,000 Monte Carlo draws per test). Columns report the p-values of the tests of the null of a spatial unit root (I(1)) and of stationarity (I(0)), and the 95% confidence set for the spatial half-life — the distance at which the spatial correlation of the process falls to one half. A spatial unit root cannot be rejected for the raw distance instrument, whose half-life confidence set is unbounded; it is rejected for the recentered instrument $z_i - \mu_i$ of Table 8, which is consistent with stationarity and has a bounded half-life. Under a spatial unit root, cluster and spatial-HAC corrections do not protect against spurious regression (Müller and Watson 2024); the recentered design therefore addresses this concern by construction.

Table B5: IV: Land-reform exposure and other measures of political competition (1980–2022)

	Num. Parties	Herfindahl Index	Top-two Concentr.	Hyperfrag. Index	Volatility
	(1)	(2)	(3)	(4)	(5)
Panel A. Reduced form					
Distance to nearest ARZ office (1 SD)	-0.490***	0.019***	0.020***	-0.256***	0.013***
	(0.049)	(0.002)	(0.003)	(0.032)	(0.002)
Panel B. Second stage (2SLS)					
Land reform exposure (1 SD)	2.511***	-0.095***	-0.101***	1.312***	-0.064***
Clustered S.E. (district)	(0.495)	(0.020)	(0.022)	(0.276)	(0.016)
Anderson–Rubin 95% CI	[1.754, 3.936]	[-0.151, -0.064]	[-0.161, -0.067]	[0.882, 2.090]	[-0.107, -0.037]
Anderson–Rubin p-value	<0.001	<0.001	<0.001	<0.001	<0.001
VtF 95% CI	[1.358, 3.665]	[-0.141, -0.049]	[-0.151, -0.050]	[0.670, 1.953]	[-0.102, -0.026]
Panel C. First stage					
Distance to nearest ARZ office (1 SD)	-0.195***	-0.195***	-0.196***	-0.195***	-0.198***
	(0.035)	(0.035)	(0.035)	(0.035)	(0.035)
Effective first-stage F	31.5	31.5	31.6	31.5	31.7
Geographic controls	Yes	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes	Yes
State capacity controls	Yes	Yes	Yes	Yes	Yes
Hacienda and ethnicity controls	Yes	Yes	Yes	Yes	Yes
Agrarian Reform Zone FE	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes
Observations	15,184	15,184	15,085	15,184	14,668
Clusters	1,235	1,235	1,235	1,235	1,235
Mean Dep. Var.	5.473	0.343	0.706	4.107	0.785

Notes: IV estimates for five alternative measures of district-level political competition, all computed from the same raw-vote panel and aggregation rule as the main outcomes (elector-weighted across the unit's district contests within 1975 boundaries; provincial capitals excluded): number of parties with votes; Herfindahl concentration of vote shares; joint vote share of the top two parties; hyperfractionalization index $\exp(-\sum_j s_j \ln s_j)$; and total volatility relative to the previous election (undefined in 1980, hence the smaller sample). Single specification, equal to col (3) of Tables 5–7: geographic, agriculture and labor force, institutional, state capacity and hacienda/ethnicity controls, with Agrarian Reform Zone and election FE; standard errors clustered at the district level. Panels, estimators and the Anderson–Rubin and VtF rows are as in Tables 5–7. Note that the hyperfractionalization index is mechanically close to the effective number of parties (correlation 0.98 across contests), so col (4) is essentially a rescaled version of Table 5. *** p<0.01, ** p<0.05, * p<0.1.

Regression Discontinuity Design

Table B6: RDD sample definition: eligible zones, exclusions, and districts by distance to the core/periphery boundary

				Mean reform intensity		
	Districts	Core	Periphery	Core	Periphery	Gap (pp)
Panel A: sample definition (district cross-section, 1975 boundaries)						
All reform-era districts	1,570					
excluded: outside eligible zones (8–9, 10/13, 12)	−551					
excluded: urban districts (1972 population $\geq$ 75,000)	−17					
Panel B: eligible sample, by distance to the core/periphery boundary						
Eligible sample (all distances)	1,002	519	483	0.155	0.137	+1.9
|distance| $\leq$ 75 km	560	266	294	0.212	0.123	+8.9
|distance| $\leq$ 50 km	417	198	219	0.217	0.119	+9.8
|distance| $\leq$ 25 km	215	96	119	0.192	0.119	+7.3

Notes: District cross-section on 1975 boundaries (the 1,570 reform-era units, one observation per district). Every variable in the table (zone eligibility, the urban exclusion, the distance to the boundary and reform intensity) is time-invariant, so the accounting describes the design sample for any election year (1963–2022). Estimation samples per election, which can be smaller where electoral data are missing, are reported in the corresponding results tables. The running variable is the signed distance (km) from the district to the core/periphery boundary of its Agrarian Reform Zone (positive = core side), inherited from the replication materials of Albertus (2020). Panel A: eligibility follows three criteria. First, the 1969 reform law (D.L. 17716) applied, which excludes the Amazonian zones 8–9, where reform consisted of colonization of public lands. Second, zone boundaries did not change during the reform, which excludes zone 10/13 (split in 1974, with core/periphery status shifting). Third, the zone has both a core and a periphery, which excludes Puno's zone 12, whose zone boundary coincides with the department boundary. This leaves zones 1–7 and 11. The urban exclusion drops districts with 1972 population of 75,000 or more, and the eligibility mirrors the agro-zone falsification of the IV design. No eligible district is missing the running variable or the treatment. Panel B: reform intensity is the share of district area adjudicated in 1969–80. Means are raw (no controls), so the Gap column is the crude dose contrast at each bandwidth, not an estimate. The contrast is a boundary phenomenon: it is +1.9pp over the full eligible sample and rises to +7–10pp within 25–75 km of the cutoff, which is what the local-linear first stage formalizes.

Table B7: First stage: local-linear estimates of the dose discontinuity at the core/periphery boundary

	(1)	(2)	(3)	(4)
Panel A: exposure = expropriated area in use / district area (`intRA_Atot`, the paper's measure)				
RD estimate (conventional)	7.65	7.84**	1.72	7.29**
	(5.36)	(3.20)	(3.55)	(3.47)
Robust bias-corrected	6.53	9.00***	-7.91	7.10*
	[5.93]	[3.44]	[9.62]	[3.78]
Robust 95% CI	[-5.1, 18.1]	[2.3, 15.7]	[-26.8, 10.9]	[-0.3, 14.5]
Effective F (conv. / robust)	2.0 / 1.2	6.0 / 6.9	0.2 / 0.7	4.4 / 3.5
Bandwidth left / right (km)	59.6 / 72.4	41.5 / 51.8	25.0 / 25.0	50.0 / 50.0
Eff. N left / right	251 / 258	188 / 207	119 / 96	219 / 198
Panel B: robustness — redistributed share of district area (Albertus 2019's treatment)				
RD estimate (conventional)	5.43	9.85**	5.07	7.03
	(6.12)	(4.05)	(3.61)	(4.73)
Robust bias-corrected	3.62	11.55***	-5.80	10.08**
	[6.60]	[4.46]	[12.38]	[5.09]
Robust 95% CI	[-9.3, 16.6]	[2.8, 20.3]	[-30.1, 18.5]	[0.1, 20.1]
Effective F (conv. / robust)	0.8 / 0.3	5.9 / 6.7	2.0 / 0.2	2.2 / 3.9
Bandwidth left / right (km)	56.0 / 49.3	33.8 / 44.9	25.0 / 25.0	50.0 / 50.0
Eff. N left / right	234 / 198	157 / 177	119 / 96	219 / 198
Bandwidth selection	MSE-two	MSE-two	25 fixed	50 fixed
Segment covariates	No	Yes	Yes	Yes

Notes: Local-linear estimates (triangular kernel) of the jump in land-reform exposure (pp of district area) at the core/periphery boundary, on the design sample of Table B6. Panel A uses the paper's exposure measure, so the fuzzy-RD scaling is in the same units as the IV and DiD designs. Panel B uses the redistributed share of Albertus (2020), from whose replication the running variable is inherited (correlation between the two measures: 0.80). Cols (1)–(2): MSE-optimal bandwidths per side. Cols (3)–(4): fixed bandwidths of 25 and 50 km. Cols (2)–(4) add the boundary controls of Albertus (2020) (coordinate polynomials and boundary-segment fixed effects). Nearest-neighbor clustering by department. Segment-side clustering and wild cluster bootstrap are in Table B10. Stars attach to each row's own p-value, conventional in parentheses and robust bias-corrected in brackets. Both sides of the cutoff are treated. The discontinuity is an intensive-margin jump of roughly +8–9pp of exposure (about 0.4 standard deviations), not a treated-vs-untreated contrast. The effective F is at most 6.9, so downstream boundary effects are reported as reduced-form ITT and fuzzy LATE rescalings carry a weak-identification caveat. At 25 km the covariate-rich specification becomes unstable (col 3). *** p<0.01, ** p<0.05, * p<0.1.

Table B8: Electoral competition and turnout at the core/periphery boundary: reduced-form estimates, pooled 1980–2022

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: effective number of parties, pooled 1980–2022						
RD ITT (conventional)	0.415*	0.431***	0.559***	0.426**	0.329**	0.372***
	(0.213)	(0.156)	(0.210)	(0.173)	(0.161)	(0.121)
Robust bias-corrected	0.320	0.441***	0.540	0.548**	0.333**	0.358***
	[0.238]	[0.163]	[0.332]	[0.222]	[0.167]	[0.136]
Fuzzy LATE (per pp of exposure)		0.058** (0.024)				
Mean dep. var. (periphery, 50 km)			3.23			
Bandwidth left / right (km)	51 / 57	40 / 61	25 / 25	50 / 50	33 / 54	39 / 38
Eff. N left / right	198 / 192	165 / 199	106 / 86	195 / 174	121 / 169	110 / 113
Panel B: victory margin (pp), pooled 1980–2022						
RD ITT (conventional)	1.318	1.456	2.100	1.420	2.196*	1.223
	(2.336)	(1.044)	(1.585)	(1.057)	(1.158)	(1.077)
Robust bias-corrected	1.882	1.163	4.451	1.603	1.989	0.902
	[2.603]	[1.160]	[4.198]	[1.739]	[1.281]	[1.187]
Fuzzy LATE (per pp of exposure)		0.218 (0.214)				
Mean dep. var. (periphery, 50 km)			18.86			
Bandwidth left / right (km)	73 / 60	58 / 54	25 / 25	50 / 50	57 / 72	53 / 42
Eff. N left / right	254 / 198	217 / 184	106 / 86	195 / 174	182 / 206	141 / 126
Panel C: turnout (%), pooled 1980–2022						
RD ITT (conventional)	-2.084	-1.490*	-1.769*	-1.601**	-2.615***	-1.294
	(1.877)	(0.798)	(0.910)	(0.766)	(0.782)	(0.911)
Robust bias-corrected	-3.234*	-2.005**	-4.240*	-1.860	-3.301***	-1.823
	[1.786]	[0.998]	[2.544]	[1.137]	[1.030]	[1.146]
Fuzzy LATE (per pp of exposure)		-0.263 (0.192)				
Mean dep. var. (periphery, 50 km)			74.15			
Bandwidth left / right (km)	52 / 51	50 / 38	25 / 25	50 / 50	47 / 36	35 / 39
Eff. N left / right	200 / 177	198 / 137	106 / 86	195 / 174	165 / 117	98 / 117
Bandwidth selection	MSE-two	MSE-two	25 fixed	50 fixed	MSE-two	MSE-two
Segment covariates	No	Yes	Yes	Yes	Yes	Yes
Slope + hacienda controls	No	No	No	No	Yes	Yes
Pre-outcome covariate (NEP 63/66)	No	No	No	No	No	Yes

Notes: Local-linear RD estimates on the design sample (891 of the 1,002 eligible districts have a post-reform electoral outcome). The dependent variables are the district means over all post-reform municipal elections, 1980–2022, the same window as the IV tables (Tables 5–7): 13 elections for NEP (Panel A) and the victory margin (Panel B), and the 10 elections with a voter registry for turnout (Panel C; no registry in 1986, 1989, 1993). Estimates are reduced-form ITT. Given the weak boundary first stage (effective F of at most 6.9), the ITT is the reliably identified object. The Fuzzy LATE row rescales spec (2) by the dose jump (per pp of exposure) and should be read with the weak-identification caveat. Specs: (1) no covariates, MSE-two bandwidths; (2) adds the boundary-segment covariates of the first stage; (3)–(4) common fixed bandwidths of 25/50 km; (5) adds slope and hacienda controls (the two balance imbalances, plus hacienda counts and private land area); (6) adds the pre-reform outcome (mean NEP 1963/66) as a precision covariate. Nearest-neighbor clustering by department. Segment-side clustering and wild cluster bootstrap are in Table B10. Stars follow each row's own p-value. A donut check excluding districts within 5 km of the boundary leaves the NEP effect intact ($+0.42/+0.43$, robust $p \leq 0.014$), with margin and turnout not significant as here. *** p<0.01, ** p<0.05, * p<0.1.

Table B9: Heterogeneity of the boundary effects: 1961 language, pre-reform indigenous candidate supply, and the 1933 indigenous electorate, pooled 1980–2022

	1961 mother tongue		Indig. maj.: 63/66 cand. supply		1933 indig. electorate	
	Indigenous majority	Spanish-speaking majority	High supply	Low supply	Above median	Below median
Panel A: effective number of parties, pooled 1980–2022						
RD ITT (MSE-two)	-0.028	0.564***	-0.034	0.752***	0.683***	0.089
	(0.169)	(0.177)	(0.153)	(0.224)	(0.249)	(0.172)
Robust bias-corrected	-0.014	0.626***	0.006	1.013***	0.312	0.066
	[0.199]	[0.208]	[0.181]	[0.259]	[0.462]	[0.212]
RD ITT (h = 50 km)	-0.087	0.556***	0.049	0.162	0.529*	0.157
	(0.091)	(0.153)	(0.152)	(0.177)	(0.278)	(0.188)
Mean dep. var. (periphery, 50 km)	3.22	3.26	3.23	3.16	3.16	3.26
Bandwidth left / right (km)	27 / 36	36 / 63	50 / 24	24 / 32	38 / 33	23 / 32
Eff. N left / right	73 / 55	50 / 108	44 / 18	22 / 19	73 / 46	42 / 67
Panel B: victory margin (pp), pooled 1980–2022						
RD ITT (MSE-two)	7.864**	0.550	2.014	1.149	1.367	2.464
	(3.081)	(1.371)	(3.035)	(3.950)	(1.613)	(2.404)
Robust bias-corrected	8.550**	-0.732	2.768	-0.199	0.819	2.100
	[3.498]	[1.778]	[3.478]	[4.544]	[2.486]	[2.885]
RD ITT (h = 50 km)	6.522***	0.279	0.156	7.653***	1.571	2.798
	(1.939)	(1.379)	(2.523)	(2.634)	(1.702)	(2.317)
Mean dep. var. (periphery, 50 km)	19.94	17.04	20.35	21.09	20.36	18.15
Bandwidth left / right (km)	31 / 28	55 / 40	32 / 39	24 / 22	52 / 44	38 / 46
Eff. N left / right	81 / 37	77 / 84	23 / 33	22 / 13	83 / 61	71 / 96
Panel C: turnout (%), pooled 1980–2022						
RD ITT (MSE-two)	-4.808***	-1.346	-1.837	-7.552***	-3.240**	-0.885
	(1.434)	(1.049)	(1.267)	(2.438)	(1.295)	(1.301)
Robust bias-corrected	-4.803***	-1.047	-1.326	-9.371**	-2.037	-1.277
	[1.711]	[1.239]	[1.271]	[3.707]	[1.869]	[1.673]
RD ITT (h = 50 km)	-3.689***	-1.414	-1.160	-1.719	-3.223**	-1.027
	(0.846)	(0.999)	(1.030)	(1.398)	(1.453)	(1.032)
Mean dep. var. (periphery, 50 km)	73.03	76.02	71.82	72.02	73.89	74.35
Bandwidth left / right (km)	23 / 20	40 / 33	23 / 21	24 / 18	47 / 33	36 / 31
Eff. N left / right	62 / 26	59 / 66	20 / 17	22 / 11	78 / 42	69 / 65

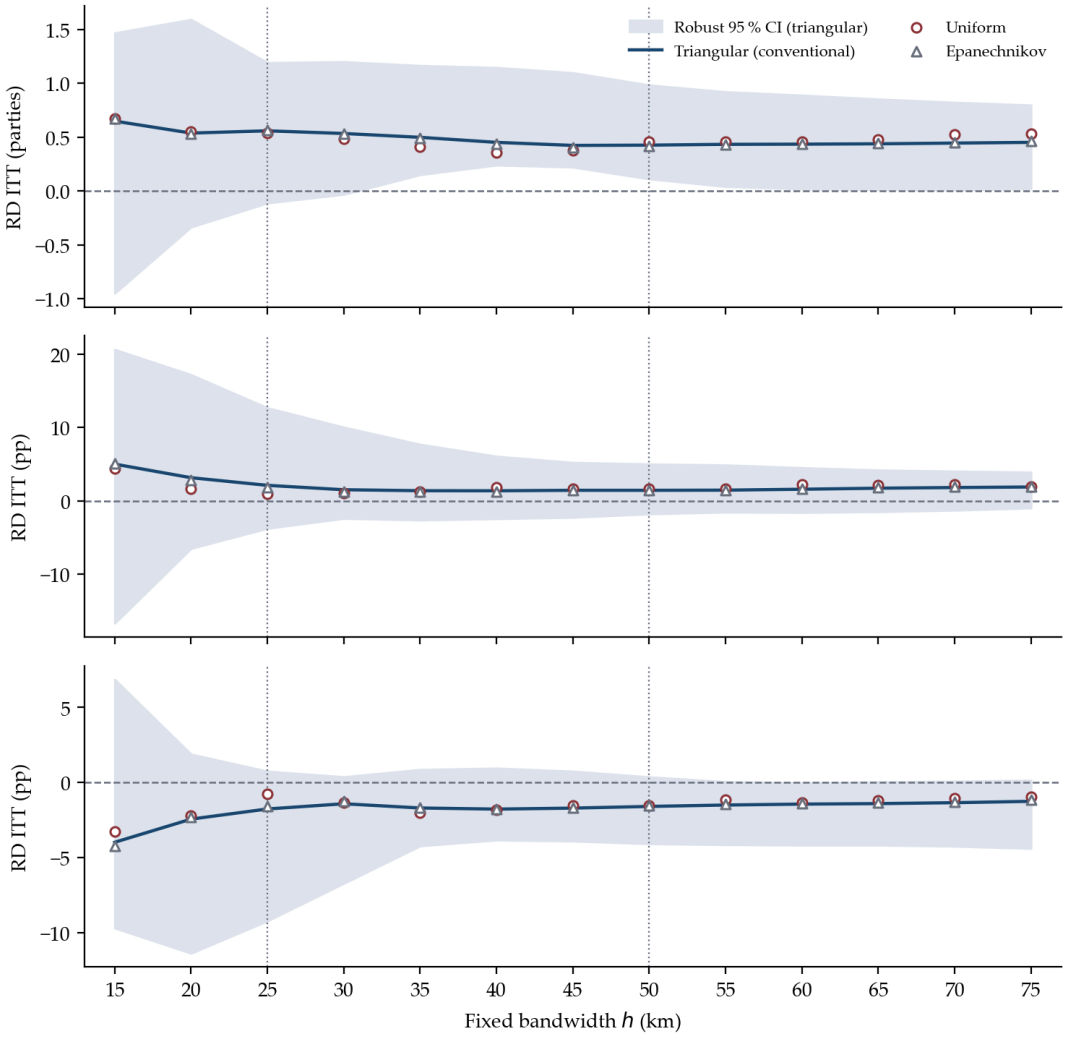
Notes: Subsample local-linear RD estimates of the pooled 1980–2022 outcomes, the window of Table B8. Splits: (i) 1961 mother-tongue majority (indigenous share above 0.5); (ii) within indigenous-majority districts, pre-reform supply of indigenous candidates, defined as the median share of 1963/66 municipal candidacies with an indigenous surname (Artiles et al. dictionary). The measure predates the reform, so it is not conditioned on a reform outcome. (iii) 1933 indigenous electorate (median share of indigenous among registered voters in the 1933 electoral census). All cells use the boundary-segment covariates, MSE-two and 50 km bandwidths, and nearest-neighbor clustering by department. Stars follow each row's own p-value. The candidate-supply cells are thin (11 to 46 effective districts, with the bandwidth rows disagreeing), so they convey direction rather than magnitude. Splits are exploratory. *** p<0.01, ** p<0.05, * p<0.1.

Table B10: RDD inference under few clusters: segment-side clustering and wild cluster bootstrap

	RD ITT (SE)	p (cluster-t, seg.×side)	p (wild, seg.×side)	p (wild, department)	Eff. N
Panel A: $h = 25$ km					
Reform dose (first stage, share of area)	0.073 (0.040)	0.084	0.111	0.181	215
NEP, pooled 1980–2022	0.538 (0.240)	0.039	0.121	0.117	192
Victory margin (pp), pooled 1980–2022	0.946 (1.303)	0.478	0.472	0.561	192
Turnout (%), pooled 1980–2022	-0.745 (1.066)	0.495	0.538	0.589	192
Turnout, 1980–89	-3.496 (3.793)	0.371	0.355	0.367	159
NEP, 1993–98	0.729 (0.291)	0.023	0.072	0.072	192
NEP, 2002–10	1.055 (0.381)	0.014	0.021	0.016	192
Victory margin, 1993–98	-6.891 (2.241)	0.007	0.004	0.020	192
Panel B: $h = 50$ km					
Reform dose (first stage, share of area)	0.057 (0.036)	0.137	0.164	0.355	417
NEP, pooled 1980–2022	0.459 (0.188)	0.026	0.088	0.069	369
Victory margin (pp), pooled 1980–2022	1.620 (1.572)	0.317	0.391	0.284	369
Turnout (%), pooled 1980–2022	-1.540 (0.904)	0.107	0.138	0.137	369
Turnout, 1980–89	-4.224 (2.140)	0.066	0.119	0.103	322
NEP, 1993–98	0.674 (0.247)	0.014	0.057	0.097	369
NEP, 2002–10	0.960 (0.273)	0.003	0.014	0.018	369
Victory margin, 1993–98	-2.310 (1.763)	0.208	0.270	0.269	369

Notes: The boundary estimates cluster by department (about 18 clusters), few enough that cluster-robust asymptotics can overstate precision. This table re-estimates each headline boundary result with the most conservative feasible inference. Design: parametric local-linear analog of the RD. Within $|\text{dist}| \leq h$, the outcome is regressed on a core indicator, the distance trend interacted with side, the boundary-segment covariates and boundary-segment fixed effects. Standard errors are clustered by boundary segment × side (16–18 clusters), and the last two columns report wild cluster bootstrap p-values (Webb weights, 9,999 replications; Roodman et al. 2019) under segment-side and department clustering. Outcomes are the pooled 1980–2022 series and the era cells that carry the paper's reading. The weakening of the first-stage dose jump under wild inference reinforces the choice to communicate boundary effects as reduced-form ITT rather than fuzzy LATE.

Figure B2: RDD bandwidth and kernel sensitivity of the pooled boundary effects, 1980–2022



Notes: Each panel sweeps the pooled 1980–2022 RD ITT over fixed bandwidths of 15–75 km in 5 km steps. The solid line and shaded band are the conventional estimate and the robust bias-corrected 95% CI with the triangular kernel, the baseline throughout this section. Hollow circles and triangles are conventional estimates with uniform and Epanechnikov kernels. Dotted vertical lines mark $h = 25$ and $h = 50$, the fixed-bandwidth specifications. All cells use the boundary-segment covariates and nearest-neighbor clustering by department.

Table B11: RDD compound treatments: departmental-level electoral history at the core/periphery boundary

	RD (MSE-two)	RD ($h = 50$)	Mean periphery	Eff. N
Presidential turnout 1963 (%, departmental)	2.54***	2.44***	92.5	134 / 156
	(0.45)	(0.42)		
Presidential turnout 1980 (%, departmental)	5.18***	5.05***	78.3	188 / 156
	(0.75)	(0.75)		
Electoral registration 1933 (% of pop., district)	0.08	-0.48	6.6	104 / 101
	(0.57)	(0.64)		
Electoral registration 1933 (% of men, district)	-0.77	-1.75	13.8	110 / 114
	(1.17)	(1.44)		

Notes: The core/periphery boundary coincides for 98.2% of its length with 1975 departmental limits, so boundary estimates could in principle bundle the reform discontinuity with other department-level differences (Keele–Titiunik compound treatments). This table probes that margin with the electoral history available at each level. Rows 1–2: presidential turnout in 1963 and 1980, available only at the departmental level (each district inherits its department's value). Inference caveat: these variables take only about 18–24 distinct values, so the clustered p-values overstate precision and the point estimates are the informative object. Rows 3–4: electoral registration in 1933 (share of population and of adult men registered, from the 1933 electoral census matched at the district level). All cells: local-linear RD with boundary-segment covariates, MSE-two and 50 km bandwidths, nearest-neighbor clustering by department. Stars follow each row's own p-value. *** p<0.01, ** p<0.05, * p<0.1.

Table B12: RDD placebo cutoffs: the pooled boundary effects at shifted thresholds

	NEP	Victory margin (pp)	Turnout (%)
Placebo cutoff at -20 km (periphery side only)			
Local linear, MSE-two	0.169	2.469***	-1.192
	(0.127)	(0.910)	(1.354)
Local linear, $h = 25$	0.066	3.069***	-1.389
	(0.120)	(1.021)	(1.084)
Local quadratic, MSE-two	0.103	2.944*	-1.238
	(0.229)	(1.530)	(2.550)
Placebo cutoff at -10 km (periphery side only)			
Local linear, MSE-two	-0.295***	2.387*	0.683
	(0.103)	(1.451)	(1.288)
Local linear, $h = 25$	-0.272***	1.098	1.141
	(0.103)	(1.689)	(1.206)
Local quadratic, MSE-two	-0.292***	1.088	1.532
	(0.098)	(1.210)	(1.784)
Placebo cutoff at +10 km (core side only)			
Local linear, MSE-two	-0.110	-2.504	0.041
	(0.237)	(2.167)	(1.349)
Local linear, $h = 25$	-0.237	-2.054	1.147
	(0.250)	(2.428)	(1.267)
Local quadratic, MSE-two	0.003	-0.184	-0.548
	(0.306)	(3.069)	(1.495)
Placebo cutoff at +20 km (core side only)			
Local linear, MSE-two	0.010	0.462	-2.365***
	(0.210)	(1.474)	(0.744)
Local linear, $h = 25$	-0.044	0.651	-3.102***
	(0.199)	(1.461)	(0.733)
Local quadratic, MSE-two	-0.170	0.119	-3.170***
	(0.270)	(1.894)	(0.895)

Notes: Each panel re-estimates the pooled 1980–2022 RD ITT at a placebo threshold shifted 10 or 20 km into one side of the real boundary, using only districts from that side, so that no cell crosses the true cutoff. Boundary-segment covariates and nearest-neighbor department clustering throughout. Stars follow each cell's own conventional p-value. Three specifications per cutoff: local linear with MSE-two and fixed 25 km bandwidths, and local quadratic with MSE-two bandwidths. The quadratic is the diagnostic row, since a spurious jump produced by curvature that the local-linear fit misses should vanish once the polynomial can bend. The ± 10 km cells are mechanically fragile, as MSE bandwidths truncate at the real boundary with as few as 38–48 effective districts. Shifted cutoffs are not pure placebos in a continuous-dose design, because reform intensity varies within each side of the boundary. *** p<0.01, ** p<0.05, * p<0.1.

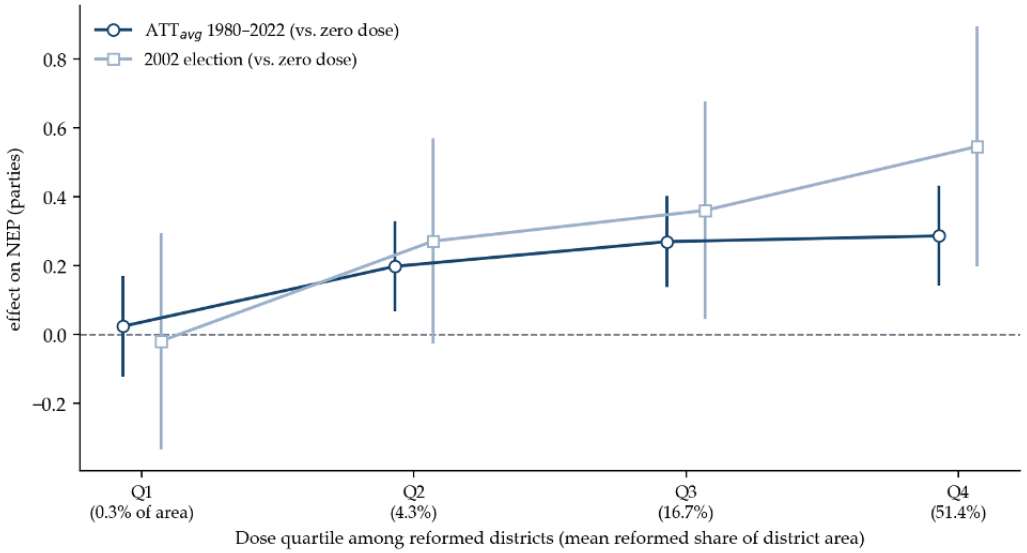
Differences in Differences

Table B13: Land-reform dose and electoral competition: event-study estimates with a 1966 base period

	(1)	(2)	(3)	(4)
Panel A: effective number of parties				
ATT_{avg}, 1980–2022	0.103***	0.102***	0.075***	0.071***
	(0.023)	(0.024)	(0.026)	(0.024)
SE clustered by department			(0.032)	
Wild bootstrap p (department)			0.042	
Peak election (2002)	0.240***	0.241***	0.166***	0.161***
	(0.056)	(0.059)	(0.057)	(0.054)
Pre-reform placebo (1963)	-0.024	-0.024	-0.029	-0.031*
	(0.019)	(0.018)	(0.019)	(0.019)
Mean dep. var. (1980–2022)	3.604	3.600	3.600	3.600
Panel B: victory margin (pp)				
ATT_{avg}, 1980–2022	-1.256*	-1.196*	0.045	0.026
	(0.668)	(0.694)	(0.740)	(0.710)
SE clustered by department			(1.054)	
Wild bootstrap p (department)			0.970	
Peak election (2002)	-1.046	-0.911	0.286	0.255
	(0.681)	(0.707)	(0.742)	(0.711)
Pre-reform placebo (1963)	1.617*	1.687*	1.986*	2.054*
	(0.978)	(0.984)	(1.096)	(1.054)
Mean dep. var. (1980–2022)	17.634	17.794	17.794	17.794
Baseline covariates × election	No	No	Yes	Yes
Fixed effects	District	District	District	ARZ
Sample	All	Covariate	Covariate	Covariate
Observations	19,171	16,689	16,689	16,689
Districts	1,384	1,194	1,194	1,194

Notes: District × election panel of the 15 municipal elections 1963–2022 (non-capital districts). Each column estimates $y_{it} = \sum_{e \neq 1966} \beta_e (D_i \times \mathbf{1}[t = e]) + \lambda_t + \eta_i + \varepsilon_{it}$, where D_i is standardized land-reform exposure (1 SD = 21.1pp of district area) and 1966 is the omitted base period. The table reports the average of the 13 post-reform coefficients (ATT_{avg}), the peak election, and the free pre-reform coefficient (1963) as a placebo. Columns (1)–(2) include election and district fixed effects only, on the full and the covariate-complete samples. Column (3) is the headline specification, adding the paper's baseline covariate set (geography, 1961 census, institutions, state capacity, hacienda structure) interacted with election dummies, so each predetermined characteristic follows its own free time path, the flexible form of conditional parallel trends. No time-varying controls are used by design, since every candidate control is measured after 1969 and is therefore a potential outcome of the reform. Column (4) replaces district with Agrarian Reform Zone fixed effects, including the dose and covariate levels directly. Standard errors clustered by district. For the headline column the table also reports the department-clustered standard error and a wild cluster bootstrap p-value (Webb weights, 9,999 replications, 24 departments). A doubly robust binary version comparing the top dose tercile (mean 44% of district area) to low-dose districts on long differences from 1966 tracks the continuous-dose estimates once rescaled by the 2.1 SD dose contrast between the groups, with three to ten times the standard errors. The NEP estimates are also robust to allowing a free time path by pre-reform political demand for the reform: adding the district's 1963/66 vote share for the reform-promising bloc (Acción Popular–DC against the APRA–UNO coalition) interacted with election dummies moves the ATT_{avg} from 0.074 to 0.069 and leaves the 2002 peak at 0.151 ($p = 0.011$). *** p<0.01, ** p<0.05, * p<0.1.

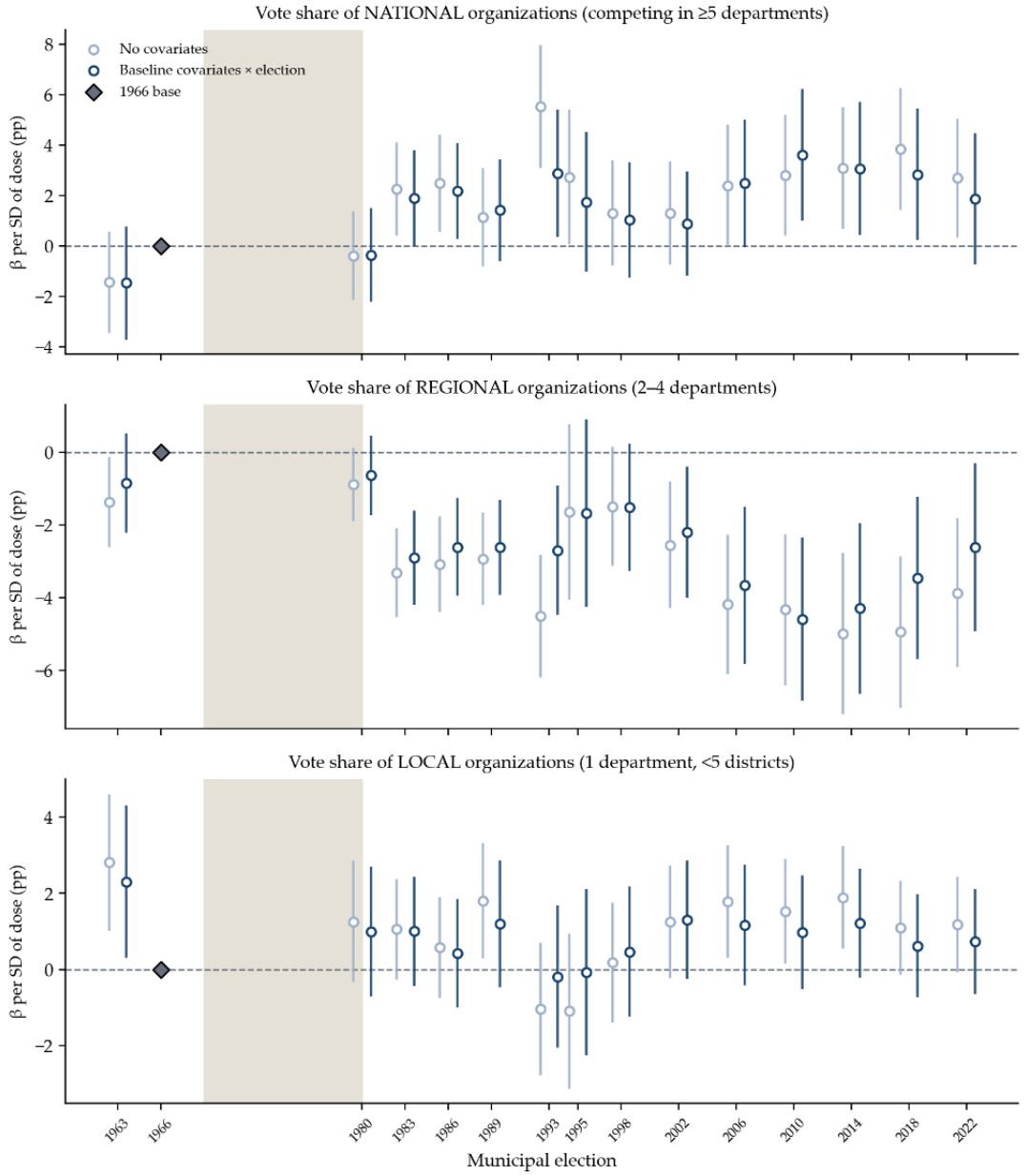
Table B14: The dose-response of the effect on the number of effective parties: event-study estimates by dose quartile



	Q1 (0.3%)	Q2 (4.3%)	Q3 (16.7%)	Q4 (51.4%)
ATT$_{avg}$ vs. zero dose	0.024	0.198***	0.269***	0.286***
	(0.074)	(0.067)	(0.068)	(0.074)
2002 election	-0.019	0.271*	0.360**	0.545***
	(0.160)	(0.152)	(0.161)	(0.177)
ATT$_{avg}$ vs. Q1 (no zero-dose group)	—	0.174**	0.246***	0.262***
		(0.073)	(0.078)	(0.083)
Pre-reform placebo (1963)	-0.087	-0.010	-0.095*	-0.118*
	(0.061)	(0.058)	(0.055)	(0.061)
Districts	246	246	246	245

Notes: With a continuous treatment, the single TWFE dose coefficient identifies a proper average of marginal effects only under strong parallel trends: the counterfactual path must be identical regardless of the dose actually received (Callaway, Goodman-Bacon and Sant'Anna 2024). That assumption is suspect here, since Table 10 shows dose was assigned where pre-reform hacienda settlement was denser. This exhibit therefore estimates the dose-response function directly. Each column is a dose group: districts with exactly zero reformed area are the reference group (402 districts), and reformed districts are split at the quartiles of the positive dose distribution (group means 0.3, 4.3, 16.7 and 51.4 percent of district area). The figure and table report, from a single event-study regression with the headline specification (group × election interactions, 1966 base, district and election fixed effects, baseline covariates × election, SEs clustered by district): the average of each group's 13 post-reform coefficients and its 2002 coefficient, both relative to zero-dose districts, the average effect relative to Q1 (which never uses the zero-dose group), and each group's 1963 placebo. These are level effects identified under ordinary parallel trends, not the strong version. One flagged cell: the Q4 placebo is −0.12 (t of about 1.9), with the other three placebos at zero. *** p<0.01, ** p<0.05, * p<0.1.

Figure B3: Party breadth election by election: national, regional and local vote shares



Notes: Event-study coefficients from the specification of Table B15 for the three breadth outcomes, all 15 municipal elections, with 1966 normalized to zero (diamond). Shaded band, the reform period. Whiskers, 95% confidence intervals with district-clustered standard errors. Light markers, no covariates. Dark markers, baseline covariates × election.

Table B15: Land-reform dose and the territorial reach of the vote: national, regional and local organizations, 1963–2022

	National (≥5 depts.)		Regional (2–4 depts.)		Local (1 dept., <5 distr.)	
	(1)	(2)	(3)	(4)	(5)	(6)
ATT_{avg}, 1980–2022	2.407***	1.973**	-3.290***	-2.731***	0.883	0.758
	(0.927)	(0.985)	(0.686)	(0.748)	(0.640)	(0.693)
ATT, 1980–89		1.290		-2.193***		0.903
		(0.890)		(0.585)		(0.717)
ATT, 1993–98		1.895*		-1.964**		0.068
		(1.120)		(0.883)		(0.804)
ATT, 2002–10		2.335**		-3.486***		1.150
		(1.116)		(0.925)		(0.747)
ATT, 2014–22		2.601**		-3.460***		0.860
		(1.193)		(1.013)		(0.688)
Pre-reform placebo (1963)	-1.437	-1.456	-1.378**	-0.845	2.815***	2.301**
	(1.031)	(1.147)	(0.636)	(0.701)	(0.917)	(1.022)
Wild bootstrap p (department)		0.361		0.190		0.343
Mean dep. var. (post-1980)	59.7		32.1		8.2	
Baseline covariates × election	No	Yes	No	Yes	No	Yes
District and election FE	Yes					
Observations	14,469					

Notes: Every party in every election, 1963–2022, is classified by its revealed territorial footprint in that election: national (running in five or more departments), regional (two to four departments, or five or more districts within one), or local (a single department, fewer than five districts). Event-study estimates with the specification of Table B13 (standardized dose × election, 1966 base, district and election fixed effects, and, in even columns, baseline covariates × election, with SEs clustered by district). Outcomes: the share of valid votes won by organizations of each class, built from the JNE results archive for 1963–2002 and the modern district files for 2006–2022. Two construction safeguards: generic numbered lists ("LISTA N° 5"), which are distinct district-level organizations sharing a name, are keyed by name × district so they cannot masquerade as one national organization, and the splice across sources is validated exactly (the 2002 national share is 63.7% in both). Against the official registry type available for 2002–2022, the rule classifies 82–100% of registered-party votes as national, 87–99.5% of regional-movement votes as regional, and about 100% of local-organization votes as local. The rule measures the municipal footprint of the vehicle in that election, not its national brand (for example Cambio 90–Nueva Mayoría in 1995, whose municipal candidacies ran in fewer than five departments, classifies as regional). The local share shows a +2.3pp pre-reform placebo, so that column should be read as descriptive. *** p<0.01, ** p<0.05, * p<0.1.

Table B16: Land-reform dose and votes cast per capita, 1963–2022

	Votes cast per capita (pp)		Winsor. 100	ln(votes cast)
	(1)	(2)	(3)	(4)
ATT_{avg}, 1980–2022	2.060***	2.222***	2.285***	0.078***
	(0.542)	(0.527)	(0.501)	(0.024)
ATT, 1980–89		2.426***	2.411***	0.083***
		(0.665)	(0.662)	(0.023)
ATT, 2002–10		2.561***	2.568***	0.070***
		(0.632)	(0.624)	(0.027)
Pre-reform placebo (1963)	-0.196	-0.343	-0.333	-0.022
	(0.181)	(0.221)	(0.216)	(0.019)
Wild bootstrap p (department)		0.004	0.003	0.031
RI office-placebo p (unif. / pop.-w.)	0.322 / 0.517 (event-study form; levels: 0.181 / 0.282)			
Baseline covariates × election	No	Yes	Yes	Yes
Unit (1876-parent) and election FE	Yes			
Units / observations	490 / 6,796			

Notes: Turnout (votes over registered voters) cannot enter the event-study design because no municipal voter registry exists before 1980, so the outcome has no pre-period. Votes cast (party votes plus blank and null ballots) is recorded for 98–100% of districts in every election back to 1963, and population is observed in the censuses of 1961, 1972, 1981, 1993, 2007 and 2017. Votes cast per capita divides the numerator, aggregated to the 490 stable 1876-parent territorial units without provincial capitals so that post-1980 district splits cannot contaminate the series, by log-linearly interpolated census population. Event-study estimates on this panel over the 15 elections, with the specification of Table B13 (dose × election, 1966 base, unit and election fixed effects, baseline covariates × election in columns (2)–(4), SEs clustered by unit). Column (3) winsorizes the 1% of unit-years above 100, an artifact of intercensal interpolation. The ln(votes cast) column uses no population denominator. The pre-1980 literate franchise requires no denominator adjustment: the franchise change is national and absorbed by election effects, and its differential bite is absorbed by the 1961 illiteracy rate interacted with election dummies, part of the covariate set. Figure 15 reports the randomization-inference test on this outcome. *** p<0.01, ** p<0.05, * p<0.1.

Table B17: Rambachan–Roth sensitivity of the event-study estimates to violations of parallel trends

	Original	$\bar{M} = 0$	$\bar{M} = 0.25$	$\bar{M} = 0.5$	$\bar{M} = 1$
Panel A: relative magnitudes (Δ^{RM}) — post violation $\leq \bar{M} \times$ observed pre-period violation					
NEP, ATT_{avg}	[0.03, 0.13]	[0.03, 0.12]	[-0.05, 0.19]	[-0.15, 0.29]	[-0.37, 0.51]
NEP, 2002 election	[0.06, 0.28]	[0.06, 0.28]	[-0.01, 0.34]	[-0.12, 0.44]	[-0.35, 0.67]
Victory margin, ATT_{avg}	[-1.41, 1.50]	[-1.36, 1.45]	[-6.38, 7.42]	[-12.99, 14.04]	[-26.24, 27.30]
Victory margin, 2002 election	[-1.17, 1.74]	[-1.16, 1.73]	[-7.11, 8.59]	[-14.70, 16.18]	[-29.88, 31.01]
Panel B: smoothness (Δ^{SD}) — differential trend may curve by at most M per period; $M = 0$ allows a free linear trend					
	Original	$M = 0$	$M = 0.01$	$M = 0.02$	
NEP, ATT_{avg}	[0.03, 0.13]	[-0.41, 0.15]	[-0.71, 0.46]	[-1.06, 0.81]	
NEP, 2002 election	[0.06, 0.28]	[-0.39, 0.27]	[-0.70, 0.57]	[-1.06, 0.93]	

Notes: Robust confidence sets (Rambachan and Roth, 2023) for the headline event-study estimates under two restriction classes on the post-treatment violation of parallel trends. Panel A (relative magnitudes, Δ^{RM}): the differential trend may deviate, per period, by at most $\bar{M}$ times the largest deviation observed in the pre-period. $\bar{M} = 0$ imposes exact post-treatment parallel trends and essentially reproduces the original interval. Panel B (smoothness, Δ^{SD}): the differential trend may depart from a linear extrapolation by at most M per period, so $M = 0$ allows an unrestricted linear differential trend. Targets: the average of the 13 post-reform coefficients and the 2002 peak, from the headline specification. With two pre-reform elections the design has a single free pre-coefficient, so the observed pre-period deviation is small for NEP (-0.029 parties) and no linear slope is pinned down, properties that make both restriction classes mechanically demanding over a 13-election post-horizon. *** p<0.01, ** p<0.05, * p<0.1.

Appendix C — Candidate Selection

Figures

(*exhibits to be assigned*)

Tables

Table C1: Land-reform exposure and candidates' education levels (2002–2022)

	Education (2002–2022)			
	Years Edu.	Secondary	Technical	University
	(1)	(2)	(3)	(4)
Panel A: OLS estimates				
Land reform exposure (SD)	0.075**	-0.007**	-0.002	0.011**
	(0.032)	(0.003)	(0.003)	(0.005)
Panel B: Second-stage 2SLS estimates				
Land reform exposure (SD)	1.520***	-0.141***	-0.109***	0.292***
	(0.390)	(0.038)	(0.035)	(0.069)
Anderson–Rubin p-value	0.000	0.000	0.000	0.000
F-statistic of excluded instrument	23.92	23.92	23.92	23.92
Sociodemographic controls	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes
State capacity controls	Yes	Yes	Yes	Yes
Hacienda and ethnicity controls	Yes	Yes	Yes	Yes
Agrarian Reform Zone FE	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes
Observations	40976	40977	40977	40977
Clusters	1375	1375	1375	1375
Mean Dep. Var.	14.241	0.268	0.196	0.480

Notes: Panel construction, specification, inference and caveats as in Table 13. All columns use the col (3) specification. Dependent variables: years of education and mutually exclusive highest-level dummies (complete secondary, technical, university). *** p<0.01, ** p<0.05, * p<0.1.

Table C2: Land-reform exposure and other experience measures (2002–2022)

	Experience and party type (2002–2022)				
	Yrs. elected	Yrs. mayor	Public Exp.	Private Exp.	National Party
	(1)	(2)	(3)	(4)	(5)
Panel A: OLS estimates					
Land reform exposure (SD)	-0.008	-0.007	0.002	0.013***	0.010***
	(0.018)	(0.013)	(0.004)	(0.004)	(0.004)
Panel B: Second-stage 2SLS estimates					
Land reform exposure (SD)	0.023	-0.130	-0.070**	0.201***	0.095***
	(0.128)	(0.107)	(0.032)	(0.046)	(0.030)
Anderson–Rubin p-value	0.857	0.212	0.014	0.000	0.001
F-statistic of excluded instrument	24.64	24.64	23.58	23.58	24.92
Sociodemographic controls	Yes	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes	Yes
State capacity controls	Yes	Yes	Yes	Yes	Yes
Hacienda and ethnicity controls	Yes	Yes	Yes	Yes	Yes
Agrarian Reform Zone FE	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes
Observations	43430	43430	37198	37198	44463
Clusters	1375	1375	1375	1375	1375
Mean Dep. Var.	1.490	0.897	0.551	0.506	0.470

Notes: Panel construction, specification, inference and caveats as in Table 13. All columns use the col (3) specification. Dependent variables: years in elected office, years as mayor, any public-sector and any private-sector work experience, and running for a national party. *** p<0.01, ** p<0.05, * p<0.1.

Table C3: Land-reform exposure and indigenous political entry: IV estimates by era

	1980–89	1993–98	2002–10	2014–22
Panel A: candidate pool				
2SLS: land-reform exposure (1 SD)	1.35	1.62	2.03	5.35*
	(1.65)	(2.09)	(2.31)	(2.77)
First-stage F	23.7	17.7	27.2	21.6
Mean dep. var. (%)	3.3	4.8	8.0	8.6
Observations	9,462	12,599	21,481	22,989
Panel B: elected mayors				
2SLS: land-reform exposure (1 SD)	1.62	0.39	4.26	10.61***
	(2.33)	(3.12)	(3.28)	(3.82)
First-stage F	30.7	30.3	36.6	32.7
Mean dep. var. (%)	3.5	4.1	7.6	8.8
Observations	3,873	3,364	3,600	3,652

Notes: Specification, sources and samples as in Table 15, estimated separately by era with election fixed effects within each era. Coefficients in percentage points. Standard errors clustered by district. *** p<0.01, ** p<0.05, * p<0.1.

Table C4: Land-reform exposure and other demographics (2002–2022)

	Female		Age	
	Candidate	Elected	Candidate	Elected
	(1)	(2)	(3)	(4)
Panel A: OLS estimates				
Land reform exposure (SD)	0.000	0.000	0.084	0.153
	(0.002)	(0.002)	(0.086)	(0.148)
Panel B: Second-stage 2SLS estimates				
Land reform exposure (SD)	0.046***	0.010	2.509***	3.246***
	(0.017)	(0.019)	(0.809)	(1.065)
Anderson–Rubin p-value	0.001	0.581	0.000	0.000
F-statistic of excluded instrument	24.92	35.39	24.90	35.60
Sociodemographic controls	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes
State capacity controls	Yes	Yes	Yes	Yes
Hacienda and ethnicity controls	Yes	Yes	Yes	Yes
Agrarian Reform Zone FE	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes
Observations	44463	7252	44463	7252
Clusters	1375	1375	1375	1375
Mean Dep. Var.	0.068	0.034	45.995	44.613

Notes: Panel construction, specification, inference and caveats as in Table 13. All columns use the col (3) specification, with the candidate pool in odd columns and elected winners in even columns. Dependent variables: female indicator and age. *** p<0.01, ** p<0.05, * p<0.1.

Table C5: Land-reform exposure and other representation characteristics (2018–2022)

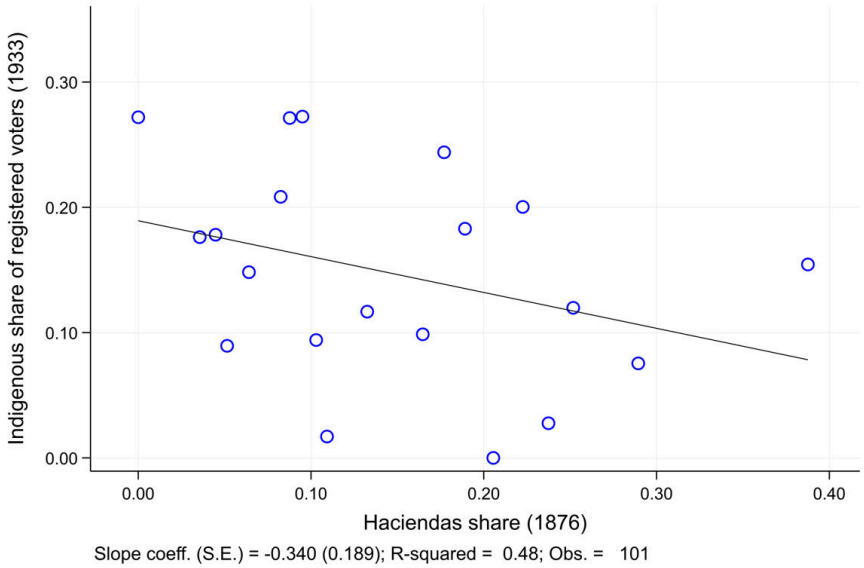
	Born in district		ln(income)		Public income share		Public-office conviction	
	Candidate	Elected	Candidate	Elected	Candidate	Elected	Candidate	Elected
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: OLS estimates								
Land reform exposure (SD)	-0.011	0.004	0.015	-0.034	-0.006	-0.006	-0.001	-0.002
	(0.007)	(0.011)	(0.015)	(0.031)	(0.006)	(0.012)	(0.001)	(0.002)
Panel B: Second-stage 2SLS estimates								
Land reform exposure (SD)	-0.242***	-0.134*	0.249**	0.380**	-0.103**	-0.014	0.002	-0.021*
	(0.082)	(0.079)	(0.121)	(0.193)	(0.051)	(0.075)	(0.007)	(0.012)
Anderson–Rubin p-value	0.000	0.071	0.026	0.038	0.024	0.855	0.749	0.059
F-statistic of excluded instrument	17.36	28.99	17.15	30.87	16.96	30.54	17.36	28.99
Sociodemographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State capacity controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hacienda and ethnicity controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Agrarian Reform Zone FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	13542	2318	12035	2156	11956	2140	13542	2318
Clusters	1193	1191	1193	1182	1193	1182	1193	1191
Mean Dep. Var.	0.626	0.679	10.195	10.333	0.426	0.424	0.008	0.009

Notes: Candidate-level panel of district-mayor candidates in the estimation sample, 2018–2022 only. These declaration items entered the sworn CV with Law 30326 (2015) and do not exist for earlier elections. Land-reform exposure standardized and instrumented as in the previous tables, with the col (3) specification (all control groups, sociodemographic controls of age, gender and indigenous surname, Agrarian Reform Zone and election fixed effects) and standard errors clustered at the district level. Odd columns use the candidate pool and even columns restrict to election winners. Outcomes: born in district (the declared birth-district coincides with the district of candidacy), ln(income) (log of one plus total declared annual income in the previous fiscal year, in soles), public income share (share of declared income from public-sector sources, conditional on positive income), and public-office conviction (any declared conviction for crimes linked to public office, from a keyword classification of the free-text offense field). Declarations are sworn and their omission is sanctionable. Two interpretive caveats: the birth-district effect may partly reflect post-reform family migration in addition to political entry by outsiders, and income is self-declared (the log specification tempers reporting outliers). No effects are found on declared asset stocks or on overall convictions in the candidate pool. *** p<0.01, ** p<0.05, * p<0.1.

Appendix D — Mechanisms

Pre-reform electoral control

Figure D1: Hacienda exposure (1876) and indigenous registered voters (1933)



Notes: Binned scatter of the share of the indigenous population registered as voters in 1933 against the share of the resident population living on *haciendas* in 1876, at the province level. Controls are geographic (slope, ruggedness, longitude and latitude) and demographic characteristics of 1876 (log population, population density, urban share, indigenous share, agricultural and mining shares). The horizontal axis is divided into 20 equal-sized bins and each point plots the within-bin means of both axes. Reproduced from Espinoza and Paredes (2026).

Note D1. Data construction and specification: incumbent families and elite surnames

The surname-composition figure: source and sample. The descriptive figure of the mechanisms section draws on the historical JNE records of the 1963–2002 municipal elections: 406,415 candidacies with full names (paternal and maternal surnames recorded separately), office, party and an elected indicator. The figure uses the *municipal district* contests only (312,417 candidacies, 1,879 districts) and, within them, *elected* officials, mayor plus councilors: 17,149 seats in the pre-reform elections (1963, 1966, held under the literate franchise) and 72,426 in the post-reform elections (1980–2002, seven elections). Surnames are normalized (accents and non-alphabetic characters removed). Married-name particles in the maternal-surname field (*DE X*, *VDA DE X*) are reduced to the root X, genuinely compound surnames (*DE LA CRUZ*, *DEL CASTILLO*, ...) are preserved, and single-letter initials are treated as missing.

Landed-elite series (panels (a) and (b)). List A collects 48 surnames of documented traditional landed-elite families, curated from the historiography with page-verified sources: the 22 elite groups of Quiroz (1988, appendix pp. 78–81), the hacendado families in Gonzales (1991) and Miller (1982), the hacienda–family pairs of Matos Mar (1976), and the 1950–68 continuity via Maushammer (Quiroz, 1988, p. 56), each with its home department(s) and period. A person bears a List-A surname when the paternal or maternal root matches exactly (compound surnames require a whole-word match on the full name). Matching is *strict*, following a person-level audit: a surname counts only within the family's home department, and is excluded altogether when its frequency exceeds 25 per 10,000 tokens in the candidate panel nationally or 2.5 per 10,000 within the home department. This drops, for example, Cáceres, with 36 per 10,000 within Puno, whose bearers there are overwhelmingly unrelated homonyms. The series is the number of elected officials bearing a List-A surname per 10,000 elected officials in the period. The placebo List B (panel (b)) applies the same rule to the post-reform economic elite of ca. 2000 (Figueroa, 2008) purged of families with pre-reform presence.

Indigenous series (panels (c) and (d)). Surnames are classified as Quechua or Aymara with the dictionaries of Artiles (2022) (20,719 unique surnames with manual quality review, the same classification used in the candidate-level tables). Panel (c) plots the share of elected officials with *at least one* indigenous surname and the share with *both* surnames indigenous, the strict marker least contaminated by surname intermixing. Panel (d) shows the most frequent indigenous surnames on their own scale. The key reading caveat: the franchise was restricted to literates until 1980, so the pre/post

contrast conflates the land reform with enfranchisement. Causal statements come from the exposure design of the replacement table.

Incumbent families: construction. Within each district, families are the connected components of the surname-sharing network over *elected officials* 1963–2002: two elected officials are linked when they share a paternal or maternal surname root, excluding the 52 surnames with national frequency above 25 per 10,000 (so that, for example, two unrelated Quispes do not form a family). Married-name particles do link, so a spouse joins the family. Linking over elected officials only, rather than over all candidates, prevents a transitive-chaining degeneracy: with all candidates, the median district collapses half its roster into one component. Districts where the largest component still spans more than half of all elected officials (385 districts, reflecting Andean patronymic naming) are flagged and excluded in the Panel B robustness of the appendix tables.

The replacement specification. Table 17 estimates, on the district cross-section,

$$\mathrm{IncShare}_d^w \;=\; \alpha \;+\; \beta\,\mathrm{Expo}_d \;+\; X_d'\gamma \;+\; \mu_{a(d)} \;+\; \varepsilon_d,$$

by OLS and by 2SLS instrumenting Expo_d with the standardized distance to the nearest Agrarian Reform Zone office, where:

- $\mathrm{IncShare}_d^w$: share of elected district offices (mayor and councilors) in window w, either 1980–83 (two elections, symmetric to the pre-reform baseline) or 1980–2002 (seven elections), held by *incumbent families*, i.e. families with at least one elected seat in 1963/66. Mean 0.51 in the short window and 0.46 in the long window.
- Expo_d: land-reform exposure, share of district area adjudicated 1969–80, standardized (1 SD = 21.1 pp).
- X_d: the column (3) control set of the main tables. Geographic (elevation, slope, longitude, latitude), agriculture and labor force (1961 agricultural labor share, GAEZ crop suitability, 1961 labor force), institutional (mita, social movements, communal uprisings, caudillos), state capacity (log state personnel and illiteracy, 1961), hacienda and ethnicity (haciendas 1961, indigenous mother-tongue share 1961).
- $\mu_{a(d)}$: Agrarian Reform Zone fixed effects.

Standard errors are clustered at the province level (about 190 clusters). The merge between the electoral panel (JNE ubigeo convention) and the exposure and controls dataset (INEI convention) runs through the JNE–INEI district crosswalk (1,378 matched

districts). Panel B replaces the numerator and denominator with *candidacies*: non-elected candidates inherit a family when they share a non-excluded surname with the district's elected network, and candidates outside the network form their own surname groups and can never be incumbent by construction.

The modern-era table (2002–2022). Table D1 carries the incumbent-family marker to the candidate-level panel of the 2002–2022 municipal elections (LR estimation sample). Surnames come from the validated panel of Artiles (2022) for 2002–2014 and its extension with the JNE sworn declarations for 2018–2022, normalized with the same rules as the historical panel. Each candidate's district is mapped through the JNE–INEI crosswalk, and the candidate is an *incumbent-family* member when either surname belongs, within that district, to the 1963–2002 elected family network of a family holding a 1963/66 seat. The estimating equation is the candidate-level analog of the replacement specification: a linear probability model of the incumbent-family indicator on standardized exposure (instrumented as above), the column (3) controls, sociodemographic controls (age, gender, indigenous surname), and Agrarian Reform Zone and election fixed effects, with standard errors clustered at the district level.

Tables

Table D1: Incumbent families in the modern era: candidacies and mayoralties, 2002–2022

	(1)	(2)	(3)	(4)
	Candidates		Elected mayors	
	OLS	2SLS	OLS	2SLS
Land-reform exposure (std)	-0.020***	-0.099*	-0.011	-0.087
	(0.007)	(0.053)	(0.010)	(0.063)
Geographic controls	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes
State capacity controls	Yes	Yes	Yes	Yes
Hacienda and ethnicity controls	Yes	Yes	Yes	Yes
Sociodemographic controls	Yes	Yes	Yes	Yes
Agrarian Reform Zone FE	Yes	Yes	Yes	Yes
Election FE	Yes	Yes	Yes	Yes
Mean dep. var.	0.369	0.369	0.408	0.408
First-stage F (KP)		18.9		27.2
Observations	28944	28944	4843	4843

Notes: Candidate-level panel of district-mayor candidates in the estimation sample, pooling the six municipal elections 2002–2022. Sources and the incumbent-family marker as in Note D1. The dependent variable equals one when the candidate shares a non-excluded surname with the district's 1963–2002 elected family network, in a family holding at least one elected seat in 1963/66. Four decades after the pre-reform baseline, 37% of candidacies and 41% of elected mayors still come from incumbent families (Mean Dep. Var. row). Columns (1)–(2) use the full candidate pool (does an old-family member run?). Columns (3)–(4) restrict to election winners (does one win?). Specification as in the candidate-level tables: standardized exposure instrumented with the standardized distance to the nearest Agrarian Reform Zone office, column (3) controls plus sociodemographic controls (age, gender, indigenous surname), Agrarian Reform Zone and election fixed effects, and standard errors clustered at the district level. Era splits: 2002–2014 mirrors the pooled estimates, and 2018–2022 is noisier with the same signs. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table D2: Land-reform exposure and the change in familial concentration of local office

	(1)	(2)	(3)	(4)
	ΔH seats		ΔH supply	
	OLS	2SLS	OLS	2SLS
Panel A: all districts				
Land-reform exposure (std)	-0.002	0.004	-0.001	0.052*
	(0.005)	(0.029)	(0.004)	(0.029)
Mean dep. var.	-0.015	-0.015	-0.017	-0.017
First-stage F (KP)		13.4		13.6
Observations	1043	1043	1053	1053
Panel B: excluding low surname-informativeness districts				
Land-reform exposure (std)	-0.003	0.001	-0.002	0.027
	(0.004)	(0.024)	(0.003)	(0.026)
Mean dep. var.	-0.014	-0.014	-0.014	-0.014
First-stage F (KP)		10.5		10.5
Observations	784	784	790	790

Notes: District cross-section as in Table 17. The dependent variable is the change in the Herfindahl index of familial concentration between the symmetric two-election windows 1963/66 and 1980/83: over elected seats (columns (1)–(2)) and over all candidacies (columns (3)–(4), with supply-side families linked within period). Panel B excludes the 385 districts where the largest family component spans more than half of all elected officials 1963–2002, micro-districts where Andean patronymic naming makes surnames uninformative about family ties (Note D1). Exposure standardized and instrumented, column (3) controls and Agrarian Reform Zone fixed effects, standard errors clustered at the province level, cluster-robust Kleibergen–Paap first-stage F. The precise zeros here, against the significant replacement result, are the point: the reform did not deconcentrate familial power on average, it rotated the families exercising it. *** p<0.01, ** p<0.05, * p<0.1.

Table D3: Land-reform exposure and the survival of pre-reform political families

	(1)	(2)	(3)	(4)
	Any pre family wins post mayoralty		Share of pre families with post seat	
	OLS	2SLS	OLS	2SLS
Panel A: all districts				
Land-reform exposure (std)	-0.020**	-0.057	-0.007	-0.056
	(0.010)	(0.049)	(0.007)	(0.052)
Mean dep. var.	0.930	0.930	0.491	0.491
First-stage F (KP)		13.5		13.5
Observations	1053	1053	1053	1053
Panel B: excluding low surname-informativeness districts				
Land-reform exposure (std)	-0.022*	-0.070	-0.007	-0.051
	(0.012)	(0.060)	(0.007)	(0.062)
Mean dep. var.	0.906	0.906	0.471	0.471
First-stage F (KP)		10.6		10.6
Observations	791	791	791	791

Notes: District cross-section as in Table 17. Columns (1)–(2): indicator that at least one family with a 1963/66 elected seat wins a post-reform mayoralty (1980–2002). Columns (3)–(4): share of 1963/66 families holding any elected seat post-reform. Panel B excludes low surname-informativeness districts (Note D1). Exposure standardized and instrumented, column (3) controls and Agrarian Reform Zone fixed effects, standard errors clustered at the province level, cluster-robust Kleibergen–Paap first-stage F. Survival levels are not comparable to mayor-only dynasty benchmarks in the literature: the post window offers seven mayoral elections and families are seat-based networks, both of which mechanically raise survival. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table D4: Heterogeneity by pre-reform familial capture: number of effective parties and turnout

	(1)	(2)	(3)	(4)
	NEP		Turnout	
	OLS	2SLS	OLS	2SLS
Panel A: pre-reform capture = H seats (1963–66, std)				
Exposure (std)	0.126***	0.703**	0.489***	3.623***
	(0.047)	(0.288)	(0.113)	(1.001)
Exposure × H pre	0.017	-0.430	0.106	0.595
	(0.045)	(0.642)	(0.111)	(2.249)
First-stage F (KP)		0.9		0.9
Observations	3133	3133	3133	3133
Panel B: pre-reform capture = H supply (1963–66, std)				
Exposure (std)	0.120**	0.626**	0.468***	3.618***
	(0.048)	(0.304)	(0.115)	(1.005)
Exposure × H pre	-0.008	-0.728	0.018	0.467
	(0.053)	(0.567)	(0.133)	(1.673)
First-stage F (KP)		1.5		1.5
Observations	3139	3139	3139	3139

Notes: District-year panel on the canonical estimation sample of the main tables (municipal elections 2002, 2006, 2010, provincial capitals excluded). Each column regresses the outcome on standardized exposure, its interaction with the standardized pre-reform familial concentration measure (Herfindahl over 1963/66 elected seats in Panel A, over 1963/66 candidacies in Panel B), and the column (3) control set with Agrarian Reform Zone and election fixed effects. 2SLS instruments the two endogenous terms with the standardized distance to the nearest Agrarian Reform Zone office and its interaction with the pre-reform concentration measure, with the cluster-robust Kleibergen–Paap first-stage F reported. Standard errors clustered at the district level. The uniformly null interactions indicate that the effects of the reform on the number of effective parties and turnout operate through a channel orthogonal to pre-reform familial capture, consistent with beneficiary empowerment and organization rather than dynasty destruction, and caution against using familial concentration as a mediator in the mechanism decomposition. *** p<0.01, ** p<0.05, * p<0.1.

Table D5: Did the reform target districts with higher familial capture?

	(1)	(2)
	Land-reform exposure (std)	
H seats pre (1963–66)	-0.246	
	(0.150)	
H supply pre (1963–66)		-0.318*
		(0.164)
Observations	1053	1055

Notes: District cross-section as in Table 17. OLS of standardized land-reform exposure on the pre-reform familial concentration measures (Herfindahl over 1963/66 elected seats in column (1), over candidacies in column (2)), with the column (3) controls and Agrarian Reform Zone fixed effects and standard errors clustered at the province level. The weakly negative coefficients indicate the reform did not target captured districts conditional on the control set, supporting the interpretation of the replacement result as an effect of exposure rather than selection. *** p<0.01, ** p<0.05, * p<0.1.

Social organization

Note D2. The cohort difference-in-differences: design and data

The cohort evidence of Figure 19, and the reported-preferences results of the political-preferences subsection below, follow the age-cohort difference-in-differences design of Albertus, Espinoza and Fort (2020) (AEF). We rebuild their person-level base from the raw ENAHO household surveys, 2004–2017 (about 1.5 million adults), preserving survey keys and interview year, and validate the reconstruction by replicating their education result (coefficients within a fraction of a standard error, 1,146 vs. 1,153 clusters). Each adult is assigned the land-reform intensity of their district of birth (expropriated land in active private use over 1972 agricultural land) and their age when the reform began there. The sample is restricted to adults born in coast and highland districts where some land reform took place, following AEF.

Estimated specification. For adult i born in district d and cohort c, surveyed in year t,

$$y_{idct} = \sum_{w \in W} \beta_w \left(\mathrm{Expo}_d \times \mathbf{1}[\, a_i \in w \,] \right) + \alpha_d + \delta_c + \lambda_t + f_{p(d)}(c) + X_i' \gamma + \varepsilon_{idct},$$

where Expo_d is the land-reform intensity of the district of birth, a_i is the individual's age when the reform began there, W is the menu of age-at-reform windows, α_d are district-of-birth fixed effects, δ_c birth-cohort fixed effects, λ_t survey-year fixed effects, $f_{p(d)}(c)$ province-of-birth cubic cohort trends, and X_i collects gender and native mother tongue. Standard errors are clustered by district of birth. In the single-year version of Figure 19, panel (b), the window dummies are replaced by single-year age-at-reform dummies with ages 26–40 as the reference.

Outcomes. The governance module (one adult informant per household) provides trust in political institutions (political parties, congress, district and provincial municipality, judiciary, on a 1 "none" to 4 "a lot" scale, with "don't know" set to missing) and support for democracy (importance and functioning, harmonized across questionnaire regimes), summarized as inverse-covariance-weighted indices. Organizational membership comes from the citizen-participation module, which records members person by person. The stable-item indicator uses the twelve organization types surveyed in every wave. Question numbering, scale changes, and a reversed-coded 2004 questionnaire round were harmonized against the printed questionnaires.

Exposure windows. Two window menus are reported. The AEF windows (ages 0–5, 6–11 and 12–17 at reform, reference 18–24) maximize comparability with the educa-

tion benchmark. The formative windows (0–11 and 12–25, reference 26–40, with the sample extended to ages 0–40 at reform) target the political-socialization hypothesis directly. Following the experience-effects evidence of Besley et al. (2026), where lifetime experiences with recency-decaying weights explain trust in government while growth during the impressionable years does not, our prior is that a shock arriving at ages 35–45 should leave little imprint on reported trust, whatever the window.

Note D3. The spatial gradient: construction and specification

The spatial exercise assigns each georeferenced ENAHO dwelling (the survey provides coordinates from 2007 onward; 66,625 unique locations, 2007–17) its Euclidean distance (UTM 18S) to the boundary of the nearest comunidad campesina polygon of each recognition epoch. Polygons come from the IBC national cadastre (6,083 communities). The recognition year is attached from the 2009 COFOPRI directory by ubigeo and normalized name, cross-validated against the SICCAM 2016 directory (99.1% identical years), yielding 876 polygons recognized in 1969–80, the reform's formalization wave, and 2,154 recognized before 1969. Within the 1969–80 wave, 515 polygons match communities whose recognition resolution carries the ORAMS/SINAMOS stamp. Outcomes are person-level membership indicators from the citizen-participation module. The specification regresses the outcome on distance-bin dummies to the nearest reform-era community and, in the same regression, to the nearest historical community (reference >10 km in both), with district-of-residence and survey-year fixed effects, individual controls, and standard errors clustered by primary sampling unit. This is a residence-based design. Migration selection is addressed with the non-mover sample, and its estimates are read as triangulation with the district-level IV rather than as stand-alone causal effects.

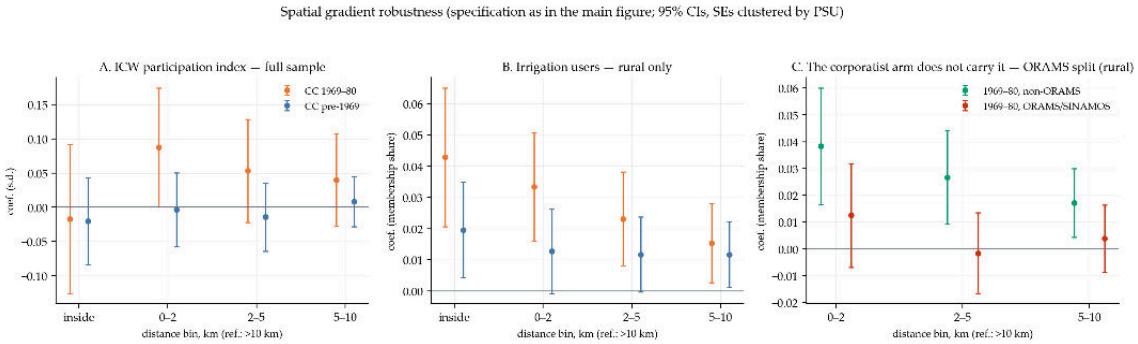
Estimated specification. For individual i in dwelling j and survey year t,

$$y_{ijt} \;=\; \sum_{b\in B} \beta_b \, \mathbf{1}\big[\, d_j^{\text{ref}} \in b \,\big] \;+\; \sum_{b\in B} \gamma_b \, \mathbf{1}\big[\, d_j^{\text{hist}} \in b \,\big] \;+\; X_i'\delta \;+\; \alpha_{d(j)} \;+\; \lambda_t \;+\; \varepsilon_{ijt},$$

where y_{ijt} is an organizational-membership indicator, in the main exhibits membership in an irrigation-users association (junta de usuarios de riego), taken from the person roster of the ENAHO citizen-participation module. d_j^{ref} is the Euclidean distance (km, UTM 18S) from dwelling j to the boundary of the nearest comunidad campesina recognized in 1969–80 (zero if the dwelling lies inside the polygon), and d_j^{hist} the analogous distance to the nearest community recognized before 1969. $B = \{\text{inside},\ (0,2],\ (2,5],\ (5,10]\}$ km, with >10 km the omitted category in both sets, so every β_b is identified conditional on the distance to the historical communal fabric. X_i collects age, age squared, a female indicator, and a native-mother-tongue indicator. $\alpha_{d(j)}$ are district-of-residence fixed effects and λ_t survey-year fixed effects. Standard errors are clustered by primary sampling unit (conglomerado). The identification test of panel (d) of Figure 20 is the within-regression contrast $\beta_b - \gamma_b$ (delta-method standard errors from the full covariance matrix). The ORAMS split replaces the 1969–80 bins with two separate sets, distance to the nearest reform community with, and without, an ORAMS/SINAMOS recognition resolution,

keeping the pre-1969 bins as controls.

Figure D2: Spatial gradient robustness: participation index, rural sample, and the ORAMS split



Notes: Specification as in Figure 20 and Note D3. Panel (a): the inverse-covariance-weighted participation index over the six full-period organization types. The gradient is diluted relative to irrigation-users membership, consistent with averaging one responsive item with five unresponsive ones. Panel (b): the irrigation-users gradient restricted to rural areas strengthens ($+4.3$pp inside, t $= 3.8$, with monotone decay significant through 5–10 km). Panel (c): splitting the 1969–80 communities by whether their recognition resolution carries the stamp of ORAMS/SINAMOS, the reform's corporatist mobilization agency. The gradient lives around non-ORAMS reform communities (rural 0–2 km $+3.8$pp, t $= 3.5$, clean decay). Around ORAMS communities there is essentially nothing beyond the polygon itself, and the ORAMS minus non-ORAMS differential is negative (rural 2–5 km -2.8pp, t $= -2.2$). The productive arm of the reform's formalization carries the organizational legacy and the corporatist arm does not. The split doubles as a placebo: mere proximity to state recognition does not produce the gradient.

Table D6: Spatial gradient of irrigation-users membership: levels, identification differentials, and the ORAMS split

	(1) Full sample	(2) Non-movers	(3) Rural only
Panel A: distance to nearest CC recognized 1969–80 (ref. >10 km)			
Inside CC polygon	4.10***	4.12***	4.29***
	(1.00)	(1.06)	(1.13)
0–2 km	2.86***	2.93***	3.34***
	(0.82)	(0.86)	(0.89)
2–5 km	1.73**	1.94**	2.31***
	(0.73)	(0.78)	(0.77)
5–10 km	0.50	0.34	1.54**
	(0.66)	(0.69)	(0.65)
Panel B: differential vs. distance to nearest pre-1969 CC (same bin, joint regression)			
Inside CC polygon	2.41**	2.30**	2.33*
	(1.07)	(1.14)	(1.22)
0–2 km	2.12**	2.14**	2.07*
	(0.90)	(1.00)	(1.08)
2–5 km	1.14	1.24	1.14
	(0.82)	(0.92)	(0.99)
5–10 km	0.08	−0.22	0.37
	(0.74)	(0.86)	(0.85)
Panel C: ORAMS split — CC recognized 1969–80 with vs. without ORAMS/SINAMOS resolution			
Non-ORAMS CC, 0–2 km	2.77***		3.83***
	(0.89)		(1.11)
Non-ORAMS CC, 2–5 km	2.16***		2.67***
	(0.79)		(0.89)
Non-ORAMS CC, 5–10 km	1.00		1.72***
	(0.66)		(0.65)
ORAMS CC, 0–2 km	1.97**		1.25
	(0.86)		(0.99)
ORAMS CC, 2–5 km	0.01		−0.16
	(0.65)		(0.77)
ORAMS CC, 5–10 km	−0.30		0.39
	(0.59)		(0.64)
Observations	733,272	355,828	266,486
District-of-residence FE	Yes	Yes	Yes
Survey-year FE	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes

Notes: Coefficients in percentage points of the membership share (base 2.2%). Panel A: distance-bin dummies to the nearest 1969–80 community (reference >10 km), from the joint regression that also includes the pre-1969 bins. Panel B: the within-regression differential (1969–80 bin minus pre-1969 bin, delta-method standard errors from the full covariance matrix). This is the identification test: the reform-era gradient exceeds the generic communal gradient significantly at 5% in the close bins for the full and non-mover samples, and at 10% in the rural sample. Panel C: 1969–80 bins split by ORAMS/SINAMOS resolution (columns (1) and (3) only). All regressions include district-of-residence and survey-year fixed effects and individual controls (age, age squared, gender, native mother tongue), with standard errors clustered by primary sampling unit. Non-movers are respondents born in their district of residence. Construction and specification in Note D3. *** p<0.01, ** p<0.05, * p<0.1.

Table D7: The organizational footprint by prior mobilization and communal vacuum

	(1)	(2)	(3)	(4)	(5)	(6)
	ICW participation index (s.d.)		Irrigation-users membership		Any local organization	
Panel A: by pre-reform peasant mobilization of the district						
	Prior mobilization	None	Prior mobilization	None	Prior mobilization	None
Inside CC polygon	−0.096	−0.003	0.096***	0.029***	−0.008	0.000
	(0.165)	(0.059)	(0.029)	(0.010)	(0.044)	(0.018)
≤2 km	0.122	0.067	0.078***	0.020**	0.042	0.025*
	(0.119)	(0.050)	(0.021)	(0.010)	(0.033)	(0.015)
2–5 km	0.144	0.030	0.070***	0.009	0.045	0.012
	(0.103)	(0.044)	(0.021)	(0.009)	(0.029)	(0.013)
5–10 km	−0.004	0.041	0.010	0.004	−0.004	0.012
	(0.066)	(0.039)	(0.010)	(0.008)	(0.024)	(0.012)
placebo: pre-69 CC, ≤2 km	0.007	−0.027	−0.000	0.006	−0.003	−0.008
	(0.119)	(0.032)	(0.020)	(0.004)	(0.034)	(0.009)
Observations	24,983	548,682	24,983	548,682	24,983	548,682
Panel B: by pre-1969 communal presence around the dwelling						
	No pre-69 CC ≤10 km	Pre-69 CC nearby	No pre-69 CC ≤10 km	Pre-69 CC nearby	No pre-69 CC ≤10 km	Pre-69 CC nearby
Inside CC polygon	−0.058	−0.027	0.071***	0.031***	−0.012	−0.011
	(0.211)	(0.055)	(0.026)	(0.010)	(0.065)	(0.017)
≤2 km	0.249	0.041	0.100***	0.016*	0.075*	0.015
	(0.170)	(0.044)	(0.027)	(0.008)	(0.046)	(0.014)
2–5 km	0.129	0.017	0.087***	0.006	0.052	0.005
	(0.153)	(0.038)	(0.025)	(0.007)	(0.042)	(0.012)
5–10 km	0.032	0.019	0.037*	−0.000	0.017	0.005
	(0.131)	(0.033)	(0.021)	(0.007)	(0.039)	(0.011)
placebo: pre-69 CC, ≤2 km	–	–	–	–	–	–
Observations	380,688	352,584	380,688	352,584	380,688	352,584

Notes: Coefficients on distance bins to the nearest 1969–80 community (reference >10 km), spatial-gradient specification of Note D3 (district-of-residence and survey-year fixed effects, age, age squared, gender and mother-tongue controls, standard errors clustered by primary sampling unit), estimated separately by cell. Outcomes: inverse-covariance-weighted participation index (in standard deviations), irrigation-users membership (base 2.2%), and membership in any local organization. Panel A splits by whether the district of residence recorded community-based peasant mobilization before the reform. The gradient is about four times stronger where it did (irrigation, ≤2 km: +7.8pp vs. +2.0pp), and the pre-1969 placebo row is a precise zero in the mobilized cell: prior mobilization does not gravitate toward communities in general, only toward reform-recognized ones. A broader mobilization moderator gives the same pattern (+7.1 vs. +1.8pp). Panel B splits by pre-existing communal infrastructure around the dwelling. The gradient concentrates where recognition arrived on communal vacuum, with no pre-1969 community within 10 km (+10.0pp vs. +1.7pp at ≤2 km). Read jointly with the ORAMS split of Table D6, the pattern supports a specific reading of the regional archetypes of Cant (2021): the footprint is deepest where pre-existing political demand met an absence of formal communal vehicles, amplification of the demand and activation of the vehicle. The Piura archetype, with SINAMOS working through coastal unions and cooperatives rather than communities, is not testable with this proxy: no dwellings sit inside community polygons in coastal Piura. These splits are exploratory, motivated by the historiography rather than pre-registered. The mobilized-district cell covers 4.3% of persons (8.2% under the broader moderator) and moderators are assigned by district of residence. *** p<0.01, ** p<0.05, * p<0.1.

Political preferences

Table D8: Cohort difference-in-differences: land-reform exposure and reported political attitudes

	AEF exposure windows (ref. ages 18–24 at reform)			Formative windows (ref. ages 26–40)	
Age at reform start:	0–5	6–11	12–17	0–11	12–25
Panel A. Reported attitudes					
Trust in political institutions (ICW, s.d.)	0.022	0.015	0.002	0.029	0.017
	(0.032)	(0.028)	(0.021)	(0.026)	(0.021)
				[0.64]	[0.76]
Trust in political parties (1–4)	0.007	−0.003	−0.012	—	—
	(0.020)	(0.018)	(0.015)		
Importance of democracy (1–4)	0.028	0.013	−0.014	—	—
	(0.023)	(0.022)	(0.018)		
Democracy functions well (1–4)	0.014	−0.007	−0.012	—	—
	(0.023)	(0.019)	(0.017)		
Support for democracy (ICW, s.d.)	0.045	0.008	−0.018	0.009	−0.016
	(0.032)	(0.029)	(0.024)	(0.026)	(0.022)
				[0.76]	[0.76]
Panel B. Behavior (reference)					
Member of ≥1 local organization	−0.001	0.004	0.011	0.009	0.023***
(stable 2004–17 item set)	(0.010)	(0.009)	(0.008)	(0.007)	(0.007)
				[0.56]	[0.006]

Notes: Each cell reports the coefficient on land-reform exposure interacted with the age-at-reform window dummy, with standard errors clustered by district of birth in parentheses and Romano–Wolf familywise p-values in brackets (stepdown with 999 cluster-bootstrap replications over the six index-outcome hypotheses of the formative-window specification). All regressions include district-of-birth, birth-cohort and survey-year fixed effects, province-of-birth cubic cohort trends, and gender and native-mother-tongue controls (design, outcomes and window menus in Note D2). Attitude outcomes: ENAHO governance-module informants (N about 88,000–93,000 in the AEF windows and 117,000–125,000 in the extended formative sample). Participation: all household members covered by the citizen-participation module (N about 238,000 and 312,000), stable 2004–17 organization types. The behavioral result in Panel B strengthens among non-movers ($+0.035$, t $= 4.0$), survives sampling weights ($+0.016$, t $= 2.0$), replicates in pre-2012 waves alone, and is not driven by the community-membership item introduced in 2012, whose own coefficient is zero. Reported attitudes are precise zeros throughout: with 80% power we can rule out effects larger than about 0.09 s.d. of either index for full exposure. Consistent with the experience-effects evidence of Besley et al. (2026), reported trust responds to recent experience rather than to early-life exposure, while behavior formed during the reform's mobilization years persists. *** p<0.01, ** p<0.05, * p<0.1.

Economic vis-à-vis political changes

Note D4. Economic outcomes: inference conventions, the community-gradient exercise, and the first-stage decomposition

Inference and sample conventions. The economic tables report Conley spatial-HAC standard errors (Bartlett kernel, 100 and 200 km cutoffs) alongside the clustered ones. Spatial correlation matters more for economic outcomes than for the electoral ones, and it matters in both directions: the early-period results of 1981 and the technification estimates of 2012 are untouched by the correction, while the 1994 land-distribution levels, the 1994→2012 reconcentration and the 2007 expenditure Gini all weaken under it. The reader should take the Conley rows as the operative inference throughout. Every outcome table also carries a memo row excluding the Amazon (Agrarian Reform Zones 8–9). The estimation sample keeps the Amazon, as the main tables do, because that is what makes the paper's falsification test possible: the instrument has no first stage in those zones, where the 1969 decree did not expropriate private haciendas and land transfer took the form of public-land colonization. The memo row therefore drops precisely the districts the falsification test identifies as untreated by the reform. The first-stage F rises from about 35 to about 42 when they are excluded, confirming that they dilute the instrument. Where the memo row changes what a table says, the note says so.

The community-gradient exercise (Figure 23). Mechanism 2 shows that the reform left an organizational imprint in space: households living close to a comunidad campesina formalized during the reform years are more likely to belong to a producers' organization, with no such gradient around communities formalized earlier. The exercise asks whether the material outcomes share that geography. It is not a second identification strategy, since which communities were formalized in 1969–80 is itself an outcome of the reform's intensity, but a triangulation: if the early economic gains are a by-product of the same local organizational shock, they should fade with distance from the communities the reform formalized, and not with distance from the historical communal fabric. For district i in Agrarian Reform Zone $z(i)$,

$$y_i \;=\; \sum_{b \in B} \beta_b \, \mathbf{1}\big[\, d_i^{\mathrm{ref}} \in b \,\big] \;+\; \sum_{b \in B} \gamma_b \, \mathbf{1}\big[\, d_i^{\mathrm{hist}} \in b \,\big] \;+\; W_i'\delta \;+\; \alpha_{z(i)} \;+\; \varepsilon_i,$$

where y_i is a district outcome (in the figure, mean log household income per capita and the share of children aged 6–17 in paid work, both from the 1981 census), d_i^{ref} is the distance in kilometres from the district's anchor point to the boundary of the nearest community recognized in 1969–80, and d_i^{hist} the analogous distance to the nearest

community recognized before 1969. $B = \{(0,2],\ (2,5],\ (5,10],\ (10,20]\}$ km, with >20 km the omitted category in both sets, so each β_b is identified holding the distance to the historical communal fabric fixed. W_i is the column (3) control vector of the main tables and $\alpha_{z(i)}$ are Agrarian Reform Zone fixed effects, with standard errors clustered by district. The anchor point is the same one that defines the instrument, so that distance-to-community and distance-to-office are measured off a common reference. The γ_b are the structural placebo. Table D10 repeats the exercise with the distances entered in logs and with Conley spatial-HAC standard errors.

What the instrument moves: the first-stage decomposition (Table D11). The natural candidate for where the early gains attach, the land the reform actually transferred, cannot be tested by asking who received it: the underlying records are expropriation resolutions published in El Peruano, which document whose land was taken, not which cooperative, peasant group or community it was awarded to. What the records do document is the kind of land taken, which maps onto two institutionally distinct programs operating under the same reform: the expropriation of private estates on the one hand, and the transfer of idle public land (tierras eriazas) on the other, the latter a colonization instrument closer in spirit to the earlier public-land legislation than to the 1969 decree's assault on the hacienda. District exposure is expropriated hectares over district area, and the numerator is the sum of three mutually exclusive land types, so exposure decomposes exactly:

$$\mathrm{RA}_i \;=\; \frac{EE_i^{\mathrm{use}} + EE_i^{\mathrm{eriazo}} + EE_i^{\mathrm{aband}}}{A_i}, \qquad \text{hence} \qquad \pi \;=\; \pi^{\,\mathrm{use}} + \pi^{\,\mathrm{eriazo}} + \pi^{\,\mathrm{aband}},$$

where π is the first-stage coefficient of exposure on the standardized distance to the nearest Agrarian Reform Zone office and each $\pi^{\,k}$ is the coefficient from regressing the corresponding component, scaled by the same standard deviation, on the instrument, with the column (3) controls and zone fixed effects throughout. Unlike the community-distance exercise, this is not a descriptive co-location: the three components are pieces of the treatment itself, not post-treatment outcomes, so no bad-control problem arises. The decomposition answers which kind of expropriation the instrument actually moves, that is, what the local average treatment effect of every 2SLS estimate in this section is an effect of.

Table D9: Land distribution and rural labor markets 25 years in: the 1994 agricultural census

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: land distribution across agricultural units, 1994						
	Land Gini (0–1)		Top-10% land share (%)		Units ≥50 ha (%)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Land-reform exposure (std)	−0.000	−0.070**	−0.30	−9.06**	0.14	−8.24***
Clustered S.E. (district)	(0.004)	(0.033)	(0.52)	(4.21)	(0.31)	(2.58)
Conley S.E. (100 Km.)	[0.005]	[0.045]	[0.60]	[5.87]	[0.33]	[4.27]*
Conley S.E. (200 Km.)	[0.005]	[0.049]	[0.64]	[6.33]	[0.30]	[4.99]*
Excl. Amazon (*ARZ zones 8–9*), 2SLS		−0.034		−4.45		−5.89***
		(0.027)		(3.46)		(2.15)
Mean dep. var.	0.777	0.777	71.80	71.80	3.86	3.86
First-stage F (KP)		37.2		34.8		34.8
Observations	1363	1363	1369	1369	1369	1369
Panel B: rural labor markets and technology, 1994						
	Units hiring paid labor (%)		Technical assistance (%)		Credit (%)	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Land-reform exposure (std)	1.82***	18.91***	1.06***	7.49***	1.17***	4.04
Clustered S.E. (district)	(0.67)	(4.94)	(0.38)	(2.36)	(0.40)	(2.88)
Conley S.E. (100 Km.)	[0.73]**	[6.40]***	[0.39]***	[3.04]**	[0.41]***	[3.99]
Conley S.E. (200 Km.)	[0.85]**	[6.80]***	[0.40]***	[3.47]**	[0.37]***	[4.16]
Excl. Amazon (*ARZ zones 8–9*), 2SLS		15.51***		6.72***		4.28*
		(4.24)		(2.06)		(2.55)
Mean dep. var.	33.70	33.70	10.18	10.18	14.28	14.28
First-stage F (KP)		35.0		35.0		35.0
Observations	1370	1370	1370	1370	1370	1370
Geographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes	Yes	Yes
State capacity controls	Yes	Yes	Yes	Yes	Yes	Yes
Hacienda and ethnicity controls	Yes	Yes	Yes	Yes	Yes	Yes
Agrarian Reform Zone FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: District cross-section (1,563 districts merge and roughly 1,365–1,370 enter the regressions once the column (3) controls bind; unit sizes reconstructed from the CENAGRO-94 parcel microdata, with an agricultural unit's landholding equal to the sum of its parcels' hectares, and labor and technology items from the unit questionnaire). Panel A: the land Gini across agricultural units, the land share of the top decile of units, and the share of units of 50 hectares or more all fall with exposure, while the median unit size does not move (-1.1 ha, $t = -0.3$): the reform compressed the top tail of the distribution without touching the typical smallholding, and there is no sign of reconcentration by 1994 (the 1994→2012 test is Table 21). Inference and sample both matter here, in the same direction. Under Conley spatial-HAC standard errors the Gini and top-decile effects lose conventional significance ($t \approx 1.4$–1.6) while the ≥50-hectare margin, the most reform-specific signature, survives at 10%. Excluding the Amazon (memo row) does the same: the Gini falls to -0.034 and the top-decile share to -4.5, both insignificant, while the ≥50-hectare effect stays at -5.9 and 1% significance. Part of the measured 1994 deconcentration therefore comes from large Amazonian holdings, where the reform operated as public-land colonization rather than hacienda expropriation. Panel A should accordingly be read as a directionally consistent pattern whose one robust margin is the disappearance of large units, not as three independently significant results. Panel B: the share of units hiring paid labor rises by 19pp, read with care: this is employer-side incidence (mostly seasonal harvest labor on smallholdings), not the model's dependent-labor share. Where haciendas disappeared, thousands of smallholders each hire some spot labor, so incidence rises precisely as tied labor dies, a rural labor market replacing dependence. Technical assistance rises 7.5pp on a 10.2% base and credit is positive but insignificant. Panel B is robust to the spatial correction (hired labor and technical assistance keep 1–5% significance under both Conley cutoffs). Specification and conventions as in Note D4. *** p<0.01, ** p<0.05, * p<0.1.

Table D10: Do the economic effects share the geography of the reform's organizational footprint?

	(1)	(2)	(3)	(4)
Panel A: 1981 census outcomes	Log HH income per capita	Child labor 6–17 (%)	Electricity (%)	Running water (%)
Distance to nearest CC recognized 1969–80	0.02	0.29	−1.40**	0.27
Clustered S.E. (district)	(0.03)	(0.23)	(0.70)	(0.72)
Conley S.E. (200 Km.)	[0.02]	[0.19]	[1.21]	[0.79]
Distance to nearest CC recognized pre-1969	0.07***	0.02	0.66	−0.27
Clustered S.E. (district)	(0.02)	(0.24)	(0.80)	(0.74)
Conley S.E. (200 Km.)	[0.04]*	[0.26]	[0.82]	[0.83]
1969–80 distance, controlling for reform exposure	0.02	0.27	−1.33*	0.27
	(0.03)	(0.23)	(0.70)	(0.72)
1969–80 distance, sierra only	0.01	0.23	−1.55**	−0.05
	(0.03)	(0.24)	(0.72)	(0.75)
Mean dep. var.	1.74	7.46	16.43	17.32
Observations	1107	1107	1107	1107
Observations (sierra only)	915	915	915	915
Panel B: agrarian structure and technology, CENAGRO 1994	Land Gini (0–1)	Units ≥50 ha (%)	Units hiring paid labor (%)	Technical assistance (%)
Distance to nearest CC recognized 1969–80	0.012**	1.56***	−4.18***	−1.51***
Clustered S.E. (district)	(0.005)	(0.39)	(0.89)	(0.40)
Conley S.E. (200 Km.)	[0.007]*	[0.64]**	[1.06]***	[0.53]***
Distance to nearest CC recognized pre-1969	−0.026***	2.18***	−0.22	0.36
Clustered S.E. (district)	(0.006)	(0.50)	(0.90)	(0.41)
Conley S.E. (200 Km.)	[0.009]***	[0.71]***	[1.12]	[0.49]
1969–80 distance, controlling for reform exposure	0.013**	1.59***	−3.92***	−1.35***
	(0.005)	(0.41)	(0.89)	(0.40)
1969–80 distance, sierra only	0.015***	1.31***	−3.39***	−1.35***
	(0.005)	(0.41)	(0.94)	(0.39)
Mean dep. var.	0.777	3.86	33.70	10.18
Observations	1363	1369	1370	1370
Observations (sierra only)	1018	1019	1019	1019
Geographic controls	Yes	Yes	Yes	Yes
Agriculture and labor force controls	Yes	Yes	Yes	Yes
Institutional controls	Yes	Yes	Yes	Yes
State capacity controls	Yes	Yes	Yes	Yes
Hacienda and ethnicity controls	Yes	Yes	Yes	Yes
Agrarian Reform Zone FE	Yes	Yes	Yes	Yes

Notes: District cross-sections. Regressors: the standardized log distance ($\ln(1 + \text{km})$) from the district to the nearest peasant community recognized during the reform (1969–80) and, entered jointly, to the nearest community recognized before 1969, the structural placebo (876 reform-vintage and 2,154 pre-1969 communities carry a recognition year in the 2009 COFOPRI directory). Distances are measured from the same district anchor point that defines the instrument. This is not identification but triangulation (Note D4). A specification note: in levels the two distances correlate at 0.94, an artifact of the right tail (coastal and Amazonian districts hundreds of km from any community). In logs the correlation falls to 0.51 nationally and to 0.06 within the sierra, which is where the usable variation lives, hence the log specification and the sierra-only row. Positive coefficients mean the outcome rises with distance, i.e. falls with proximity. Panel A: the 1981 outcomes show no gradient. Only electricity declines with distance to reform-vintage communities, and it does not survive the Conley correction. Income, child labor and water are flat: the early boom is not spatially organized around where communities were being formalized. Panel B splits in an informative way. The two organizational margins behave exactly as the design predicts: hired-labor incidence and technical assistance fall sharply with distance to reform-vintage communities (-4.2 and -1.5 per SD, both surviving Conley at 1%) while the pre-1969 placebo is a precise zero for both. The two land-distribution margins do not: the share of units of 50 hectares or more rises with distance to either vintage, so that gradient is a general feature of community country rather than a reform-specific one, and the land Gini loads with opposite signs on the two vintages. Read together: the community channel shows up in state contact and labor-market organization, the margins Mechanism 2 is about, and not in the land distribution, which the reform reshaped through expropriation rather than through communities. All specifications carry the column (3) controls and Agrarian Reform Zone fixed effects, with standard errors clustered by district in parentheses and Conley spatial-HAC (Bartlett, 200 km) in brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table D11: What kind of expropriation does the instrument move? A decomposition of the first stage

	(1) Coefficient on ARZ distance (1 SD)	(2) Share of expropriated area (%)	(3) Share of the first stage (%)
Total expropriated area	−0.188***	100.0	100.0
	(0.032)		
of which:			
Land in use (private estates)	−0.133***	68.3	70.7
	(0.026)		
Uncultivated public land (*eriazo*)	0.003	15.6	-1.9
	(0.008)		
Abandoned land (private, idle)	−0.058***	16.0	31.0
	(0.014)		
Observations	1375		
Geographic controls	Yes		
Agriculture and labor force controls	Yes		
Institutional controls	Yes		
State capacity controls	Yes		
Hacienda and ethnicity controls	Yes		
Agrarian Reform Zone FE	Yes		

Notes: District cross-section (N = 1,375, the estimation sample of the economic tables). Column (1) reports the coefficient on the standardized distance to the nearest Agrarian Reform Zone office, first for total expropriated area over district area, the paper's first stage in standard-deviation units, and then for each of its three components, scaled by the same standard deviation so that the three coefficients sum to the total ($-0.133 + 0.003 - 0.058 = -0.188$). The identity holds exactly at displayed precision. In the source data one district's recorded total is internally inconsistent with its by-type components (a gap of about 2,400 hectares) and floating-point noise accounts for the rest, which is why the shares in columns (2)–(3) sum to 99.9 and 99.8 rather than 100. Column (2) gives each type's share of expropriated hectares in this sample, 68.3 / 15.6 / 16.0, reproducing the 68 / 17 / 15 split reported by Albertus, Espinoza and Fort (2020) from the same decrees. Column (3) expresses each coefficient as a share of the total first stage. Land in use is privately held land under active cultivation or grazing, the haciendas. Eriazo is uncultivated land of public domain, transferred under the colonization statutes. Abandoned is privately held land left idle, in many cases run down by owners anticipating expropriation. The instrument moves private property and nothing else: on eriazo land the coefficient is a precise zero (0.003, se 0.008) even though such land is 16% of expropriated hectares. Every 2SLS estimate of the economic section is therefore an effect of the private-estate channel. Standard errors clustered by district. *** p<0.01, ** p<0.05, * p<0.1.

Table D12: Productivity and mechanization proxies from the agricultural censuses: changes 1994–2012

	(1) Mean change	(2) 2SLS (full)	(3) 2SLS excl. Amazon	(4) 2SLS sierra
Panel A: mechanization (% of units, pp change)				
Uses a tractor	6.41	15.15***	14.73***	6.44**
		(3.30) [4.82]***	(3.15)	(2.53)
of which: own tractor	−0.16	0.22	0.15	−0.14
		(0.42) [0.45]	(0.38)	(0.11)
of which: hired tractor	6.52	15.28***	14.90***	6.54***
		(3.26) [4.84]***	(3.12)	(2.51)
Panel B: purchased inputs (% of units, pp change)				
Chemical fertilizer	4.31	−2.87	2.51	3.17
		(3.28) [5.06]	(2.57)	(2.73)
Certified seed	−4.12	4.59	9.37***	2.14
		(3.74) [4.97]	(3.28)	(2.90)
Panel C: credit (% of units, pp change)				
Applied for a loan	0.05	4.77***	4.54***	3.64***
		(1.68) [2.16]**	(1.49)	(1.26)
Obtained a loan	1.09	5.37***	5.14***	3.50***
		(1.61) [2.24]**	(1.44)	(1.19)
Approval rate (obtained | applied)	12.63	17.13***	12.05**	7.21
		(5.94) [7.96]**	(4.76)	(5.48)
Panel D: state services (% of units, pp change)				
Received technical assistance	−4.47	−8.76***	−9.90***	−6.00***
		(2.62) [3.15]***	(2.34)	(1.86)
Panel E: Anderson inverse-covariance-weighted indices (SD)				
Inputs and mechanization	0.02	0.55**	0.83***	0.37**
		(0.22) [0.29]*	(0.20)	(0.16)
Credit	0.01	0.57***	0.55***	0.40***
		(0.18) [0.25]**	(0.16)	(0.14)
Overall (incl. technical assistance)	0.02	0.31	0.45***	0.21
		(0.19) [0.23]	(0.17)	(0.16)
Observations		1365		
First-stage F (KP)		35.5		
Geographic controls		Yes	Yes	Yes
Agriculture and labor force controls		Yes	Yes	Yes
Institutional controls		Yes	Yes	Yes
State capacity controls		Yes	Yes	Yes
Hacienda and ethnicity controls		Yes	Yes	Yes
Agrarian Reform Zone FE		Yes	Yes	Yes

Notes: District cross-section (N = 1,365, and 1,287 for the approval rate, which is defined only where some unit applied). All outcomes are district shares of agricultural units, in percentage points, differenced between the 1994 and 2012 agricultural censuses, with the 1994 items built under the same coding rules as the 2012 ones. Levels are not reported because they are uninformative here: exposure already predicted tractor use and fertilizer in the 1994 levels at nearly the magnitudes it would show in 2012, so the level gradient is pre-existing geography rather than an effect. Column (2) is the paper's 2SLS with column (3) controls and Agrarian Reform Zone fixed effects, with clustered standard errors in parentheses and Conley spatial-HAC (Bartlett, 200 km) in brackets, with stars from the latter. Columns (3) and (4) repeat the estimate excluding the Amazon and on the sierra alone, with clustered standard errors. The tractor coding was verified internally: among users, those reporting own machinery report owning a tractor in 100% of cases and those reporting hired machinery in 0%. Panel A: the entire mechanization effect is hired machinery (+15.3pp) and ownership is a precise zero (+0.2, se 0.4). Ownership fell nationally, from 0.9% to 0.6%. Land reform did not capitalize farms. It is associated with the growth of a machinery-services market, which is what mechanization looks like in a smallholding landscape. Panel B: purchased inputs do not move, and the large fertilizer coefficient of the levels specification disappears entirely once differenced. Panel C: applications rise 4.8pp, loans obtained rise 5.4pp, and the approval rate, loans obtained among units that applied, rises 17pp. That the supply margin moves more than the demand margin is the signature a collateral mechanism predicts: the window between the two censuses contains the PETT titling wave, and titles are what turn land into collateral. Panel D: technical assistance falls 8.8pp more where the reform hit, against a decline of 4.5pp in the estimation sample. Panel E: the inputs-and-mechanization and credit indices, inverse-covariance-weighted following Anderson (2008), both rise by about 0.55 SD, but the overall index that also carries technical assistance is indistinguishable from zero. Private access expanded by roughly as much as public provision withdrew. *** p<0.01, ** p<0.05, * p<0.1.

Table D13: Land titling: the reform, the PETT drive, and whether reconcentration tracks titles

	(1) Mean (2SLS sample)	(2) 2SLS (full)	(3) 2SLS excl. Amazon	(4) 2SLS sierra
Panel A: registered title among owner-held parcels (%, pp)				
Level 1994	32.56	12.70***	13.78***	5.55
		(4.69) [5.16]**	(4.27)	(4.28)
Level 2012	29.74	12.90***	14.21***	14.93***
		(4.95) [7.25]*	(4.47)	(4.81)
Change 1994–2012	−2.76	1.16	1.91	10.45**
		(5.47) [7.39]	(4.78)	(5.32)
Change, area-weighted	−12.05	6.62	4.44	15.76*
		(8.13) [8.95]	(7.11)	(8.43)
Panel B: the ownership margin (%, pp)				
Owner-held share of parcels, level 1994	65.63	21.67***	14.16***	21.81***
		(6.28) [12.02]*	(4.93)	(6.76)
Owner-held share, change 1994–2012	4.85	−10.67**	−10.07**	−10.52*
		(5.43) [6.71]	(4.66)	(5.68)
Any title among owner parcels, change	−20.80	0.13	1.43	5.77
		(6.05) [7.96]	(5.38)	(6.11)
Panel C: does the reconcentration track titling? (2SLS with interaction)				
Δ Land Gini (0–1) and Δ top-10% share (pp) on exposure, Δtitle (std) and their interaction				
Δ Gini: exposure		0.093***	0.081***	0.121***
		(0.035)	(0.030)	(0.039)
exposure × Δtitle		−0.041	−0.035	−0.033
		(0.028)	(0.028)	(0.031)
Δtitle (main effect)		0.030*	0.028	0.016
		(0.016)	(0.018)	(0.018)
Δ Top-10%: exposure		11.72***	10.65***	14.68***
		(4.44)	(3.92)	(4.87)
exposure × Δtitle		−4.71	−3.83	−4.30
		(3.56)	(3.68)	(3.93)
Δtitle (main effect)		3.52*	3.15	2.14
		(2.07)	(2.27)	(2.34)
Observations (Panels A–B / C)		1361 / 1353		
First-stage F (KP; A–B / C joint)		35.9 / 18.1		
Geographic controls		Yes	Yes	Yes
Agriculture and labor force controls		Yes	Yes	Yes
Institutional controls		Yes	Yes	Yes
State capacity controls		Yes	Yes	Yes
Hacienda and ethnicity controls		Yes	Yes	Yes
Agrarian Reform Zone FE		Yes	Yes	Yes

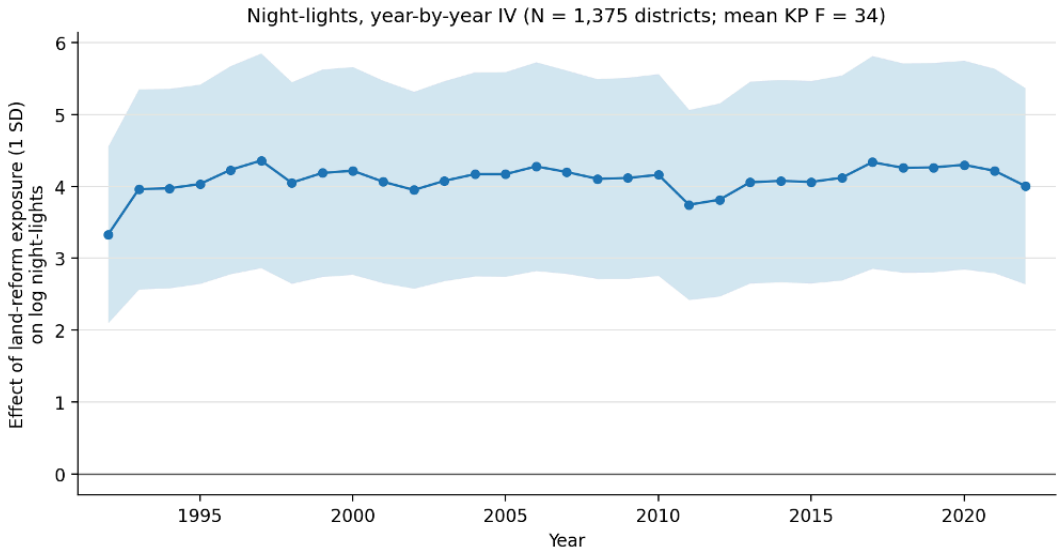
Notes: District cross-section (N = 1,361–1,366 in Panels A–B and 1,353 in Panel C). Parcel-level tenure items aggregated by counts from the two agricultural censuses (for 2012, parcel records are georeferenced by matching the record's unit identifier, with a 98% match covering 5.07 million parcels). Both censuses ask the tenure question with the same four-rung ladder, registered title, unregistered title, title in process, no title, conditional on the parcel being owner-held, so levels and changes are measured on identical definitions. Panel A: share of owner-held parcels whose title is registered, in levels and in the 1994–2012 change, with an area-weighted row weighting parcels by surface. Panel B: the share of parcels held in ownership and the change in the any-title share. Column (2) is the paper's 2SLS with the full control set and Agrarian Reform Zone fixed effects, with clustered standard errors in parentheses and Conley spatial-HAC (Bartlett, 200 km) in brackets with their own stars. Columns (3)–(4) report clustered standard errors. Three readings. First, exposure predicts registered titling in levels in both censuses at nearly the same size (+12.7 and +12.9pp), but the geography rotates: in 1994 the gradient is coastal (the coast entered the 1990s titled first, 35% of parcels against 16% in the sierra, and the reform's own adjudications came with titles, Zegarra 1999), while by 2012 the sierra carries it (+14.9, Conley t = 2.4). The change is a full-sample zero with a sierra coefficient of +10.5 (+15.8 area-weighted), at the 5% boundary under clustering but t ≈ 1.5 under Conley: suggestive rather than solid. Second, the ownership margin: the reform made owners, with the owner-held share of parcels 21.7pp higher in 1994, the direct footprint of adjudication, and the margin partly reverts by 2012 (−10.7pp). Third, Panel C asks whether the reconcentration of Table 21 runs along the titling margin, instrumenting exposure and its interaction with the standardized titling change. It does not: the interaction is negative and insignificant in every sample, splitting the sierra at its median titling change yields reconcentration on both sides, and conditional on controls the titling change does not predict the Gini change within the sierra (t = 0.5). The land reshuffle of 1994–2012 is not a titling-enabled market effect, consistent with field accounts of transfers through imperfect deeds before justices of the peace (Zegarra 1999), while the titling wave shows up instead in the credit margin of Table D12. This bears on Albertus's (2021) reading that late registration cemented in the post-sales status quo: titles where they arrived did not accelerate the reshuffle. Caveat: the titling change is itself an outcome of the period, so Panel C is descriptive, not a mediation estimate. *** p<0.01, ** p<0.05, * p<0.1.

Table D14: Night lights and land reform: initial level and long differences, 1992–2022

	(1)	(2)	(3)	(4)	(5)	(6)
		Level 1992		Δ 1992–2007	Δ 2007–2022	Δ 1992–2022
	OLS	2SLS	2SLS	2SLS	2SLS	2SLS
Panel A: log night lights, ln(0.01 + mean DN)						
Land-reform exposure (std)	0.18***	3.33***	3.73***	0.87***	−0.19	0.67**
Clustered S.E. (district)	(0.05)	(0.63)	(1.29)	(0.28)	(0.24)	(0.31)
Conley S.E. (100 Km.)	[0.06]***	[1.07]***	[1.79]**	[0.35]**	[0.32]	[0.42]
Conley S.E. (200 Km.)	[0.06]***	[1.30]**	[2.14]*	[0.32]***	[0.27]	[0.39]*
Excl. Amazon (ARZ zones 8–9), 2SLS		2.82***	3.77***	0.92***	−0.09	0.83***
		(0.51)	(1.31)	(0.26)	(0.21)	(0.28)
Mean dep. var.	−3.24	−3.24	−3.19	1.32	1.52	2.84
First-stage F (KP)		34.5	9.5	34.5	34.5	34.5
Observations	1375	1375	1224	1375	1375	1375
Panel B: share of district pixels lit (0–1)						
Land-reform exposure (std)	0.02***	0.40***	0.59***	0.11***	0.20***	0.31***
Clustered S.E. (district)	(0.01)	(0.08)	(0.20)	(0.03)	(0.04)	(0.06)
Conley S.E. (100 Km.)	[0.01]***	[0.14]***	[0.30]**	[0.04]***	[0.07]***	[0.09]***
Conley S.E. (200 Km.)	[0.01]***	[0.18]**	[0.37]	[0.03]***	[0.07]***	[0.09]***
Excl. Amazon (ARZ zones 8–9), 2SLS		0.35***	0.60***	0.11***	0.21***	0.32***
		(0.07)	(0.20)	(0.03)	(0.04)	(0.06)
Mean dep. var.	0.09	0.09	0.10	0.08	0.15	0.23
First-stage F (KP)		34.5	9.5	34.5	34.5	34.5
Observations	1375	1375	1224	1375	1375	1375
Col. (3) controls + ARZ FE	Yes	Yes	Yes	Yes	Yes	Yes
Remoteness controls	No	No	Yes	No	No	No

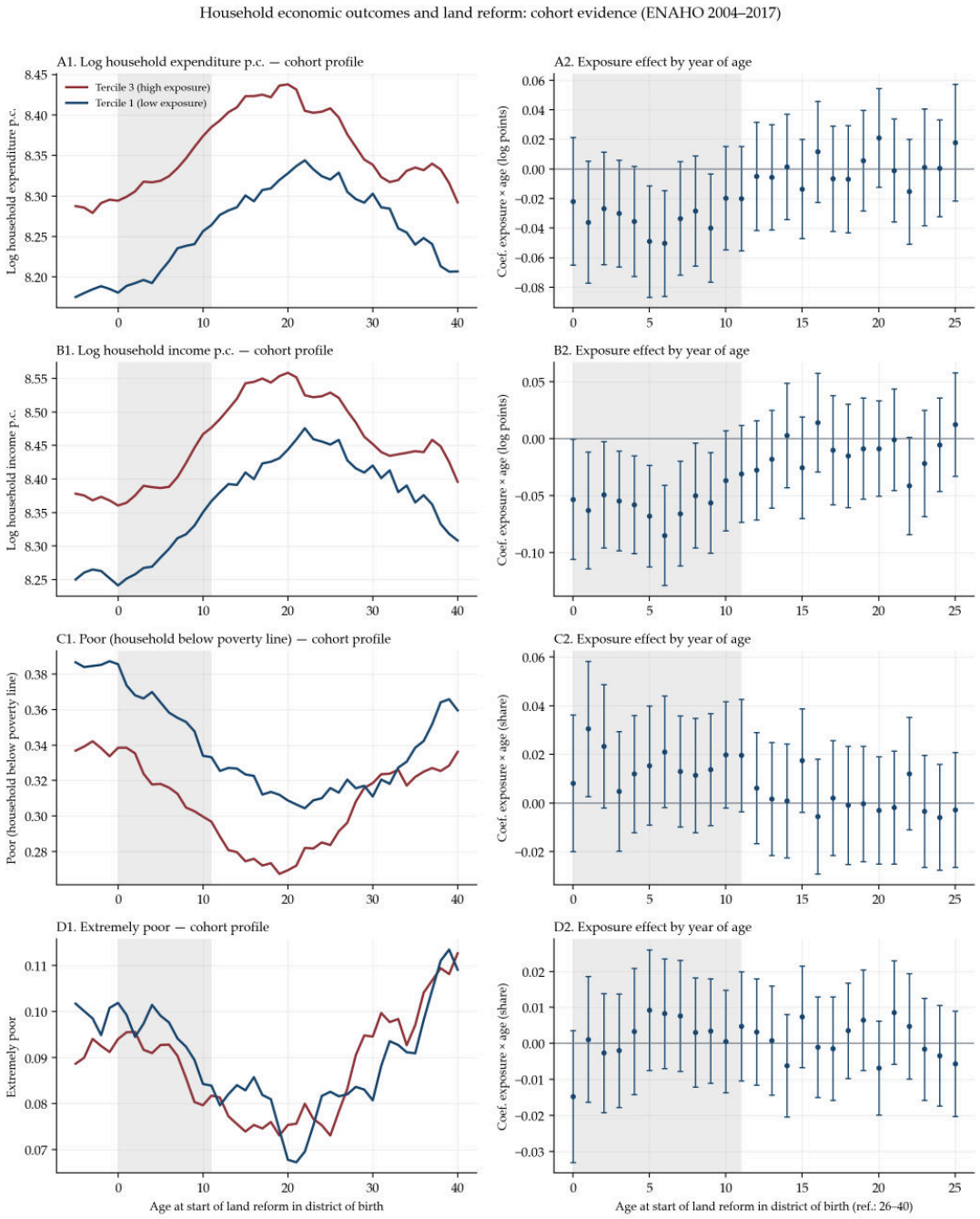
Notes: District cross-sections (N = 1,375, and 1,224 with the remoteness controls). Lights data: harmonized DMSP–VIIRS annual composites (Tang et al. 2025, 1 km), aggregated to districts by zonal statistics. The table compresses the 31-year coefficient trajectory (Figure D3) into an initial level and two long differences. Columns (1)–(3), the level in 1992: large and positive, surviving 1961 town-distance, distance-to-Lima and distance-to-coast controls (column 3, with the Kleibergen–Paap F dropping to 9.5), but read as a benchmark rather than causally: reform exposure targeted accessible agricultural valleys, lights levels partly price that accessibility, and 23 years after the reform began there is no pre-period to net it out. The interpretable objects are the long differences: over 1992–2007 the gap widens (+0.87 log points, robust to Conley spatial-HAC standard errors at both cutoffs) and over 2007–2022 it is flat, while the lit-area share (Panel B) keeps widening in both windows (+0.11 then +0.20, Conley-robust at 1%). The extensive margin continues to spread while mean intensity plateaus. Exposure standardized and instrumented as in the main tables, column (3) controls and Agrarian Reform Zone fixed effects, clustered standard errors in parentheses, Conley (Bartlett, 100/200 km) in brackets with stars from their own p-values. The memo row excluding the Amazon cuts the 1992 level by about 15% while strengthening every long difference (1992–2007 rises to +0.92 and 1992–2022 to +0.83, significant at 1%). Caveat: 1992–2007 lies within the DMSP era of the harmonized series while 2007–2022 crosses the DMSP→VIIRS splice (2013). Night lights show no late-period erosion of the exposed districts' aggregate luminosity, which pushes the reconciliation with the flat 2007 poverty margins toward within-district distribution rather than aggregate decline. *** p<0.01, ** p<0.05, * p<0.1.

Figure D3: Night-lights trajectory, year by year: 1992–2022



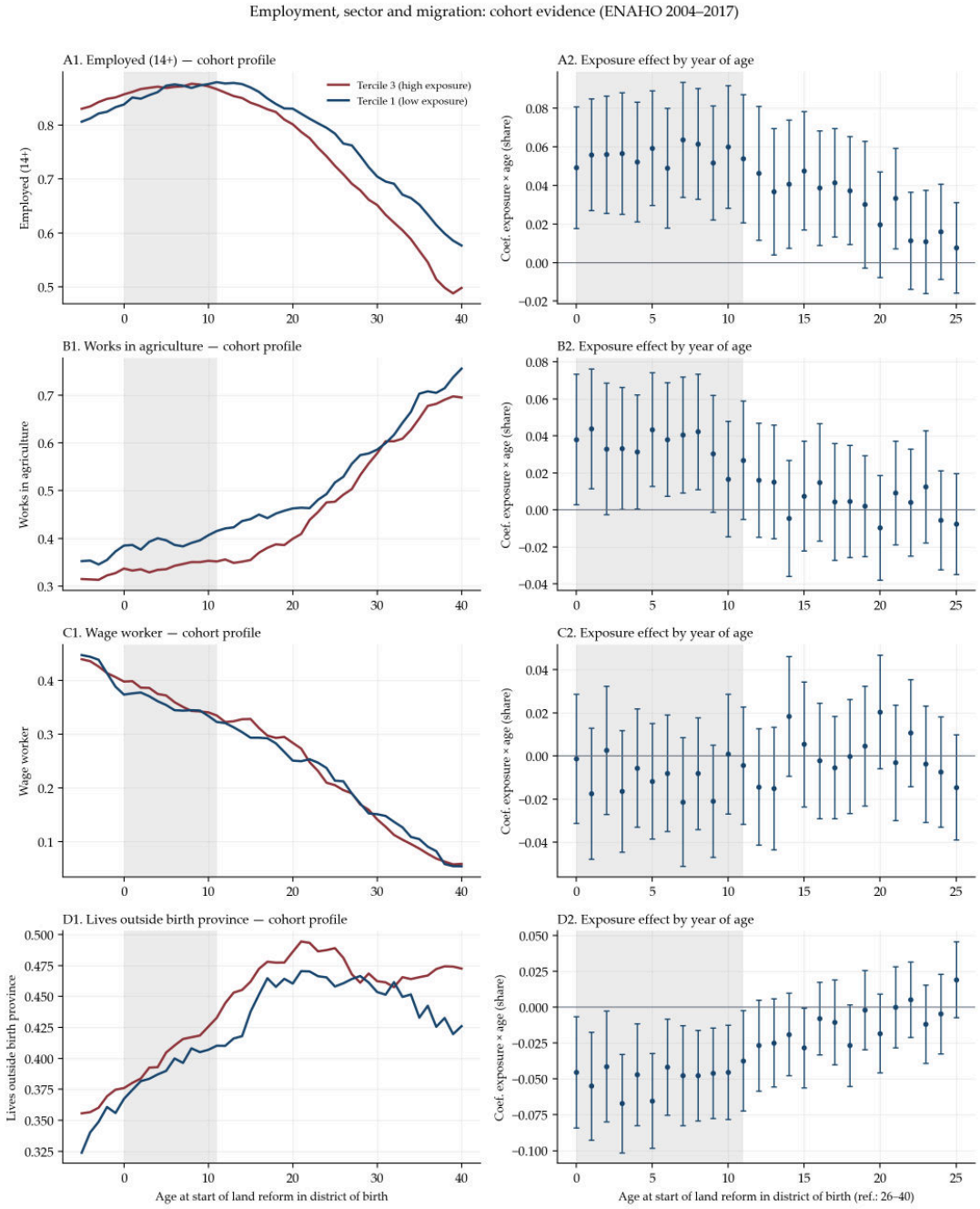
Notes: Supporting detail for Table D14: each dot is the coefficient from a separate cross-sectional 2SLS regression of district log night-lights ($\ln(0.01 + \text{mean DN})$) on standardized land-reform exposure, one per year, with a shaded band of 95% confidence intervals and district-clustered standard errors, column (3) controls and Agrarian Reform Zone fixed effects throughout (N = 1,375 districts per year, Kleibergen–Paap F $\approx$ 34). This is not an event study: the panel starts 23 years after the reform began, so there is no pre-period. The trajectory shows the widening-then-plateau pattern that the table's long differences summarize, and dates the plateau to the late 2000s. The dip at 2011–12 coincides with the end of the DMSP series in the harmonized product.

Figure D4: Household economic outcomes by cohort exposure window



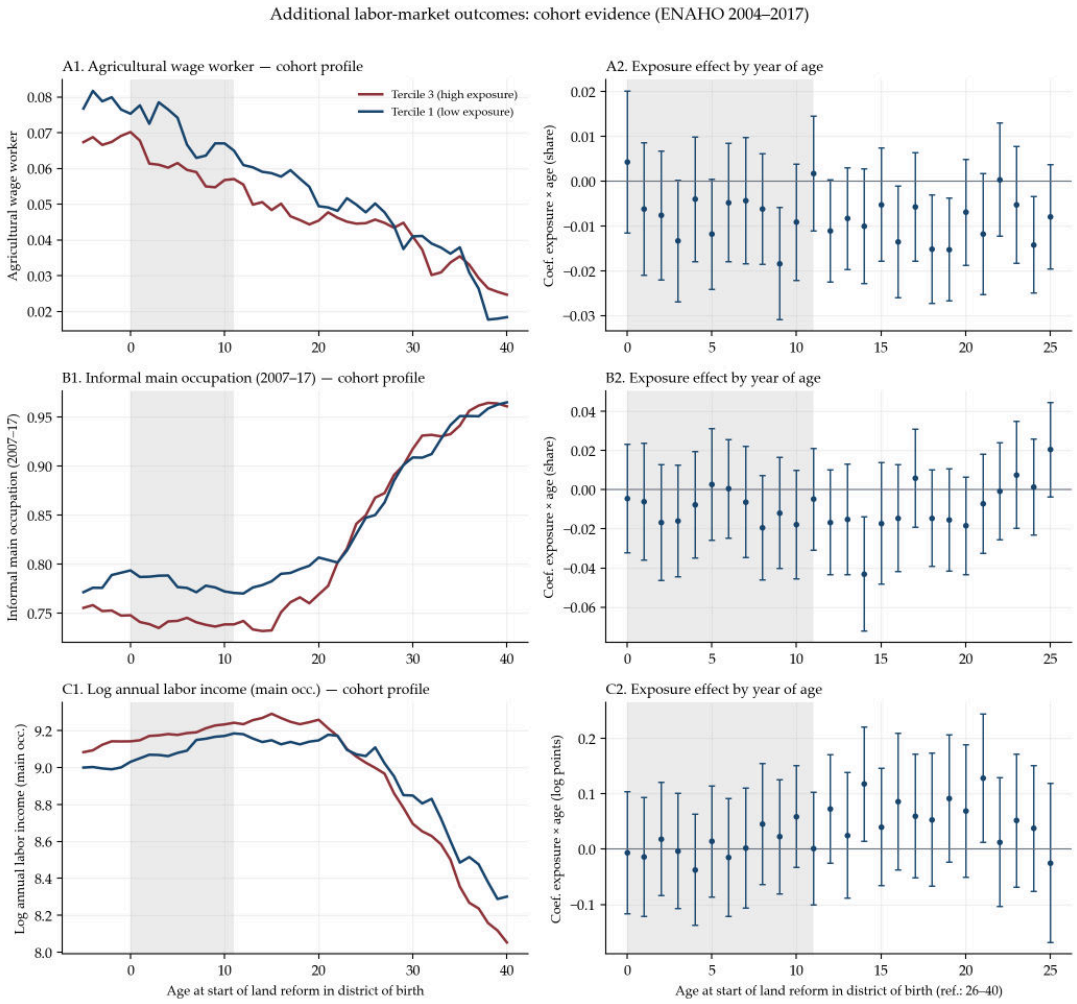
Notes: Rows: log household expenditure per capita, log household income per capita, poverty, extreme poverty (household-level outcomes assigned to each adult member's birth cohort; nominal values, with survey-year fixed effects absorbing national price changes). Design, specification, clustering and Romano–Wolf conventions as in Figure 24 and Note D2. Binned estimates for ages 6–11 at reform (full exposure): expenditure -3.8% ($t = 2.7$), income -4.8%, poverty $+2.6$pp on a 30% base ($t = 2.8$). Extreme poverty is a precise zero throughout: the effect operates above the extreme-poverty line. The expenditure and poverty margins survive the 15-hypothesis Romano–Wolf correction ($p = 0.05$ and 0.04).

Figure D5: Employment, sector and migration by cohort exposure window



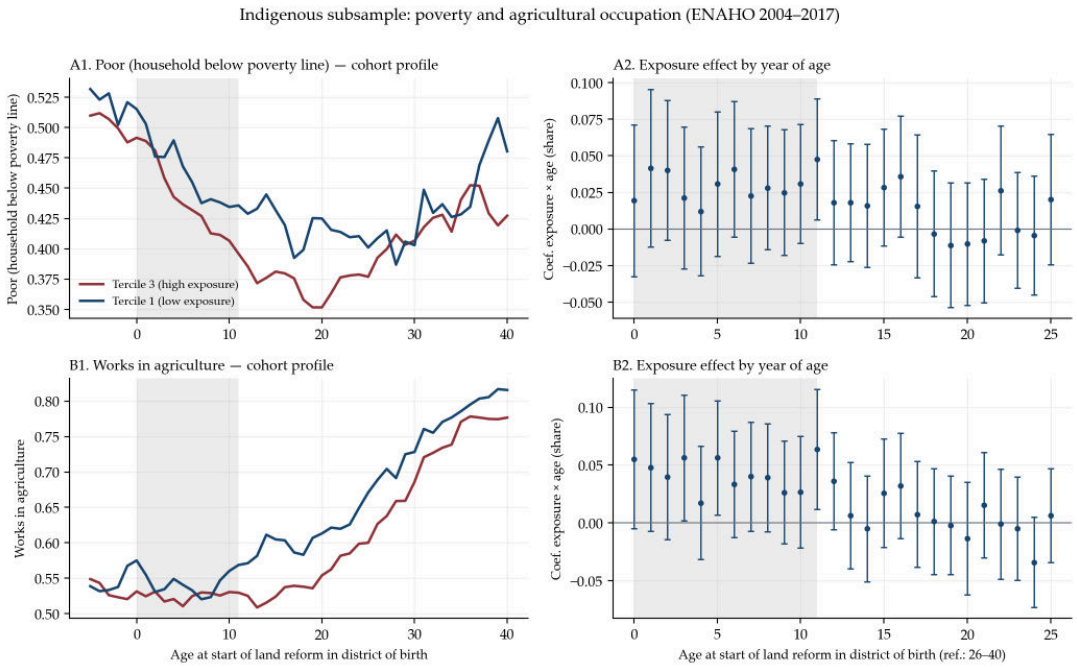
Notes: Rows: employed (14+), works in agriculture, wage worker, lives outside the birth province. Design and conventions as in Figure 24 and Note D2. The occupational margin mirrors the consumption one: childhood-exposed cohorts are $+2.8$pp more likely to work in agriculture (ages 6–11, t $= 2.4$) and -3.5 to -5.3pp less likely to have left their birth province (Romano–Wolf p $= 0.003$ for ages 0–5). They stayed, and they stayed in agriculture. Wage employment does not move, so the extra agricultural work is own-account or family labor, consistent with the reform reorganizing smallholder household production rather than creating wage jobs.

Figure D6: Additional labor-market outcomes by cohort exposure window



Notes: Rows: agricultural wage work (the person-level analogue of the model's dependent-labor margin, a precise zero, as the model predicts once haciendas are gone), informal main occupation (defined 2007–2017, positive but small and not robust), and log annual labor income in the main occupation (negative for childhood cohorts, -4.7% at ages 6–11, imprecise since income is reported by a subset of workers). Design and conventions as in Figure 24 and Note D2.

Figure D7: Poverty and agricultural occupation among indigenous cohorts



Notes: Profiles and event studies re-estimated on the indigenous subsample (native mother tongue; about 96,000 person-year observations and 1,000 clusters). Design and conventions as in Figure 24 and Note D2. The economic scar concentrates here: poverty $+5.3/+5.8$pp for ages 0–5/6–11 (against $+1.5$pp, n.s., among the non-indigenous) and the share working in agriculture $+6.9$pp. Uniquely in this subsample, the poverty effect extends into ages 12–17 ($+4.0$pp), so for indigenous cohorts the damage reaches into adolescence. Together with the coast/highlands split (consumption losses are largest in coastal birth districts, -9.0% for ages 0–5, while the agricultural-employment channel is exclusively serrano), this locates the incidence of the long-run cost on the reform's poorest intended beneficiaries.

Mediation Analysis

Note D5. The Kwon–Roth mediation test: implementation, hypotheses, and logic

Which of the four mechanisms can account for the reform's effects on political competition and turnout? Following Kwon and Roth (2026), each candidate mechanism enters as a district-level mediator measured before the outcome window: the seat share of 1963–66 incumbent families in the 1980–83 councils (local political capture), organizational membership of agricultural producers in 1994 (social organization), the broad-left vote share in the 1980–89 municipal elections (political preferences), and the 1994 rural economic state, hired-labor incidence and the land Gini, for the economic channel. The framework requires a binary treatment, so the distance instrument is dichotomized at the within-Agrarian-Reform-Zone median (near vs. far from an ARZ office), each mediator at its within-zone median in the direction of its empirical first stage, and the outcomes, district mean NEP and turnout over the 2002–2010 municipal elections, in quintiles.

The hypothesis and its logic. The hypothesis under test is the sharp null of full mediation, $H_0 \colon Y(1, m) = Y(0, m)$ for all m: the treatment moves the outcome only through the mediator(s). Under monotonicity the treatment cannot change the outcome distribution of always-takers, units whose mediator does not respond, so the null implies testable inequalities on the joint distributions $P(Y, M \mid Z)$. To fix ideas, take the organization mechanism with binary mediator M (high vs. low organization) and monotonicity $M(1) \geq M(0)$: under $Z = 1$ the low-organization cell contains only never-takers, while under $Z = 0$ it also contains compliers, so under the sharp null the instrument can only remove probability mass from the $(Y, M = 0)$ cells, never reshuffle Y within them. For every outcome quintile A_k this yields

$$
\begin{aligned}
P(Y \in A_k,\, M = 0 \mid Z = 1) \;&\leq\; P(Y \in A_k,\, M = 0 \mid Z = 0), \\
P(Y \in A_k,\, M = 1 \mid Z = 1) \;&\geq\; P(Y \in A_k,\, M = 1 \mid Z = 0).
\end{aligned}
$$

Violations of the first set of inequalities can only come from never-takers whose outcome responded to the instrument despite their organization status not responding. Aggregating their positive parts gives the reported lower bound on the fraction of never-takers affected (Kwon–Roth Proposition 3.1, closed form under monotonicity):

$$
\hat{\nu}_{NT} \;=\; \frac{\sum_k \left[\, \hat{P}(Y \in A_k,\, M = 0 \mid Z = 1) - \hat{P}(Y \in A_k,\, M = 0 \mid Z = 0) \,\right]_+}{\hat{P}(M = 0 \mid Z = 1)}.
$$

Rejection therefore means the mechanism, alone or as a set, cannot be the whole story. With multi-valued or multi-dimensional M (the mechanism sets of Table 23) the closed form is replaced by a linear program over the partially identified type shares, with the same logic.

Implementation. Estimates are produced with the authors' TestMechs package: Proposition 3.1 lower bounds solved by linear programming over the full set of implications, Cox–Shi and Andrews–Roth–Pakes p-values with analytic variance, and the defier-share breakdown at which each rejection would be overturned. Two standing caveats. The within-zone construction of treatment and mediators sits outside the formal i.i.d. framework of the test. And the turnout rejections would be overturned by defier shares of 3–5%, so the two-channel reading of the decomposition leans on monotonicity.

Appendix E — Conceptual Framework

This appendix presents a model of local electoral capture and its breakdown, extending Baland and Robinson (2008) to a setting with a literate franchise and land expropriation, and derives four implications: (i) exposure to expropriation raises party entry, and mainly where the reform also left organizational capacity behind; (ii) the balance of political weight shifts toward peasants, so support for redistributive parties rises with no change in preferences; (iii) candidates with attributes the freed voters value become both cheaper to recruit and more valuable to field; and (iv) turnout falls where the mobilization that capture itself produced is lost and rises where organization and a wider menu of parties add participation, so the net effect depends on which force dominates. The model is static, and it is meant to interpret and organize the pattern of empirical results.

E.1. Setting

Consider a district with landlords ℓ, dependent workers w employed on the estates, and self-employed peasants s. Landlords prefer a party R that protects land rents. Workers and peasants prefer a party L that favors redistribution and peasant empowerment. Dependent workers can be disciplined through the labor contract, self-employed peasants cannot. I take the occupational masses as given, as in the political block of Baland and Robinson (2008).

Production on an estate is subject to moral hazard. Output is high with probability γ_h if the worker exerts high effort, which costs her e, and with a lower probability if she does not, effort is not observed, and the worker has limited liability. The landlord therefore pays an efficiency wage $\tilde{w}$ in the high-output state to induce effort. With $\bar{w}$ the income from self-employment, the worker's per-period employment rent is

$$r \equiv \gamma_h \tilde{w} - e - \bar{w}, \tag{E.1}$$

and, following Baland and Robinson (2008), the high-effort contract is assumed profitable for the landlord and acceptable to the worker, which under the efficiency wage is equivalent to $r > 0$. The rent exists because limited liability rules out fines, so the landlord must make continued employment valuable to elicit effort, and the punishment for a deviating worker is dismissal. As in the original, r is the per-period rent and the results below are stated in terms of it. The same object has an explicit microfoundation in the online appendix of Anderson, Francois and Kotwal (2015).

Two features of the Peruvian setting are added. First, under the literacy requirement in force until the 1979 Constitution an illiterate adult cannot register to vote. Second, voting is compulsory, so a registered citizen who abstains pays a sanction. Normalize the adult population to one and partition it into landlords, of mass n_ℓ, literate dependent workers, who are in the electorate, of mass m^0, illiterate adults, who are outside it, of mass i^0, and literate self-employed peasants, of mass $F^0 = 1 - n_\ell - m^0 - i^0$. A share q of the illiterate are dependent workers. Two aggregates will be used: all dependent workers, $D^0 \equiv m^0 + q\, i^0$, and the illiterate self-employed, $E^0 \equiv (1-q)\, i^0$. The superscript 0 indexes the pre-reform district, under the literate franchise. Citizens are indexed by i, and $T_i \in \{0, 1\}$ indicates whether citizen i casts a vote.

E.2. Two margins of political control

Landlords control local politics through two instruments. The first works through the employment rent and decides how those who vote cast their ballot. The second works through the literacy requirement and decides who votes at all.

E.2.1. The freedom margin: vote control through the labor contract

The political block follows Baland and Robinson (2008). Voting is observable to landlords and parties. Let $s_L > 0$ be the ideological loss of a worker or peasant who prefers L but votes for R, and $s_R > 0$ the analogous loss of a landlord who votes for L. Party $j \in \{R, L\}$ values office at W_j and one vote changes its probability of winning by μ, so it is prepared to pay up to $v_j \equiv \mu W_j$ for a vote. Parties compete for votes in Bertrand fashion, prices are announced before labor contracts are signed, and R values office more, $v_R > v_L$.

A landlord can condition continued employment on voting for R. To do so he must raise the wage so that the worker still accepts the contract. Her loss from voting for R is the ideological loss s_L plus the price v_L that L would pay for her vote, and the rent r that the relationship already concedes offsets part of it, so the required increase in the expected wage is $v_L + s_L - r$ (Baland and Robinson, 2008, equation 8). Selling the vote to R yields v_L. Control is profitable if and only if $v_L \geq v_L + s_L - r$, that is,

$$r > s_L \tag{E.2}$$

(Baland and Robinson, 2008, Proposition 2).[22] The worker complies because a deviation

[22] I maintain Assumption 1 of Baland and Robinson (2008), which guarantees that the wage increase required by the dynamic incentive constraint is always worth paying, so that the participation constraint is the one that decides whether control pays.

is observed and costs her the rent. In the resulting equilibrium the prices that R pays for the votes of landlords and of dependent workers are

$$p_R^{\ell} = v_L - s_R, \qquad p_R^{w} = v_L, \tag{E.3}$$

with $v_L \geq s_R$. The landlord already prefers R, so R obtains his vote at a discount equal to his own ideological preference. The dependent worker's vote is obtained through the landlord, who is paid p_R^w and absorbs within the wage the part of the worker's loss that the rent does not cover, so R does not compensate the worker directly. The self-employed peasant is outside landlord control. R buys her vote at $p_R^s = v_L + s_L$ if it values office enough to match L's offer and compensate her ideological loss, $v_R \geq v_L + s_L$, and otherwise she sells her vote to L and votes sincerely. In either case the price R must pay for an independent vote, $v_L + s_L$ in the first and v_R in the second, exceeds p_R^w: buying votes through landlords is cheaper than buying them directly from independent peasants (Baland and Robinson, 2008, Proposition 1). I focus on the second case, $v_L < v_R < v_L + s_L$, in which the independent vote is not for sale to R and is the vote a challenger can contest. Land and labor-market power operate as a political technology that converts economic dependence into electoral control. I refer to this channel as the freedom margin of capture. It operates on citizens who are in the electorate and determines the direction of their vote.

Two remarks. First, the original model has no abstention margin, but a controlled vote is valuable only if it is cast, and the descriptive record the authors report is explicit: landlords of large estates registered their employees by teaching them to sign their names and went to vote with them on election day (Baland and Robinson, 2008, Section IV.A). I therefore set $T_i = 1$ for every controlled worker and treat the mobilization of dependents as part of the capture arrangement. Second, the margin does not depend on the ballot being literally observable. Anderson, Francois and Kotwal (2015) document landed elites retaining control of village councils in rural India under elections that are secret and freely contested, trading privately provided benefits for political support. The freedom margin should be read as this kind of control, with the labor rent as the private good.

E.2.2. The eligibility margin: schooling and the literate franchise

The freedom margin presumes that the worker is a voter. Under a literate franchise the landlord has a second instrument: keeping the worker out of the electorate by blocking schooling, which in a rural district requires the estate's land, buildings, tax base and the release of children from work. Because the veto over schools is a collective decision

of the district's landlords, I treat them as one decision maker. If schooling is allowed the illiterate mass i^0 becomes literate and enters the electorate, $q\,i^0$ as dependents and $(1-q)\,i^0$ as self-employed. The literate dependents m^0 are the workers landlords found it worth registering under the limiting case discussed below, in which signature-level literacy does not raise the outside option, so their rent is r and condition (E.2) applies to them unchanged. The veto is a decision over the remaining illiterate mass i^0, whose schooling would raise the outside option.

Three objects enter the calculation. Let W_ℓ be the landlords' gain from R's victory, the analogue of W_j, and $b_\ell \equiv \mu W_\ell$ their valuation of an additional vote for R.[23] Let schooling lower the employment rent by $\Delta r > 0$, because a literate worker has access to non-agricultural employment and her outside option rises. And let Δy be the productivity gain of the district's estates from a literate workforce. Galor, Moav and Vollrath (2009) show that human capital is less complementary to land than to physical capital, so Δy is small for agricultural estates.

A literate independent votes for L and is worth $-b_\ell$ to the landlords. A literate dependent can be kept under control or released. Under control, the argument behind condition (E.2) applied to the reduced rent $r-\Delta r$ gives the landlords the vote price net of the wage increase that control now requires, $\min\{p_R^w,\, r - s_L - \Delta r\}$, plus the stake b_ℓ. Released, she votes for L. Her political value to the landlords is therefore

$$\pi_w \equiv \max\Big\{ \underbrace{b_\ell + \min\{p_R^w,\, r - s_L - \Delta r\}}_{\substack{\text{value if kept}\\ \text{under control}}},\ \underbrace{-b_\ell}_{\substack{\text{value if}\\ \text{released}}} \Big\}, \tag{E.4}$$

which is weakly decreasing in Δr. Blocking schooling forgoes the productivity gain and the political value of the dependents and keeps every illiterate out of the electorate at no cost. The landlords block schooling whenever

$$\underbrace{\Delta y}_{\substack{\text{productivity gain}\\ \text{from a literate workforce}}} + \underbrace{q\,i^0\,\pi_w}_{\substack{\text{political value of the newly}\\ \text{literate dependents}}} < \underbrace{b_\ell\,(1-q)\,i^0}_{\substack{\text{political cost of the newly}\\ \text{literate independents}}}. \tag{E.5}$$

The trade-off is that of Bourguignon and Verdier (2000), in which the oligarchy weighs the productivity gain of education against the political cost of enfranchising the educated. When schooling erodes the rent enough, $\Delta r \geq r - s_L + 2b_\ell$, a literate dependent is worth as little as a literate independent, $\pi_w = -b_\ell$, and the landlords block whenever $\Delta y < b_\ell\, i^0$:

[23] In Baland and Robinson (2008) landlords sell their workers' votes as brokers and no such stake appears. A stake b_ℓ would leave condition (E.2) unchanged: under Bertrand pricing it lowers p_R^w by $2b_\ell$ and raises the value of a controlled vote by the same amount, so the two cancel. I keep the original statements.

every illiterate, dependent or not, is worth more to them outside the electorate than inside it. At the other extreme, $\Delta r = 0$, $\pi_w > 0$ and the landlords have an interest in registering their own workers, the behavior Baland and Robinson (2008) document in Chile, where a signature sufficed to register and did not raise the outside option. This is the rationale for the pattern in our companion paper (Espinoza and Paredes, 2026), where districts exposed to the hacienda system received fewer schools and teachers, most of all where labor control was concentrated in fewer estates. The margin is specific to the literate franchise: under universal suffrage, Anderson, Francois and Kotwal (2015) find that captured councils cut the programs that raise workers' outside option but leave those targeted at children and the elderly untouched.

Total district capture under the pre-reform order is the sum of the two margins,

$$\Gamma^0 \;=\; \underbrace{m^0}_{\text{directed votes}} \;+\; \underbrace{i^0}_{\text{disenfranchised}} \;=\; D^0 + E^0, \tag{E.6}$$

and the electorate contestable by a challenger is the literate self-employed mass F^0. Under capture the local outcome is pinned down by the hacienda structure of the district: R's share rises with m^0 and the vote a challenger can contest is F^0. Pre-reform elections therefore measure the directed-vote margin m^0 against the contestable mass F^0, while the disenfranchised margin i^0 is measured by illiteracy rather than by votes.

E.3. Land expropriation: the break of both margins

Let $\alpha_d \in [0, 1]$ denote exposure to land reform in district d, measured empirically by the share of district area expropriated, and write α for α_d when no confusion arises. Post-reform objects carry the subscript d and the argument α, and they describe the district under the universal franchise of 1979. A post-reform district with $\alpha = 0$ is therefore not the pre-reform district: the two differ by the franchise. Expropriation weakens the control technology through two channels and the organizations it creates add a third:

$$m_d(\alpha) = (1-\alpha)\, D^0, \qquad \delta_d(\alpha) = (1-\alpha)\, \delta^0, \qquad c_d(\alpha) = \bar{c} - \lambda\, Org_d(\alpha), \quad \lambda > 0. \tag{E.7}$$

Here $m_d(\alpha)$ is the mass of workers who remain dependent, so that $m_d(0) = D^0 > m^0$ because the franchise has brought the illiterate dependents into the electorate, where the estates that survive control them as before. $\delta_d(\alpha)$ is the landlords' punishment capacity (firing, eviction, exclusion from land), with δ^0 its pre-reform level. $c_d(\alpha)$ is the participation cost of a free citizen of the district, with $\bar{c}$ its level in the absence of organization. Organization lowers that cost because the cooperatives, recognized communities, irrigation boards and unions through which adjudicated land was held

supply information, transport and coordination, and I read c_d as the average cost among free citizens, lower where organization is denser. $Org_d(\alpha)$ is increasing in α because adjudication required beneficiaries to organize, an institutional feature of the reform, and $Org_d(0) > 0$ because some organization, the communities in particular, predates it. That Org_d increases in α is an assumption grounded in the adjudication rule rather than a result, and it is the hinge of the model. What the model adds is not the existence of the link but its multiplicative form in equation (E.9), in which liberation without organization and organization without liberation both produce little. Org_d is local organizational capacity rather than the stock of cooperatives. The cooperative form of tenure was parcelled out in the following decades, while the recognized communities, irrigation boards and unions through which land was also held remained, and it is that capacity, not the original vehicle, that the model carries forward. I write Org_d for $Org_d(\alpha)$ when the argument is not needed.

Capture as a regime that breaks. The linear forms are a first-order approximation to a regime that breaks, as in Anderson, Francois and Kotwal (2015), where control obtains only when landlords can profitably buy a majority of the worker vote. The rent contains no estate size directly, but the outside option behind it and the punishment that enforces it do, so below some retained size control fails altogether rather than weakening in proportion. Aggregating across the estates of a district, the response is the distribution of their thresholds, close to a step if these are concentrated and S-shaped if they are spread, so that little happens until expropriation is large enough to break the local regime and not much more happens beyond it.

The freedom margin breaks because the employment relationship on an expropriated estate no longer exists: the rent is zero, condition (E.2) fails and the worker votes sincerely. The eligibility margin is removed nationally, not locally: it is the 1979 Constitution that enfranchises the illiterate everywhere at once. The removal of the landlords' veto would raise schooling differentially in expropriated districts over the long run, a channel the static model brackets because, once the franchise is universal, literacy no longer determines eligibility, and I maintain as an assumption that the reform leaves the masses m^0 and i^0 unchanged across districts. What the reform determines is whether the released citizens count. The mass that was formerly either excluded from the electorate or subject to landlord control, and that is now both enfranchised and autonomous, which I call the bloc, is

$$B_d(\alpha) = \alpha\, D^0 + E^0. \tag{E.8}$$

At $\alpha = 0$ universal suffrage has brought the whole illiterate mass i^0 into the electorate, its dependent part $q\, i^0$ as controlled voters, which is why $m_d(0) = D^0 > m^0$, and its

independent part E^0 as autonomous. As expropriation rises, a share α of the dependent mass D^0 is released from landlord control, and the bloc rises from E^0 at $\alpha = 0$ to Γ^0 at $\alpha = 1$.

Effective political weight. As in standard probabilistic-voting models, a group's weight in the platforms candidates choose is its size times the responsiveness of its members (Persson and Tabellini, 2000). When groups differ in the rate at which their members are mobilized to vote, that weight acquires a group-specific conversion factor (Strömberg, 1998). Here I normalize responsiveness to one, so that a group's weight is its size times its conversion factor. Being free and enfranchised is not the same as counting. A released citizen weighs as part of a bloc only if she is reachable and coordinated with others like her. The always free peasants F^0 already vote and parties already reach them, while a released citizen is new to the electorate or newly autonomous, so her responsiveness has to be built. Organization is what builds it, and I let $h(Org) \in (0, 1]$, with $h' > 0$, be the share of the bloc it makes responsive. The effective weight of the bloc in district d, as a share of the electorate, is then

$$\omega_d(\alpha) = h\big(Org_d(\alpha)\big) \cdot B_d(\alpha), \qquad \frac{d\omega_d}{d\alpha} = \underbrace{h'\big(Org_d\big)\frac{dOrg_d}{d\alpha}B_d(\alpha)}_{\text{organization}} + \underbrace{h\big(Org_d\big)D^0}_{\text{liberation}} > 0. \quad \text{(E.9)}$$

Three readings. B_d is the released part of the formerly captured mass, so at full exposure it equals the capture Γ^0 of equation (E.6): what the landlord controlled is what is up for grabs. $h(Org_d)$ is the rate at which nominal enfranchisement converts into effective weight, and because the two enter as a product, neither a large released mass without organization nor dense organization over a small bloc produces much. And a citizen still under control votes for R whatever the platforms, so her own preferences carry no weight here, though her vote is not lost to the platform calculus: the landlord delivers it and it responds to platforms through him, a point I use in Section E.5.

E.4. Party entry

Let $\mathcal{K}_d(\alpha)$ be the set of parties that run in district d: the landlords' party R, which always runs, and the challengers. A potential entrant pays a fixed cost and presents a candidate with observable attributes a_k (education, partisan experience, indigenous background), under an existing label or one of its own, whose policy position x_k is fixed in the short run, so the entrant's choice is the candidate and not the platform. A party enters when the value of office times the probability of winning covers the cost of running, as in Feddersen, Sened and Wright (1990) and in the citizen-candidate model of Osborne and Slivinski (1996), where the number of candidates is bounded by the ratio of the

benefit of office to the cost of running. Two features of the capture environment enter that condition.

Gatekeeping. The punishment capacity δ^0 did not only discipline voters. Estate employees, tenants, debtors and the tradesmen who depend on the estate, the natural local challengers, were exposed to the same sanctions, so the cost of running against the landlord included the expected punishment. The effective entry cost of party k is

$$\tilde{K}_k(\alpha) = K_k + \rho\,\delta_d(\alpha), \qquad \rho > 0, \qquad \frac{d\tilde{K}_k}{d\alpha} = -\rho\,\delta^0 < 0, \tag{E.10}$$

where $K_k \in \{K^{ex}, K^{own}\}$ is the fixed cost of the vehicle, lower under an existing registration than for a new organization, $K^{ex} < K^{own}$, a feature of Peruvian party law.

Organizational subsidy. Fielding a candidate is cheaper when a local organization supplies the meeting hall, the list of members, the assembly in which a leader is already known and the volunteers. Let $C(a_k; Org_d)$ be the cost of fielding a candidate of type a_k, with $\partial C/\partial Org < 0$ for types $a_k \in \mathcal{A}^B$ credible to the organized bloc (a cooperative leader, a community authority, a candidate with an indigenous surname) and zero otherwise.

Vote shares and payoffs. Let σ_k^B be support for party k within the organized bloc and σ_k^G its support in the general electorate, which consists of the landlords, the literate self-employed and the released citizens whom organization does not reach, the last assumed to vote as the rest of the general electorate does. Support depends on the candidate's type, $\sigma_k^B(a_k)$ and $\sigma_k^G(a_k)$, an argument I suppress until Section E.6, and both are taken as independent of α for a given party set, over which they sum to one. Holding them fixed is what makes equation (E.12) a pure recomposition effect, and it rules out that entry dilutes support within the bloc. The directed votes $m_d(\alpha)$ go to R. Party k's expected vote share, the weighted average of its support in the three segments, and its derivative are

$$\sigma_k(\alpha) = \underbrace{\omega_d(\alpha)\,\sigma_k^B}_{\substack{\text{organized-bloc}\\ \text{support}}} + \underbrace{\big[1-\omega_d(\alpha)-m_d(\alpha)\big]\sigma_k^G}_{\substack{\text{general-electorate}\\ \text{support}}} + \underbrace{m_d(\alpha)\,\mathbf{1}\{k=R\}}_{\substack{\text{controlled votes}\\ \text{delivered to } R}}, \tag{E.11}$$

$$\frac{d\sigma_k}{d\alpha} = \underbrace{\frac{d\omega_d}{d\alpha}\big(\sigma_k^B - \sigma_k^G\big)}_{\substack{\text{shift from general to}\\ \text{bloc voting behavior}}} + \underbrace{D^0\big(\sigma_k^G - \mathbf{1}\{k=R\}\big)}_{\substack{\text{release of votes}\\ \text{previously directed to } R}}. \tag{E.12}$$

Shares sum to one and the general electorate has positive mass, since $\omega_d + m_d \leq \Gamma^0$. They are measured over the electorate: allowing for the differential participation of Section E.7 would scale the two free segments by ν_d and leave the directed segment at one, which understates rather than overstates the derivatives below. Substituting equation (E.9) into (E.12) shows what moves a challenger's share: organization moves the bloc toward the parties it favors, and liberation adds D^0 former dependents who vote σ_k^B if organized and σ_k^G otherwise. For R the same derivative is negative whenever $\sigma_R^B \leq \sigma_R^G$ and $\sigma_R^G < 1$, so the share available to challengers rises with exposure. The entrant's payoff, with V_k the value of office and $\chi_k \equiv \partial \Pr(\text{win} \mid \sigma_k)/\partial \sigma_k > 0$ the return to an additional unit of share, is

$$\Pi_k(\alpha) = \underbrace{V_k \Pr\big(\text{win} \mid \sigma_k(\alpha)\big)}_{\substack{\text{expected value} \\ \text{of office}}} - \underbrace{\tilde{K}_k(\alpha)}_{\substack{\text{vehicle and} \\ \text{gatekeeping cost}}} - \underbrace{C\big(a_k; Org_d(\alpha)\big)}_{\substack{\text{candidate-fielding} \\ \text{cost}}}, \tag{E.13}$$

$$\frac{d\Pi_k}{d\alpha} = \underbrace{V_k \chi_k \frac{d\sigma_k}{d\alpha}}_{\substack{\text{electoral return from} \\ \text{vote-share changes}}} + \underbrace{\rho\, \delta^0}_{\substack{\text{fall in landlord-imposed} \\ \text{entry costs}}} + \underbrace{\Big(-\frac{\partial C}{\partial Org}\Big)\frac{dOrg_d}{d\alpha}}_{\substack{\text{organizational reduction in} \\ \text{candidate-fielding costs} \geq 0}}. \tag{E.14}$$

Relative to the citizen-candidate payoff, equation (E.14) keeps only the office motive and a smooth probability of winning, written as a function of own share alone, and it holds the set of rivals fixed: it is a reduced form in partial equilibrium. For a challenger credible to the bloc, $\sigma_k^B \geq \sigma_k^G$ and $a_k \in \mathcal{A}^B$, the first two terms are positive and the third non-negative, and the support term is largest for the parties with the highest σ_k^B.

Since the payoff of every bloc-credible challenger rises with exposure, the set of profitable ones is nested in α and their number $N_d^*(\alpha)$ is non-decreasing. Entry here means a party running in the district, whether under an existing label or a new one. The conclusion survives free entry, because the share available to challengers rises with α. Party law rather than the reform fixes the cheapest route in: with the fixed cost lowest under an existing registration, $K^{ex} < K^{own}$, entry leans toward established parties, and new vehicles appear where registration is cheap enough.

Empirical implication 1: party entry. Land expropriation frees voters by breaking their economic dependence on landlords, and it also strips landlords of the power to punish those who run against them. Those voters turn into a bloc worth competing for through the organizational capacity the reform left behind. The model therefore predicts more party entry in districts more exposed to the reform, and mainly where that capacity was built.

E.5. Whose preferences count

The equilibrium policy is the weighted mean of group ideal points, the weights being the effective masses of Section E.3 (Persson and Tabellini, 2000). Let policy be two-dimensional, redistribution and peasant empowerment, with ideal points $z_\ell = (0, 0)$ for landlords and $z_w = (1, 1)$ for workers and peasants. Two assumptions fix the weights. The released citizens whom organization does not reach cast votes and still respond to a candidate's attributes, but they have no swing density in the policy space, so they carry no weight here. This is load-bearing: were they weighted, the organization term would drop out of the center. And the directed votes respond to platforms through the landlord who delivers them, so they carry his ideal point (Baland and Robinson, 2008): payments decide who controls the vote, policy decides what the winner does with it. The effective masses are then $\Omega_w(\alpha) = \omega_d(\alpha) + F^0$, the organized bloc plus the always free peasants, and $\Omega_\ell(\alpha) = n_\ell + m_d(\alpha)$, the landlords plus the votes they still direct, and they sum to less than one by the unreached mass. Then

$$\underbrace{z^{eff}(\alpha)}_{\substack{\text{effective policy center}\\ \text{of the district}}} = \theta(\alpha)\, z_w + \big[1 - \theta(\alpha)\big] z_\ell, \quad \theta(\alpha) \equiv \underbrace{\frac{\Omega_w(\alpha)}{\Omega_w(\alpha) + \Omega_\ell(\alpha)}}_{\substack{\text{share of effective weight held by the}\\ \text{bloc and the always free peasants}}}, \tag{E.15}$$

$$\frac{dz^{eff}}{d\alpha} = \theta'(\alpha)\,(z_w - z_\ell), \qquad \theta' = \frac{\Omega'_w\,\Omega_\ell - \Omega_w\,\Omega'_\ell}{(\Omega_w + \Omega_\ell)^2},$$

and the derivative is positive because $\Omega'_w = d\omega_d/d\alpha > 0$ and $\Omega'_\ell = -D^0 < 0$: the center moves toward the bloc along the whole path, with no change in anyone's preferences. With vehicle positions fixed in the short run, z^{eff} is not a platform that any party chooses. I read it as the district's representational target, the position a vote-maximizing platform would take given the effective weights, and the model's claim is that this target moves with exposure. Two readings follow. The mechanism is representation and not persuasion, so it should show up in behavior rather than in reported attitudes. And since positions are fixed, the shift is absorbed by who runs under those vehicles rather than by what they promise (Austen-Smith, 1984; Persson and Tabellini, 2000, page 52).

Empirical implication 2: electoral support. Land expropriation shifts the balance of political weight toward peasants, because the organized bloc gains weight while the votes landlords still direct fall away. The model therefore predicts more support for parties that represent redistribution in districts more exposed to the reform, and predicts it as a change in whose preferences count.

E.6. Candidate selection

Two ingredients from the literature matter here. A candidate's valence, attributes such as education or experience that every voter values, raises her share in every segment, and its return is largest where the race is close (Galasso and Nannicini, 2011). Alignment with a group is the second: voters weigh group identity alongside quality, so a candidate credible to the bloc draws more support inside it than outside (Banerjee and Pande, 2018).

Let a' be a type with attributes electorally valuable relative to a, and write $\sigma_k(\alpha; a)$ for the share in equation (E.11) when party k fields type a. Using χ_k as a first-order approximation, type a' is selected over a when

$$\underbrace{V_k\,\chi_k\left[\sigma_k(\alpha; a') - \sigma_k(\alpha; a)\right]}_{\substack{\text{electoral return to fielding}\\ \text{candidate type } a' \text{ rather than } a}} \;\geq\; \underbrace{C(a'; Org_d) - C(a; Org_d)}_{\substack{\text{additional cost of fielding}\\ \text{candidate type } a' \text{ rather than } a}}, \tag{E.16}$$

the electoral return against the additional recruiting cost. Writing $\Delta\sigma^B$ and $\Delta\sigma^G$ for the support advantage of a' over a within the organized bloc and in the general electorate, the party index dropped, equation (E.11) gives, the directed votes cancelling because the candidate's type does not change them,

$$\frac{d}{d\alpha}\left[\sigma_k(\alpha; a') - \sigma_k(\alpha; a)\right] = \underbrace{\frac{d\omega_d}{d\alpha}\left[\Delta\sigma^B - \Delta\sigma^G\right]}_{\substack{\text{gain in vote-share advantage}\\ \text{from greater effective-bloc weight}}} + \underbrace{D^0\,\Delta\sigma^G}_{\substack{\text{vote-share advantage of } a'\\ \text{among released voters}}}. \tag{E.17}$$

Exposure raises the value of an attribute that the organized bloc values more than the general electorate does, and, through the liberation term, of any attribute the general electorate values. Combined with the fall in $C(a'; Org_d)$ for bloc-credible types, condition (E.16) becomes easier to satisfy for them. Three channels follow. Credibility applies to descriptive representation, an indigenous background that the bloc values and that organization makes cheaper to recruit. Valence applies to education and experience, whose return rises as races become closer. And the pool channel follows from the end of gatekeeping alone: types previously deterred become available, which raises the number of candidates of every type without tilting their composition.

Empirical implication 3: candidate selection. Land expropriation makes formerly controlled peasants more politically influential, helps their organizations recruit candidates who represent them, and weakens landlords' ability to deter potential candidates. Candidates with the attributes those voters value therefore become both cheaper to recruit and more valuable to field, and the model predicts that districts more exposed to the

reform field more of them.

E.7. Turnout: demobilization against autonomous participation

I model the participation decision as expressive: a free citizen votes when the payoff from supporting the best available party, net of the participation cost and including the sanction $\psi \geq 0$ for abstaining under compulsory voting, is non-negative,

$$T_i = \mathbf{1}\Big\{ \max_{k \in \mathcal{K}_d(\alpha)} \big[u_i(x_k) + \varepsilon_{ik}\big] + \psi - c_d(\alpha) \geq 0 \Big\}, \tag{E.18}$$

where $u_i(x_k)$ is citizen i's utility from the position of party k, ε_{ik} her idiosyncratic preference for it, the same shocks that give the groups of Section E.3 their swing density, and the utility of abstaining is normalized to zero.[24] Let $\nu_d(\alpha) \equiv \Pr(T_i = 1)$ for a free citizen, evaluated at the menu of parties $\mathcal{K}_d(\alpha)$ that entry produces and assumed common to the always free, landlords included, the freed and the newly enfranchised, and let ν^0 be the corresponding probability in the pre-reform district, at cost $\bar{c} - \lambda\, Org_d(0)$ and the pre-reform menu. ν_d rises with α because organization lowers c_d and because the maximum in equation (E.18) rises with the number of parties, and it rises with ψ. The punishment capacity δ_d enters only indirectly, through the menu of parties that entry produces. It has no direct effect on a free citizen's threshold: it acts on dependents through control and on challengers through gatekeeping.

Under capture, controlled workers are mobilized by the landlord ($T_i = 1$), the illiterate are outside the electorate, and the free, landlords included, decide by the threshold. After the reform and the franchise every adult is in the electorate, registration being universal and compulsory. Measuring turnout over the electorate, the empirical object,

$$\tau^0 = \frac{m^0 + (n_\ell + F^0)\,\nu^0}{1 - i^0}, \qquad \tau_d(\alpha) = m_d(\alpha) + \big[1 - m_d(\alpha)\big]\nu_d(\alpha). \tag{E.19}$$

The two expressions are not compared: the change of franchise between them is national and reaches every district, and the model's prediction is the cross-district comparative static. Differentiating the post-reform expression with $m_d(\alpha) = (1-\alpha)D^0$,

$$\frac{d\tau_d}{d\alpha} = \underbrace{-\,D^0\big[1 - \nu_d(\alpha)\big]}_{\substack{\text{loss of landlord-enforced turnout} \\ \text{among released dependents}} \leq 0} + \underbrace{\big[1 - m_d(\alpha)\big]\frac{d\nu_d}{d\alpha}}_{\substack{\text{higher autonomous turnout through} \\ \text{organization and party entry}} \geq 0}. \tag{E.20}$$

[24] This is the expressive formulation of Schuessler (2000), with the compulsory-voting sanction entering as a negative cost of voting. It is not the pivotal calculus, in which participation depends on the difference between the candidates and vanishes as platforms converge.

Force (i) is the turnout that capture itself produced: every freed dependent who does not clear the threshold on her own is a vote the landlord used to deliver and that is no longer cast. Force (ii) is the participation that organization and a wider menu of parties add. The sign of the sum is indeterminate, and the compulsory-voting sanction compresses both, since as ψ rises $\nu_d \to 1$ and $d\nu_d/d\alpha \to 0$. What the model does locate is where the negative force bites: it is large where the dependent mass D^0 was large and where $1 - \nu_d$ is large, that is, where participation costs were still high and no party worth voting for had entered.

Empirical implication 4: turnout. Where expropriation breaks up the estates, the turnout landlords used to enforce is lost, while peasant organization and additional party entry make autonomous participation easier. The model therefore predicts that the net effect depends on which force dominates, with any decline largest where dependence ran deepest and no credible party had yet entered, and both forces shrinking under compulsory voting.

E.8. Summary of predictions

Prediction	What produces it	Key relation	Section
More party entry	Organization converts released votes into weight, and the end of gatekeeping lowers the cost of running	$d\Pi_k/d\alpha > 0$, eq. (E.14)	E.4
More support for redistributive parties	The bloc gains weight while directed votes fall, so the policy center moves toward the peasant ideal point	$dz^{eff}/d\alpha > 0$, eq. (E.15)	E.5
More candidates with the attributes freed voters value	Their electoral return rises while organization lowers the cost of fielding them	eq. (E.16)	E.6
Turnout depends on which force dominates	Coerced mobilization is lost, organization and a wider menu add participation	$d\tau_d/d\alpha$, eq. (E.20)	E.7

All three positive predictions run through the effective weight $\omega_d = h(Org_d) \cdot B_d$ of equation (E.9), which is zero if either factor is.

E.9. The economic channel

A reader may ask whether the reform simply made its beneficiaries richer, and whether it is that new income, rather than the end of political control, that allowed them into politics. In this setting the two point in opposite directions. What sustains control in Section E.2.1 is not poverty but the employment rent of equation (E.1): the dependent worker earns more on the estate than she would on her own, and it is precisely that surplus she stands to lose that makes her vote deliverable. The captured voters are therefore not the poorest in the district. They are the ones with something to lose, and a channel running through income alone would have made them the most independent voters rather than the least.

This does not rule out an income channel and the model does not try to. If the reform raised beneficiaries' incomes enough, they could cover the cost of running and of turning out, and entry would rise for a reason the model leaves out. The same holds for a levelling of the economic playing field, which would work through the distribution of resources rather than through the end of control. What separates those accounts from mine is organization. Neither of them needs it, and this one cannot do without it, because what converts a freed voter into political weight is being reachable and coordinated, which is what the cooperatives and communities supplied. Economic outcomes are consequences of the same shock rather than steps on the path from expropriation to politics, and whether an income channel operates alongside is an empirical question the main text takes up. A reform that changes whose preferences local politics represents is compatible with inequality that stays flat or widens, if a new rural elite recomposes around the land and credit it left in place.