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Land reform and human capital development: Evidence from Peru☆

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ABSTRACT

The early establishment and persistence of landholding inequality is linked to poor long-run development outcomes. One crucial channel runs through human capital: large landowners historically underinvested in public goods such as schools, restricted workers and their children from to attending school, and extracted surplus from laborers that could have been invested in human capital. By equalizing landholdings, land redistribution should facilitate human capital accumulation. Using original data on land reform across Peru in the 1970s paired with household surveys, we conduct an age cohort analysis and find instead that higher exposure to land reform negatively impacted educational attainment as measured by the number of years of school attended. The driving mechanisms appear to be economic opportunity as well as income and child labor: individuals exposed to land reform are more likely to remain in rural areas and to have their children contribute labor to agriculture, driving down income in the long term.

Many scholars point to early modern disparities in landholding as a key driver of the "Great Divergence" in income per capita between rich and poor nations over the last several centuries. One important channel runs through human capital accumulation. From the colonial era until at least the early-mid 20th century, landless peasants, wage laborers, and poor tenant farmers in regions as disparate as Latin America, the Middle East and North Africa, and East and South Asia struggled to send their children to school (Galor et al., 2009). Large landowners underinvested in or blocked the emergence of public goods such as schools in order to restrict the outside opportunities of their workers and drive down rural wages (Banerjee and Iyer, 2005; Engerman and Sokoloff, 2000).[1]

Land reform, under this logic, can facilitate human capital accumulation as large landowners are removed as an obstacle to the widespread state supply of schooling (Galor et al., 2009). The elimination of labor coercion through land reform also enables rural workers to freely decide to attend schools (Cinnirella and Hornung, 2016). In part because of these consequences, as well as others associated with income gains (Besley et al., 2016; Keswell and Carter, 2014) and health gains (Ghosh, 2008), land reform is often viewed as a powerful tool of development policy (e.g., Lipton, 2009).

There are few direct empirical tests of the effects of land reform on human capital, however. Important existing contributions typically evaluate this relationship indirectly by examining how land inequality impacts human capital accumulation.

This paper makes important new advances by using local-level data on land transfers paired with individual-level data on educational attainment in order to assess the long-term impact of land reform on subsequent human capital accumulation. We focus on a major case of land reform: Peru. A military regime seized power in Peru in 1968 and implemented a drastic land reform that redistributed half of all privately held land through a cooperative system by 1980. Individuals within the cooperatives lacked property rights and the ability to use land as collateral or make market-based land transactions. We collected original data on all land transfers that occurred through this reform and exploit variation in land reform location, intensity, and timing to examine how it impacted individual educational attainment across birth cohorts that we capture using both census data and a series of pooled household surveys.

We find that land reform actually *lowered* levels of human capital accumulation in relative terms as measured by the number of years of schooling. Individuals fully exposed to land reform from birth fell behind individuals not exposed to land reform by an estimated 0.32 fewer years of schooling.

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[1] Dell (2010) dissents from this view and argues that large landholding in Peru was associated with higher incentives for investing in public goods and the political connections necessary to extract benefits from the state.

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We attribute this effect to a drop in the demand side for schooling as opposed to the supply side that most existing literature examines. Further analyses suggest that two related mechanisms drive our findings: an economic opportunity mechanism and an income and child labor mechanism. Land reform beneficiaries that did not have opportunities to supplement their income with off-farm labor instead focused solely on farming. This generated intergenerational rural stasis. The greater reliance of land reform beneficiaries on farming also led to relative stagnation in income over time and a greater reliance on children to drop out of school in order to contribute labor to agriculture.

The findings suggest that there can be distinct effects on the supply of education versus the demand for education in response to land inequality and land reform. This could help to explain why there may be ambiguous relationships between land inequality and schooling across countries or regions, where both supply and demand are working against one another. While land redistribution equalizes landholdings, under certain conditions it also has the potential to stunt urbanization, wealth accumulation, and ultimately education by encouraging land reform beneficiaries to remain in the countryside and employ their children on the farm rather than migrating to urban areas where opportunities for upward mobility are greater. Land redistribution can also have broader general equilibrium effects on these outcomes by generating spillover effects in land and labor markets.

1. Land inequality, land reform, and human capital

Societies characterized by concentrated political power among a small elite tend to establish exclusionary institutions and develop more slowly than societies where political power and economic resources are distributed more widely (e.g., Acemoglu et al., 2001). Growing evidence suggests that this relationship has historically been driven in part by a lopsided distribution in land. Development tends to stagnate where a small number of large landowners dominate political and economic life (Easterly, 2007; Galor et al., 2009).

Large landowners historically underinvested in or blocked public goods such as schools (Banerjee and Iyer, 2005; Engerman and Sokoloff, 2000), restricted the ability of workers and their children to attend school (Rueschemeyer et al., 1992), and extracted surplus from rural laborers that could have been used to invest in human capital (Baland and Robinson, 2008). These important existing accounts center mainly on how large landowners limit the *supply* of schooling. Land inequality is tied to delays in the introduction of educational institutions as well as the expansion of formal education both cross-nationally (Baten and Juif, 2014; Kourtellos et al., 2013) and sub-nationally in countries as wide-ranging as Spain (Beltrán and Martínez-Galarraga, 2015), Prussia (Cinnirella and Hornung, 2016), and the United States (Galor et al., 2009).

It is therefore unsurprising that land reform is a widely used policy tool to address the negative consequences of landholding inequality. The vast majority of countries around the world have implemented some type of major land reform since 1900 (Bhattacharya et al., 2019). Land reform as a development policy remains on the agenda in countries such as Brazil, Colombia, Namibia, the Philippines, South Africa, Venezuela, and elsewhere.

There are several reasons why land reform could encourage human capital formation through dynamics linked to the supply of schooling. Redistributive land reform weakens the economic basis for the political power of large landowners. This makes it easier for ruling political coalitions to implement policies that may be inimical to traditional landholding interests (Albertus, 2015), such as extending the franchise to rural laborers or investing in education and training programs that increase the autonomy and independence of rural workers. Land reform can also mitigate or eliminate labor coercion (Rueschemeyer et al., 1992), freeing workers on former large estates to make their own decisions about education and relocation to urban areas to gain access to resources that would enhance family prospects for upward mobility.

Several existing studies support a positive link between land reform and advances in human capital, though some of it runs indirectly through landholding inequality. For instance, Galor et al. (2009) point to historical evidence from Japan, Russia, South Korea, and Taiwan showing that major land reform either paved the way for, or occurred simultaneously with, significant education reforms. These authors also link landholding inequality in U.S. states to lower education expenditures. In a partial global sample, Baten and Juif (2014) demonstrate first that land reform is associated with lower landholding inequality, and secondarily that land inequality is negatively linked to math and science test scores.

As these important contributions indicate, most evidence to date on land reform and human capital is available at the macro level. Scholars have not previously gathered data on the timing and extent of localized land transfers in order to examine the consequences of exposure to land reform on individual-level human capital accumulation.[2]

We collected data on the universe of land transfers under Peru's land reform from 1969 to 1985 through a comprehensive search through publications of *El Peruano*, the official government daily. *El Peruano* contemporaneously reported all expropriations of individual properties along with detailed information such as when an expropriation occurred, the district location, and the physical area of expropriated land. We also compiled individual-level data for roughly 200,000 individuals drawn from a pooled sample of Peruvian households over the time period 2004–2017 as well as census-based data. These data contain information on when and where individuals were born, where they currently live, how many years they have attended school, and a host of other characteristics on gender, native language, employment activity, landholding, and income and expenses.

By examining characteristics across different birth cohorts that experienced different levels of exposure to local land reform, we can evaluate the impact that land reform exposure had on educational attainment. Contrary to most theoretical expectations, we find that land reform in fact *undermined* rather than underpinned human capital accumulation as measured by the number of years of schooling.

We examine two related mechanisms through which this negative relationship operated. Both influenced the demand for schooling as opposed to its supply. The mechanisms are generalizable ones that could operate in cases beyond Peru.

The first mechanism is linked to economic opportunity. Prior to land reform in Peru, most rural inhabitants worked for large landowners or enterprises on unfavorable and often inflexible terms. Many did not capture the marginal product of their labor due to various forms of labor coercion. Migration to nearby towns or urban areas was not easy to afford or to accomplish logistically in many cases, but it was attractive if these barriers could be overcome. Land reform occurred in a top-down fashion and delivered rural workers direct land access through cooperatives. But the government owned the title over these cooperatives as was common in many land reform programs around the world at the time (Lipton, 2009). Cooperatives were obligated to repay the value of the land to the government over a 20-year period along with any debt that the cooperative took on in order to finance inputs and improvements. Beneficiaries therefore could not legally alienate their land for years.[3] In most cases, peasants received family plots and also had to contribute a specified

[2] Several recent studies, however, have used surveys to examine how receiving land can increase earnings and savings in some contexts (Besley et al., 2016; Keswell and Carter, 2014). This could theoretically shift intra-household decision making around family size and the tradeoff between using children as farm labor or sending them to school, impacting the demand for education. But this link has not yet been directly examined, and we return below to a discussion of the conditions under which this might operate.

[3] Peasants lost their claim over cooperative profits and lands if they left the cooperative to work elsewhere or if they did not fulfill their statutory labor requirement. Many cooperatives broke up in the 1980s, but most peasants still lacked land titles until the 1990s or 2000s, which depressed land values and made purchasing land elsewhere a somewhat risky enterprise.

amount of labor to the cooperative while sharing in the profits (McClintock, 1981). This meant that while peasants could more directly capture the product of their labor on their family plots, they also had a degree of flexibility in allocating their labor once they met their cooperative work obligations.

Land access, the possibility of land ownership in the future, and a stream of income from the cooperative made land beneficiaries' choice to migrate (whether permanently or seasonally) or to supplement their income with off-farm labor mainly attractive to those individuals who had particularly desirable opportunities, such as living close to other markets or to transit routes that could take them quickly to nearby towns or cities.[4] For those who lacked outside opportunities, receiving land access through a cooperative made the option of full-time farming relatively more attractive and foregoing land access to migrate or engage in other non-farm labor relatively less attractive. This generated intergenerational rural stasis among these latter families. They remained at arm's length from more dynamic labor markets and urban areas where their children could attend school beyond a basic level.[5] Relatedly, where nearby economic opportunities were absent and the quality of land received through the land reform was poor, families were forced to work the land longer to make a living. This also made it less likely that these families could afford to send their children to school.

The second mechanism is linked to income and child labor. The greater reliance of land reform beneficiaries on farming led to relative stagnation in income over time notwithstanding a short-term bump linked to heavy government support of most cooperatives in the first few years of reform. Land reform that converts wage laborers into either landowners or members of cooperatives can transition them to more volatile and uncertain income flows. Nonetheless, landholding can remain attractive because it simultaneously acts as a form of social insurance in many parts of the developing world. It provides not only employment and income, but also sustenance, a place to live, insurance for infirmity and age, and security for future generations. Receiving land through a land distribution program may therefore act as insulation against risk associated with negative employment or health shocks. While pursuing farming part-time or full-time may therefore not generate the highest earnings in expectation, it has the advantage of protecting against unanticipated negative shocks that can be particularly hard for the poor to overcome.

Through these logics, land reform led many land beneficiaries to remain in rural areas that were economically stagnant and subject to income volatility linked to harvests.[6] Faced with uncertainty and income constraints, many families could not predictably afford expenses associated with sending their children to school such as transportation costs and accommodation and living costs where schools are too distant for daily transportation.[7] To the contrary, families of land reform beneficiaries relied to a greater degree on having their children contribute labor to agricultural and other activities to supplement family income.

Due to these dynamics, land reform plausibly had more general equilibrium effects that operated through spillovers on broader land and labor markets as well as consequences for urban expansion. Land reform delayed the transformation from rural to more urban economies and out of agricultural labor. This could have stunted opportunities for upward mobility associated with urban agglomeration for non-beneficiaries of land reform. The slower growth in urban expansion and urban labor markets likely generated bottlenecks for robust and broad-based growth in the industrial and manufacturing sectors. This worked against the government's efforts to steer a portion of agricultural surplus from cooperatives to boost the industrial sector.

Furthermore, lower and more volatile incomes linked to land reform beneficiaries could have impacted other market participants through depressed consumption rates. Lower consumption among beneficiaries could have depressed demand for mechanized farming equipment, labor to make improvements in land, rural housing, rural infrastructure, and other consumer goods and services. This in turn could have forced non-land beneficiaries to employ their children as labor at higher rates to make ends meet.

While there are reasons to suggest that the bulk of our estimated effects derive from land beneficiaries, we nonetheless return to a discussion of these broader potential effects and their relative importance below.

2. Land reform and human capital accumulation in Peru

The expansion of education in Peru proceeded slowly at the outset of the 20th century but accelerated into the 1960s. From 1958 to 1968, the number of primary school students doubled and the number of secondary and university students nearly tripled, along with steep increases in education spending (Gall, 1974, 8). One out of every four Peruvians had some form of publicly sponsored education by 1967. Even so, educational attainment remained low: only 12 of every 100 pupils enrolled in kindergarten would finish secondary school (Gall, 1974, 7).

Peru in the 1960s lagged its peers on a number of social and economic dimensions, including schooling, despite its level of income per capita (Palmer, 1973). Many professional Peruvians attributed this to the lopsided and archaic agrarian structure. Land ownership and use remained a cornerstone of the Peruvian economy. Census calculations indicate that 50% of the economically active population in 1961 worked in agriculture. But the largest 1% of landowners held 80% of private land. Land tenure relations varied from relatively modern if still unequal in coastal export-oriented enterprises to archaic in many parts of the country, especially in the semi-feudal highland haciendas. Many workers in highland haciendas were indebted, underpaid, or forced to contribute free labor to the agricultural and household needs of the hacienda (Mayer, 2009; McClintock, 1981). Large landowners systematically sought to keep their workers uneducated in order to limit their mobility to other economic sectors and to ensure that they failed to meet literacy requirements for voting. This resistance was even more important given that the Peruvian state had delegated the provision of local education to large landowners in many rural parts of the country.

Popular pressure for land reform in the mid-20th century that came through labor strikes and land invasions was systematically stymied by large landowners in the legislature. But when the military seized power in October 1968, it set out to restructure Peru's economy and society.

A centerpiece of the military's reform efforts was a wide-ranging land reform. General Velasco initiated land reform in 1969 through Decree Law 17,716. The law instituted landholding ceilings at or below 150 hectares (depending on location) and it expropriated land, capital assets, and animals on properties that exceeded the ceiling. It also expropriated land that was farmed under forced labor. The reform typically redistributed land as cooperatives to former enterprise or hacienda workers that had worked on the property that was expropriated. Indigenous communities adjacent to expropriated properties also received land access in some cases (Cleaves and Scurrah, 1980; Mayer, 2009; McClintock, 1981).

The fulfillment of longstanding peasant demands for land and the removal of abusive landowners made the land reform popular at first (Mayer, 2009). This was amplified by the government's heavy support of most cooperatives in the early 1970s. Individuals did not need to request or apply for land. Land was granted in a top-down fashion as the

[4] Even so, the lack of formal land titles and secure property rights within cooperatives distorted migration and land use patterns, as it did within *ejidos* in Mexico (see de Janvry et al., 2015). An analogous effect of informality distorting labor allocation and increasing child labor has been observed in urban Peru (see Field, 2007).

[5] Schooling beyond the elementary level in Peru at this time (and for several decades hence) was still restricted to more urban areas.

[6] Insurance to protect against crop failures was not readily available in Peru since property informality made the government the principal source of lending, credit, and insurance, which was often underprovided.

[7] See Goetghebuer and Platteau (2010) for a discussion of the relevance of these costs in rural Peru.

government expropriated it and fashioned cooperatives. The two principal forms of land reform cooperatives were known as CAPs and SAISs.[8] Cooperatives did not own full title to the land until they repaid the government. Consequently, cooperative members did not enjoy free disposal: the only opportunity to exit was to forfeit land access and a share in the cooperative's profits. Furthermore, cooperatives had a statutory labor requirement tied to membership (Brass, 2007). Profit sharing within cooperatives, while not directly proportional to work effort, was intended to be proportional to the individual contribution of each member in working days (Matos Mar and Mejía, 1980).

Cooperatives began to break up in the 1980s and devolve land to their members. But individuals rarely received land titles to their property. And until Legislative Decree No. 653 in 1991, there were restrictions on the subdivision of land received through the land reform as well as restrictions on land sales, transfers, rentals, mortgaging, and inheritance. Even then, land markets remained sluggish given property rights limitations on redistributed land that lasted into the 1990s. Major land formalization efforts did not begin until the mid-1990s.

Law 17,716 ultimately led to the expropriation of 10 million hectares of land, making it one of Latin America's most sweeping land reforms relative to agricultural land along with Bolivia, Chile, Cuba, Mexico, and Nicaragua (see Albertus, 2015, Ch. 4). Land reform was most intense from 1969 to 1976. It then slowed under General Morales Bermúdez, especially once a severe economic crisis hit in the late 1970s. Land reform largely halted when Peru transitioned back to democracy in 1980, though there were several trailing land expropriations in the early 1980s.

The elimination of large landowners paved the way for an expansion of education in rural areas. Velasco himself highlighted this in his first annual message to Peru: "An overall and realistic approach to the problem of illiteracy deserves special attention through the development of an authentic rural school intimately linked to the actions of the agrarian reform." The government formally appointed an Education Reform Commission in November 1969. It produced a report followed by a draft law on education, and it decreed a new General Education Law in 1972 covering all levels and programs of education.

The education reform endorsed instruction in vernacular languages in indigenous areas and developed localized school district networks (*nucleos*) that encouraged community participation. *Nucleos* consisted of a central schoolhouse providing full coverage of primary education and satellite schools in the surrounding countryside providing education through second or third grade (Gall, 1974, 12). Satellite schools focused mainly on agricultural training and sought to holistically improve health, poverty, and education. This system was modeled on a successful school plan financed with aid from the United States starting in the 1940s and 1950s in southern Peru. The World Bank contributed loans for new school construction.

To be sure, education reform under the military was not without obstacles. The first major obstacle was the founding of the Peruvian Teacher's Union (SUTEP), which attracted many teachers and came to oppose the regime's efforts to retrain them, eventually through strikes and other activities. The military, however, was hardly receptive to this type of feedback. This related to a second obstacle: the labyrinthine Ministry of Education bureaucracy that the military inherited. The military imposed centralized control of education but struggled to get bureaucrats to implement their reforms in a faithful and timely manner. Nonetheless, one previously major obstacle to the expansion of education was removed with land reform: large landowners.

How did Peru's land reform ultimately impact educational attainment in Peru? Despite its enormous scope and importance, researchers have not previously collected comprehensive local data on the land reform and linked this data with later education outcomes. Nonetheless, many scholars concluded that the gains were positive, especially among former hacienda workers that were included in the new cooperatives. These individuals "were no longer concerned with subsistence, but with secondary-school education for their children in the provincial capital" (McClintock, 1981, 66). Similarly, Mayer (2009, xviii) concludes that these peasants "were materially, socially, and economically much better off than before the reform."

Other scholars, however, reach a different conclusion regarding the final balance of Peru's land reform. Matos Mar and Mejía (1980) conclude the reform program failed since it did not eliminate agricultural underdevelopment and peasant marginalization. Several reasons include a lack of complementary economic policies (del Castillo, 2003; Matos Mar and Mejía, 1980), limits to the amount of land redistributed (Caballero and Alvarez, 1980), and distortions to incentives driven by the distribution of land in cooperative settlement schemes such that individuals lacked secure property rights and tradeable land rights (del Castillo, 2003; Matos Mar and Mejía, 1980).[9]

The rest of this paper represents the first empirical attempt to investigate how land reform exposure impacted individual-level educational attainment in the wake of reform. We find that land redistribution unleashed a series of unintended consequences that ultimately led individuals in districts where land reform was intensely implemented to complete fewer years of schooling than similar individuals that were exposed to very little land reform or entirely unexposed.

3. Research design

To examine the impact of Peru's land reform on subsequent educational attainment, we combine pooled data from large-scale nationally representative household surveys with data on the timing and location of the land reform. This enables the estimation of a difference-in-differences specification in which an individual's district of birth and date of birth jointly determine their exposure to the land reform and the rural conditions that either preceded or postdated it.

This type of age-cohort analysis had been used in other studies to estimate the impact of a massive school construction program in Indonesia on educational achievement (Duflo, 2001; Akresh et al., 2018), the effect Peru's civil conflict on human capital accumulation (Leon, 2012), the long-run impacts of agricultural shocks on educational attainment in the U.S. (Baker et al., 2018), and the consequences of Burundi's civil war on children's health status (Bundervoet et al., 2009) and school attainment (Akresh and de Walque, 2010).

Identifying an individual's year of birth and district of birth allows us to calculate how old each individual was when the land reform began and therefore their exposure to the reform.[10] Children in Peru typically attend primary school between the ages of 6–11 and secondary school between the ages of 12–17. Therefore, the schooling decisions made for children younger than 6 at the time of land reform occurred under "full exposure" to the reform: the choice of whether, and for how long, to send children to school for this cohort occurred in the aftermath of land reform. Children who were of primary or secondary school age received "partial exposure" to land reform, since schooling decisions of whether to attend school, and for how long, were already in process at the time the land reform occurred. In contrast, individuals aged 18 or older at the time of land reform are unlikely to have been affected by it: most schooling decisions for these individuals were already made by the time land

[8] See McClintock (1981) for more discussion of cooperative structures.

[9] Early observers of the land reform legislation such as Lafosse de Vega-Centeno (1969) anticipated that the property rights scheme created incentives that would reduce out-migration and income diversification via working in non-agricultural activities.

[10] We use district of birth to proxy for where an individual was educated during childhood given that data on the latter are not available. However, according to census data, about 85% of individuals under 18 years old lived in their place of birth in 1981 (the year of Peru's oldest census with available microdata). Furthermore, we later verify that the results remain robust when restricting the sample to individuals who remained in their district of birth during their lifetime.

reform began in their district of residence during childhood. We use this information to compare the educational attainment of those affected by land reform at young ages with that of older cohorts that were unaffected by the land reform or only partially affected.

In addition to variation across birth cohorts, we consider the variation across districts in the intensity of land reform. Some districts were blanketed with intense land reform that covered most or all of the agricultural land area. Land reform was less extensive in other districts. And some districts experienced little or no land reform.

Unfortunately, data on whether individuals were direct land beneficiaries were never collected and are unavailable. Our measure of land reform exposure is constructed at the district level and does not differentiate between beneficiaries and non-beneficiaries. The effects we estimate across individuals are therefore best identified at the district level. However, we leverage additional data in analyses discussed below that suggest that the effects were likely stronger among beneficiaries. This is consistent with the abrupt, involuntary, and transformational nature of receiving land and the inability to freely dispose of it for many years. Furthermore, data on district-level rural populations and the overall number of land beneficiaries across Peru suggest that the density of land reform beneficiaries within districts was generally quite high where land reform occurred. A reasonable estimate is that land reform impacted on average about half of the rural population where it was implemented.[11] Because most land was received by middle-class peasants that previously labored on large estates (McClintock, 1981), most or all members of this economic class typically became beneficiaries where land reform occurred.

Exploiting birth cohort and geographical variation, we can estimate the effect of land reform on educational attainment using the following regression specification:

$$y_{ijt} = \alpha + \sum_{l=0}^{24-1} (LR_j \cdot d_{il})\beta_l + X'_{ijt}\delta + \omega_j + \phi_t + \gamma_p(t) + \varepsilon_{ijt} \quad (1)$$

where y_{ijt} is the number of years of schooling completed by individual i born in district j in year t. LR_j is a measure of land reform intensity in district j and d_{il} is a dummy that indicates whether individual i is age l in the year land reform began in their district of birth.[12] Individuals aged 24 at the time land reform began form the control group, and this dummy is omitted from the regression. X_{ijt} is a vector of individual time-invariant characteristics, such as gender or ethnicity. ω_j are district of birth fixed effects, ϕ_t are cohort of birth fixed effects, and, following Leon (2012), $\gamma_p(t)$ are flexible province-specific trends that account for the differential developments of each province of the country through time. For example, provinces may differ in intensity of the construction of schools, directly capturing the supply side of education. Finally, we cluster standard errors by district of birth to allow for an arbitrary variance-covariance structure within birth districts.

Each coefficient β_l in Equation (1) can be interpreted as an estimate of the impact of land reform on a given birth cohort. This approach also allows us to construct an important placebo test of whether, as anticipated, the β_l are equal to 0 for $l \geq 18$, since the land reform should not impact basic educational decisions for cohorts that were past the basic schooling age when land reform occurred. We confirm this empirically below.

We then impose a restriction on Equation (1) to assume that all β_l such that $l \geq 18$ are actually zero. The equation is then estimated as follows:

$$y_{ijt} = \alpha + \sum_{l=0}^{17} (LR_j \cdot d_{il})\beta_l + X'_{ijt}\delta + \omega_j + \phi_t + \gamma_p(t) + \varepsilon_{ijt} \quad (2)$$

where the omitted group is now comprised of individuals aged 18 to 24 at the time land reform started in the district of birth. This is more efficient and leads to more precise estimates of the impact of the reform.

One potential concern about our estimation approach is the possibility of errors in individuals' reported age, which may lead to an erroneous assignment of land reform exposure (see, e.g., Leon, 2012). Missing information on the month of birth or mistakes in reported age (e.g., due to transcription errors or the lack of a birth certificate) could lead us to assign land reform exposure with a margin of error. We minimize this potential problem by principally analyzing land reform exposure across age cohort groups rather than assigning it to specific years of birth. Specifically, we define four groups: "Fully exposed" (aged 0 to 5 when land reform began), "partially exposed" (aged 6 to 11), "weakly exposed" (aged 12 to 17), and "not exposed" (aged 18 to 24).[13] The estimation of Equation (2) is therefore mainly conducted using age cohort group dummy interactions instead of age cohort year dummy interactions, though we also present the main results using age cohort years.

4. Data

To evaluate the effects of land reform on educational attainment in Peru, we analyze original district-level data on land transfers from 1969 to 1985 along with individual-level data on educational attainment and other covariates from annual household surveys.[14] We minimize the extent to which changing political geography impacts the results by assigning individuals to districts that prevailed at the time land reform began. We also create several composite units of districts where large expropriated properties spanned district boundaries. This yields 1,598 total districts covering Peru.

4.1. Land reform exposure

We constructed an original dataset that registers all land expropriations in Peru from 1969 to 1985 in order to determine an individual's exposure to land reform. This dataset documents nearly 15,000 expropriation decrees covering 10 million hectares of land in active private use, abandoned or long-fallowed private land, and agriculturally unproductive (often public) land (*eriazos*). We constructed these data by composing a research team in Lima to conduct a comprehensive search through publications of the official government daily, *El Peruano*. By law, *El Peruano* published all supreme decrees, supreme resolutions, and ministerial resolutions that expropriated individual properties. These decrees and resolutions contain detailed information regarding district location and the amount of land expropriated for subsequent transfer to local rural workers.[15]

The land reform intensity measure we construct captures the

[11] Data from Matos Mar and Mejía (1980, 182) indicate 375,246 land reform beneficiaries from 1969 to 1979. Relative to a rural population of 4.1 million in 1961 that were located in coastal and sierra districts later affected by land reform, this translates into an estimated 48% of the rural population receiving land given a household size of 5.2 based on the 1961 census.

[12] We lagged the land reform variable by two years since the process of land affectation, expropriation, and subsequent distribution typically took one to two years (Matos Mar and Mejía, 1980, 115–119). The main results are very similar and robust if land reform is not lagged or if it is instead lagged by one period.

[13] The results are robust to the specific choice of ages grouped together.

[14] We also conducted a similar analysis using individual-level data from 1981 to 1993 population censuses. The results are similar (Appendix Figure A.1 reports the main results using the 1993 census data). We rely mainly on results using the household surveys because these surveys contain more detailed individual-level information that can help with discerning mechanisms.

[15] In a small number of cases, especially toward the end of the 1970s, expropriated properties were returned in part or whole to their previous owners. These reversions typically occurred before the land was redistributed to reform beneficiaries. Because land reversions were also conducted through official decrees, we track them and remove them from our measure of land reform intensity.

Table 1
Summary statistics of Peru's land reform (district level).

District-level variables	Mean	Median	Min	Max	Std. Dev.	Sum	N
Land reform measure							
% of expropriated land	41%	32%	0%	100%	35%		990
1st *tercile*	4%	2%	0%	14%	4%		330
2nd tercile	33%	32%	14%	56%	12%		330
3rd tercile	85%	89%	56%	100%	15%		330
Other statistics of the land reform							
Agricultural land (ha)	14,869	6427	1	267,303	24,028	14,700,000	990
Expropriated actively used private agricultural land (ha)	6246	1817	0	151,543	12,227	6,183,336	990
Year of beginning of land reform	1973	1973	1969	1984	2.7		990
Year of end of land reform	1975	1976	1969	1985	2.4		990
Duration of land reform (years)	3	2	1	13	2.0		990
% Not affected by land reform	29%	0%	0	1	46%		1402

Notes: Only districts located in the coast and highlands regions are considered. All but the last row considers districts with some land reform.

percentage of actively used private agricultural land in a district that was redistributed via the main land reform legislation from 1969 to 1985, which covered the coastal and highland sierra regions of Peru where the vast majority of the population lived. We divide land redistribution by agricultural land area since districts vary in size and in the amount of land that is used for agricultural purposes. We measure agricultural land area from the 1972 agricultural census, which is the census most proximate to the initiation of land reform.[16] Across the duration of the land reform, most land redistributed in the coastal and highland regions was in active private use (76% of land redistributed). This was the most valued land, and we focus on it as the centerpiece of the land reform. A smaller portion of distributed land was either abandoned or long-fallowed (21%). Remaining lands were agriculturally unproductive and typically public (3%) and were covered by different legislation than private lands. The distribution of public lands did not remove large landowners as occurred with private lands, and most of this land reform occurred in the sparsely populated Amazon basin.[17]

We restrict the data to districts where at least some land reform took place. This excludes mainly urban districts where land reform is not especially relevant as well as other unaffected districts, such as those from non-agricultural areas or areas where large properties subject to expropriation were absent (this comprises 28% of all districts). We do this because individuals from such districts are unsuitable as a control group for individuals in rural districts where at least some land reform occurred.[18] Finally, we exclude 162 districts from the Amazon region that were not covered by the main land reform legislation (Law 17,716).

Table 1 presents descriptive statistics on land reform expropriations at the district level. The first row presents the selected measure of land reform intensity at the district level. On average, 42% of district agricultural land area was expropriated, while the median level was 35%. The bottom third of districts had less than 14% of agricultural land expropriated. Districts in the middle third had between 14% and 56% of agricultural land expropriated. The remaining group of districts received high intensity land reform with over 56% of agricultural land was expropriated. Fig. 1 displays the geographical distribution of the land reform intensity variable at the district level. Land reform was most heavily concentrated in the southern and central highlands and in the north coast.

Overall, more than 6 million hectares of private land in active use was expropriated through the land reform between 1969 and 1985. This figure represents 42% of the agricultural land of coastal and sierra regions.[19] In the average district, land reform started in 1973 and ended in 1975 and it had a median duration of two years. This indicates that the process of land reform itself was rather quick within districts. Land reform endured three years or less in over 75% of districts and four years or less in nearly 85% of districts.[20]

4.2. Education outcomes

The individual-level data we utilize comes from a pooled sample of households from the Peruvian National Household Survey (ENAHO) for the period 2004–2017. ENAHO is the official household survey run by the Peruvian National Institute of Statistics to monitor household living conditions and poverty. This annual survey is representative at the national, urban and rural level, and covers detailed data about households and individual economic, demographic, and social characteristics. Most importantly for our purposes, it collects information on when and where individuals are born, where they currently live, and how many years they have attended school. Between 2004 and 2017, the average number of households surveyed per year was 25,000.

The outcome of interest is educational achievement measured as the number of completed years of primary and secondary education.[21] To

[16] These data are not available at the district level in previous censuses; the subsequent agrarian census only occurs in 1994.

[17] The results that follow are robust to alternative definitions of the land reform measure that focus on private land (see Table A.1). By contrast, a measure of land reform that focuses only on the expropriation of agriculturally unproductive public lands that are sparsely populated yields null findings, as anticipated. This serves effectively as a placebo test for the main findings.

[18] Districts not affected by the land reform had less agricultural land and a greater share of their population living in urban areas. Appendix Table A.2 demonstrates that while the main results remain strong and statistically significant for fully and partially exposed cohorts when enlarging the sample to include individuals born in districts where land reform was not particularly relevant, the effects of land reform are unsurprisingly attenuated. Table A.2 also explores the impact of land reform in further alternative samples of districts that are theoretically relevant.

[19] To double check the consistency of our data, we compare our database of expropriations with other sources that report the aggregate number of hectares expropriated by the agrarian reform. For example, according to Matos Mar and Mejía (1980), one of the authoritative books on Peru's land reform, the reform expropriated about 9.1 million hectares (including actively used agricultural private land, abandoned land and *eriazos*) between 1969 and 1979; according to our database, 9.3 million hectares were expropriated in that period.

[20] Table A.3 displays the distribution of Peruvian districts as well as our sample of individuals according to the duration of land reform exposure in years. The relatively few districts in which the land reform process was long limits the ability to construct a robust comparison group to test whether exposure to more enduring land reform had a greater negative effect on education than exposure to shorter land reform. Nonetheless, our hypothesized mechanisms hinge mainly on exposure to land reform itself as opposed to its duration. This supports using measures of the share of land expropriated locally. However, research in contexts where the process of land reform itself was more lengthy might fruitfully examine whether temporal duration also impacts human capital accumulation.

[21] In the main regressions we truncate the years of education variable at 11 years, which corresponds to the completion of the secondary schooling cycle in Peru. The main results are unchanged if truncation is dropped.

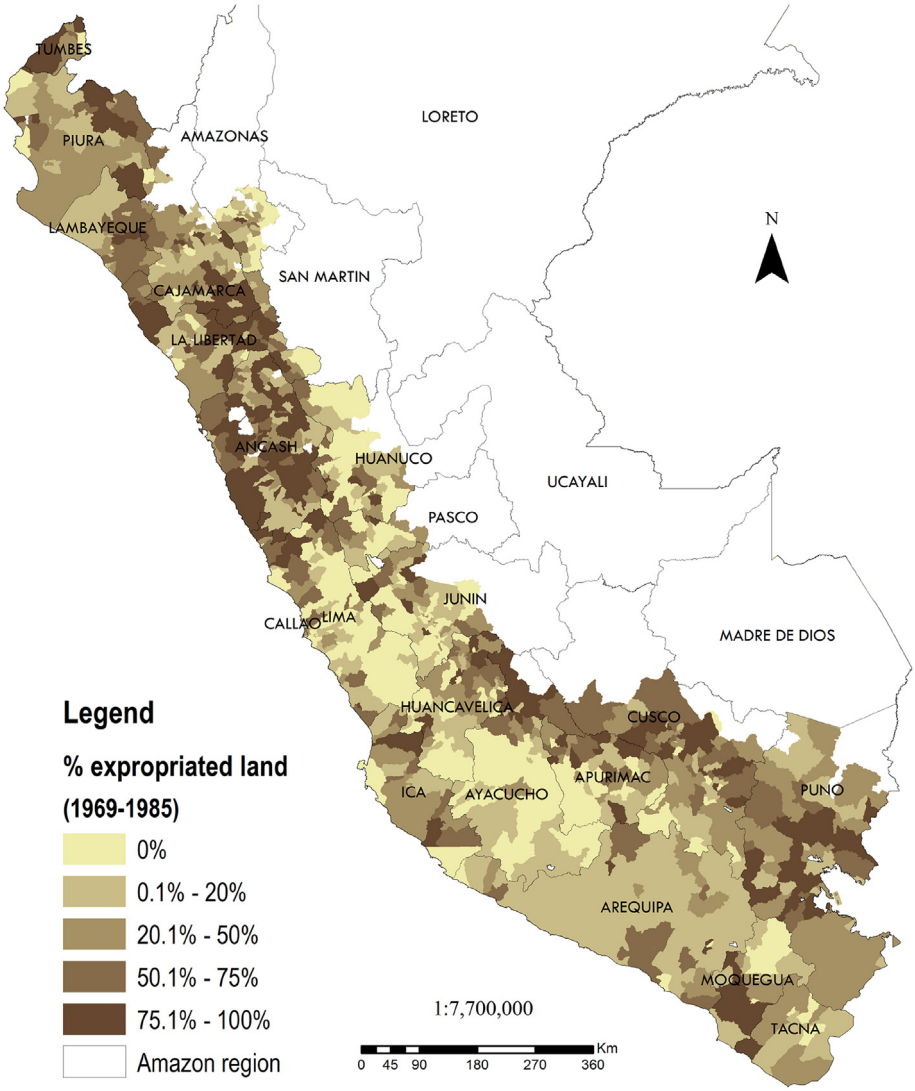

Fig. 1. District-level Land Reform Intensity. Notes: Colored district are those from coastal and highland regions (1,402 districts from 1972).

Table 2
Summary statistics from household surveys (individual level).

Variable descriptions	Mean	Min	Max	Std. Dev.	Median	N
PANEL A						
Years of schooling (primary and secondary education)	7.18	0.0	11.0	4.0	8.0	194,068
Completed primary school	0.72	0	1	0.45	1	194,068
Completed secondary school	0.43	0	1	0.50	0	194,068
Age at the beginning of land reform in the district of birth	10.95	0.0	24.0	7.0	10.0	194,068
Age at the year of ENAHO survey	47.21	20.0	70.0	8.5	47.0	194,068
PANEL B						
Female gender	0.52	0	1	0.50	1	194,068
Native mother tongue	0.29	0	1	0.46	0	194,068
PANEL C						
Hold land for agricultural purposes at the year of ENAHO survey	0.39	0	1	0.49	0	194,068
Lived in a rural town at the year of ENAHO survey	0.32	0	1	0.47	0	194,068
Lived in a province different to the province of birth at the year of ENAHO survey	0.42	0	1	0.50	0	194,068
Log (household total income at the year of ENAHO survey+1)	9.84	0.0	14.0	0.9	9.9	194,068

Notes: The sample is composed of individuals aged 0 to 24 at the time the land reform became effective in their district of birth and who were born in districts that had at least some land reform and were located in coastal or highlands districts where the main land reform legislation applied.

ensure that the sample of individuals is old enough to have finished their schooling cycle at the time of data collection, we focused on men and women who were at least 18 years old at the time ENAHO surveyed them. Furthermore, as mentioned above, we restrict the data to individuals born in districts where at least some land reform took place to ensure a suitable control group.

Summary statistics of schooling achievement for the final sample are presented in Table 2 (panel A). There are 194,068 individuals in the sample, with an average level of 7.18 years of primary and secondary schooling completed. About 72% of individuals completed at least 6 years of education, which corresponds to graduation from primary school, while 43% completed at least 11 years of education, which corresponds to graduation from secondary school.

Panel A also indicates that individuals were on average 11 years old when land reform first began in their district of birth and 47 years old at the time of the ENAHO survey.

4.3. Controls

The ENAHO surveys also collect a host of other individual characteristics linked to gender, native language, employment activity, landholding, and income and expenses. We employ some of these as controls since individuals may differ in educational attainment for reasons other than land reform, such as gender. We also use ENAHO data to examine the mechanisms whereby land reform impacts education.

Table 2 (Panel B) reports descriptive statistics of two important control variables used in the analysis: gender and native language (which proxies for indigeneity). The sample is composed of 52% women. Meanwhile, 29% of the sample had a native language as their mother tongue. Panel C of Table 2 provides descriptive statistics for other relevant variables used to investigate mechanisms and heterogeneous effects across different populations. We discuss these in greater detail as they arise.

5. Empirical results

We present the results in five sections. First, we present the main results. We next analyze the parallel trends assumption underpinning identification. Third, we examine several important mechanisms through which land reform may have impacted educational attainment with a focus on likely land beneficiaries. Fourth, we provide additional evidence that land reform impacted educational attainment in the long term by depressing demand. Finally, we examine the extent to which effects were likely concentrated among beneficiaries versus operating more generally at the local level.

5.1. Long-term effects of land reform on educational attainment

We begin by presenting the overall effects of being exposed to land reform in childhood or adolescence on educational attainment as measured by the number of years of schooling. Fig. 2 plots the estimated coefficients for β_l from equation (1) along with associated 95 percent confidence intervals. These are coefficients of the interaction between a dummy variable for an individual being a certain age at the year land reform began in their district of birth and the intensity of land reform in that district. Each point estimate β_l for a given age cohort can be interpreted as a measure of the impact of all of a district's land being affected by land reform on educational attainment within the cohort.[22]

Fig. 2 coefficients are negative and statistically distinguishable from zero for cohorts aged 0–11 in the year land reform began in their district. The confidence interval of the coefficients begins to overlap with zero for cohorts older than 11 years old, and the coefficients themselves approach zero for cohorts aged 18 or greater.

The results suggest that land reform slowed human capital accumulation for children in districts where land reform occurred prior to beginning primary school (below 6–7) or during primary school age. For example, exposure to complete land reform in a district at the age of 5 yields an estimated 0.37 fewer years of education ultimately attained. As anticipated, the reform had no substantial effect on the education of cohorts that were past the basic schooling age when land reform occurred (i.e., older than 17). Given our estimation equation and that the overall trend in educational attainment in Peru has been strongly positive over time (see Figure A.2), the findings indicate that individuals exposed to land reform fell behind the educational attainment of unexposed individuals as opposed to experiencing an absolute reduction in educational attainment vis-à-vis the pre-reform period.

Table 3 presents a related set of results by analyzing land reform exposure across age cohort groups rather than specific years of birth. Column 1 presents an unrestricted model where the omitted comparison group is comprised of individuals that were 24 years old at the time land reform began in their district of birth. Column 2 presents a restricted model, following equation (2), where the omitted group is comprised of post-primary and secondary school aged individuals that were 18–24 years old when land reform began in their district of birth. This model yields more efficient estimates of the effects of land reform. The estimates suggest that children exposed to blanket land reform covering all of a district's agricultural land before entering school accumulated an estimated 0.32 fewer years of schooling on average upon reaching adulthood relative to unexposed individuals. The effect of blanket or full land reform on children exposed at ages 6 to 11 was a reduction of 0.27 years of schooling, while the effect on children exposed at ages 12 to 17 was a reduction of 0.16 years of schooling.

[22] The sample mean level of the land reform intensity measure is 47% and the median 41%. These statistics are calculated by individuals whereas those in Table 1 are calculated by districts.

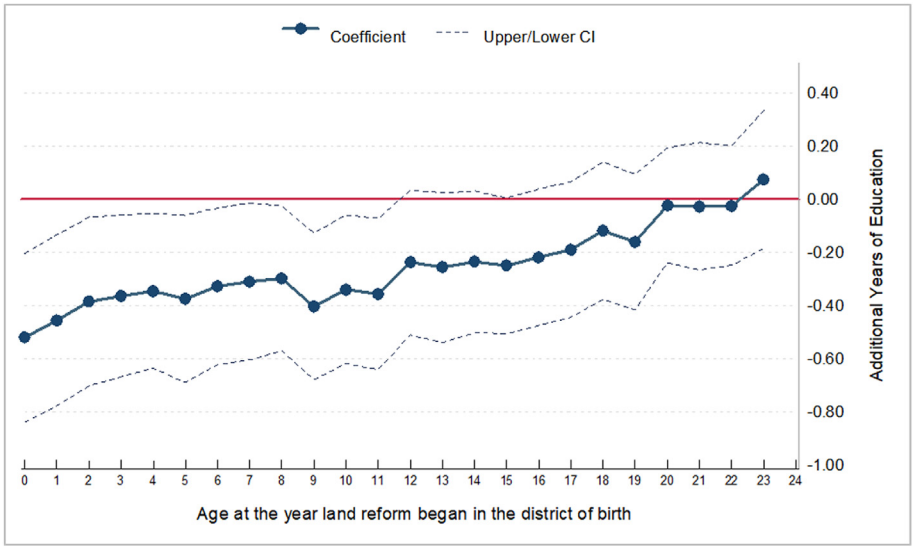

Fig. 2. The Long-Term Impact of the Land Reform on Educational Attainment. Notes: Fig. 2 presents the coefficients of the interactions between dummies indicating individuals' age at the beginning of land reform in their district of birth and the intensity of land reform in their district of birth. The dependent variable is the number of years of primary and secondary schooling. The control variables included are gender, mother tongue, district fixed effects, year of birth fixed effects, and a province level cubic trend. The omitted group is composed of individuals aged 24 at the year land reform began in their district of birth.

Not all districts received full land reform saturation, however. The sample mean value of this measure among individuals is 0.47. This implies that relative to the unexposed cohort, the *average* effect of land reform was a reduction of 0.15 years of education for the fully exposed cohort, a reduction of 0.13 years of education for the partially exposed cohort, and a reduction of 0.07 years of education for the weakly exposed cohort. These estimated effects are substantial even when compared to other types of extreme events or direct policy interventions aimed at schooling.[23]

Fig. 3 further explores the negative impact of land reform exposure on educational attainment. The figure shows the impact of high land reform exposure on the likelihood of completing at least a certain number of years of education. These effects were captured by an interaction term between a cohort dummy for the fully exposed cohort (aged 0 to 5 when land reform began) and the intensity of land reform in the district of birth.[24] Each coefficient was estimated by a different linear probability model, where the dependent variable in each regression was a dummy that indicates whether the individual attended at least n-years of schooling, for n = 1, 2, 3, …, 18. As for Fig. 2, the models include gender and mother tongue as control variables. They also include district fixed effects, year of birth fixed effects, and a province level cubic trend.

The shape of the top portion of Fig. 3 indicates at what levels of education the land reform undermined school attendance. The negative effect is increasing until the eighth year of education, decreasing until the eleventh, and not statistically distinguishable from zero thereafter. For instance, individuals exposed to complete land reform in their district of birth reduced the likelihood of completing at least primary education (6th grade) by 5.2 percentage points, while the impact on the probability of completing secondary education (11th grade) was a reduction of 3.6 percentage points. The bottom portion of Fig. 3 indicates the percentage of individuals within the fully exposed cohort (aged 0 to 5) that reach each n-years of education.

Table 3
The long-term impact of the land reform on educational attainment.

	(1)	(2)
	Years of Schooling	
	Unrestricted Model	Restricted Model
Exposure (Age at LR in district of birth)		
Fully Exposed (0–5)	−0.337**	−0.321***
	(0.138)	(0.097)
Partially Exposed (6–11)	−0.289**	−0.274***
	(0.122)	(0.083)
Weakly Exposed (12–17)	−0.174	−0.158**
	(0.112)	(0.062)
Not Exposed (18–23)	−0.016	
	(0.099)	
Controls		
Mother tongue (native = 1)	−2.714***	−2.714***
	(0.062)	(0.062)
Gender (female = 1)	−1.575***	−1.575***
	(0.066)	(0.066)
Constant	6.521***	6.523***
	(0.140)	(0.139)
District fixed effects	YES	YES
Year of birth fixed effects	YES	YES
Province time trends	YES	YES
R-squared	0.385	0.385
Observations	194,068	194,068
Districts	990	990

Notes: *p < 0.10, **p < 0.05, ***p < 0.01 Robust standard errors clustered by district of birth in parentheses. The sample is composed of individuals aged 0 to 24 at the year land reform (LR) began in their district of birth. The dependent variable is the number of years of primary and secondary schooling. The explanatory variables that capture the impact of land reform on education are interaction terms between dummies indicating age group at the beginning of land reform in their district of birth and the intensity of land reform in the district of birth. District fixed effects, year of birth fixed effects, and a province level cubic trend are included but not reported.

5.2. Analyzing the parallel trends assumption

There is a critical assumption underlying our empirical strategy that underpins the conclusion that exposure to land reform exposure had a negative impact on educational attainment: the parallel trends

[23] For instance, Leon (2012) finds that children exposed to violence during Peru's civil conflict (1980–2000) suffered an average reduction of 0.10 years of education. Duflo (2001) finds that the effect of the massive school construction program in Indonesia on school attainment is only slightly larger, but in the opposite direction: children exposed to the average of new schools constructed experienced an increase of 0.22–0.27 years of education.

[24] Alternative exposure definitions yield similar results. Specifically, we replicated Fig. 2 considering individuals aged 0 to 11 as the exposed cohort (fully plus partially exposed), and separately considering individuals aged 0 and 17 as the exposed cohort (fully, partially, or weakly exposed). The results are in Appendix Figure A.3.

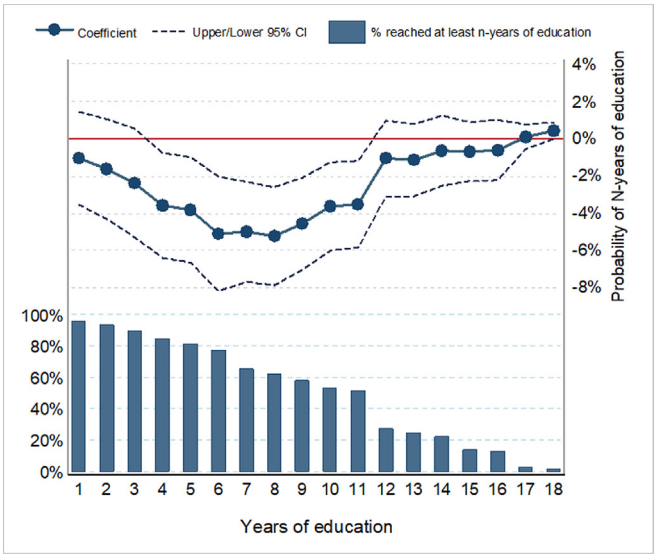

Fig. 3. The Effect of Land Reform on the Probability of Reaching at Least N-years of Education. Notes: The coefficients depicted in the upper part of Fig. 3 represent the impact of high exposure to land reform on the probability of completing at least n-years of schooling in percentage points. We show estimated regression coefficients and their respective 95% confidence intervals. The sample is composed of individuals aged 0 to 5 (young cohort) and 18 to 24 (old cohort) at the year land reform began in their district of birth. The lower part of the figure presents the percentage of individuals aged 0 to 5 at the year land reform began that reach each n-years of education.

assumption. The parallel trends assumption in this case is that the change in educational attainment across birth cohorts in the districts more affected by the land reform should have been the same as the change across birth cohorts in the districts less affected by the reform in the run-up to reform and in its absence. However, it could be the case that educational attainment patterns differed between regions that were more versus less affected by the land reform even prior to the reform, setting the stage for further divergence. For instance, there could be systematic differences in school construction or we could simply be picking up a lagged "catching-up effect" if the land reform targeted less educated regions.[25]

To provide evidence in favor of the parallel trends assumption, we estimate placebo regressions of equation (1) in which we compare educational attainment among post-school age cohorts aged 18–41 at the time land reform began in their district. If the parallel trends assumption holds, educational attainment across cohorts in this age group should not differ systematically across districts that were more or less affected by the reform. The results, depicted in Fig. 4 above, show there are no differential time trends in educational attainment prior to the land reform, since the estimated effect of land reform on years of primary and secondary education is statistically indistinguishable from zero for all age cohorts that were past the schooling age when the reform occurred. These results provide evidence that the main differences in differences estimates are not driven by the existence of non-linear pre-reform trends.

5.3. Mechanisms linking land reform to educational attainment

The results thus far indicate that land reform exposure hampered educational attainment overall in Peru. But what were the mechanisms that drove this outcome? We examine two principle, related mechanisms: an economic opportunity mechanism and an income and child labor mechanism. We focus our discussion of mechanisms first mainly on land reform beneficiaries. We then discuss evidence that the effects were likely largest among this group, though they could have spilled over locally to non-beneficiaries as well.

5.3.1. Economic opportunity and rural stasis

The first mechanism we analyze is linked to economic opportunity. Land reform loosened the restrictions on labor mobility that large landowners had enforced in many regions of Peru. It simultaneously granted beneficiaries land access through cooperatives. But beneficiaries could not legally withdraw land from cooperatives or "cash out" by selling their share in the cooperative and the collectively owned land and then taking that money to purchase land elsewhere or move to an urban area. Beneficiaries consequently had access to a valuable asset but did not have rights to alienate their land and invest the sale proceeds elsewhere. Nonetheless, they had a degree of flexibility in allocating their labor to other pursuits once their work obligations were met within the cooperative. Land reform beneficiaries differed in their ease of migrating seasonally and in accessing markets to supplement their income with off-farm labor. For beneficiaries that were shut off from these opportunities, which was quite common in many of the remote rural areas where land reform occurred, they were more likely to focus solely on farming rather than searching for off-farm occupation, generating contemporaneous and intergenerational rural stasis. Furthermore, because of more limited opportunities to sending their children to schools in nearby urban centers, children of these land reform beneficiaries were more likely to drop out of school earlier to work on the farm.

We assess this mechanism using several pieces of data. First, we split the sample of individuals according to the distance of their district of birth to local markets. This variable measures the geographical distance (in kilometers) of the district of birth to the nearest town of 3,000 of more inhabitants circa the 1961 census.[26] Second, we split the sample according to the road density in individuals' district of birth. Both of these variables capture ease of access to outside economic opportunities.

Third, we split the sample by potential land productivity. Where land quality received through the land reform was poor, families were forced to work the land longer to make a living. This also made it less likely that families could afford to send their children to school. Land productivity is measured with satellite data according to potential annual agricultural output (measured in log calories per hectare) based on crop yields for crops suitable for cultivation in a district according to climatic factors (e.g., temperature and rain), soil characteristics, and geography (Galor and Özak, 2016). Productivity data are averaged within districts over 5 arc-minute grid cells.

Table 4 depicts the results. Columns 1–4 indicate that the negative impact of land reform on educational attainment is concentrated in areas with lower accessibility to outside economic options, whether measured via distance to the nearest local market or via the density of local road networks.[27] Likewise, Columns 5 and 6 reveal that this negative impact is stronger for districts with lower potential land productivity compared to those with higher potential.

Together, these results suggest that the reform had a stronger negative effect on educational attainment among individuals that were living in places with more limited income opportunities inside and outside agriculture. This could be because it was harder for individuals located in these areas to supplement income via working part-time in nearby cities or in other non-agricultural jobs locally. It is likely that people were more

[25] We also confirmed that our results are robust to controlling for a district's pre-reform literacy rate (1961). See Appendix Table A.4.

[26] The geographic distance is calculated by measuring the length of the shortest path between the district of birth and the nearest town of 3000 of more inhabitants along the surface of a mathematical model of the earth (see Picard, 2010). District of birth coordinates correspond to its centroid and town coordinates correspond to the district centroid location where the nearest town is located.

[27] In further related tests, we also found that the negative effects of land reform on educational attainment were concentrated in the Andes over the coast.

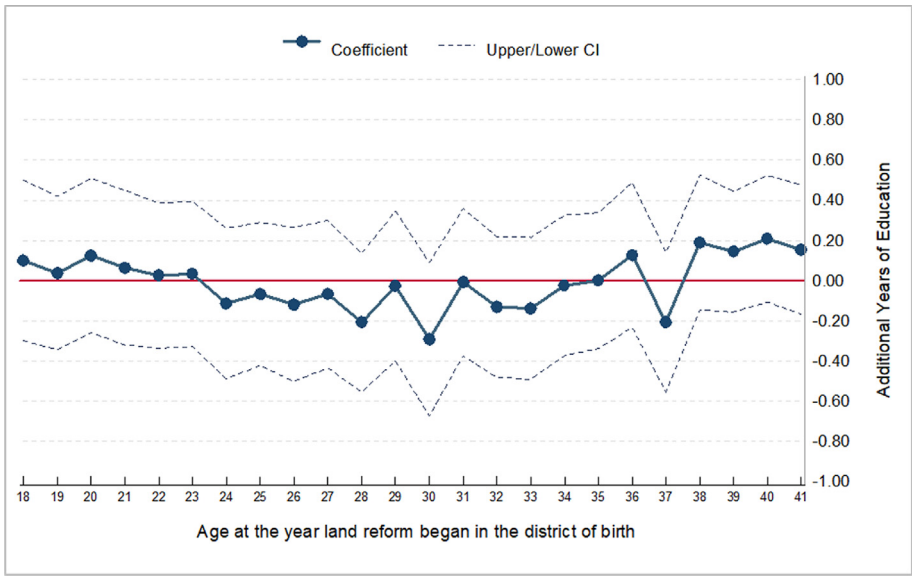

Fig. 4. Examining the Parallel Trends Assumption Using Older Age Cohorts. Notes: The figure presents the coefficients of the interactions between dummies indicating age group at the time land reform began in an individual's district of birth and the intensity of land reform in the district of birth. The sample is composed of individuals aged 18 to 42 at the year land reform began in their district of birth. The control variables included are gender, mother tongue, district fixed effects, year of birth fixed effects, and a province level cubic trend. The omitted group is composed of individuals aged 42 in the year land reform began in their district of birth.

encouraged to stay on the farm and work in agriculture in those cases.

Further analysis on the longer-term impacts of land reform on rural stasis and income support this conclusion. Table 5 presents a set of models estimated in a similar fashion to those in Table 3 Column 2 but with a different set of dependent variables. These models examine the effects of land reform on agricultural landholding, residence in a rural town, previous migration status, and household income.

It is important to mention, however, that Table 5 models have inferential limitations derived from the fact that, unlike our previous difference-in-difference estimates which make use of the fact that older cohorts in districts both affected by and unaffected by land reform were not exposed to land reform because they were past schooling age, older cohorts in these models could have been affected by broader land reform dynamics that spill over across age cohorts (e.g., migration capacity or income sharing). Therefore, the results should be interpreted as suggestive but not necessary causal.

The results indicate that land reform exposure is differentially associated with current landholding, residing in a rural town versus an urban area, and a lower likelihood of an individual having moved away from their place of birth. This suggests that while economic opportunity conditioned the impact of land reform on education as indicated in Table 4, the effects of land reform worked toward broader rural stasis in places where it was implemented. Furthermore, Column 4 indicates that land reform exposure is also linked to lower household income in the long term.[28]

5.3.2. *Income and child labor*

The second mechanism we analyze is linked to income and child labor. Peasants that received land through Peru's land reform program transitioned from working for large landowners, where pay was typically low and relatively stable, to members of cooperatives. While this transition enabled them to capture a greater portion of the productivity of their labor vis-á-vis the pre-reform era, income also became more volatile and uncertain. This made the prospect of sending children to school, as opposed to having them supplement family income through working, relatively riskier.

At the same time, many land reform beneficiaries were able to farm small family plots alongside work that they performed for the cooperative.[29] And given that children were no longer required to perform household or other duties for a large landowner, families could use their labor to farm these plots to a greater degree. Whether this farming was for subsistence purposes or to sell or trade products locally, it had the benefit of acting as a form of insurance against negative income, health, and employment shocks.

These dynamics provided incentives for families to have children work as opposed to attend school. As a result, this mechanism is related to, but not entirely the same as, the first mechanism linked to economic opportunity.

We investigate this dynamic in several ways. First, because boys are more likely than girls to be used as farm labor in Peru (e.g., Ersado, 2005), we examine the heterogeneous treatment effects of land reform on education by gender. Consider Fig. 5, which is constructed in a similar way to Fig. 3 but splits the sample of individuals by gender. The negative impact of high land reform exposure is concentrated in late primary and secondary schooling (6–11 years of education) for males. The impact on females is mainly observed in primary schooling (3–7 years of education). Women exposed to blanket land reform covering all of a district's agricultural land had a 5.6 percentage point reduction in the likelihood of completing at least 5th grade, while the impact on men is not distinguishable from zero. In contrast, men exposed to complete land reform had a reduced likelihood of completing at least 8th grade by 6.9 percentage points, while no impact is observed on women. Whereas boys may have been favored over girls to attend and complete primary school in land reform areas, the evidence in Fig. 5 is consistent with boys being subsequently drawn away from school to help farm.[30]

To investigate this mechanism in further depth, we conduct an additional analysis using individual-level data from the 1981 census to explore the short-term effects of land reform on child labor participation

[28] Data are not available from the immediate post-reform era to examine whether land reform was associated with a short-term increase in income in the first several years. Nonetheless, the finding on long-term income coincides with recent work by Mattheis and Raz (2019) on the U.S. Homestead Act. These authors find that homesteading areas remained more rural, were less agriculturally productive, and became poorer over time vis-à-vis similar non-homesteading regions.

[29] In some cases, this was also true prior to land reform. However, the time dedicated to farming these plots prior to land reform was limited by the long hours required to work for the landowner. The link between work commitment for a cooperative and compensation was looser.

[30] That most secondary schools are closer to urban areas may have contributed to boys being drawn away from school after completing primary education (see Goetghebuer and Platteau, 2010).

Table 4
Impact of land reform on educational attainment according to economic opportunity.

	Distance to local market (1961)		Density of paved roads (1975)		Potential land productivity	
	Near	Far	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
Exposure (Age at LR in district of birth)						
Fully Exposed (0–5)	−0.167	−0.250**	−0.099	−0.324**	−0.343**	−0.437***
	(0.177)	(0.112)	(0.149)	(0.130)	(0.138)	(0.151)
Partially Exposed (6–11)	−0.166	−0.270***	−0.104	−0.331***	−0.201*	−0.401***
	(0.138)	(0.095)	(0.126)	(0.106)	(0.110)	(0.134)
Weakly Exposed (12–17)	−0.081	−0.184**	−0.053	−0.189**	−0.101	−0.225**
	(0.096)	(0.081)	(0.090)	(0.089)	(0.083)	(0.098)
Controls						
Individual-level controls	YES	YES	YES	YES	YES	YES
District fixed effects	YES	YES	YES	YES	YES	YES
Year of birth fixed effects	YES	YES	YES	YES	YES	YES
Province time trends	YES	YES	YES	YES	YES	YES
R-squared	0.298	0.346	0.361	0.377	0.339	0.352
Observations	97,107	96,961	96,660	96,683	96,643	96,700
Districts	150	840	337	653	553	437

Notes: *p < 0.10, **p < 0.05, ***p < 0.01 Robust standard errors clustered by district of birth in parentheses. The sample is composed of individuals aged 0 to 24 at the year land reform (LR) began in their district of birth. Models (1) and (2) split the sample into districts of birth according to their distance to the nearest town of 3,000 or more inhabitants in 1961 (above the median is classified as far and below the median is classified as near). Models (3) and (4) split the sample into districts with a density of paved roads in 1975 above the median value (classified as high) and those with a density of roads below the median value (classified as low). Finally, models (5) and (6) split the sample among districts with high potential land productivity (above the sample median) and low potential (below the median). The dependent variable is the number of years of primary and secondary schooling accumulated during one's lifetime. The explanatory variables that capture the impact of land reform on education are interaction terms between dummies indicating age group at the beginning of land reform in their district of birth and the intensity of land reform in their district of birth. The control variables included in the equation are gender, mother tongue, district fixed effects, year of birth fixed effects, and a province level cubic trend. The omitted group in all models is composed by individuals aged 18 to 24 in the year land reform began in their district of birth.

Table 5
Impact of land reform on other development outcomes.

	(1)	(2)	(3)	(4)
	Agriculture	Rurality	Migration	Income
Dependent Variable:	Currently hold land for agricultural purposes	Current residence in a rural town	Lives in a province different to the province of birth	Log (current household total income)
Exposure (Age at LR in district of birth)				
Fully Exposed (0–5)	0.020**	0.028***	−0.047***	−0.057**
	(0.010)	(0.011)	(0.012)	(0.023)
Partially Exposed (6–11)	0.010	0.014	−0.030***	−0.039*
	(0.009)	(0.009)	(0.010)	(0.022)
Weakly Exposed (12–17)	−0.004	0.010	−0.005	−0.014
	(0.007)	(0.007)	(0.007)	(0.015)
Controls				
Language (native = 1)	0.315***	0.203***	−0.124***	−0.327***
	(0.012)	(0.009)	(0.014)	(0.013)
Gender (female = 1)	−0.017***	−0.020***	−0.011***	−0.009**
	(0.002)	(0.002)	(0.003)	(0.004)
Constant	0.290***	0.273***	0.508***	9.790***
	(0.018)	(0.015)	(0.021)	(0.040)
District fixed effects	YES	YES	YES	YES
Year of birth fixed effects	YES	YES	YES	YES
Province time trends	YES	YES	YES	YES
R-squared	0.310	0.293	0.181	0.199
Observations	194,068	194,068	194,068	194,068
Districts	990	990	990	990

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Robust standard errors clustered by district of birth in parentheses. The sample is composed of individuals aged 0 to 24 at the year land reform (LR) began in their district of birth. The dependent variable for each model is indicated in the column headings.

and educational attainment. The 1981 census was conducted just as Peru's major phase of land reform came to an end and therefore is ideally timed to examine the short-term impact of land reform on the educational and labor status of school-aged individuals age at the time of the census.

According to the 1981 census, child labor participation in agriculture was almost absent for individuals aged 6 to 9 (0.6%–0.9%), low for those aged 10–12 (1%–2%), and somewhat higher for adolescents aged 13 (3%) to 17 (12%).[31] These figures are most likely understated.[32] For instance, because the period of reference for the employment section of the census only considered economic activities carried out during the last seven days, it did not capture agricultural labor among individuals that did not engage in farming activities the week before the survey.[33] Consistent with the discussion of Fig. 5, however, a gender gap is observed among individuals aged 13 to 17: more than 10% of boys from this age group worked in agriculture, while this value was below 4% in the case of girls.[34]

Table 6 reports a series of regression analyses that investigate the effect of land reform exposure first on child labor participation and then on educational attainment for a sample of individuals aged 13 to 17 in 1981.[35] As previously, we restrict the sample to individuals who resided in coastal and highlands districts that received at least some land reform prior to 1981. We also restrict the sample to males given their disproportionately higher labor participation rate in agriculture.[36]

The dependent variable in the first four models is a dummy variable that indicates whether the individual worked in agriculture in 1981. In addition, we also investigate if the negative effect of land reform on educational attainment is already observed by 1981, and thus we estimate similar regressions using years of schooling as the dependent variable. The relevant explanatory variable is our measure of land reform intensity in the district of residence.

It is important to mention, however, that the Table 6 models have inferential limitations. Unlike our previous difference-in-difference estimates for educational attainment, older cohorts are not a clean control group in the analyses of agricultural labor because we cannot know whether they worked in agriculture when they were children. We only observe if they worked in agriculture at the time of the 1981 census, when they were old enough to no longer be considered as child laborers. Furthermore, in the analyses of educational attainment circa 1981, younger individuals (and some older ones) could still be enrolled in school, eliminating the applicability of the previous difference-in-differences estimation strategy. These results should therefore be interpreted as suggestive but not necessary causal.

To help address potential confounders, the models include several control variables related to characteristics of individuals (mother tongue), their parents (head of household age and gender, parents' maximum years of education attained, and a dummy for not having a mother at home), their household characteristics (economic dependency ratio, household size, number of siblings by age group), their housing conditions (whether their house had a dirt floor, electric power, or running water) and some demographical attributes of the district of residence (log of district population, proportion of rural population in the district, and a measure of hacienda presence in the district). Moreover, the models also include province of residence fixed effects and age cohort fixed effects. Standard errors are clustered by the district of residence.

Column 1 of Table 6 reports a baseline model derived from a linear probability model regression.[37] The results suggest a positive effect of land reform intensity on child labor participation. Full land reform in the district of residence yields an estimated 1.6% increase in the probability of male children working in agriculture.

To evaluate whether the effect occurred because these individuals were taken out of school shortly after land reform implementation, in Column 2 we add an interaction term between the land reform intensity measure and a dummy variable that identifies individuals whose district of residence was affected by the land reform relatively late in the main phase of the program and more proximate to the 1981 census (in 1974 or after). The results show that the impact of land reform on child labor participation is mainly observed for individuals in districts with later land reform. Among those individuals, the effect of full land reform exposure is a 2.4% increase in the likelihood of working in agriculture.

Columns 3 to 6 restrict the sample to individuals who lived in districts affected by late land reform (1974 onwards). The observed impact of the reform on child labor participation in Column 3 increases to 3.2%. Column 4 disentangles the estimated effect according to age cohort by adding interaction terms between the land reform intensity measure and age cohort dummies. The omitted group in this model is composed of individuals aged 13 in 1981. The results show that the impact of the reform on child labor participation is concentrated in adolescents aged 15, 16, and 17, where the effects are larger and more robust. For instance, full land reform increased the likelihood of individuals aged 17 working in agriculture by nearly 19%.

Columns 5 and 6 examine whether the negative impact of land reform on educational attainment was already observed among individuals of schooling age by 1981. The estimates suggest that children exposed to blanket land reform covering all of a district's agricultural land accumulated, on average, an estimated 0.145 fewer years of schooling. The impact of land reform on the older cohort (e.g., aged 17) is the largest: full land reform exposure reduced their educational attainment by 0.77 years of schooling.

5.4. *Lingering effects of land reform as evidence of impact on educational demand*

It is possible that land reform exposure lowered human capital accumulation simply because it was disruptive to existing agricultural relations and local labor markets, and this shock and its attendant uncertainties lowered demand for education. In other words, the process of land reform itself may have depressed educational attendance and attainment. If this were the case, then the effects of land reform on educational attainment should be transitory rather than lingering in ways that the mechanisms indicate.

We examine this possibility by turning to data from additional exposed birth cohorts that are excluded in our main analysis. In particular, we re-estimate equations (1) and (2) but this time include birth

[31] 4.8% of individuals aged 14 were working in agriculture, 7.5% of those aged 15, and 9.8% of individuals aged 16.

[32] For supporting evidence, see for instance Boyden (1988, 195–196), and the findings in Ersado (2005).

[33] This will understate the agricultural child labor figure, since agricultural labor demand has a strong seasonal component. Furthermore, since the census information is self-reported, there may be a downward bias in reported child labor since some household heads may have had incentives to hide this information.

[34] These results should be interpreted as a lower bound of the true impact given that the likely underreporting of agricultural child labor should generate downward bias in our estimates.

[35] These individuals were between 0 and 12 years of age after land reform began, depending on the year it was deployed in each district (between 1969 and 1981). We excluded individuals younger than 13 years old since child labor participation was almost nonexistent among this group.

[36] We do not find discernible impacts of the land reform on either labor participation or educational attainment for young women in 1981 (see Appendix Table A.5). This most likely differs from Fig. 5 findings because parents may have underreported female child labor and overstated their involvement in school relative to what grown women who were raised in this era later reported themselves.

[37] Using this over a probit or logit model facilitates interpretation of the coefficients and is also a much more efficient estimator given the large number of fixed effects and clusters for estimating standard errors.

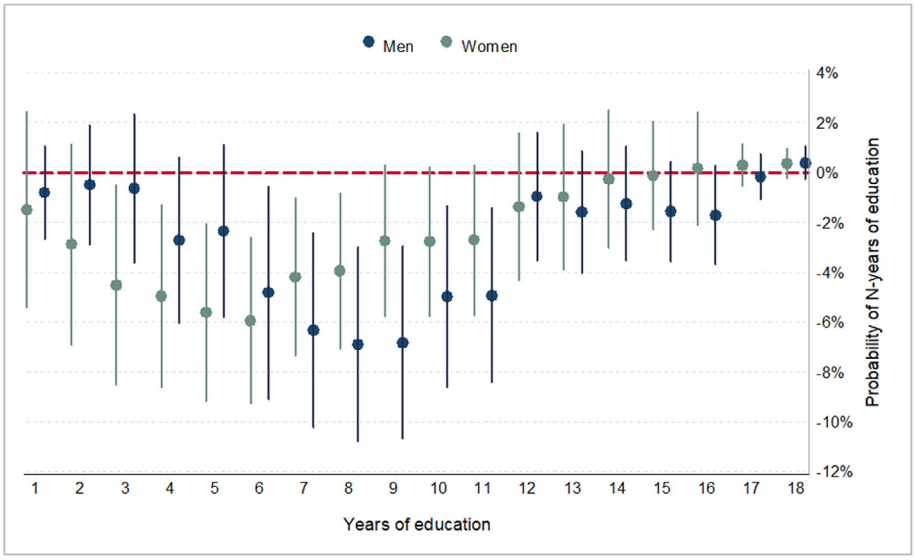

Fig. 5. The Effect of Land Reform on the Probability of Attending at least N-years of School by Gender. Notes: This figure is constructed in a similar way to Fig. 3 but provides separate estimates for men and women.

Table 6
Short-term impact of the land reform on child labor and educational attainment (males).

Dependent variable:	Districts with any land reform (1969–1980)		Districts with recent land reform (1974–1980)			
	Work in agriculture		Work in agriculture		Years of schooling	
	(1)	(2)	(3)	(4)	(5)	(6)
LR intensity	0.016**	0.011	0.032*	−0.044	−0.195**	0.178
	(0.008)	(0.008)	(0.018)	(0.038)	(0.089)	(0.178)
LR intensity * recent LR		0.024*				
		(0.012)				
LR intensity * aged 14				0.024		−0.224
				(0.021)		(0.144)
LR intensity * aged 15				0.078**		−0.386**
				(0.039)		(0.192)
LR intensity * aged 16				0.136**		−0.659**
				(0.057)		(0.293)
LR intensity * aged 17				0.186**		−0.770**
				(0.075)		(0.334)
Individual control variables[a]	YES	YES	YES	YES	YES	YES
Parent control variables[b]	YES	YES	YES	YES	YES	YES
Household control variables[c]	YES	YES	YES	YES	YES	YES
Housing control variables[d]	YES	YES	YES	YES	YES	YES
District control variables[e]	YES	YES	YES	YES	YES	YES
Province of residence fixed effects	YES	YES	YES	YES	YES	YES
Year of birth fixed effects	YES	YES	YES	YES	YES	YES
R-squared	0.279	0.280	0.318	0.321	0.431	0.432
Observations	112,982	112,982	35,788	35,788	35,787	35,787
Districts	909	909	360	360	360	360

Notes: *p < 0.10, **p < 0.05, ***p < 0.01 Control variables estimated but not reported included: [a] Mother tongue; [b] Head of household age and gender, parents' maximum years of education attained, dummy for mother not living in the household; [c] economic dependency ratio, household size, number of siblings by age group; [d] dirty floor dummy, electric power dummy, running water dummy; [e] log of district population, proportion of rural population in the district, and a measure of hacienda presence in the district prior to land reform. In addition, the models also include province of residence fixed effects and year of birth fixed effects. All models cluster standard errors by district of residence.

cohorts born after the reform who grew up entirely exposed to its hypothesized effects.[38] The results are in Table 7.

If land reform indeed has lingering effects as our mechanisms anticipate, rather than merely transitory effects, then the impact of land reform on education should be similar for all exposed individuals (e.g., those aged 0–6 years old when land reform occurred versus those born after the reform). The results confirm that the effect of land reform is also observed in exposed birth cohorts that follow from the youngest one in our main analyses. This is strong evidence for the demand side effects of land reform. The table also indicates, as anticipated, that land reform has no impact on entirely unexposed cohorts that were far past schooling age at the time land reform began.

5.5. Distinguishing effects among beneficiaries from general equilibrium effects

The previous discussion and findings suggest that land reform slowed educational attainment for those exposed to it. But to what extent was this driven by effects among land reform beneficiaries versus more general equilibrium effects deriving from the consequences on local land and labor markets? After all, if land reform delayed rural-urban transformation, interrupted rural labor markets, and yielded lower long-run incomes and greater use of child labor among land beneficiaries, these effects could have spilled over to non-beneficiaries as well by shaping markets.

While our data do not allow us to directly distinguish between land beneficiaries and non-beneficiaries, we are able to construct imperfect proxies for the share of land beneficiaries locally. We do so using data from the 1961 Directory of Population Centers, which lists all human settlements in Peru in 1961 as well as the type of settlement and population size. This directory includes information on the presence of haciendas (the principle target of land reform) and the size of the population living in these haciendas (the principle land reform beneficiaries). It therefore encodes information on the pervasiveness of the hacienda economy just prior to land reform.

Table A.6 presents the results. The models in this table replicate the Table 3 Column 2 specification but splits the sample of districts into those with a high or low presence of haciendas. Where hacienda presence or the portion of the population living in haciendas was high, we can be most confident that there were a lot of land reform beneficiaries in the district. Columns 1–2 restrict the analyses to the upper tercile and lower two terciles of districts, respectively, according to the number of haciendas in 1961. These models indicate that the negative effects of land reform exposure on educational attainment is significantly higher in districts with the greatest presence of haciendas – and therefore likely a greater number of land beneficiaries. Columns 3–4 instead restrict the analyses to the upper tercile and lower two terciles of districts according to the percentage of the total population living in haciendas in 1961. These models again indicate negative effects of land reform exposure on educational attainment in both samples, but the magnitude in districts with the largest likely share of land beneficiaries is substantially higher. Taken together, these results are suggestive that land reform exposure had a greater impact on land reform beneficiaries than non-beneficiaries, but that there were also local general equilibrium effects impacting non-beneficiaries in a similar fashion to beneficiaries.

6. Alternative explanations and inferential threats

This section raises and addresses several potential alternative explanations for the negative effect of land reform exposure on educational attainment as well several possible threats to identification.

First, there may be omitted factors correlated with both land reform intensity and educational attainment that bias our estimates. Table A.4 in the appendix addresses the robustness of the results to the inclusion of several additional controls. First we test a control for parents' education, which can influence a child's educational trajectory and which is linked to socio-economic status at the time of exposure to land reform.[39] As expected, this variable is positive and highly significant in explaining the year of schooling.[40] The coefficients of the variables of interest, however, remain very similar and maintain their statistical significance. Next we test a variable for the number of conflict deaths during Peru's internal conflict from 1980 to 2000, which was driven by the Shining Path insurgency. Data on conflict deaths are from Peru's Truth and Reconciliation Commission report. Again the main results hold.

Table 7
Persistent impact of land reform on educational attainment of younger cohorts born after land reform.

	(1)	(2)
	Years of schooling	
	Restricted	Unrestricted
Exposure (Age at LR in district of birth)		
Fully Exposed (Born 7–12 years after LR)	−0.342**	−0.384**
	(0.141)	(0.181)
Fully Exposed (Born 1–6 years after LR)	−0.384***	−0.426***
	(0.123)	(0.162)
Fully Exposed (Aged 0–5 at LR)	−0.373***	−0.415***
	(0.112)	(0.151)
Partially Exposed (Aged 6–11 at LR)	−0.306***	−0.349***
	(0.088)	(0.131)
Weakly Exposed (Aged 12–17 at LR)	−0.183***	−0.225*
	(0.065)	(0.120)
Not Exposed (Aged 18–23 at LR)		−0.045
		(0.104)
Controls		
Individual-level controls	YES	YES
District fixed effects	YES	YES
Year of birth fixed effects	YES	YES
Province time trends	YES	YES
R-squared	0.392	0.392
Observations	306,082	306,082
Districts	990	990

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Robust standard errors clustered by district of birth in parentheses. Columns 1 and 2 extend the Table 3 sample to include individuals born after the land reform (LR) was in place in their district of birth (two additional age cohorts: those born 1–6 years after land reform and those born 7–12 years after land reform). The dependent variable is the number of years of primary and secondary schooling accumulated during one's lifetime. The explanatory variables that captures the impact of land reform on education are interactions terms between dummies indicating age group at the beginning of land reform in their district of birth and the intensity of land reform in their district of birth. The control variables included in the equation are gender, mother tongue, district fixed effects, year of birth fixed effects, and a province level cubic trend. The omitted comparison group in Column 1 is composed of individuals aged 18 to 24 at the year land reform began in their district of birth; in Column 2 this group is composed of individuals 24 years old at the year land reform began in their district of birth.

[38] Our argument also implies that the effect of land reform should not be observed in other unexposed cohorts that were far past schooling age at the time land reform began. We confirmed this using data on additional birth cohorts not exposed to land reform: those born at least 25 years before reform, just prior to the oldest observed cohort in our main analysis.

[39] Nearly 15% of observations have missing data for this variable because parental education was only available for individuals who i) were heads or partners of heads of household; or ii) were listed as son/daughter, son-in-law/daughter-in-law, or grandchild, and who had at least one of their parents alive at the time ENAHO surveyed them. Due to this missingness, we did not include this variable as a control in the main regressions.

[40] These results are not reported to save space but are available upon request.

We also examine the robustness of the results to the inclusion of several pre-land reform district-level controls, such as total population, agricultural land area, the literacy rate, and the share of the rural population living in haciendas.[41] These factors could capture circumstances that condition both the likelihood of later land reform as well as educational trajectories. These variables were constructed using data from the 1961 agricultural and population census. The main results hold to the inclusion of each of these controls.

A related concern is that it is possible that there was time-varying spending in education or school construction as part of the 1972 education reform that was negatively correlated with land reform. This could constitute an identification violation through policy substitution effects. Scholarship on the 1972 reform casts doubt on this. Tovar (1985), for instance, concluded that one of the most important failures of the education reform was limited investment in school infrastructure expansion and resource-intensive programs. Most of the other significant measures introduced by the reform, mainly related to curriculum content issues, did not differ between regions. District-level data on education spending and infrastructure through the 1972 reform are unfortunately not available. However, we tested the robustness of the results to excluding districts where land reform began in 1972 or later; these are districts where policy substitution effects could theoretically have taken place. The main results hold under this restriction (see Table A.7 in the Appendix).

Another important consideration is the fact that the sample includes individuals who might have migrated to other districts before or during the age at which they would have attended primary or secondary school. Thus, the assignment to land reform exposure could contain some degree of measurement error. The ENAHO survey only collects information on an individual's district of birth and district of residence at the time of the survey. Therefore, we do not know the migration history of the sample; we only observe whether individuals remained in their district of birth or whether they migrated to another district at some point. Omitting this information could bias our estimates if, for instance, people who were more educated migrated to other areas after the reform.

One way to deal with this concern is to estimate separate results for both the sample of individuals that migrated at some point and for the sample of non-migrants. The results, depicted in Appendix Table A.8, are robust to restricting the sample to migrants or non-migrants. In both cases the negative and significant impact of the reform on education is observed in the fully and partially exposed cohorts. In contrast, the impact of land reform exposure on the weakly exposed cohort (aged 12 to 17 at the time of land reform) is only observed in the non-migrant sample. This could be because more of the migrant population left their birthplace during adolescence.

A final question is whether the lack of liquid land markets and weak property rights could have contributed to the drag that land reform had on education demand. Recall that Peru's land reform beneficiaries did not receive full property rights to their land. Existing research indicates that a lack of access to formal title makes it more difficult for those in the informal sector to use their land as collateral for bank loans to purchase credit or inputs and suppresses its marketability and value for individuals that seek to sell their land (Deininger, 2003). It is entirely possible that this could have operated in Peru. Indeed, it is a consideration that is consistent in many ways with the mechanisms we propose. It is also worth underscoring, however, that land reform programs that redistribute land to aggregated communities and provide incomplete property rights to beneficiaries are historically quite common (Lipton, 2009, Ch. 4–5). Countries such as Chile, China, Mexico, Nicaragua, Portugal, Sri Lanka, and many others structured their land reform programs in similar ways. Peru's experience with land reform is therefore typical rather than exceptional.[42]

7. Conclusion

Developing societies with high landholding inequality have suffered from slower development than their counterparts with lower land inequality over the long run. One common hypothesis for this divergence is that large landowners in unequal societies use their outsized power to suppress rural wages, slow urbanization, and retard the expansion of education by withholding investment in public goods and preventing their workers from attending school. By this logic, land redistribution should speed human capital accumulation by increasing the supply of schooling, ultimately with positive consequences for development.

We identify separate demand side effects of land reform on human capital that can operate distinctly from, and even against, supply side effects. We conduct an age cohort analysis using original data on land reform from Peru along with household surveys and census data on educational attainment and labor activity. Contrary to conventional expectations, exposure to land reform had a negative impact on the number of years an individual attended school. Further analysis suggests that this was driven by economic opportunity and by its effects on income and the use of child labor, particularly among boys, in the wake of land reform. That land reform can impact both supply and demand in education may help to explain disparate accounts of the effects of land reform on development across different contexts.

The results point to limitations to land reform as a straightforward path to human capital development. But they also suggest policy interventions, such as pairing land reform with conditional cash transfers or rural education initiatives, that could help to ameliorate these limitations. Countries engaged in ongoing land reforms, such as Brazil, Colombia, and South Africa, could stand to profit from adopting cognate policies alongside land distribution.

CRediT authorship contribution statement

Michael Albertus: Funding acquisition, Conceptualization, Methodology, Investigation, Project administration, Writing - original draft, Writing - review & editing, Supervision. **Mauricio Espinoza:** Methodology, Software, Visualization, Writing - original draft, Writing - review & editing. **Ricardo Fort:** Funding acquisition, Conceptualization, Methodology, Investigation, Project administration, Writing - original draft, Writing - review & editing, Supervision.

Declaration of competing interest

None.

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[41] The conflict variable and the pre-reform district level controls are included by interacting these with dummies indicating age group at the beginning of land reform in an individual's district of birth.

[42] The findings of land reform on education in Peru may therefore differ from some more exceptional contexts in which land beneficiaries receive complete and individual property rights, as in contemporary South Africa (see Keswell and Carter, 2014). Future research might try to examine variation in property rights or land markets within the context of a land reform program or its aftermath in order to address this issue.

Appendix

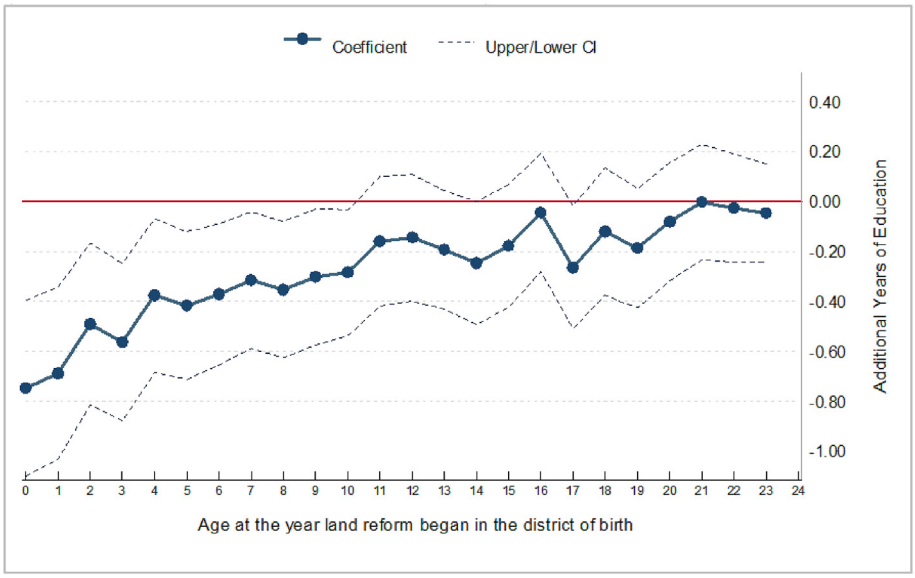


Fig. A.1. The Long-Term Impact of the Land Reform on Educational Attainment (using 1993 population census). Notes: This figure replicates Figure 2 but instead uses data from a 5% random sample of the 1993 population census (N=237,594).

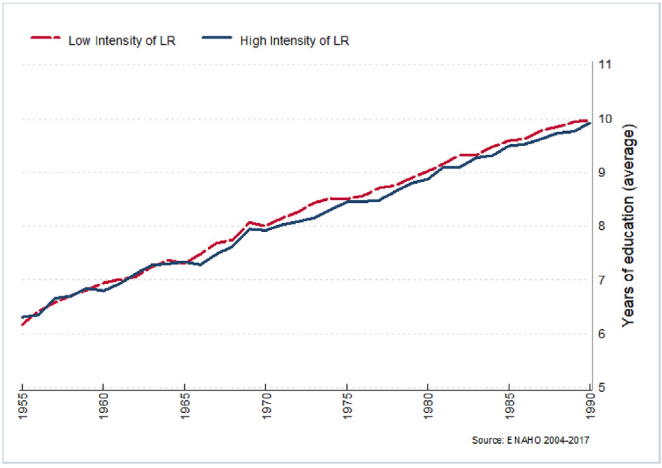
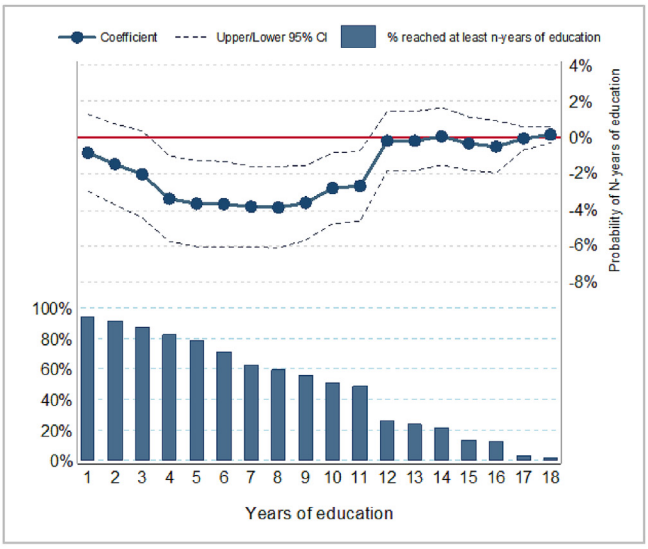


Fig. A.2. Average Years of Education by Cohort of Birth and High/Low Land Reform Intensity. Notes: Districts with high intensity of land reform are defined as those where at least 45% of district agricultural land area was expropriated by the reform. Low intensity land reform districts comprise the rest of districts where at least some land reform took place.

Land reform was most heavily implemented between 1969 and 1976. Individuals that attended school post-reform were fully exposed to the reform. For instance, the median year of the beginning of land reform in a district is 1973. Children in the median district, born in 1967 or afterwards (age 6 or less when land reform arrived to the district of birth), were fully affected by land reform. Older children born in 1966 or before were less affected. Individuals born in 1955 or before not were affected at all: they were 18 years old when the reform began in their district of birth. Land reform exposure of specific cohorts varies depending on the year land reform began in each district.



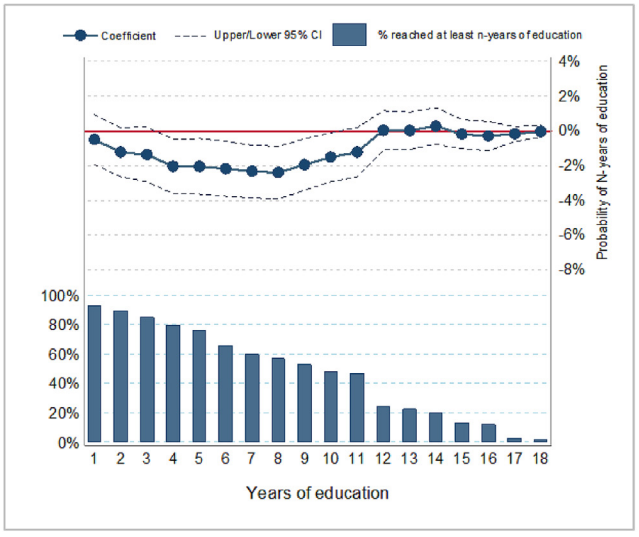


Fig. A.3. The Effect of the Land Reform on the Probability of Attending at Least N-years of School (Alternative exposed cohort definition). **Panel A:** Exposed cohort: Individuals Aged 0 to 11 at the year land reform was effective in the district of birth. **Panel B:** Exposed cohort: Individuals Aged 0 to 18 at the year land reform was effective in the district of birth. Notes: See notes of Figure 2. Panel A shows the results considering as the young cohort individuals aged 0 to 11 at the year land reform was effective in the district of birth, while Panel B consider as the young cohort individuals aged 0 to 18 at the year land reform was effective in the district of birth.

TABLE A.1
Robustness to Different Definitions of Land Reform Intensity

	(1)	(2)	(3)	(4)	(5)
			Years of schooling		
Land reform variable:	Expropriated private land in use as % of agricultural land in district	Expropriated total private land as % of agricultural land in district	Expropriated private land in thousands of has. (controlling for district's agricultural land area)	Dummy for highly exposed if expropriated private land as % of agricultural land in district > 50%	Expropriated unproductive land as % of total district area
Exposure (Age at LR in district of birth)					
Fully Exposed (0–5)	−0.321***	−0.246***	−0.010**	−0.229***	0.359
	(0.097)	(0.091)	(0.004)	(0.074)	(0.945)
Partially Exposed (6–11)	−0.274***	−0.194**	−0.008**	−0.204***	1.205
	(0.083)	(0.076)	(0.003)	(0.061)	(1.129)
Weakly Exposed (12–17)	−0.158**	−0.099*	−0.000	−0.095*	1.347*
	(0.062)	(0.056)	(0.003)	(0.050)	(0.818)
Controls					
Mother tongue (native = 1)	−2.714***	−2.714***	−2.714***	−2.714***	−2.714***
	(0.062)	(0.062)	(0.062)	(0.062)	(0.062)
Gender (female = 1)	−1.575***	−1.575***	−1.575***	−1.575***	−1.575***
	(0.066)	(0.066)	(0.066)	(0.066)	(0.066)

(*continued on next column*)

TABLE A.1 (*continued*)

	(1)	(2)	(3)	(4)	(5)
	Years of schooling				
Land reform variable:	Expropriated private land in use as % of agricultural land in district	Expropriated total private land as % of agricultural land in district	Expropriated private land in thousands of has. (controlling for district's agricultural land area)	Dummy for highly exposed if expropriated private land as % of agricultural land in district > 50%	Expropriated unproductive land as % of total district area
Constant	−2.714*** (0.062)	−2.714*** (0.062)	−2.714*** (0.062)	−2.714*** (0.062)	−2.714*** (0.062)
District fixed effects	YES	YES	YES	YES	YES
Year of birth fixed effects	YES	YES	YES	YES	YES
Province time trends	YES	YES	YES	YES	YES
R-squared	0.385	0.385	0.385	0.385	0.385
Observations	194,068	194,068	194,068	194,068	194,068
Districts	990	990	990	990	990

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Robust standard errors clustered by district of birth in parentheses. The sample is composed of individuals aged 0 to 24 in the year land reform (LR) began in their district of birth. The dependent variable is the number of years of primary and secondary schooling accumulated during one's lifetime. The explanatory variables that captures the impact of land reform on education are interaction terms between dummies indicating age group at the beginning of land reform in their district of birth and the intensity of land reform in the district of birth. The control variables included in the equation are gender, mother tongue, district fixed effects, year of birth fixed effects, and a province level cubic trend. The omitted group in all models is composed of individuals aged 18 to 24 at the year land reform began in their district of birth. The land reform intensity variable varies by model as indicated in the column headings. Model 1 uses the main land reform intensity variable. Model 2 uses expropriated private land as a percentage of district agricultural land (i.e., including actively used private agricultural land and abandoned or long-fallowed land). Model 3 uses the raw number of hectares of actively used private agricultural land expropriated by the reform, while controlling for district agricultural land area. Model 4 uses a high land reform intensity dummy, which identifies districts where at least 50% of the actively used private agricultural land was expropriated. Model 5 uses expropriated unproductive land (eriazos) as a percentage of total district area.

TABLE A.2
Robustness to Sample Selection

	(1)	(2)	(3)	(4)	(5)	(6)
	Years of schooling					
Sample:	All districts	Districts with some land reform				
		All with some LR	Agriculture importance (1961)		Rural population (1961)	
			>50%	<50%	>50%	<50%
Exposure (Age at LR in district of birth)						
Fully Exposed (0–5)	−0.249**	−0.321***	−0.339***	−0.140	−0.297***	−0.194
	(0.097)	(0.097)	(0.108)	(0.193)	(0.113)	(0.184)
Partially Exposed (6–11)	−0.165*	−0.274***	−0.304***	−0.184	−0.256***	−0.251
	(0.087)	(0.083)	(0.089)	(0.171)	(0.094)	(0.162)
Weakly Exposed (12–17)	−0.058	−0.158**	−0.200***	−0.081	−0.161**	−0.122
	(0.067)	(0.062)	(0.076)	(0.112)	(0.075)	(0.113)
Controls						
Individual-level controls	YES	YES	YES	YES	YES	YES
District fixed effects	YES	YES	YES	YES	YES	YES
Year of birth fixed effects	YES	YES	YES	YES	YES	YES
Province time trends	YES	YES	YES	YES	YES	YES
R-squared	0.409	0.385	0.327	0.251	0.324	0.217
Observations	241,924	194,068	120,876	73,192	121,921	72,147
Districts	1402	990	863	127	793	197

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Robust standard errors clustered by district of birth in parentheses. The sample is composed of individuals aged 0 to 24 at the year land reform (LR) began in their district of birth. Column 1 still drops districts in the Amazon basin where the main land reform legislation did not apply. In addition, models presented in Column 2 to 6 exclude individuals born in districts not affected by the land reform (412 districts). Columns 3 and 4 split the sample into districts of birth with a high importance of agriculture (>50% of workers labored in agriculture in 1961) and a low importance of agriculture (<50% of workers labored in agriculture in 1961). Likewise, Columns 5 and 6 split the sample into mostly rural (>50% rural population in 1961) and mostly urban (<50% rural population in 1961) districts of birth. The dependent variable in all models is the number of years of primary and secondary schooling accumulated during one's lifetime. The explanatory variables that capture the impact of land reform on education are interaction terms between dummies indicating age group at the beginning of land reform in their district of birth and the intensity of land reform in their district of birth. The control variables included in the equation are gender, mother tongue, district fixed effects, year of birth fixed effects, and a province level cubic trend. The omitted group in all models is composed of individuals aged 18 to 24 in the year land reform began in their district of birth.

Table A.2 explores how our findings are impacted by the set of districts we analyze. In the main analyses in the article, we restrict our sample to individuals from districts where at least some land reform took place. This excludes mainly urban districts where land reform is not especially relevant as well as other unaffected districts, such as those from non-agricultural areas or areas where large properties subject to expropriation were absent. We do this because individuals from such districts are unsuitable as a control group for individuals in rural districts where at least some land reform occurred. Table A.2 first compares this sample to an unrestricted sample of all districts. It then examines other samples. We expect to observe effects of the land reform on education in districts with a high importance of agriculture prior to the land reform, and a small or null impact of the reform on districts where the importance of agriculture was low. A similar difference should be observed in a comparison between highly rural versus highly urban districts. The

results are consistent with this.

TABLE A.3
Distribution of Districts and the Sample of Individuals According to the Duration of Land Reform Exposure in Years

Land reform duration (years)	Sample of individuals			Districts		
	Freq.	Percent	Cum.	Freq.	Percent	Cum.
1	59,233	30.52	30.52	467	47.17	47.17
2	41,220	21.24	51.76	146	14.75	61.92
3	33,354	17.19	68.95	137	13.84	75.76
4	22,497	11.59	80.54	88	8.89	84.65
5	15,290	7.88	88.42	65	6.57	91.21
6	9101	4.69	93.11	32	3.23	94.44
7	7921	4.08	97.19	29	2.93	97.37
8	4402	2.27	99.46	15	1.52	98.89
9	184	0.09	99.55	3	0.30	99.19
10	212	0.11	99.66	4	0.40	99.60
11	108	0.06	99.72	1	0.10	99.70
12	524	0.27	99.99	1	0.10	99.80
13	22	0.01	100	2	0.20	100
Total	**194,068**	**100**		**990**	**100**	

TABLE A.4
Robustness to Including Additional Control Variables

	(1)	(2)	(3)	(4)	(5)	(6)
	Years of schooling					
Additional controls:	+ parents' education	+ # civil conflict deaths (*1000)	+ district's agricultural land (ha*1000) (1972)	+ district's population (*1000) (1961)	+ district's literacy rate (1961)	+ district's percentage of people living in haciendas
Exposure (Age at LR in district of birth)						
Fully Exposed (0–5)	−0.343***	−0.288***	−0.322***	−0.170*	−0.225**	−0.277***
	(0.105)	(0.097)	(0.099)	(0.100)	(0.105)	(0.105)
Partially Exposed (6–11)	−0.292***	−0.244***	−0.281***	−0.181**	−0.268***	−0.255***
	(0.086)	(0.084)	(0.083)	(0.079)	(0.090)	(0.092)
Weakly Exposed (12–17)	−0.190***	−0.137**	−0.164**	−0.120*	−0.172**	−0.195***
	(0.066)	(0.063)	(0.065)	(0.066)	(0.076)	(0.070)
Controls						
Individual-level base controls	YES	YES	YES	YES	YES	YES
District fixed effects	YES	YES	YES	YES	YES	YES
Year of birth fixed effects	YES	YES	YES	YES	YES	YES
Province time trends	YES	YES	YES	YES	YES	YES
R-squared	0.445	0.385	0.385	0.385	0.381	0.385
Observations	165,866	194,068	194,068	194,068	189,745	194,032
Districts	990	990	990	990	990	990

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Robust standard errors clustered by district of birth in parentheses. The sample is composed of individuals aged 0 to 24 in the year land reform (LR) began in their district of birth. The dependent variable is the number of years of primary and secondary schooling accumulated during one's lifetime. The explanatory variables that capture the impact of land reform on education are interaction terms between dummies indicating age group at the beginning of land reform in their district of birth and the intensity of land reform in their district of birth. The control variables included gender, mother tongue, and additional variables given in the column heading. Since in Column 2 to 6 the additional control variable is expressed at the district level, we interacted them with age group dummies. The omitted group is composed of individuals aged 17 to 24 in the year land reform began in their district of birth.

TABLE A.5
Short-term Impact of the Land Reform on Child Labor and Educational Attainment (females)

Dependent variable:	Districts with any land reform (1969–1980)		Districts with recent land reform (1974–1980)			
	Work in agriculture		Work in agriculture		Years of schooling	
	(1)	(2)	(3)	(4)	(7)	(8)
LR intensity	0.002	0.002	0.020	0.003	−0.148	0.209
	(0.005)	(0.005)	(0.013)	(0.013)	(0.103)	(0.258)
LR intensity*Recent LR		−0.002				
		(0.010)				
LR intensity * aged 14				0.020		−0.106
				(0.017)		(0.219)
LR intensity * aged 15						

(*continued on next column*)

TABLE A.5 (*continued*)

Dependent variable:	Districts with any land reform (1969–1980)		Districts with recent land reform (1974–1980)			
	Work in agriculture		Work in agriculture		Years of schooling	
	(1)	(2)	(3)	(4)	(7)	(8)
				0.026*		−0.360
				(0.014)		(0.289)
LR intensity * aged 16				0.025		−0.757**
				(0.020)		(0.357)
LR intensity * aged 17				0.018		−0.787
				(0.020)		(0.523)
Individual control variables[a]	YES	YES	YES	YES	YES	YES
Parents control variables[b]	YES	YES	YES	YES	YES	YES
Household control variables[c]	YES	YES	YES	YES	YES	YES
Housing control variables[d]	YES	YES	YES	YES	YES	YES
District control variables[e]	YES	YES	YES	YES	YES	YES
Province of birth fixed effects	YES	YES	YES	YES	YES	YES
Year of birth fixed effects	YES	YES	YES	YES	YES	YES
Clustered standard errors	YES	YES	YES	YES	YES	YES
R-squared	0.169	0.169	0.150	0.151	0.508	0.509
Observations	108,740	108,740	34,234	34,234	34,234	34,234
Districts	908	908	361	361	361	361

Note: See notes of Table 6.

TABLE A.6
Distinguishing Effects Among Beneficiaries from General Equilibrium Effects

	(1)	(2)	(3)	(4)
	# of *haciendas* in the district of birth		% population living in *haciendas* in the district of birth	
	1st/2nd tercile	3rd tercile	1st/2nd tercile	3rd tercile
Exposure (Age at LR in district of birth)				
Fully Exposed (0–5)	−0.204*	−0.780***	−0.340***	−0.465***
	(0.122)	(0.201)	(0.128)	(0.161)
Partially Exposed (6–11)	−0.191*	−0.645***	−0.287**	−0.362***
	(0.113)	(0.150)	(0.114)	(0.125)
Weakly Exposed (12–17)	−0.150*	−0.273**	−0.220***	−0.103
	(0.079)	(0.114)	(0.081)	(0.101)
Controls				
Mother tongue (native = 1)	−2.666***	−2.837***	−2.679***	−2.788***
	(0.066)	(0.145)	(0.066)	(0.143)
Gender (female = 1)	−1.580***	−1.562***	−1.509***	−1.705***
	(0.073)	(0.139)	(0.084)	(0.103)
Constant	6.382***	6.842***	6.720***	6.135***
	(0.172)	(0.228)	(0.152)	(0.258)
District fixed effects	YES	YES	YES	YES
Year of birth fixed effects	YES	YES	YES	YES
Province time trends	YES	YES	YES	YES
R-squared	0.377	0.403	0.384	0.374
Observations	134,433	59,599	129,376	64,656
Districts	830	160	655	335

Notes: *p < 0.10, **p < 0.05, ***p < 0.01 Robust standard errors clustered by district of birth in parentheses. The sample is composed of individuals aged 0 to 24 at the year land reform (LR) began in their district of birth. Column 1 restricts the sample to the two lower terciles of districts according to the number of haciendas in 1961, while Column 2 only considers the upper tercile of districts. Meanwhile, Columns 3 and 4 instead restrict the analyses to the lower two terciles (Column 3) and upper tercile (Column 4) of districts according to the percentage of the total population living in haciendas in 1961. The dependent variable is the number of years of primary and secondary schooling. The explanatory variables that capture the impact of land reform on education are interaction terms between dummies indicating age group at the beginning of land reform in their district of birth and the intensity of land reform in the district of birth. District fixed effects, year of birth fixed effects, and a province level cubic trend are included but not reported.

TABLE A.7
Robustness to Districts Experiencing Land Reform Before 1972

	(1)	(2)
	Land reform at any year (base model)	Land reform before 1972
Exposure (Age at LR in district of birth)		
Fully Exposed (0–5)	−0.321***	−0.505***
	(0.097)	(0.166)
Partially Exposed (6–11)	−0.274***	−0.491***
	(0.083)	(0.144)

(*continued on next column*)

TABLE A.7 (*continued*)

	(1)	(2)
	Land reform at any year (base model)	Land reform before 1972
Weakly Exposed (12–17)	−0.158**	−0.294***
	(0.062)	(0.113)
Individual-level controls	YES	YES
District fixed effects	YES	YES
Year of birth fixed effects	YES	YES
Province time trends	YES	YES
R-squared	0.385	0.378
Observations	194,068	74,177
Districts	990	265

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Robust standard errors clustered by district of birth in parentheses. The sample is composed of individuals aged 0 to 24 at the year land reform (LR) began in their district of birth. Column 1 reports the base results considering all districts from coastal and highlands regions affected by the main land reform legislation. The model presented in Column 2 drops all districts where land reform began in 1972 or after. The dependent variable is the number of years of primary and secondary schooling accumulated during one's lifetime. The explanatory variables that capture the impact of land reform on education are interaction terms between dummies indicating age group at the beginning of land reform in their district of birth and the intensity of land reform in their district of birth. The control variables included in the equation are gender, mother tongue, district fixed effects, year of birth fixed effects, and a province level cubic trend. The omitted group in all models is composed of individuals aged 18 to 24 in the year land reform began in their district of birth.

TABLE A.8
Robustness to Migration Status

	(1)	(2)
	Years of schooling	
	Non-migrant	Migrant
Exposure (Age at LR in district of birth)		
Fully Exposed (0–5)	−0.291**	−0.362***
	(0.125)	(0.119)
Partially Exposed (6–11)	−0.218*	−0.332***
	(0.112)	(0.102)
Weakly Exposed (12–17)	−0.202**	−0.104
	(0.083)	(0.087)
Individual-level controls	YES	YES
District fixed effects	YES	YES
Year of birth fixed effects	YES	YES
Province time trends	YES	YES
R-squared	0.472	0.305
Observations	112,261	81,807
Districts	950	990

Notes: *p < 0.10, **p < 0.05, ***p < 0.01. Robust standard errors clustered by district of birth in parentheses. The sample is composed of individuals aged 0 to 24 at the year land reform (LR) began in their district of birth. Columns 1 and 2 split the sample into individuals that currently live in the same province of birth (non-migrants) and individuals that currently live in a different province that the province of birth (migrants). The specification of both models is the same as in Column 2, Table 3.

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