Tied to the Land? Intergenerational Mobility and Agrarian Reform in Colombia

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This version: February 10, 2020

Abstract

This study examines the intergenerational effects of providing land to the rural poor. I use ID numbers to track applicants to the 1968 Colombian agrarian reform and their children in various administrative data. Exploiting discontinuities in the allocation of parcels, I find that the children of recipients exhibit higher intergenerational mobility. In contrast to the view that land would tie them to the countryside, today these children participate more in the modern economy. They have better living standards and are more likely to work in formal and high-skilled sectors. These findings appear driven by a relief of credit constraints that allowed recipient families to migrate to urban centers and invest in the education of their children.

Keywords: Intergenerational mobility, agrarian reform, modern economy, Colombia.
JEL Codes: E24, J62, N36, O15, Q15.

*Universidad de los Andes, Bogotá, Colombia, ju-galan@uniandes.edu.co. I thank Emily Breza, Moya Chin, Melissa Dell, Leopoldo Fergusson, Alejandro Gaviria, Rema Hanna, Soeren Henn, Ana María Ibañez, Michael Kremer, Horacio Larreguy, Stephanie Majerowicz, Marcela Meléndez, Eduardo Montero, Nathan Nunn, Rohini Pande, Pablo Querubin, James A. Robinson, Priya Shanmugam, Román A. Zarate, and participants at the Harvard Development, Economic History, and Political Economy Lunches for excellent comments and suggestions. Sergio Briceño, Liseth León, Jacob Muñoz, María Martinez, Sofía Parada, Santiago Pérez, Wilson Rangel, Juan Rodriguez, Daniel Taboada, and Andrés Valderrama provided excellent research assistance. I'm deeply indebted to numerous officials at the National Land Agency, National Planning Department, Ministry of Health and Social Protection, National Registry of Civil Status and Universidad de los Andes for their invaluable help and wisdom. All remaining errors are my own.

1 Introduction

Improving economic mobility among the rural poor is a pressing challenge across the developing world. In the 20th century, providing land through agrarian reform was a central development strategy assisting this purpose. Such policy is thought to have helped East Asian tigers drastically reduce extreme poverty but to have mostly failed in other latitudes (Dai and Tai, 1974). Its relevance continues into the present, as debates about implementing similar measures are recurrent in several developing countries, including South Africa, China, India and many Latin American countries (Narayan et al., 2018; World Bank, 2008, 2006). Yet, empirical evidence on whether land can expand economic opportunity remains remarkably scarce. This often leads to widespread controversy about the effectiveness of agrarian reform, because it consumes significant resources and provokes political tensions in society.

A widely held view against providing land to rural families is that it often ties them to the countryside. Since at least the 1960s, leading development experts have been concerned that it can produce a mass of "poor farmers working their small parcels with hand tools" (Currie, 1961, p. 37). Agrarian reforms typically introduce prohibitions to sell or rent the land, which may discourage migration and curtail economic mobility. These possibly force recipient families to remain in the traditional agricultural sector instead of transitioning to more productive sectors (Banerjee and Newman, 1993; De Janvry et al., 2015). At the same time, however, advocates of reform have spoken about the need to create owners of land. With greater assets, the rural poor can obtain a permanent source of income. They can also secure more credit or insurance, which in turn will help them invest more effectively (Besley et al., 2012; Goldstein and Udry, 2008; De Soto et al., 1989). Thus, the children of recipients may have better education, improving their productivity (Banerjee et al., 2015, 2011, 2000; Deininger and Feder, 2001).

In this paper, I examine the intergenerational impacts of providing land to the rural poor through the lens of the 1968 Colombian agrarian reform. This is a challenging question to study, because historical information that tracks recipients of land across time and space is rare, and the allocation of land is not a random phenomenon. I overcome these issues by employing newly available archival records from the extinguished Colombian Institute for Agrarian Reform (or INCORA) in Bogotá to construct a dataset characterizing 2,178 applicants to the *Sharecroppers and Tenants Program* in 1968–1970. Using names and national identification numbers (IDs), I search birth certificates in notarial records to find the children of a quarter of applicants. I match this information with various government administrative data from the 2000s – most notably social security records – to track 45% of (or 86% of

living) recipients and non-recipients and 89% of the subsample of children.

I estimate causal impacts by exploiting a source of plausibly random variation in the allocation of parcels. Aware of the high demand for land, the Colombian government designed a selection mechanism to benefit the most vulnerable families. After expropiations took place, poor farmers interested in the land were surveyed. A simple grading system combined data from several socioeconomic dimensions to assign them a score. Depending on geographic conditions, INCORA officials determined the number of parcels available for allocation. Only after having this information, they set score thresholds, making manipulation difficult. Applicants with scores above thresholds were eligible to be allocated a parcel intended to generate between two to three times the average annual income of a rural household, but with restrictions to sell it for ten years. My analysis, therefore, uses a regression discontinuity design to compare applicants who were just above and below a predicted score threshold but similar along many pre-treatment characteristics.

After four decades, I find that accessing land actually led recipients, and particularly their children, to participate *more* in the modern economy. Indeed, recipients experienced improved living standards; the baseline estimates document that applicants eligible to be allocated land in 1968-1970 increased their wealth index by 2006 in 0.2 standard deviations relative to non-recipients. These effects are mainly explained by better housing conditions, one of the components of the wealth index. However, they were marginally less likely to register for government poverty subsidies. In 2010, they were 4 percentage points more likely to earn above minimum wages, relative to a sample mean of 3%. Although they were also marginally more successful in leaving agriculture, they were not necessarily more likely to enter the formal economy, work in high-skilled sectors or become entrepreneurs. This suggests positive but modest developmental effects.

I then look at intergenerational dynamics to understand whether these effects persisted across generations or faded out. The children of recipients exhibited, on average, better living standards relative to those from non-recipients and their parents. In 2006, their household wealth index was 0.3 standard deviations higher. The effects are explained by various components of the index, including better housing conditions, asset property, and access to public services. In 2010, they were 22 percentage points more likely to earn above minimum wages, compared to a base of 17%, and 24 percentage points more likely to enter the formal sector, relative to a sample mean of 39%. They were also more likely to work in high-skilled sectors and as formal entrepreneurs – an entire bundle of measures that form the nexus of modern economic life. Since pre-treatment socioeconomic characteristics of applicants were balanced around the predicted score threshold, these findings are indicative of considerable upward economic mobility.

As previous estimates only consider local treatment effects, I complement the analysis by studying intergenerational mobility across the whole sample of applicant families. I use wealth and years of schooling as outcome variables. Similar to Chetty et al. (2014), I rank applicants based on their outcome levels relative to other applicants with children in the 1970s–1980s birth cohorts. I then rank the children of applicants based on their outcome levels relative to other children in the sample. I characterize intergenerational mobility for recipient and non-recipient families based on the slope of rank-rank relationships, which identify the correlation between children's and parents' positions in the outcome distributions. While causal claims cannot be made with this exercise, it is still informative about economic mobility.

The ranks are almost linear and highly robust to alternative specifications. Relative intergenerational mobility was low among applicant families, but the children of recipients exhibited higher rates. A 10-percentile point increase in recipients and non-recipients rank was associated with 4.8 and 6.0 percentile increases in their children's relative wealth rank, respectively. Similar results are reached when using education. I find that upward mobility for the children of non-recipients in the bottom quarter of the wealth distribution was 29; for children of recipients, it was 34, which rules out that effects are caused by worse outcomes for better-off rural families. In summary, these findings illustrate that providing a father with a productive asset can significantly *improve* a family's well-being and change the intergenerational path of their children. They do not merely reveal persistence, but rather *amplifying* effects across generations.

After documenting intergenerational effects, I draw from Colombian historiography to explore theoretical mechanisms. I focus on how land could have helped recipient families enter the modern economy (Harris and Todaro, 1970; Lewis, 1954). First, I look at geographic mobility by comparing the place where applicants lived in 1968–1970 to where they resided four decades after. In contrast to a widely held view that land tied rural families to the countryside, I find recipients were 20 percentage points more likely to migrate, relative to a mean of 50%, and 11 percentage points more likely to have done so to large urban centers, compared to a base of 19%. Likewise, their children were 27 percentage points more likely to move, relative to a mean of 70%. These children were also 22 percentage points more likely to do so to large cities, compared to a base of 39%, where they presumably found new economic opportunities. Suggestive evidence discards that civil conflict influenced these patterns.

I then study whether land was used to invest in the education of children, who subsequently adquired the skills to enter the modern economy. In 2006, the children of recipients born after the reform had accumulated 1.5 more years of schooling on average, relative to

a mean of 5.3. They were also 17 percentage points more likely to finish primary school, compared to a base of 52%. Effects are attenuated if the whole sample of children is used in the analysis. Consistent with a setting where an asset appears to have relieved credit constraints on urban migration costs and education, notarial records show that almost 30% of recipients formally sold when their prohibitions expired. Possibly, even more did so through informal land markets.

Finally, I evaluate the cost-effectiveness of the policy, an important element when analyzing its convenience. I compare previous intergenerational benefits with the fiscal costs of the *Sharecroppers and Tenants Program* using a cost-benefit analysis. Historical data suggests that land redistribution cost the state 0.5% of GDP in 1970, a sizable effort equivalent to 7% of the national budget (Tamayo, 1970). However, only a bit less than twenty thousand rural families received land (INCORA, 1970). Using previous estimates, I predict the lifetime earnings for an average recipient child. I then calcute different net-present benefit scenarios per recipient family. While caution is warranted because calculations reported rely on several strong assumptions, estimates suggest providing land through agrarian reform was not cost-effective. The baseline fiscal investment made per recipient family had a rate of return of -80%, while the most favorable scenario still yields rates of -40%.

This paper contributes to a growing empirical literature on intergenerational mobility and the persistence of past shocks. Efforts to understand intergenerational mobility have mostly focused on the US and other developed nations (Black and Devereux, 2010; Chetty et al., 2014; Clark, 2014; Corak, 2013; Solon, 1999). Yet, economic mobility in developing countries remains an under-researched area, primarily due to data limitations and selection bias. In Colombia, as in much of the developing world, available studies underscore the persistence of low intergenerational mobility rates (Narayan et al., 2018; Montenegro and Meléndez, 2014; De Ferranti et al., 2004). Moreover, contrary to previous studies that document modest or nonexistent intergenerational impacts of shocks or lotteries, I uncover new findings on how transfering assets can alleviate poverty across generations (Bleakley and Ferrie, 2016; Cesarini et al., 2016; Sacerdote, 2005).

The paper also complements attempts to understand agrarian reforms and the development process. A majority of research efforts in the social sciences have focused on the aggregate economic and political effects of these reforms, particularly in India and Latin America, finding mixed results (Montero, 2018; Besley et al., 2016; Dell, 2012; Banerjee et al., 2002; Besley and Burgess, 2000). This study takes a different approach and provides, to the best of my knowledge, the first micro-level evidence about the long-run consequences for recipients of land. I can precisely investigate the channels of persistence and explore theories of migration and economic transformation (Harris and Todaro, 1970; Lewis, 1954). This

exercise is uninformative about general equilibrium shifts, but it is not necessarily relevant in this context because the reform only affected a small number of rural families.

Overall, these findings have broad implications for development policy. If the reason that recipients benefit from accessing land is to sell it to relieve credit constraints, then policy-makers can think of alternative policies that would subsidize these costs. Future research could shed light on whether, for example, other asset transfers or credit incentives, can be a more socially effective tool for raising the well-being of the rural poor than politically costly land redistribution. This paper is organized as follows. In the next section, I describe the Colombian agrarian reform in 1968. In section 3, I explain the data sources and the linkage methods and present the empirical strategy, providing evidence on its validity. In section 4, I present the main findings on intergenerational mobility. Section 5 explores the mechanisms behind the effects of the reform. In section 6, I perform a cost-benefit analysis of the policy. Finally, section 7 concludes.

2 Historical Background

2.1 Overview of the Colombian Agrarian Reform

Just prior to the reform, Colombia had ended a decade-old civil war known as *La Violencia*, and the National Front, a political agreement to govern between the two traditional political parties, the Liberals and Conservatives, had come into effect. Heated public debates among policy makers called for a solution to the "land problem" (Hirschman, 1967, 1962; Currie, 1961, 1951). The country suffered from a legacy of high rural poverty and inequality, which not only discouraged the productive use of land, but also incited social conflicts and violence in the countryside (Robinson and Urrutia, 2007; Kalmanovitz and López, 2006; LeGrand, 1988; Urrutia and Berry, 1976; Berry, 1972; Fals-Borda et al., 1962). A World Bank mission in 1950 concluded that around 50% of the private rural land was owned by the top 1% of landowners, but the smallest 10% of farmers were twice as productive as the top decile (DANE, 1960).

Harvard professor and leading development expert, Albert Hirschman, pushed for an agrarain reform that would improve economic mobility for the rural poor. [1] Other defendants of this policy also argued it could appease civil unrest at a time when revolutionary threats were looming (Karl, 2017; Fals-Borda, 1957). [2] In contrast, Lauchlin Currie, another former

[1] Albert Hirschman spent much of his career studying Colombia. He served as a advisor to the National Planning Department (1952–1954) and was a private economic counselor (1954–1956). He was in favor of other land policies, including taxing unproductive land and updating the national cadastre.

[2] In many parts of the country, such as the departments of Tolima and Huila, redoubts of liberal guerrillas

Harvard professor and advisor to US president Franklin Roosevelt during World War II, was concerned that redistributing land would actually produce a mass of "poor farmers working their small parcels with hand tools" (Currie, 1961, p. 37). After leading the World Bank mission, he considered that the best solution was to promote rural migration into the cities, freeing land to be cultivated by fewer and larger landowners with more sophisticated techniques.

In 1961, after overcoming opposition from the landowning elite in Congress, Colombian president Alberto Lleras Camargo enacted an agrarian reform (Law 135). It combined a traditional approach – initiated under the *Sharecroppers and Tenants Program* – whereby the government could expropiate land that was inadequately being exploited and transfer it to sharecroppers, tenants, or smallholders, with a massive colonization program through the titling of *baldios* – or state-owned lands – to settlers at the frontier (CNMH, 2016; INCORA, 1974). President Lleras Camargo summed up his vision in a famous speech, where he proclamed that "more than a country of laborers, Colombia must be a country of owners" (Lleras-Restrepo, 1961, p. 41). Although the initiative had the support of numerous politicians, civic organizations, other Latin American countries, and even the United States, experts cautioned it would be difficult to roll out (Villamil-Chaux, 2015; Machado, 2013). [3]

The government created the Colombian Institute for Agrarian Reform (INCORA) in 1962 to centralize operations and granted it considerable autonomy and relatively sizable human and financial resources (INCORA, 1974). Initial progress on the much anticipated land redistribution was hindered by numerous administrative problems and a low pace of expropiations, as the reform met fierce resistence from landowners, who considered it a threat to their power (Palacios, 2011; Fajardo, 1986, 1979; Zamosc, 1978). [4] Consequently, in 1966, newly elected liberal President Carlos Lleras Restrepo significantly expanded the *Sharecroppers and Tenants Program.* [5] Through Law 1 of 1968, he eased the legal requirements for expropiating land and regulated the conversion of sharecroppers and tenants into owners of parcels called Agricultural Family Units (or AFU) (CNMH, 2016; INCORA, 1971).

from the 1950s and newly created rebel groups (FARC, ELN, etc) increasingly attacked large landowners.

[3]See essays in Lleras-Restrepo (1961) about the ideological positions of different political movements, including those from opposition leaders, and financial and technical aid provided through the Alliance for Progress by the Kennedy administration in the USA, which was worried about the spread of communism in the region.

[4]Landowners appealed to legal maneveurs and political connections in the justice system to delay or stop expropiations. Other extreme methods included targeted violence against former tenants and sharecroppers. See, for example, important essays in CNMH, 2014 (Molano, 1985) about emblematic cases in the Caribbean Coast and Antioquia.

[5]President Lleras Restrepo promoted other measures to garner political support, most notably the creation of the National Peasant Association (or ANUC) to organize farmers and press for social change (Zamosc, 1978).

According to official projections, INCORA estimated nine million hectares of land were available for redistribution. Nevertheless, with almost a million landless farmers in the countryside, policy makers considered it "impossible to allocate a parcel of land to every rural family" (INCORA, 1970, p. 78). In fact, the agency saw no other alternative but to establish a criteria that allowed to it "quantify and classify by priorities the families subject to agrarian reform" (INCORA, 1970, p. 78). A selection mechanism was designed – based on a scoring system that ranked applicants according to their socioeconomic conditions – to prioritize the redistribution of land to the most vulnerable among the rural poor (Directive 23 of 1966). The target was to benefit a third of rural families. I further describe this selection mechanism in section 2.2 and employ it in the empirical strategy in section 4.

The actions undertaken considerably increased the reach of the reform and targeted the heart of the country, most notably the central and southwestern regions in the Andes, and the Caribbean Coast (see Figure 1). [6] Between 1968 and 1970, the Lleras Restrepo administration initiated more than twelve thousand expropiation processes. However, in the midst of intense opposition from landowners, only 10% of the farms actually came into possesion of INCORA through the National Agrarian Fund (NAF). The majority of acquired lands were reported to be of regular quality and lacked access to markets. [7] The agency only managed to allocate 389,630 hectares to 19,478 rural families, providing them with an average parcel of 20 hectares at a cost of 0.5% of GDP (CNMH, 2016; INCORA, 1988); (Balcázar et al., 2001). [8] The policy was therefore considered a national failure and had negligible general equilibrium economic effects.

In 1970, an INCORA report vowed to not "capitulate to the pressure and inflexible position of the landowners" (INCORA, 1970, p. 199). Yet, the decline of agrarian reform began when conservative Misael Pastrana was sworn in as president. The *Sharecroppers and Tenants Program* was notably underfinanced, and the agency concentrated its efforts on implementing other land policies (INCORA, 1974). In 1972, the Pastrana administration signed the *Pact of Chicoral* to stop expropiations and modify the purposes of Law 135. [9] A few months later, the enacment of Law 4 of 1973 effectively ended attempts of the National

[6] 71% of expropiations were concentrated in the following departments: Antioquia, Cundinamarca, Tolima, Nariño, Valle del Cauca, Cauca, Magdalena, Bolivar, Cesar (CNMH, 2016).

[7] The law targeted farms of over 100 hectares that were deemed ineficiently used by INCORA officials. However, in practice, 68% of the lands that entered the NAF had less than 60 hectares (CNMH, 2016). 91% of expropiation processes were knocked down, either because officials deemed farms were used efficiently or judges – many of them connected to the landowing elite – reversed initial expropiations.

[8] This translated into USD 2,700 or ($50,000 Colombian pesos) per recipient in 1970. These costs included compensation and purchase from landowners, legal expenses, and agricultural investments in acquired lands (Tamayo, 1970).

[9] The pact was made in conjunction with representatives from the Liberal and Conservative parties, and the landowing elite assembled in the municipality of Chicoral, Tolima (Villamil-Chaux, 2015).

Front to change the country's land structure through land redistribution [10] (INCORA, 1974); Machado (2013); Palacios (2011). As the General Manager of INCORA, Carlos Villamil-Chaux, concluded decades after: "the country was simply not ready for it". [11]

2.2 The Allocation of Land

During the Lleras Restrepo administration, the goverment issued a series of decrees to regulate the process of expropiation and allocation of land (Decrees 2861 of 1996 and 719 of 1968). After INCORA opened an inquiry, agronomists and technicians were dispatched to evaluate whether a farm was unproductive according to the guidelines of Law 135. Their evaluation was based on key geographical and agronomical conditions. [12] The final report, called *informe de visita* (or visit report), was transmitted to the regional office of the agency. In conjunction with central authorities, an expropiation was then recommended or rejected, and acquired lands entered the National Agrarian Fund (NAF). [13] Landowners could appeal the decision before judicial authorities, who were then responsible for reviewing the case, and confirming or reversing the initial decision, oftentimes instigating a power clash with the central government.

Once a farm was cleared for redistribution, INCORA used a selection mechanism to allocate land to the rural poor (Directive 23 of 1966). Several steps were followed. First, the agency issued a statement informing the public about the decision. Then, authorities convened a local board of representatives elected among farmers, which was in charge helping them in the selection process. Next, they opened the registration of applicants interested in the land. Sharecroppers, tenants, and nearby landless farmers were eligible to apply. After a list was compiled, officials surveyed applicants along their family characteristics, agricultural experience, assets, and income (*formulario de aplicación* or application form). They used a simple grading system to aggregate responses into a continuous score for each applicant and ranked them. As shown in Table 1, the grading system was designed to reward more mature, and poorer rural families, as well as more experienced applicants.

Afterwards, the agency used technical studies to split the land into Agricultural Family Units (AFU) – or parcels intended to generate between two to three times the average rural household income. These varied considerably in size, reflecting the large variation of

[10] Law 4 of 1973 was not retroactive. As such, expropiations were not reversed.

[11] Interview on December 4, 2017.

[12] The evaluation factors included the level of: agricultural production, soil quality, terrain ruggedness, water availability, degree of market access, and presence of sharecroppers or tenants.

[13] The law established different modalities of land adquisition: expropiation, compensated expropiation, purchase, cession, and extinction of private domain. Approximately 80% of the lands that entered the NAF were purchased after cumbersome and often lengthy negotiations (CNMH, 2016; INCORA, 1971).

geographic and climatic conditions across the country. [14] With this information in hand, officials then allocated land to applicants with the highest scores up to the minimum score needed to fit into the last available parcel. Crucially, the procedure implicitly generated a threshold to select recipients, which was unique to each expropiation process because the number of applicants and the size of parcels varied (see Online Appendix). [15] In section 3.4, I discuss how the discontinuities induced by the selection mechanism provides an opportunity to implement a local linear regression discontinuity (RD) design to causally estimate the effects of providing land through agrarian reform.

The final steps of the process involved transferring property rights to recipients. Upon receiving the land, most recipients also agreed to a specific set of conditions. For instance, they could not sell their parcel for at least 10 years without approval from INCORA, a measure that was designed to incentivize their retention in rural areas. Also, they accepted the financial terms of the transfer, which in certain cases involved the payment of a loan at subsidized interest rates, and could not reapply to any agency program in the near future. On the other hand, those who did not receive land were mandated to vacate the land, a relevant issue for interpreting the results. The government never tracked applicants over time, making it impossible to evaluate the effects of the *Sharecroppers and Tenants Program* up to now.

3 Data and Empirical Strategy

3.1 Agrarian Reform Data

This study employs historical micro-level data constructed from the archives of the extinguished Colombian Institute for Agrarian Reform (INCORA) in Bogotá, Colombia. The archives are managed by the National Land Agency (ANT) and were salvaged in 2015 after the Colombian government centralized the organization of agrarian records. [16] They contain information about all INCORA operations between 1962 and 2002. Only 1/3 of the archives have been properly catalogued, particularly files from 1962 to 1993. While difficult to quantify, anecdotal evidence, including interviews with former INCORA and current ANT officials, suggests certain historical records may have been lost, stolen, or burned during the past decades. Even if the information collected cannot be considered complete, there is no

[14] For instance, in section 3, I discuss that the parcels in my sample varied between 6 and 40 hectares.

[15] In the last years of the reform, an alternative option was to create a cooperatives or community firms among former sharecroppers and tenants (INCORA, 1971). I don't include these cases in the empirical analysis.

[16] The INCORA archives are protected by legal reserve and were accessed through a confidentiality agreement with the ANT.

reason to believe these episodes targeted certain files disproportionately more than others.

I gathered information on 218 successful expropiation processes under the *Sharecroppers and Tenants Program* during 1968–1970. Most of them were concentrated in the Caribbean Coast and the Andean departments of Antioquia, Cundinamarca, and Tolima, and total more than thirty thousand hectares (see Figure 1). The files contain legal documents, technical studies, including the demarcation of parcels, and original surveys. [17] The surveys include information characterizing applicants' personal information and socioeconomic conditions, such as: full name, ID number (or *cédula de ciudadania*), address, household members, occupation, working experience, wages, assets, types of crops grown, and in several cases, the scores assigned by INCORA. Some of the expropiation files were hand written, while others used typewriters. This data is crucial for reconstructing the scores and thresholds used in the empirical strategy in section 3.4.

At the last phase of land redistribution, recipients were issued parcel titles. This information is not found in the expropiation files, but in individual agrarian records. In order to identify which applicants were effectively allocated land and titled – that is, the treatment variable – I merge the survey data with micro-level land title data from the National Land Agency (ANT) and notarial records from the Superintendence of Notaries and Registry (SNR) – the government agency in charge of issuing formal property rights. Using all this information, I constructed a novel dataset characterizing 2,178 agrarian reform applicants, of which 36% were recipients of land. In this sample, on average, expropiation processes included around 10 applicants and recipients were allocated 18 hectare parcels, which is very similar to government statistics.

3.2 Linking Applicants and Children

Next, I used the agrarian reform dataset to find the children of applicants at the National Identification Archive (ANI) of the National Registry of Civil Status (RNEC). [18] This government agency compiles relevant identification and vital statistics information on every individual issued an ID number, such as birth and death certificates, voting registration, and biometrics. At the time of the reform, parents were mandated to register new borns at notaries by filing a birth certificate. However, this norm was not necesarilly enforced in rural areas, where the supply of notaries was low and the rural poor commonly registered new borns at churches instead. While this could prove problematic, numerous expropiations

[17] To construct the dataset, I use information of applicants included both in visit reports (*informes de visita*) and application forms (*formularios de aplicación*) described in section 2.2.

[18] While many surveys contained information on the children of applicants, they did not register their ID numbers, because they were minors.

actually ocurred in places near the center of the country, increasing the chances of finding them.

Specifically, I matched the names and ID numbers of applicants to birth certificates and tracked the children registered at notaries. I identified 1,094 children out of 493 (or 23%) applicants, and collected their names and ID numbers. Unlike numerous studies on intergenerational mobility, I found both sons and daughters. This subsample does not suffer from differential attrition among recipients and non-recipients, reassuring that results derived from empirical exercises have external validity. In fact, the probability of finding a child in birth certificates was uncorrelated with the allocation of land or other pre-treatment socioeconomic characteristics of applicants (see Online Appendix B.1, Table A.4). However, the subsample probably does capture applicant families who, on average, resided closer to populated areas, and thus, were more likely have birth certificates for their children.

3.3 Contemporary Administrative Data

Lastly, I combined various sources of outcome data. Using names and ID numbers from applicant families, and a simple phonetic algorithm, I merged the agrarian reform data with contemporary administrative information (see Online Appendix A for a full explanation of the algorithm). [19] First, I use three components of social security records in 2010 from the Ministry of Health and Social Protection: Health Affiliations (RUAF-afilicaciones), Vital Statistics (RUAF-nacimientos y defunciones), and Social Security Contributions (PILA) databases. These datasets encompass 90% of the population and register information on living standards and labor markets, including the nature of employment, social security, formal wages, and occupation. As shown in Table 2, I tracked roughly 46% of applicants (or 86% of living applicants) and 89% of the children of applicants. I consider this my baseline dataset for the empirical analysis.

Second, I used social benefits records (SISBEN) designed by the National Planning Department (or DNP) and collected by municipal governments in 2006. [20] SISBEN tracks poverty conditions of over 30 million people (around 66% of the population) in need of receiving social benefits from the government. The dataset contains individual and household questions about education levels, housing conditions, public services, assets, and employment. While there have been critiques of SISBEN, overall, the evidence points to the source as being reliable, if potentially noisy. I tracked a bit more than a third of applicants and

[19] I unsuccesfully tried to find applicants and children in the 1980s and 1990s, but the quality of administrative data made it an impossible endeavor. Before the 2000s, most micro-level records, such as population censuses or household surveys, were erased or lost at DANE.

[20] This dataset is used by the government to prioritize poverty subsidies. Although indicative of who seeks aid, not all individuals in the dataset are poor or receive help.

almost two thirds of their children. I found no statistical evidence of differential attrition across applicants or children in the previous datasets, a fact that I confirm using death certificates (see Table 2). I analyze the issue of differential attrition in detail and the implications it entails for the interpretation of the results in section 4, when I present the findings. [21]

Morever, I employed other administrative sources to complement the empirical analysis. I drew from business records from the chambers of commerce (RUES) – the universe of formal firms in the country – to measure whether the rural poor set up a formal businesses. While this information spans several years, I focus on 2010 to make it comparable to social security records. Less than 1% of applicants and 10% of children are found in this dataset (see Table 2). Finally, when I investigate potential mechanisms, I used the Unique Registry of Victims – administered by the Agency of Victims (UV) –, which contains information on civil conflict victims. Most importantly, this data registers the date, place and description of violent events from the late 1980s to the present. I also web scraped information on criminal records of applicant families at the Office of the Attorney General and National Police.

3.4 Empirical strategy

A simple OLS estimation of the intergenerational effects of providing land would most likely be biased, because recipients and non-recipients differed along a range of observable (and most likely unobservable) characteristics. For example, recipients had, on average, more experience and lower wages, and they were also younger. Thus, the most informative estimation approach is to use a local linear regression discontinuity (RD) design that exploits variation from discontinuities induced by the INCORA selection mechanism. The analysis compares applicants just above and below thresholds, who were very similar along other socioeconomic dimensions. Notice that because applicants were mandated to vacate the land before the redistribution of parcels, my interpretation is that this comparison occurs among landless farmers.

To implement this empirical strategy, I employ archival information to predict applicant scores and thresholds. As discussed in section 2.2, I define these as the minimum score needed to fit into the last available parcel for each expropiation file, and then rescale them to zero to make them and applicants comparable (see Online Appendix A for a detailed explanation). In some instances, applicants unqualified for receiving land were reported to have been allocated parcels and vice-versa. While this could reflect random errors, the most plausible explanation is administrative or corruption problems involving officials. Also, information files is not always systematic, and predicted scores and thresholds must surely suffer from

[21] The match with certain administrative datasets, such as PILA or RUES, is not balance. This is not problematic because appearance in these datasets is precisely used as outcome variables.

measurement error. Even if compliance was imperfect and measurement error significant, the discontinuity generated by the selection mechanism could still induce a change in the probability of accessing land through agrarian reform.

The empirical specification used for applicants and children is as follows:

$$y_{i,e} = \gamma_1 recipient_{i,e} + \gamma_2 f_d(dist_{i,e}) + \gamma_3 d_{i,e} f_d(dist_{i,e}) + \alpha_l + X'_{i,e}\beta + \epsilon_{i,e} \quad (1)$$

where $y_{i,e}$ is a comtemporary outcome for applicant (or child of applicant) i in expropiation file e, and $recipient_{i,e}$ is an indicator variable equal to 1 if applicant i was above the predicted score threshold in expropiation file e, $f_d(dist_{i,e})$ is an RD polynomial in distance to the predicted score cutoff, $X_{i,e}$ is a set of covariates, and α_e an expropiation file fixed-effect, which captures common geographic and time characteristics affecting applicant families within each expropiation process. Finally, $\epsilon_{i,e}$ an error term that is normally and independently distributed. Robust standard errors are clustered at the applicant family level, because applicants are the treated unit, and children received treatment through their parents.

In this setup, the coefficient of interest is γ_1 – the causal difference in contemporary outcomes between applicants and children eligible to receive land according to predicted score thresholds relative to those that were not. Notice that this does not capture the effects of applicants who received land regardless, either because they were extremelly poor or capable of manipulating the system. This intent-to-treat effect underestimates the local treatment effect of receiving land through agrarian reform, which can be estimated using instrumental variables (2SLS) near the predicted score threshold. I do not use this alternative method in the baseline specifications but do include it in the robustness checks. As will become evident in the empirical excercises, relatively small sample sizes may lack proper power and coefficients become increasingly noisy (Andrews et al., 2019). [22]

Following Calonico et al. (2014), the baseline specification for equation (1) uses a linear polynomial estimated separately on each side of the cutoff, where the optimal bandwidth minimizes the mean squared error of the point estimator. This follows recent advice from (Gelman and Imbens, 2019), who argue that controlling for high order polynomials may lead to noisy estimates, particularly in small sample settings. [23] The Online Appendix provides a series of robustness tests using different RD polynomials, kernel functions, and various sample bandwidths to address concerns that the estimation results are specific to the choice of RD polynomial or bandwidth. Furthermore, baseline specification requires the existence of a strong first stage and two identifying assumptions: 1) applicants must not have selectively

[22] However, I do show IV estimations as a robustness check in section 4.3.

[23] I also estimate specifications following Imbens and Kalyanaraman (2012) and show that results are fairly consistent to using alternative optimal bandwidth methods.

sorted around the predicted cut-off based on their pre-treatment characteristics; and 2) all relevant factors besides treatment must change smoothly at the threshold.

3.4.1 First Stage

I first examine the existence of a first stage. Figure 3a graphically looks at the relationship between being above the predicted INCORA score threshold and the likelihood of receiving land. Each point in the figure represents the percentage of recipients within score bins. Dashed lines show 95% confidence intervals. The solid line plots predicted values from a local linear regression of receiving land on a quadratic polynomial in the predicted score, estimated separately on either side of the predicted threshold. Applicants with scores just above the predicted threshold were approximately 69% more likely to receive land during the agrarian reform, while the F statistic hovers around 9. Similarly, Figure 3b shows these results are extensive to the subsample of children, although somewhat less powerful. Overall, the graphs and regressions show that even if compliance is imperfect, there is of a strong first stage for both applicants and children.

3.4.2 Identifying Assumptions

Next, it would be problematic if applicants colluded with local officials or consistently manipulated surveys to place their scores just above the required thresholds and these actions were correlated with their pre-treatment characteristics. This would be the case, for instance, if better-off applicants bribed or lied to officials, or, conversely, if parcels were allocated to benefit friends or families. Ex-ante, it is unclear the nature of the biases that such hypothetical scenarios would introduce in the empirical exercises. Nevertheless, as explained in section 2.2, applicant manipulation required that they had previous knowledge of INCORA's calculations about the size of parcels and thresholds, which seems unlikely based on the available historiographical evidence (INCORA, 1970).

In order to check for selective sorting, I implement a McCrary test by collapsing the data into score-bins and using the number of observations within each bin as the dependent variable in equation (1). If some type of manipulation ocurred, then one would most likely observe some bunching of applicants just above the predicted threshold. Figure 3c illustrates that there is no discontinuous change or bunching in the number of observations in each bin around the predicted threshold, suggesting that applicants, on average, were unable to manipulate their score to become recipients. Furthermore, Figure 3d shows this finding is extensive to the subsample of children, as expected. The results are consistent with the version of then director general of INCORA, Carlos Villamil Chaux, who emphasized in an

interview the professionalism of their work. [24]

Finally, another potential problem would arise if applicants with scores just above the predicted threshold systematically differed in their pre-treatment characteristics from those just below. To test for this, I use archival and vital statistics information to estimate equation (1) for key socioeconomic characteristics in 1968-1970 – collected before the allocation of parcels – and present the results in Table 3. In columns (1) – (8), the coefficients document that applicants with scores within the optimal RD bandwith, regardless of whether they later received land or not, were statistically balanced in terms of their age, education, working experience, crop cultivation, area, housing, and income. Figure 2 graphically looks at the same relationships, also documenting pre-treatment balance. Together with previous checks, these results provide evidence that the assumption of relevant factors varying smoothly at the predicted threshold is reasonable and further validate the empirical strategy.

4 Impacts on the Rural Poor

I now investigate whether providing land through agrarian reform changed the lives of recipients and their children. Using the local linear RD design, I first examine the long-run effects on living standards and a bundle of measures that characterize modern economic life, including employment in formal and high-skilled sectors, and entrepreneurship. As discurssed in section 2.1, this is particularly relevant, because critics of the reform – including leading development experts – argued it would incentivize the retention of the rural poor in traditional sector of the economy, where productivty is low (Currie, 1951, 1961; Hirschman, 1962, 1967). Likewise, as many other developing countries, Colombia implemented major social and market reforms in the 1980s and 1990s, which could have differentially impacted recipient and non-recipients. I later complement this exercise by studying intergenerational mobility among all applicant families.

I first constructed living standards measures from social benefits records (SISBEN) in 2006. While I don't have information on household income or consumption, I used questions about housing quality, access to public services (electricity, running water, sewage), and quantity of assets to design a wealth index. To address multiple hypothesis testing concerns – and also to show that effects are not driven by the coding of categorical questions into binary outcomes – I computed a summary measure created using principal component analysis (PCA) that combined information from available welfare information. [25] Additionally,

[24] Interview on December 4, 2017.

[25] PCA is described in detail in the Online Appendix. Estimates using this dataset are probably underestimated, because individuals registered in SISBEN are generally among the most vulnerable.

I coded dummy variables from security records (RUAF) in 2010, that indicate whether applicants and children registered for poverty subsidies and were alive that year.

Furthermore, to understand the extent with which applicant families entered the formal economy and moved out of agriculture – in summary, their participation in the modern economy – I again used social secturity records (RUAF) in 2010. I coded dummy variables that indicate whether applicants and children earned above minimum wages, paid social security contributions, and were employed in the formal sector. I also aggregated sectoral (CIIU 2 Rev 4) employment descriptions to categorize whether they worked in agricultural, manufacturing or services sectors. Lastly, I complemented these variables with information from business records from the chambers of commerce (RUES) and coded a dummy variable that distinguishes if they became entrepreneurs by setting up a formal business before or on that year. In the next sections, I discuss the estimation results.

4.1 Applicants

Table 4 reports the baseline results for applicants. In all regressions, I show RD coefficients, specify the optimal bandwith used and the mean, and include the following controls to improve precision: age, ethnicity, sex, and expropiation file fixed-effects. Columns (1) to (2) document that forty years after the agrarian reform, recipients had on average 0.2 and 0.3 standard deviations higher wealth and housing indexes than to non-recipients. The assets index barely changes, suggesting that effects are mostly driven by the quality of housing. Likewise, column (3) shows that recipients were 9 percentage points less likely to register for poverty subsidies, relative to a sample mean of 72%. Most results are significant at 5% confidence level and qualitatively large compared to the sample means. Improved living standards are unlikely to be explained by differential attrition, because recipients and non-recipients were equally likely to be alive at the time (see Table 2).

Next, I look at labor market outcomes. Columns (4) to (6) illustrate that in 2010 recipients were 4 percentage points less likely to earn wages above the minimum wage and 11.8 percentage points more likely to work in the formal sector. Conversely, they were not disproportionately morel likely to be entrepreneurs. The estimates are marginally statistically significant, probably due to low variation in the data but, they are quite large relative to the sample means of 3% and 1%, respectively. A similar pattern appears in columns (7) to (9), which shows that recipients were 15 percentage points less likely to have been employed in agriculture, relative to a sample mean of 64%, and 15 percentage points more likely to have been employed in services, relative to a sample mean 14%. Both results are significant at the 10% confidence level. Likewise, Figure 5 shows RD plots for the most relevant outcome

variables for applicants.

While recipients experienced improved living standards, they did not necesarilly participate more in the modern economy. In contrast to the view that land retains the rural poor in traditional activities, these estimates suggest recipients were, in fact, marginally more succesfull in moving out of agriculture (De Janvry et al., 2015; Besley et al., 2012; Goldstein and Udry, 2008; Banerjee and Newman, 1993; De Soto et al., 1989). Yet, despite receiving a parcel of land, albeit with incomplete property rights, they still faced important constraints that kept them in the informal economy. The effects are considerably small compared to aggregate numbers. For example, only 3% of applicants earned above minimum wages and worked in the formal sector. These findings complement recent short-run studies on the effects of productive assets and suggest that while these can also lift the rural poor out of poverty in the long-run, other developmental effects are much more limited (Banerjee et al., 2015, 2011, 2000; Deininger and Feder, 2001).

4.2 Children

I now focus on exploring the intergenerational effects on the children of applicants. The analysis includes sons and daughters born after the agrarian reform, precisely those more likely to have been affected by it. Table 5 shows that, on aggregate, the children not only exhibited improved living standards relative to their parents, but also participated more in the modern economy. For instance, a larger proportion of them earned above minimum wages (17% vs. 2%), worked in the formal sector of the economy (39% vs. 3%) and moved out of agriculture (65% vs. 36%). This is somewhat expected, because the country implemented major social and market reforms in the following decades that significantly improved the quality of life for younger generations. A more interesing question, of course, is whether land had differential effects on the children of recipients and non-recipients.

I first look at living standards. Columns (1) to (2) document that in 2006, the children of recipients scored, on average, 0.32 and 0.37 standard deviations higher on the wealth and housing indexes relative to those of non-recipients. Unlike their parents, these children ranked better compared to their counterparts across all welfare attributes used to construct the indexes, including housing, assets and public service provision (see Online Appendix). Also, column (3) shows these children were 22 percentage points less likely to demand poverty subsidies, relative to a sample mean of 58%, suggesting they are less poor. Estimates are statistically significant at the 1% and 5% confidence level, much stronger than those obtained in the regressions of their parents. [26] As before, there is no evidence of differential attrition

[26] Coefficients in Tables 4 and 5 are much larger than the OLS estimates (see Online Appendix). This could be the case of downward attenuation bias, or the OLS could be a biased estimate of an average treatment

that may bias the results (see Table 2).

On the other hand, in columns (4) – (5) I find that the children of recipients were, on average, 22 percentage points more likely to earn above minimum wages, compared to a base of 17%, and 25 percentage points more likely to be working in the formal sector, relative to a sample mean of 39%. Consequently, they also contributed more to social security. They were also 10 percentage points more likely to be entrepreneurs, relative to a sample mean of 10%. All RD estimates are significant at the 5% level. Crucially, too, the children of recipients were more likely to move out of agriculture and find jobs in high-skilled sectors, such as manufacturing. In columns 7 and 9, a working child of a recipient was equally likely to be employed in agriculture or services but 13 percentage points percentage points more likely to be employed in manufacturing, relative to a sample mean of 11%. The coefficient is significant at the 5% confidence level. Moreover, Figure 6 shows RD plots for the most relevant outcome variables for the children of applicants, which graphically confirm previous insights.

In summary, these findings document substantial intergenerational effects of providing land through agrarian reform. They illustrate that the children of recipients had better living standards, participated more in the formal economy and worked more in high-skilled sectors and entrepreneurship, compared to those from non-recipients – an entire bundle of things that form the nexus of modern economic life. Moreover, they do not merely reveal persistence, but also amplifying effects across generations as the magnitudes of estimates are notoriously larger and statistically more significant for the children relative to their parents. Since pre-treatment characteristics are statistically balanced for applicants within RD optimal bandwidth – as previously shown in Table 3 – these effects are indicative of considerable upward economic mobility.

At first sight, the evidence is consistent with older studies using aggregate data in India, but are particularly at odds with recent social science research in Latin America, which documents adverse or ambiguous long-run effects of similar policies in Mexico and El Salvador (Besley et al., 2016; Banerjee et al., 2002; Dell, 2012; Montero, 2018). Nevertheless, a key distinction with previous studies is the use of micro-level data, which allows me to examine direct effects on recipient families as opposed to general equilibrium changes. Furthermore, they complement a growing literature on intergenerational mobility, but are also distinct from recent studies that find inconsequential intergenerational effects of wealth shocks, both in historical and contemporary settings (Bleakley and Ferrie, 2016; Cesarini et al., 2016; Chetty et al., 2014; Clark, 2014; Corak, 2013; Black and Devereux, 2010; Sacerdote, 2005; Solon, 1999).

effect that is different from the local average treatment effect estimated by the RD.

4.3 Robustness Checks

As I pointed out in section 3.4, I perform several robustness checks to address concerns that the intergenerational effects may be specific to the baseline estimation. In the Online Appendix, Tables A.5 and A.6 reveal that estimates for applicants and children are robust to alternative bandwiths and polynomials. For simplicity, I only show the relevant coefficients for variables in Tables 4 and 5. In columns (1)–(4), I first run checks using bandwidths that are half and twice the size of the baseline optimal bandwidth according to Calonico et al. (2014) and Imbens and Kalyanaraman (2012), as well as different kernel functions: triangular and epanechnikov. Then, columns (5) – (6) reproduce the baseline specification, but including quadratic and cubic polynomials. While certain regressions lack the proper sample power, and coefficients become increasingly noisy, these remain relatively stable and statistically significant for applicants and children.

Moreover, another possibility is that the discontinous change in outcomes is not unique to the predicted INCORA threshold, or that this one is misplaced. Such scenarios would suggest the existence of confounding covariates and invalidate my empirical strategy. Thus, I design two placebo checks estimating the baseline specification at two fictitious score thresholds: plus and minus 10 points in distance from the predicted threshold. In Tables A.5 and A.6, columns (7) – (8) document that, under these circumstances, estimates for both applicants and children across all outcomes of interest are statistically insignificant, and even change signs in certain regressions. These exercises provide further evidence for the robustness of the results found in sections 4.1 and 4.2. [27]

Lastly, I also run the same regressions for applicants and children in a 2SLS design near the predicted score threshold. I use $recipient_{i,e}$ – or having predicted scores above the predicted threshold – as an instrumental variable for receiving a parcel, and for which Figure 2 already showed a strong first stage. Tables A.7 and A.8 in the Online Appendix present the results. As expected, in both tables columns (1) – (6) document that the effects increase considerably in all cases. This is intuitive because the 2SLS estimator is equivalent to the reduced form divided by the first stage. Nevertheless, many baseline estimations loose their statistical significance – particularly in the case of the children. A most likely explanation is that with small sample sizes, instrumental variable estimates become increasingly noisy (Andrews et al., 2019). Despite this, I interpret the robustness checks as overall favorable to the main RD estimates. [28]

[27] In the Online Appendix, Tables A.9 and A.10 show the OLS regressions smaller lower coefficients. This could be the case of downward attenuation bias, or the OLS could be a biased estimate that is different from those estimated by the RD.

[28] Equally important is the fact that the reform had negligible general equilibrium effects, which even at the local level, discards major changes to labor market conditions.

4.4 Intergenerational Mobility

So far, I have shown that providing land through agrarian reform improved economic opportunity for recipient families, and particularly, their children. However, the local linear RD estimates local intent-to-treat effects and do not necessarily imply that the reform improved aggregate intergenerational mobility. One possible scenario is that the children of non-recipients closed down economic advantages that existed between recipients and non-recipients relative to the children of recipients, yet they were still worse off in absolute terms. Conversely, another possibility is that the children of recipients may have distanced themselves even further from those of non-recipients relative to their point of departure. Therefore, in this section, I analyze intergenerational mobility across the whole sample of applicant families.

Following Chetty et al. (2014), I investigate two classes of economic mobility measures that capture different normative concepts: *relative* and *absolute* mobility. The first, which has been the subject of most prior research on intergenerational mobility, compares outcomes of children from better-off applicants to those of children from worse-off applicants. The focus of this measure is on the relative outcomes of children from different applicant backgrounds (Corak, 2013; Black and Devereux, 2010; Solon, 1999). Meanwhile, absolute mobility measures the mean outcomes of children who grew up in worse-off applicant families. This may be of greater normative interest than relative mobility, because increases in absolute mobility at a given outcome level, holding fixed absolute mobility at other outcome levels, unambiguously increase welfare if one respects the Pareto principle.

Throughout the analysis, I calculate differences in intergenerational mobility between recipient and non-recipient families by examining the joint distribution of applicants' and childrens' outcome ranks for children in the birth cohorts of the 1970s and 1980s. Crucially, father-child rank distributions are also more easily comparable across these groups in this context. I focus on the rank-rank slope, which measures the association between a child's position in the outcome distribution and his parents' position in the distribution, and contrast this statistic against more traditional ways of measuring economic opportunity: (i) the correlation coefficient between children's outcomes and fathers' outcomes; or (ii) the parent-child outcome elasticity ($\frac{dE[logY|X=x]}{glogx}$).

Since I don't have income data, I use educational attainment – measured in years of schooling – and wealth indexes from social benefits records in 2006 as outcomes of interest. Both variables present advantages and disadvantages. Education may be measured more precisely than wealth among the rural poor and suffers less from life-cycle bias. Yet, social

status is observed only in coarse bins, and thus, exhibits less variation (Solon, 1999). [29] For instance, in my sample, a third of the applicants with children in the 1970s and 1980s birth cohorts had zero years of education. Any latent differences in economic opportunity within the bottom third of the applicants' education distribution are difficult to observe. [30] Therefore, when appropiate, in the Online Appendix I calculate bounds on a range of intergenerational mobility statistics that take into account interval censoring.

4.4.1 Relative mobility

I begin by calculating measures of relative mobility. I rank the children in the birth cohorts of the 1970s and 1980s based on years of schooling and wealth relative to other children in the same birth cohorts. I then rank the fathers of these children based on their years of schooling and wealth relative to other fathers with children in these birth cohorts. Notice that rankings are constructed employing the whole sample of applicant families and not within recipients or non-recipients. Let c denote a child's rank and p a father's rank. I characterize mobility based on the slope of the rank-rank relationships $Y^i(c) = E^i(p|c)$ for recipients and non-recipients $i \in [r, nr]$, which identify the correlations between childrens' and fathers' positions in the wealth and education distributions. [31]. Meanwhile, the intercepts measure the expected rank for children from applicant families at the bottom of the these distributions.

Figure 4 presents binned scatter plots of the wealth and education mean percentile rank of children c vs. their fathers' percentile rank p for three groups: Panel A is a pooling of all applicants, and Panel B presents recipients and non-recipients separately. Applicant education is observed in nine bins – representing the highest year of schooling attained by each father – while applicant wealth is seen in ten bins reflecting mean decile averages. The points in the graphs show the mean child rank conditional on having a father in a given bin, which is r_k. In the case of wealth, the conditional expectation of a child's rank given his fathers' rank (or CEF) in all cases is increasing and linear (see Figures 4a and 4c). In the case of education, the gradient is convex but approximates a linear relationship, as well (see Figures 4b and 4d). [32]

[29] In developing countries, transitory incomes can be noisy estimates of lifetime income. These problems are exacerbated among the rural poor. As a result, studies of intergenerational mobility often proxy lifetime opportunity with education.

[30] Also, when ranks are coarsely observed, there is no established methodology for calculating measures that depend on observing fixed quantiles of the parent rank distribution, such as absolute upward mobility or quantile transition matrices.

[31] In the case of education, the expected child outcome in the k^{th} bin is defined as $r_k = E^i(p|c \in [c_k, c_{k+1}]) = \frac{1}{c_{k+1}-c_k} \int_{c_k}^{c_{k+1}} Y^i(c)dc$ where c_k and c^{k+1} define the bin boundaries. For the outer rank bins, $c_0 = 0$ and $c_{K+1} = 100$.

[32] If the rank-rank gradient is understood as a linear approximation to a potentially nonlinear CEF, then many gradients can fit the underlying data equally well. In this scenario, however, linearity seems like a

In Figures 4a and 4b, the gradient of the wealth and education CEFs that pool all applicants is indicative of low relative intergenerational mobility. A child's expected outcome rank is primarily determined by his or her parent's outcome rank. To confirm this, I estimate OLS regressions on the child outcome rank vs. father's outcome rank and parents and childrens' outcome correlations and report them in Table 6. On average, I find that a one percentage point (pp) increase in parent wealth rank is associated with a 0.56 pp increase in the child's mean wealth rank, as reported in column (1). Results are statistically significant at 1% and 5% confidence levels and fairly similar if I use parent and child wealth correlations. Moreover, if I look at education, an additional year of parental schooling is associated with ≈ 0.6 more years of child schooling, suggesting mobility estimates are consistent when using alternative outcomes and statistical methods. These findings are consistent with pervasive rural poverty in Colombia and recent mobility studies in other developing countries (Montenegro and Meléndez, 2014; Corak, 2013; Black and Devereux, 2010).

When I differentiate between recipient and non-recipient families, subtle but revealing patterns emerge. In Figures 4b and 4d, while the CEFs of recipients and non-recipients are askewed, the wealth and educational CEFs of the first are more flattened than those of the former. This probably indicates that, comparing across the whole outcome distributions of applicants, the children of recipients enjoyed higher intergenerational mobility. In Table 6, columns (2) and (3) document that for recipients, a one percentage point (pp) increase in parent wealth rank is associated with a 0.48 pp increase in the child's mean wealth rank, while for non-recipients, this coefficient is 0.61. The difference in wealth rank-rank estimates between recipients and non-recipients is 0.13 pp. Analogous estimates are calculated using education. For recipients, a one year increase in parental education is associated with a 0.48 years increase in the child's education, while for non-recipients, this coefficient is 0.62.

4.4.2 Absolute mobility

The CEFs used in the previous section also allow me to calculate measures of absolute upward mobility, a potentially more relevant measure from a normative perspective. I define absolute upward mobility as μ_{25}^{w} or the expected outcome of children born to applicants who occupy positions in the bottom quarter of the parent rank wealth distribution. I also look at the least educated applicants μ_{33}^{e} – precisely those who had zero years of schooling. When comparing all applicants, I find that these statistics are mechanically related to the rank-rank slopes and do not provide any additional information about mobility. However, when studying groups of recipients and non-recipients, I find that a child's rank in the outcome

plausible assumption.

distributions are effectively absolute outcomes. Upward wealth mobility for non-recipients in μ^w_{25} is 29, while for recipients, it is 34.

Moreover, other measures of upward mobility exhibit similar variation across groups. Table 7 presents quintile transition matrices for the two groups: the probability that a child of group i is in quintile m of the child outcome distribution conditional on his parent being in quintile n of the parent outcome distribution. For instance, in Table 7.a, the probability that a child of a non-recipient reaches the top quintile of the wealth distribution conditional on having fathers in the bottom quintile is 3% compared to the same probability for the child of a recipient, which is 6%. [33] Meanwhile, in Table 7.b the probability that a child of a non-recipient finishes high school conditional on having a father with zero years of education is 16% compared to the same probability for the child of a recipient, which is 24%. One can construct additional measures of mobility beyond those considered here.

Overall, these results complement previous RD estimates presented in sections 4.1 and 4.2. Fortunately, I find that the patterns of group variation in absolute and relative intergenerational mobility are very similar using alternative measures. However, caution should be exerted when drawing conclusions. Statistics were calculated among applicants and not across the whole sample of individuals registered in social benefits records. Estimates are mostly likely underestimated, as they do not consider better-off applicants, who presumably did not register for poverty subsidies. Similarly, these intergenerational mobility effects are still far from average wealth and education levels in the country. According to SISBEN statistics, average years of schooling for cohorts born in the 1970s and 1980s was 6.3 years, compared to 5.1 years for the children of applicants.

5 Mechanisms

The past findings raise the intriguing question of why providing land through agrarian reform had significant intergenerational effects on the rural poor. Past reforms often included prohibitions on sale and other restrictions – including in the Colombian context –, which might well be expected to have decreased economic mobility (Banerjee and Newman, 1993; De Janvry et al., 2015). The country also faced major societal upheavals in the following decades, including urbanization, the implementation of major market and social reforms, and the Colombian civil war. Understanding the channels of persistence is crucial, because these can lead to very different policy conclusions about the convenience of land redistribution. In this section, I draw from Colombian historiography discussed in section 2 to explore

[33] It is useful to analyze multiple measures of mobility, because these depend upon one's normative concept (Fields and Ok, 1999).

theoretical mechanisms that could elucidate why recipients, and particularly their children, were actually more succesful in entering the modern economy (Robinson and Urrutia, 2007; Hirschman, 1967, 1962; Fals-Borda et al., 1962; Currie, 1961, 1951).

5.1 Geographic Mobility

As seminal development economists emphasized at the time, an integral part of moving the rural poor from the traditional sector into the modern economy may be rural-urban migration (Harris and Todaro, 1970; Lewis, 1954). From a theoretical perspective, a widely held view is that this process is often inhibited by migration costs, as the rural poor face important credit constraints that ties them to the countryside (Bryan et al., 2014; Lagakos et al., 2018). I therefore examine geographic mobility as a prime candidate linking applicants of the *Sharecroppers and Tenants Program* to their development paths. In order to make more appropiate comparisons, I use social security records from 2010 to code different measures of migration, distangling between population movements to large cities, small cities, or other rural places. I do this by comparing the municipality where applicants applied for land in 1968–1970 with the municipality where they reported to be residing forty years later.

Table 8 documents the effects on geographic mobility using the baseline RD specification. Columns (1)–(3) report outcomes for applicants, while columns (4)–(6) do the same for their children. In contrast to the view that land ties rural families to the countryside, column (1) illustrates that recipients were on average 20 percentage points more likely to migrate, relative to a mean of 50%. Moreover, columns (2) and (3) show that the majority of this effect is driven by urban migration. Recipients were on average 11 percentage points more likely to migrate to a large city, relative to a mean of 19%, while they were also less likely to migrate to other rural areas by almost the same margin. The estimates are significant at the 5% and 10% confidence. These findings are somewhat striking given that market restrictions from INCORA forbade recipients from selling, and even renting, their parcels during the first ten years of tenure.

Next, results on the children of applicants reinforce this picture. On aggregate, the children had higher migration rates than their parents (50% vs. 72%), a fact consistent with historical national trends. Yet, as reported in column (4), the children of recipients were 27 percentage points more likely to migrate relative to those from non-recipients. As in the case of their parents, effects appear mostly driven by urban migration. Columns (5) to (6) show that the children of recipients were 23 percentage points more likely to move to large urban centers, relative to a mean of 39%, and less likely to had done so to other rural places. Most geographic mobility effects are statistically significant at the 5% confidence level, and

as before, are amplified across generations. As before, Figures 5 and 6 show RD plots for migration.

To further unpack this mechanism, I take a step back and run the same RD specifications that were previously shown in section 4, but discriminate between applicants that did not migrate to large urban centers with those that did. While this excercise is evidently endogenous, it is still informative about the transmission of intergenerational effects. In the Online Appendix Tables A.11 and A.12, columns (1)–(6) reveal that, on average, recipients who did not migrate to large cities and their children exihibit no appreciable statistical differences when compared to their non-recipients counterparts and children along a number of living standards and labor market outcomes. I interpret this robustness check as additional evidence that moving from less-productive rural areas to large urban centers was central to encourage participation in the modern economy.

Moreover, I track whether and when recipients sold their land. In Colombia, market transactions need to be registered in notaries to possess legal validity, although vast informal norms operate in the countryside. Using parcel-level information from the Superintendence of Notaries (*SNR*), I find that up to 30% of recipients formally sold their parcel right after sale prohibitions expired, suggesting they used the land to move in search of better opportunities. [34] These results are consistent with a setting where a productive asset relieved credit constraints (Banerjee et al., 2015, 2011). It also fits with the broader Colombian historiography, which argues that the second half of the 20th century was a period of rapid urbanization and numerous recipients in departments such as Antioquia or Cundinamarca sold their land (Palacios, 2011; Kalmanovitz and López, 2006; Zamosc, 1978).

Overall, the results on geographic mobility shed light on prominent development debates (Harris and Todaro, 1970; Lewis, 1954). While it may well be that this was not the only intermediating mechanism, historical and empirical evidence make it difficult to consider an explanation where migration did not play a central role. Unlike what original proponents of the agrarian reform envisioned, the intergenerational effects do not appear mediated by the consolidation of a mass of productive farmers, despite market restrictions from INCORA designed to do so (Banerjee et al., 2000). Rather, and in contrast with a large body of evidence on the intergenerational effects of shocks or lotteries, these are indicative of an asset shock that relieved credit constraints on urban migration costs and enabled the rural poor to move to large cities, where they accessed better living standards, moved out of agriculture, and entered the formal economy (Bleakley and Ferrie, 2016; Cesarini et al., 2016; Sacerdote, 2005).

[34] Some transactions ocurred while prohibitions on sales were theoretically in place, signaling possible corruption or administrative ineptitude among notaries.

5.2 Investment in Education

Another potential explanation is that recipients could have used the land to invest in the education of their children, who may have subsequently acquired the skills to enter the modern economy. When facing credit constraints, large transfers may be necessary to move rural families past the threshold at which it becomes feasible to invest in their children (Banerjee et al., 2015, 2011, 2000; Galor and Zeira, 1993; Becker and Tomes, 1979). Using information from social benefits records (SISBEN) in 2006, I measure educational attainment with years of schooling and code dummy variables to understand whether a child finished primary school, high school, vocational education, or college, was attending school or incurred in child labor. These variables should reflect investments made decades earlier, even if the timing of measurement was long after the reform. Again, any result should be interpreted as a lower bound, because better-off households do not normally register for poverty subsidies.

In Table 9, I examine the educational effects on the children of applicants born after the reform. I divide the sample between adult and young children – or those below 18 years of age in 2006. Column (1) indicates that adult children of recipients had, on average, 1.5 more years of schooling relative to those from non-recipients, a significant effect compared to a mean of 5.1 years. Column (2) shows that they were also, on average, 17 percentage points more likely to had completed primary school, compared to a mean of 52%. These coefficients are significant at the 5% confidence level. Moreover, columns (3) to (5) also suggest that adult children of recipients were more likely to finish high school, vocational education, or college, but the coefficients are statistically insignificant. Finally, columns (6) and (7) show that young children were then, on average, 4 percentage points more likely to attend school and less likely to incur in child labor, although this last coefficient was not statistically significant. [35]

These findings highlight that investment in the education of children was also an important intermediating mechanism. Consistent with previous evidence, investment in education may have been facilitated by migration to large urban centers, where economic agglomeration complemented acquired skills allowing the children of recipients to enter the modern economy. While data availability partially restricts the analysis, the fact that the difference in average years of education in 2006 between rural and urban areas was approximately 3.5 years suggests that almost all educational differences among the children of applicants are explained by geographic mobility. Conversely, the results are also in line with the broad expansion of the education system, particularly in urban areas, following major social reforms

[35] When I run these same regressions, but include the children born before the reform, the educational results attenuate (see Online Appendix, Table A.13). This suggests that, as expected, the children of recipients born after the reform were more likely to be affected.

in the 1980s and 1990s.

5.3 Conflict

In the second part of the 20th century, many developing countries that pushed for agrarian reforms also suffered severe social unrest and even civil conflict. [36] As explained in section 2, in Colombia, numerous historical accounts suggest civil conflict may also have affected the rural poor differentially, as the reform was in part implemented to apease revolutionary threats (Karl, 2017; Palacios, 2011; Fajardo, 1986, 1979; Fals-Borda et al., 1962; Lleras-Restrepo, 1961). In this section, I explore this issue. I exploit information from civil conflict victims in the Unique Registry of Victims (RUV) 1985–2010, death certificates from Vital Statistics (RUAF), and criminal records from judicial authorities, to code dummy variables that measure patterns of violent deaths, rural displacement, enlistment in rebel movements or criminal activities.

Though caution is warranted, because data suffers from severe measurement error, I find very marginal effects of the civil conflict on recipients. Among applicants who died before 2006, column (1) in Table 10 illustrates recipients were 12 percentage points more likely to had suffered a violent death, relative to a mean of 8%. Similarly, recipients were only 5 percentage points more likely to had been forcefully displaced, relative to a mean of 3%. The effects are only weakly significant at 10% confidence level and are mostly driven by applicants who lived in places where the conflict was particularly intense. Moreover, column (3) shows recipients and non-recipients were equally likely to had engaged in criminal activities. In general, the evidence suggests few applicants suffered violent actions in such a proportion that could explain the main findings.

Additionally, in columns (4) to (6), I show the coefficients for the children of applicants. Weak results for recipients completely disappear when looking at the subsample of children, indicating these dissipated across generations. Throughout the columns, estimates are small and statistically insignificant, which suggests it was highly unlikely that conflict had any direct differential effect on the children of applicants. Also, on aggregate, only 5% of them were forcefully displaced – a figure that is higher than their parents but still below the national historical average of 10% (CNMH, 2016). In summary, the results highlight that while conflict was a formative event in the history of the country – and in some cases evidently impacted applicant families – it is hard to argue that, on aggregate, violence drove the intergenerational effects of sections 4.1 to 4.3.

[36] For instance, most Latin American nations (El Salvador, Guatemala, Nicaragua, Bolivia, or Perú), the Philippines, Vietnam, Zimbabwe, and South Africa (Dai and Tai, 1974).

6 Cost-Benefit Analysis

After investigating mechanisms, in this section I perform a simple cost-benefit analysis of the *Sharecroppers and Tenants Program* and discuss possible implications for development policy. I weigh the intergenerational effects against the fiscal costs of the reform to further evaluate its convenience. I first calculate the benefits for rural families, focusing on increased earnings for the children of recipients. I caution that all of the calculations reported should be treated as rough estimates, because they rely on several strong assumptions, starting with the basic premise that the local RD estimates from section 4.2 can be extrapolated to all recipient families. Recall that the children of recipients accumulated, on average, 1.5 more years of education relative to those from non-recipients. Several studies indicate returns to education in the 1970s and 1980s in Colombia oscillated around 10%.

I translate these estimates into a predicted lifetime earnings impact by assuming that (1) this 10% increase in the children's earnings remains constant over the life cycle; (2) the life cycle profile of earnings for recipients follows half of the Colombian minimum wage starting in 1985, the year when a child born in 1970 would be graduating from school; (3) the real wage growth rate is 1%, approximately the rate of wage growth in the country over the past three decades; and (4) the discount rate is 7%, approximately the 10-year government bond rate. This a reasonable approach, as 80% of the children of applicants earned less than the minimum wage in 2010, and recent studies suggest average rural wages are equivalent to half of the minimum wage (Montenegro and Meléndez, 2014). Under these baseline assumptions, a child of a recipient born just after the reform increased total lifetime earnings to USD 4,515 today. The present value of this increase was USD 694 (or $12,846 Colombian pesos) in 1970. [37]

Next, I turn to the fiscal costs of the reform. According to INCORA statistics, land redistribution cost the state 0.5% of GDP in 1970, a sizable effort equivalent to 7% of the national budget. As described in section 2.1, the program only benefited 19,478 rural families at an average cost of USD 2,711 (or $50,000 Colombian pesos) per recipient family (Tamayo, 1970). Today, the figure roughly amounts to USD 17,638. Three quarters of this value corresponded to the costs of land expropiations, which involved compensating landowners for the acquired lands. The rest included costs related to legal advice when judicial sentences challenged INCORA decisions, and agricutural investments made before

[37] I estimate the lifetime earnings of a child by projecting half of the minimum wage in 1985 over 47 years (18 to 65 years old) and multiplying it by 10%. I apply a 1% growth rate and a 7% discount rate to this profile to obtain an undiscounted sum of lifetime earnings and a PDV in 1970 of USD 552. I also employ sensitivity analysis to show how results evolve conditional on various parameters. Following empirical evidence in Colombia, in Table 11 I use higher and lower ranges for educational returns, wage levels, real wage growth paths, and discount rates.

the allocation of parcels to recipient families (INCORA, 1970; INCORA, 1974).

Combining information on benefits and costs, the cost-benefit analysis indicates the *Sharecroppers and Tenants Program* most likely yielded net losses. In the baseline scenario presented in the Online Appendix Table A.14, the fiscal investments made per recipient family had a private rate of return of -80%. More favorable scenarios still yield rates of -40%, suggesting that, despite considerable intergenerational effects, the reform most likely did not outweigh its fiscal costs by a large margin. In general, too few rural families were allocated parcels, while expropiations consumed significant resources. However, the analysis neglects important factors that ought be considered in a more comprehensive cost-benefit evaluation. First, it ignores other potential benefits, such as improved living standards of recipients or future generations. It also excludes possible social externalities, although, as explained earlier, these may have been negligible in this particular context. [38]

Together with findings on geographic mobility and investment in education, the evidence has broad implications for development policy. If the reason that recipients benefit from accessing land is to sell it to relieve credit constraints and migrate to urban areas in search of better economic opportunities, then policymakers can think of alternative policies that would subsidize these costs instead going through the cumbersome process of redistributing land. Apart from the fiscal costs incurred, agrarian reforms often provoke widespread political tensions in society. Future research should shed light on whether, for example, other asset transfers or credit incentives can be a more socially effective tool for improving economic mobility among the rural poor.

7 Conclusions

This papers examines the intergenerational effects of providing land to the rural poor through agrarian reform – a common strategy for improving economic mobility in developing countries. I use ID numbers to track applicants to the 1968 Colombian agrarian reform and their children in contemporary administrative data. I then exploit discontinuities in the allocation of parcels and find that, after forty years of the reform, recipients experienced increased living standards relative to non-recipients. The effects amplified across generations as the children of recipients were also more likely to enter the formal economy, work in high-skilled sectors and become entrepreneurs – an entire bundle of measures that form the nexus of modern economic life and indicative of considerable upward economic mobility.

I draw from Colombian historiography to empirically explore theoretical mechanisms. In

[38] Improving intergenerational mobility has been shown to generate various externalities in other developed and developing settings (Corak, 2013; Black and Devereux, 2010; Solon, 1999).

contrast to a widely held view that land ties the rural poor to the countryside, these findings appear mediated by a relief of credit constraints that allowed recipient families to migrate to urban centers and invest in the education of their children, who subsequently used the skills to find new economic opportunities in the modern economy. Finally, I evaluate the cost-effectiveness of this policy, an important element when analyzing its convenience. I compare previous intergenerational benefits with the fiscal costs of agrarian reform. Across a number of potential scenarios, calculations from a simple cost-benefit evaluation yield that land redistribution was largely not cost-effective.

Consequently, I argue these findings have broad implications for development policy. If the reason that recipients benefit from accessing land is to sell it to relieve credit constraints, then policymakers can think of alternative policies that would subsidize these costs, rather than going through the very costly process of seizing land from powerful interests. Future research should shed light on whether, for example, other asset transfers or credit incentives can be more a more socially effective tool for reducing poverty and improving economic mobility. Moreover, another important question is whether the general equilibrium effects of these types of policies are welfare improving for society, an exercise that would inquire about broader externalities and the fate of expropiated landowners.

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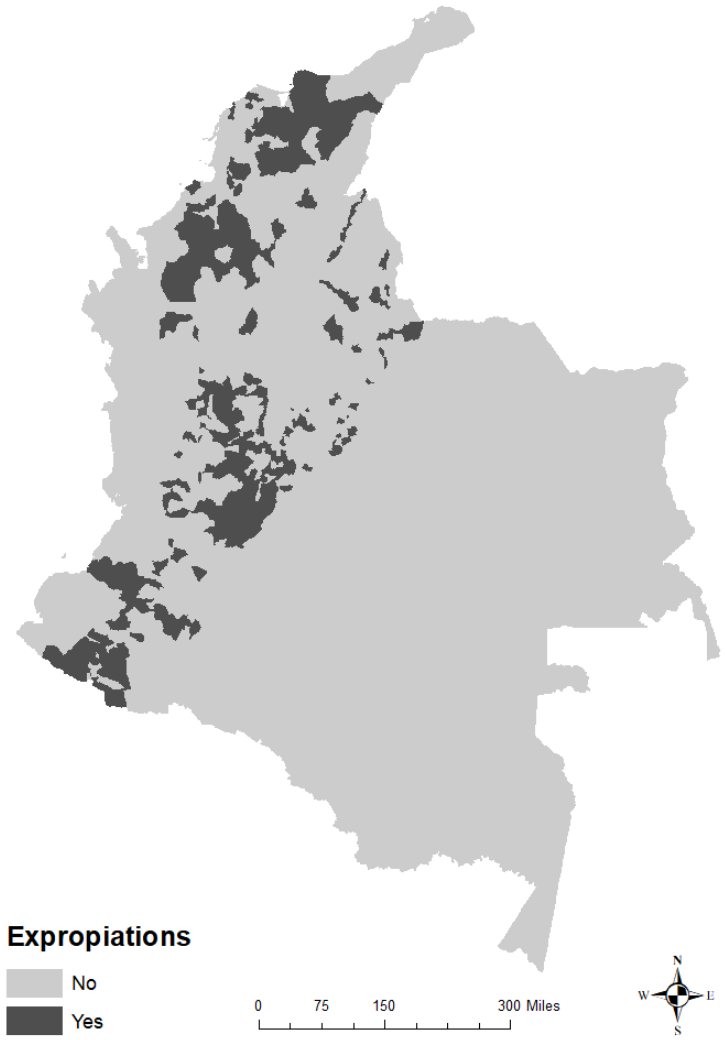
Figure 1: Expropiations in 1968-1972



Notes: This map shows the geography of expropiations between 1968-1972. Municipalities in darker colour experienced at least one expropiation during agrarian reform. Most expropiations occured in the Andean and Caribbean regions. Source: INCORA.

Table 1: INCORA Score System

	Points
Age (in years):	
14-17	10
18-24	15
25-44	20
45-54	15
55-60	10
<14 or >60	3
Agricultural Experience (in years):	
Points per year	2
Assets (in pesos):	
0-5.000	20
5.001-10.000	15
10.001-20.000	10
20.001-30.000	5
>30.000	0
Housing Investments (in pesos):	
0-5.000	0
5.001-10.000	5
10.001-20.000	10
20.001-30.000	15
>30.000	20

Notes: This table presents the INCORA score system used to allocate land during agrarian reform. Surveyed applicants were assigned points in each category, which were then aggregated into a continous score. Poorer families with more experienced household heads were prioritized. Those with scores above expropiation thresholds were eligible to receive a parcel. Source: INCORA.

Table 2: Applicants and Children in Contemporary Administrative Data

Database	Name	Year Observed	Recipients	Non-Recipients	Difference	Standard Error	Children of Recipients	Children of Non-Recipients	Difference	Standard Error
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Social Security	RUAF	2010	46.3	43.9	0.0237	(0.0222)	88.9	89.5	-0.00585	(0.0314)
Formal Labor Market	PILA	2010	2.3	1.1	0.0122**	(0.00544)	23.6	15.5	0.0815**	(0.0388)
Social Benefits	SISBEN	2006	33.6	33.5	-0.0316	(0.0319)	61.7	64.9	-0.0238	(0.0530)
Business Records	RUES	2010	0.7	0.5	0.00223	(0.00195)	12.2	8.5	0.0379*	(0.0217)

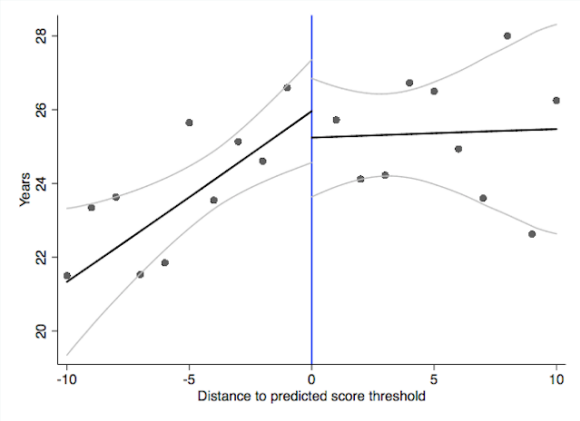
Notes: This table shows the linkage between agrarian reform data in 1968-1970 with contemporary administrative databases for applicants and their children. Columns (1) and (2) indicate the official name of an administrative database, shown in rows, and the year in which it is observed. Coeficients in columns (3)-(4) and (7)-(8) show the matching rates for applicants and children in percentage terms, differentiating between recipients and non-recipients. The difference of these two pairs of columns is shown in columns (5) and (9) and the standard errors in columns (6) and (10). The linked data of 45% of (or 87% of living) applicants and 89% of children in social security records (RUAF) are the baseline samples for regressions in Tables 4 and 5 (Columns 3-8), 8, and 10. The linked data of 33% of applicants and 63% of children in social benefits records (SISBEN) are the baseline samples for regressions in Tables 4 and 5 (Columns 1-2), 6, 7 and 9. *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Pre-Treatment Balance in 1968-1970

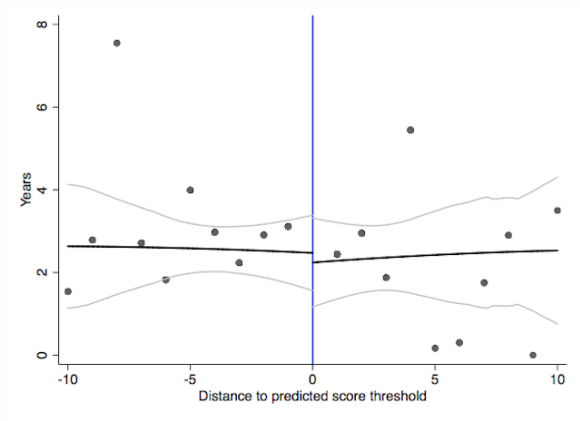
	Age	Years of Schooling	Years of Agricultural Experience	Log(Wages)	Has House	Plot Area (in Hec)	Grows Cash Crops	Grows Staple Crops
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Applicants							
Recipient	-2.137	-0.125	1.134	-0.107	-0.0379	-0.222	0.0934	-0.0342
	(1.627)	(0.975)	(0.842)	(0.137)	(0.0265)	(0.296)	(0.142)	(0.0183)
Observations	410	401	410	462	540	540	462	462
Bandwidth	4.3	5.2	4.8	5.4	6.1	6.1	5.5	5.5
Mean Dep. Var.	24.8	2.06	7.0	2.3	0.17	2.1	0.43	0.80

Notes: This table documents pre-treatment balance among applicants within the optimal RD bandwidth. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at applicant family level are in brackets. *Recipient* is an indicator variable equal to 1 if an applicant was eligible to be allocated land during the agrarian reform 1968-1970. The unit of observation is the applicant. All regressions include the following controls: age, sex, marital status, expropiation file fixed-effects. The RD regressions also include a local linear polynomial estimated separately on each side of the threshold. Bandwidths are chosen using the MSE optimal procedure suggested by Calonico et al. (2017). The outcome data source for column (1) is RUAF, column (2) is SISBEN and columns (3)-(8) is INCORA. For a description of each dependent variable see Online Appendix B Table A.1 and Table A.3.

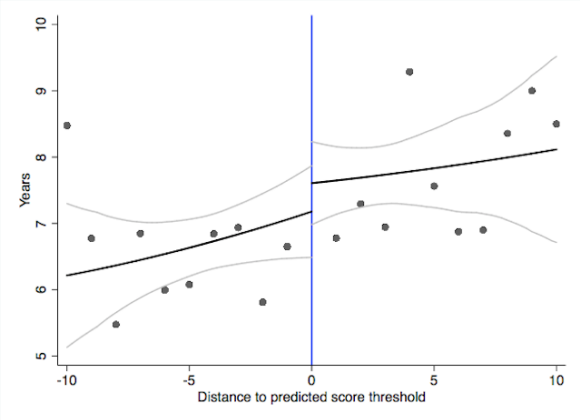
Figure 2: Pre-Treatment Balance 1968-1970



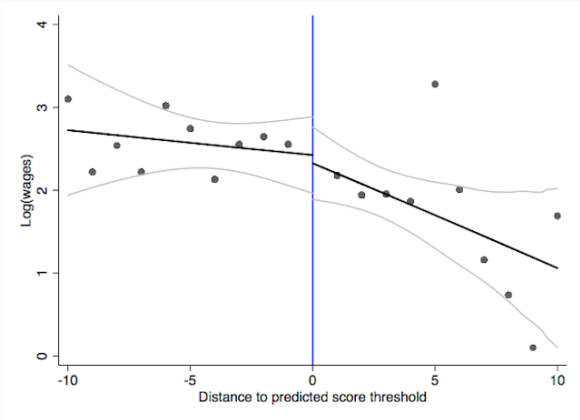
(a) Age



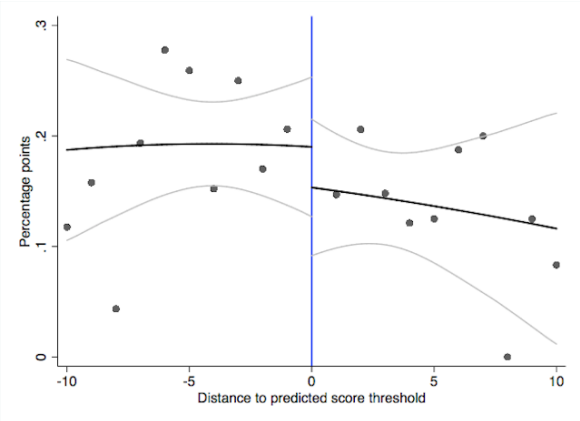
(b) Years of Schooling



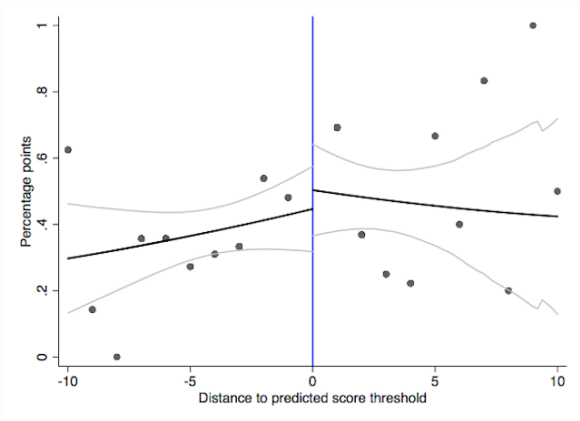
(c) Years of Agricultural Experience



(d) Log(Wages)



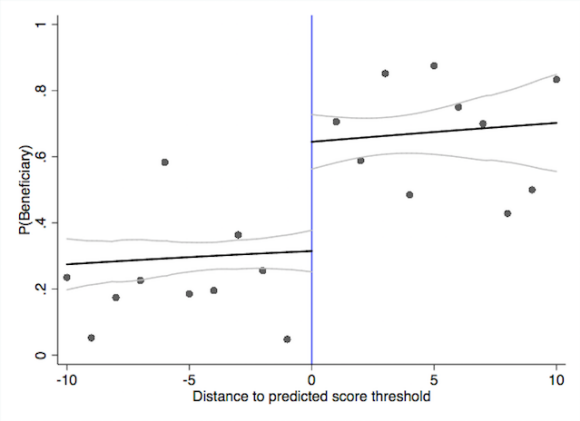
(e) Has House



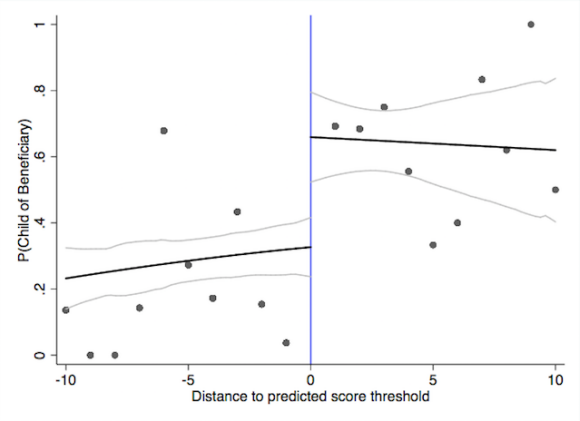
(f) Staple Crops

Notes: This figure graphically documents pre-treatment balance within the RD optimal bandwidth. It shows RD plots estimates from the effect of being an applicant eligible to receive land during the agrarian reform 1968-1970 on different pre-treatment characteristics. Each point plots an average value within a bin. Discontinuity fixed-effects have been partialled out. The solid line plots a local linear regression and dashed lines show 95% confidence intervals. Source: INCORA, RUAF, SISBEN.

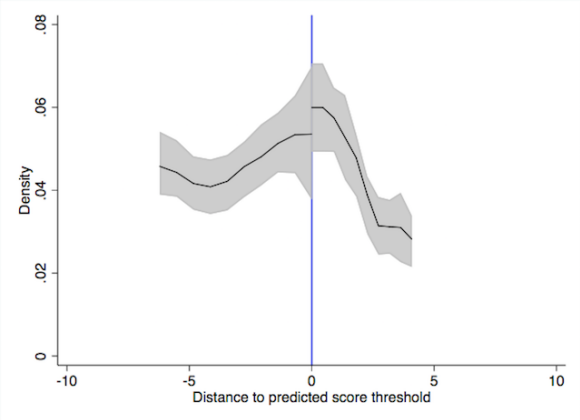
Figure 3: First Stage



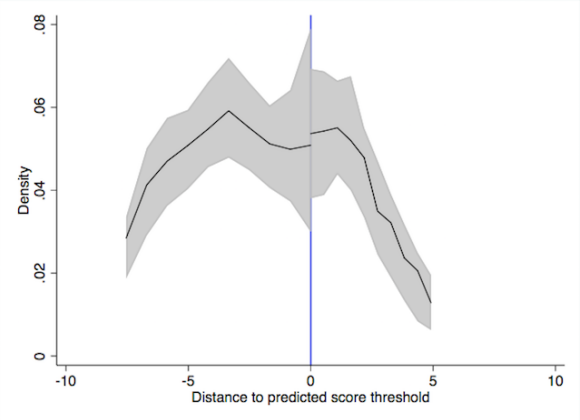
(a) First Stage of Applicants



(b) First Stage of Children



(c) McCrary Test of Applicants



(d) McCrary Test of Children

Notes: This figure graphically documents the first stage of the RD design and McCrary tests. Panel (a) presents the estimated RD plot on an indicator variable equal to 1 if an applicant was allocated land during the agrarian reform 1968-1970. Panel (b) presents the same regression on an indicator variable equal to 1 if a child had an applicant parent that was allocated land during the agrarian reform 1968-1970. The points represent the average value of the outcome variable in score bins. The regressions are estimated using local quadratic polynomials in the predicted INCORA score estimated separately on each side of the reform threshold and use an uniform kernel. Panels (c) and (d) implement the sorting test suggested by McCrary (2008) and plots the number of observations in each cumulative predicted INCORA score bins for applicants and children. The plotted regressions use the number of observations in each bin as the dependent variable on each side of the cut-off to test if there is a discontinuity in the density of applicants at the score cut-off. 95% confidence intervals around the estimated lines are shown in the shaded area. Source: INCORA, SRN.

Table 4: Applicants in the Modern Economy

	In 2006		In 2010						
	Wealth Index	Housing Index	Registered for Poverty Subsidies	Above Minimum Wages	Formal Sector	Employed in: Entrepreneurship	Agriculture	Manufacturing	Services
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Recipient	0.196**	0.298**	-0.0927	0.0429*	0.118	0.00585	-0.152*	0.0108	0.154*
	(0.0999)	(0.124)	(0.108)	(0.0252)	(0.0833)	(0.0111)	(0.0811)	(0.0138)	(0.0827)
Observations	405	345	324	577	415	445	445	415	415
Bandwidth	7.0	5.3	4.0	7.2	4.2	6.4	5.2	5.3	5.2
Mean Dep. Var.	0	0	0.72	0.02	0.03	0.01	0.64	0.04	0.14

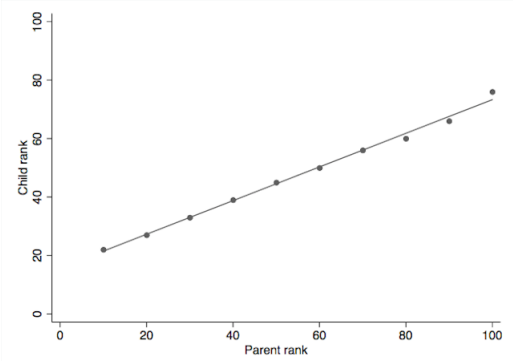
Notes: This table documents the long-run effects of providing land in 1968-1970 on contemporary economic outcomes using the RD design. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at applicant family level are in brackets. *Recipient* is an indicator variable equal to 1 if an applicant was eligible to be allocated land during the agrarian reform 1968-1970. All regressions include the following controls: age, sex, marital status, expropiation file fixed-effects. Regressions also include a local linear polynomial estimated separately on each side of the threshold. Bandwidths are chosen using the MSE optimal procedure suggested by Calonico et al. (2017). The outcome data source for columns (1)-(3) is SISBEN, for (4)-(5) and (7)-(9) is RUAF and column (6) is RUES. For a description of each dependent variable see Online Appendix A Table A.3.

Table 5: Children of Applicants in the Modern Economy

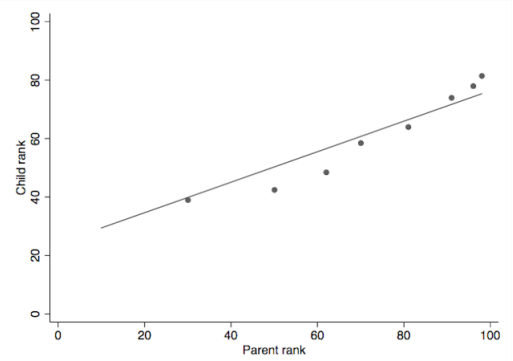
	In 2006		In 2010						
	Wealth Index	Housing Index	Registered for Poverty Subsidies	Above Minimum Wages	Formal Sector	Employed in: Entrepreneurship	Agriculture	Manufacturing	Services
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Recipient	0.319**	0.369***	-0.215*	0.221**	0.245**	0.104**	0.0239	0.132**	-0.149
	(0.151)	(0.108)	(0.123)	(0.0868)	(0.111)	(0.0467)	(0.101)	(0.0607)	(0.115)
Observations	393	298	460	460	460	460	496	460	496
Bandwidth	6.4	4.1	4.2	4.6	4.2	4.5	5.4	4.4	5.4
Mean Dep. Var.	0	0	0.58	0.17	0.39	0.10	0.35	0.11	0.30

Notes: This table documents the intergenerational effects of providing land in 1968-1970 on contemporary economic outcomes using the RD design. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at applicant family level are in brackets. *Recipient* is an indicator variable equal to 1 if a child had an applicant parent eligible to be allocated land during the agrarian reform 1968-1970. All regressions include the following controls: age, sex, marital status, expropiation file fixed effects. Regressions also include a local linear polynomial estimated separately on each side of the threshold. Bandwidths are chosen using the MSE optimal procedure suggested by Calonico et al. (2017). The outcome data source for columns (1)-(3) is SISBEN, for (4)-(5) and (7)-(9) is RUAF and column (6) is RUES. For a description of each dependent variable see Online Appendix A Table A.3.

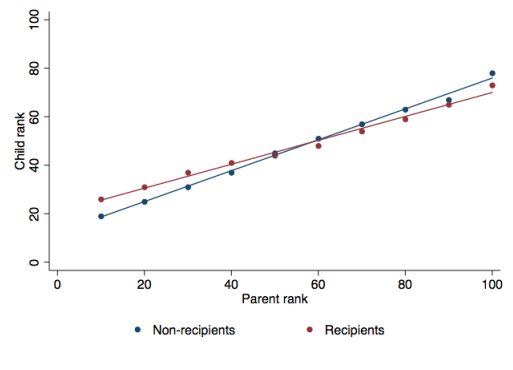
Figure 4: Intergenerational Mobility



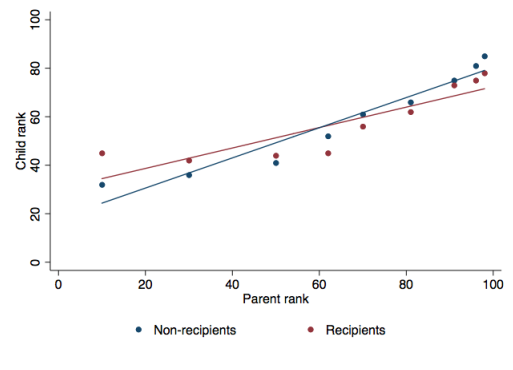
(a) Wealth Index



(b) Education



(c) Wealth Index



(d) Education

Notes: This figure graphically documents the intergenerational effects of providing land in 1968-1970 among all applicants. It shows plots of child rank against parent rank using a wealth index and years of schooling in 2006 as outcomes of interest for all applicants (Figures 3a and 3b) and recipients and non-recipients separately (Figures 3c and 3d). Source: INCORA, SISBEN.

Table 6: Intergenerational Mobility

Child Outcome	Parent Outcome	All Applicants	Recipients	Non-recipients
		(1)	(2)	(3)
1. Wealth index	Wealth index	0.606***	0.519***	0.643***
		(0.0178)	(0.0128)	(0.0192)
2. Wealth rank	Wealth rank	0.576***	0.479**	0.605**
		(0.182)	(0.204)	(0.278)
3. Education	Education	0.557***	0.476***	0.623***
		(0.127)	(0.0978)	(0.106)
4. Education	Wealth rank	0.586***	0.557***	0.623***
		(0.0843)	(0.104)	(0.0929)

Notes: This table documents the intergenerational effects of providing land in 1968-1970 among all applicants. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in brackets. Wealth index and years of schooling in 2006 are used as outcomes of interest. Each cell reports the coefficient from a univariate OLS regression of an outcome for children in the 1970s and 1980s cohort on a measure of their parents' outcomes, with standard errors in parentheses. All rows report estimates of slope coefficients from linear regressions of the child outcome on the parent outcome measure. The unit of observation is the rank in rows (2) and (4) and the individual in rows (1) and (3). Source: INCORA, SISBEN.

Table 7: Transitional Matrices

(a) Wealth

Parent Quintile	Child Quintile 1	2	3	4	5
			Non-recipients		
1	45	24	16	12	3
2	22	40	22	14	2
3	17	22	34	16	9
4	8	15	18	39	20
5	4	8	16	24	48
			Recipients		
1	40	23	18	13	6
2	19	37	23	14	7
3	15	20	37	16	12
4	5	14	20	38	23
5	5	10	16	25	44

(b) Education

	Child Education Level None	Some Primary	Primary	Middle	High School	Technical	College
				Non-recipients			
	(7%)	(27%)	(26%)	(12%)	(21%)	(6%)	(0%)
None (38%)	10	34	22	12	16	5	0
Some primary (46%)	7	29	24	11	23	6	1
Primary (15%)	0	5	44	15	28	8	0
Middle (1%)	0	0	0	34	33	33	0
				Recipients			
	(8%)	(16%)	(32%)	(18%)	(21%)	(4%)	(1%)
None (24%)	7	18	33	13	24	4	0
Some primary (47%)	9	24	36	13	11	5	2
Primary (23%)	0	9	30	38	33	0	0
Middle (5%)	0	0	10	40	30	20	0

Notes: These tables show wealth (Table 9a) and education (Table 9b) intergenerational transition matrices for applicants and children, differentiating between recipient and non-recipient families. Each cell reports the percentage of children in the outcome level given by the column conditional on having parents in the outcome level given by the row for children in the 1970s and 1980s birth cohorts. Source: INCORA, SISBEN.

Table 8: Geographic Mobility

	In 2010					
	Applicants			Children of Applicants		
	Migration	Urban Migration	Rural Migration	Migration	Urban Migration	Rural Migration
	(1)	(2)	(3)	(4)	(5)	(6)
Recipient	0.198**	0.111*	-0.0937*	0.265***	0.227***	-0.121
	(0.0869)	(0.0626)	(0.0526)	(0.0861)	(0.0832)	(0.118)
Observations	451	415	533	560	424	460
Bandwidth	5.0	4.9	6.5	6.4	4.3	4.5
Mean Dep. Var.	0.50	0.19	0.16	0.72	0.39	0.24

Notes: This table documents the intergenerational effects of providing land in 1968-1970 on migration using the RD design. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at applicant family level are in brackets. *Recipient* is an indicator variable equal to 1 if an applicant was eligible to be allocated land during the agrarian reform 1968-1970. All regressions include the following controls: age, sex, marital status, expropiation file fixed-effects. Regressions also include a local linear polynomial estimated separately on each side of the threshold. Bandwidths are chosen using the MSE optimal procedure suggested by Calonico et al. (2017). The outcome data source for columns (1)-(6) is *RUAF*. For a description of each dependent variable see Online Appendix A Table A.3.

Table 9: Investment in Education

	In 2006						
	Adult Children					Young Children	
	Years of Schooling	Primary School	High School	Vocational Education	College	Attending School	Child Labor
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Recipient	1.551**	0.165**	0.136	0.0822	0.0688	0.0409**	-0.116
	(0.732)	(0.0805)	(0.105)	(0.0722)	(0.0549)	(0.0195)	(0.112)
Observations	298	367	367	367	367	107	107
Bandwidth	4.4	5.3	5.3	5.3	5.3	5.8	5.8
Mean Dep. Var.	5.1	0.52	0.28	0.05	0.03	0.74	0.10

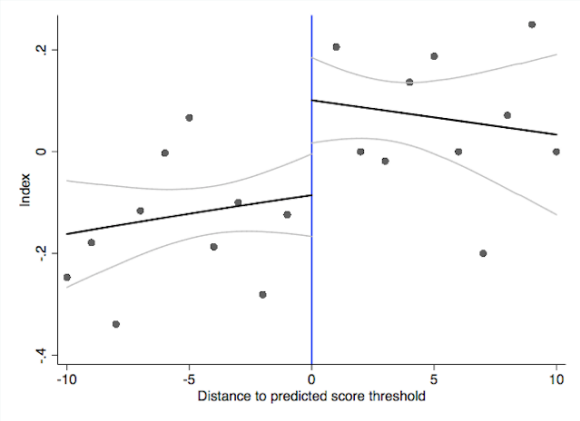
Notes: This table documents the impacts of having received land in 1968-1970 on the education of children born after agrarian reform using an RD design. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at applicant family level are in brackets. *Recipient* is an indicator variable equal to 1 if a child had an applicant parent eligible to be allocated land during the agrarian reform 1968-1970. The unit of observation is the child of an applicant born after the reform. All regressions include the following controls: age, sex, marital status, expropiation file fixed-effects. Regressions also include a local linear polynomial estimated separately on each side of the threshold. Bandwidths are chosen using the MSE optimal procedure suggested by Calonico et al. (2017). The outcome data for columns (1)-(7) is SISBEN. For a description of each dependent variable see Online Appendix A Table A.3.

Table 10: Conflict

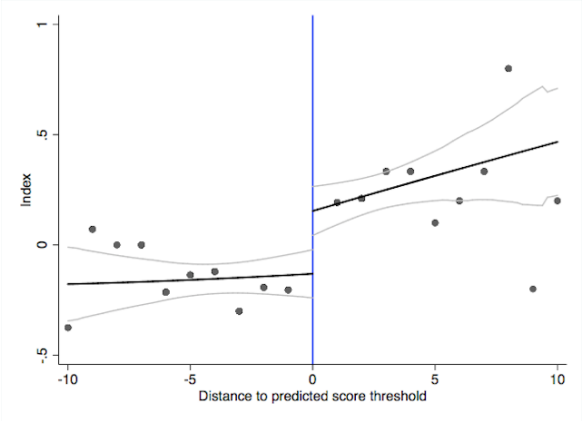
	In 2010					
	Applicants			Children of Applicants		
	Violent Death	Forced Displacement	Criminal Record	Violent Death	Forced Displacement	Criminal Record
	(1)	(2)	(3)	(4)	(5)	(6)
Recipient	0.117*	0.0490*	-0.0379	0.0538	0.0945	0.0214
	(0.0504)	(0.0193)	(0.0165)	(0.0673)	(0.152)	(0.0366)
Observations	645	573	468	645	1328	468
Bandwidth	5.2	6.5	6.2	6.2	7.5	6.2
Mean Dep. Var.	0.08	0.03	0.01	0.11	0.05	0.01

Notes: This table documents the intergenerational effects of providing land on conflict outcomes using the RD design. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at applicant family level are in brackets. *Recipient* is an indicator variable equal to 1 if an applicant was eligible to be allocated land during the agrarian reform 1968-1970. All regressions include the following controls: age, sex, marital status, expropiation file fixed-effects. Regressions also include a local linear polynomial estimated separately on each side of the threshold. Bandwidths are chosen using the MSE optimal procedure suggested by Calonico et al. (2017). The outcome data sources for columns (1) - (6) is INCORA, RUAF, RUPTA, PROCURADURIA. For a description of each dependent variable see Online Appendix A Table A.3.

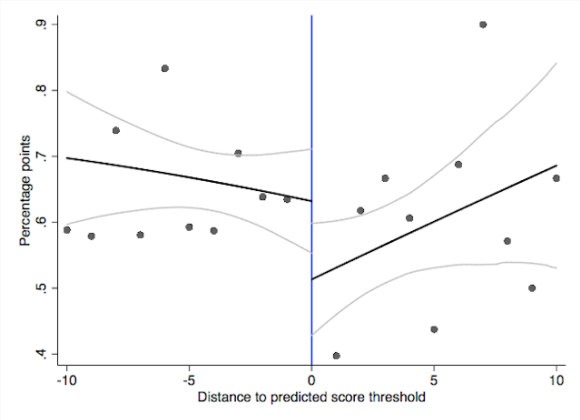
Figure 5: Reduced Forms for Applicants



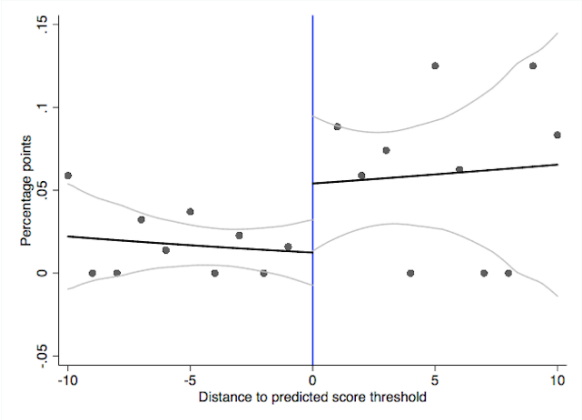
(a) Wealth index



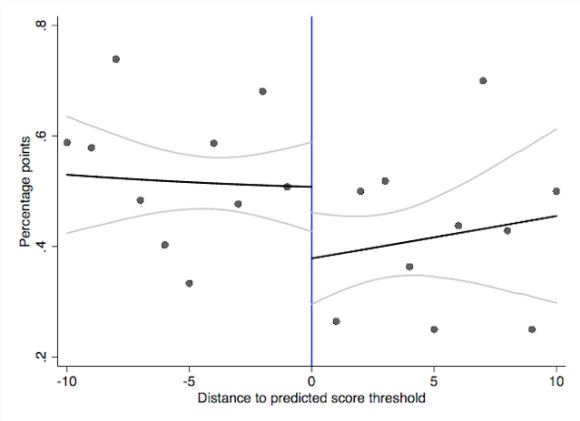
(b) Housing Index



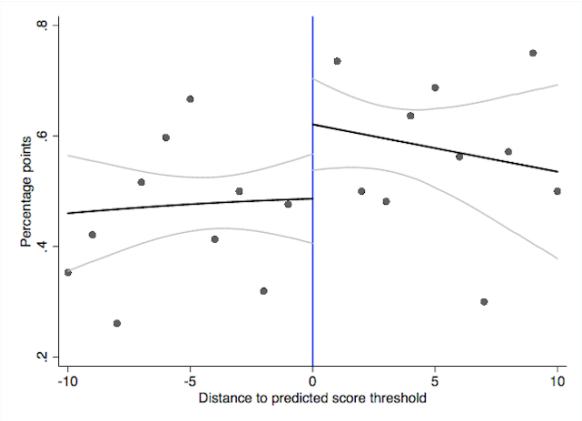
(c) Registered for Poverty Subsidies



(d) Formal Sector



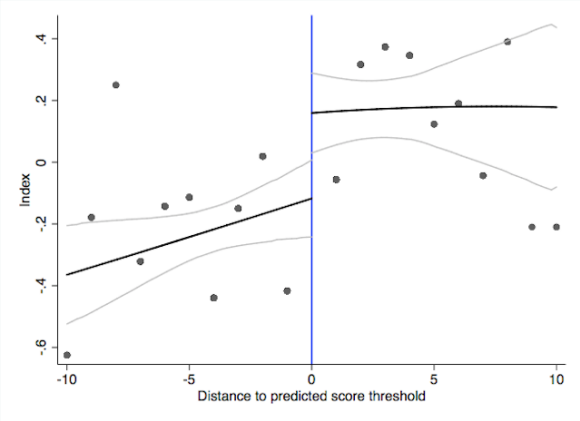
(e) Agriculture



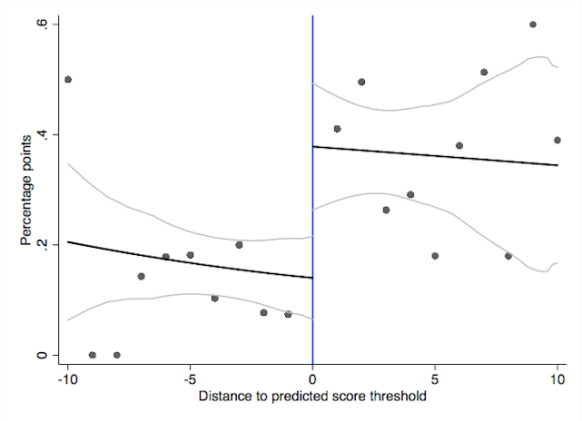
(f) Migration

Notes: This figure graphically documents RD reduced forms for applicants. RD plots show the long-run effects of providing land during the agrarian reform 1968-1970 (*Recipient*) on different outcome variables. Each point plots an average value within a bin. Discontinuity fixed-effects have been partialled out. The solid line plots a local linear regression and dashed lines show 95% confidence intervals. Source: INCORA, RUAF, SISBEN.

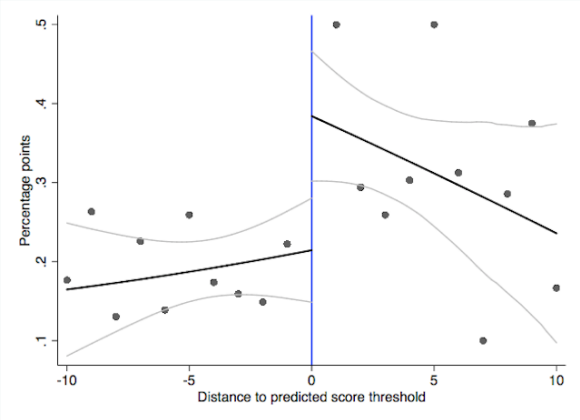
Figure 6: Reduced Forms for Children of Applicants



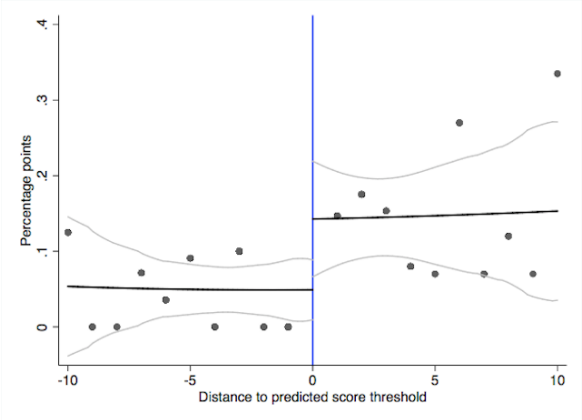
(a) Wealth Index



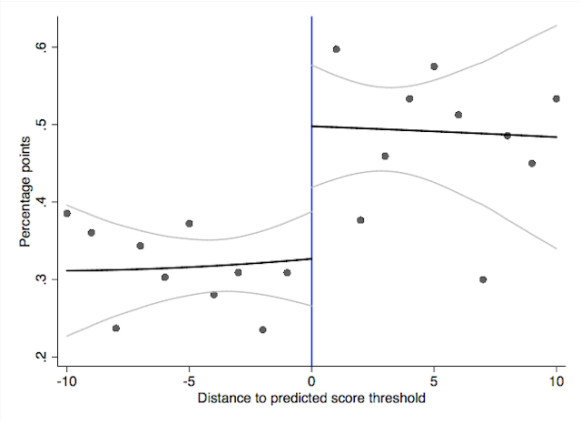
(b) Above Minimum Wages



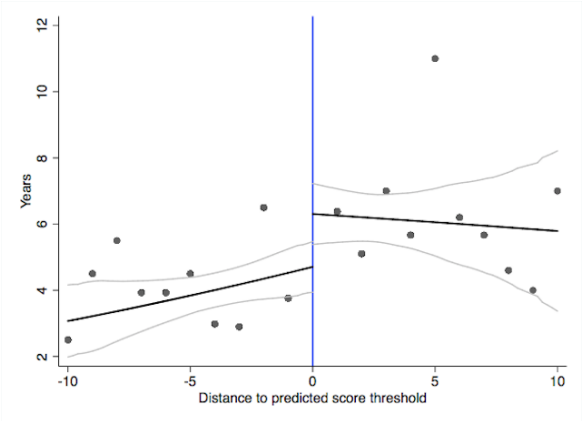
(c) Formal Sector



(d) Manufacturing



(e) Urban Migration



(f) Years of Schooling

Notes: This figure graphically documents RD reduced forms for children. RD plots show the intergenerional effect of providing land during the agrarian reform 1968-1970 (*Recipient*) on different outcome variables. Each point plots an average value within a bin. Discontinuity fixed-effects have been partialled out. The solid line plots a local linear regression and dashed lines show 95% confidence intervals. Source: INCORA, SISBEN, RUAF.