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

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Abstract

This study examines the intergenerational impacts 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 recipients to migrate to urban centers and invest in the education of their children.

Keywords: Agrarian reform, intergenerational mobility, modern economy, Colombia.

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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. This 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 them ties 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 to track 45% of (or 86% of living) recipients and non-recipients and 89%

of the subsample of children. [1]

I estimate the causal impacts of providing land by exploiting a source of plausibly random variation that influenced 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 characteristics to assign them a score. Depending on geographic conditions, INCORA officials determined the number of parcels available for allocation. Only after having this information, officials set score thresholds, making applicant manipulation difficult. Applicants with scores above thresholds were eligible to receive 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 socioeconomic dimensions. [2]

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 households eligible to be allocated land between the years of 1968 and 1970 increased their wealth index in 2006 by 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. In 2010, they were marginally less likely to register for government poverty subsidies and 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, which limited their use of financial markets. This suggests positive but modest developmental impacts.

I then look at intergenerational dynamics to understand whether these impacts 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 24 percentage points more likely to enter the formal sector, relative to a sample mean of 39%, and 22 percentage points more likely to earn above minimum wages, compared to a base of 17%. They were also more likely to

[1] This includes the following databases: Vital Statistics, Social Security, Social Benefits, Financial System, Civil Conflict, and Chambers of Commerce.

[2] As a validity check on the identification strategy, I further show that there is no selective sorting that could affect my outcomes of interest or consistently bias the findings. In Appendix A, I present evidence that results are highly robust to the selection of bandwidth and RD functional form.

work in high-skilled sectors, work as formal entrepreneurs, contribute to Social Security, and use financial markets – an entire bundle of measures that form the nexus of modern economic life. As 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 report local treatment effects, I complement the analysis by studying intergenerational mobility among all applicants. While causal claims cannot be made with this exercise, it is still informative about economic mobility. 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.

These ranks are almost linear and highly robust to alternative specifications. Across all applicants, relative intergenerational mobility was low, but the children of recipients exhibited better 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 show 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, show *amplifying* effects across generations.

After documenting intergenerational impacts, I draw on Colombian historical evidence 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. Using the regression discontinuity design, I compare the municipality where applicants lived in 1968–1970 to where they resided four decades later. In contrast to a widely held view that recipient families may have been tied to the land, I find they exhibited *higher* geographic mobility. Recipients were 20 percentage points more likely to have migrated, 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 have moved, relative to a mean of 70%. These children were also 22 percentage points more likely to have gone to big cities, compared to a base of

39%. In cities they presumably found new economic opportunities. To confirm migration is a prime mediating factor, I run the baseline estimations excluding urban migrants, and find that impacts disappear.

I also study whether land was used to invest in the education of children, who subsequently had 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 have finished primary school, compared to a base of 52%. Effects are attenuated if the whole sample of children is used in the analysis. Moreover, I also rule out alternative channels such as the civil conflict. [3] 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 their land within a few years of the reform. Presumably, 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

[3] Suggestive evidence indicates that migration may have also been mediated by the Colombian civil war but is unlikely to account for the main impacts of the reform. Recipients were marginally more prone to have been forcefully displaced from their plots in 1985-2000. This is consistent with accounts in the Colombian historiography documenting violence against small farmers (CNMH, 2016).

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 social science focuses 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). While this exercise is uninformative about general equilibrium shifts, it is not necessarily relevant in this context as 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 policymakers 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 impacts of the reform. In section 6, I perform a cost-benefit analysis of the policy. Finally, section 7 concludes.

2 Historical Background

2.1 The Policy Debate

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" (Currie, 1951, 1961; Hirschman, 1962, 1967). Colombia 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 (Fals-Borda and Luna, 1962; LeGrand, 1988). A World Bank mission in 1950 concluded that there were too many landless peasants struggling in the mountains

to achieve barely enough subsistence, while large landowners, many of them cattleranchers and in possesion of the best lands, were highly unproductive. It was estimated that around 50% of the private rural land was owned by the top 1% of landowners (Kalmanovitz and López, 2006; Berry and Cline, 1979). Furthermore, the smallest 10% of farms was twice as productive as the top decile (DANE, 1960). [4]

Policy makers were divided around potential solutions to transform the rural reality. Harvard professor and leading development expert, Albert Hirschman, suggested that providing land could facilitate a mass of productive farmers and improve their economic mobility. [5] Other defendants of this stance also argued that such a policy could appease civil unrest in the countryside at a time when revolutionary threats were looming (Karl, 2017; Fals-Borda, 1957). [6] In contrast, Lauchlin Currie, another former Harvard professor and advisor to US president Franklin Roosevelt during World War II, favored a radically different approach. He was afraid agrarian reform would produce "poor farmers working their small parcels with hand tools" (Currie, 1961, p. 37). Having led the World Bank mission, he determined that the best solution was to promote rural migration into the cities, freeing land to be cultivated by fewer and larger farms with more sophisticated techniques.

Both Hirschman and Currie saw the need for industrialization. They were inspired by Arthur Lewis's 1954 seminal theory of development with unlimited supplies of labor (Lewis, 1954). In the model, the traditional agricultural sector is typically characterized by an abundance of labor and a fixed amount of land. As a result, the agricultural sector has a quantity of peasants who do not contribute to agricultural output, since their marginal productivities are close to zero. On the other hand, the modern manufacturing sector is defined by higher wages relative to the subsistence sector, higher marginal productivity, and a demand for more workers. Thus, the central process of development consists of moving people from the traditional agrarian sector to the expanding modern manufacturing sector as the economy transforms. [7] A few years later, Harris and Todaro (1970) postulated that urban migration played a pitoval role in such a transition.

[4] Other related problems described in the Currie Mission included technical and financial assistance, as well as the provision of rural public goods such as health, education, and transport services.

[5] 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 complementary measures for solving the "land issue", including taxing unproductive land and updating the national cadastre.

[6] In many parts of the country, such as the departments of Tolima and Huila, redoubts of liberal guerrillas from the 1950s and newly created rebel groups (FARC, ELN, etc) threatened social order and increasingly attacked large landowners.

[7] Numerous critics, however, have pointed out that the rigid assumptions in the Lewis model fail to capture the difficulties of structural transformation, such as the costs of migration or educational investment (Kirkpatrick and Barrientos, 2004).

2.2 Overview of the Colombian Agrarian Reform

In 1961, after overcoming opposition from the landowning elite in Congress, Colombian president Alberto Lleras Camargo enacted an agrarian reform (Law 135) to raise the living standards in rural areas. 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 [8] (INCORA, 1974). However, experts cautioned that the reform would be difficult to roll out. The law created numerous, often complicated, and even contradictory legal procedures, in a country with precarious institutional capacity (Villamil-Chaux, 2015). 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).

The initiative had the support of numerous politicians and civic organizations; [9] it also received help from other Latin American countries and even the United States through the Alliance for Progress. [10] In 1962, the government created the Colombian Institute for Agrarian Reform (INCORA) to centralize operations and granted it with considerable autonomy and relatively sizable human and financial resources. [11] Progress on the much anticipated land redistribution was hindered by a low pace of expropiations and administrative problems inside the agency, as the reform met fierce political resistence from landowners, who considered it a threat to their power. [12] In 1966, newly elected liberal President Carlos Lleras Restrepo significantly expanded the *Sharecroppers and Tenants Program.* [13] Through Law 1 of 1968, he eased the legal requirements for acquiring land and regulated the conversion of

[8] In this paper, I concentrate on investigating the first program, although a second companion paper studies the titling of *baldios* at the agricultural frontier.

[9] See the ideological positions of different political movements, including those from opposition leaders, such as López Michelsen (MRL), Álvaro Gómez Hurtado (Conservative Party), and Diego Montana Cuellar (Communist Party), in Lleras-Restrepo (1961).

[10] In particular, the Kennedy administration, worried about the spread of communism in the region, provided financial and technical aid through USAID.

[11] INCORA delegated the execution of policies to its 8 territorial entities, each of which was responsible to the General Manager (INCORA, 1974). The agency also pioneered a variety of data systems to monitor the progress of its operations and determine resource allocation. It leased two mainframe computers (models IBM 360 and IBM 1620) from IBM, the first of its kind in the country (INCORA, 2002).

[12] Historical evidence suggests landowners appealed to legal maneveurs and used political connections in the justice system to delay or stop expropiations (Fajardo, 1986; Palacios, 2011). More extreme methods included targeted violence against former tenants and sharecroppers. See, for example, important essays in CNMH, 2014 about emblematic cases in the Caribbean Coast and Antioquia.

[13] President Lleras Restrepo also promoted the creation of the National Peasant Association (or ANUC) to organize small farmers (Zamosc, 1978). The organization assembled one million members and played a crucial role in pressing for change through social protest.

sharecroppers and tenants into owners of Agricultural Family Units (or AFU) – or parcels intended to generate between two to three times the average annual household income of a rural family [14] (INCORA, 1971).

According to official projections, the agency made an estimated 9 million hectares of land available for redistribution. Nevertheless, with more than 800,000 landless farmers in the countryside, policy makers considered it "impossible to think about allocating a parcel of land to every rural family" (INCORA, 1970, p. 78). The government set the target to acquire 1 million hectares over a 20-year period and benefit almost 400,000 rural families. Thus, INCORA saw no other alternative but to establish a criteria that allowed to "quantify and classify by priorities the families subject to agrarian reform" (INCORA, 1970, p. 78). The agency designed a selection mechanism that used a scoring system to rank rural families interested in receiving land based on their socioeconomic conditions and prioritized the most vulnerable of them (Directive 23 of 1966). I further describe this mechanism in Section 2.3 and use it in the empirical strategy in Section 4.

The actions undertaken during these years considerably increased the reach of the reform and targeted the heart of the country, most notably the Andean regions (Antioquia, Cundinamarca, Tolima), southwestern regions (Nariño, Valle del Cauca, Cauca) and the Caribbean Coast (Magdalena, Bolivar, Cesar). Between 1968 and 1970, the government initiated more than 12,000 expropiation processes, targeting 1/3 of the landholdings elegible for redistribution (see Figure 1). [15] Of these, half were found to be uncultivated or inefficently used. Nevertheless, only around 10% of these landholdings actually came into possesion of INCORA through the National Agrarian Fund. [16] Similarly, the agency only managed to title 389,630 hectares to 19,478 rural families, providing them with an average parcel of 20 hectares. [17] Consequently, the reform was considered a national failure after only accomplishing 5% of official targets at a considerable financial cost equilavent to 0.5% of GDP [18] (CNMH, 2016; INCORA, 1988); (Balcázar et al., 2001). While expectations were far from materializing, the results were not negligible, considering the tremendous administrative and legal difficulties to acquire land and transfer property rights to recipients.

At the end of 1970, an INCORA report vowed to not "capitulate to the pressure and

[14] This amounted to USD 800 (or $15,000 Colombian pesos) in 1970.

[15] These included properties of over 100 hectares registered in the cadastre that were deemed ineficiently used by INCORA officials.

[16] Around 90% of expropiation processes were knocked down by local judges with political connections ot the landowing elite. Of these landholdings, 68% were farms of less than 60 hectares.

[17] The majority of acquired lands were reported to be of regular quality and lacked access to markets. INCORA often needed to invest in infrastructure and agricultutural adaptation before redistributing it back.

[18] 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 investment requirements (Tamayo, 1970).

inflexible position of the landowners, who [...] are determined to maintain the status quo" (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 the titling program of *baldios* at the frontier, considered much more economical and politically viable (INCORA, 1974). In 1972, the Pastrana administration signed the *Pact of Chicoral* to stop expropiations and modify the purposes of Law 135. [19] A few months later, the enacment of Law 4 of 1973 effectively ended attempts of the National Front to change the country's land structure through land redistribution [20] (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". [21]

2.3 The Allocation of Land

During the Lleras Restrepo administration, the national 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 expropiation inquiry, agronomists and technicians were dispatched to evaluate whether a particular landholding was uncultivated or used unproductively according to the guidelines of Law 135. Their evaluation was based on key geographical and agronomical conditions. [22] The final report, called *informe de visita* (or visit report), was transmitted to the regional office of the agency. In conjunction with central authorities, the expropiation of a particular landholding was then recommended or rejected, and expropiated lands entered the National Agrarian Fund (NAF). [23] 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 landholding was cleared for redistribution, INCORA used a selection mechanism to allocate poor farmers into parcels (Directive 23 of 1966). Several steps were followed. First, the agency issued a statement informing the public about the decision. Then, authorities

[19] This pact was made in conjunction with representatives from the Liberal and Conservative parties and the landowing elite assembled in the municipality of Chicoral, Tolima.

[20] Law 4 of 1973 was not retroactive. As such, most expropiation processes that were initiated during the reform continued their course, but new processes were forbidden.

[21] Interview on December 4, 2017.

[22] For example: the level of agricultural production, soil quality, ruggedness, water, etc., the degree of market access, and the presence of sharecroppers or tenants.

[23] Law 35 established different modalities of land adquisition: expropiation, compensated expropiation, purchase, cession, and extinction of private domain. In the case of a purchase, negotiations with the owner were carried out to agree on a price and form of payment. Approximately 80% of the land that entered the program was purchased after cumbersome and often lengthy negotiations.

convened a local board of representatives elected among sharecroppers or tenants through voting procedures. The local board oversaw the selection process and helped officials. Next, they opened the registration of applicants interested in the land. Sharecroppers, tenants, and nearby landless farmers were eligible to apply. Once a list was compiled, officials and the local board surveyed applicants on their family characteristics, agricultural experience, assets, and income (*formulario de aplicación* or application form). The regional office of INCORA used a simple grading system to aggregate responses into a continuous score for each applicant and ranked them (see Appendix B). As shown in Table 1, the grading system was designed to reward larger, mature, and poorer families, as well as more experienced sharecroppers and tenants.

Furthermore, using technical studies that measured agricultural capacity, the agency decided how to split the land into similar Agricultural Family Units (AFU). They were intended to generate between two to three times the average rural household income and varied considerably in size, reflecting the large variation of geographic and climatic conditions across the country. [24] Knowing the avaibility of land, officials then decided on a score threshold to select recipients. The threshold was different for each expropiation process because the number of applicants and the size of AFUs also varied (see Appendix B). It was equivalent to the minimum score needed to fit into the last available parcel, conditional on the number of available AFUs. In order to prioritize the poorest families, implicitly only those above the score threshold were eligible to receive land. Meanwhile, those below were mandated to vacate the landholding. [25]

The final steps of the process involved registering parcels at a notary and formally transferring property rights to each recipient. According to archival records, the procedure was followed in the majority of cases. However, figures also reveal that not all of the potential recipients were actually allocated into parcels, suggesting possible corruption or administrative problems at later stages of the process. Upon receiving the UFA, most recipients also agreed to a specific set of conditions. For instance, they could not sell their land for at least 10 years without the approval of INCORA, a measure that was designed to incentivize their retention in rural areas. Moreover, 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 future. The government never tracked applicants over time, making it impossible to evalute the effects of the *Sharecroppers and Tenants Program* up to now.

[24] In my sample, they varied between 6 and 40 hectares.

[25] In the last years of the reform, the option to create a cooperative or community firm was also included (INCORA, 1971).

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 (or INCORA) in Bogotá, Colombia. The archives are managed by the National Land Agency (or ANT) and were salvaged in 2015 after the Colombian government centralized the organization of agrarian records. [26] They contain information about all INCORA operations between 1962 and 2002. Only 1/3 of the archives have been properly catalogued, including the majority of agrarian reform records from 1962 to 1993. [27] While difficult to quantify, historical and anecdotal evidence, including interviews with former INCORA and current ANT officials, suggests some agrarian reform records may have been lost, stolen, or burned during the past decades. As such, the information collected cannot be considered complete. To the best of my knowledge, however, there are no relevant complaints suggesting these episodes targeted certain files disproportionately more than others.

I gather information on 218 successful expropiation processes under the *Sharecroppers and Tenants Program* during 1968–1970. [28] Consistent with historical evidence, most of them were concentrated in the Caribbean Coast and the Andean departments of Antioquia, Cundinamarca, and Tolima, and total more than 30,000 hectares (see Map 1). The expropiation files contain legal documents and technical studies of each expropiation process, including the delimitation of Agricultural Family Units, and original surveys carried out by officials on applicant families. [29] The surveys contain information characterizing peasants' socio-economic conditions according to the questions designed by INCORA (see Appendix B). In particular, they register applicants' personal information, including their 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 officials to rank each family. Some of the expropiation files are written by hand, while others show the use of typewriters, but all are fairly consistent in the information reported. This data is crucial for reconstructing the scores, thresholds, and pre-treatment

[26] The INCORA archives were previously scattered accross 16 different territorial entitites and are protected by legal reserve. They were accessed through a confidentiality agreement with the ANT.

[27] With the help of USAID and the national archives (AGN), the ANT has catalogued agrarian records from 1962 to 1993.

[28] In total, I inspected almost 12,000 expropiation processes. However, as mentioned in section 2, 90% of them were discarded based on judicial sentences that struck down INCORA's operations, and only a handful of those were successfully redistributed to landless peasants.

[29] Information of applicants is included both in visit reports and application forms, and I use both to construct the dataset.

balance used in the empirical strategy.

After INCORA expropiated a landholding, officials allocated parcels and issued land titles to recipients. Nevertheless, this information is not found in the expropiation files, but rather in individual agrarian records. Thus, in order to identify which applicants were effectively allocated land and titled – that is, the treatment variable – I merge the previous data with micro-level land titles from the National Land Agency and notarial records from the Superintendence of Notaries and Registry (or *Superintendencia de Notariado y Registro*). Using all this information, I construct a novel dataset of 2,178 agrarian reform applicants, of which 36% were recipients. [30] These figures translate into around 10 applicants per process and an average AFU of 18 hectares allocated to recipients.

3.2 Linking Applicants and Children

Once I have the agrarian reform dataset, I use the names and national ID numbers of applicants to find their children at the National Identification Archive (ANI) of the National Registry of Civil Status (or *Registraduria Nacional del Estado Civil*). [31] This government agency is in charge of identification duties and vital statistics. It keeps track of all adult Colombians issued an official ID number, compiling relevant information such as birth and death certificates, voting registration, and biometric information. When a child was born, fathers were mandated by law to register the birth at a notary by filing a birth certificate. Historically, though, this norm was not usually followed by rural families residing at peripheral areas of the country, where notarial services were seldom offered and people instead registered new borns at churches. However, numerous expropiations ocurred in places near the center of the country, where presumably, a higher supply of notaries existed.

I use birth certificates, which list both father names and their ID numbers, to track the descendants of applicants. Out of the 2,178 applicants in the agrarian reform database, I am able to identify the children for approximately 23% of them. This represents 1,094 children out of 493 applicants, or 2.2 children per applicant. In the process, I search for both names and adult ID numbers (or *cédulas de ciudadanía*) of the children. Unlike numerous studies on intergenerational mobility, I am able to track both sons and daughters. The subsample does not suffer from differential attrition among recipients and non-recipients, reassuring that results derived from its use have external validity. Furthermore, the probability of finding a child in notarial records is uncorrelated with the treatment variable or other pre-treatment

[30] Through out the process, I only take recipients of AFUs into account, and not those who created cooperative or community firms.

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

characteristics from applicants (see Appendix B.1, Table A.4). Nonetheless, it represents applicants who, on average, resided closer to populated areas.

3.3 Contemporary Administrative Data

I combine diverse sources of outcome data to test the impacts of providing land. Using full names and ID numbers from applicant families and a simple phonetic algorithm, I merge the agrarian reform data with administrative information in the 2000s to measure living standards (see Appendix B.3 for a full explanation of the algorithm). [32] First, I use three components of Social Security records in 2010 from the Ministry of Health and Social Protection: Health Affiliations (or *RUAF-afilicaciones*), Vital Statistics (or *RUAF-nacimientos y defunciones*), and Social Security Contributions (or *PILA*) databases. [33] Together, these datasets encompass 90% of the population and register personal information regarding birth and death characteristics, the nature of employment and Social Security, formal wages, informality and occupation. While Social Security records contain most adults in the country, they do not systematically collect the same information for people in informal and formal sectors. Importantly, wages are not observed for those working in the informal sector. Altogether, in 2010, I am able to track roughly 46% of applicants (or 86% of living applicants) and 89% of the children in the subsample of applicants for which I found their children (see Table 2). I consider this my baseline dataset for the empirical analysis.

Second, I use Social Benefits records (or *SISBEN*) designed by the National Planning Department (or DNP) and implemented by municipal governments in 2006. [34] The SISBEN has a dataset with national coverage that tracks the poverty and vulnerability conditions of over 30 million people (around 66% of the population) in need of receiving social benefits from the central government. It contains personal and household questions regarding 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 reasonable, if potentially noisy. I track a bit more than a third of applicants and almost two thirds of their children. I find no statistical evidence of differential attrition based on matching rates between recipients and non-recipients, a fact that I confirm using death certificates. Furthermore, I find no differential attrition between the children of recipients and those of

[32] I unsuccesfully tried to find applicants and their 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 the statistical office (or *DANE*).

[33] Records collect information after 2008 from all Colombians affiliated to Social Security and on birth and death certificates across the country.

[34] The *SISBEN* tracks all Colombians who register to receive government poverty subsidies. The information collected is used by the government to prioritize and focus poverty subsidies. Although indicative of who seeks aid, it does not mean that all individuals in the dataset are poor or receive help.

non-recipients (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. [35]

Next, I use alternative administrative data about financial markets, entrepreneurship, and civil conflict. First, I use Financial and Business records from 2010 from the Superintendence of Finance (*SuperFinanciera*) and Chambers of Commerce (*RUES*). They store micro-level information regarding the nature of financial products (bank accounts, loans, etc.) and transactions made by individuals in the formal financial system and the creation of new firms. The datasets are considered high quality, but they exclude people who live in the informal economy. I also use the *Unique Registry of Victims* – administered by the Colombian Agency of Victims – and death certificates that contain personal level information of people victimized in the midst of civil conflict. Most importantly, this database registers the date and place, and in the majority of cases, a description of events. Finally, using names and IDs, I web scrape information on criminal records from all applicant families at the prosecutors' offices and national police.

3.4 Empirical strategy

A simple OLS estimation of the intergenerational impacts of providing land would most likely be biased, because recipients and non-recipients differed along a range of observable (and probably unobservable) characteristics. For example, recipients had, on average, more experience and smaller wages, and they were also younger. Thus, the most informative estimation approach is to use a local linear regression discontinuity design that exploits variation from discontinuities induced by the INCORA selection mechanism. The analysis compares applicants with predicted scores just above, or being eligible to become recipients of land, and below predicted thresholds who were very similar along other socioeconomic dimensions. Thresholds were defined as the minimum score able to fit into the last UFA for each expropiation process and rescaled around zero to make them and applicants comparable (see Appendix B).

Archival information shows the selection mechamism was usually carried out, but not always executed perfectly. In some instances, applicants unqualified for receiving a parcel were reported to have been allocated one and vice-versa. While this could reflect random errors, the most plausible explanation is the presence of administrative or corruption problems involving government officials at the end of the process. Moreover, information across expropiation files was not always systematic, and predicted scores and thresholds must surely

[35] Datasets that are not balanced (*PILA*, *SuperFinanciera*, and *RUES*) are precisely used as outcome variables.

suffer from measurement error. Nevertheless, even if compliance was imperfect and measurement errors significant, the discontinuity generated by the selection mechanism at each threshold still induced a change in the probability of accessing land through agrarian reform. Therefore, applicants just above the predicted threshold serve as a reasonable counterfactual to those below it. The empirical specification used for applicants and children is as follows:

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

where $y_{i,e}$ is a relevant outcome variable for applicant (or child of applicant) i in expropiation process e, and $d_{i,e}$ is an indicator variable equal to 1 if applicant i was elegible to become a recipient in expropiation process 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, α_e an expropiation process fixed-effect, and $\epsilon_{i,e}$ an error term that is normally and independently distributed. Robust standard errors are clustered at the applicant family level, since applicants are the treated unit, and children received treatment through their fathers. This RD specification estimates a local average treatment effect of the casual impact of accessing land on applicants (and children) who were inclined to become agrarian reform recipients because their predicted score was above the threshold in the selection mechanism. It is important to notice that this empirical exercise compares applicants and families within each expropiation process, and estimates are not influenced by applicants who would have been allocated land regardless, either because they were extremelly poor or capable of manipulating the system.

Now, following Calonico et al. (2014), the baseline specification for equation (1) uses a local linear RD specification estimated separately on each side of the cutoff. Furthermore, the baseline bandwidth is the optimal bandwidth that minimizes the mean squared error of the point estimator. Appendix A provides 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. I also use alternative methods developed by Imbens and Kalyanaraman (2012) to demonstrate consistency in the results. The local linear RD setup requires the existence of a first stage and two identifying assumptions: 1) agrarian reform applicants must not have selectively sorted around the cutoff based on their pre-treatment characteristics; and 2) all factors besides being selected as a recipient using the INCORA score system must change smoothly at the threshold.

3.4.1 First Stage

I first examine the existence of a first stage. Figure 2a graphically looks at the relationship between being above the predicted INCORA score threshold and the likelihood of being allo-

cated land. Each point in the figure represents the percentage of recipients within score bins. Dashed lines show 95% confidence intervals. A positive distance signifies that the applicant is above the threshold. The solid line plots predicted values from a local linear regression of being allocated land on a quadratic polynomial in the score, estimated separately on either side of the predicted threshold. Applicants with a score just above the predicted threshold are approximately 50% more likely to have been allocated land during the agrarian reform, and the F statistic hovers around 9–10, providing evidence of a strong first stage. Similarly, Figure 2b shows these results are extensive to the sample of children, although somewhat less powerful. While compliance with the reform was far from perfect, the key issue for identification is the sharp discontinuous change in the probability of receiving of land near the threshold.

3.4.2 Identifying Assumptions

Next, it would be problematic if applicants were consistently able to manipulate information or colluded with local selection boards to change their scores to place them just above the required threshold and these actions were correlated with their characteristics. Some examples of these problems include if better-off applicants bribed or lied to officials, or, on the contrary, if authorities manipulated the system to benefit certain families. Ex-ante, it is unclear the actual sign and magnitude of the bias. In any case, this would require previous knowledge of INCORA's calculations about the size of AFUs and thresholds, which seems unlikely based on the available historiographical evidence. In order to check for the presence of 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). Figure 2c 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. In Figure 2d, this finding is extensive to the subsample of children and consistent with the version of then director general of INCORA, Carlos Villamil Chaux, who emphasized in an interview the professionalism of their work. [36]

Despite the lack of statistical evidence of selective sorting, it could still be the case that applicants with scores just above the threshold differed systematically in their characteristics (such as experience, income) from those just below. To test this, I examine whether key characteristics in 1968–1970 used to predict scores are balanced across the threshold using micro-level agrarian reform and vital statistics data, including information regarding age, education, working experience, crop cultivation, area, housing, and income. In particular,

[36] Interviewed on December 4, 2017.

I estimate equation (1) for these socioeconomic characteristics and present the estimated coefficient of interest,$d_{i,l}$, for each of these variables in Table 3. There is no substantial jump at the threshold. In the baseline estimates, there are no statistical differences within the optimal RD bandwith between recipients and non-recipients across all variables, providing evidence that the assumption of relevant factors varying smoothly at the threshold is reasonable.

4 Impacts on the Rural Poor

4.1 Living Standards

I now investigate how providing land impacted the lives of recipients and their children. I first look at the long-run effects of having been an agrarian reform recipient on living standard outcomes. Using Social Security records (*RUAF*) in 2010, I code life expectancy as the probability of being alive and other dummy variables measuring the likelihood that a person registers for goverment poverty subsidies and earns formal wages above the minimum wage. While I don't have information on household income or consumption, I use information from Social Benefits records (*SISBEN*) in 2006 on housing quality, access to public services (electricity, running water, sewage), and quantity of assets to construct 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 compute a summary measure created using principal component analysis (PCA) that combines information from all available welfare questions. PCA is described in detail in Appendix B. As people registered in the SISBEN are generally among the most vulnerable, estimates using this dataset are probably underestimated.

Table 4 reports results for applicants in Panel A and for their children in Panel B. I show RD linear estimations, specify the bandwith used, and include in all regressions a set of the following controls to improve precision: age, ethnicity, sex, and expropiation process fixed effects. Since treatment is at the individual level, I use robust standard errors clustered at applicant family level for applicants and their children. In Panel A, columns (1) to (3) illustrate that being a recipient incremented the wealth and housing indexes by 0.2 and 0.3 standard deviations, respectively. Estimates are significant at 5% confidence level. Meanwhile, the assets index and access to different to public services barely change and are statistically insignificant, suggesting the effects are mostly driven by the quality of housing. Column (4) documents that recipients of the reform are, on average, equally likely to be alive in 2010 than non-recipients. The coeficient is statistically insignificant,

indicating similar attrition rates among applicants. Finally, columns (5) – (6) show that recipients were 9 percentage points less likely to register for government poverty subsidies and 4 percentage points less likely to have formal wages above the minimum wage. These results are qualitatively large compared to the sample means.

In Panel B, I explore the effects on the children of recipients. The sample size is considerably smaller because, as explained in section 3, I was unable to find the descendents for all applicants. I consider sons and daughters born after the reform and divide them into groups of adults (more than 18 years old) and young children (less than 18 years old, but older than 5 years), as applicants had children at different points in time between 1970 and 2000. Columns (1) to (3) document that in 2006, the children of recipients scored 0.32, 0.37, and 0.23 standard deviations higher on average on the wealth, housing, and assets indexes relative to those of non-beneficiares. They also had marginally better access to public services, including being 18 percentage points more likely to have running water, relative to a sample mean of 37%. Furthermore, in column (4), estimates show that the children of recipients were also equally likely to be alive in 2010, suggesting no differential attrition. Finally, they were 22 percentage points less likely to demand government poverty subsidies, relative to a sample mean of 58%, and 22 percentage points, more likely to have been earning above minimum wages, compared to a base of 17%. [37] Estimates are statistically significant at the 1% and 5% confidence level. The magnitudes of coefficients are large relative to sample means and those of applicants, signaling substantial intergenerational impacts.

Across both applicants and children, RD estimates surpass a variety of robustness checks. Tables A.1 and A.2 document that the estimated impacts on living standards are robust to the choice of bandwidth, kernel function used to construct the local-polynomial estimator, and RD polynomial. I use bandwidths that are half and twice the size of the baseline optimal bandwidth according to Imbens & Kalyanaraman (2011) (the Calonico et al. (2014) bandwidth is nearly identical), different kernel functions (for example: triangular and epanechnikov), and quadratic and cubic polynomials, although certain specifications lack the proper sample power, and estimates can become incresingly noisy. Moreover, I run two placebo checks estimating regressions at two fictitious INCORA thresholds (plus and minus 10 points in distance from the actual predicted score threshold). Results in Appendix A also show that the effects described in the previous paragraphs emerge only at the particular predicted INCORA cutoff. In all regressions across the placebo checks, the coefficients remain statistically insignificant but sometimes change signs. [38]

[37] While rural wages in the informal sector are not observed, they often tend to be below the legal minimum wage.

[38] Estimates in Table 2 are much larger than the OLS estimates (see Appendix A, Table A.4). This could be the case of downward attenuation bias, or the OLS could be a biased estimate of an average treatment

4.2 Modern Economy

Next, I show that recipients, and in particular their children, not only exhibited better living standards, but also participated more in the modern economy. Measures of this include having better labor market conditions, working in high-skilled sectors, and having access to other private services in the formal economy, such as financial markets. Table 5 documents these effects using various sources. For those matched in Social Security records in 2010, I code various dummy variables measuring the likelihood of being unemployed (conditional on being able to work) and working in the formal sector, as well as the nature of employment and contributing to the Social Security system. I also use worker and sectoral (CIIU 2) employment codes to understand differences in labor mobility and the propensity to move out of agriculture. I complement this exercise with information from financial records (*Superfinanciera*) in 2010 and business records from the Chambers of Commerce. I code additional variables measuring the likelihood of having access to different financial services (bank accounts, credit cards, and loans) and starting a formal business or becoming entrepreneurs. As before, Panel A shows results for applicants, while Panel B does so for their children.

Across developing countries, labor formality is a salient sign of transition to a modern economy. Informal employees tend to be paid lower wages, work in less productive activities, and are disproportionately located in rural areas. They also receive fewer benefits and legal protections. In Panel A, column (1) shows that recipients of the reform were not necessarily more likely to be working in 2010 than non-recipients. Columns (2) and (3) illustrate that they were marginally more likely to have done so in the formal sector and have contributed to Social Security, which includes both the subsidized poor population by the government and those who contribute to private insurers. The RD estimates are not statistically significant but are close to the 10% confidence level. While it may seem small, the effect is quite large relative to the sample mean of 3%, and relevant, as labor informality among the rural poor is generally widespread. This also indicates that recipients were marginally more likely to contribute to health care and retirement plans, as well.

Moreover, columns (7) to (10) suggest that living recipients were 15 percentage points less likely to have been employed in agriculture in 2010, relative to a sample mean of 64&, and 15 percentage points more likely to have been employed in services. Both results are significant at the 10% confidence level. Thus, in contrast to the view that land incentivizes the retention of people in traditional activities, where productivity is low, these estimates signal that recipients were, in fact, more keen to move out of agriculture on average. However, columns

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

(4) to (6) illustrate no differential or largely marginal impacts of having been a recipient on access to formal financial markets, including having a bank account, credit card, or loans. A most plausible explanation is the overwhelming informality of the majority of applicants, which would have restricted their entry into the financial system. Across all specifiations, the results are conditioned by low variation in the data but provide suggestive evidence that recipients were marginally more succesful than non-recipients to enter the modern economy, albeit still facing important constraints.

On the other hand, in Panel B, I look at the impacts on adult descendants of recipients. The children of recipients exhibited, on average, more signs of participation in the modern economy. This may be explained by the considerable expansion of Social Security and capital markets during the 1990s and 2000s, which, according to official figures, increased coverage to nearly 90% of the population. Major market reforms also helped to speed up structural transformation in the country, reducing the share of people employed in agriculture. However, consistent with results in the previous section, in columns (3) and (4) I find that in 2010, the children of recipients were, on average, 25 percentage points more likely to be working in the formal sector, relative to a mean of 39%, and 18 percentage points more likely to be contributing to Social Security, compared to a base of 18%. The RD estimates are statiscally significant at 5%, and again relatively large, when compared to sample means. Consequently, the children of recipients were also more likely to contribute to health care and retirement plans.

Furthermore, in 2010, being a child of a recipient increased the chances of using a bank account in 16 percentage points, having a credit card in 4 percentage points, and a consumer or micro loan approved in around 14 percentage points. All RD estimates are significant at the 5% or 10% confidence level. Likewise, there were no major impacts on additional financial products such as insurance or mortgages, indicating that the intergenerational impacts of the reform were limited to less-complex financial services. Partially overcoming credit constraints is a notable accomplishment. Crucially, too, the children of recipients were more likely to move out of agriculture and find jobs in high skilled sectors such as manufacturing. A child of a recipient working in the formal sector was equally likely to be employed in agriculture (column 4) but 13 percentage points and 10 percentage points more likely to have been employed in manufacturing and become formal entrepreneurs (columns 5 and 6). These estimates are significant at the 5% confidence level. As complementary information, although not reported in the paper, there was no appreciable variation in the propensity to pay taxes, file patents, or export.

The empirical evidence presented in this section suggests substantial positive intergenerational impacts of land. They do not merely reveal persistence, but also amplifying effects

across generations as the magnitudes of coefficients are notoriously larger for children relative to their parents. Given that pre-treatment characteristics are statistically balanced for applicants close to the RD threshold, the findings are indicative of considerable upward economic mobility of the children of recipients relative to those from non-recipients. Measures of this include better living standards, entry into the formal economy, employment in high-skilled sectors, access to formal financial markets, and entrepreneurship – an entire bundle of things that form the nexus of modern economic life.

At first sight, the results are consistent with older studies in economics using aggregate data on agrarian reform in India (Besley et al., 2016; Banerjee et al., 2002) but are particularly at odds with recent social science research in Latin America (Dell, 2012; Montero, 2018), which document adverse long-run effects of such policies in Mexico and El Salvador. Nervetheless, a key distinction with previous studies is the use of micro-level data, which allows me to examine direct impacts on recipient families as opposed to the whole economy. They are also hard to reconcile with recent studies that find nonexistent or marginal intergenerational impacts of wealth shocks, both in historical and contemporary settings (Bleakley and Ferrie, 2016). In contrast, the findings suggest that productive assets can help alleviate poverty in the long-run. Finally, they complement a growing body of evidence on intergenerational mobility (Black and Devereux, 2010; Chetty et al., 2014; Clark, 2014; Corak, 2013; Solon, 1999).

4.3 Intergenerational Mobility

So far, I have shown that broading access to land through agrarian reform had significant positive impacts on recipients, and especially on children. However, these are local treatment effects and do not necessarily imply that the reform improved intergenerational mobility among all agrarian reform applicants. For instance, it could be 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 are still worse off in absolute terms. Or, it could also be the case that the children of recipients distanced themselves even further from those of non-recipients relative to their point of departure. Therefore, in this section, I analyze intergenerational mobility among all applicants to the reform.

I investigate two classes of mobility measures that capture different normative concepts: *relative* and *absolute* mobility (Chetty et al., 2014). The first, which has been the subject of most prior research on intergenerational mobility (Black and Devereux, 2010; Solon, 1999), focuses on the relative outcomes of children from different parental backgrounds. However, it may have ambiguous normative implications: for example, reflecting worse outcomes for

better-off applicant families rather than better outcomes for the most vulnerable. Meanwhile, absolute upward mobility is valuable because it can measure the mean outcomes of children who grew up in poorer families. I estimate changes in mobility over time by examining the joint distribution of fathers' 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 between recipients and non-recipients. 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 these statistics with more traditional ways of measuring mobility: (i) the correlation coefficient between children's outcomes and fathers' outcomes; or (ii) the parent-child outcome elasticity ($\frac{dE[logY|X=x]}{glogx}$).

Furthermore, I use educational attainment, measured in years of schooling, and wealth indexes from SISBEN in 2006 as outcomes of interest. Both variables present advantages but also challenges. Education may be measured more precisely than wealth among the very poor, and it is less likely to be influenced by life-cycle bias, but social status is observed only in coarse bins. In the sample, a third of the fathers of children in 1970s and 1980s birth cohorts had zero years of education. Any latent differences in opportunity within the bottom third of the distribution are thus not observed. [39] Therefore, I calculate bounds on a range of social mobility statistics that take into account interval censoring in the parent education rank distribution. On the other hand, as wealth in measured at different ages for applicants and children, estimates can suffer from life-cycle bias.

4.3.1 Relative mobility

I begin by calculating measures of relative mobility for recipients and non-recipients of agrarian reform. I rank the children of all applicants 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 fathers of these children based on their years of schooling and wealth relative to other fathers with children in these birth cohorts. Define the children rank as c and the father rank as p. I characterize mobility based on the slope of the rank-rank relationships $Y^i(c) = E^i(p|c)$ for recipients and non-recipients $i \in [b, n]$, which identify the correlations between children's and father's positions in the wealth and education distributions. [40] The slopes measure differences in outcomes between children from top vs. bottom families among

[39] 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.

[40] 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 $c_{K+1} = 100$.

all applicants. The intercepts measure the expected rank for children from families at the bottom of the outcome distributions. Note that in this scenario, the comparison between fathers and children does not occur within groups, but across all applicants.

Figure 3 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. Parent education is observed in nine bins, representing the highest year of schooling attained by each father, while parent wealth is seen in ten bins reflecting mean decile averages. For example, in Figures 5b and 5d, the bottom bin comprises around a third of fathers, all of whom report zero years of schooling. The points in the graphs show the mean child rank conditional on having a parent 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 5a and 5c). In the case of education, the gradient is convex but approximates a linear relationship, as well (see Figures 5b and 5d). [41]

In Figures 5a and 5b, the gradient of the wealth and education CEFs that pool all applicants appears substantially high and 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. Across all applicants, 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 not surprising given pervasive rural poverty in the country and are similar to other mobility studies in developing countries and Colombia (Black and Devereux, 2010; Montenegro and Meléndez, 2014).

However, in Figures 3b and 3d, and once I differentiate between recipients and non-recipients, subtle but revealing patterns emerge. 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.

[41] 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 plausible assumption.

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 data.

4.3.2 Absolute mobility

The CEFs used in the previous section also allow me to calculate measures of absolute upward mobility. Increases in relative mobility could be undesirable if they are caused by worse outcomes for better-off applicants. In contrast, increases in absolute mobility at a given wealth or educational level, holding fixed absolute mobility at other wealth or educational levels, unambiguously increase welfare. Similar to (Chetty et al., 2014), I define absolute upward mobility as μ_{25} 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, precisely those who had zero years of schooling in the education distribution and were presumably illiterate. This framework allows me to consider, for example, the possibility that a child born at the 10th percentile of the education distribution has a different expected outcome from a child born at the 30th percentile.

When using data on all applicants, I find that this statistic is mechanically related to the rank-rank slope and does 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 distributions are effectively absolute outcomes. Upward wealth mobility for non-recipients in μ_{25} is 29, while for recipients, it is 34. Moreover, other measures of upward mobility exhibit similar between group variation. 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, 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%. [42] The reader can construct additional measures of mobility beyond those considered here.

Fortunately, I find that the patterns of between group variation in absolute and relative intergenerational mobility are very similar using alternative measures. Overall, these results provide further evidence that productive assets can be a tool for alleviating poverty. However, caution should be exerted when drawing conclusions. Statistics were calculated among

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

applicants and not across the whole sample of people registered in SISBEN, and estimates are mostly likely underestimated, as they do not consider better-off applicants, who presumably did not register for government poverty subsidies. Similarly, the intergenerational mobility impacts of the reform are still far from average wealth and education levels in the country observed for cohorts born in the 1970s and 1980s. In 2006, government statistics revealed that average wealth and education levels were almost twice than those from the children of recipients. [43]

5 Mechanisms

The past findings raise the intriguing question of why receiving land through agrarian reform would have had such intergenerational impacts 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. The country also faced major societal upheavals in the following decades, including the Colombian Civil War, urbanization, and the implementation of market and social reforms. Understanding the channels of persistence is crucial, because they can lead to very different policy conclusions about the convenience of land redistribution. In this section, I draw from historical evidence in the Colombian historiography (Fajardo, 1979; Fals-Borda and Luna, 1962; Kalmanovitz and López, 2006; Karl, 2017; Palacios, 2011) to explore theoretical mechanisms discussed in section 2.1 – many of which were used by defendants and critics of the reform in the 1960s – that could help elucidate why recipients, and particularly their children, fared much better in life.

5.1 Geographic Mobility

Development economists agree that an integral part of the development process consists of moving people from a traditional-informal sector to a modern-formal sector as the economy transforms, and they also emphasize the importance of rural-urban migration (Harris and Todaro, 1970; Lewis, 1954). According to Colombian historiography, the second half of the 20th century was a period of rapid urbanization (Kalmanovitz and López, 2006; Karl, 2017; Palacios, 2011). Therefore, I examine geographic mobility as a prime candidate linking applicants of agrarian reform to their development paths. Specifically, I use Social Security records to calculate different measures of migration by comparing the municipality where rural families applied for land in 1968–1970 with the municipality where applicants and their

[43] Considering the whole sample of *SISBEN*, average years of schooling for the same cohorts was 9 years, while the wealth index was 30 vs. 14.

children reported to be residing in 2010. [44] I code dummy variables reflecting the likelihood of migrating and disentangle specific migration to large cities, small cities, and other rural places.

Table 8 documents the impacts of receiving land on geographic mobility forty years after the reform took place. Panel A reports outcomes for applicants, while Panel B shows outcomes for their children. In Panel A, and in contrast to conventional expectations that land incentivizes the retention of rural families in the countrysie, column (1) illustrates that recipients were 20 percentage points more likely to have migrated relative to non-recipients and a mean of 50%. The RD estimates are statistically significant at the 5% confidence level. This result is striking, given that land market restrictions from INCORA forbade recipients from selling (and even renting) their parcels during the first 10 years of tenure. Columns (2) and (3) illustrate that the majority of this effect is driven by rural migration to urban centers, suggesting that recipients did not just migrate to other rural places, but remained in the countryside. Being a recipient increased the likelihood of moving to a large city by 11 percentage points, relative to a mean of 19%, while it decreased migration to other rural areas by almost the same margin. Again, the RD estimates are significant at the 10% confidence and substantial relative to sample means.

Next, results on the children of applicants in Panel B reinforce this picture. On average, the children have higher migration rates than their fathers (50% vs. 72%), a fact consistent with historical national trends. Yet, the children of recipients tended to migrate 27 percentage points more, as reported in columns (1) through (3), uncovering evidence that they were not necessarily tied to the land and, in fact, enjoyed higher geographic mobility. As before, effects appear driven mostly by migration to large cities. Columns (2) to (3) display that the children of recipients were 23 percentage points more likely to move to a large city, relative to a mean of 39%, and less likely to move to other rural places. The first effects are statistically significant at the 5% confidence level, while the former ones are not. Moreover, splitting the sample among those who migrated and those who did not further reveals the effects are mainly mediated by urban migration. In Appendix A, Table A.5, applicants and children who did not migrate to cities show no appreciable differences in living standard or modern economy outcomes.

These findings may be consistent with a story where land relieved credit constraints. Thus, I consulted the Superintendence of Notaries (*SNR*) to find whether and when beneficiaries sold their land. In Colombia, market transactions need to be registered in notaries to possess legal validity, although vast informal norms regulate land markets in the countryside. I find that up to 30% of recipients formally sold the land to other parties by 1980, or ten

[44] Results are very similar if I use the SISBEN data instead.

years after the reform. Some transactions were done while prohibitions on sales were theoretically in place, signaling possible corruption or administrative ineptitude. Presumably, more recipients could have done the same in informal markets, although at lower prices. This coincides with numerous historical accounts that document the incapacity of INCORA to track recipients and properties over time and the selling of parcels in departments such as Cundinamarca and Antioquia. Recipients complained of being unable to fully exploit the land (Zamosc, 1978).

The results on migration shed light on prominent development debates over the years. They suggest that rural families used the land to relieve credit constraints on urban migration costs. Therefore, the impacts do not appear mediated by the consolidation of a mass of productive farmers, despite transaction restrictions incorporated in the reform designed to do so (Banerjee et al., 2000). Rather, they are indicative of an asset shock that enabled rural families to move to urban centers, where they accessed higher quality public goods, moved out of agriculture, and entered the formal economy. I do not claim that migration was the only mechanism linking agrarian reform applicants to their development paths, but the historical and empirical evidence make it difficult to tell a story where migration does not play a central role.

5.2 Investment in Education

Another potential explanation is that applicants 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 (Becker and Tomes, 1979; Galor and Zeira, 1993). Also, education is a definitive measure of progress in developing countries, where wealth information is scarce and measurement error problems significant. [45] I use information from Social Benefits records (*SISBEN*) in 2006 to measure years of schooling and code dummy variables capturing the likelihood of finishing primary school, high school, vocational education, and college for adult children. This information should reflect investments made decades earlier, even if the timing of measurement is long after the reform. Moreover, for young children, I also code variables measuring the probability of attending school and incurring in child labor.

In Table 9, I look at the education impacts on the children of applicants born after the reform. Column (1) indicates that adult descendants of recipients had, on average, 1.5 more

[45] 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 social mobility often proxy lifetime opportunity with education, an approach that has been validated in countries where both are possible (Solon, 1999).

years of schooling than their counterparts, a large result when compared to the sample mean (5.3 years). Similarly, column (2) shows that they were also, on average, 17 percentage points more likely to have completed primary school, compared to a mean of 52%. Both coefficients are significant at the 5% confidence level. Columns (3) to (5) also suggest the children of recipients were more likely to graduate from high school, vocational education, or college, but the coefficients are statistically insignificant. Finally, columns (6) and (7) consider young children, or those who were younger than 18 years in 2006. Young children with a parent who benefited from the reform were, on average, 4 percentage points more likely to be attending school in 2006 and less likely to incur child labor activities. The first coefficient is significant at the 5% confidence level.

Recall from Table 3 that applicants within the optimal bandwidth were balanced in their education levels, so any differences in educational attainment of children can be attributed to the policy. Also, results should be interpreted as a lower bound, because better-off households do not normally register for government poverty subsidies in SISBEN. Performing the previous exercise and including children born before the reform attenuates the results, suggesting that the impacts may be disproportionately concentrated on younger children (see Appendix A, Table A.6). Overall, these findings highlight that investment in the education of children was also an important channel linking applicants to their development paths. Consistent with previous evidence, investment in education may have been facilitated by migration to urban centers, where economic agglomeration also complemented acquired skills.

5.3 Conflict

In the second part of the 20th century, many developing countries that pushed for agrarian reforms also suffered severe civil unrest and even war. [46] As explained in section 2, in Colombia, numerous historical accounts suggest civil conflict could also be an intermediating factor, as the reform was in part implemented to apease social unrest and revolutionary threats (Fajardo, 1979; Fals-Borda and Luna, 1962). I explore patterns of peasant displacement and enlistment in rebel movements and criminal activities using administrative data. I exploit information from Colombian civil war victims in the *Unique Registry of Victims* (or URV) 1985–2010 and death certificates from Vital Statistics (*RUAF*), as well historical criminal records from judicial authorities.

Though caution is warranted, since data suffers from severe measurement error, I find marginal differential impacts of the civil conflict on recipients. On average, few applicants

[46] This includes, for example, most Latin American nations (El Salvador, Guatemala, Nicaragua, Bolivia, or Perú), the Philippines, Vietnam, Zimbabwe, and South Africa.

seem to have suffered violent actions. In Appendix A, Table A.7, among applicants who died before 2006, the year in which I observe many development outcomes, column (1) illustrates recipients were 12 percentage points more likely to have suffered a violent death, relative to a mean of 8%. Similarly, among living family members in 2010, recipients were only 5 percentage points more likely to report having been displaced as part of the civil conflict. These results disappear when looking at the sample of children. The effects are mostly driven by applicants who lived in places where the civil conflict is known to have been intense. Moreover, column (3) shows that non-benefiaries were equally likely to engage in criminal actions and social disorder. In summary, these results highlight that while civil conflict was a formative event in the history of the country, it is unlikely to have driven the main findings.

6 Cost-Benefit Analysis

Findings on the intergenerational impacts of land ought to be weighted against the fiscal costs of the reform to further evaluate its convenience. In this section, I perform a simple cost-benefit analysis of the *Sharecroppers and Tenants Program* and discuss possible implications for development policy. I begin by calculating 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 treatment effects estimated in section 4 can be extrapolated to all recipient families. Recall that the children of recipients accumulated, on average, 1.5 more years of education than 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. I also employ sensitivity analysis to show how results evolve conditional on various parameters. [47] Under the baseline assumptions, a child of a recipient

[47] In Table A.5, following empirical studies in Colombia, I use higher and lower ranges for educational returns, wage levels, real wage growth paths, and discount rates.

born just after the reform increased total lifetime earnings to USD 4,515 today. The present value of this increase in lifetime earnings was USD 694 (or $12,846 Colombian pesos) in 1970. [48] For a family with two young children born after the reform, being a recipient therefore had an estimated present value of approximately USD 1,388 in terms of increased children's earnings.

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, the program only benefited approximately 20,000 rural families at an average cost of USD 2,711 (or $50,000 Colombian pesos) per recipient (Tamayo, 1970). Today, the figure roughly amounts to USD 17,638. Three quarters of this value corresponded to costs incurred during land expropiations by INCORA, a majority of which went to compensating landowners for the acquired lands. The rest included costs related to legal advice and agricutural investments made to landholdings that entered the National Agrarian Fund. Combining calculations on benefits and costs, the data suggests the *Sharecroppers and Tenants Program* most likely yielded net losses. In the baseline scenario, the fiscal investments made per recipient family had a private rate of return of -80%. More favorable scenarios still yield rates of -40% (see Appendix A, Table A.8).

The estimates presented neglect important factors that should be considered in a more comprehensive cost-benefit evaluation. First, they do not account for gains from better outcomes in future generations (ie: the grandsons of recipients) and ignore other potential benefits, such as improved living standards of recipients. Importantly, they do not consider any externalities of the reform, either, which most likely drove private and social rates of return to differ. Improving social mobility in other settings has been shown to generate positive externalities (less crime, more social capital, etc). Yet, in Colombia, agrarian reform has also been associated with disorder and civil conflict.

7 Conclusions

Providing land through agrarian reform has been a common strategy for improving economic mobility in developing countries. Yet, it is often a very costly and controversial process. This study identifies the causal intergenerational impacts of this policy. I track applicants to the 1968 Colombian agrarian reform and their children in contemporary administrative data. Exploiting discontinuities in the allocation of parcels, I find that the children of recipients

[48] 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.

exhibited considerable upward economic mobility. They experienced better living standards relative to those from non-recipients and their parents. They were also more likely to work in high-skilled sectors, become entrepreneurs, contribute to Social Security, and use formal financial markets – an entire bundle of measures that form the nexus of modern economic life. This illustrates that providing a father with a productive asset can help alleviate poverty and change the intergenerational path of his children.

In contrast to a widely held view that land traps rural families in the countryside, these findings appear mediated by a relief of credit constraints that allowed recipients to migrate to urban centers and invest in the education of their children, who subsequently used these skills to find new opportunities in the modern economy. This is consistent with Colombian historical evidence and notarial records, which suggest that up to a third of recipients sold their land a few years after the reform. Furthermore, I evaluate the cost-effectiveness of the policy, an important element when analyzing its convenience. I compare previous intergenerational benefits against the fiscal costs of the reform. Estimates from a simple cost-benefit analysis yield that it was most likely not cost-effective. The fiscal investment per recipient family had a return of -80%. However, the analysis neglects important social externalities.

Overall, 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 impacts 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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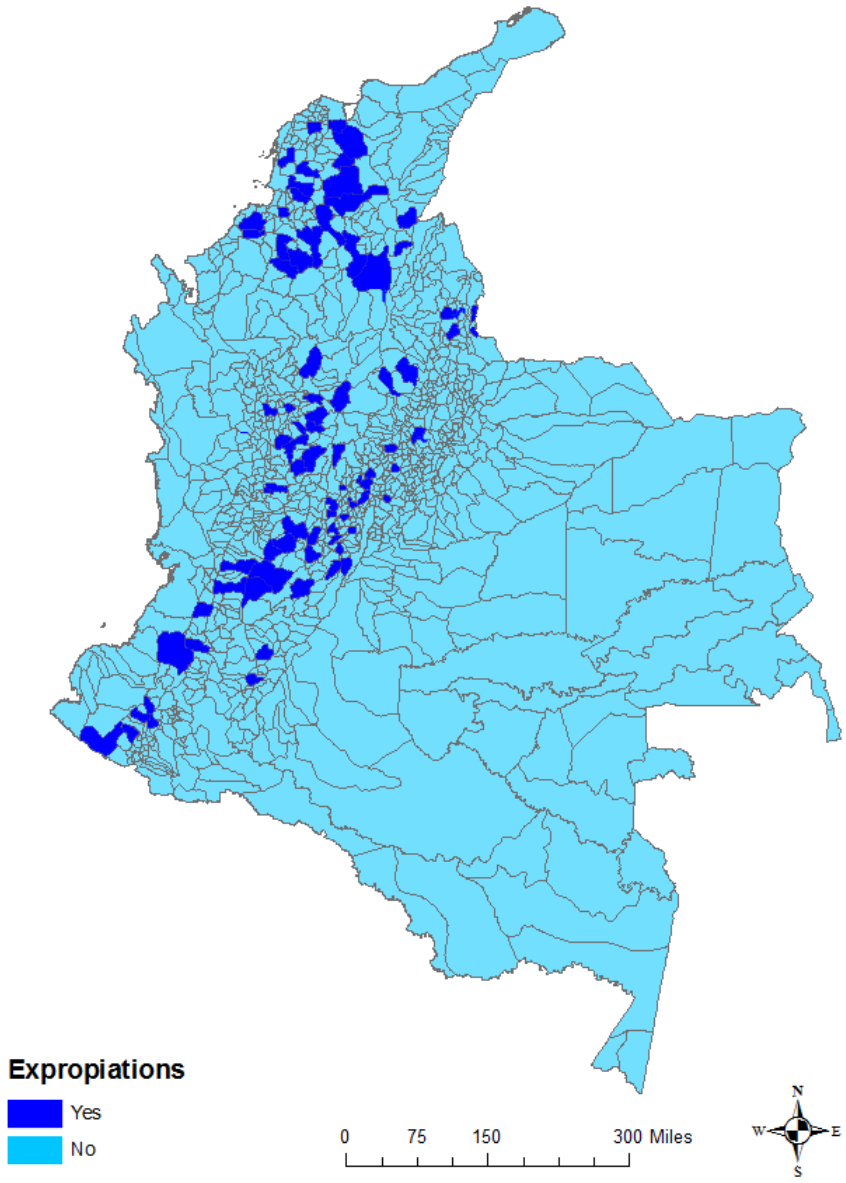
Figure 1: Agrarian Reform Expropiations 1968-1972



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 concentrated in the Andean and Caribbean regions. Source: INCORA.

Table 1: INCORA Score System

	Points
Family Age (in years):	
14-17	10
18-24	15
25-44	20
45-54	15
55-60	10
<14 or >60	3
Years of Agricultural Experience:	
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. After an expropiation, applicants were surveyed and ranked. Points assigned in each category were aggregated into a continous score. Poorer, larger and mature families with more experienced household heads were prioritized. Those with scores above the expropiation process threshold were eligible to receive a parcel (or Agricultural Family Unit) intended to generate between two to three times the average annual income of a rural family. Source: INCORA.

Table 2: Applicants and Children in Contemporary Administrative Data

Database	Name	Year Observed	Recipients	Non-Recipients	Difference	Standard Error
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Applicants						
Social Security	*RUAF*	2010	46.3	43.9	0.0237	(0.0222)
Formal Labor Market	*PILA*	2010	2.3	1.1	0.0122**	(0.00544)
Social Benefits	*SISBEN*	2006	33.6	33.5	-0.0316	(0.0319)
Financial System	*SuperFinanciera*	2010	11.9	10.9	0.0102*	(0.00595)
Business Records	*RUES*	2010	0.7	0.5	0.00223	(0.00195)
Panel B: Children						
Social Security	*RUAF*	2010	88.9	89.5	-0.00585	(0.0314)
Formal Labor Market	*PILA*	2010	23.6	15.5	0.0815**	(0.0388)
Social Benefits	*SISBEN*	2006	61.7	64.9	-0.0238	(0.0530)
Financial System	*SuperFinanciera*	2010	47.4	41.9	0.0556**	(0.0264)
Business Records	*RUES*	2010	12.2	8.5	0.0379*	(0.0217)

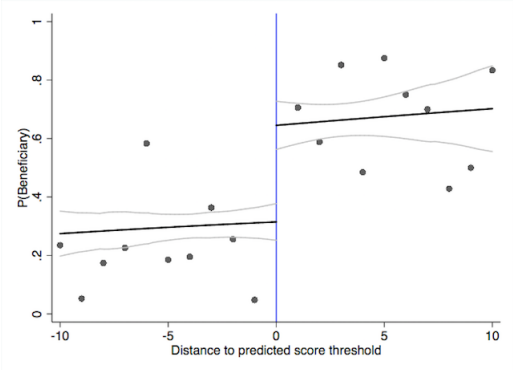
Notes: This table shows the linkage between agrarian reform data in 1968-1970 with contemporary administrative databases for applicants (Panel A) and their children (Panel B). Columns (1) and (2) indicate the official name of an administrative database, shown in rows, and the year in which it is observed. Coeficients in columns (3) and (4) show the matching rates in percentage terms, differentiating between recipients and non-recipients. The difference of these two columns is shown in column (5) and the standard error in column (6). The linked data of 45% of (or 87% of living) applicants and 89% of children in Social Security records are the baseline samples for regressions in Tables 4 (Columns 1-3), 5 and 8. The linked data of 33% of applicants and 63% of children in Social Benefits records are the baseline samples for regressions in Tables 4 (Columns 4-6), 6, 7 and 9.

Table 3: Pre-Treatment Balance in 1968-1970

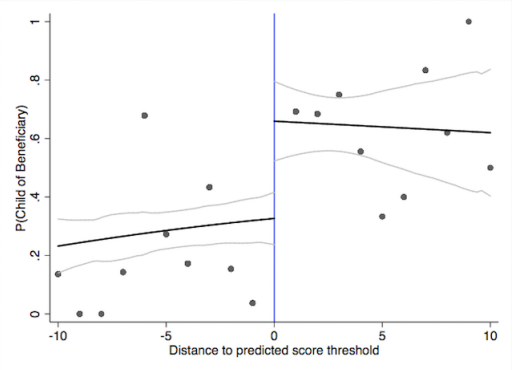
	Age	Years of Schooling	Years of Agricultural Experience	Log(Wages)	Has House	Plot Area (in Hec)	Grows Cash Crops	Grows Staple Crops
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Applicants				
Recipient	-2.137	-0.125	1.134	-0.107	-0.0379	-0.222	0.0934	-0.0342
	(1.627)	(0.975)	(0.842)	(0.137)	(0.0265)	(0.296)	(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 Appendix B Table A.9.

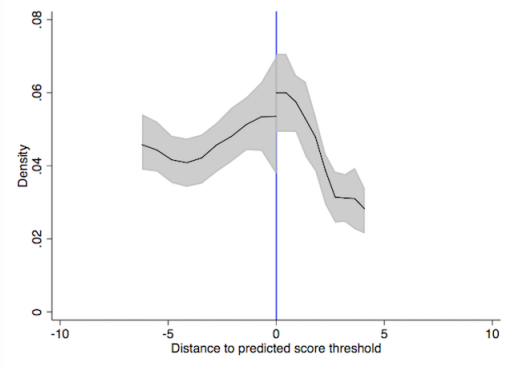
Figure 2: First Stage



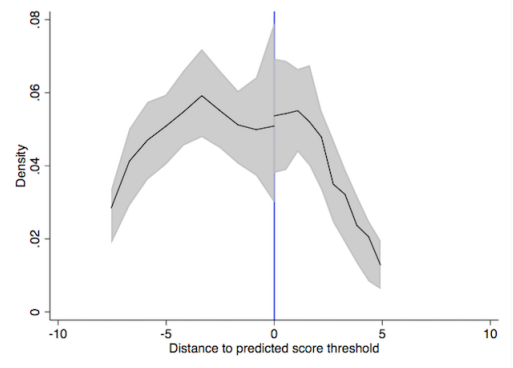
(a) First Stage of Applicants



(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. Panel (a) presents the estimated regression discontinuity 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.

Table 4: Living Standards

	In 2006			In 2010		
	Wealth Index	Housing Index	Asset Index	Alive	Register for Poverty Subsidies	Above Minimum Wage
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Applicants					
Recipient	0.196**	0.298**	0.0172	0.0238	-0.0927	0.0429*
	(0.0999)	(0.124)	(0.239)	(0.0512)	(0.108)	(0.0252)
Observations	405	345	345	963	324	577
Bandwidth	7.0	5.3	5.3	5.0	4.0	7.2
Mean Dep. Var.	0	0	0	0.46	0.72	0.02
	Panel B: Children					
Recipient	0.319**	0.369***	0.232**	-0.0543	-0.215*	0.221**
	(0.151)	(0.108)	(0.109)	(0.0625)	(0.123)	(0.0868)
Observations	393	298	298	646	460	460
Bandwidth	6.4	4.1	4.1	6.2	4.2	4.6
Mean Dep. Var.	0	0	0	0.89	0.58	0.17

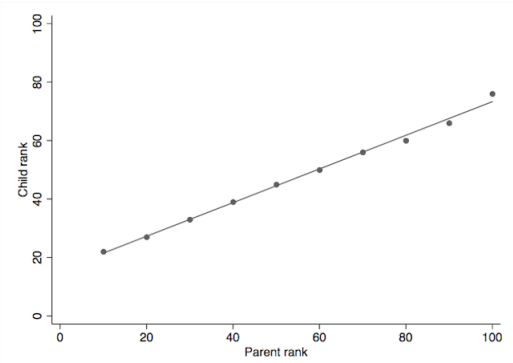
Notes: This table documents the intergenerational impacts of having received land in 1968-1970 on contemporary living standards using an RD design. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at applicant family level are in brackets. *Recipient* is an indicator variable equal to 1 if an applicant was eligible to be allocated land during the agrarian reform 1968-1970. The unit of observation is the applicant in Panel A and the children in Panel B. 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* and for columns (4)-(6) is *RUAF*. For a description of each dependent variable see Appendix B Table A.11. For a description of the construction of the wealth index see Appendix B Table A.12.

Table 5: Modern Economy in 2010

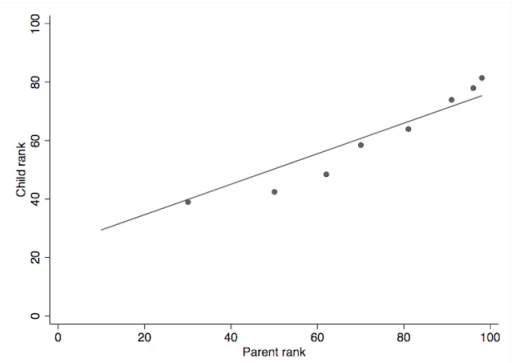
	Labor Markets			Financial Markets			Economic Activity			
	Works	Works in Formal Sector	Contributes to Social Security	Has Bank Account	Has Credit Card	Has Loan	Agriculture	Manufacturing	Services	Entrepreneurship
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
					Panel A: Applicants					
Recipient	-0.0143	0.118	0.0264	0.0672*	0.0171	0.0433	-0.152*	0.0108	0.154*	0.00585
	(0.0967)	(0.0833)	(0.0243)	(0.0388)	(0.0111)	(0.0492)	(0.0811)	(0.0138)	(0.0827)	(0.0111)
Observations	345	415	543	387	456	456	445	415	415	445
Bandwidth	5.8	4.2	6.4	4.2	5.5	5.5	5.2	5.3	5.2	6.4
Mean Dep. Var.	0.45	0.03	0.01	0.26	0.01	0.02	0.64	0.04	0.14	0.01
					Panel B: Children					
Recipient	-0.0422	0.245**	0.180**	0.157**	0.0390**	0.138**	0.0239	0.132**	-0.149	0.104**
	(0.111)	(0.111)	(0.0830)	(0.0684)	(0.0152)	(0.0565)	(0.101)	(0.0607)	(0.115)	(0.0467)
Observations	367	460	460	460	512	452	496	460	496	460
Bandwidth	5.1	4.2	4.2	5.2	6.5	5.5	5.4	4.4	5.4	4.5
Mean Dep. Var.	0.41	0.39	0.18	0.44	0.18	0.25	0.35	0.11	0.30	0.10

Notes: This table documents the intergenerational impacts of having received land in 1968-1970 on contemporary modern economy outcomes 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 an applicant was eligible to be allocated land during the agrarian reform 1968-1970. The unit of observation is the applicant in Panel A and the children in Panel B. 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) and (7)-(9) is *RUAF*, for (4)-(6) is *SuperFinanciera* and column (10) is *RUES*. For a description of each dependent variable see Appendix B Table A.11.

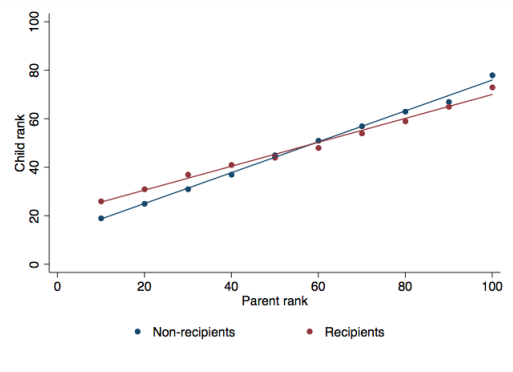
Figure 3: Intergenerational Mobility



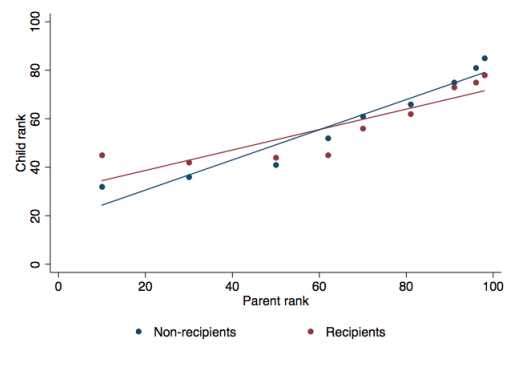
(a) Wealth Index



(b) Education



(c) Wealth Index



(d) Education

Notes: This figure graphically documents the intergenerational mobility impacts of having received 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 3c) and recipients and non-recipients separately (Figures 3b 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 mobility impacts of having received land in 1968-1970 among all applicants. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in brackets. It uses a wealth index and years of schooling in 2006 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

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

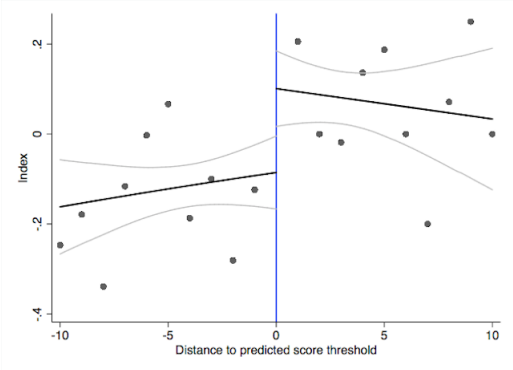
Notes: This table documents the intergenerational impacts of having received land in 1968-1970 on migration using an RD design. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at applicant family level are in brackets. *Recipient* is an indicator variable equal to 1 if an applicant was eligible to be allocated land during the agrarian reform 1968-1970. The unit of observation is the applicant in Panel A and the children in Panel B. 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 *RUAF*. For a description of each dependent variable see Appendix B Table A.11.

Table 9: Investment in Education in 2006

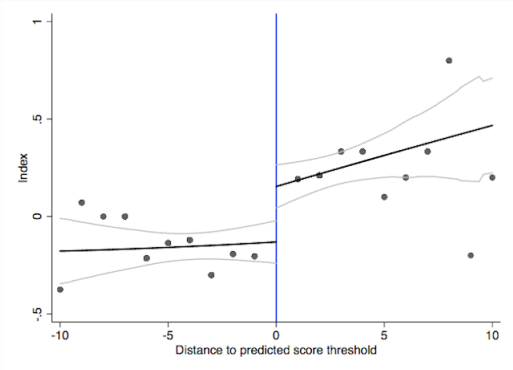
	Years of Schooling	Primary School	High School	Vocational Education	College	Attending School	Child Labor
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Adult Children					Young Children	
Recipient	1.551**	0.165**	0.136	0.0822	0.0688	0.0409**	-0.116
	(0.732)	(0.0805)	(0.105)	(0.0722)	(0.0549)	(0.0195)	(0.112)
Observations	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 an applicant was 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. 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 for columns (1)-(7) is *SISBEN*. For a description of each dependent variable see Appendix B Table A.11.

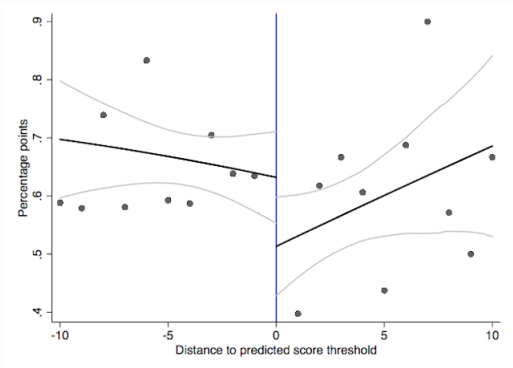
Figure 4: Reduced Forms for Applicants



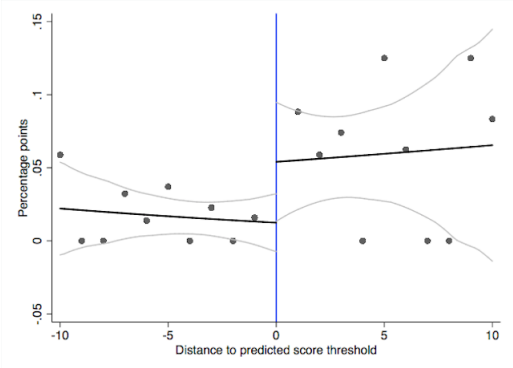
(a) Wealth index



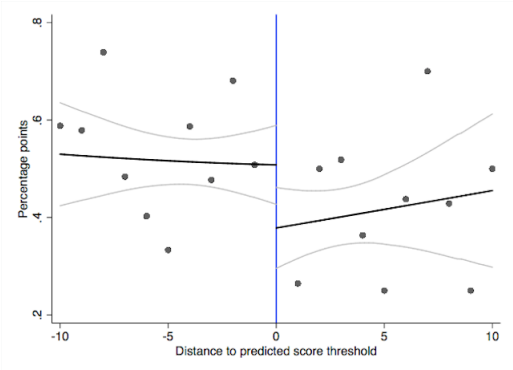
(b) Housing Index



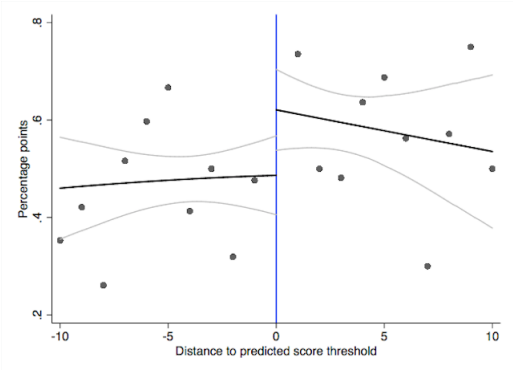
(c) Register for Poverty Subsidies



(d) Formal Sector



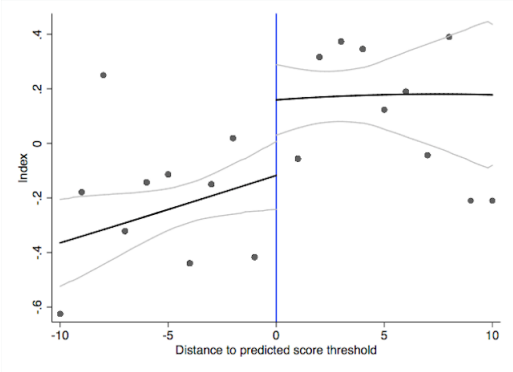
(e) Agriculture



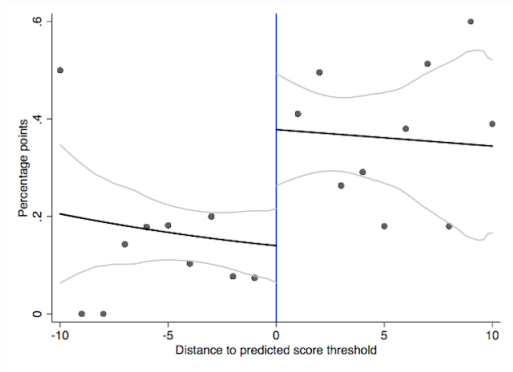
(f) Migration

Notes: This figure graphically documents RD reduced forms for applicants on different outcome variables. It shows RD plots documenting the effect of being eligible to become a *Recipient* of land during the agrarian reform 1968-1970 for 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*.

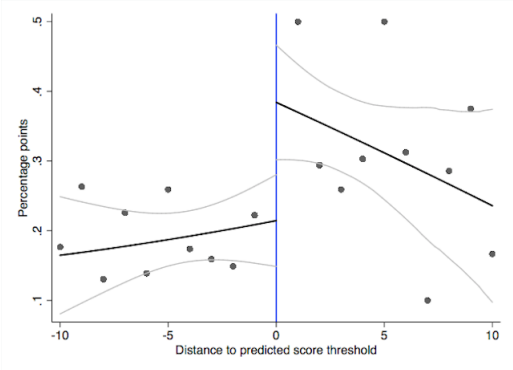
Figure 5: Reduced Forms for Children



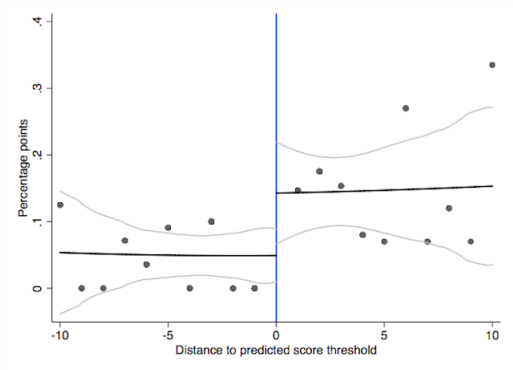
(a) Wealth Index



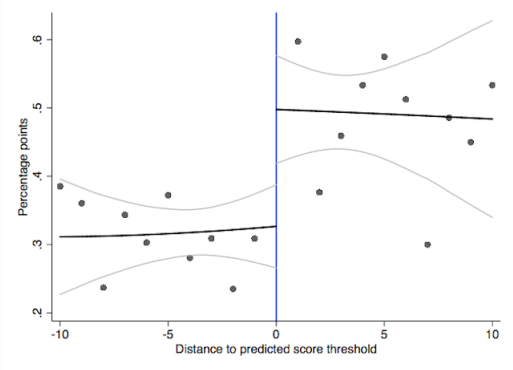
(b) Above Minimum Wages



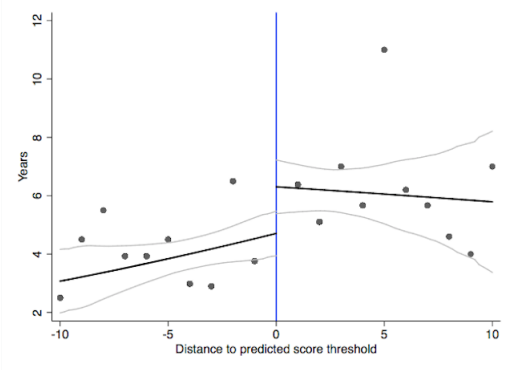
(c) Formal Sector



(d) Manufacturing



(e) Urban Migration



(f) Years of Schooling

Notes: This figure graphically documents RD reduced forms for children on different outcome variables. It shows RD plots documenting the effect of being a child of an applicant eligible to become a *Recipient* of land during the agrarian reform 1968-1970 for 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*.

Appendices

Appendix A Robustness checks

A.1 Tables

Table A.1: Robustness Checks for Table 3

	Linear Half optimal bandwith	Linear Twice optimal bandwith	Linear Triangular bandwith	Linear Epanechnikov bandwith	Quadratic	Cubic	Placebo 1	Placebo 2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: Applicants				
Wealth Index	0.199*	0.248**	0.185	0.187	0.140	0.171**	-0.231	0.412
	(0.120)	(0.113)	0.137	0.138	(0.182)	(0.0867)	(0.940)	(0.306)
Housing Index	0.261**	0.197**	0.302**	0.307*	0.337**	0.321*	0.0479	-0.370
	(0.129)	(0.100)	(0.147)	(0.158)	(0.155)	(0.172)	(0.159)	(0.320)
Asset Index	0.0581	0.112	-0.0470	-0.0646	0.0471	0.0191	0.0823	0.0771
	(0.242)	(0.210)	(0.254)	(0.255)	0.298	(0.179)	(0.300)	(0.0658)
Registers for Poverty Subsidies	-0.0223	-0.174***	-0.158**	-0.202***	-0.0504	0.0101	0.0678	0.179
	(0.120)	(0.0635)	(0.0667)	(0.0611)	(0.0905)	(0.121)	(0.159)	(0.246)
Above Minimum Wages	0.0207	0.0441**	0.0302	0.0361	0.0163	0.0147	-0.0801	0.0350
	(0.0304)	(0.0213)	(0.0239)	(0.0240)	(0.0286)	(0.0291)	(0.0681)	(0.0265)
				Panel B: Children				
Wealth Index	0.421***	0.238**	0.327***	0.332***	0.254	0.273	0.0741	0.206
	(0.104)	(0.106)	(0.117)	(0.0864)	(0.258)	(0.445)	(0.933)	(0.633)
Housing Index	0.410***	0.374***	0.408***	0.390***	0.357	0.527	0.284	0.160
	(0.133)	(0.102)	(0.0943)	(0.0953)	(0.291)	(0.566)	(0.625)	(0.682)
Asset Index	0	0.184*	0.194**	0.194	0.200**	0.139	0.0319	-0.0790
	(0)	(0.105)	(0.0880)	(0.244)	(0.0898)	(0.117)	(0.0264)	(0.102)
Registers for Poverty Subsidies	-0.0574	-0.198**	-0.205*	-0.228*	-0.113	-0.0178	0.412	-0.211
	(0.192)	(0.0976)	(0.115)	(0.118)	(0.151)	(0.178)	(0.306)	(0.305)
Above Minimum Wages	0.213	0.157**	0.186**	0.189**	0.226	0.180	0.121	0.128
	(0.139)	(0.0761)	(0.0776)	(0.0781)	(0.158)	(0.216)	(0.0921)	(0.106)

Notes: This table documents different robustness checks for outcome in Table 3. *** p<0.01, ** p<0.05, * p<0.1. Each cell reports the coefficient from a type of RD regression, shown columns, of an outcome on *Recipient*, an indicator variable equal to 1 if an applicant was eligible to be allocated land during the agrarian reform 1968-1970, shown in rows. The unit of observation is the applicant in Panel A and the children in Panel B. 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). Source: INCORA, *SISBEN*, *RUAF*. For a description of each dependent variable see Appendix B Table A.11.

Table A.2: Robustness Checks for Table 4

	Linear Half optimal bandwith	Linear Twice optimal bandwith	Linear Triangular bandwith	Linear Epanechnikov bandwith	Quadratic	Cubic	Placebo 1	Placebo 2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Applicants								
Works	-0.0405	-0.0516	-0.0211	-0.0199	-0.0371	-0.0273	0.0381	-0.00827
	(0.104)	(0.111)	(0.0748)	(0.0742)	(0.0831)	(0.181)	(0.125)	(0.0970)
Works in Formal Sector	-0.146	0.113	0.143	0.171	0.00934	-0.0581	-0.855	0.136
	(0.242)	(0.112)	(0.131)	(0.136)	(0.168)	(0.191)	(0.788)	(0.147)
Contributes to Social Security	0.0258	0.0314	0.0326	0.0289	0.0161	0.00712	-0.0166	-0.0517
	(0.0274)	(0.0262)	(0.0293)	(0.0278)	(0.0254)	(0.0251)	(0.0433)	(0.0555)
Agriculture	-0.0208	-0.107	-0.140*	-0.131*	-0.138	-0.151	0.239	-0.110
	(0.127)	(0.0656)	(0.0786)	(0.0740)	(0.0929)	(0.111)	(0.286)	(0.0965)
Manufacturing	-0.00345	0.0111	0.00988	0.0139	0.0107	0	0.0108	-0.00627
	(0.0259)	(0.0134)	(0.0120)	(0.0198)	(0.0133)	(0)	(0.0138)	(0.0261)
Services	-0.106	0.0903	0.139*	0.137*	0.159	0.153	-0.200	0.107
	(0.158)	(0.0705)	(0.0802)	(0.0759)	(0.0983)	(0.110)	(0.259)	(0.0960)
Panel B: Children								
Works	0	-0.0323	-0.0591	-0.0536	-0.0323	-0.0362	0.00188	-0.00190
	(0)	(0.103)	(0.0896)	(0.0899)	(0.0996)	(0.184)	(0.114)	(0.111)
Works in Formal Sector	0.0975	0.160**	0.157***	0.175***	0.0890	-0.00570	-0.111	0.105
	(0.0820)	(0.0589)	(0.0575)	(0.0785)	(0.0968)	(0.0575)	(0.284)	(0.0788)
Contributes to Social Security	0.177	0.160*	0.185**	0.181**	0.140	0.163	0.197	0.117
	(0.159)	(0.0819)	(0.0866)	(0.0858)	(0.107)	0.163	(0.186)	0.117
Agriculture	-0.0358	-0.0225	0.00218	-0.00617	-0.0178	-0.0705	0.0386	-0.0255
	(0.176)	(0.0824)	(0.0995)	(0.100)	(0.116)	(0.148)	(0.374)	(0.106)
Manufacturing	0.137*	0.0601	0.0945**	0.1000**	0.0967*	0.0737	0.0625	-0.0198
	(0.0738)	(0.0469)	(0.0466)	(0.0476)	(0.0588)	(0.0648)	(0.0446)	(0.116)
Services	-0.145	-0.0762	-0.157	-0.145	-0.124	-0.0810	-0.356	-0.141
	(0.139)	(0.0856)	(0.115)	(0.115)	(0.139)	(0.169)	(0.382)	(0.139)

Notes: This table documents different robustness checks for outcome in Table 4. *** p<0.01, ** p<0.05, * p<0.1. Each cell reports the coefficient from a type of RD regression, shown columns, of an outcome on *Recipient*, an indicator variable equal to 1 if an applicant was eligible to be allocated land during the agrarian reform 1968-1970, shown in rows. The unit of observation is the applicant in Panel A and the children in Panel B. 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). Source: INCORA, *RUAF*. For a description of each dependent variable see Appendix B Table A.11.

Table A.3: Robustness Checks for Table 8 and Table 9

	Linear Half optimal bandwith	Linear Twice optimal bandwith	Linear Triangular bandwith	Linear Epanechnikov bandwith	Quadratic	Cubic	Placebo 1	Placebo 2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Applicants								
Migration	0.0350	0.120*	0.160**	0.157**	0.182*	0.182	-0.170	0.127
	(0.130)	(0.0665)	(0.0794)	(0.0747)	(0.0976)	(0.113)	(0.286)	(0.0965)
Urban Migration	0.117*	0.135***	0.111*	0.116*	0.0940	0.0841	0.000164	0.109
	(0.0630)	(0.0436)	(0.0627)	(0.0609)	(0.0783)	(0.0957)	(0.147)	(0.0726)
Rural Migration	-0.0635	-0.0908*	-0.0934*	-0.0985*	-0.0515	-0.0304	-0.0632	-0.0660
	(0.0991)	(0.0506)	(0.0543)	(0.0529)	(0.0747)	(0.0898)	(0.120)	(0.0698)
Panel B: Children								
Migration	0	0.0575	0.287***	0.293***	0.277***	0.282**	0.102	0.0193
	(0)	(0.0504)	(0.119)	(0.111)	(0.102)	(0.121)	(0.151)	(0.362)
Urban Migration	0	0.289***	0.282***	0.284***	0.249***	0.122	-0.0957	-0.405
	(0)	(0.103)	(0.0673)	(0.0649)	(0.0621)	(0.175)	(0.290)	(0.148)
Rural Migration	0.147	0.0921	0.130	0.129	0.198	0.243	0.0297	0.172
	(0.183)	(0.0892)	(0.107)	(0.105)	(0.142)	(0.171)	(0.295)	(0.122)
Years of Schooling	1.426*	1.218*	1.890**	1.866**	0.432	0.759	0.322	0.296
	(0.843)	(0.637)	(0.940)	(0.936)	(0.725)	(0.703)	(0.246)	(0.325)
Primary School	0.234***	0.168	0.191***	0.170***	0.146	0.163	0.0699	0.0663

Notes: This table documents different robustness checks for outcomes in Table 8 and 9. *** p<0.01, ** p<0.05, * p<0.1. Each cell reports the coefficient from a type of RD regression, shown columns, of an outcome on *Recipient*, an indicator variable equal to 1 if an applicant was eligible to be allocated land during the agrarian reform 1968-1970, shown in rows. The unit of observation is the applicant in Panel A and the children in Panel B. 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). Source: INCORA, *SISBEN*, *RUAF*. For a description of each dependent variable see Appendix B Table A.11.

Table A.4: OLS Regressions

	In 2006		In 2010			
	Wealth Index	Housing Index	Register for Poverty Subsidies	Above Minimum Wage	Formal Sector	Agriculture
	(1)	(2)	(2)	(4)	(5)	(6)
			Panel A: Applicants			
Recipient	0.14	0.217*	-0.0307	0.00302	0.0454	-0.0221
	(0.113)	(0.114)	(0.0540)	(0.0133)	(0.0450)	(0.0523)
R^2	0.32	0.16	0.30	0.35	0.27	0.32
Observations	728	728	975	975	975	975
Mean Dep. Var.	0	0	0.72	0.03	0.23	0.50
			Panel B: Children			
Recipient	0.198***	0.259**	-0.187	0.132	0.145	-0.134
	(0.0725)	(0.107)	(0.425)	(0.333)	(0.197)	(0.0887)
R^2	0.48	0.45	0.58	0.60	0.61	0.71
Observations	638	638	991	991	991	991
Mean Dep. Var.	0	0	0.58	0.17	0.43	0.32

Notes: This table documents the intergenerational impacts of having received land in 1968-1970 on selected outcome variables using OLS regressions. *** 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 in Panel A and the children in Panel B. All regressions include the following controls: age, sex, marital status, expropiation file fixed effects. Source: INCORA, *SISBEN*. For a description of each dependent variable see Appendix B Table A.11.

Table A.5: RD Regressions Excluding Urban Migrants

	In 2006		In 2010			
	Wealth Index	Housing Index	Register for Poverty Subsidies	Above Minimum Wage	Formal Sector	Agriculture
	(1)	(2)	(2)	(4)	(5)	(6)
			Panel A: Applicants			
Recipient	0.0739	0.222	-0.0745	0.0374	0.0655	-0.0817
	(0.102)	(0.123)	(0.0945)	(0.0307)	(0.0792)	(0.106)
Observations	314	286	316	316	316	316
Bandwidth	5.2	4.5	4.4	4.5	4.5	4.3
Mean Dep. Var.	0	0	0.80	0.03	0.15	0.63
			Panel B: Children			
Recipient	-0.0513	0.357	-0.130	0.135	-0.00125	-0.246
	(0.215)	(0.291)	(0.185)	(0.123)	(0.183)	(0.210)
Observations	302	244	358	358	358	358
Bandwidth	5.4	4.3	4.3	3.9	4.7	3.5
Mean Dep. Var.	0	0	0.65	0.17	0.28	0.45

Notes: This table documents the intergenerational impacts of having received land in 1968-1970 on selected outcome variables excluding from the sample urban migrants and 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 an applicant was eligible to be allocated land during the agrarian reform 1968-1970. The unit of observation is the applicant in Panel A and the children in Panel B. 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). Source: INCORA, *RUAF*. For a description of each dependent variable see Appendix B Table A.11.

Table A.6: Investment in Education Among All Children

	Years of schooling	Primary school	High school	Vocational education	College
	(1)	(2)	(3)	(4)	(5)
Recipient	0.759	0.0994	-0.169	-0.0591	-0.0353
	(0.703)	(0.0887)	(0.106)	(0.0896)	(0.0592)
Observations	298	367	367	367	367
Bandwidth	4.4	5.3	5.3	5.3	5.3
Mean Dep. Var.	5.1	0.52	0.28	0.05	0.03

Notes: This table documents the impacts of having received land in 1968-1970 on investment in the education of children among all adult children 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 an applicant was eligible to be allocated land during the agrarian reform 1968-1970. The unit of observation the child of an applicant. 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)-(5) is *SISBEN*. For a description of each dependent variable see Appendix B Table A.11.

Table A.7: Civil Conflict

	Violent Death <2006 (1)	Forced Displacement 1985-2010 (2)	Criminal Record 1990-2010 (3)
	Panel A: Applicants		
Recipient	0.117*	0.0490*	-0.0379
	(0.0504)	(0.0193)	(0.0165)
Observations	645	573	468
Bandwidth	5.2	6.5	6.2
Mean Dep. Var.	0.08	0.03	0.01
	Panel B: Children		
Recipient	0.0538	0.0945	0.0214
	(0.0673)	(0.152)	(0.0366)
Observations	645	1328	468
Bandwidth	6.2	7.5	6.2
Mean Dep. Var.	0.11	0.05	0.01

Notes: This table documents the impacts of having received land in 1968-1970 on civil conflict outcomes 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 an applicant was eligible to be allocated land during the agrarian reform 1968-1970. The unit of observation is the applicant in Panel A and the children in Panel B. 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). Source: INCORA, *RUAF*, *RUPTA*, *PROCURADURIA*.

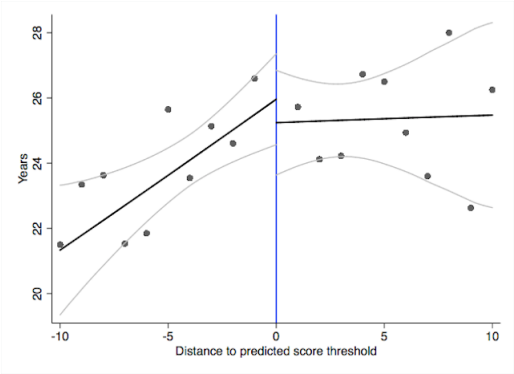
Table A.8: Cost-Benefit Analysis

Returns to Education (1)	% Benefited (2)	Rate of Return (3)
10%	40%	-79.5%
10%	60%	-69.3%
10%	80%	-59.0%
15%	40%	-69.5%
15%	60%	-54.0%
15%	80%	-38.5%

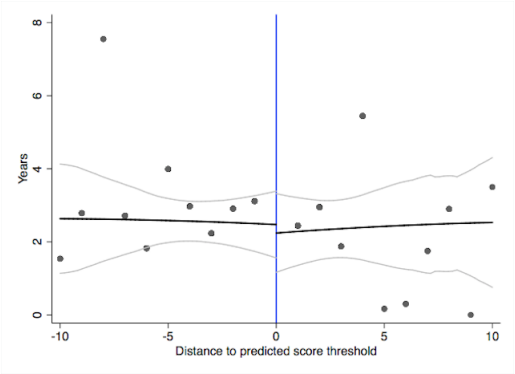
Notes: This table presents different scenarios for the cost-benefit analysis. Column (1) shows the returns to education assumption used, column (2) the percentage of the children of recipients benefiting from these returns and column (3) the fiscal investment per recipient family rate of return. Source: INCORA.

A.2 Figures

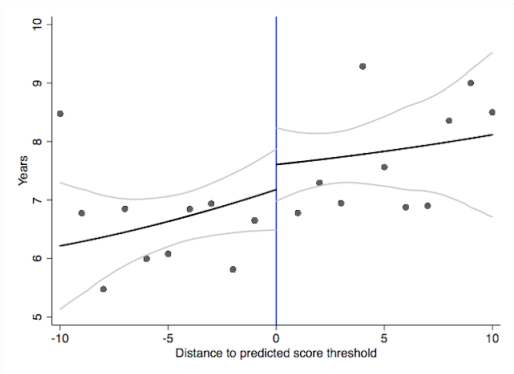
Figure A.1: Pre-Treatment Balance 1968-1970



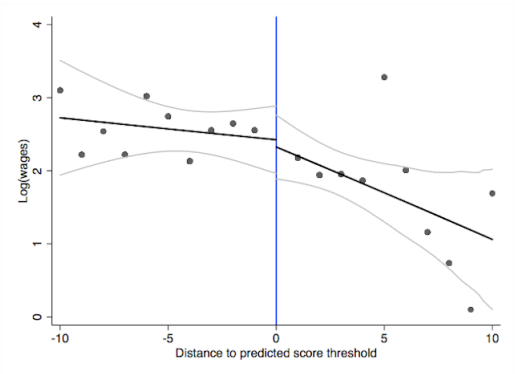
(a) Age



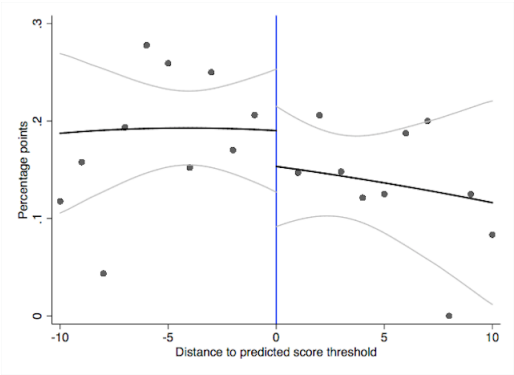
(b) Years of Schooling



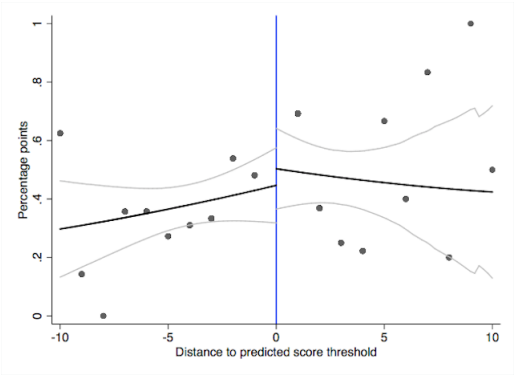
(c) Years of Agricultural Experience



(d) Log(Wages)



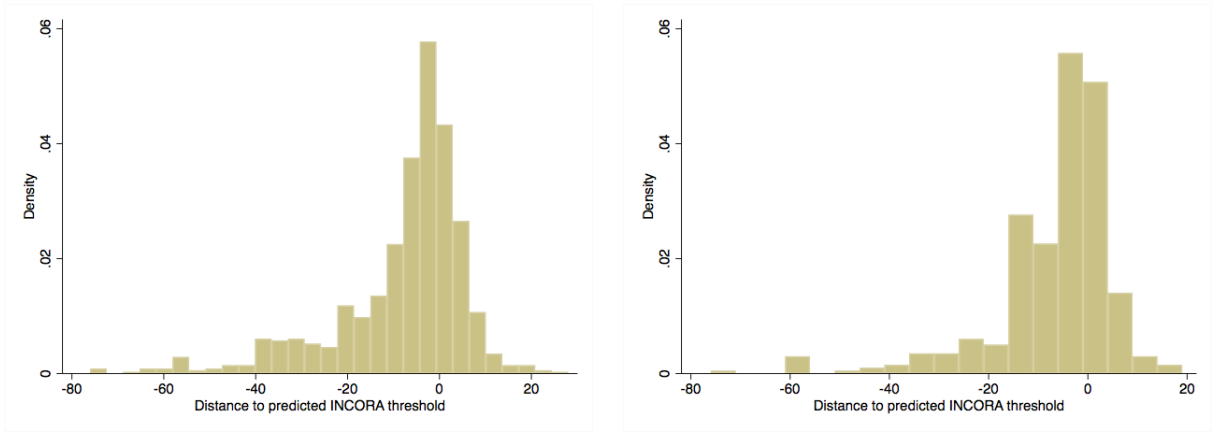
(e) Has House



(f) Staple Crops

Notes: This figure graphically documents pre-treatmen 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, *SISBEN*, *RUAF*.

Figure A.2: Histograms of Distance to Predicted Score Threshold



Notes: This figure plots histograms documenting the number of observations in each cumulative predicted INCORA score bins for applicants and children. Source: INCORA, *SISBEN*, *RUAF*.

Appendix B Data Construction

B.1 Agrarian Reform Data

In this section, I explain in detail the data sources and construction of agrarian reform information. As explained in section 3, this study uses historical data constructed from the archives of the extinguished Colombian Institute for Agrarian Reform (or INCORA), which are currently managed by the National Land Agency (ANT) at Bogotá, Colombia. Specifically, I draw upon three archival series: expropiation files from the *Sharecroppers and Tenants Program* and land titles issued by the agency and the National Registry of Civil Status (*Registraduria Nacional*) during the years 1966-1972. The archives are protected under Colombian law by privacy measures that prohibit the publication and use of personal information (Laws 1581 of 2012, 1712 of 2014, 79 of 1993, and Decree 1743 of 2016). Consequently, the data is accessed through a confidentiality agreement with the unique purpose of promoting academic research under the present research project.

The archives contain information on approximately twelve thousand expropiation files from the *Sharecroppers and Tenants Program* and one hundred thousand land titles granted by INCORA during this period, which include state-owned lands (or *baldios*), parcels and other types of transactions. Research assistants helped to tabulate information and construct a database. Each expropiation file includes the following information: legal documents (INCORA and judicial decisions, notarial records, etc.), technical studies of the landholding made by INCORA officials (*informe de visita*) and, if the expropiation took place, applicant surveys (*formulario de aplicación*). Each land title contains the name, ID number, date, place and area titled. As explained in section 2, only 10% of expropiation processeses were successfull. However, of these, only 218 effectively include systematic information regarding applicants to the reform. I use all data sources to collect personal data about each applicant: full name, ID number (or *cédula de ciudadania*), address, household members, occupation, working experience, wages, assets, housing, types of crops grown and whether it was titled a parcel of land or not. The scores assigned by INCORA are reported in numerous files but

not all.

Based on INCORA Directive 23 of 1966, I use information collected from the archives (complemented with notarial records) to reconstruct the scores used in the empirical strategy. The evaluation of each applicant was made along 4 key topics: family age characteristics, agricultural experience, assets and housing investments according to the grading system described in Table 1. Summing across all attributes, I calculate a predicted INCORA score for each applicant family. I compare them with those available in actual surveys to confirm they are similar. Next, for each expropiation process e I define its score threshold s_e as the minimum score needed to become a Recipient based on the number of Agricultural Family Units available for allocation. For instance, if officials decided there were 5 available AFUs, then the fifth ranked predicted score would become the cutoff for that process. This means thresholds varied at an expropiation process level. To make applicants comparable, I rescale each cutoff to zero by defining the distance between an applicant's score s_i and its respective score cutoff s_e as $dists_e = s_i - s_e$. Under this set up, applicants with a score above (or on) zero would be eligible to become recipients, while those below would not. I employ this variable to implement th RD design in the empirical strategy in section 5.

Table A.9: Agrarian Reform Data

Variable	Description	Type	Source
Agricultural Experience	Years	Integers	INCORA
Log(Wages)	Colombian pesos in 1968-1970	Continuous	INCORA
Has House	1=has house, 0=otherwise	Dummy	INCORA
Plot Area	Hectares	Continuous	INCORA
Grows Cash Crops	1=grows cash crops, 0=otherwise	Dummy	INCORA
Grows Staple Crops	1=grows staple crops, 0=otherwise	Dummy	INCORA

Figure A.3: Expropiation File

Nº29845

No.

REPUBLICA DE COLOMBIA

incora

CLASE DE ACTUACION ADQUISICIONES

INTERESADOS

NOMBRE DEL PREDIO

MUNICIPIO OVEJAS CORREGIMIENTO

DEPARTAMENTO SUCRE

INTENDENCIA

COMISARIA

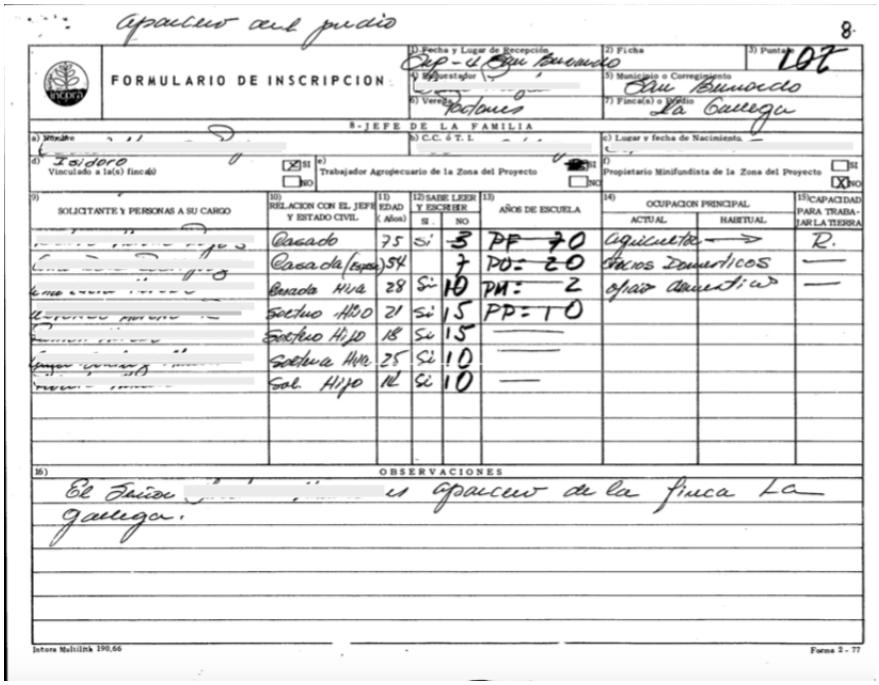
RADICADO

LIBRO I TOMO I FOLIO

BOGOTA, DICIEMBRE, MAYO 8 - 1969

2/11/73

Figure A.4: Agrarian Reform Applicant Survey



FORMULARIO DE INSCRIPCION

8

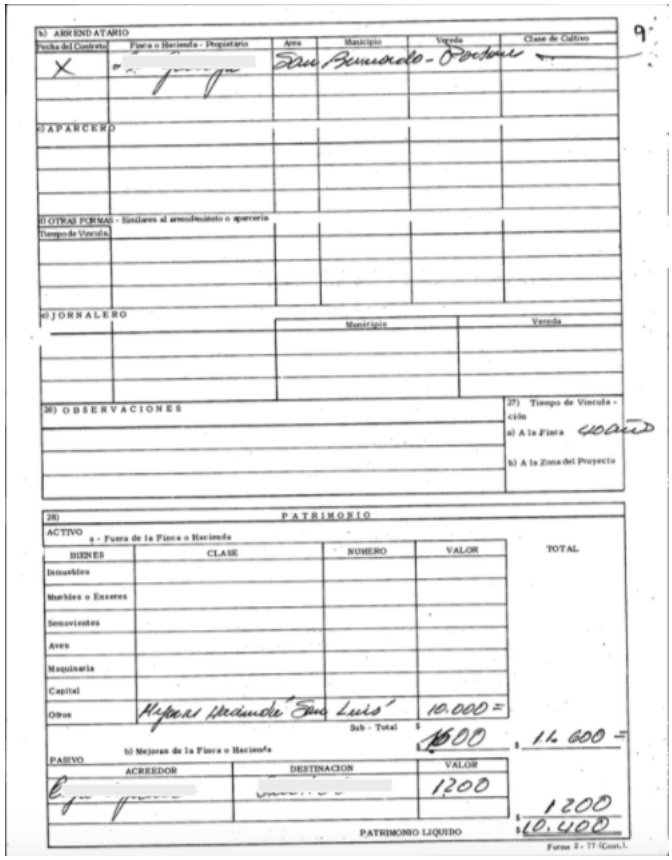
8. JEFE DE LA FAMILIA

SOLICITANTE Y PERSONAS A SU CARGO	RELACION CON EL JEFE Y ESTADO CIVIL	EDAD (Años)	SABE LEER Y ESCRIBIR SI	SABE LEER Y ESCRIBIR NO	AÑOS DE ESCUELA	OCUPACION PRINCIPAL ACTUAL	OCUPACION PRINCIPAL HABITUAL	CAPACIDAD PARA TRABAJAR LA TIERRA
	Casado	75	Si		3 PP: 2 PO: 2	Aquicultor		R.
	Casada (esposa)	54			7	Labores Domesticos		—
	Casada Hija	28	Si		10 PM: 2	Ayud. Domestica		—
	Soltero Hijo	21	Si		15 PP: 10			
	Soltero Hijo	18	Si		15	—		
	Soltera Hija	25	Si		10	—		
	Sol. Hijo	12	Si		10	—		

16) OBSERVACIONES

El Jefe [redacted] es aparcero de la finca La garriga.

Intera Multilink 190.66 Forma 2 - 77



9

b) ARRENDATARIO

Fecha del Contrato	Finca o Hacienda - Propietario	Area	Municipio	Vereda	Clase de Cultivo
X	[redacted]		San Bernardo - Pedernal		

c) APARCERO

d) OTRAS FORMAS - Similares al arrendamiento o aparcería

Tiempo de Vinculo				

e) JORNALERO

	Municipio	Vereda

26) OBSERVACIONES

27) Tiempo de Vinculación
a) A la Finca 40 años
b) A la Zona del Proyecto

28) PATRIMONIO

ACTIVO a - Fuera de la Finca o Hacienda

BIENES	CLASE	NUMERO	VALOR
Inmuebles			
Muebles o Enseres			
Semovientes			
Aves			
Maquinaria			
Capital			
Otros	Mejoras Hacienda "San Luis"		10.000 =

Sub - Total $ 1600 $ 11.600 =

b) Mejoras de la Finca o Hacienda

PASIVO

ACREEDOR	DESTINACION	VALOR
[redacted]		1200

$ 1200

PATRIMONIO LIQUIDO $ 10.400

Forma 2 - 77 (Cont.)

B.2 Subsample of Applicants and Children

In this section, I present the correlation between the probability of finding a child of an applicant in notarial records, the treatment variable and other relevant pre-treatment applicant characteristics.

Table A.10: Correlations of Subsample of Children

	RD	
	Coefficient	Standard Error
Recipient	-0.0239	(0.0365)
Score	-0.000565	(0.00120)
Years of Schooling	0.0245	(0.0185)
Years of Agricultural Experience	-0.000449	(0.00119)
Log(Wages)	-0.00669	(0.00915)
Has House	-0.00282	(0.00183)
Plot Area	-0.00282	(0.00183)
Distance to Urban Center (in Km)	-0.000337***	(0.000118)

*** p<0.01, ** p<0.05, * p<0.1. Each cell in Column (1) in this table reports the coefficient from a RD regression following Calonico et al (2017) of a pre-treatment applicant characteristic in 1968-1970 on the likelihood of finding a child in notarial records, with standard errors in parentheses in Column (2).

B.3 Administrative Data Linkage Algorithm

The merging of agrarian reform records with the outcome data (including Social Security and Benefits, Vital Statistics, Financial and Violence records) follows a simple algorithm involving the full names and ID numbers of applicants. In Colombia, as in most spanish speaking countries, a person has two legal last names: the first last name is inherited from the father and the second last name is inherited from the mother. A person can have more than one first name, with two first names being a popular combination. Moreover, names and last names can be often mispelled, which is why an error term in the linkage process is introduced. All government agencies mandated to match first on ID number and then on a combination of the 4 name variables. To be consistent across estimations, I use the same method for matching publicly collected data (including Entrepreneurship, Patents, Elections and Criminal records). Therefore, the algorithm is designed to match ID numbers and full names (two first names and two last names) based on phonetic coincidence along 16 criteria in descending order of importance.

1. 100% phonetic coincidence. Matches ID number, two first names and two last names.
2. 100% phonetic coincidence. Matches ID number, concatenate all first names and last names.
3. 100% phonetic coincidence. Matches ID number, concatenate all last names and first names.

4. 100% phonetic coincidence. Matches ID number, first names and first last name in agrarian reform data with at least one last name in outcome data.

5. 100% phonetic coincidence. Matches ID number, first names and second last name in agrarian reform data with at least one last name in outcome data.

6. 95% phonetic coincidence. Matches ID number, two first names and two last names.

7. 95% phonetic coincidence. Matches ID number, one first name and two last names (in absense of middle name in agrarian reform data).

8. 95% phonetic coincidence. Matches ID number, one first name and two last names (in absense of middle name in outcome data).

9. 90% phonetic coincidence. Matches ID number, two first names (second first name at 90%) and first last name.

10. 90% phonetic coincidence. Matches ID number, two first names at 90% and two last names.

11. 90% phonetic coincidence. Matches ID number, two first names and two last names at 90%.

12. 90% phonetic coincidence. Matches ID number, one of two first names at 90% and one of two last names at 90%.

13. 90% phonetic coincidence. Matches ID number, first names in outcome data match last names in agrarian reform data and vice-versa.

14. 90% phonetic coincidence. Matches ID number, first first name in agrarian reform data with one of the two first names in outcome data and two last names.

15. 90% phonetic coincidence. Matches ID number, second first name in agrarian reform data with one of the two first names in outcome data and two last names.

16. 90% phonetic coincidence. Matches ID number, second first name in agrarian reform data with one of the two first names in outcome data and two last names.

B.4 Contemporary Administrative Data

B.4.1 Sources and Description

As in the case of agrarian records, personal information in administrative data is also protected by privacy laws. Therefore, the outcome data that is legally safeguarded is accessed through confidentiality agreements with: National Planning Department, Ministry of Health and Social Protection, National Registry of Civil Status and Universidad de los Andes. All agreements guarantee the data is employed for academic research but prohibit personal information sharing, disclosure, or usage, in partial or full. A minority of the outcome data (RUES, etc.) used is publicly available at different government websites and web scapred.

Next, I describe in detail the outcome data sources and construction of the various administrative data used in the paper.

Table A.11: Outcome Data

Variable	Description	Type	Date	Source
Wealth Index	1-10 score	Continuous	2006	*SISBEN*
Household Index	1-10 score	Continuous	2006	*SISBEN*
Assets Index	1-10 score	Continuous	2006	*SISBEN*
Electricity	1=has electricity, 0=otherwise	Dummy	2006	*SISBEN*
Sewage	1=has sewage, 0=otherwise	Dummy	2006	*SISBEN*
Aqueduct	1=has aqueduct, 0=otherwise	Dummy	2006	*SISBEN*
Running Water	1=has running water, 0=otherwise	Dummy	2006	*SISBEN*
Gas	1=has gas, 0=otherwise	Dummy	2006	*SISBEN*
Alive	1=alive in 2010, 0=otherwise	Dummy	2010	*RUAF-Estadísticas Vitales*
Registers for Poverty Subsidies	1=found in *SISBEN*, 0=otherwise	Dummy	2010	*RUAF*
Above Minimum Wages	1=wage>minimum wage, 0=otherwise	Dummy	2010	*PILA*
Works	1=works, 0=otherwise	Dummy	2010	*RUAF*
Works in Formal Sector	1=is in contributory regime, 0=otherwise	Dummy	2010	*RUAF & PILA*
Contributes to Social Security	1=contributions>0, 0=otherwise	Dummy	2010	*PILA*
Has Bank Account	1=has bank account, 0=otherwise	Dummy	2010	*SuperFinanciera*
Has Credit Card	1=has credit card, 0=otherwise	Dummy	2010	*SuperFinanciera*
Has Loan	1=has loan, 0=otherwise	Dummy	2010	*SuperFinanciera*
Agriculture	1=works in sectors CIIU Rev 4: A, 0=otherwise	Dummy	2010	*RUAF-Afiliaciones Salud & PILA*
Manufacturing	1=works in sectors CIIU Rev 4: C, 0=otherwise	Dummy	2010	*RUAF-Afiliaciones Salud & PILA*
Services	1=works in sectors code CIIU Rev 4: H-S, , 0=otherwise	Dummy	2010	*RUAF-Afiliaciones Salud & PILA*
Entrepreneurship	1=has mercantile register, 0=otherwise	Dummy	2005-2018	*RUES*
Migration	1=if migrated, 0=otherwise	Dummy	2010	*RUAF*
Urban Migration	1=if migrated to city>300 thousand inhab., 0=otherwise	Dummy	2010	*RUAF*
Rural Migration	1=if migrated to places<25 thousand inhab., 0=otherwise	Dummy	2010	*RUAF*
Years of Schooling	Years	Integers	2006	*SISBEN*
Primary School	1=finished primary school, 0=otherwise	Dummy	2006	*SISBEN*
High School	1=finished high school, 0=otherwise	Dummy	2006	*SISBEN*
Technical Education	1=finished technical education, 0=otherwise	Dummy	2006	*SISBEN*
College	1=finished college, 0=otherwise	Dummy	2006	*SISBEN*
Attending School	1=finished attending school, 0=otherwise	Dummy	2006	*SISBEN*
Child Labor	1=is child works, 0=otherwise	Dummy	2006	*SISBEN*
Violent Death	1=death is homicide-massacre, 0=otherwise	Dummy	Death year	*RUAF-Estadísticas Vitales*
Displaced	1=appears in RUPTA, 0=otherwise	Dummy	1980-2010	*RUPTA*
Criminal Record	1=has criminal record at Procuraduria, 0=otherwise	Dummy	1980-2018	*Procuraduria*

B.4.2 Principal Component Analysis (PCA)

To calculate housing and asset indices with the *SISBEN* data, I use standard principal component analysis. This statistical procedure uses an orthogonal transformation to convert a set of observations of possibly correlated variables into a new system such that the greatest variance by some projection of the data comes to lie on the first coordinate (called the first principal component), the second greatest variance on the second coordinate, and so on (Jolliffe, 2002). Consider a data matrix, X, with column-wise zero empirical mean (the sample mean of each column has been shifted to zero), where each of the n rows represents a different repetition of the experiment, and each of the p columns gives a particular kind of feature (say, the results from a particular sensor). Mathematically, the transformation is defined by a set of p-dimensional vectors of weights $w_k = (w_1, ..., w_p)_{(k)}$ that map each row vector X_i of X to a new vector of principal component scores $t_i = (t_1, ..., t(_l)_{(i)}$ given by:

$$t_{k(i)} = x_i w_k \tag{2}$$

In such a way that the individual variables t of t considered over the data set successively inherit the maximum possible variance from x, with each loading vector w constrained to be a unit vector. In order to maximize variance, the first loading vector w_1 satisfies:

$$w_1 = argmax \frac{w^T X^T X w}{w^T w} \tag{3}$$

The quantity to be maximised can be recognised as a Rayleigh quotient. A standard result for a positive semidefinite matrix such as XTX is that the quotient's maximum possible value is the largest eigenvalue of the matrix, which occurs when w is the corresponding eigenvector. With w_1 found, the first principal component of a data vector x_i can then be given as a score $t_{1(i)} = x_i w_1$ in the transformed co-ordinates. Table A.12 presents the variables used to calculate the wealth index using principal component analysis.

Table A.12: Wealth Index Composition

Variable	Description	Type
Housing type	1=house or apartment, 2=room, 3=other	Integers
Risk	1=high, 2=regular, 3=low	Integers
Walls	1=block, brick, stone, polished wood 2=clay, 3=wattle and daub 4=prefabricated material 5=coarse wood, plank 6=bamboo, cane, mat, other vegetable 7=zinc, cloth, cannon, cans, waste, plastics 0=without walls	Integers
Floor	1=carpet or rug, marble, marque, polished wood 2=tile, vinyl, tablet or brick 3=cement, gravel 4=rough or shabby wood plank 5=dust, sand	Integers
Rooms	Number of rooms	Continuous
Kitchen	1=has kitchen, 0=otherwise	Dummy
Bathrooms	Number of bathrooms	Continuous
Toilet	1=toilet connected to aqueduct 2=toilet connected to septic tank 3=toilet not connected 4=latrine 0=no toilet	Integers
Shower	1=has shower, 0=otherwise	Dummy
Trash	1=has trash disposal, 0=otherwise	Integers
Fridge	1=has fridge, 0=otherwise	Dummy
Washing machine	1=has washing machine, 0=otherwise	Dummy
TV	1=has TV, 0=otherwise	Dummy
Cable TV	1=has cable TV, 0=otherwise	Dummy
Telephone	1=has telephone, 0=otherwise	Dummy
Oven	1=has oven, 0=otherwise	Dummy
Heater	1=has heater, 0=otherwise	Dummy
Computer	1=has computer, 0=otherwise	Dummy
Car	1=has car, 0=otherwise	Dummy
Electricity	1=has electricity, 0=otherwise	Dummy
Aqueduct	1=has aqueduct, 0=otherwise	Dummy
Sewage	1=has sewage, 0=otherwise	Dummy
Running water	1=has running water, 0=otherwise	Dummy
Gas	1=has car, 0=otherwise	Dummy