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Sent Away: The Long-Term Effects of Slum Clearance on Children and Families

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Sent Away: The Long-Term Effects of Slum Clearance on Children and Families

A thesis submitted in partial satisfaction of the requirements for the degree Doctor of Philosophy in Economics

by

Fernanda Catalina Rojas Ampuero

2022

ABSTRACT OF THE DISSERTATION

Sent Away: The Long-Term Effects of Slum Clearance
on Children and Families

by

Fernanda Catalina Rojas Ampuero

Doctor of Philosophy in Economics
University of California, Los Angeles, 2022
Professor Dora L. Costa, Chair

In this dissertation I study the long-term effects of moving to a high-poverty neighborhood on earnings and schooling using evidence from a slum clearance program implemented in Santiago, Chile, between 1979 and 1985. During the country's dictatorship, the government mandated the eviction of entire slums and their relocation to public housing in low-income areas: Two-thirds of slums were relocated to new housing projects on the periphery of the city, and the rest received housing at their initial location. This dissertation is divided into three chapters. In Chapter 1, I estimate a displacement effect by comparing the outcomes of displaced and non-displaced children and their parents 20 to 40 years after the end of the policy. I construct a novel data set that combines archival records with administrative data containing 19,852 homeowners matched to 55,343 children. I show that displacement is unrelated to families' demographics or neighborhood attributes prior to eviction, and I find negative effects on children and families: Displaced children have 10% lower earnings and 0.5 fewer years of education as adults than non-displaced. Moreover, displaced children are more likely to work in informal jobs and their parents are more likely to die after the intervention. In Chapter 2, I study the mechanisms that explain the negative effects experienced by displaced children when young. I find that destination characteristics mediate my results:

Lower social cohesion in destination projects reduces children's schooling, and their earnings are also affected by worse labor market access. Finally, in Chapter 3, I describe the data collection process in detail. I explain the processes to construct family composition and how I link parents and children. I also describe how I match families to slums of origin, and I discuss potential selection issues due to missing data.

The dissertation of Fernanda Catalina Rojas Ampuero is approved.

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2022

To my mother and my father.

A mi madre y mi padre.

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Introduction

This dissertation provides evidence on the long-term impacts of moving to high poverty neighborhood on children's and adults' socioeconomic outcomes. To do so, I study a slum clearance and urban renewal program that occurred between 1979 and 1985 in Santiago, Chile, when the country was under a dictatorship. I collect and digitize historical data that I combine with administrative sources that allows me to follow individuals 20 to 40 years after the end on the policy.

In Chapter 1, titled "Sent Away: The Long-Term Effects of Slum Clearance," I investigate the long-term effects of moving to a high-poverty neighborhood on children's adult earnings and education. The setting is a slum clearance and urban renewal program that occurred between 1979 and 1985 in Santiago, Chile, when the country was under a dictatorship. The program mandated the eviction of entire slums across the city and turned slum dwellers into homeowners; however, whereas some slums were upgraded, other slums were relocated to high poverty areas in peripheral sectors of the city.

I use the quasi-random variation between displaced families and non-displaced families to estimate a displacement effect on children's long-term outcomes. The selection of slums into the displaced and non-displaced depended on urban renewal feasibility, not on individual family or slum population characteristics. I find no evidence that the choice of slums to be evicted was correlated with the demographic and socioeconomic characteristics of the slum's families before the program, nor with families' access to public goods, nor their access to labor markets prior to eviction.

To estimate a displacement effect, I create a novel dataset that follows the children and parents from displaced and non-displaced slums 20 to 40 years after the end of the policy. This dataset is constructed from archival records and administrative data. I determine where families were sent, match children with their families, and match individuals with data on employment, labor earnings, school enrollment, and mortality. The final data includes 55,343 children and 34,000 parents.

My results show that displacement was detrimental for children: Compared with the non-displaced, displaced children earned on average 10% less per month across their life cycle. This negative effect on earnings is not associated with lower employment but with the quality of employment: displaced children were more likely to work in temporary jobs and without a formal contract. In addition, I find that displacement reduced children's educational attainment: A displaced child lost 0.5 fewer years of education and was 12% less likely to graduate from high school relative to a non-displaced child.

I also look at the long-term consequences of displacement on adults' economic outcomes. I examine adult mortality by gender. I find that both displaced men and women are 25% more likely to die within 35 years of the intervention compared to non-displaced adults, and this effect is concentrated in the first ten years after the intervention. I find that around one-third of the effect can be attributed to location characteristics.

The magnitude of the results is similar for mothers and fathers; although, the causes seem to be different: Mothers' mortality correlates negatively with access to health care centers in the new locations, while fathers' mortality correlates negatively with the quality of the new locations, measured by aggregate schooling and employment in the new neighborhoods. In addition, conditional on survival to the year 2007, I find that displaced heads of households were more likely to be employed after retirement ages and had lower earnings and lower pensions.

In Chapter 2, titled "Mediating mechanisms: Place effects," I explore the mechanisms driving the displacement effect on children. I find that a great share of the effect can be attributed to destination municipalities and to projects' characteristics. I find that municipalities of destination explain 70% of the effect on earnings. Low labor market access in the destination neighborhoods, measured as access to public transportation, reduces children's earnings. Likewise, projects that mixed slum families from different origins create fragmented neighborhoods (as measured by HHI) that impact children's educational attainment negatively.

In Chapter 3, I describe the data collection process to link slums, parents and children. I describe each of the stages to link individuals across datasets and time, from archival records to administrative sources. To construct family composition, I use the structure of names in Chile and administrative data on birth certificates for the adult population. Finally, I discuss selection issues due to missing data from the archives, and how this type of attrition might affect the displacement estimates from Chapters 1 and 2.

This dissertation is part of an ongoing research agenda that aims to understand the long-term consequences of urban policies in developing countries. Specifically, I am interested in understanding, both the individual and the aggregate effects of urban renewal and slum clearance policies on segregation, poverty, and income inequality in urban areas. In addition, I aim to investigate the role of preferences in location choice on children's long-term outcomes, by comparing the urban renewal program studied in this dissertation to a slum clearance program that allowed families to choose their destination municipality. To continue with this endeavor, I am in the process of collecting the archival records in the second housing program.

Chapter 1

The Long-Term Effects of Slum Clearance

1. Introduction

More than 25% of the world's urban population today live in slums (UN-Habitat, 2020). A common policy response to high poverty and the large share of slum dwellers in developing countries has been to provide low-income housing in city peripheries and suburban areas (Belsky et al., 2013).[1] However, it is unclear whether these policies benefit recipients: Despite the improvement in housing quality, families lose in terms of proximity to jobs, social networks, and access to public goods, such as schools and health provision (Lall et al., 2006; Barnhardt et al., 2016). There is little evidence on how moving to peripheral neighborhoods, rather than upgraded housing on site, affects the long-run outcomes of residents and their children.

[1] Examples of this policy can be found in Brazil (Dasgupta and Lall, 2009); India (Barnhardt et al., 2016); and Kenya (see here). Many slum clearance programs around the world are characterized by being forced relocations of the poor; for more details, see Goetz (2012). Historically, building social housing on city peripheries used to be a common policy in many European cities during the 1950s and 1960s, as described by Power (1993) and Hall (1997). In countries like the US or Canada, public housing is not necessarily built in city peripheries, but rather are usually located in poor areas of the city (Chyn, 2018; Oreopoulos, 2003).

In this chapter, I study the long-term effects of moving to a high-poverty neighborhood on the earnings and schooling of children and their families. I examine the impacts of a large-scale slum clearance and urban renewal program, the *Program for Urban Marginality*, that was implemented during the Chilean dictatorship between 1979 and 1985. The program was large in scope, because it affected more than 5% of the total population of Greater Santiago (the capital). All of the slum dwellers in the program became homeowners, but whereas some slums were upgraded into neighborhoods, other slums were relocated to suburban areas. The program consisted of two types of intervention. In the first, whenever urban conditions permitted it, a slum was upgraded into a proper neighborhood and families could remain in the same place; these are the non-displaced families. In the second, when upgrading was not possible, families were evicted and forced to move in groups to new public housing projects; these are the displaced families.

I use variation between the two groups to estimate a displacement effect. While both groups of families became homeowners, the displaced were *forced* to move to a new location. Thus, what differed between groups was the disruption from having to move and the characteristics of their destination locations. First, I use the variation with respect to which slums were moved to identify the total impact of displacement, because the selection of slums into the displaced or non-displaced group depended on the feasibility of urban renewal, not on individual family or slum population characteristics. Urban conditions such as slum density, geographic location within a municipality, and the price of land determined whether slums were appropriate locations for building on-site public housing. Within municipalities, however, slums were very similar to each other. I find no evidence that the choice of slums to be evicted was correlated with the demographic or socioeconomic characteristics of the slum's families before the program. I also find no evidence that displaced and non-displaced slums differed in their access to public goods, the characteristics of their populations, or their access to labor markets prior to eviction.

Second, in addition to the forced movement, displaced families were *assigned* a destination. This variation in destination allows me to isolate the place effect and identify some of the mechanisms driving the displacement effect, which I study in detail in chapter 2 of this dissertation. Displaced families were disproportionately moved to low-income municipalities and housed in neighborhoods mostly located on the periphery of the city. Although on average these new areas were characterized by high poverty rates, high unemployment, low provision of public goods, and lack of public transportation, there were differences in the intensity of changes between the destination and origin that I can use to identify which neighborhood characteristics account for displacement effects. Because displaced families could not choose when or where to move and they were required to move to a specific location, this limited potential selection at destination. I provide evidence that displaced families' demographics do not predict the attributes of their destination locations.

I create a novel dataset that follows children and parents from displaced and non-displaced slums 20 to 40 years after the end of the policy. This dataset is constructed from archival records and administrative data. I determine where families were sent, match children with their families, and match individuals with data on employment, labor earnings, school enrollment, and mortality. My final sample contains 19,852 families treated between 1979 and 1985 and observed from 2007 to 2019. The final data comprise 55,343 children and 34,000 parents.

My results show that displacement was detrimental for children aged 0 to 18 at baseline: Compared with the non-displaced, displaced children earned on average 10% less per month across their life cycle. This negative effect on earnings is not associated with lower employment but with the quality of employment: Displaced children were more likely to work in temporary jobs and without a formal contract. In addition, I find that displacement reduced children's educational attainment: A displaced child lost 0.5 years of education and was 12% less likely to graduate from high school relative to a non-displaced child.

I study heterogeneous displacement effects by age at intervention. I find that young children who were 0 to 2 years old at the time of the intervention were the most affected. This group of children faced a more negative effect on earnings and were less likely to attend college compared with the non-displaced. The effect was especially negative for children with formal employment (with a contract). These results are consistent with what previous work has called an *exposure effect* of moving (Chyn, 2018; Chetty et al., 2016; Laliberté, 2021).

I also study heterogeneous displacement effects on children by gender. I do not find differences in children's schooling outcomes, but I do on labor market outcomes: While both displaced men and women suffered earnings losses, displaced women were less likely to be employed than displaced men. Moreover, conditional on employment, women were more likely to be employed in the informal labor market (without a formal contract), and men were more likely to have temporary jobs relative to non-displaced children.

Next, I proceed to study the effects on adults. I find that displacement had long-lasting consequences for the parents of the children in my sample. I find that displaced mothers and fathers died at higher rates than non-displaced. The higher mortality per year was substantial in the first 10 years after the end of the intervention. Twenty percent more mothers die (0.14 percentage point more per year), and 38% more father die compared to non-displaced (0.4 percentage point more per year). Conditional on survival to the year 2007, I find that displaced heads of households were more likely to be employed after retirement age and had lower earnings and lower pensions.

This project contributes to several strands of the literature. First, to the literature that studies slums as a particular type of urban poverty (Marx et al., 2013). Slum clearance and housing upgrading programs were common in developed countries (LaVoice, 2021; Collins and Shester, 2013), and are still common practice in developing countries, where low-income housing is usually built in suburban areas (Dasgupta and Lall, 2009). Prior research on developed countries has mainly focused on the effects of slum clearance on neighborhood quality. In developing contexts, little evidence has been provided for the effects of slum clearance policies on individuals, because most of the literature has focused on property rights (Field, 2007; Franklin, 2020), improvements on site (Galiani et al., 2017), or aggregate effects on urban development (Michaels et al., 2021). Barnhardt et al. (2016) is the most similar to my paper, but they mainly study adults in a small sample. My paper studies the slum dwellers themselves and their children and I follow them in the long-term. In this paper, I am able to shed light on the negative consequences of building public housing in low-quality neighborhoods for individuals' long-term outcomes.

Second, a large literature in economics and sociology studies the role of neighborhoods on individuals' economic outcomes and on intergenerational mobility (Sampson, 2008; Galster, 2012; Ludwig et al., 2013; Chetty et al., 2016; Chetty and Hendren, 2018a; Chyn, 2018; Pinto, 2019; Mogstad and Torsvik, 2021; Chyn and Katz, 2021), with results for children varying by the examined outcome and age and with different results for children and adults.[2] I examine the effects of moving to a poor neighborhood on both children and adults over a longer period of time than many studies. In contrast to previous literature, I find persistent and negative effects for all the individuals in the program, which suggests that not only children but also adults are affected, and the effects are detected in the long term.

The rest of the chapter is organized as follows. Section 2 describes the historical background and eviction policies. Section 3 summarizes the data-collection process, and section 4 presents the empirical framework. Section 5 presents my baseline results on income and schooling, as well as heterogenous displacement effects by demographic groups. Section 6 concludes the chapter.

[2]Mogstad and Torsvik (2021) and Chyn and Katz (2021) conduct extensive literature reviews on neighborhood effects. With respect to mixed results, the Moving to Opportunity (MTO) results show very positive effects on children's earnings and college attendance, and null effects on adults' earnings. Nakamura et al. (2021) find different effects on children and adults, and they attribute the difference to different comparative advantages across groups. Chyn (2018) finds more positive effects on earnings than MTO for all age groups, and the effects are mainly driven by reductions in criminal activity. In addition, two recent papers study neighborhood effects in developing countries: Camacho et al. (2021) for Colombia and Carrillo et al. (2021) for South Africa.

2. Historical Background: Eviction policies

Between 1973 and 1989, when Chile was a dictatorship, the country was characterized by high levels of urban poverty. In the late 1970s, the population of Greater Santiago, the capital, was around 1 million. Thirty percent were considered poor, and of these, 50% lived in a slum (1970 and 1982 Population Censuses).[3] A slum was defined as a squatter settlement without access to drinking water, electricity, or sewage (MINVU, 1979). Slums were located all over the city: Of the 42 municipalities in the metropolitan area, all contained slums regardless of the socioeconomic status of the municipality. The median slum had around 250 families, with an average size of 5.2 persons per family.

From 1979 to 1985, Chile's Ministry of Housing (MINVU) implemented a set of policies aimed at the "eradication" of poverty through social housing for low-income families. The main program in this effort was *the Program for Urban Marginality* (*El Programa para la Marginalidad Urbana* in Spanish), which targeted urban slums with the goal of clearing slums. Proponents of this program believed that the way to end poverty was to house poor families, regardless of the location of new housing units (Murphy, 2015).

The Program for Urban Marginality was a slum clearance and urban renewal program containing two features. First, it aimed to increase the supply of public housing for low-income families by building units where land was cheap. Second, it aimed to provide low-income families with housing in places where they could afford it. With these goals, MINVU implemented two different types of interventions for slum dwellers: Whenever conditions permitted it, families would remain in their original location and their slum would go through an urban renewal process to provide them with housing on site; I call this group the *non-displaced*, or stayers, families. If this was not possible, the slums' residents would be evicted from their original location and families would receive a housing unit in a different location;

[3] Santiago at the time contained 34.8% of Chile's population.

I call this group the *displaced* families. All families in the same slum would receive the same treatment, and all slum dwellers would become homeowners.[4]

At the onset of the program in 1979, the government conducted a census of slums and targeted 340 slums to be cleared.[5] According to Molina (1986) and Morales and Rojas (1986), by 1985 between 40,000 and 50,000 families were involved in the program, accounting for 5% of the population of Greater Santiago at the time.

The features of each intervention are as follows. The *displaced* slums accounted for two-thirds of the total number of families. Displaced families were evicted and moved in groups to public housing projects located in peripheral sectors of the city. Families received a house or an apartment in these new neighborhoods and became the owners of a new housing unit that had a 75% governmental subsidy.

The *non-displaced* slums accounted for one-third of the total number of families in the program. These slums went through a process of urban renewal. In some cases families would get an apartment in projects constructed very close to their original site; in other cases, the slum's land was subdivided among all the residents and families received a "starting-kit unit."[6] These new neighborhoods were provided with all of the basic services (water, electricity, and sewage). To pay for the new units, non-displaced families also received a 75% governmental subsidy.

Families did not participate in the decisions made by authorities; they were not asked what they wanted or which locations they wanted to move to. Instead, displaced families were assigned to destination locations based on the availability of housing projects built in

[4]Both groups of residents were granted property rights to the new housing unit they received, and thus, I abstract from studying the effect of property rights and land security on labor market outcomes. A good example of the effects of granting property rights to slum dwellers on labor force participation is Field (2007).

[5]Some other evictions took place between 1976 and 1978 and are considered a precedent for this massive eviction program. They were called *Operación Confraternidad I, II and III.* These were forced evictions that were politically motivated, and hence I do not include them in my analysis. More information can be found in Celedón (2019).

[6]A starting kit consisted of a living room, a bathroom, and a kitchen; hence, families could build bedrooms on top of the unit, which looked more like a house.

Santiago during this period.[7] The new public housing units were mainly located in peripheral municipalities, where land was cheaper. Many lacked access to public transportation and public goods and services, such as schools and health care centers; many were rural areas that were transformed into public housing projects. These places were not prepared to receive the large number of displaced families involved in this program (Molina, 1986; Aldunate et al., 1987). The cost of the program for the government was low: The average housing unit cost US$7,700 and the average total annual cost of the program was US$34 million, which was about 0.2% of Chilean GDP at the time.

The decision to clear a slum stemmed from a variety of circumstances that prevented slum families from staying in their original locations. These circumstances ranged from slums' being too close to freeways or being on a riverbank—especially the Mapocho River which had high risk of flooding during winter months. Other circumstances were related to features of the land itself, such as public versus private property, the density of a slum (number of families per site), and potential difficulties for the provision of sewage, water, and electricity. Land value also mattered, as Rodríguez and Icaza (1998) explain "...Other criteria included the reputation of the municipality of origin, their land values, and the speculation about future prices."

A well-documented example of how the decision to displace a slum was made is presented by Murphy (2015) for Las Palmeras, a slum located in a low-income municipality. Originally, MINVU's official plan was to create a new neighborhood for families on the original location. However, by 1981 the high density of Las Palmeras made it impossible to reallocate plots inside the slum in a manner consistent with the size of regular housing units. Thus the necessary reductions in size of some plots caused authorities to include Las Palmeras among the slums to be displaced. In late 1983, residents were moved to a new neighborhood built

[7] I interviewed social workers who accompanied families during the eviction processes and asked them how the new locations were determined. In most of the cases they reported that it depended on which public housing projects were available to receive families at each point in time.

on the outskirts of the municipality, and the former slum became a park.

Figure 1.1 plots the urban limits of Greater Santiago and its municipalities. Panel (a) depicts the location of slums in 1979 and shows that slums were located everywhere without a particular concentration in any municipality. Panel (b) shows the location of the housing projects built to receive slum dwellers in 1985. Neighborhoods where housing projects for the displaced were built are purple, and housing projects for the non-displaced are light blue. Two important conclusions can be drawn from this figure. First, the new housing projects were disproportionately built in the peripheral areas of the city; second, public housing projects were farther from job opportunities (in gray scale), and projects for the displaced were even farther from jobs than projects for the non-displaced (this will be discussed in more detailed in Section 5).

After the program ended in 1985, Aldunate et al. (1987) evaluated the program by surveying 592 families that were displaced in 1983. The families in their sample reported liking their home better, but the quality of the new neighborhoods was worse than the slums in several respects: they had fewer job market opportunities and it was harder to access transportation, education, and health care services. The families perceived their new neighborhoods as lacking public services and to be more dangerous (I report a summary of these results in Appendix Figure B.2a).

3. Data

I study the causal effects of the program on children's and families' outcomes by constructing a novel dataset that tracks parents and their children, their slum of origin, and their destination neighborhood. I match these individual records to administrative data from Chilean agencies such as the Ministry of Education, the Ministry of Social Development, and the Superintendency of Social Security. In this section I summarize the data-collection process.The full data collection and linking processes are described in Chapter 3.

3.1 Archival data: Homeowners

I digitized two slum censuses conducted by Chile's Ministry of Housing (MINVU) in 1979 and 1984 that contain information on slums' names and destination projects. With this information, I classify each slum as displaced or non-displaced and the final destination of the displaced. I complement the slums census data with information collected by Molina (1986) and Morales and Rojas (1986), who compiled a full list of slums and their locations, the number of families evicted in each episode, and their destination neighborhood by year.

The next step in my data-collection process was to find the families in the program. I collected and digitized archival data from the Regional Housing and Urban Planning Service, which administered the program in the Metropolitan Region (Greater Santiago), and historical records kept by the Municipality of Santiago.[8] These records correspond to the lists of homeowners and their spouses who received a property deed in the context of the Program for Urban Marginality. I collected data for 22,689 unique recipients of social housing, representing 56% of the total number of recipients (according to the numbers in Molina (1986)).[9]

The archival data contain information of the recipient on the property deed (head of the household) and their spouse, their full names, their national identification numbers (NID) and the address of their new housing unit. These records are grouped by year of eviction/urban

[8]Each region of Chile (equivalent to a state) has an Urban Development and Housing Service, which is dependent on the National Ministry of Housing and Urban Development. These agencies administer and implement housing policies at the local level.

[9]I was not able to find all of the records for two reasons. First, since the program was administered by individual municipalities, many of the records were kept by the municipalities and were not sent to the central administration. During the dictatorship there was an order in place that allowed municipalities to keep administrative records for only 5 years; after that, municipalities were allowed to destroy them. This has been confirmed by several municipalities. The second reason is that some of the records in the Chilean National Archives were lost during a flood at the beginning of the 1990s, and thus it could be that some of the data I were looking for were lost during that incident. More details are presented in Chapter 3.

renewal and project of destination; hence, I matched them to their slum of origin using the slums censuses of 1979 and 1984. My matched sample contains 19,852 recipients with a valid National ID number. I use this variable to match adults to their children.

Table 1.1 summarizes the numbers from the original program (panel A), my archival records (panel B), and my matched sample (panel C). Compared with the total program, I was more likely to find the displaced slums (70% versus 65%), and I was also more likely to find bigger slums. In Appendix Table A.1 I show this in a regression format. Also, these results show that the probability of a slum's being found in the archives is not a function of the characteristics of the municipalities of origin.[10]

When I compare the archival records with the matched sample (panels B and C), the fraction of displaced families increases from 70% to 72.5%. I lose one slum, but no destination projects, which indicates that I am losing displaced families across all destination neighborhoods. The reason I lose people from panel B to C is mainly due to individuals without a valid national ID, because they were mistakes or they had an older version. Thus, I was not able to validate them using contemporaneous data.[11] Missing national ID numbers were more common for older people or those who did not report having a spouse. Hence, in my matched data I am more likely to observe younger heads of households and married individuals.

3.2 Matching process: Children sample

The second stage in the construction of a full database consisted of finding the children of each family. I did not have access to administrative data on family composition at the time of the intervention, and thus I constructed these records. I worked with Genealog Chile

[10] In the robustness check section, I use these estimates to re-weight my baseline results. In Chapter 3 I discuss how this type of selection might impact my results.

[11] I used data from Chilean Electoral records in 2016 to validate full names and NID numbers. After the data were validated, I searched for people's birth certificates.

and web-scraped birth and marriage certificates from Chile's National Civil Registration, and collected birth certificates for the population 18 or older in 2016. The birth certificates contain full name at birth, date of birth, National ID number, and parents' full names. I matched homeowners' archival data with their children using their NID. If the birth certificate did not contain at least one parent's NID, I matched using a first name, a middle name, and two last names.[12]

I found 55,343 children of 17,651 unique families (2,201 families did not have a child). Of these, 37,889 individuals are children aged 0 to 18 at the time of the intervention. This is my estimation sample. Because of attrition due to the loss of NID numbers (panels B and C in Table 1.1), it is likely that in my matched sample younger children will be overrepresented, because I am losing the oldest heads of households.

3.3 Measuring outcomes: Matching to administrative data

I match children and parents to several administrative data sources using NID numbers. The first source of data is from the *Social Household Registry*, or the RSH (*Registro Social de Hogares* in Spanish), which is an information system managed by the Ministry of Social Development. The RSH is used to provide information on a family's needs and use of social and governmental benefits for income, housing, and education. Approximately 70% of all Chilean households are in this system and voluntarily register to be in the RSH. I have access to biannual data from June 2007 to December 2019, and I observe self-reported income, employment status, and schooling, as well as family composition and dwelling characteristics.

The second source of administrative data is a system called the GRIS Mutuales. The

[12]In most Spanish-speaking countries, people have two last names. The first last name of a child (in order from left to right) corresponds to the first last name of the father, while the second last name is the first last name of the mother. Hence, both paternal last names from the parents are transmitted to their children; for example, assume that María Pérez Rojas (mother) has a child with Juan Rodríguez González (father). Their child will have "Rodríguez Pérez" as the family name. See the Appendix for a full explanation of the process.

GRIS is an information system managed by Chile's Superintendency of Social Security. This system collects data on all workers in the formal sector who contribute to social security each month. Hence any worker with a contract is in this database. I observe monthly data on taxable income starting from July 2016 until December 2019.

The last source of data I have access to is enrollment in the school system from 2002 to 2019. This is provided by Chile's Ministry of Education and corresponds to individual data on all students enrolled in K-12 in a given year in each school. Since only a small portion of the sample of children I am interested in matches with this sample because of their age, I will use it mainly to validate the self-reported years of schooling I observe in RSH data for the youngest children in my sample.[13]

3.4 Municipality and district attributes

I measure location attributes such as education and employment by municipality and by census district, which come from the Census of Population of 1982, in which I observe variables such as years of education and employment status. I combine these measures with historical records from the Ministry of Education and the Ministry of Health in 1985 or earlier on schools, hospitals, and family health care centers. In addition, I have information on subway stations built in Santiago at each point in time and their geolocations; these are publicly available from Greater Santiago's subway system. Finally, I collect information on waiting times for public transportation at the municipality level from the historical records of Santiago's Origin-Destination Surveys conducted in 1977 and 1991.

[13] In this data, school attendance is capped at 12 years of education. The correlation between self-reported years of schooling and school enrollment is 0.7.

In my estimation sample, I keep all children who were at least 18 years old at the time of income/employment measurement. In Table 1.2 I present summary statistics of the children in my full sample at the time of the intervention (column 1). I observe that 71% of children come from families that were displaced. Half are females, and the average age is 8.18 years at the time the intervention took place. They have three siblings on average at the time of the intervention, and 37% are first-born. Their parents are 34.9 years old on average at baseline, 30% come from a female-headed household, and 81% have parents who were married at the time of the intervention. Only 0.5% of the total number of children in my baseline sample died before 2007.

I find 82% of my baseline sample at least once in the RSH (column 2) and 67% at least once in the GRIS (column 3). In the RSH I matched slightly more displaced children, with a share of 72%, and in the GRIS I matched slightly fewer displaced children, with a share of 70%.

In the last two columns of the table I regress the probability of being found in each of the two datasets on a set of demographic characteristics that observe at baseline. Two demographic variables are critical for matching: Age and gender. Age is determined by data availability; as it can be seen in the table, the newer the data the less likely I am to match with older children. For gender, I find that females are overrepresented in the RSH and underrepresented in the GRIS. This is consistent with the fact that women are more likely to be in the lower part of the income distribution and are also more likely to request social benefits. Thus, I expect to find more women in the RSH than in the GRIS. Since in Chile female labor force participation is only 45%, it is not surprising that fewer women are in the GRIS. Also not surprisingly, I do not find children who died, but deaths are too rare to account for all non-matched individuals.

These summary statistics, combined with the attrition rates from the archives, imply that my matched RSH sample of children corresponds to 40% of the total number of children in the program (0.815*0.49). In this group, children who were displaced, young, or female are overrepresented. The fact that I find more females and younger children will bias my estimates only if these characteristics are not balanced between the displaced and non-displaced, or if they affect the displaced and non-displaced differentially. In the next section I show that this is not the case.

Finally, not surprisingly, I conclude that the individuals in my sample are poor. They have lower income than the universe of individuals in the RSH (see Figure A.3). In 2018 the population in the RSH reports a median monthly salary of CLP$183.998 ($\sim$ US$250), and the median monthly salary in my sample is even lower CLP$178.855 ($\sim$ US$240). These numbers are low compared with estimates for the full Chilean population, since the median monthly salary for a Chilean worker in 2018 is CLP$450,000 ($\sim$ US$600), which is almost three times larger than the numbers in RSH.[14]

4. Empirical Strategy

4.1 Identification of a displacement effect

To estimate the impact of forced displacement on children, I exploit the fact that within the same municipality, certain slums were chosen for eviction while others were not. Thus the empirical strategy I adopt is to compare the children of displaced families with children of non-displaced families conditional on the municipality of origin. Since the process of choosing slums into displaced and non-displaced did not depend on households' characteristics but

[14]This discrepancy between national estimates and the RSH data occurs for two reasons: Underreporting (the income data I use is self-reported) and a higher proportion of informality in the RSH compared with the rest of the population. In my sample period, around 70% of the total Chilean population is registered in the RSH and they report higher informality compared with the full labor force. Informality pre-Covid in Chile was around 20% (CASEN, 2017), while in the RSH 40% of adults report to work without a contract.

instead on the feasibility of renewal on site, non-displaced children serve as a comparison group for the displaced *within* the same municipality. Any differences between children in the displaced group and the non-displaced group are attributed to the eviction process and subsequent relocation to a new project.

I estimate a linear model to study the impact of the displacements on children using the following specification:

$$Y_{it} = \alpha + \beta Displaced_{s\{i\}} + \psi_o + \psi_\tau + X'_{it}\theta + \varepsilon_{it}, \tag{1.1}$$

where Y_{it} is current outcome for individual i at time t, such as labor income, employment status, or years of schooling; $s(i)$ indexes the slum of origin for individual i's family. The variable $Displaced_{s\{i\}}$ takes the value 1 if an individual's family lived in a displaced slum and 0 otherwise. ψ_o are municipality of origin fixed effects that control for any initial differences between families living in slums located in different municipalities, such as access to public services or higher-quality neighborhoods. I add ψ_τ, year of intervention fixed effects (1979 to 1985), to control for aggregate temporal differences between the 6 years this housing program was in effect. For precision, I add baseline controls for individual and familiy characteristics, such as gender, child's year of birth, female head of household, married head of household, head of household's age, indigenous last name, and birth-order dummies. When the outcome is income or employment, I include semester fixed effects to account for common temporal shocks across individuals.

The treatment was at the slum level; however, within the same municipality, displaced and non-displaced slums could have been subject to common shocks or similar social policies. Thus, to account for any potential correlation between slum residents with the same origin, I cluster standard errors at the level of municipality of origin.[15]

[15] I compute other clustering, such as clustering by slum or Conley standard errors. I discuss these in

4.2 Comparing displaced and non-displaced children at baseline

The validity of my research design depends on whether the decision to displace a slum was uncorrelated with the characteristics of the families living in the slums conditional on their origin. Under the assumption that conditional on municipality of origin (ψ_o), the covariance between $Displaced_{s\{i\}}$ and ε_{it} is 0, the coefficient β estimates the causal effect of the displacement on children's outcomes. To provide support for this assumption, I compare the demographics of the displaced and non-displaced children at the time of the intervention (baseline).

In Table 1.3 the first column reports means for several demographics for the non-displaced. Column (2) shows that conditional on ψ_o, there are no statistical differences between both groups for 9 out of 11 observables, but displaced children come from families in which the head of household is less likely to be married (7% less) and are more likely to come from a household with an indigenous last name (Mapuche Head of Household).[16] This last difference is sizable relative to the non-displaced (0.02/0.05); however, the share of the population I identify as indigenous is small relative to my full sample (only 5%). Hence I do not expect this variable to determine the displacement effect.[17]

As a measure of families' socioeconomic status, I measure mothers' years of schooling. This variable is only available for children whose parents I find in the RSH data (70% of my full sample of children). Hence the variable might be subject to selection, especially if displacement impacts the likelihood of finding these mothers in the data.[18] That is why whenever I use this variable, I take a control variable approach and include an estimate

more detail when I present my baseline results.

[16]See the Data Appendix for variables definitions

[17]In Appendix Table A.2 I compute these differences for the adults in my sample, and the differences across demographics are the same.

[18]This is exactly the case, because displaced mothers are more likely to die than non-displaced mothers before 2007. Thus this can widen the difference between the two groups. Later in the paper I show the results on parents' mortality.

of the likelihood of finding a mother in the RSH.[19] As expected, the children in my sample have mothers with very low education: On average, non-displaced mothers have 6.25 years of education. Displaced mothers have 0.28 fewer years of schooling relative to the non-displaced, but this coefficient is small and not statistically different from 0.

The results are very similar for the children I matched to the RSH (columns 3 and 4), and for the children I matched to the GRIS (columns 5 and 6). This confirms that attrition by gender and age is not different between displaced and non-displaced children.

Overall, I conclude that these two groups are very similar in their observables conditional on municipality of origin. However, a concern arises because I do not have a measure of household income at the time of intervention.[20] I claim that part of the variation is already captured by the municipalities of origin, since they were highly homogeneous units by socioeconomic status at the time the program took place.[21] To provide evidence for this claim, in Table A.1 I report the share of the variance of income and schooling that can be attributed to variation within municipalities in several sources of data. The share varies from 21% to 28%. Thus at least one-fourth of the variation in outcomes is captured by the municipalities of origin, which reduces the concerns on potential bias in my estimates.

4.3 Slums' characteristics and location attributes before and after the intervention

Even though children and families are observationally equivalent in their demographic characteristics it is possible that their slums and neighborhoods were very different before the intervention. In this subsection I provide evidence to refute this claim.

In Panel A of Table 1.4 I compare slums' characteristics. Within municipalities, the slums

[19] See the Data Appendix for details on construction of the demographic variables.

[20] These data exist, but the Ministry of Development does not share this information with researchers.

[21] A municipality is a subunit of Greater Santiago. On average, a municipality has a population of 200,000, and its extension can vary between 20 km2 and more than 100 km2, so, it is smaller than a county in the US. A municipality is a geographical and political unit, and each municipality has an elected mayor. During the dictatorship, however, mayors were appointed by the central government and not through elections.

in my sample were located in plots of similar size (area) but the displaced slums had more families, which indicates that denser slums (more families per hectare) were more likely to be displaced, This is consistent with the historical evidence presented in Section 2, but the difference is not statistically different from 0.

In Panel B of Table 1.4 I compare the characteristics of the census districts in which the original slums were located. I compute the average for several location attributes at the census-district level, which corresponds to a smaller geographic level than a municipality. Conditional on municipality of origin, displaced and non-displaced families lived in neighborhoods that were very similar to each other. The only sizable difference is the number of schools per student, which is not statistically significant.[22]

Finally, I look at neighborhood characteristics after the intervention (columns 4 and 5). Displaced families were moved to places in which the population had lower levels of schooling, higher levels of unemployment, and higher high school dropout rates. There were fewer schools in total and per student, families ended up farther from public transportation, and they had longer commuting times. These results are consistent with evidence provided by Aldunate et al. (1987).

4.4 Displaced families' characteristics do not predict new location attributes

My identification strategy relies on the idea that displaced and non-displaced families were quasi-randomly selected for eviction. However, since families did not choose to move to a particular location, a concern arises that certain types of families were systematically sent to worse locations, which could potentially explain any negative displacement effect I find in my sample. Qualitative evidence from interviews with social workers who worked with

[22] As opposed to the US context, in Chile students are not mandated to go to schools within their neighborhood because parents can choose any school in the city regardless of where they live. Even though it is likely that children go to the school in their neighborhood, the likelihood decreases with the child's age, especially for high schoolers. Meneses (2021) documents this using contemporaneous data.

the families in the eviction process cause me to believe that the assignment was as good as random. According to them, the Ministry of Housing would choose the location of the new housing projects, and assignment to destinations was mainly based on the availability of units.

To provide statistical evidence for the assumption that there is no selection on observables in the displaced group, I test whether families' demographics predict destination location attributes. I run a regression of several location attributes on a set of families' demographics at the time of the intervention in the sample of displaced families. My results are reported in Tables A.4 and A.5. I report the F-test of joint significance of baseline controls and its corresponding p-value: For 8 out of 9 different location attributes, I do not reject the null of joint significance of controls. I interpret these results as evidence of the new locations' being quasi-randomly assigned to displaced families.

5. Results

In this section I provide evidence of the existence of a negative displacement effect for children aged 0 to 18 at baseline and on other family members: children born after the intervention and parents.

5.1 Displacement effect on earnings and schooling

Labor market outcomes

I start my analysis by looking at the earnings and employment of individuals aged 0 to 18 at baseline, who are 18 to 60 at the time their income is measured, with non-missing education. The main outcome for earnings is self-reported labor income in the RSH for which I have many observations. This measures income from both formal and informal employment and includes wage income and proprietors' labor income, but excludes pensions and transfers. Earnings are measured in 1,000 Chilean pesos per month (CLP$1,000/month).[23] Employment is reported in the RSH and includes both formal and informal employment.

Table 1.5 shows negative effects of the displacement on earnings (panel A) and null effects on employment (panel B). In column (1) I report the difference in earnings and employment between displaced and non-displaced children conditional on the municipality of origin, and column (2) includes baseline controls for precision. This column indicates that displaced children have lower earnings compared with the non-displaced: The coefficient of -15.314 in column (2) panel A is statistically significant at 5%. This means that displaced children earn 9.9% less than the non-displaced on average per month (see row labeled "% Variation w.r.t non-displaced"). In contrast, panel B shows no effect on employment. Column (3) is equivalent to (2), but it only includes children whose mothers are in the RSH data, and column (4) controls for mother's years of education (which is only available for children whose mothers I found in the RSH data). The coefficients on earnings and employment are almost unchanged with the inclusion of this variable. Thus my baseline results correspond to column (2).

For comparison, in Table 1.5, I report Conley standard errors in brackets (Conley, 1999) to take into account any spatial dependence across slums.[24] This shows that the municipality of origin is a conservative measure of clustering. Thus in all of the following estimations I report clustered standard errors by municipality of origin.[25]

I estimate a displacement effect on children's earnings across the age cycle (Figure A.4). Across the whole age distribution the income trajectories of displaced children are below

[23]CLP$1,000 corresponds to approximately US$1.5 in 2018.

[24]I compute Conley standard errors for all regressions at the cutoff distance of 14 km. I choose 14 km because it is the distance that maximizes standard errors for my main outcomes, as shown in Table D.2. I estimate the standard errors at different cutoffs between 2 km and 14 km. I limit the upper bound to 14 km because a cutoff of 14 km would include the largest municipality in Santiago, measured in square kilometers.

[25]Another option is to cluster standard errors at the level of intervention by the slum; however, clustering by slum does not take into account the potential correlation between slums within the same municipality. As a robustness check I compute clustered standard errors by slum in my baseline regressions and find that the standard errors are smaller than clustering by municipality. Results can be found in Appendix Table D.2.

those of the non-displaced. The difference is statistically different from zero for almost the whole age distribution (right panel). This difference increases after the age of 27, when most people have finished their schooling. In Appendix Figure A.4 I show this pattern is the same for all cohort groups, and the effects range from -10% to -5%. According to CASEN (2017) this is equivalent to 1 fewer years of schooling.[26]

Displaced children's lower earnings are related to higher informality rather than lower employment. Table 1.6 shows that displaced children are 4.1 percentage points less likely to work with a contract (column 3), which is equivalent to a 10% lower probability relative to non-displaced children. They are also 4 percentage points more likely to work in temporary jobs (column 4), which is 7% more than non-displaced children.

In column (5) of Table 1.6 I estimate a displacement effect on taxable income, which is only available for individuals who contribute to social security; thus these are the earnings of workers with formal jobs. I find a bigger displacement effect in magnitude (displaced children earn CLP$37.463 less per month), but smaller in relative terms (-6.4%). Compared with column (1) this number is smaller. To see whether this last result is consistent with my self-reported measure of earnings, I split labor income into formal earnings (the individual works with a contract) and informal earnings (the individual works without a contract). I estimate a displacement effect on these measures in columns (6) and (7): The negative displacement effect is mainly due to lower earnings in the formal labor market, since the percentage variation with respect to the non-displaced is bigger in magnitude and equal to -14.7%, while the effect on informal earnings is positive, small, and not statistically different from zero.

I find differences by gender in earnings and employment (Appendix Table A.10 panels B and C). Both men and women have lower earnings relative to non-displaced children, but women face a bigger displacement effect in relative terms. Women are more likely to work

[26]CASEN stands for "Encuesta de Caracterización Socioeconómica." CASEN is similar to the Current Population Survey (CPS) in the US.

without a contract, and men are more likely to have temporary jobs relative to non-displaced men.

Finally, I find that the magnitude of the displacement effect varies across the income distribution in levels. It becomes more negative the higher the income (also found in Nakamura et al., 2021). In relative terms, however, the displacement effects are constant and vary between -10% and -5% (Figure A.7).

Educational outcomes

I now examine how displacement affected the educational outcomes of children and discuss how much of this effect can account for the negative effect on earnings.

My estimates show that displacement had negative impacts on the educational outcomes of children. Column (2) of Table 1.7 indicates that displaced children have 0.64 fewer years of schooling than the non-displaced. In column (3) I look at the sample of children whose mothers are in the RSH data, and in column (4) I control for mother's education. Including mother's education reduces the magnitude of the coefficient from 0.586 to 0.473 (columns 3 and 4). The coefficient of -0.473 corresponds to 4.1% less schooling relative to the non-displaced, which is still sizable and significant. In contrast to what happened with earnings, including mother's education as a control reduces the magnitude of the displacement estimate. Hence, in all of my schooling outcomes I will include this covariate.

I find that the displacement effect on high school graduation and college attendance are more negative than on years of schooling. The results indicate that displaced children are 12% less likely to graduate from high school, 18.3% less likely to attend a 2-year college (technical degree, such as mechanic, electrician), and 25% less likely to attend a 5-year college (professional degree such as medicine, engineer, economics, etc.) relative to the non-displaced. Overall, my results suggest that displacement affected children's educational attainment by reducing their likelihood of getting their high school diploma.

I do not find gender differences across educational outcomes (A.11), except that displacement effects are more negative for women than men for college attendance.

The negative effect on years of education can explain around half of the negative effect on earnings I find in my sample. According to CASEN (2017), 1 extra year of education for the population that finishes high school increases earnings by about 10%. My displacement effect on earnings is -9.9%, while the effect on education is -0.47 years of education. Hence the decrease in years of schooling accounts for about half of the total effect on earnings.[27]

5.2 *Robustness checks*

I perform several robustness checks. A first concern arises if the negative displacement I find in my data is driven by slums' characteristics at origin and is not a result of the forced movement itself. In section 5.3 I showed that within municipalities of origin, displaced and non-displaced slums were very similar in terms of land size and their proximity to rivers, but were denser (they had more families). To rule out that the displacement effect is related to a slum's higher density or any slums' characteristic, I show that these characteristics do not predict the outcomes of the non-displaced children in my sample (Appendix Table D.3). Another option is to control for these characteristics in my baseline regressions. I find that the inclusion of slums' characteristics does not change the displacement effect.

A second concern is related to the fact that non-displaced families and their children saw an improvement in their neighborhoods, especially in richer municipalities after the expulsion of low-income families.[28] Hence, the negative displacement effect I find might not be a negative effect on the displaced but a positive effect on the comparison group that

[27] I repeat this exercise using a mediation analysis and my results are very similar: The decrease in schooling explains 55% of the displacement effect on earnings. I estimate a mediation analysis in which the treatment is the displacement, the outcome is earnings, and the mediator is years of schooling. My results indicate that 55% of the total effect on earnings is mediated by the reduction in years of schooling relative to non-displaced children.

[28] The places in which slums were originally located were used to build parks or new public goods, especially in municipalities that collected higher revenue.

became better off due to improvements in their neighborhoods. To test for this hypothesis, I drop from my sample the richest municipalities that were also net expellers (i.e. they expelled more families than they received). By doing this I do not find evidence of a displacement effect's being driven by improvements for the comparison group (Appendix Table D.4).

Third, I check whether differential attrition due to selection from the National Archives or from matching to administrative data could bias my results. To do this, I estimate the probability of being found in the archives as a function of slums' characteristics and municipalities of origin by combining data from the Slum Censuses of 1979 and 1984 (column 5 in Table A.1). I include estimates of the propensity of being found as a polynomial in my baseline regressions (see Appendix Table D.5). I do not find evidence of differential attrition driving my results. If anything, correcting for attrition leads to a bigger displacement effect in magnitude.

Finally, in the previous sections I provided evidence of no selection on observables. However, some concerns arise if the demographic variables I are measuring do not account for all of the selection types in my sample. For example, I do not observe other characteristics of slum dwellers at baseline, such as their relationship with local authorities or the difficulties each slum's residents might have had when they left their original location.

Unobserved characteristics could potentially lead to biased estimates of the displacement effect if the unobserved characteristics determined selection into treatment and/or the new locations of the displaced families. To account for the degree of selection of unobservables in my setting, I follow Oster's (2019) procedure. I would need a huge degree of selection on unobservables relative to the baseline controls—even bigger than what Oster (2019) suggests—to conclude that my displacement effects on earnings and schooling are zero or even positive (see Appendix 3.1).

5.3 Heterogeneous displacement effects

In this subsection I explore whether displacement had differential impacts on different demographic groups.

Displacement effect by age at intervention

The effects of the displacement may vary by age at the intervention, as has been shown in previous settings (Chetty et al., 2016; Chyn, 2018; Laliberté, 2020; Nakamura et al., 2021). This pattern has been called a *childhood exposure effect* of neighborhoods, which means that the longer a child spends in a new environment the larger the neighborhood effect is expected to be. This implies that younger children are more affected than teenagers.

I test whether the displacement effect varies by age at baseline. To do so, I stratify the displacement dummy in equation (1) by age at intervention into four groups: 0 to 2; 3 to 7; 8 to 12; and 13 to 18. I chose these four groups after performing a structural break at each age from 0 to 18 to test whether there is a change in the slope at each single age. F-tests show two clear breaks on labor earnings at age 3 and between ages 6 and 9 (depending on the definition used for labor income).[29]

I find some evidence of an exposure effect on earnings but not on employment (Figure 1.4). The youngest children in my sample face a more negative displacement effect on earnings, but I am not able to reject that the coefficients are different across age groups. However, I do find more negative effects on taxable income for the youngest children. This measure is only available for children with formal earnings.

On schooling outcomes I find mixed results, which suggests the existence of a disruption effect for adolescents. The results in panel (a) of Figure 1.5 do not show an exposure effect on years of schooling, because children of all ages face a displacement effect of similar

[29] See Appendix A.6 for estimates of the structural breaks.

magnitude (~ 0.5 years). However, on high school graduation and college attendance I observe differences by age. On the one hand, for high school graduation I find the opposite of an exposure effect because older children face a more negative effect. This result could be attributed to a disruption effect is having a larger effect in magnitude on these children. On the other hand, the reduction in years of schooling is particularly pervasive for very young children who are less likely to attend college (right panel of b), where I do see a differential effect by age.

I interpret these results for schooling outcomes as a cohort effect: Younger children are more likely to go to college, and since the baseline is bigger for the youngest group in the non-displaced, displacement is preventing older children from finishing high school, and the youngest children from attending college. This last result is consistent with the finding of a more negative effect on formal labor earnings for the youngest group of children (0 to 2 years old at baseline).

Displacement effect by demographic groups

I find gender differences in employment (first panel in Figure 1.3). Women are less likely to be employed (not significant) and men are more likely to be employed as a consequence of the displacement. However, this higher employment does not translate into higher labor earnings for boys. This is consistent with results whereby men on average are more likely to have temporary jobs, which might pay lower wages (Table A.10).

I also find that children of single mothers are less likely to be employed, without differences in earnings across groups. In general, in earnings and years of schooling, I do not find important differences between demographic groups. However, children in families with indigenous last names experienced a more negative displacement effect on the three outcomes I analyze. This is not surprising, because in the Chilean population, indigenous individuals are poorer on average than the rest of the population. However, standard errors are large due to the small proportion of children in my sample who are identified as indigenous (only 5%). Thus it is not always possible to reject the hypothesis that coefficients between groups are equal to each other.

5.4 Displacement effects on other family members

Children born after the intervention. I find a displacement effect on children born 1 to 5 years after their families became homeowners. The treatment these children received is different from that of children in my main sample: They were born in the new neighborhoods and their families were already treated. They did not suffer a disruption effect. The resulting differences in this group of children can be attributed to differences in their environments, both at the family level (because households became relatively poorer) and at the neighborhood level, because these children had access to lower-quality neighborhoods on average. There is also a concern about the selection of families that decided to have a child, because they could have adjusted their fertility decisions as a consequence of the displacement. Hence, I interpret the results for this group of children as suggestive.

Children born to displaced families have lower income—but only those with formal earnings (Table E.3 column 3)—and the coefficient is not significant at the 10% level. They also have lower schooling: Displaced children have 0.48 fewer years of education and a 6.4% lower probability of finishing high school relative to children from non-displaced families, with null effects on college attendance.

I examine whether these results are related to lower school attendance. Because most of these children were born after 1983, I can match them to school enrollment data from Chile's Ministry of Education that starts in 2002. In this data I observe each student in the education system at each grade to study grade progression and attendance. Children born to displaced families attend school less after age 16 (Figure E.2). They are also more likely to be old for their corresponding grade. I take these results as suggestive evidence of children's having access to lower-quality schooling, which translates into a lower probability of finishing high school.

Parents. I study the long-term labor market outcomes and mortality of parents of the children in my main sample. In ongoing research, I study the adult population in the Program for Urban Marginality and consider women and men separately.[30] In this section I summarize my findings for the population with children.

To estimate the effects on mortality, I collected the death certificates of mothers and fathers from 1985 to 2019. I estimate the relationship between annual mortality and displacement by estimating the following equation separately for men and women. I include observations for each individual i at time t from 1985 through 2019 and omit any observation after the year the individual dies (Deryugina and Molitor, 2020). I estimate

$$Died_{it} = \sum_{\tau \neq 1985}^{2019} \beta_\tau 1(t=\tau) \cdot Displaced_{s\{i\}} + X_i'\theta + \psi_o + \gamma_t + \varepsilon_{it}, \tag{1.2}$$

where $Died_{it}$ is equal to zero if individual i survived through year t and 1 the year the individual dies. Controls in X_{it} include the number of children at baseline, year of birth fixed effects, Mapuche last name, marital status, and year of intervention fixed effects.

I plot β_τ coefficients from 1986 to 2014 in E.1.[31] The results show that both displaced mothers and fathers have higher annual mortality, especially in the first 10 years after the end of the intervention. In Table E.1 I summarize the estimates in panel B. Per year, 11.7% more mothers die per year (but this is not significant), which corresponds to 2.4 percentage points more after 35 years relative to non-displaced mothers. Thirty-nine percent more fathers die per year, which corresponds to 14.1 percentage points more after 35 years relative to

[30]This is a work in progress.
[31]I plot the coefficients from 1985 to 2004 to make the exposition clearer.

non-displaced fathers.[32]

Second, I look at the displacement effect on labor market outcomes of the parents I matched to the RSH data (Table E.2).[33] Conditional on survival until 2007, displaced heads of households are 5.9 percentage points more likely to be employed (15% more) compared with non-displaced heads of households, even after age 65 (19% more). This higher employment does not translate into higher income, because displaced adults have lower earnings (12% lower) mainly due to lower retirement income (34% lower). This is indicative of fewer formal jobs or of low-quality jobs across their lifetime working cycles.

6. Conclusions

Slum dwellers represent more than 25% of the urban population in developing countries. According to the NGO TECHO (2020), just before the Covid pandemic 104 million people in Latin America lived in slums, and that number increased after 2020.[34] Slum demolitions and urban renewal programs were common policies in several developed countries in the past (Collins and Shester, 2013), and they are still implemented in the developing world (Marx et al., 2013).

This dissertation presents new evidence on the long-term consequences of being displaced and growing up in a low-quality neighborhood. I construct a novel data set that combines archival records and administrative data on a large proportion of the families that participated in the largest evictions program in the history of Chile. This policy forced the relocation of more than 50,000 families and more than 170,000 people, accounting for 5% of

[32]I am currently working on an extension of this project in which I look at adults only. Preliminary evidence suggests that displaced men are more likely to suffer from alcoholism and are more likely to die violent deaths, but this last result is noisy and not statistically significant.

[33]I am able to match 70% of the heads of household in my sample to the RSH. As opposed to children, displaced parents are less likely to be found in the RSH relative to non-displaced. This difference is mainly due to the higher mortality of displaced parents. Hence I can identify the source of attrition for this group and correct for it. Here I report the non-corrected results as conservative estimates of the displacement effect on parents (lower bound on mortality effect).

[34]This number is even bigger in African cities (UN-Habitat, 2020).

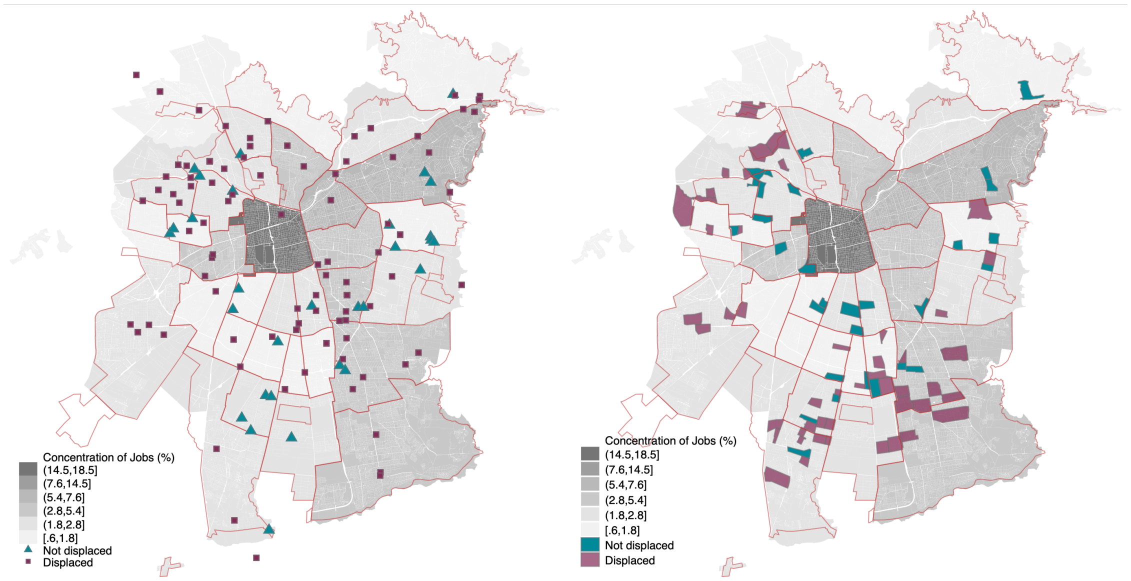
the total population of Greater Santiago.

I exploit a policy that forced poor families living in slums located all over the city to be relocated to low-income areas. Regardless of the fact that families received a housing unit in the new location, the new projects were built in places that lacked access to public goods and were very disconnected from their original locations. Many of the families in the program reported having lost their jobs as a consequence of the displacement. My results show that displaced children have 0.5 fewer years of education relative to non-displaced, they earn 10% less income, and are 10% more likely to work in the informal labor market.

In the next chapter, I study the mechanisms that drive the negative displacement effects found in my sample.

7. Figures and Tables

Figure 1.1: Eviction Policies 1979-1985: Location of families living in slums



(a) Slums location before 1979 **(b)** Public Housing Projects after 1985

Notes: Red lines represent the urban limits of Greater Santiago and its municipalities. Municipalities are colored in gray scale to depict the concentration of job across the city. These two figures show the change in the location of families living in slums in 1979 (panel (a)) and their final destination in 1985 (panel (b)). Purple squares represent families living in slums that were moved out from their original location to a new neighborhood, while blue triangles represent the slums that were not evicted but received a housing unit in their original location. The goal of this figure is to show how the dispersion of the location of these families decreases and, how families are relocated to the periphery of the city after the policy. For context, consider that the richest districts of Santiago by that time (and today) are the ones located in the North-East of this map, while the poorer districts are located in the South and North-West of the city, which is exactly where the new public housing projects were built. The data to construct this map come from MINVU (1979), Molina (1986), FLACSO (1982, 1986), and Population Censuses of 1982 and 1992.

Table 1.1: Archival Data 1976-1985

Treatment	Displaced	Non-displaced	Total
A. *The program 1979-1985 (Molina,1986)*			
Number of families	26,291	14,200	40,491
Share %	65%	35%	100 %
Number of slums	211	67	278
Number of projects	63	67	130
B. *Archival Data 1979-1985*			
Number of families	15,866	6,823	22,689
Share %	70%	30%	100%
Number of slums	84	47	130
Number of projects	56	47	96
C. *Matched Sample 1979-1985*			
Number of families	14,384	5,468	19,852
Share %	72.5%	27.5%	100%
Number of slums	83	47	129
Number of projects	56	47	96

source: Molina (1986) and archival data found by the author.

Table 1.2: Summary Statistics for Children aged 0 to 18 at baseline

	Full Sample	In RSH (2007-2019)	In GRIS (2016-2019)	P(in RSH)	P(in GRIS)
	(1)	(2)	(3)	(4)	(5)
Individuals	37,889	30,882	25,336		
Matching rate		81.5%	66.9%		
Displaced	0.71	0.72	0.70	0.057***	-0.023***
	[0.454]	[0.45]	[0.451]	(0.010)	(0.005)
Demographics at displacement					
Female	0.50	0.54	0.44	0.125***	-0.154***
	[0.50]	[0.50]	[0.49]	(0.004)	(0.005)
Age	8.18	8.16	7.87	-0.001	-0.007***
	[4.85]	[4.87]	[4.79]	(0.001)	(0.001)
# Siblings	2.87	2.92	2.78	0.010***	-0.011***
	[1.82]	[1.84]	[1.76]	(0.002)	(0.001)
First Born	0.37	0.36	0.37	-0.014***	0.014**
	[0.48]	[0.48]	[0.48]	(0.004)	(0.006)
HH age	34.94	34.97	34.72	-0.000	-0.000
	[7.11]	[7.14]	[7.02]	(0.000)	(0.001)
Female HH	0.30	0.30	0.28	-0.008	-0.024***
	[0.46]	[0.46]	[0.45]	(0.006)	(0.008)
Married HH	0.81	0.81	0.82	0.001	0.017
	[0.39]	[0.39]	[0.38]	(0.008)	(0.012)
Mother Age at birth	24.57	24.61	24.65	0.001	0.002**
	[5.68]	[5.70]	[5.66]	(0.001)	(0.001)
Mapuche HH	0.051	0.053	0.052	0.035***	0.020*
	[0.22]	[0.22]	[0.22]	(0.011)	(0.011)
Demographics measured after 2007					
Died before 2007	0.005	0.00	0.00	-0.816***	-0.629***
	[0.07]	[0.00]	[0.00]	(0.009)	(0.015)
Mother's schooling	6.00	5.88	6.14		
	[3.42]	[3.38]	[3.44]		
Mother is in RSH	0.86	0.87	0.87		
R^2				0.067	0.053

Notes: Summary statistics for children aged 0 to 18 at baseline. Column (1) reports summary statistics for the full sample, column (2) for children matched at least once to the RSH, and column (3) for children matched at least once to the GRIS. Columns (4) and (5) estimate a linear regression of the probability of being found in the RSH or the GRIS (correspondingly), on a full set of demographics at baseline, treatment (displacement), died before 2007, year of intervention fixed effects and municipality of origin fixed effects. Clustered standard errors by municipality of origin in parenthesis. 10%*, 5%**, 1%***. Standard deviations in brackets.

Table 1.3: Comparing Displaced and Non-displaced children aged 0 to 18 at baseline (year of intervention)

	All children 0 to 18		Children matched to RSH		Children matched to GRIS	
	Non-displaced mean	Difference (within municip)	Non-displaced mean	Difference (within municip)	Non-displaced mean	Difference (within municip)
	(1)	(2)	(3)	(4)	(5)	(6)
Female	0.50	0.01	0.54	0.01	0.45	0.00
		(0.01)		(0.01)		(0.01)
Age	8.65	-0.33	8.72	-0.49*	8.37	-0.36
		(0.25)		(0.27)		(0.25)
First Born	0.36	0.01	0.35	0.01	0.36	0.019
		(0.01)		(0.01)		(0.012)
# Siblings	2.75	0.13	2.84	0.11	2.67	0.08
		(0.12)		(0.12)		(0.12)
HH age	35.80	-0.53	35.94	-0.62	35.63	-0.62
		(0.39)		(0.42)		(0.40)
Mother age at birth	24.99	-0.18	25.04	-0.18	25.11	-0.28
		(0.15)		(0.14)		(0.17)
Female HH	0.30	-0.002	0.30	0.004	0.28	-0.001
		(0.02)		(0.023)		(0.02)
Married HH	0.85	-0.03***	0.85	-0.03***	0.86	-0.03*
		(0.01)		(0.01)		(0.02)
Widowed HH	0.01	0.001	0.01	-0.001	0.01	-0.001
		(0.002)		(0.003)		(0.002)
Mapuche HH	0.05	0.02***	0.05	0.02**	0.05	0.02**
		(0.01)		(0.01)		(0.01)
Mother's schooling	6.25	-0.28	6.08	-0.20	6.38	-0.27
		(0.21)		(0.20)		(0.23)
Individuals	37,889		30,882		25,336	
Families	15,369		14,102		13,306	
Slums	124		123		123	

Notes: Within difference corresponds to the coefficient of *displaced* in equation (1) conditional on municipality of origin and year of intervention fixed effects. Marital status *married* and *widowed* are computed conditional on finding a marriage and/or death certificate. Mother's years of schooling is computed in the sample of mothers found in the RSH, because of differential matching rates between displaced and non-displaced parents, the conditional difference is computed including an estimate of the probability of an individual's mother being found in the RSH (see Appendix for variables definitions). Clustered standard errors by municipality of origin. 10%*, 5%**, 1%***.

Table 1.4: Location Attributes before and after intervention

Location Attributes by Census District	Non-displaced mean (1)	Displaced mean at origin (2)	Difference (within munic.) (3)	Displaced mean at destination (4)	Difference (within munic.) (5)
Panel A. Slums Characteristics					
Area (hectares)	12.17	5.40	0.86		
			(0.81)		
# Families	292.98	247.53	46.60		
			(84.53)		
Military Name	0.21	0.19	-0.03		
			(0.12)		
Distance to River (km)	1.74	1.40	-0.06		
			(0.32)		
# Slums	44	77	121		
Panel B. Location Attributes					
Schooling HH	7.24	7.50	0.68	6.59	-0.69**
			(0.72)		(0.28)
Unemployed HH	0.18	0.18	0.00	0.22	0.04***
			(0.02)		(0.01)
HS Dropout students	0.33	0.32	-0.01	0.36	0.04
			(0.01)		(0.03)
Schools per census district	3.89	3.63	0.05	2.83	-1.31
			(0.78)		(1.00)
Schools per 1000 students	1.19	0.85	-0.44	0.64	-0.87
			(0.75)		(0.86)
Pub. Schools per 1000 students	1.00	0.70	-0.43	0.58	-0.69
			(0.80)		(0.85)
Priv. Schools per 1000 students	0.18	0.12	-0.03	0.06	-0.15
			(0.10)		(0.11)
Family Care Centers per 1000 HH	0.01	0.01	0.01	0.01	0.01
			(0.01)		(0.01)
Hospitals per 1000 HH	0.03	0.02	-0.01	0.01	-0.01
			(0.02)		(0.02)
Distance to (closest) metro station in km	7.95	9.63	-0.37	9.84	2.49**
			(0.38)		(1.17)
Commuting time to Work (min)[a]	42.25	42.38	0.13	47.47	5.06**
			(0.80)		(2.14)
Commuting time to Study (min)[a]	32.92	32.94	0.02	32.82	0.64
			(0.60)		(0.79)
Observations			160		160
# Slums			124		124
# New Projects			84		84

Notes: In panel A each observation is a slum, in Panel B each observation is a slum-neighborhood pair. Within difference corresponds to a regression of each location attribute on a displacement dummy conditional on municipality of origin. Clustered standard errors by municipality of origin. 10%*, 5%**, 1%***. All location attributes correspond to population averages by census district level in 1982. (a) Commuting times to work and to study are measured as the weighted average in minutes that takes the average person in each municipality to go to work/study using public transportation. Since these two variables are measured at the municipality level, the difference in column (3) does not include municipality fixed effects.

Table 1.5: Displacement Effect on Labor Income and Employment

Panel A.	Outcome: Self-reported income (CLP$1,000/month)			
	(1)	(2)	(3)	(4)
Displaced	-16.548	-15.314	-15.562	-15.144
	(6.229)**	(6.098)**	(6.151)**	(5.965)**
	[1.395]***	[1.371]***	[1.231]***	[1.230]***
Non-displaced mean	155.24	155.24	156.16	156.16
% Var. w.r.t. non-disp.	**-10.6**	**-9.9**	**-10.0**	**-9.7**
R^2	0.018	0.127	0.129	0.129
Panel B.	Outcome: 1[Employed]			
Displaced	-0.001	0.002	0.003	0.001
	(0.015)	(0.014)	(0.013)	(0.013)
	[0.003]	[0.003]	[0.003]	[0.003]
Non-displaced mean	0.670	0.670	0.671	0.671
% Var. w.r.t. non-disp.	**-0.1**	**0.2**	**0.3**	**0.1**
R^2	0.003	0.108	0.105	0.105
Municipality of origin FE	✓	✓	✓	✓
Baseline Controls		✓	✓	✓
Mother's Schooling				✓
Observations	620,329	620,329	540,734	540,734
Individuals	30,882	30,882	26,871	26,871

Notes: Regressions for children aged 0 to 18 at baseline matched to the RSH data that report non-missing schooling. Clustered standard errors by municipality of origin in parenthesis (42 unique municipalities), and Conley standard errors in brackets. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects and semester of income reporting fixed effects. Baseline controls include: female, mother head of household, married head of household, head of households' marital status unknown, age of mother at birth, number of siblings, first-born dummy, and year of birth fixed effects. Row labeled as *% Var. w.r.t. non-disp.* stands for "percentage variation with respect to non-displaced mean."

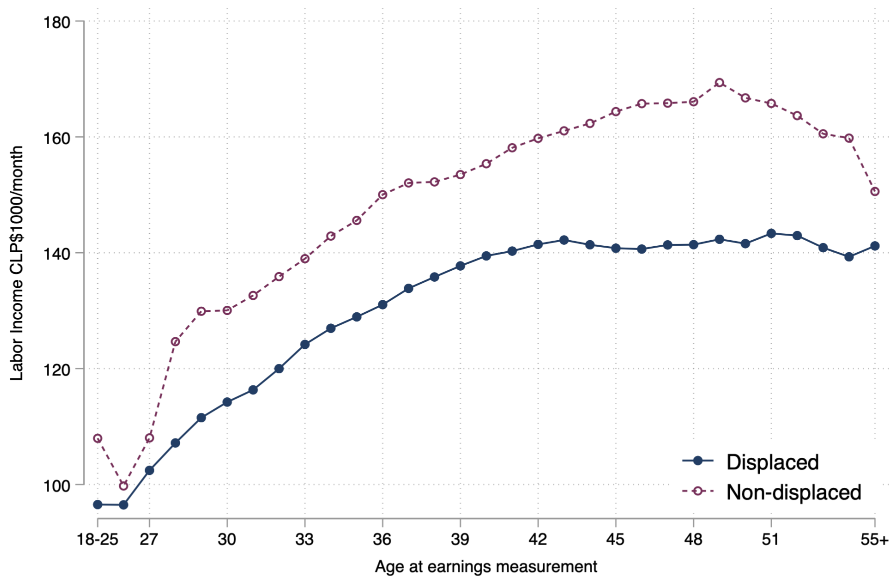
Table 1.6: Displacement Effect on Labor Market Outcomes

Outcome	Labor Income	Employed	Has a Contract	Temp. Worker	Taxable Income	Formal Income	Informal Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Displaced	-15.314**	0.002	-0.041***	0.039**	-37.463**	-16.047***	0.733
	(6.098)	(0.014)	(0.012)	(0.016)	(14.317)	(4.715)	(1.879)
Non-displaced mean	155.24	0.67	0.41	0.56	581.35	109.10	46.15
% Var. w.r.t. non-disp.	**-9.9**	**0.2**	**-10.0**	**7.0**	**-6.4**	**-14.7**	**1.6**
Observations	620,329	620,329	620,329	620,329	115,841	620,329	620,329
Individuals	30,882	30,882	30,882	30,882	20,806	30,882	30,882
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓	✓

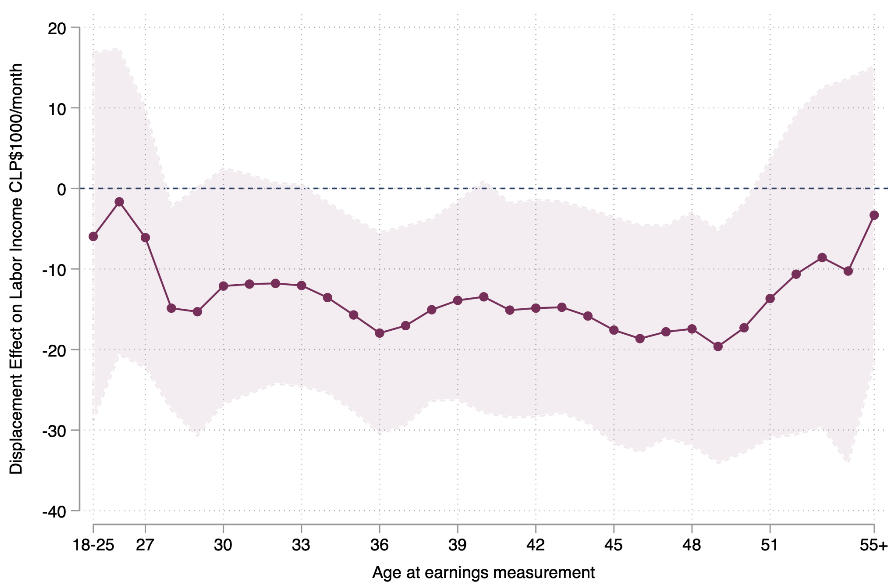
Notes: Regressions for children aged 0 to 18 at baseline matched to the RSH data that report non-missing schooling. Clustered standard errors by municipality of origin (42 clusters). 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects and semester of income reporting fixed effects. Baseline controls include: female, mother head of household, married head of household, head of household's marital status unknown, age of mother at birth, number of siblings, birth order, and year of birth fixed effects. Row labeled as *% Var. w.r.t. non-disp.* stands for "percentage variation with respect to non-displaced mean."

Figure 1.2: Displacement effects on labor market outcomes by Age at Earnings Measurement: Children age 0 to 18 at baseline

(a) Labor Income Trajectories (CLP$1,000/month)



(b) Displacement Effect on Labor Income (CLP$1,000/month)



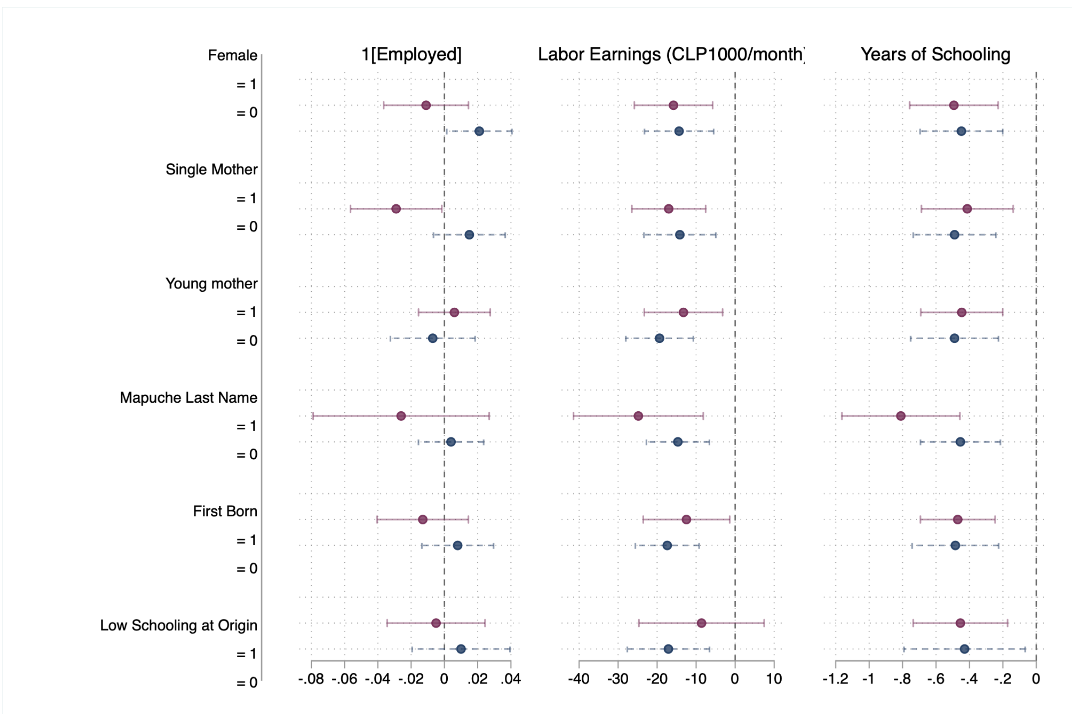
Notes: Regressions for children aged 0 to 18 at baseline and matched to the RSH data that report non-missing schooling. Clustered standard errors by municipality of origin. Controls include: female, mother head of household, married head of household, number of siblings, first-born dummy, head of household's marital status unknown, and year of birth fixed effects. Figure (a) plots the predicted trajectories for the displaced and non-displaced children between ages 18 to 55 from the previous regression. Figure (b) plots coefficients β_τ and their 95% confidence intervals from the regression: $y_{it} = \sum_{\tau=18}^{55} \beta_\tau Displaced * 1[Age = \tau] + \sum_{\tau=18}^{55} \delta_\tau 1[Age] + \psi_o + X_{it}'\gamma + u_{it}$.

Table 1.7: Displacement Effect on Schooling Outcomes

Outcome	Years of Schooling				1[HS graduate]	1[2y Coll. Att.]	1[5y Coll. Att.]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Displaced	-0.640	-0.637	-0.586	-0.473	-0.081	-0.022	-0.015
	(0.152)***	(0.136)***	(0.139)***	(0.111)***	(0.014)***	(0.010)**	(0.006)***
	[0.140]***	[0.133]***	[0.118]***	[0.117]***	[0.017]***	[0.009]**	[0.005]***
Non-displaced mean	11.37	11.37	11.37	11.37	0.66	0.12	0.06
% Var. w.r.t. non-disp.	**-5.6**	**-5.6**	**-5.2**	**-4.1**	**-12.1**	**-18.3**	**-25.0**
R^2	0.040	0.116	0.116	0.142	0.113	0.021	0.026
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓
Baseline Controls		✓	✓	✓	✓	✓	✓
Mother's schooling				✓	✓	✓	✓
Observations (Individuals)	30,882	30,882	26,871	26,871	26,871	26,871	26,871

Notes: Regressions for children aged 0 to 18 at baseline, matched to the RSH data that report non-missing schooling. Clustered standard errors by municipality of origin in parenthesis, and Conley standard errors in brackets. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include: female, mother head of household, married head of household, head of household's marital status unknown, age of mother at birth, number of siblings, first-born dummy, and year of birth fixed effects. Row labeled as *% Var. w.r.t. non-disp.* stands for "percentage variation with respect to non-displaced mean."

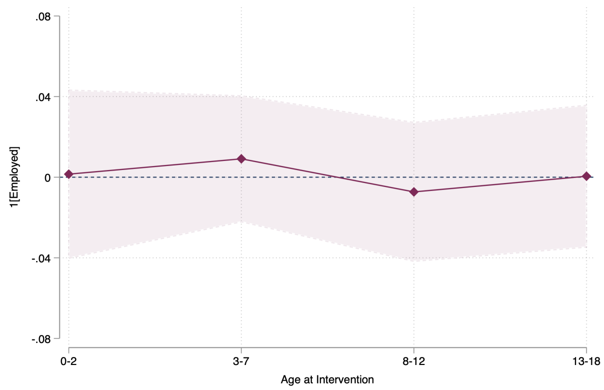
Figure 1.3: Displacement Effect by Demographic Groups on main outcomes



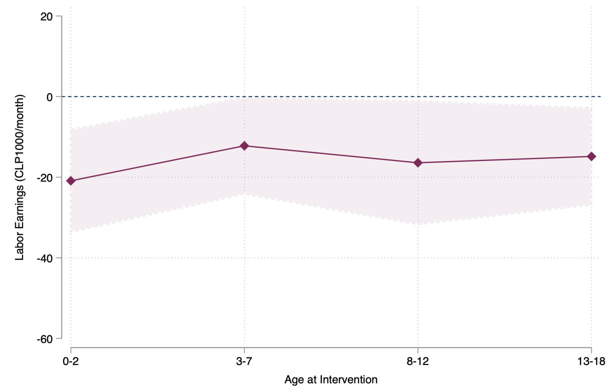
Notes: Regressions for children who were 0 to 18 years old the time of intervention and matched to the RSH. Clustered standard errors by municipality of origin. Controls include: female, mother head of household, single head of household, number of siblings, Mapuche last-name, cohort fixed effects, and time fixed effects. The figure plots the displacement coefficient and its 95% confidence interval resulting from estimating equation (1) stratified by demographic groups. Single mother is measured at the time of intervention, young mother stands for mothers younger than 25 (sample median) at the time their child is born, and Low Schooling at origin stands for municipalities of origin where the population's average schooling is below the sample median.

Figure 1.4: Displacement Effect on Labor Market Outcomes by Age at Intervention

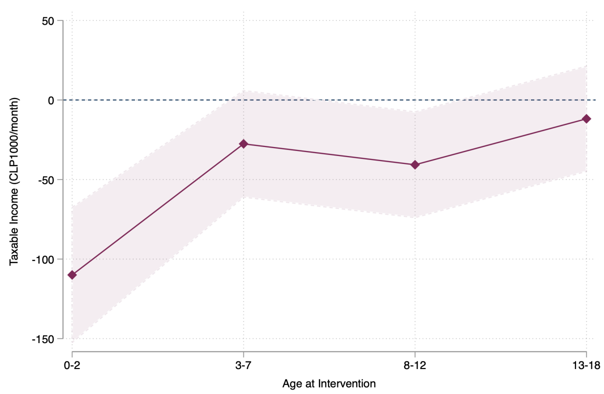
(a) Employment



(b) Self-reported Labor Earnings



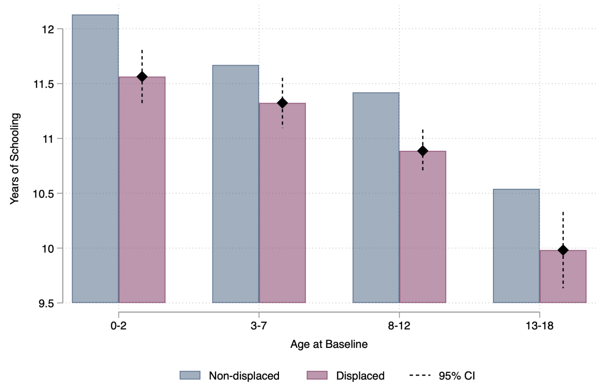
(c) Taxable Labor Earnings



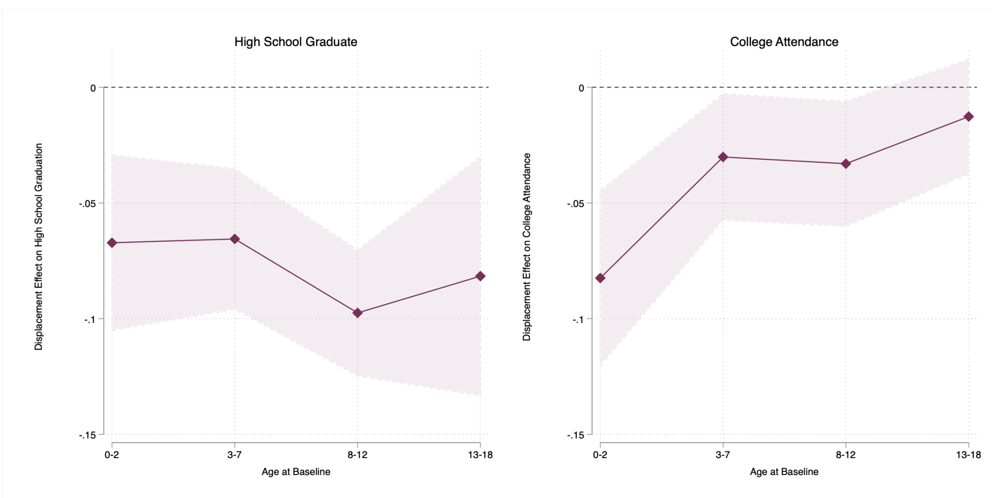
Notes: Regressions for children aged 0 to 18 at baseline and matched to the RSH or the GRIS. Clustered standard errors by municipality of origin. Controls include: female, mother head of household, married head of household, number of siblings, first-born dummy, head of household's marital status unknown, and year of birth fixed effects. Figures plot coefficients β_τ and their 95% confidence intervals from regression: $y_{it} = \sum_{\tau=0}^{18} \beta_\tau Displaced * 1[Age\ at\ baseline = \tau] + \sum_{\tau=0}^{18} \delta_\tau 1[Age\ at\ baseline = \tau] + \psi_o + X_{it}'\gamma + u_{it}$.

Figure 1.5: Displacement Effect on Schooling Outcomes by Age at Intervention

(a) Years of Schooling



(b) High School Graduation and College Attendance



Notes: Regressions for children who were 0 to 18 years old at baseline and matched to the RSH data. Clustered standard errors by municipality of destination. Controls include: female, mother head of household, married head of household, number of siblings, first-born dummy, head of household's marital status unknown, and year of birth fixed effects. Figures plot coefficients β_τ and their confidence intervals from regression: $y_{it} = \sum_{\tau=0}^{18} \beta_\tau Displaced * 1[Age\ at\ baseline = \tau] + \sum_{\tau=0}^{18} \delta_\tau 1[Age\ at\ baseline = \tau] + \psi_o + X_{it}'\gamma + u_{it}$. Panel (a) plots β_τ plus the non-displaced mean for years of schooling, while panel (b) plots β_τ for high-school graduation and college attendance.

Table 1.8: Parents mortality and Children's outcomes

Outcome	Mother dies after displacement				Father dies after displacement			
	Labor Income		Schooling		Labor Income		Schooling	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Displaced	-15.300**	-15.097**	-0.638***	-0.635***	-15.502**	-16.087**	-0.648***	-0.656***
	(6.090)	(6.114)	(0.136)	(0.137)	(6.095)	(6.117)	(0.135)	(0.137)
Parent died within 5 years	-22.501	-22.865	0.242	0.296	-25.788***	-22.811***	-1.466***	-1.324***
	(15.623)	(15.509)	(0.392)	(0.392)	(6.185)	(6.746)	(0.431)	(0.421)
Displaced* Parent died within 5 years	11.548	13.753	-1.308**	-1.278**	21.062**	16.610*	0.755	0.635
	(17.420)	(17.085)	(0.536)	(0.546)	(9.998)	(9.399)	(0.487)	(0.480)
Parent died 6-10 years		5.330		-0.837		-13.466		-0.653**
		(15.573)		(0.508)		(10.005)		(0.269)
Displaced* Parent died 6-10 years		-23.073		0.085		24.353**		0.479
		(16.286)		(0.554)		(11.291)		(0.289)
R^2	0.127	0.127	0.115	0.115	0.127	0.127	0.115	0.116
Non-displaced mean	155.24	155.24	155.24	155.24	11.37	11.37	11.37	11.37
% Displ. Parents died within 5 years	0.3	0.3	0.4	0.4	1.4	1.4	1.4	1.4
% Displ. Parents died in years 6-10	0.8	0.8	0.9	0.9	2.6	2.6	2.6	2.6
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓	✓	✓
Observations	620,329	620,329	30,882	30,882	620,329	620,329	30,882	30,882

Notes: Regressions for children aged 0 to 18 at baseline matched to the RSH data that report non-missing schooling. Clustered standard errors by municipality of origin. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include: female, mother head of household, married head of household, head of household's marital status unknown, age of mother at birth, number of siblings, birth order, and year of birth fixed effects. Earnings regressions include semester of income reporting fixed effects.

Chapter 2

Mediating mechanisms: Place effects

1. Introduction

In this chapter I study the mechanisms that explain the long-term negative displacement effects on earnings and education of children. I start the discussion with the expected effects of the displacement and their theoretical arguments. Then, I estimate place effects using the variation in the destination locations of the families displaced by the policy. And finally, I discuss how my estimates compare to the literature.

The analysis starts by describing the theoretical mechanisms behind the displacement effect. Several mechanisms could explain the negative displacement effect on children aged 0 to 18 at the time of intervention. These include parents' mortality, which I use as a proxy for parents' worse socioeconomic status, and neighborhood quality, which refers to schools, access to transportation, the disruption of existing social networks, and social cohesion. I find that children whose parents died within the first 10 years after the intervention had lower schooling; however, because only 3% of parents died during these years, parents' mortality does not account for the full displacement effect on the average child in my sample.

I find that the destination municipalities explained 70% of the total effect of displacement on labor earnings and 35% of the total effect on schooling, measured by destination municipality fixed effects. When I explore granular changes to neighborhood characteristics, I find that lower access to transportation in the new location was an important determinant of children's labor earnings after conditioning on the characteristics of the destination municipality. Although children entered the labor market several years later, public transportation in Santiago did not improve substantially until the 2000s. Thus, I use the rollout of subway stations in Santiago between 2007 and 2019 to explore changes in access to transportation. I find that when a new metro station is built close to a family's destination neighborhood the earnings difference between the displaced and non-displaced children is reduced by about 25%.

I find that the increase in neighborhood fragmentation in new locations explains almost all of the estimated effect on years of schooling. I use fragmentation as a measure of social cohesion in new neighborhoods, and I measure it as a Herfindahl-Hirshman index (HHI). To estimate the effect on children, I compare housing projects that mixed slums from very different origins with those that did not. I find that the displacement effect was more negative for children when slums were mixed and that the more fragmented the neighborhood (the lower the HHI), the more negative the displacement effect on children's education, even when I control for the characteristics of destination municipalities or other project features, such as the size (number of units) of the new neighborhood.

The analysis conducted in this chapter contributes to the literature that studies the mechanisms that shape neighborhood effects. Previous researchers have emphasized the roles of schools (Laliberté, 2021); peers (Damm and Dustmann, 2014); and public investment (?). I am able to study the mechanisms by exploiting movements in groups and variation in destination locations. I emphasize that children's schooling and adult earnings respond to different neighborhood characteristics. I decompose the different mechanisms for each outcome: Children's education is more responsive to social cohesion and children's adult labor earnings are determined by labor market access, as measured by access to transportation.[1] This last result is consistent with the literature on uneven geographical access to jobs and the spatial mismatch hypothesis (Kain, 1968; Kain, 2004; Andersson et al., 2018; Haltiwanger et al., 2020; Pérez Pérez et al., 2021).

The rest of the chapter is organized as follows. Section 2 discusses the expected theoretical effects of the displacement. Section 3 estimates the mediating mechanisms. Section 4 discusses the total effect of the displacement and compares my results with those in other settings. Section 5 concludes.

2. Negative Displacement Effect: Potential Mechanisms

I find that all individuals in my sample face a negative displacement effect. In this section I discuss mechanisms that could mediate the displacement effect.

The effects of displacement can be separated into a disruption effect and a place effect. A disruption effect is defined as the impact of moving due to changes in neighborhood environments and the loss of social networks. The disruption effect is expected to be non positive, as has been shown by Chetty et al. (2016) in the context of MTO. Moving may impact children, because adaptation to new environments is costly due to changes in schools or social environments.

A place or neighborhood effect is associated with the location attributes that families were assigned to. Families in the displaced group received a bundle of treatments. They became homeowners of new housing units, they were sent to isolated and lower-quality areas with low access to transportation, and their neighbors changed as a result of mixing individuals in the new locations. The effect of homeownership is not present in my estimates, because

[1] The question regarding fragmentation and social cohesion is more common in the development economics literature that looks at indigenous reservoirs and forced displacements, and how fragmentation and forced coexistence do or do not enhance economic development. See, for example, Dippel (2014) or Bazzi et al. (2019).

the comparison group also received a new housing unit in an upgraded neighborhood. Thus the negative effect can be attributed to isolation and new neighbors.

Isolation and lack of services are geographical characteristics of neighborhoods.[2] Based on the theory of spatial mismatch (Kain, 1968, 2004), and the short-term evidence of Aldunate et al. (1987), I expect the lack of employment and lower access to transportation to impact displaced children and adults' economic outcomes negatively. Heads of households reported that they lost their jobs after the displacement and it was harder for them to find a new one in the destination location. This would imply a decrease in earnings after relocation.[3] This is consistent with previous work by Takeuchi et al. (2007), whereby the benefits of slum relocation depend on how easy is for adults to change jobs.

In addition, destination municipalities had less public infrastructure than the original slums' locations, such as fewer schools and less access to public transportation (Table 1.4 Panel B column 5). As Molina (1986) shows, on average, destination municipalities had fewer resources and did not invest in new public infrastructure upon the arrival of the new families. For example, public investment in transportation did not occur to a substantive degree until the 2000s, and thus displaced families remained isolated for a range of years after the intervention. This might have been reinforced by the fact that all families in the program became homeowners, which has the potential to reduce mobility (DiPasquale and Glaeser, 1999).[4]

[2]Galster (2012) classifies neighborhood characteristics into four categories: Social-interactive, environmental, geographical, and institutional. The first involves interaction with peers and social networks; the second refers to attributes of the local space that may affect mental and physical health, such as pollution or exposure to violence; geographical refers to spatial mismatch and access to public services; and the last is related to stigmatization and discrimination.

[3]Notice that this is after considering that families became homeowners. As shown in previous research, housing stability can have positive impacts on children and adults who move out of slums or who receive upgraded housing, especially on adults' mental health (Galiani et al., 2017). However, since both the displaced and the comparison groups received and owned a new house, displaced families might have decreased their earnings relative to the non-displaced.

[4]This contrasts to the case of many US cities, in which the poor live in city centers rather than in suburban areas (Glaeser et al., 2008). In the Chilean context, the periphery offers more affordable options for low-income households, which was reinforced by urban sprawl due to lower land regulation during the dictatorship. This is consistent with the idea of urban sprawl discussed by Kahn (2001).

Families in the displaced group experienced a change in their neighbors for two reasons. First, people who already lived in the destinations had on average lower schooling than the population at the origin (Table 1.4 column 5). And second, because they were mixed with other displaced families in their destination projects. These changes correspond to the social-interactive attributes of neighborhoods. The new projects were mixing poor individuals with more poor individuals, and the new projects had small housing units.[5] This concentration of the poor can generate harmful local spillovers that exacerbate social problems (Case and Katz, 1991). This is consistent with Aravena and Sandoval (2005), who argue that mixed projects increased social conflict between neighbors because families did not know each other. Thus, if families had preferences for neighborhood composition, as Takeuchi et al. (2007) suggest, being mixed with different people could have negative consequences for children's outcomes.

3. Mechanisms

In this section I study the mechanisms that mediate the displacement effect for children aged 0 to 18 at baseline. First, displacement affected parents directly through their health (higher mortality). Second, displaced families faced a change in the attributes of their neighborhoods. Third, families' neighbors changed substantially. I examine these three mechanisms next.

3.1 Parents' mortality and children's outcomes

In Chapter 1, I showed that displaced parents died at higher rates than the non-displaced, especially in the first 10 years after the end of the program. To see how this affected children, I interact the displacement dummy with a variable that indicates whether the child's mother

[5] Families reported that their new apartments were smaller than they expected, and smaller than the space they had in their original slums. Some of these testimonies can be found in the newspapers at the time (Morales and Rojas, 1986).

or father died in the first 5 years after families were treated or between years 6 and 10. The results are in Table 1.8.

My results suggest that parents' mortality increases the magnitude of the displacement effect on children's education but not on earnings. On the one hand, the results show that the mother's mortality in the 5 years after the intervention more than doubles the displacement effect on children's schooling. The effects on earnings go in the opposite direction, but are very noisy. Father's mortality, on the other hand, affects all children by reducing their education, but it does not render the displacement effect more negative; if anything, it reduces the negative impact of a father's death.

These results are unexpected, because displaced children with parents who die soon after the intervention have lower schooling but not lower income. The point estimates are big and noisy, and can be a reflection of a small proportion of the children in my sample who face these events: Only 1.2% of the mothers and 3.9% of the fathers died in the 10 ten years.[6] In summary, parents' mortality does not explain a big proportion of the displacement effect on children.

3.2 Destination locations

In this section I show that an important fraction of the displacement effect on earnings and education can be attributed to destination municipalities at the time of the intervention. Based on the fact that destination municipalities were poorer on average, I start the analysis by looking at how the inclusion of destination municipalities fixed effects in regression (1) changes the displacement effect on earnings, employment, and education. The results are shown in Panel A of Table 2.1.

First, destination municipality fixed effects are identified because the same municipality

[6] To see where these effects are coming from, I look at different age groups and find that the decrease in years of schooling is concentrated among children older than 8 at baseline whose mothers died in the first 5 years since displacement (see Table F.1 for results).

can expel and receive families at the same time. Thus, the fixed effects are identified as the difference in mean outcomes between individuals from origin o and destination d and the mean outcome of individuals from origin o. They measure all common attributes shared by families in the destination municipalities. My results show that 70% of the displacement effect can be attributed to variation in municipalities' characteristics (columns 1 and 2), and 35% of the displacement effect on years of schooling (columns 5 and 6).

In Figure 2.2, I stratify the displacement effect on earnings by municipality of origin and plot the estimated coefficients against average changes in location attributes.[7] The figure shows that the effects correlate positively with the population's schooling and with number of schools at the destination. The correlation is negative with longer distances to the subway or longer commuting times. In Panel B of Table 2.1, I repeat this exercise in a regression form and include all covariates at the same time. I interact the displacement dummy with changes in location attributes.

I find that of all the changes in attributes I measure, lower access to labor markets—measured as distance to subways or waiting time for public transportation—reduce the earnings and employment of displaced children (columns 1 and 3). These coefficients become bigger in magnitude when I control for destination municipality fixed effects. The coefficients are statistically different from zero and economically important; for example, children who end up farther from a subway station at the time of the intervention have 0.7% lower earnings per kilometer. For employment, the number is 0.4% per kilometer.

The displacement effect on years of education is not as sensitive to the inclusion of changes in characteristics as are earnings and employment (columns 5 and 6). In fact, the inclusion of changes in location attributes does not change the displacement effect. Even though the coefficients on access to schools are positive, none are statistically different from

[7] All changes in location characteristics are measured as the difference in an attribute at the census district of destination minus the census district of origin for the year 1985 or earlier, if available. These are measures that vary at a smaller level than municipalities, and I choose to look at changes to proxy for the shocks faced by families.This implies that the shocks for non-displaced are all zeros.

zero. These results on schooling contrast with previous findings regarding the role of schools in children's education (Laliberté, 2020; Chyn and Katz, 2021). I do not find evidence of schools' having an effect on years of education (even though the coefficients are positive in column 6), perhaps due to low school quality measures.[8]

3.3 Changes to labor market access

My results indicate that one of the determinants of why displacement was negative for children in their adulthood is labor market access. A question arises here, because my measures of access are for the years when families moved out of slums, while children in my sample would enter the labor market several years after, in the 1990s and 2000s. The city of Greater Santiago has changed substantially over the last 30 years in terms of its transportation system and urban limits; perhaps the improvements in public transportation reduce the earnings gap between the displaced and non-displaced.

To test this, I examine the rollout of the new metro lines in Santiago during recent decades to see whether the construction of a new metro station close to families' destination locations impacts the displaced and non-displaced children differently. In Figure A.2, I show the location of subway stations in Santiago at different points in time. I exploit the variation in new subway stations between the years 2006 and 2019.[9]

To estimate the impact of access to the subway, I estimate the following regression:

$$Y_{it} = \alpha + \beta Displaced_{s\{i\}} + \gamma_1 Subway_{\lambda t} + \gamma_2 Displaced_{s\{i\}} \cdot Subway_{\lambda t} + \psi_o + \psi_\tau + X'_{it}\theta + \varepsilon_{it}, \tag{2.1}$$

[8]In addition, in the context of Chile, as opposed to countries like the US or Canada, students are not mandated to go to the schools located in their district of residence and families can choose between public and private schools (mainly voucher schools) regardless of their location. Thus I cannot rule out that children in my sample were not attending schools they lived closer to.

[9]Three new lines were inaugurated during this time period, in the years 2010, 2011, 2017, and 2019.

where all variables are defined as in equation (1), and $Subway_{\lambda t}$ is a dummy equal to 1 if a subway station is available in year t within a distance λ from children's destination project. If access to the subway reduces the gap between displaced and non-displaced children, I expect γ_2 to be positive.

I report the results on income and employment in Table 2.2 for different values of λ (1, 1.5, and 2 kilometers). I find that subway stations very close to destination neighborhoods (less than 2 kilometers) impact earnings positively but do not affect employment. However, the effects on earnings decrease the farther away the metro station (more than 2 kilometers). For example, if a subway station was built within 1.5 kilometers from the destination neighborhood, the absolute value of the displacement effect on earnings is reduced by 26% (5.5/20.8). In Figure 2.3, I repeat the exercise in an event study format and exploit the variation in access across years. The results confirm that the effects are detectable the closer the subway station is to the destination neighborhood, but the results are also noisier. The event study exercise shows that earnings increase and the effect remains stable across years, while employment is slightly reduced in the years after the construction of the subway, but the difference fades out across years.

Figure F.1 shows that the positive effect on earnings is mainly due to increases in informal earnings rather than formal earnings. In Table F.2 I report results by demographic groups, I find that women benefit the most from access to the subway. This is consistent with the fact that in my sample, women are more likely to work in informal jobs. The overall results on earnings are in line with previous work that studies the effects of the subway in Santiago on adults' labor market outcomes (Pérez et al., 2021).[10]

[10] In addition, Meneses (2020) finds that the subway expansion enables students from more peripheral areas to access higher-quality schools in more central districts of the city.

3.4 Composition of new neighborhoods: Project characteristics

The third mechanism I examine is how different types of displacement affect children. When a slum was considered for eviction, all families in the slum would be displaced to a new project—and most of the new projects built for the displaced received families from different slums coming from different municipalities. Around 50% of the total number of projects received families from different slums, which accounts for 70% of displaced families and 80% of children in my sample. I call these projects *mixed* neighborhoods.

Why might a mixed neighborhood impact families differently than a non-mixed project? A mixed project is associated with reductions in social cohesion among neighbors. To test whether this is a relevant mechanism, I classify each destination project as a mixed or non-mixed neighborhood and interact the displacement dummy with this variable. The results of this exercise (Table 2.3) show that children in mixed projects faced a 50% more negative effect on earnings compared with displaced children in non-mixed projects. The results on schooling are even larger (Panel B). Almost all of the negative displacement effect on years of education can be attributed to children in mixed projects. This result is robust to controlling for destination municipality characteristics (column 2).

A mixed project can differ from a non-mixed project in several respects, in particular, in size and fragmentation. Mixed projects are on average four times bigger than non-mixed projects, and non-mixed ones are very similar in size to projects for the non-displaced. In terms of fragmentation, by definition a mixed project is a fragmented project because they received families from more than one slum. However, there is great variation in the degree of fragmentation: While some projects combined families from two slums, others received families from more than 10 slums.

In Appendix Table F.3 I show that conditional on displacement, families' demographics do not predict the likelihood of being sent to a mixed project. Moreover, the characteristics of their neighborhoods before displacement were very similar to those of the non-displaced slums (Table F.4). Thus, conditional on displacement, project characteristics were assigned as good as random.

To understand the role of project features, I include size and fragmentation in my baseline regressions. I measure size as the total number of housing units built in each project. I measure fragmentation using a Herfindahl-Hirschman index that uses as shares the fraction each slum represents in the total number of families assigned to each new project. I do this for the universe of neighborhoods in the program. For better interpretation of the coefficients, I divide the HHI by 1,000 and interact it with the displacement dummy. Hence, an HHI equal to 10 indicates a non-fragmented neighborhood, and an HHI of 0 indicates a fully fragmented neighborhood. Notice that the variation in this variable is only for displaced families, because non-displaced families ended up in non-fragmented projects (HHI=10).

My results indicate that fragmentation is more important than size in explaining my results, especially for years of education. In Table 2.3 columns 3 and 4, I report results that include size as a control: While size is associated with lower income and fewer years of schooling, the coefficient on the interaction between displaced and mixed decreases in magnitude relative to column 1, but it remains statistically significant for years of schooling, and is still economically important: It represents around 75% of the average displacement effect on education. In column 5, I replace the category mixed with my HHI measure. As expected, children in fully fragmented neighborhoods (HHI=0) face the most negative displacement effect (displacement coefficient), and less fragmentation (higher HHI) offsets the negative displacement effect on both earnings and education. This result is robust to conditioning on project size and destination municipalities' characteristics (columns 6 and 7).

The results for fragmentation in the previous table assume that the effect is linear. To be more flexible, I repeat the exercise in column 5 in a nonparametric fashion by computing terciles of the distribution of the HHI and stratifying the treatment into these terciles (HHI<10) and no fragmentation (HHI=10). The results (Figure 2.4) indicate that children in projects in the first tercile of the HHI faced more negative effects, especially on their education. Children of families that did not have to go to a mixed project did not experience a negative displacement effect: The effect on schooling is very close to 0 and not statistically significant. For years of schooling I reject equality of coefficients for the first tercile relative to any other coefficient. For earnings, however, I cannot reject the null in all cases.

The effect of mixing families on years of schooling is concentrated in the children who were between 8 and 18 years old at baseline, especially those older than 12 (Appendix Table F.5). This suggests that mixed projects are particularly bad for adolescents—who might be more susceptible to changes in social environments, as described by the theory on disruption. My results are robust to including measures of destination locations, which suggests that fragmentation is a crucial component in explaining the displacement effect on education, and could be a reflection of other neighborhood characteristics that are negative for children, such as increased conflict and crime as a result of mixing families from different origins.[11]

4. Comparison with Other Settings and Discussion

In this section I compare my displacement estimates with other settings. I then discuss the magnitude of my findings by estimating the total loss in earnings due to displacement for the children in my sample. Finally, I discuss potential policy alternatives to the Program for Urban Marginality.

[11] I cannot test this hypothesis in the short term, because measures of crime at the neighborhood level are only available in the 2000s.

4.1 How do my estimates compare with other settings?

My results show that in my sample, displaced children have 0.5 fewer years of education relative to non-displaced, earn 10% lower income, and are 10% more likely to work in the informal labor market. My setting is very particular: It occurs in a developing country and families are moved to high-poverty areas. This renders comparison with other settings difficult, because most of previous literature considers induced movements from high- to low-poverty areas.

With those caveats in mind, I will compare the magnitude of my estimates with other studies by computing an elasticity defined as the percentage change in earnings when there is a 1% change in neighborhood quality. I report the results of this exercise in Table A.12. According to these numbers, the implied elasticity in my setting is 1.04. This is larger than the implied elasticity in studies in the US (in Chetty et al. (2016) is 0.42, and in Chyn (2018) it is 0.72). It is also larger than the implied estimate in Barnhardt et al. (2016) for India (if neighborhood quality is measured as urbanicity), but with the difference that this paper focuses on adults and not children.

A first source of difference between my estimates and the other papers is that I include children younger than 7 in my sample. I compute the corresponding elasticities for different ages in my sample to see whether that explains the differences. I find 1.42 (ages 0 to 2); 0.83 (ages 3 to 7); 1.09 (ages 8 to 12); and 1.01 (13 to 18).[12] In all cases, the elasticities are bigger than in the other studies. Thus, even if I focus on children older than 7, I find an elasticity of bigger magnitude than in other studies.

A second source of difference across studies could be attributed to the level of development between countries—since cities in developing countries are more segregated and more unequal—and/or to the population under study, because slum dwellers were poorer than the

[12] Corresponding changes in earnings by age groups are -13.5%; -7.9%; -10.4%; and -9.6%. In all cases, I divide by -9.5% to compute the elasticity.

average poor family in Greater Santiago. I find more negative effects on high school graduation. Previous papers do not find big effects on high school completion, but do find effects on college enrollment. If on average the return to high school completion is smaller than the return to college attendance, this can explain the different results on earnings (-10% in this paper and 16% in Chyn, 2018). This makes sense in the context of a poor or a developing country, because high school graduation is a more relevant margin than tertiary education.

Finally, a third source of difference could be nonlinearities in neighborhood effects, since poorer families might be affected differently than richer families. This has been suggested by Chyn (2018) when comparing his setting with the MTO setting, and by Van Dijk (2019) in the context of public housing in Denmark.[13] I observe something along these lines; in the last row of Figure 1.3, I compare the displacement effect of children who come from areas with high education with those who come from areas with low education. I see more negative effects for the former on labor earnings (however, I cannot reject that they are equal to each other). This could be suggestive of nonlinearities in neighborhood effects because the children that faced a bigger shock were the most affected on their earnings after the displacement.

4.2 Total earnings loss due to displacement

I use the age estimates on earnings presented in Section 6.1 to calculate the present value of the loss of earnings as a consequence of displacement. Assuming the effects are constant between ages 18 to 25 and after 55 up to 60, and using an annual discount rate of 4%, the average child in my sample at the age of 45 lost CLP$6.5MM relative to non-displaced children. This is equivalent to US$9,000, which is more than the cost of the housing unit

[13]This is confirmed by my estimated elasticities. The implied elasticity in Chyn (2018) is bigger than the elasticity in Chetty et al. (2016). The percentage change in earnings is very similar in both studies, but in the first case, families experienced a smaller change in neighborhood quality as measured by the poverty rate. See Table A.12.

received by their families through the Program for Urban Marginality.[14] In aggregate terms, this is equivalent to the construction of 17 subway stations or the maintenance of 360 primary schools per year.[15]

I consider this estimate to a be lower bound, because it does not take into account the direct effect on schooling and its externalities. For example, a potential negative externality of the reduction on schooling could be reflected on increasing criminal activity.[16]

4.3 Policy alternatives

My results show that the total effect of displacement on children is negative.[17] What are alternative options to displacing families to the periphery? One policy alternative would be providing housing on site, which is one of the main policy actions proposed by the World Bank and the United Nations in recent years (UN-Habitat, 2020). However, this is not always feasible for multiple reasons, such as high urban density that impedes public housing construction on site, the high price of land, or the impossibility of providing services on site (running water, electricity, sewage).

Under those circumstances, one option would be to compensate families monetarily for displacement, as proposed by Lall et al. (2006). However, it might be difficult to assess compensation amounts. In addition, this type of compensation may not solve poor house-

[14]The results of this exercise at different ages can be found in Figure G.2, plotted in the blue line.

[15]I compute the aggregate loss as the individual loss times the number of children in my sample. The cost of building subway stations is available from Metro de Santiago, and the cost of schools has been estimated by Sanchez et al. (2015).

[16]This has not been discussed in the paper so far, but in preliminary results in which I use data on incarceration between 2000 and 2010, I find that displaced children are 28% more likely than non-displaced to be in jail at least once during this time period.

[17]The aim of this paper is to study the long-term effects of neighborhoods on children's economic outcomes. However, a valid question is whether the program was good or bad for families. To answer this question, in addition to estimating a displacement effect I would need to know the effect of slum upgrading on children. Unfortunately, the nature of my data does not allow me to answer this question, because my comparison group was also provided with housing. Moreover, administrative data for slum dwellers are not available for the 1980s because slums were not administrative units. In Appendix Section 7., I discuss whether I can bound the displacement effect to include the effect of housing.

holds' problems if the monetary compensation does not translate into access to services and they remain isolated.[18] Thus, another option is to provide families with the necessary public services they need to foster their economic development, such as schools, health care centers, and access to public transportation—meaning that to effectively foster families' and children's development, displacement should be accompanied by the provision of public services that counteract the negative disruption effect.[19] This is in line with my results on access to the subway and the decrease in magnitude of the displacement effect.

Another policy margin is communities' participation in the eviction processes and whether families can choose their final destination. Policy advocates argue that one of the main components of a successful eviction process is that families participate in the process.[20] Under the hypothesis that families have more information and greater incentives to find a proper destination, I would expect that a voluntary move is not as negative as a forced move.[21]

5. Conclusions

The setting I study is suitable to understand the role of place on children's long-term economic development, because as as consequence of the policy, families in slums were assigned to new location without choosing. This variation in the data helps me understand the causal role of different location attributes on children's adult labor earnings.

I find that for the children who were 0 to 18 years old at the time of intervention, their

[18]Dasgupta and Lall (2009) discuss several reasons for this, such as that poor populations may face bigger problems of social cohesion, low levels of empowerment, and fewer social networks, which may translate into more difficulties for them in coordinating the provision public services.

[19]One caveat to these policies is the mixed evidence found on the effects of placed-based policies (Neumark and Simpson, 2015).

[20]See research on this matter supported by the World Bank in Lall, Freire, Yuen, Rajack, and Helluin (2009).

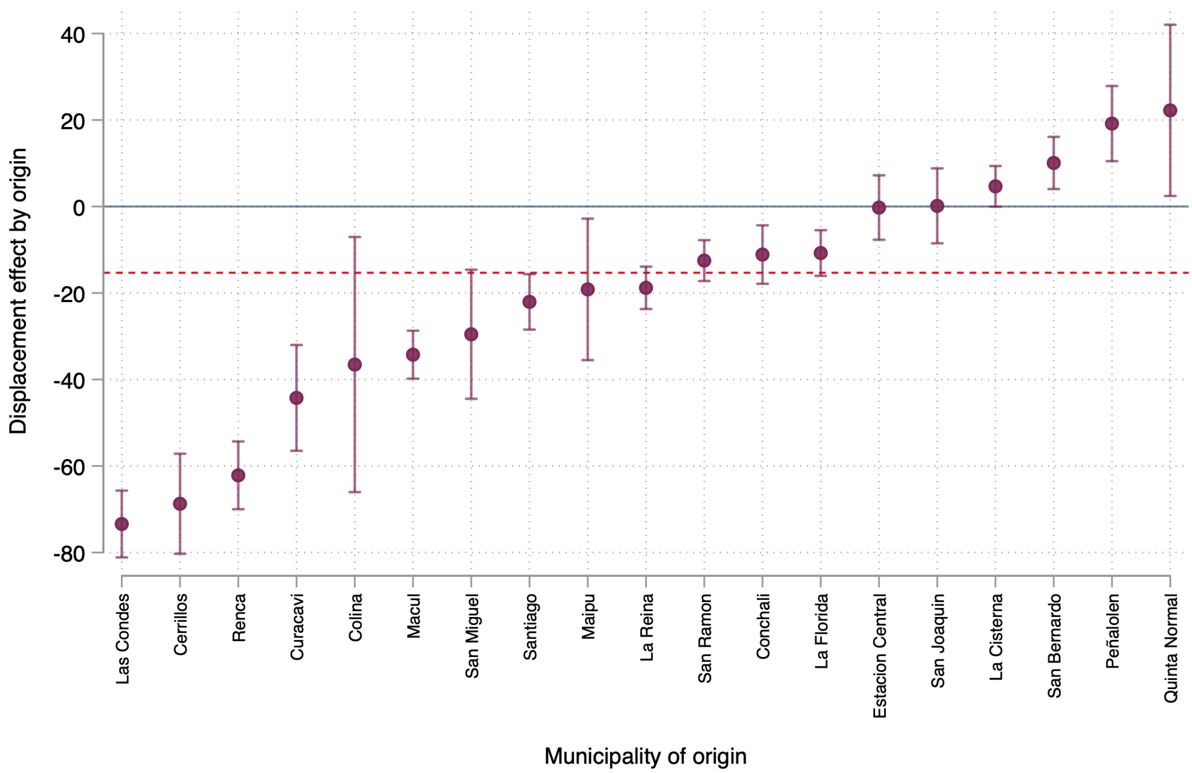
[21]I have research in progress on this question. I compare the forced estimates with a setting in which slum dwellers are allowed to choose their final destination. My results confirm the hypothesis on children's earnings and schooling. Some preliminary results can be found in Appendix Section 8..

years of schooling and labor earnings are sensitive to different neighborhood characteristics: While schooling responds to measures of social cohesion and local features of the projects themselves, labor market outcomes are also determined by the extension of and access to labor markets, as measured by public transportation. In this regard, I am currently working on evaluating the importance of each mediator on the total displacement effect on children's earnings. In addition, it is important to assess the role of families' location choice on their children's outcomes.

An important aspect of the setting I study is that families were forced to move to places that ended up being poverty traps, and potentially worse than their original slums. In the end, this led to negative consequences for children's and parents' economic development. My research contributes to understanding the effects of these policies on individuals; however, future research should take into account the general equilibrium effects that slum clearance policies have on neighboring individuals and communities, and on segregation within cities.

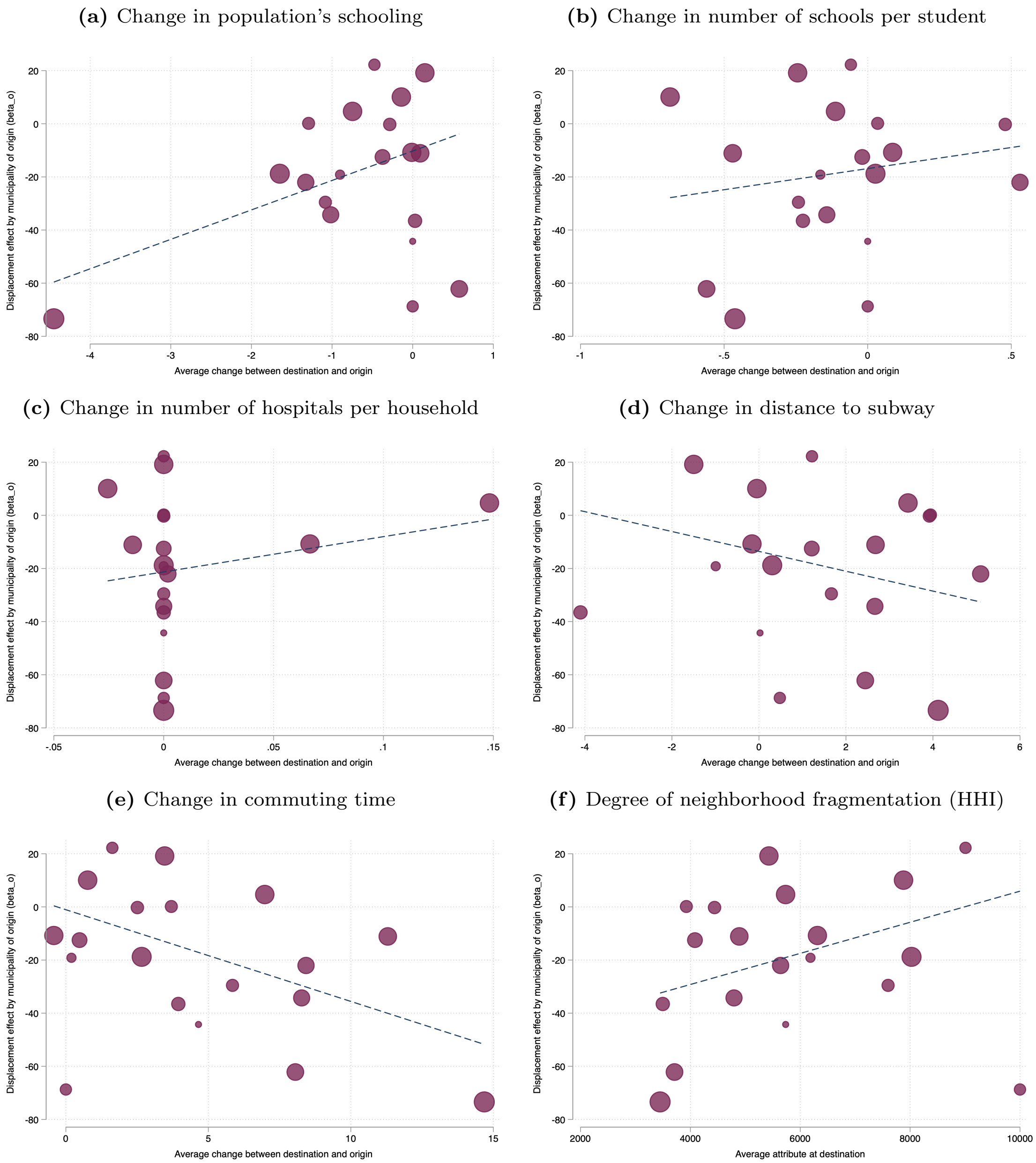
6. Tables and Figures

Figure 2.1: Distribution of displacement effect by municipality of origin



$$Y_{it} = \sum_{o=1} \beta_o Displaced_{s\{i\}} * 1[Origin = o] + \psi_{\tau o} + X'_{io}\theta + \varepsilon_{it}$$

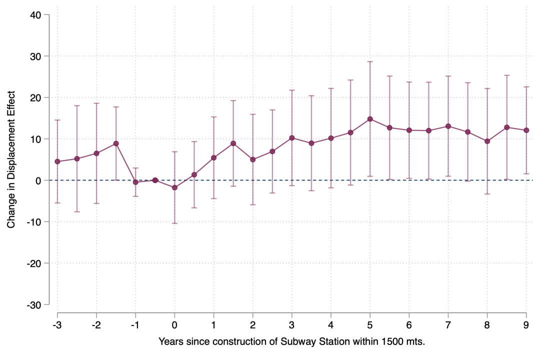
Figure 2.2: Displacement effect on labor earnings by municipality of origin and changes in location attributes



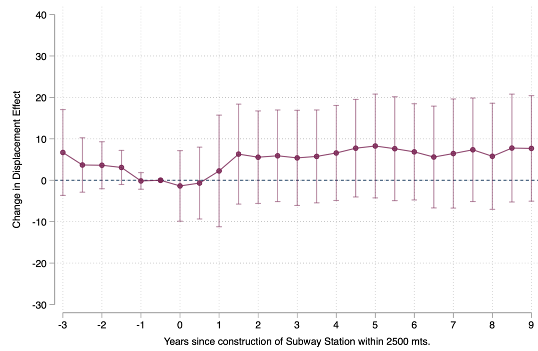
Notes: Figures plot displacement coefficients on labor income stratified by municipality of origin against average changes in location attributes by municipality of origin. Coefficients are estimated using the following regression: $y_{it} = \sum_{o=1}^{20} \beta_o Displaced * 1[Origin = o] + X'_{iot}\gamma + u_{iot}$, where o indexes the municipality of origin for child i. Changes in attributes (x-axis) are computed as $\bar{\Delta}_o = \sum_{d=1}^{30} \Delta_{iod}$. Regressions for children who were 0 to 18 years old at baseline and matched to the RSH data that report non-missing schooling. Controls include: female, mother head of household, married head of household, number of siblings, first-born dummy, head of household's marital status unknown, and year of birth fixed effects. Full sample includes 42 municipalities of origin; however, in this graph I use only 20 municipalities for which there are enough observations such that I observe displaced and non-displaced children from the same municipality. Coefficients β_o are weighted by the number of observations in each cell. Figure A.8 reports the distribution of coefficients, and Figure A.9 repeats the exercise for years of schooling.

Figure 2.3: Roll out of subway stations between 2007 and 2019 and change in displacement effect

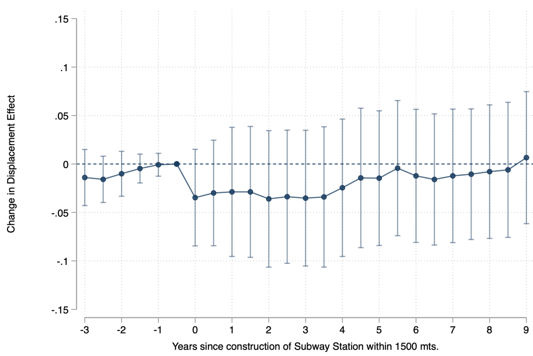
(a) Labor Earnings, distance=1.5KM



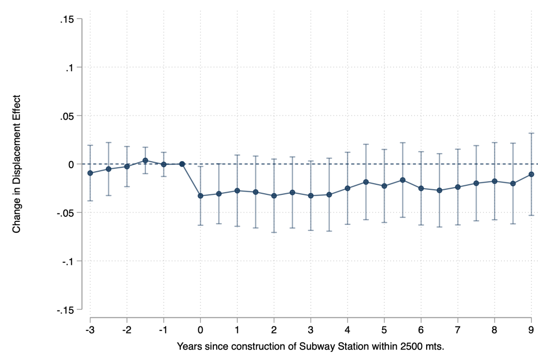
(b) Labor Earnings, distance=2.5KM



(c) Employment, distance=1.5KM



(d) Employment, distance=2.5KM



Notes: Regressions for children aged 0 to 18 at baseline matched to the RSH data that report non-missing schooling. Clustered standard errors by municipality of origin. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include: female, mother head of household, married head of household, head of household's marital status unknown, age of mother at birth, number of siblings, birth order, and year of birth fixed effects.

Table 2.1: Displacement Effect and Change in Location Attributes on Main Outcomes

Outcome	Labor Income		1[Employed]		Years of Schooling	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.	Baseline					
Displaced	-15.314**	-3.693	0.002	0.027	-0.473***	-0.351**
	(6.098)	(8.531)	(0.014)	(0.018)	(0.111)	(0.137)
R^2	0.127	0.129	0.108	0.109	0.142	0.147
Non-displaced mean	155.24	155.24	0.67	0.67	11.37	11.37
% Var. w.r.t. non-displaced	-9.9	-2.4	0.2	4.0	-4.2	-3.1
Panel B.	Change in location attributes					
Displaced	-6.896	-1.571	0.015	0.027	-0.416***	-0.426**
	(5.948)	(11.055)	(0.012)	(0.022)	(0.122)	(0.196)
* ΔHH Years of schooling	2.351	0.859	0.008***	0.005**	-0.018	-0.103
	(1.612)	(1.479)	(0.004)	(0.002)	(0.057)	(0.096)
* Δ#Private schools/child	3.479	-1.013	-0.006	-0.020	0.116	-0.174
	(5.532)	(6.621)	(0.024)	(0.020)	(0.317)	(0.374)
* Δ#Public schools/child	-1.601	1.621	0.001	-0.004	-0.022	0.259**
	(2.177)	(2.485)	(0.007)	(0.008)	(0.110)	(0.111)
* ΔDistance to Subway	-0.642**	-1.089**	-0.002***	-0.003*	0.002	0.020
	(0.245)	(0.528)	(0.001)	(0.002)	(0.009)	(0.027)
* ΔWaiting Time	-0.279	-5.432**	0.003	0.003	0.059	-0.286**
	(0.866)	(2.195)	(0.003)	(0.006)	(0.051)	(0.139)
* Distance from origin	-0.312*	-0.197	0.001*	0.0002	-0.014**	-0.012
	(0.179)	(0.284)	(0.0005)	(0.001)	(0.006)	(0.010)
* ΔHealth Care Centers	-0.007	-0.001	-0.0004**	-0.0003	0.0002	0.0009**
	(0.006)	(0.013)	(0.0002)	(0.0003)	(0.002)	(0.0004)
* ΔHospitals	0.005***	0.005	0.0001**	-0.0002	0.0007	0.0004**
	(0.001)	(0.006)	(0.00005)	(0.0001)	(0.0009)	(0.0002)
R^2	0.128	0.129	0.108	0.109	0.141	0.146
Non-displaced mean	155.24	155.24	0.67	0.67	11.37	11.37
Municipality of origin FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Municipality of destination FE		✓		✓		✓
Observations	620,329	620,329	620,329	620,329	26,871	26,871

Notes:This table shows results for $Y_{it} = \alpha + \beta Displaced_{s\{i\}} + \gamma Displaced_{s\{i\}} \cdot \Delta Attribute_{do} + \psi_o + \psi_\tau + X_i'\theta + \varepsilon_{it}$. All attributes are measured at the census district level which corresponds to a smaller level of aggregation than municipalities. Regressions for children aged 0 to 18 and matched to the RSH data. Clustered standard errors by municipality of origin in parenthesis. 10%*, 5%**, 1%***. Controls include: female, mother head of household, married head of household, number of siblings, birth order and cohort fixed effects. Schooling regressions include mother's education as a control.

Table 2.2: Displacement effect and Subway Rollout between 2007 and 2019

Outcome	Labor Income			Employment		
	(1)	(2)	(3)	(4)	(5)	(6)
Distance to new station	1KM	1.5 KM	2 KM	1KM	1.5 KM	2 KM
Displaced	-17.735**	-20.823**	-17.956**	0.001	0.002	0.005
	(7.310)	(8.576)	(7.445)	(0.016)	(0.018)	(0.017)
Subway Station	-5.351	-8.231	-0.038	-0.002	-0.006	0.005
	(5.624)	(6.176)	(4.990)	(0.018)	(0.016)	(0.013)
Displaced*Subway	13.536**	13.694*	5.171	0.007	-0.000	-0.007
	(6.701)	(6.880)	(5.545)	(0.023)	(0.017)	(0.014)
Non-displaced mean	155.24	155.24	155.24	0.67	0.67	0.67
% Displaced affected by subway	10.4	30.0	43.6	10.4	30.0	43.6
% Non-displaced affected by subway	28.2	45.5	50.3	28.2	45.5	50.3
R^2	0.127	0.127	0.127	0.108	0.108	0.108
Municipality of origin FE	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓

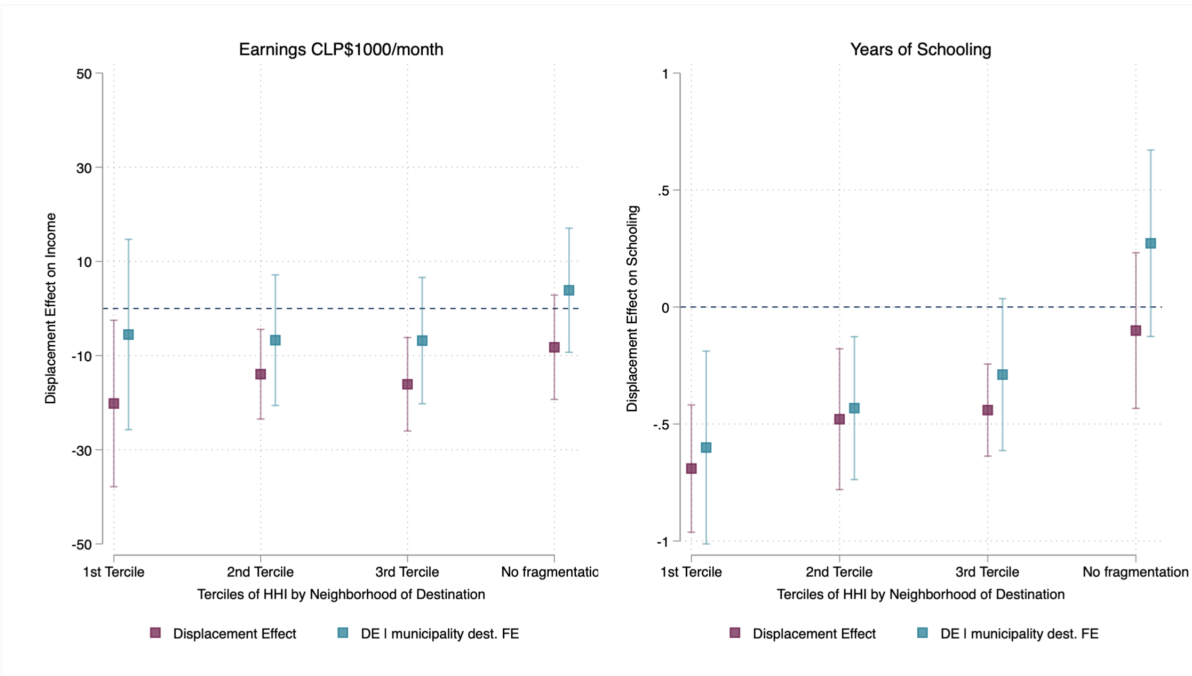
Notes: Regressions for children aged 0 to 18 at baseline matched to the RSH data that report non-missing schooling. Clustered standard errors by municipality of origin. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include: female, mother head of household, married head of household, head of household's marital status unknown, age of mother at birth, number of siblings, birth order, and year of birth fixed effects.

Table 2.3: Displacement and Project Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A.	Outcome: Labor Income						
Displaced	-10.295	5.146	-13.255**	-10.523	-19.286***	-16.940**	-8.799
	(6.560)	(7.651)	(6.031)	(6.757)	(6.939)	(7.263)	(8.203)
Displaced* Mixed	-5.835*	-10.699***		-3.668			
	(3.132)	(2.651)		(3.195)			
Project Size (per 10 units)			-0.042*	-0.033		-0.027	-0.015
			(0.02)	(0.026)		(0.026)	(0.047)
Displaced*HHI					0.914*	0.679	1.189***
					(0.491)	(0.493)	(0.404)
Non-displaced mean	155.24	155.24	155.24	155.24	155.24	154.24	154.24
R^2	0.127	0.129	0.127	0.128	0.127	0.127	0.129
Observations	620,329	620,329	620,329	620,329	620,329	620,329	620,329
Panel B.	Outcome: Years of Schooling						
Displaced	-0.101	0.209	-0.366***	-0.112	-0.756***	-0.662***	-0.643***
	(0.172)	(0.179)	(0.127)	(0.175)	(0.138)	(0.121)	(0.170)
Displaced* Mixed	-0.439**	-0.679***		-0.341***			
	(0.164)	(0.219)		(0.133)			
Project Size (per 10 units)			-0.002*	-0.001		-0.001	-0.003
			(0.001)	(0.001)		(0.001)	(0.002)
Displaced*HHI					0.064***	0.054***	0.074***
					(0.020)	(0.014)	(0.021)
Non-displaced mean	11.37	11.37	11.37	11.37	11.37	11.37	11.37
R^2	0.142	0.142	0.141	0.142	0.142	0.142	0.146
Observations	26,871	26,871	26,871	26,871	26,871	26,871	
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓	✓
Municipality of Destination FE		✓					✓

Notes: Regressions for children aged 0 to 18 at baseline matched to the RSH data that report non-missing schooling. Clustered standard errors by municipality of origin in parenthesis. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include: female, mother head of household, married head of household, head of household's marital status unknown, age of mother at birth, number of siblings, birth order, and year of birth fixed effects. Schooling regressions include mother's education as a covariate, and earnings regressions include semester of income reporting fixed effects. Average project size in the sample is 255 housing units.

Figure 2.4: Displacement Effect by Fragmentation



Notes: Regressions for children who were 0 to 18 years old at baseline and matched with the RSH data. Clustered standard errors by municipality of origin. Controls include: female, mother head of household, married head of household, number of siblings, first-born dummy, head of household's marital status unknown, and year of birth fixed effects, schooling regressions include mother's schooling as a control, and earnings regressions include semester of income reporting fixed effects. The Figures plot displacement coefficients and their 95% confidence intervals from an extended version of regression (1) in which the displacement dummy is stratified in 4 groups: no fragmentation (HHI=10,000), low fragmentation (3rd tercile of HHI), medium fragmentation (2nd tercile of HHI), and high-fragmentation (1st tercile of HHI). Each coefficients should be understood as the difference in outcomes between the displaced children in the corresponding group relative to the non-displaced children. "DE | municipality dest. FE" stands for displacement effect after controlling for municipality of destination fixed effects.

Chapter 3

Linking slums, parents and children

1. Introduction

Many examples in economics, history, and demography use parent-child linked data to study topics about intergenerational mobility, immigration, health, and demographic transitions. The availability of data containing these types of links is not a trivial task as researchers require access to administrative data, and/or to records that go back in time for the study of long-term trends, for example. A feasible solution, more commonly used by economic historians, is the use of historical archives. The archival records offer the alternative of reconstructing the data and linking individuals from the original sources (for example Giuliano and Matranga (2020), Bailey et al. (2022) or Abramitzky et al. (2019)).[1]

In this chapter, I describe one example of how to construct a dataset that combines individual historical records from the 1980s linked to administrative sources in the 2000s. My goal was to assemble a dataset that contained the recipients of the Program for Urban Marginality studied in the first two chapters of this dissertation. The final sample included parents and children, their slum of origin, neighborhood of destination, and treatment status

[1]Bailey et al. (2018) review the literature on linking methods in the context of U.S. Historical Data.

(whether they belonged to a displaced or to a non-displaced slum).

I started assembling the data by searching for the recipients of property titles in the program. I collected and cleaned the records I found in the Chilean National Archives. I continued by constructing family composition by combining the archival records with birth certificates. The objective was to find the children of the program recipients. To increase efficiency in the search for birth certificates, I used the family tree structure of names in Chile, that are composed by first name, middle name, and two last names. I finished the process of assembling my sample by collecting information about the slums in the program: their names, geolocation, number of families, projects of destination, and treatment status. I used the number of families, and projects of destination to match slums' attributes to my archival sample of families.

The final sample of children aged 0 to 18 at the time of the intervention corresponded to 37,889 individuals, of whom, 81% I found in the Ministry of Social Development's administrative records. This left me with an estimation sample of 30,882 individuals. This number corresponded approximately to 40% of the total number of children who would have been in the original program.

The data assembled for this project may suffer from selection issues due to missing records that I did not find in the archives, and/or due to individuals I could not link between the archival records and administrative sources. In Chapter 1, I discussed the second issue, and in this chapter I discuss the first. To do so, I combined detailed information of the slums that participated in the full program to study whether the slums I found in the archives are observationally different from the slums I did not. I show that the slums I found were more likely to be displaced, had more families, and were closer to public transportation in their neighborhoods of destination. I discuss how these differences may bias the displacement effect on earnings I estimated in Chapter 1.

Although the data collection described here is specific to the study of the long-term effects of the Program for Urban Marginality, the methods I used to assemble my sample may apply to other contexts. For example, many Latin-American countries have the same name structures that Chile has. The fact that women do not change their last-name upon marriage, and the use of two last-names, helped me with the reconstruction of parent-child links.

This chapter is organized as follows. Section 2 describes the archival records. Section 3 describes the process of constructing family composition, and the final sample used in this dissertation. Section 4 describe selection issues, and finally, Section 5 concludes.

2. Archival Data

2.1 Homeowners

The main effort of my data collection process was to find the families in the program. The Program for Urban Marginality was executed under the Executive Order 2552. I attempted to collect all the records of the participants of the eviction policies between 1979 and 1985 under the executive order 2552. I collected and digitized archival data from the Regional Housing and Urban Planning Service that administered the program in the Metropolitan Region (Great Santiago) and from historical records kept by the Municipality of Santiago.[2]

These data corresponded to administrative records of the lists of people and their spouses, who received a property deed as a consequence of the program in their destination project (neighborhood). I was able to collect data for 22,689 unique recipients of social housing in the Program for Urban Marginality. These individuals represent around 56% of the total number of recipients according to the numbers in Molina (1986).

I was not able to find all the records for two reasons. First, the program was administered

[2]Each region of Chile (equivalent to a state in the US) has an Urban Development and Housing Service, dependent of the National Ministry of Housing and Urban Development. These services administer and implement housing policies at the regional level.

by individual municipalities, because of this, many of the records were kept by the municipalities and not sent to the regional administration. During the dictatorship years, there was an order in place that allowed municipalities to keep administrative records for only five years, after that, they were allowed to destroy them.[3] The second reason is that some of the records in the Chilean National Archives were lost during a flood at the beginning of the 1990s. Thus, it is possible that some of the data I was looking for were lost during this incident.

Figure 3.1 shows an example of how the archival records looked like. Each document contained a list of individuals that were assigned a housing unit in a new destination neighborhood. The archival data contained information of the recipient of the property deed and their spouse (always in this order), their full names, their national identification numbers (NID), and the address of the new housing unit. These records were grouped by year of eviction/urban renewal and project of destination.

2.2 Slums and destination projects (neighborhoods)

The archival records were sorted by destination neighborhoods and not by slums, thus, a key part in the data cleaning process was to assign each family to a slum of origin in order to create the treatment variable. I constructed a comprehensive dataset of all slums and projects of destination involved in the Program for Urban Marginality. This task was not easy because different sources reported different numbers, hence, my goal was to harmonize the different data sources.

I used information from three main sources sources. First, I digitized two slum censuses conducted by Chile's Ministry of Housing in 1979 and 1984 that contained information on slums' names and projects of destination, this allowed me to classify each slum as displaced or non-displaced and their neighborhoods of destination. Second, the housing programs

[3]This issue has been confirmed by several municipalities.

implemented in the 1980s were contemporaneously studied by the Latin American Faculty of Social Sciences (FLACSO) in Santiago. I draw intensively from two of their studies. Benavides et al. (1982) compiled a comprehensive list of existing slums in the year 1982, including characteristics such as land size, number of families and location. Morales and Rojas (1986), described the treatment of slums, identified neighborhoods of destination and provided a list of non-displaced slums. Third, I complemented this information with Molina (1986), who documented the characteristics of the program for the displaced families. The author compiled a full list of displaced slums from 1979 to 1984, reporting the exact number of families and their destination for each eviction episode.

There were two main challenges in the allocation process of families in the archives to slums of origin. First, the archival records were ordered by the date on which the families moved to the destination neighborhoods and not by origin, and in most of the cases they included groups of families with more than one slum of origin. Second, when a non-displaced slum was treated, often the new neighborhood had a different name from the original slum. Moreover, adjacent non-displaced slums were sometimes treated in a single new neighborhood of destination; however, that was not identifiable in the data.

To solve the first challenge I used the number of displaced families treated in each slum of origin, which I obtained from the sources described above, jointly with the variable place of registration or *Gabinete* (see figure 3.1). This variable was a goo proxy for municipality of origin.[4] To identify non-displaced slums, I matched the address of the destination neighborhoods with the location of known non-displaced slums and number of families treated reported in the sources above. Since families were treated by slum of origin, this meant, all the families I observe in my records who I classified as non-displaced, were part of the same slum of origin. In addition, some of the records for the non-displaced included the type of

[4]Place of Registration is called *Gabinete* in the records, and corresponds to the Civil Registry and Identification Service (CRIS) office where the person was first registered. Most municipalities in Santiago had at the time a CRIS office.

dwelling, thus, when families received a starting-kit ("caseta sanitaria" in Spanish), I knew for sure these families went through an urban renewal process, hence, they belonged to the non-displaced group of slums.

2.3 Final sample of families

Table 3.1 summarizes the total number of recipients of the program and the total number of records I was able to find in the archives. In the actual program, 65% of families were displaced and 35% were non-displaced. In the records I found, 70% of the recipients were identified as displaced families, and 30% as non-displaced. By comparing the number of slums between panels A and B in the table, it is posible to see that I was able to find the bigger slums and projects of destination, as I observe a smaller share of displaced slums relative to the total number of slums in the sample, compared to the same fraction measured by number of families.

The numbers reported in Panel B of Table 3.1 correspond to the total number of individuals in the archival records before performing any cleaning of the data (but after dropping duplicates). Once the data was cleaned and digitized, I lost some individuals for whom I could not verify their NID with current sources of data, that is, all the families in which at least one of the partners had a valid national ID verified with other sources of administrative data, such as marriage certificates or electoral records. The cleaned sample is reported in Panel C. Of the 22,689 families that I found in the Archives, only 19,852 families had at least one valid national ID number. The 2,837 records that did not have a valid National ID (difference between panels B and C), were because they had mistakes, had a very old National ID number, or were single adults that never married for whom I could not verify their NID using a marriage certificate.

As a consequence of the missing data and cleaning, displaced households are overrepresented in my final sample, the number of displaced families increased to 72.5%. In addition, single adults were overrepresented in my sample (not shown in table 3.1). This overrepresentation was reflected in the summary statistics of the final sample of children used in Chapter 1, where displaced children were less likely to have a married parent compared to non-displaced children at the time of the intervention (recall Table D.5 for the results).

3. Constructing family composition

3.1 Matching process for children of couples

The next step in the construction of a full database consisted of finding the children of each family. Unfortunately, I did not have access to administrative data on family composition at the time of the intervention, so I proceeded by constructing these records. I worked with Genealog Chile to web-scrape birth and marriage certificates from Chile's National Civil Registration. I collected birth, marriage and death certificates of all the Chilean population that were 18 or older in 2016.

The only information needed to get a birth certificate is a person's the National ID number. In Chile, the NID are correlative numbers and they grow with a person's cohort (younger people have bigger numbers compared to older people), hence, it was possible to construct all the NID for the whole population.[5] A birth certificate contained a person's full-name at birth, date of birth, National ID number, and parents' full names, and parents' NID numbers.[6] Figure 3.2 shows an example of a birth certificate.

The matching process was straightforward when the NID numbers were observed; however, having access to all the birth certificates for the whole Chilean population in a short period of time (months) was not feasible, because it required web-scrapping more than 18

[5]NID are correlative numbers but not all of the are used, for example, no citizen is going to be assigned a NID such as 9,999,999 at birth.

[6]With the caveat that the older the person the less likely it was to find their parents' NID in a birth certificate, but the full names were always included.

million documents (population of Chile). Thus, to speed up the process, I started the search by looking for the children of couples. To do this, I made use of the way last names are constructed in Chile, which is the same way it is done in many Latin-American countries, where a person's family name consists of two parts, a father last name and a mother last name. To see this, consider the following example, assume María Lucía Pérez Rojas and José Miguel Rodríguez González are a couple. Each of these individuals' names consist of a first name, a middle name, and two last names. María's last name is Pérez Rojas, and José's last name is Rodríguez González. It is always the case that the first last name comes from the father's side and the second from the mother's side.[7] This means that, if María and José have children in common, their children's family name will be "Rodríguez Pérez."

By knowing this feature of the Chilean names, for each couple in the archival sample I knew exactly the combination of last names their children should have had. I combined this feature with information coming from the Electoral Records in the year 2016. In the Electoral records, I observed the population who were 18 or older in 2016, their full names, and their NID numbers. Hence, for each couple in my sample, I search for all the individuals with a particular combination of last names, that are potential candidates to be their children. By doing this, I end up with 1.9 million individuals who are candidates to be the children of the 19,852 families in my archival sample (panel C in Table 3.1). It still was a big number, but it required 1/10 of the time needed to web-scrape the birth certificates (compared to the whole population). Once I collected the certificates, the matching is simple and it is done through parents and children NID numbers. Figure 3.3 summarizes the process just described.

3.2 Final matched sample

The next step was to find the children of single parents. To do so, after a year, Genealog was able to web-scrape all the birth certificates for the Chilean population observed in the the

[7] At least until 2021. In 2022 a new law was passed, and now couples can choose the order of the last names of their children.

2016 Electoral Records. This allowed me to complete my sample by matching single parents to their children using NID numbers.

Finally, of the 19,852 valid household records, I found that 17,651 of them had at least one child, which corresponded to a total of 55,343 children in the sample. Of those, 37,889 individuals were children of ages 0 to 18 at the time of the intervention. These corresponded to my archival sample of children, that I matched to administrative records using NID numbers. Of the 37,889 children, I found 81% in the administrative data between 2007 and 2019, corresponding to 30,882 individuals. This is my matched sample.

4. Data selection

There are two selection concerns about the dataset I assembled for this project. First, I only found 56% of the total recipients of the Program of Urban Marginality, which drops to 49% once the data were cleaned. And second, 81% of the former were found in the administrative records. In Figure 3.4 I summarize these numbers: The 30,882 children in my matched sample would correspond to about 40% of the total number of children that would have been part of the program originally.

In Chapter 1, I discussed whether the children in the administrative records were observationally different from the children in the archival data. In Chapter 1 sections 3.5 an 4.2, I showed that in the administrative data I found more women and younger kids. This was the case for both displaced and non-displaced children, meaning that these differences between samples should not pose a problem on my estimation.

Even though children in archives and in administrative data looked similar in their observables, I have not yet discussed whether the final sample I used in my estimation is a good representation of the individuals in the full program. This is especially relevant if the attrition produced by missing data from the archives generates non-random selection in the origin, and/or in the destination neighborhoods. This is not possible to solve at the demographic level, but at the slum level: I do not observe families I did not find in the archives, but I know which slums are not in my final sample. By looking at the differences between the slums in my sample and the slums in the full program, I am able to understand whether those differences correlate with the treatment variable.

I used the data compiled by Morales and Rojas (1986) and geolocated all the slums in their sample. Then I matched their data with the slums in my sample. The results to this exercise are summarized in Table 3.2. In columns (1) and (2) I report the characteristics of the slums I found in the archives and the slums I did not find in the archives. In column (3) I report the difference between the two groups conditional on municipality of origin. The first thing to notice is that I find more displaced slums and bigger slums (measured by number of families), this is consistent with the numbers previously shown in Table 3.1. On average, the slums in my sample were located in census districts with a population that was more educated, they had fewer schools per student in their municipalities of origin, and they were closer to subway stations, which was consistent with being closer to Downtown. None of these differences are statistically different from 0 within municipalities (panel A).

I do not find systematic differences in the characteristics of slums at the origin, but at the destination. On average, the slums I found had families that ended up in municipalities with fewer schools per student, but they were closer to transportation and closer to Downtown. This is consistent with finding projects of destination that were less likely to be on the periphery of the city. The projects that I found were also less likely to be fragmented (higher HHI) compared to the projects I did not find in the archival data, and they were also bigger in terms of the number of families (housing units) in the neighborhoods of destination.

So far the differences I am finding are not a problem unless they vary by treatment. In the following columns (4 to 8), I repeat the previous exercise separating the sample between displaced and non-displaced slums. The numbers in panels B and C show that the differences between found and not-found are bigger in the displaced group than in the non-displaced. The question is whether these differences on the probability of finding a slum in the archival data are the reason why I found a negative displacement effect, and not the causal effect of the displacement on children's earnings and education.

The signs of the differences make me believe that the selection of slums in my sample would go against finding a negative displacement effect. In column (10) I report the double difference between finding a slum and being displaced: In my sample, displaced slums were more likely to end up in neighborhoods with a less educated population and fewer schools, which would explain a negative displacement effect, but at the same time they ended up closer to transportation, and in less fragmented projects, relative to the slums I do not find. Thus, if distance to subway and fragmentation correlate negatively with children's earnings and education, the estimate of a displacement effect I found in my matched sample is an upper bound of the true displacement effect.

The question is which of the previous differences dominate, if any. In Table 3.3 I report the same results but in regression form, so I can control for all the slums' characteristics at the same time. In this case, the results are confirmed as column (6) shows, the only variable that would go against my results is population's schooling at the destination.

5. Conclusions

This chapter describes the data collection process to compile the data used in this dissertation. My goal was to assemble a comprehensive sample of the families that participated in the Program for Urban Marginality in the city of Santiago, Chile, between 1979 and 1985. This dataset includes families, children, their slum of origin, and their neighborhood of destination. The individuals in my sample are matched to administrative records administered by governmental institutions, with the goal of estimating the effects of the forced displacement on children and families 20 to 40 years after the end of the program.

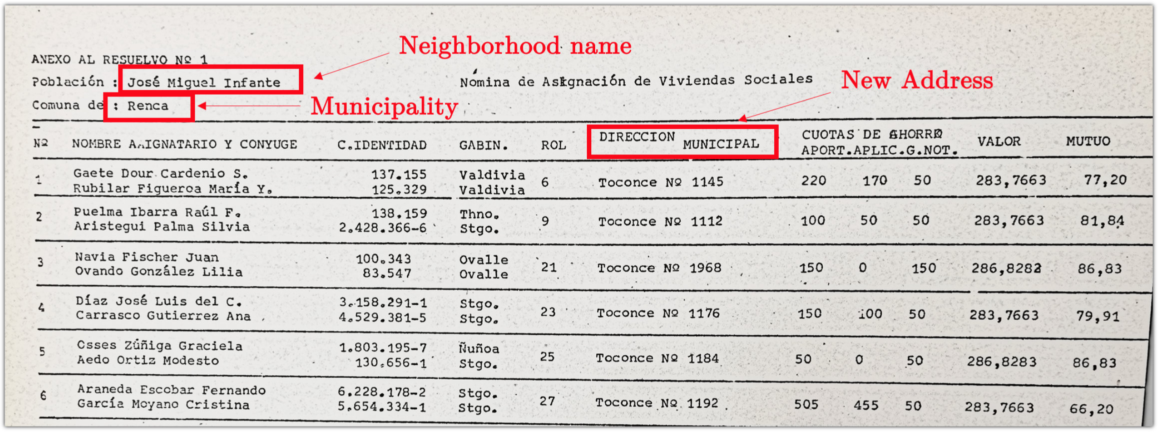
Once the archival records were found, the process of assembling the dataset had two main challenges: The identification of slums of origin, and finding the children. For the first, I drew intensively from the work produced by MINVU (1979), Benavides et al. (1982), Morales and Rojas (1986), and Molina (1986). These researchers collected and organized comprehensive data of the slums that participated in the program, their original locations, the number of families involved, and their neighborhoods of destination. They combined administrative data with newspapers to produce their datasets.

For the second challenge, I made use of the structure of names in Chile, which consists of two names and two last names. I partnered with Genealog Chile, and collected birth certificates for the adult Chilean population. Then, I used full names and National ID numbers to match parents with children.

The processes described in this chapter combine methods used by economic historians, labor economists, and demographers, to generate intergenerational datasets. The example described in this dissertation can be applied to other settings, as many Latin-American countries have the same name structures that Chile has, and there is an increase in the use of administrative data to do research and to inform policy. I strongly believe that linking historical data with administrative sources will become more and more common for the understanding of the long-term effects of public policy programs, reforms, and demographic trends.

6. Tables and Figures

Figure 3.1: Archival Records: Lists of property deeds



ANEXO AL RESUELVO Nº 1
Población : José Miguel Infante
Comuna de : Renca

Nómina de Asignación de Viviendas Sociales

Nº	NOMBRE A.IGNATARIO Y CONYUGE	C.IDENTIDAD	GABIN.	ROL	DIRECCION MUNICIPAL	CUOTAS DE AHORRO APORT.	APLIC.	G.NOT.	VALOR	MUTUO
1	Gaete Dour Cardenio S. Rubilar Figueroa María Y.	137.155 125.329	Valdivia Valdivia	6	Toconce Nº 1145	220	170	50	283,7663	77,20
2	Puelma Ibarra Raúl F. Aristegui Palma Silvia	138.159 2.428.366-6	Thno. Stgo.	9	Toconce Nº 1112	100	50	50	283,7663	81,84
3	Navia Fischer Juan Ovando González Lilia	100.343 83.547	Ovalle Ovalle	21	Toconce Nº 1968	150	0	150	286,8282	86,83
4	Díaz José Luis del C. Carrasco Gutierrez Ana	3.158.291-1 4.529.381-5	Stgo. Stgo.	23	Toconce Nº 1176	150	100	50	283,7663	79,91
5	Csses Zúñiga Graciela Aedo Ortiz Modesto	1.803.195-7 130.656-1	Ñuñoa Stgo.	25	Toconce Nº 1184	50	0	50	286,8283	86,83
6	Araneda Escobar Fernando García Moyano Cristina	6.228.178-2 5.654.334-1	Stgo. Stgo.	27	Toconce Nº 1192	505	455	50	283,7663	66,20

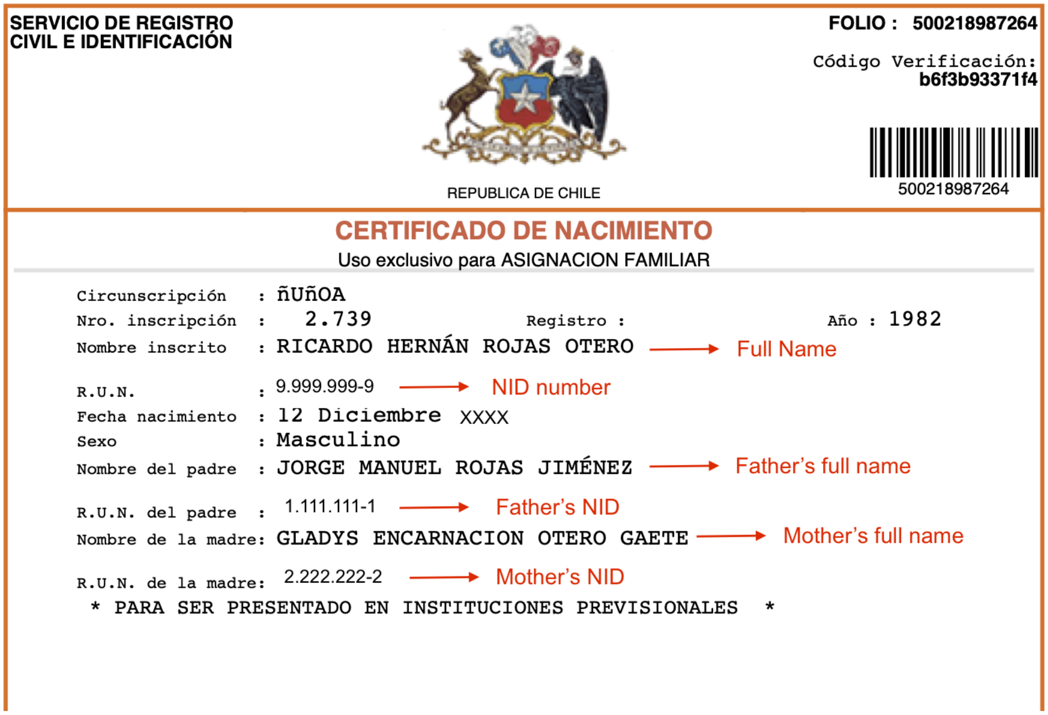
Family ID	Name	Relation to hh	NID	District	House	Address	Aport.	Aplic.	G.Not	Value
1	Gaete Dour Cardenio S.	1	137155	Valdivia	6	Toconce 1145	220	170	50	283.77
1	Rubilar Figueroa María Y.	2	125329	Valdivia	6	Toconce 1145	220	170	50	283.77

Table 3.1: Archival Data 1976-1985

Treatment	Displaced	Non-displaced	Total
A. *The program 1979-1985 (Molina,1986)*			
Number of families	26,291	14,200	40,491
Share %	65%	35%	100 %
Number of slums	211	67	278
Number of projects	63	67	130
B. *Archival Data 1979-1985*			
Number of families	15,866	6,823	22,689
Share %	70%	30%	100%
Number of slums	84	47	130
Number of projects	56	47	96
C. *Estimation Sample 1979-1985*			
Number of families	14,384	5,468	19,852
Share %	72.5%	27.5%	100%
Number of slums	83	47	129
Number of projects	56	47	96

Source: Molina (1986) and archival data found by the author. The aggregate numbers in Benavides et al. (1982) are slightly different because they report more non-displaced slums, as they collected information from newspapers and not from administrative sources like Molina (1986). For the sake of comparison, in this table I use Molina (1986) because the total number of families per episode she reports coincide almost exactly with the data I found in the archives.

Figure 3.2: Example of a birth certificate



SERVICIO DE REGISTRO
CIVIL E IDENTIFICACIÓN

FOLIO : 500218987264
Código Verificación:
b6f3b93371f4

500218987264

REPUBLICA DE CHILE

CERTIFICADO DE NACIMIENTO
Uso exclusivo para ASIGNACION FAMILIAR

Circunscripción : ñUñOA
Nro. inscripción : 2.739 Registro : Año : 1982
Nombre inscrito : RICARDO HERNÁN ROJAS OTERO
R.U.N. : 9.999.999-9
Fecha nacimiento : 12 Diciembre XXXX
Sexo : Masculino
Nombre del padre : JORGE MANUEL ROJAS JIMÉNEZ
R.U.N. del padre : 1.111.111-1
Nombre de la madre: GLADYS ENCARNACION OTERO GAETE
R.U.N. de la madre: 2.222.222-2
* PARA SER PRESENTADO EN INSTITUCIONES PREVISIONALES *

Figure 3.3: Matching process: From parents to children

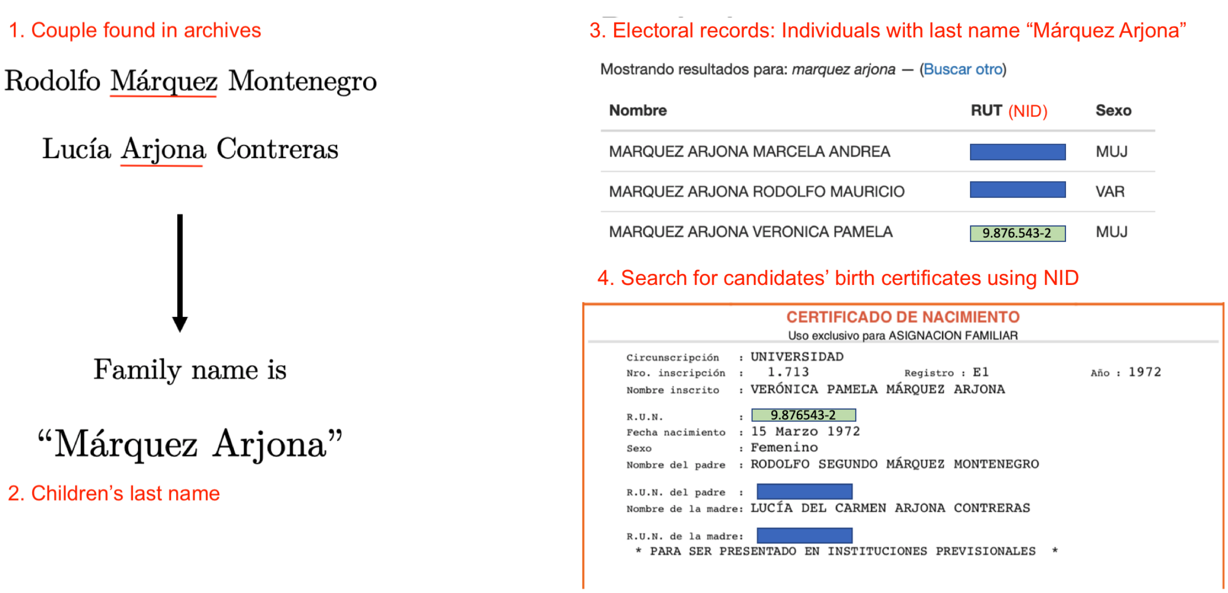


Figure 3.4: Attrition summary

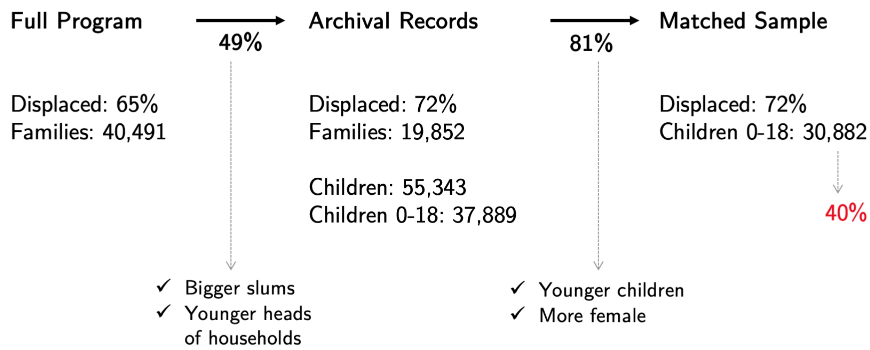


Table 3.2: Characteristics of slums

Variable	Full Sample			Displaced			Non-displaced			Diff-Diff
	In Arch.	Not in Arch.	Diff.	In Arch.	Not in Arch.	Diff.	In Arch.	Not in Arch.	Diff.	(Displ*Found)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Displaced	0.57	0.42	0.14**							
			(0.06)							
# Families	257.11	168.74	77.91***	243.12	133.60	99.17***	269.10	194.54	85.02	47.43
			(25.91)			(29.33)			(63.75)	(87.56)
Land use (hectares)	4.2	3.82	0.59	3.43	3.34	0.42	5.50	4.11	1.44	-1.03
			(0.46)			(0.64)			(1.06)	(1.16)
A. Location characteristics at origin										
Schooling HH	7.57	7.37	0.08	7.85	7.90	-0.12	7.25	7.00	0.00	-0.36
			(0.31)			(0.43)			(0.37)	(0.36)
Rural	0.03	0.02	0.01	0.04	0.02	0.02	0.01	0.02	-0.01	0.02
			(0.02)			(0.02)			(0.01)	(0.02)
#Schools per municipality	0.64	0.71	-0.02	0.68	0.75	-0.04	0.57	0.68	-0.04	0.01
			(0.05)			(0.05)			(0.06)	(0.05)
#Health Care Centers	0.01	0.01	0.00	0.01	0.01	0.00	0.01	0.01	0.00	0.00
			(0.00)			(0.00)			(0.00)	(0.00)
Distance to subway	4.82	6.10	-0.42	4.88	6.08	-0.56	4.78	6.11	-0.62	-0.29
			(0.49)			(0.66)			(0.63)	(0.83)
Distance to downtown	9.75	10.77	-0.30	9.48	10.71	-0.59	10.25	10.78	-0.13	-0.72
			(0.58)			(0.69)			(0.67)	(0.74)
B.Location characteristics at destination										
Schooling HH	6.82	6.83	-0.02	6.54	6.59	-0.08	7.25	7.00	0.00	-0.20
			(0.15)			(0.20)			(0.37)	(0.41)
Rural	0.03	0.03	0.01	0.04	0.03	0.03	0.01	0.02	-0.01	0.03
			(0.02)			(0.03)			(0.01)	(0.03)
#Schools per municipality	0.54	0.66	-0.08*	0.51	0.63	-0.12*	0.57	0.68	-0.04	-0.07
			(0.04)			(0.06)			(0.06)	(0.06)
#Health Care Centers	0.01	0.01	0.00	0.01	0.01	0.00	0.01	0.01	0.00	0.00
			(0.00)			(0.00)			(0.00)	(0.00)
Distance to subway	5.37	6.75	-1.11***	5.91	7.62	-1.58**	4.78	6.11	-0.62	-0.36
			(0.37)			(0.51)			(0.63)	(0.85)
Distance to downtown	11.29	12.08	-0.65	12.25	13.84	-1.22	10.25	10.78	-0.13	-0.43
			(0.62)			(0.79)			(0.67)	(1.02)
C. Project characteristics										
#Slums	3.86	3.66	-0.01	6.13	7.38	-1.65**	1	1		-1.98
			(0.37)			(0.65)				(0.72)
Fragmentation (HHI)	6636.41	7079.23	-249.12	3992.28	2988.21	1254.44**	10000	10000		1469
			(423.98)			(420.27)				(467.15)
#Families per project	549.69	442.45	-77.78	778	789.35	-56.37	269.10	194.54	85.02	-105.18
			(51.30)			(0.64)			(63.75)	(124.33)
Observations	99	222	321	65	108	173	34	114	148	321
Slums			251			124			148	251
Projects			195			48			148	195

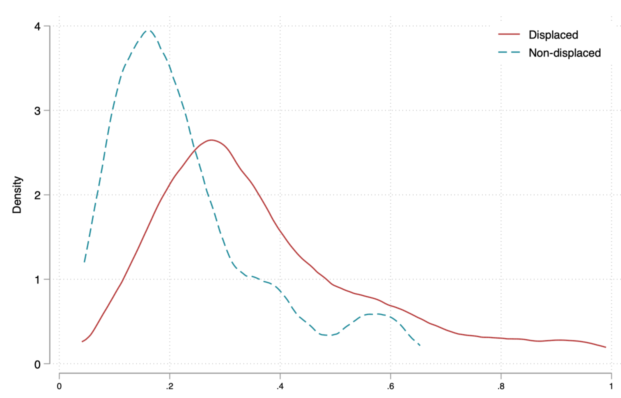
Notes: Each observation is a slum-project pair. Some slum families were sent to more than one different project. Summary statistics for all slums reported in Morales and Rojas (1986) that I was able to geolocate. Difference (3,7,9) corresponds to a regression of each attribute on a dummy that indicates a slum was found in the archival records, conditional on municipality of origin. Column (10) correspond to the double difference between found and displaced, conditional on municipality of origin. Clustered standard errors at the level of municipality of origin. 10%*, 5%**, 1%***. Morales and Rojas (1986) only include slums in the urban municipalities, and in their data the share of displaced families is smaller than in Molina (1986) because they report more non-displaced slums not found in other sources.

Table 3.3: Probability of finding a slum in archival data

Outcome	1[Found in Archives]					
	(1)	(2)	(3)	(4)	(5)	(6)
Displaced	0.156**	0.151**	0.149**	0.375***	0.401***	0.897
	(0.056)	(0.058)	(0.057)	(0.109)	(0.101)	(0.531)
Families (/100)	0.034**	0.031**	0.026**	0.022*	0.021*	0.018
	(0.012)	(0.012)	(0.010)	(0.011)	(0.011)	(0.012)
Distance to subway (origin)		-0.020*	-0.016	-0.001	0.003	-0.000
		(0.010)	(0.018)	(0.013)	(0.022)	(0.021)
Characteristics at destination						
Schooling				0.028	0.030	0.057
				(0.023)	(0.025)	(0.034)
Schools per student				-0.187	-0.162	-0.161
				(0.124)	(0.128)	(0.184)
Distance to subway				-0.028	-0.036*	-0.022
				(0.017)	(0.018)	(0.024)
Families per project (/100)				0.018	0.010	0.012
				(0.013)	(0.010)	(0.011)
Fragmentation (/1000)				0.045***	0.038***	0.044***
				(0.011)	(0.009)	(0.015)
Displaced*schooling$_{dest}$						-0.053
						(0.082)
Displaced*schools$_{dest}$						0.001
						(0.242)
Displaced*subway$_{dest}$						-0.018
						(0.028)
Municipality of origin FE			✓		✓	✓
R^2	0.068	0.088	0.171	0.153	0.236	0.242
Observations	318	318	318	318	318	318
Sample mean	0.31	0.31	0.31	0.31	0.31	0.31

Notes: Each observation corresponds to a slum-destination pair. Data found in archives was harmonized with data in Morales and Rojas (1986).

Figure 3.5: Probability of finding a slum in archival data



Notes: Fitted values correspond to the regression in column (5) in Table A.1

Chapter 4

Appendix and Supplementary Material

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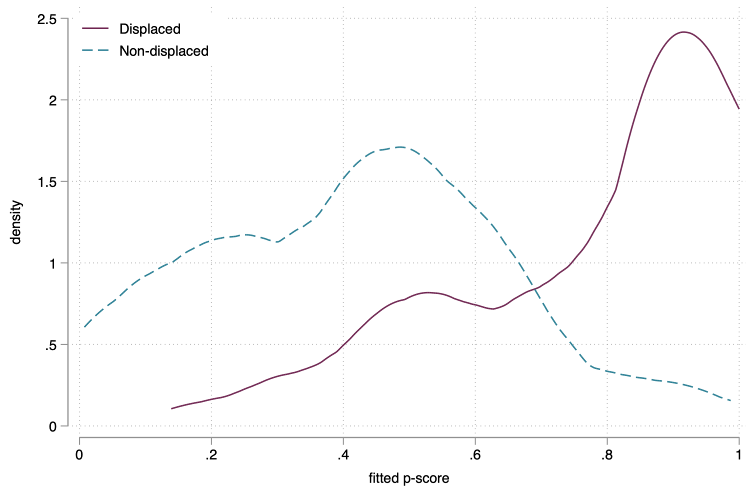
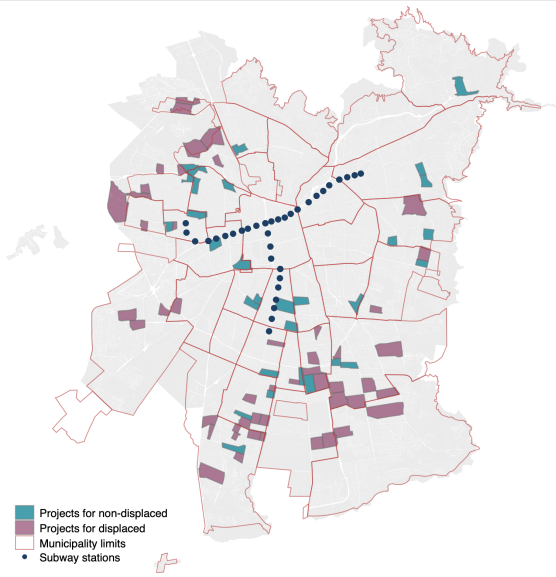
1. Additional Figures and Tables

Figure A.1: Probability of displacement as a function of slums' characteristics

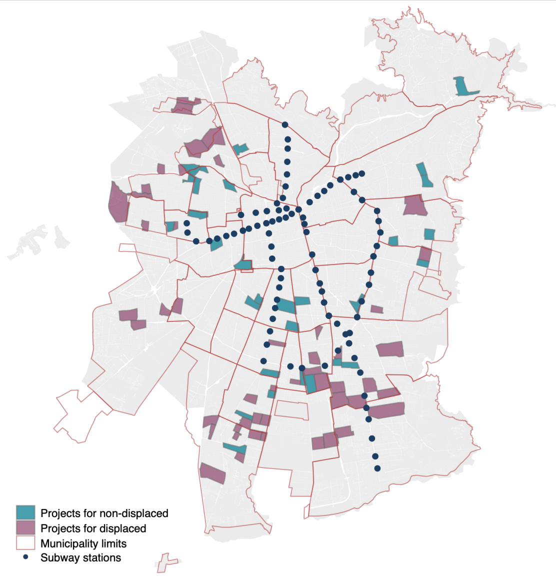


Notes: Figure shows the density of the fitted values of the probability of displacing a slum. Fitted values correspond to the predicted values of a regression that estimates the probability of displacement on slums characteristics: number of families, size of land (in hectares), military name, distance to rivers, and municipality of destination fixed effects. Means are in Table 1.4 Panel A.

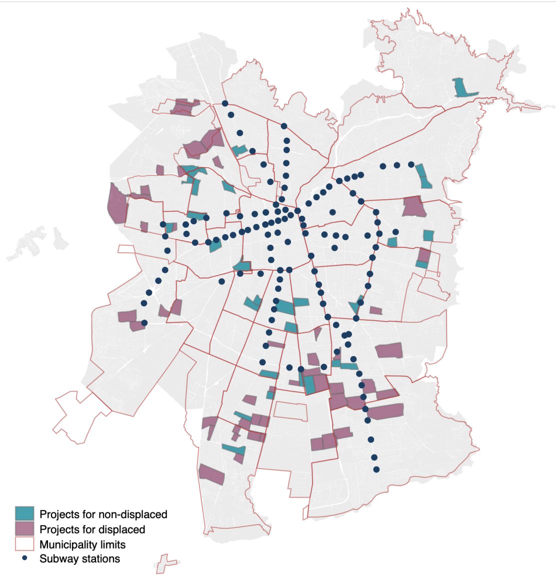
Figure A.2: Location of public housing projects and subway stations



(a) Subway in 1980



(b) Subway in 2006



(c) Subway in 2019

Notes: This figure shows the evolution of subway stations in Greater Santiago since 1980 until 2019. Red lines represent the urban limits of Greater Santiago and its municipalities. Colored areas correspond to neighborhoods created by the Program for Urban Marginality between 1979 and 1985. Purple areas correspond to projects that received displaced families, and green areas correspond to projects for the non-displaced families. Blue circles are the locations of subway stations at each moment in time. The data to construct this map come from MINVU (1979), Molina (1986), FLACSO (1982, 1986), and Metro de Santiago.

Table A.1: Variance Decomposition of outcomes within municipalities

Outcome (source)	Household Income/pc (1978 Empl. Survey) (1)	Schooling (Census 1982) (2)	Household Income/pc (CASEN 1990) (3)	Schooling (CASEN 1990) (4)
Mean	13,281.9	6.97	229,720.8	8.37
Std. Error	3,104.9	0.30	28,717.0	0.35
% Var. due to municip.	28.92	23.5	21.03	22.3
# of municip.	8	51	42	42

Notes:"% Var. due to municip." stands for percentage of the variance of outcome due to variation within municipalities. All outcomes measured for head of households in Greater Santiago. Data sources are 1978 Employment Survey conducted quarterly by University of Chile, Census of Population 1982, and CASEN1990 is the Socioeconomic Characterization Survey of 1990. Census data includes all municipalities. Employment Survey groups municipalities geographically in 8 strata. CASEN includes 42 municipalities. Income measured in Chilean pesos in 2018.

Table A.2: Summary Statistics Full Sample of Families

	Full Sample			Families with Children		
Variables	Displaced mean	Non-displaced mean	Difference (within municip.)	Displaced mean	Non-displaced mean	Difference (within municip.)
Demographics at baseline						
Head of Household age	35.59	37.30	-0.79	33.95	35.61	-0.78*
			(0.64)			(0.44)
Wife age	34.06	35.77	-0.61	32.30	34.05	-0.73*
			(0.67)			(0.42)
Husband age	35.39	37.01	-0.89	34.25	35.77	-0.75
			(0.66)			(0.49)
Female HH	0.35	0.35	0.00	0.31	0.32	-0.01
			(0.02)			(0.02)
Married HH	0.74	0.78	-0.03**	0.78	0.83	-0.03***
			(0.01)			(0.01)
Widowed HH	0.02	0.02	0.00	0.01	0.01	0.00
			(0.00)			(0.00)
Mapuche HH	0.05	0.04	0.02**	0.05	0.05	0.02***
			(0.01)			(0.01)
# Children	2.25	2.30	-0.07	2.67	2.72	-0.04
			(0.06)			(0.08)
No children	0.11	0.11	0.01			
			(0.01)			
Age youngest child	6.14	7.07	-0.45	5.30	6.06	-0.30
			(0.32)			(0.23)
Age oldest child	10.96	12.22	-0.87**	10.16	11.32	-0.73**
			(0.39)			(0.34)
Age of woman at first child	21.7	22.32	-0.07	21.7	22.32	-0.07
			(0.24)			(0.24)
Demographics measured between 2007 and 2019						
Female's schooling (raw)	6.09	6.39	-0.54**	6.15	6.46	-0.50**
			(0.21)			(0.21)
Female's schooling (corrected)	6.10	6.20	-0.34	6.18	6.26	-0.30
			(0.25)			(0.25)
Male's schooling (raw)	6.61	6.99	-0.46**	6.65	7.07	-0.43**
			(0.20)			(0.20)
Male's schooling (corrected)	6.71	6.51	-0.07	6.75	6.65	-0.07
			(0.29)			(0.24)
Share HH in RSH	0.71	0.74	-0.04**	0.74	0.77	-0.04**
			(0.02)			(0.02)
Observations	13,519	5,468	18,987	10,942	4420	15,362

Notes: Within difference correspond to the coefficient of *displaced* in equation (1) conditional on municipality of origin and year of intervention. Clustered standard errors at the municipality of origin level. 10%*, 5%**, 1%***. Marital Status of married and widowed are computed conditional on finding a marriage certificate or spouse's death certificate.

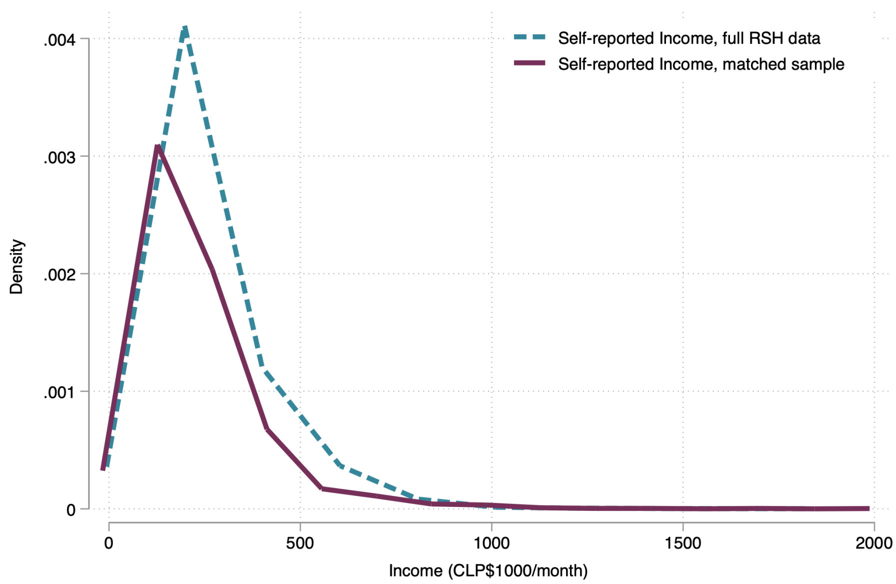
Table A.3: Summary statistics for children at the time of intervention by gender

	Women 0 to 18		Men 0 to 18	
	Non-displaced mean	Difference (within municip)	Non-displaced mean	Difference (within municip)
Age	8.59	-0.19	8.70	-0.46*
		(0.27)		(0.27)
First Born	0.35	0.01	0.37	0.01
		(0.01)		(0.02)
# Siblings	2.76	0.14	2.74	0.11
		(0.11)		(0.14)
HH age	35.79	-0.45	35.81	-0.59
		(0.36)		(0.47)
Mother age at birth	25.00	-0.22	24.97	-0.14
		(0.12)		(0.22)
Female HH	0.30	0.003	0.29	0.002
		(0.023)		(0.02)
Married HH	0.85	-0.07***	0.84	-0.047***
		(0.02)		(0.014)
Widowed HH	0.01	0.001	0.01	0.002
		(0.003)		(0.002)
Mapuche HH	0.04	0.02**	0.05	0.017**
		(0.01)		(0.008)
Mother's schooling	6.01	-0.36	6.03	-0.19
		(0.24)		(0.21)
Individuals	18,963		18,926	
Families	11,581		11,699	

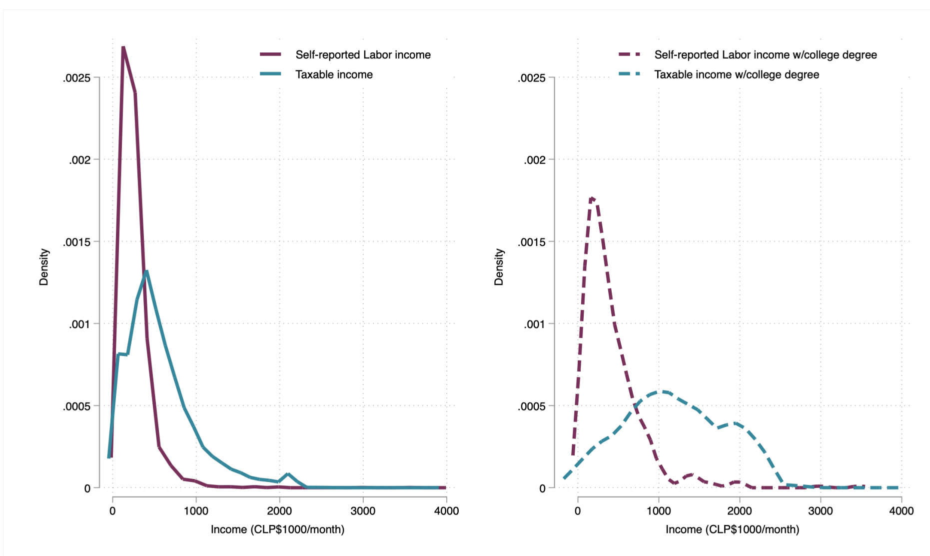
Notes: Within difference corresponds to the coefficient of *displaced* in equation (1) conditional on municipality of origin and year of intervention fixed effects. Marital status categories of *married* and *widowed* are computed conditional on finding a marriage and/or death certificate. Clustered standard errors by municipality of origin in parenthesis. 10%*, 5%**, 1%***.

Figure A.3: Labor Income distribution across different samples

(a) Labor Income distribution in the RSH and matched sample



(b) Labor Income distribution in matched sample



Notes: Income data in year 2018. Matched sample stands for children aged 0 to 18 at baseline who were matched with the RSH data and who are 18 or older in 2018. "Full RSH" corresponds to all individuals aged 18 to 60 in the RSH in year 2018.

Table A.4: Assignment Location Attributes and *Displaced* Families' Characteristics at Baseline

Location Atributtes	Adults' Years of Schooling (1)	Unempl. rate (2)	# schools/ 1000 stud. (3)	# Pub. schools/ 1000 stud. (4)	# Priv. schools/ 1000 stud. (5)	# Primary Care Cent./1000HH (6)	# Hospitals/ 1000HH (7)	Distance to Subway (8)	Distance from Origin (9)
Panel A									
HH's age	0.001	-0.000	-0.003	-0.002	-0.003	-0.003*	-0.003	-0.001	-0.001
	(0.001)	(0.001)	(0.003)	(0.002)	(0.004)	(0.002)	(0.002)	(0.001)	(0.001)
Female HH	0.007	0.011	0.031	0.026	0.042	0.019	0.013	0.004	0.012
	(0.025)	(0.021)	(0.036)	(0.031)	(0.045)	(0.028)	(0.020)	(0.015)	(0.014)
# children	0.006	-0.002	-0.005	-0.004	-0.006	-0.013**	-0.008**	-0.006**	-0.001
	(0.005)	(0.006)	(0.007)	(0.006)	(0.009)	(0.006)	(0.004)	(0.003)	(0.003)
Married HH	0.008	0.001	-0.024	-0.020	-0.029	0.021	-0.022	0.002	-0.011
	(0.023)	(0.018)	(0.027)	(0.024)	(0.029)	(0.016)	(0.028)	(0.018)	(0.023)
Marst Unknown	0.018	-0.037	-0.013	-0.011	-0.013	0.026*	-0.020	0.023	-0.016
	(0.031)	(0.025)	(0.022)	(0.021)	(0.023)	(0.015)	(0.027)	(0.027)	(0.035)
Mapuche HH	0.022	-0.017	-0.051	-0.042	-0.065	-0.020	-0.021	-0.000	-0.000
	(0.025)	(0.020)	(0.048)	(0.041)	(0.058)	(0.016)	(0.028)	(0.017)	(0.019)
R^2	0.549	0.611	0.505	0.608	0.267	0.654	0.618	0.873	0.746
Observations	13,519	13,519	13,519	13,519	13,519	13,519	13,519	13,519	13,519
Test of joint significance of baseline controls									
F	0.951	1.491	0.452	0.330	0.566	2.286	1.705	2.515	0.245
$p > F$	0.561	0.219	0.809	0.891	0.725	0.068	0.160	0.048	0.939
Panel B									
HH's age	0.002	-0.001	-0.002	-0.002	-0.001	-0.004*	-0.003	-0.001	-0.002
	(0.001)	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)	(0.002)	(0.001)	(0.001)
Female HH	-0.005	0.015	0.029	0.025	0.034	0.036	0.021	0.007	0.008
	(0.029)	(0.024)	(0.034)	(0.029)	(0.044)	(0.024)	(0.018)	(0.013)	(0.014)
# children	-0.055**	0.030	-0.006	0.006	-0.050***	0.025	0.011	-0.002	0.016
	(0.022)	(0.021)	(0.009)	(0.008)	(0.017)	(0.027)	(0.010)	(0.011)	(0.014)
Married HH	-0.002	0.009	-0.013	-0.010	-0.022	0.013	-0.014	0.006	-0.018
	(0.024)	(0.022)	(0.022)	(0.021)	(0.022)	(0.018)	(0.024)	(0.014)	(0.020)
Marst Unknown	0.087	-0.060	-0.003	-0.018	0.058*	-0.035	-0.042	0.025	-0.031
	(0.055)	(0.042)	(0.018)	(0.016)	(0.032)	(0.035)	(0.036)	(0.032)	(0.043)
Mapuche HH	0.015	-0.007	-0.053	-0.043	-0.071	-0.028*	-0.022	-0.007	-0.002
	(0.020)	(0.018)	(0.056)	(0.048)	(0.067)	(0.016)	(0.033)	(0.022)	(0.022)
Mother's Education	-0.005	0.004	0.001	0.001	0.001	0.003	0.001	0.001	0.004**
	(0.004)	(0.003)	(0.003)	(0.002)	(0.004)	(0.003)	(0.002)	(0.001)	(0.002)
R^2	0.556	0.613	0.506	0.609	0.261	0.665	0.613	0.872	0.745
Observations	10,830	10,830	10,830	10,830	10,830	10,830	10,830	10,830	10,830
Test of joint significance of baseline controls + mother's schooling									
F	1.947	0.854	0.520	0.778	3.392	1.611	2.027	1.323	5.746
$p > F$	0.102	0.537	0.789	0.593	0.010	0.175	0.090	0.274	0.000
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year of Intervention FE	✓	✓	✓	✓	✓	✓	✓	✓	✓

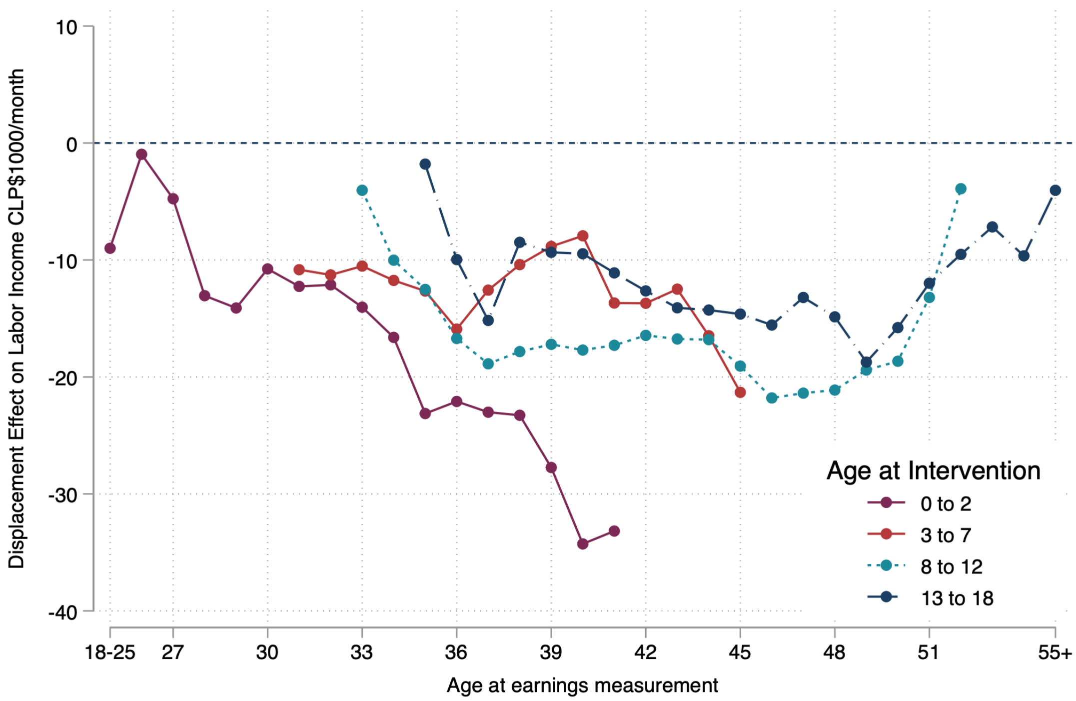
Notes: Clustered standard errors at municipality level. 10%*, 5%**, 1%***. Attributes in columns 1, 2 and 3 are measured at the census district level in 1982, when census data is available, while schools, hospitals and subway measures correspond to data from 1985.

Table A.5: Assignment Location Attributes and *Displaced* Families' characteristics at baseline

Location Atributtes	Adults' Years of Schooling (1)	Unempl. rate (2)	# schools/ 1000 stud. (3)	# Pub. schools/# 1000 stud. (4)	# Priv. schools/ 1000 stud. (5)	# Primary Care Cent./1000 pers. (6)	# Hospitals/ 1000 pers. (7)	Distance to Subway (8)	Distance from Origin (9)
Panel A									
HH's age	0.000	0.000	0.000	0.000	0.000	-0.000	0.000	-0.000**	-0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Female HH	-0.019*	0.022**	0.007	0.008	0.000	0.007*	-0.003	0.001	0.022**
	(0.010)	(0.010)	(0.010)	(0.007)	(0.018)	(0.004)	(0.002)	(0.001)	(0.010)
# children	0.004*	-0.003	-0.001	-0.001	0.000	-0.002*	0.000	-0.001	0.004**
	(0.002)	(0.002)	(0.002)	(0.001)	(0.004)	(0.001)	(0.001)	(0.000)	(0.001)
Married HH	-0.007	0.007	-0.013**	-0.009**	-0.024*	0.005	0.003*	0.001	-0.010
	(0.010)	(0.009)	(0.005)	(0.004)	(0.012)	(0.005)	(0.001)	(0.000)	(0.008)
Marst Unknown	-0.011	-0.001	-0.006	-0.004	-0.012	0.004	0.002	0.000	-0.016*
	(0.014)	(0.016)	(0.010)	(0.009)	(0.015)	(0.009)	(0.002)	(0.001)	(0.009)
Mapuche HH	0.013	-0.020*	-0.023**	-0.017**	-0.040**	-0.016**	0.006**	-0.001	0.013
	(0.010)	(0.010)	(0.009)	(0.007)	(0.015)	(0.007)	(0.002)	(0.002)	(0.008)
R^2	0.880	0.897	0.909	0.944	0.754	0.973	0.995	0.999	0.916
Observations	13,519	13,519	13,519	13,519	13,519	13,519	13,519	13,519	13,519
Test of joint significance of baseline controls									
F	1.245	1.735	2.020	1.958	1.725	1.345	1.628	1.137	1.596
$p > F$	0.310	0.153	0.101	0.110	0.156	0.269	0.179	0.360	0.188
Panel B									
HH's age	0.001	-0.000	0.001**	0.000	0.003*	0.000	-0.000**	-0.000**	0.000
	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Female HH	-0.020	0.023**	0.004	0.007	-0.008	0.008*	-0.002	0.001	0.019**
	(0.012)	(0.011)	(0.008)	(0.005)	(0.018)	(0.005)	(0.002)	(0.001)	(0.008)
# children	-0.025	0.011	-0.013*	-0.003	-0.045**	0.002	0.003*	-0.000	0.001
	(0.015)	(0.008)	(0.007)	(0.005)	(0.022)	(0.003)	(0.002)	(0.001)	(0.006)
Married HH	-0.002	0.005	-0.009**	-0.006*	-0.016	0.004	0.002*	0.001*	-0.010
	(0.011)	(0.010)	(0.004)	(0.004)	(0.010)	(0.006)	(0.001)	(0.001)	(0.009)
Marst Unknown	0.036	-0.023	0.012	-0.001	0.063*	-0.001	-0.002	0.001	-0.006
	(0.026)	(0.021)	(0.011)	(0.007)	(0.034)	(0.012)	(0.002)	(0.002)	(0.008)
Mapuche HH	0.007	-0.015*	-0.018	-0.011	-0.038**	-0.013**	0.006**	-0.001	0.002
	(0.008)	(0.008)	(0.012)	(0.010)	(0.017)	(0.005)	(0.003)	(0.002)	(0.008)
Mother's Education	-0.002	0.002	0.002	0.001	0.003*	0.002*	-0.000	0.000	-0.000
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.000)	(0.000)	(0.001)
R^2	0.883	0.899	0.909	0.944	0.751	0.973	0.995	0.999	0.917
Observations	10,830	10,830	10,830	10,830	10,830	10,830	10,830	10,830	10,830
Test of joint significance of baseline controls + mother's schooling									
F	1.058	1.460	1.155	1.171	1.671	1.255	1.129	1.005	1.767
$p > F$	0.407	0.222	0.354	0.345	0.159	0.319	0.367	0.438	0.137
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year of Intervention FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year of Destination FE	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Clustered standard errors by municipality of origin. 10%*, 5%**, 1%***. Attributes in columns 1 and 2 are measured at the census district level in 1982, when census data is available, while schools, hospitals and subway measures correspond to data from 1985.

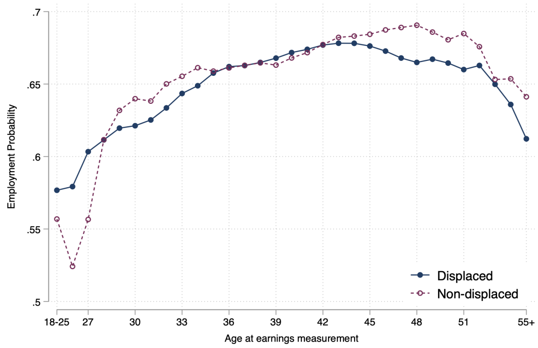
Figure A.4: Displacement Effect on Earnings by Age at Earnings Measurement and Cohort



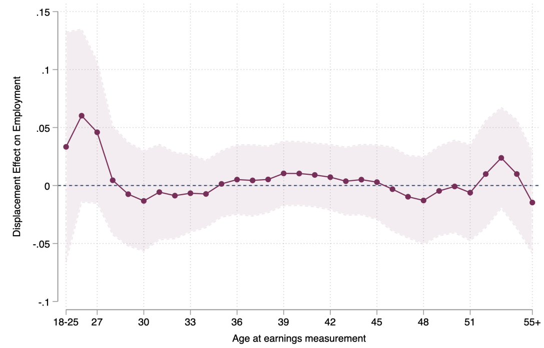
Notes: Regressions for children aged 0 to 18 at baseline and matched to the RSH data that report non-missing schooling. Clustered standard errors by municipality of origin. Controls include: female, mother head of household, married head of household, number of siblings, first-born dummy, head of household's marital status unknown, and year of birth fixed effects. Figure plots coefficients β_τ and their 95% confidence intervals from the regression: $y_{it} = \sum_{\tau=18}^{55} \beta_\tau Displaced * 1[Age = \tau] + \sum_{\tau=18}^{55} \delta_\tau 1[Age] + \psi_o + X_{it}'\gamma + u_{it}$, stratified by four groups by the age at intervention.

Figure A.5: Displacement effect on labor market outcomes by Age at Earnings Measurement

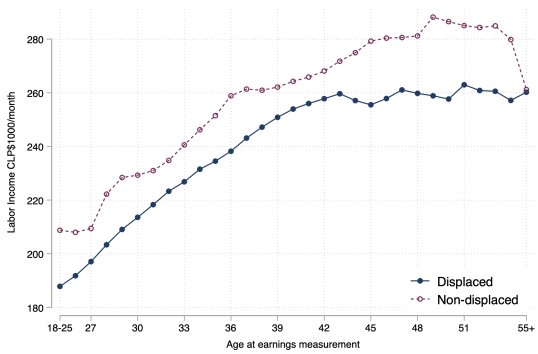
(a) Employment Trajectories



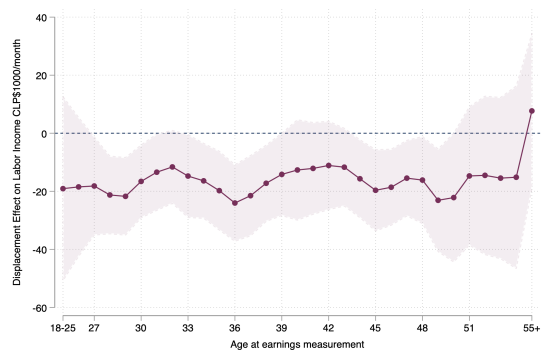
(b) Displacement Effect on Employment



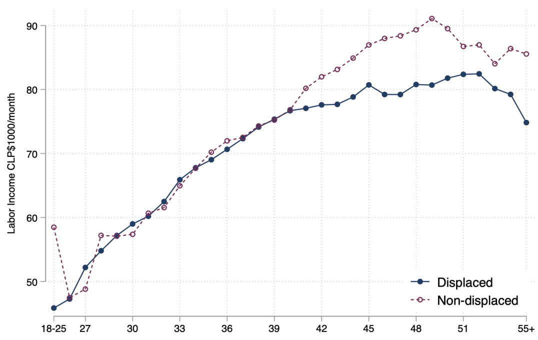
(c) Labor Income Trajectories, formal employment



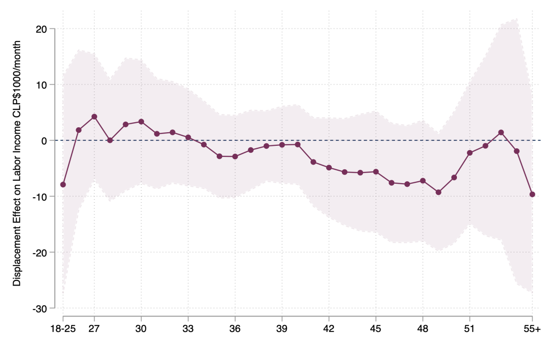
(d) Displacement Effect on Formal Labor Income



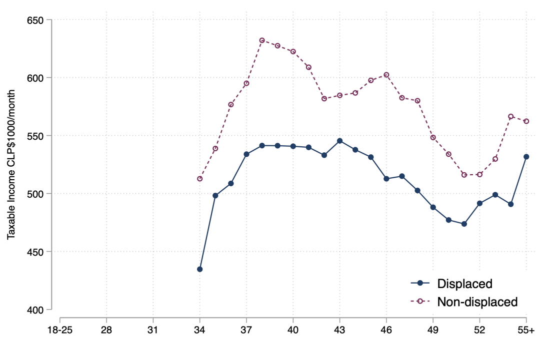
(e) Labor Income Trajectories, informal employment



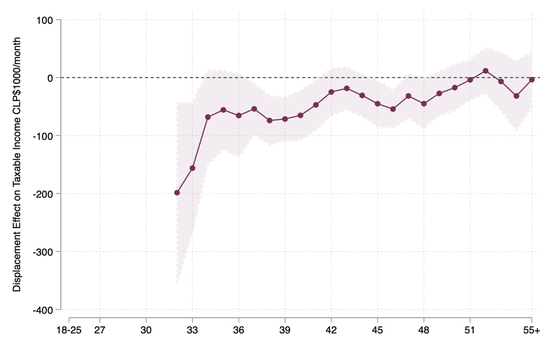
(f) Displacement Effect on Informal Labor Income



(g) Taxable Income Trajectories



(h) Displacement Effect on Taxable Income



Notes: Regressions for children who were 0 to 18 years old at baseline and matched to the RSH data that report non-missing schooling. Clustered standard errors by municipality of origin. Controls include: female, mother head of household, married head of household, number of siblings, first-born dummy, head of household's marital status unknown, and year of birth fixed effects. Figure (b) plots coefficients β_τ and their confidence intervals from regression: $y_{it} = \sum_{\tau=18}^{55} \beta_\tau Displaced * 1[Age = \tau] + \sum_{\tau=18}^{55} \delta_\tau 1[Age] + \psi_o + X_{it}'\gamma + u_{it}$. Figure (a) plots the predicted trajectories for the displaced and non-displaced children between ages 18 to 55 from the previous regression.

Table A.6: Displacement Effect on Demographics

Outcome	Married	Parent	Teen parent	# Children	Age at marriage
	(1)	(2)	(3)	(4)	(5)
Panel A: Full Sample N=39,645					
Displaced	-0.006	0.019***	0.057***	0.100***	-0.097
	(0.009)	(0.004)	(0.013)	(0.028)	(0.135)
Non-displaced mean	0.66	0.86	0.34	2.42	24.67
% Variation w.r.t. non-disp.	**0.9**	**2.2**	**16.8**	**4.1**	**-3.9**
R^2	0.061	0.027	0.090	0.039	0.047
Panel B: Women N=19,870					
Displaced	-0.008	0.015*	0.083***	0.173***	-0.309**
	(0.013)	(0.008)	(4.059)	(0.037)	(0.150)
Non-displaced mean	0.69	0.89	0.44	2.42	23.67
% Variation w.r.t. non-disp.	**-1.2**	**1.7**	**18.9**	**7.1**	**-1.3**
R^2	0.046	0.012	0.042	0.044	0.013
Panel C: Men N=19,775					
Displaced	-0.004	0.024***	0.030***	0.021	0.144
	(0.011)	(0.007)	(0.014)	(0.021)	(0.222)
Non-displaced mean	0.63	0.83	0.24	2.42	25.76
% Variation w.r.t. non-disp.	**-0.6**	**2.9**	**12.5**	**0.9**	**0.6**
R^2	0.069	0.019	0.018	0.037	0.014
Municipality of origin FE	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓

Notes: Regressions for children of aged 0 to 18 at baseline. # of Children is computed for parents only, number of individuals is 34,434 (18,103 women and 16,331 men). Age at first marriage is computed conditional on having a marriage certificate, number of individuals is 24,824 (13,232 women and 11,592 men). Clustered standard errors by municipality of origin in parenthesis. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include: female, mother head of household, married head of household, head of household's marital status unknown, age of mother at birth, number of siblings, birth order, and year of birth fixed effects.

Table A.7: Displacement Effect on Welfare Use

Outcome	1[On Welfare]			Subsidy CLP$/month		
	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Full	Women	Men	Full	Women	Men
Displaced	0.031***	0.056***	0.003	21.727**	24.555**	10.118
	(0.011)	(0.018)	(0.008)	(9.830)	(11.285)	(29.439)
R^2	0.138	0.044	0.010	0.019	0.021	0.017
Observations	370,584	200,076	170,508	100,047	82,430	17,617
Individuals	30,882	16,673	14,209	16,540	11,545	4,995
Municipality of origin FE	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓

Notes: Regressions for children aged 0 to 18 at baseline and matched with the RSH data. Clustered standard errors at the municipality of origin level in parenthesis. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects and semester of income reporting fixed effects. Baseline controls include: female, mother head of household, married head of household, head of household's marital status unknown, age of mother at birth, number of siblings, birth order, and year of birth fixed effects.

Table A.8: Attrition Measures: Children and Adults with an address in 2016

Address is in:	Greater Santiago (1)	Same Municipality (2)	Same Neighborhood (3)	Neighboring Municipality (4)	Municipality of Origin (5)
Panel A. Children aged 0 to 18 at baseline					
Displaced (N=24,242)	0.888	0.584	0.425	0.089	0.026
Non-displaced (N=10,245)	0.888	0.581	0.409	0.091	0.581
Within Difference	-0.005	-0.016	0.003	-0.015	-0.586***
	(0.010)	(0.028)	(0.036)	(0.015)	(0.026)
Panel B. Head of Households					
Displaced (N=9,384)	0.889	0.673	0.606	0.054	0.027
Non-displaced (N=4,067)	0.884	0.705	0.574	0.052	0.705
Within Difference	0.005	-0.021	0.041	-0.010	-0.703***
	(0.008)	(0.029)	(0.048)	(0.013)	(0.026)

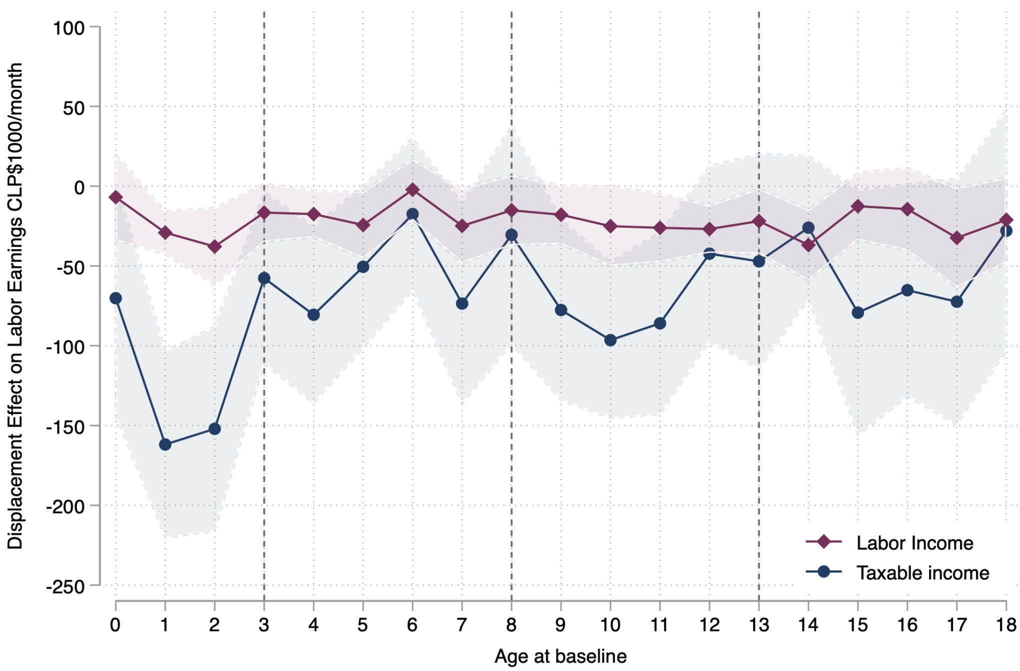
Notes: Individuals with a valid address in 2016. The outcomes correspond to the probability that place of residence is located in each of the geographic units listed in the first row of this table. Within difference corresponds to a regression of each outcome on a displacement dummy, conditional on year of intervention fixed effects and municipality of origin fixed effects. Clustered standard errors by municipality of origin in parenthesis. 10%*, 5%**, 1%***.

Table A.9: Displacement Effect between and within municipalities

Outcome	Labor Income (1)	1[Employed] (2)	Years of Schooling (3)
Displaced within same munic.	-15.338*	-0.002	-0.559***
	(7.642)	(0.017)	(0.125)
Displaced to different munic.	-15.301**	0.004	-0.688***
	(6.078)	(0.015)	(0.160)
R^2	0.127	0.108	0.114
Observations	620,329	620,329	30,882
Municipality of origin FE	✓	✓	✓
Baseline Controls	✓	✓	✓

Notes: Regressions for children of ages 0 to 18 at baseline and matched to the RSH data. Clustered standard errors at the municipality level. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include: female, mother head of household, married head of household, head of household's marital status unknown, age of mother at birth, number of siblings, birth order, and year of birth fixed effects.

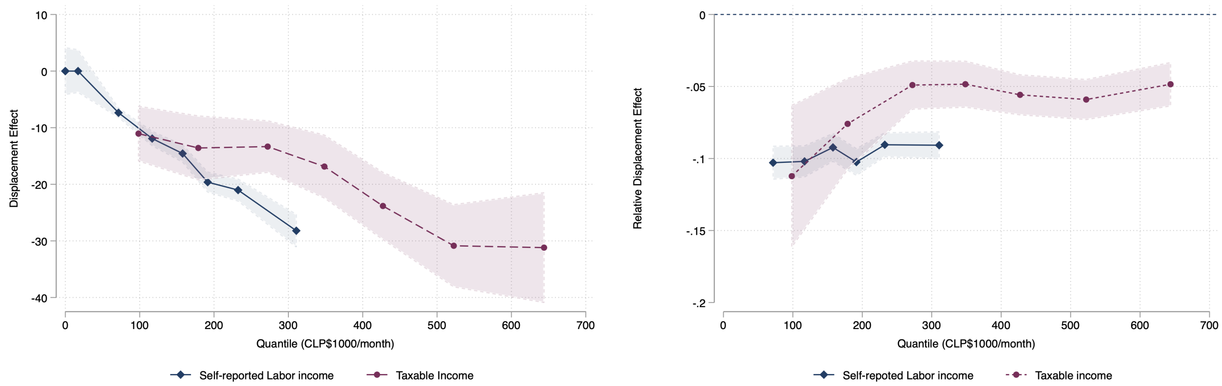
Figure A.6: Structural break test by age at intervention



Notes: Regressions for children who were 0 to 18 years old the time of intervention and matched with the RSH or the GRIS data. Clustered standard errors by municipality of origin. Controls include: female, mother head of household, single head of household, number of siblings, mapuche last-name, cohort fixed effects, and time fixed effects. The figure plots the displacement coefficient and its 95% confidence interval resulting from estimating equation (1) stratified by age at intervention. Dotted vertical lines indicate that the p-value of the structural break test at the corresponding age is smaller than 0.1.

Figure A.7: Quantile Treatment Effects for children age 0 to 18 at baseline

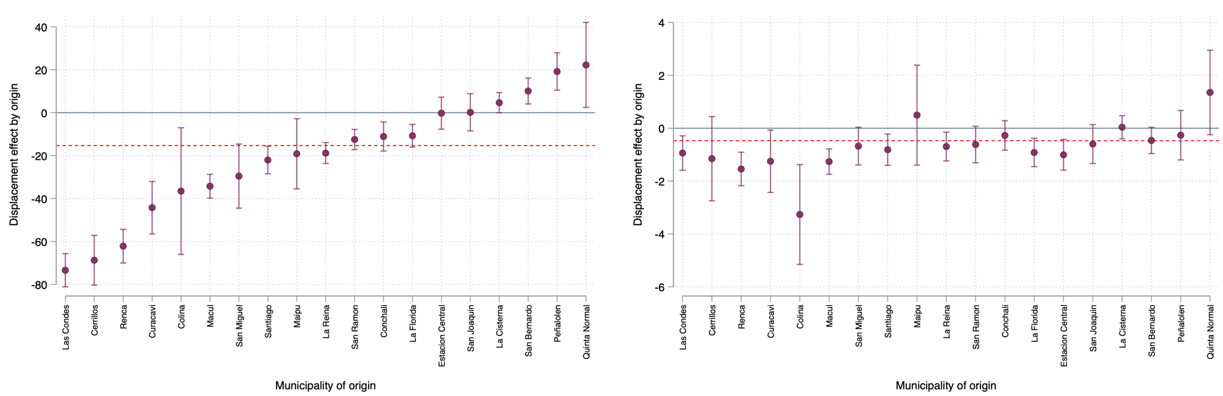
(a) Displacement Effect on Labor Income (1,000 CLP$/month) **(b)** Relative Displacement Effect on Labor Income



Notes: Quantile displacement effects and their 95% confidence intervals. Regressions for children who were 0 to 18 years old the time of treatment and matched to the RSH or the GRIS data. Clustered standard errors at the municipality level. Controls include: female, mother head of household, single head of household, number of siblings, mapuche last-name, cohort fixed effects, year of intervention fixed effects, and semester of income measurement fixed effects.

Figure A.8: Distribution of Displacement Effect by Municipality of Origin

(a) Labor Earnings **(b)** Years of Education



Notes: Figures plot displacement coefficients on earnings (left) and years of schooling (right) stratified by municipality of origin. Coefficients are estimated using the following regression: $y_{it} = \sum_{o=1}^{20} \beta_o Displaced * 1[Origin = o] + X'_{iot}\gamma + u_{iot}$, where o indexes the municipality of origin for child i. Red dotted lines correspond to the average displacement effects on income and schooling, correspondingly.

Table A.10: Displacement Effect on Labor Market Outcomes

Outcome	Labor Income	Employed	Has a Contract	Temp. Worker	Taxable Income	Formal Income	Informal Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Full Sample							
Displaced	-15.314**	0.002	-0.041***	0.039**	-37.463**	-16.047***	0.733
	(6.098)	(0.014)	(0.012)	(0.016)	(14.317)	(4.715)	(1.879)
Non-displaced mean	155.24	0.67	0.41	0.56	581.35	108.10	46.15
% Variation w.r.t. non-disp.	-9.9	0.2	-10.0	7.0	-6.4	-14.7	1.6
Observations	620,329	620,329	620,329	620,329	115,841	620,329	620,329
Individuals	30,882	30,882	30,882	30,882	20,806	30,882	30,882
Panel B. Women							
Displaced	-15.862**	-0.011	-0.054***	0.041**	-42.783**	-17.378**	1.515
	(7.166)	(0.020)	(0.019)	(0.020)	(17.625)	(6.662)	(1.873)
Non-displaced mean	108.54	0.55	0.32	0.65	522.64	76.74	31.80
% Variation w.r.t. non-disp.	-14.6	-2.0	-16.9	6.2	-8.2	-22.6	4.5
Observations	361,203	361,203	361,203	361,203	54,157	361,203	361,203
Individuals	16,673	16,673	16,673	16,673	9,155	16,673	16,673
Panel C. Men							
Displaced	-14.303**	0.021*	-0.022	0.035**	-31.326*	-13.915***	-0.388
	(5.649)	(0.012)	(0.013)	(0.016)	(17.154)	(4.242)	(3.622)
Non-displaced mean	220.14	0.84	0.54	0.44	633.89	154.06	66.08
% Variation w.r.t. non-disp.	-6.5	2.5	-4.1	8.0	-4.9	-9.0	-0.6
Observations	259,126	259,126	259,126	259,126	61,684	259,126	259,126
Individuals	14,209	14,209	14,209	14,209	11,651	14,209	14,209
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓	✓

Notes: Regressions for children of aged 0 to 18 at baseline matched with the RSH data that report non-missing schooling. Clustered standard errors by municipality of origin (42 clusters). 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects and semester of income reporting fixed effects. Baseline controls include: female, mother head of household, married head of household, head of household's marital status unknown, age of mother at birth, number of siblings, birth order, and year of birth fixed effects. Number of individuals with taxable income data is 26,517.

Table A.11: Displacement Effect on Schooling Outcomes by Gender

Outcome	Years of Schooling			1[HS graduate]	1[2y College]	1[5y College]
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Women						
Displaced	-0.657***	-0.488***	-0.325	-0.085***	-0.011	-0.016**
	(0.153)	(0.129)	(0.199)	(0.018)	(0.012)	(0.007)
Non-displaced mean	11.43	11.48	11.48	0.68	0.13	0.05
% Variation w.r.t. non-disp.	-5.3	-4.8	-2.8	-15.7		-22.5
R^2	0.131	0.135	0.157	0.121	0.034	0.048
Individuals	16,673	14,422	14,422	14,422	14,422	14,422
Panel B. Men						
Displaced	-0.609***	-0.459***	-0.367**	-0.074***	-0.035***	-0.015**
	(0.142)	(0.123)	(0.139)	(0.018)	(0.011)	(0.006)
Non-displaced mean	11.31	11.34	11.34	0.65	0.11	0.06
% Variation w.r.t. non-disp.	-5.1	-5.6	-3.2	-14.8	-36.4	-33.3
R^2	0.116	0.142	0.147	0.112	0.033	0.036
Individuals	14,209	12,449	12,449	12,449	12,449	12,449
Municipality of origin FE	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓
Mother's Schooling		✓	✓	✓	✓	✓
Municipality of destination FE			✓			

Notes: Regressions for children aged 0 to 18 at baseline matched with the RSH data that report non-missing schooling. Clustered standard errors by municipality of origin. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include: female, mother head of household, married head of household, head of household's marital status unknown, age of mother at birth, number of siblings, birth order, and year of birth fixed effects.

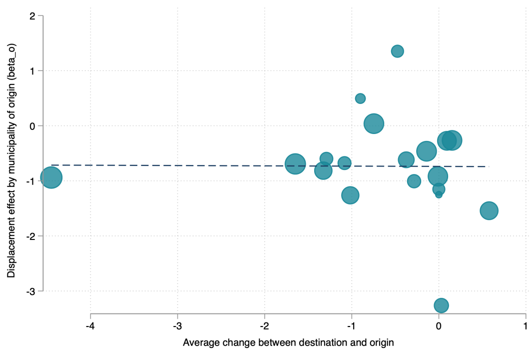
Table A.12: Comparison of earnings estimates across studies

Study	Setting	% Δ Earnings	% Δ Neighborhood Quality	$\lvert Elasticity \rvert$
	(1)	(2)	(3)	(4)
Chetty et al. (2016) (1)	MTO (children 7-13 in Exp. group)	+14%	-34% (Poverty)	0.41
Chyn (2018) (2)	Public Demolition in Chicago (children 7-18)	+16%	-22.2% (Poverty)	0.72
Barnhardt et al. (2016) (3)	Housing Lottery Ahmedabad (adults in India)	-14.5%	-37.5% (Urbanicity)—-8.1% (Housing Value)	0.38-1.8
This paper (4)	Program for Urban Marginality (children 0-18 in Chile)	-9.9%	-9.5% (Schooling)	1.04

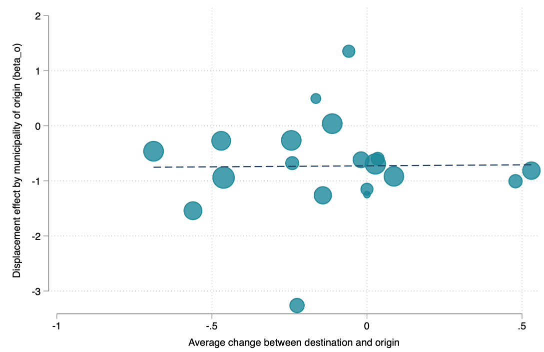
Notes: (1) Tables 2 and 3; (2) Tables 2 and 3; (3) Tables 5 and 6; (4) Tables 4 and 5.

Figure A.9: Displacement effect on years of schooling by municipality of origin and changes in location attributes

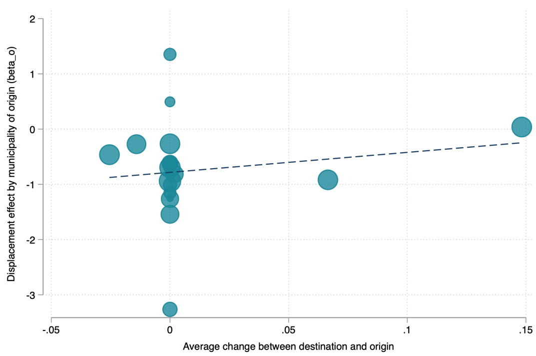
(a) Change in population's schooling



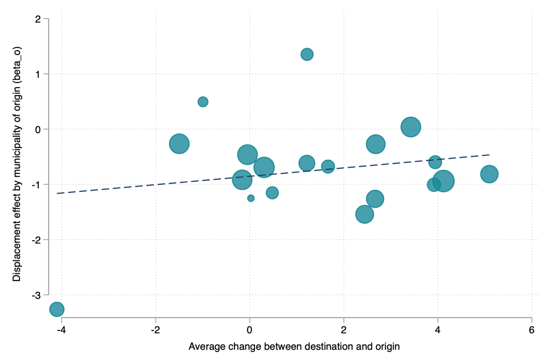
(b) Change in number of schools per student



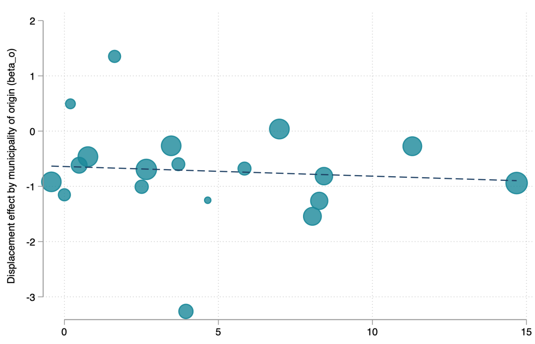
(c) Change in number of hospitals per household



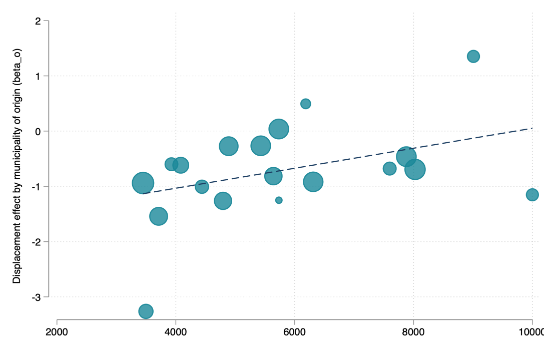
(d) Change in distance to subway



(e) Change in commuting time



(f) Degree of neighborhood fragmentation (HHI)



Notes: Figures plot displacement coefficients on years of schooling stratified by municipality of origin against average changes in location attributes by municipality of origin. Coefficients are estimated using the following regression: $y_{it} = \sum_{o=1}^{20} \beta_o Displaced * 1[Origin = o] + X'_{iot}\gamma + u_{iot}$, where o indexes the municipality of origin for child i. Changes in attributes (x-axis) are computed as $\bar{\Delta}_o = \sum_{d=1}^{30} \Delta_{iod}$. Regressions for children who were 0 to 18 years old at baseline and matched to the RSH data that report non-missing schooling. Controls include: female, mother head of household, married head of household, number of siblings, first-born dummy, head of household's marital status unknown, and year of birth fixed effects. Full sample includes 42 municipalities of origin; however, in this graph I use only 20 municipalities for which there are enough observations such that I observe displaced and non-displaced children from the same municipality. Coefficients β_o are weighted by the number of observations in each cell.

Table A.13: Comparison of schooling estimates across studies

Study	Setting	% Δ Years of Education	% Δ Neighborhood Quality	$\|Elasticity\|$
	(1)	(2)	(3)	(4)
Chetty et al. (2016) (1)	MTO (children 7-12 in Exp. group)	+15% (College Att.)	-34% (Poverty)	0.44
Chyn (2018) (2)	Public Demolition in Chicago (children 7-18)	-8.1% (HS dropout)	-22.2% (Poverty)	0.36
		28% (College Att.)	-22.2% (Poverty)	1.26
Barnhardt et al. (2016) (3)	Housing Lottery Ahmedabad (children in India)	-2.25% (schooling)	-37.5% (Urbanicity)—-8.1% (Housing Value)	0.06-0.27
This paper (4)	Program for Urban Marginality (children 0-18 in Chile)	-4.1% (schooling)	-9.5% (Schooling)	0.43
		-12.1% (HS grad)	-9.5% (Schooling)	1.27
		-18.3% (College Att.)	-9.5% (Schooling)	1.93

Notes: (1) Tables 2 and 4; (2) Tables 2 and 7; (3) Tables 5 and 6; (4) Tables 4 and 7.

Table A.14: Children's neighborhoods characteristics between 2015 and 2019

Outcome	Greater Santiago	Same UV	Labor Income	Employment	Quintile	Formal Employment	Schooling
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Children Sample, N=207,099, Individuals=27,727							
Displaced	0.010	-0.006	-9.129	0.012***	-0.837*	-0.011	-0.255*
	(0.013)	(0.007)	(5.808)	(0.004)	(0.466)	(0.007)	(0.132)
R^2	0.009	0.015	0.102	0.085	0.101	0.076	0.076
Non-displaced mean	0.87	0.02	211.76	0.64	55.98	0.37	9.62
Panel B. Projects Sample, N=596, Neighborhoods=110							
Displaced			-10.662	-0.077	-1.703	-0.103*	-0.411
			(28.268)	(0.056)	(1.767)	(0.052)	(0.276)
R^2			0.363	0.494	0.509	0.402	0.450
Non-displaced mean			231.08	0.69	54.59	0.41	10.92
Panel C. Greater Santiago Sample, N=26,282, Neighborhoods=2,104							
Displaced			6.484	-0.006	-3.230***	-0.015	0.171
			(18.247)	(0.024)	(0.686)	(0.024)	(0.160)
Non-displaced			-0.137	0.073***	-3.189***	0.054**	0.379**
			(9.926)	(0.021)	(0.907)	(0.021)	(0.148)
R^2			0.253	0.071	0.170	0.038	0.256
Other neighborhoods mean			222.56	0.63	57.22	0.37	10.36

Notes: Characteristics of neighborhoods between 2015 to 2019, neighborhood level is defined in the RSH data. Panel A corresponds to children's current residence. Panel B corresponds to average characteristics of the families that live in neighborhoods for the displaced and non-displaced. Panel C corresponds to average characteristics of the families that live in all neighborhoods in Santiago.

2. Eviction Policies

Table B.1: Characteristics of both program versions

Intervention	**Location**	Property Right	Type of dwelling	Public Services	Cost for family
Non-displaced (1/3) (Urban Renewal)	**Same**	Yes	Starting Kit (*) or Apartment	Yes	25% paid in 15 years
Displaced (2/3) (Evicted)	**New (periphery)**	Yes	Apartment or house	Yes	25% paid in 15 years

(*) A starting kit includes a living room, a bathroom and a kitchen.

Figure B.1: Example of a slum and new neighborhoods

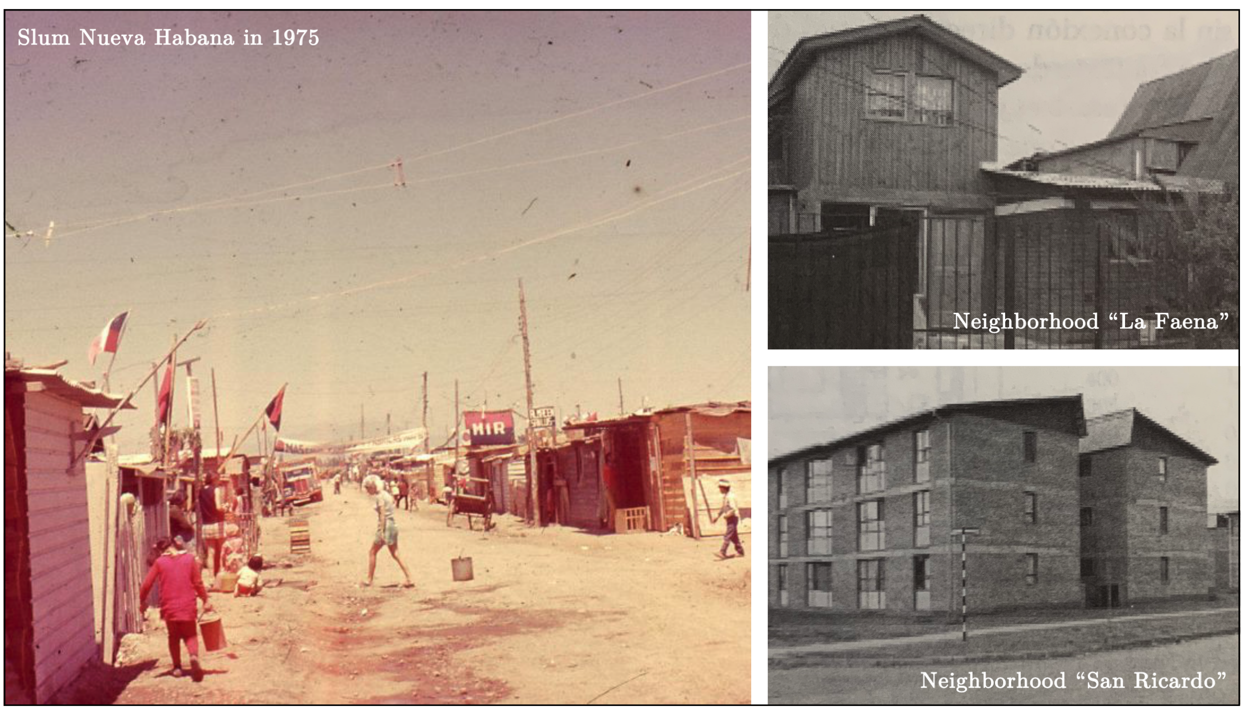
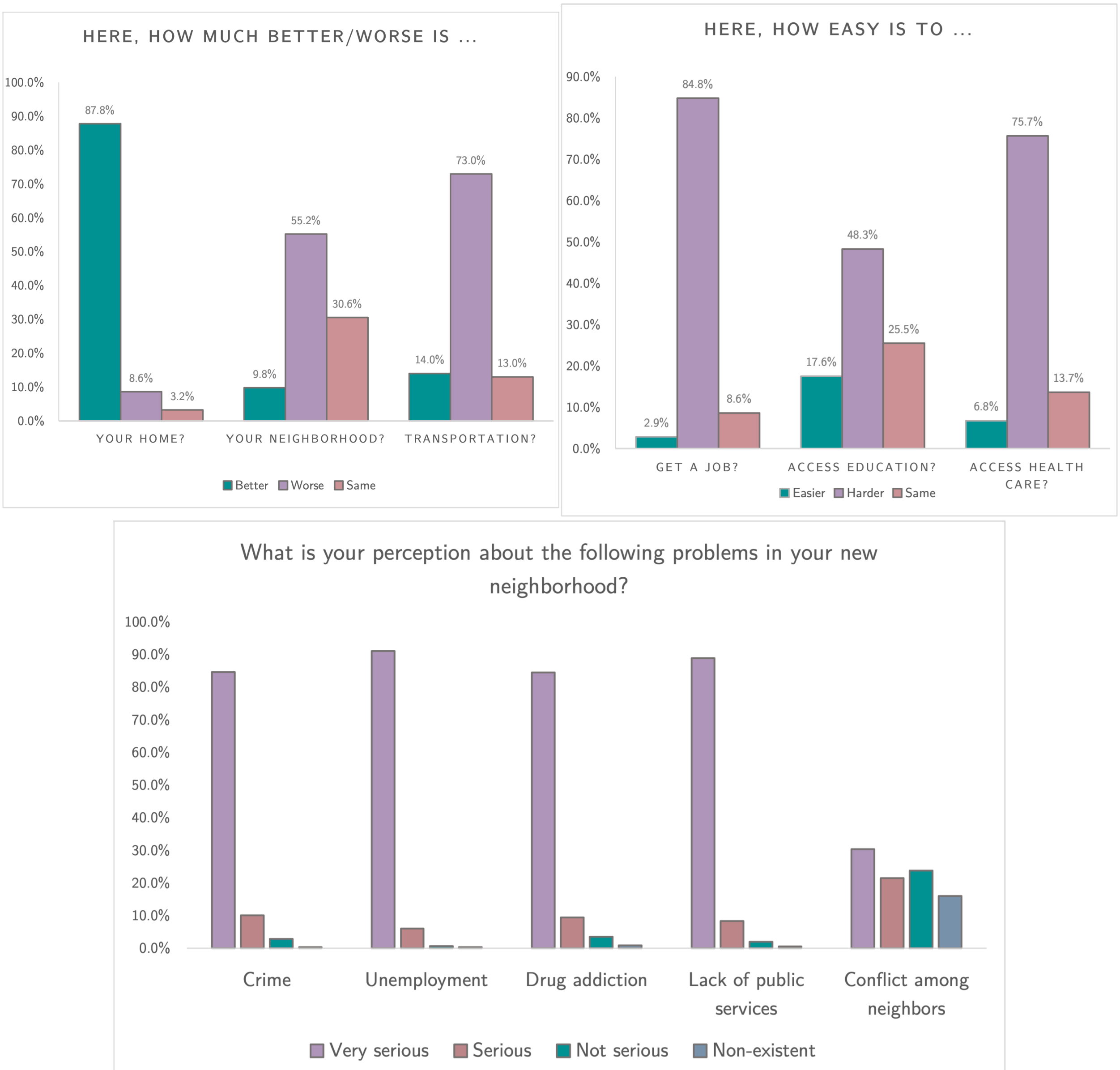


Figure B.2: Evaluation of Evictions Program in 1987 (Aldunate et al., 1987)



(a)]

Notes: Summary of results found by Aldunate et al (1987).

3. Robustness Checks

3.1 Displacement Coefficient and Sensitivity to Omitted Variable Bias

In this appendix section I discuss a sensitivity analysis in my baseline regressions on earnings and years of schooling. my goal is to estimate the degree of selection in unobservable characteristics under different scenarios following the framework of Oster (2019).

Consider the following "short" and "long" regressions, of the form

$$Y_{it} = \alpha + \beta Displaced_{s\{i\}} + \psi_o + \psi_\tau + \varepsilon_{it} \tag{1}$$

$$Y_{it} = \tilde{\alpha} + \tilde{\beta} Displaced_{s\{i\}} + \tilde{\psi}_o + \tilde{\psi}_\tau + X'_{it}\theta + \tilde{\varepsilon}_{it}, \tag{2}$$

where Y_{it} is current outcome for individual i at time t, such as labor income, or years of schooling, $s(i)$ indexes the slum of origin for individual i's family. The variable $Displaced_{s\{i\}}$ takes the value of 1 if an individual's family lived in a displaced slum and 0 otherwise. ψ_o are municipality of origin fixed effects, ψ_τ, are year of intervention fixed effects (1979 to 1985). The matrix X_{it} include baseline controls for individuals' and families' characteristics, such as gender, child's year of birth, female head of household, married head of household, head of household's age, birth-order dummies, and mother's schooling. Under the assumption that X_{it} is uncorrelated with the Displacement, I would expect that $\beta = \tilde{\beta}$.

Following Oster (2019) I can use β, $\tilde{\beta}$ and the sample R^2s from each regression to bound the true displacement effect defined by β^* when all confounders have been taken into account,

$$\beta^* \sim \tilde{\beta} + \delta(\tilde{\beta} - \beta)\frac{R_{max} - \tilde{R}}{\tilde{R} - R}, \tag{3}$$

where R and $\tilde{R}$ are the R^2s from equations (3) and (4) respectively, and R_{max} is the R^2 from the regression that controls for all confounding variables. The coefficient δ is the degree of proportional selection between the unobservable components relative to the observable variables, for example $|\delta| = 1$ implies the degree of selection on unobservables is equally important as the observables.

Then, I use equation (5) to bound the true value for β^*. First, I estimate β, β^*, R and $\tilde{R}$ from equations (3) and (4). Second, I vary the values of δ and R_{max}, I choose $R_{max} = 1.3\tilde{R}$, as recommended by Oster (2019), and I also choose $R_{max} = 5\tilde{R}$ as a more conservative case. Then I vary the value of δ to be 1, 2 or 3. For example, Altonji et al. (2005) assume $\delta = 1$. my results are in D.1.

Table D.1: Displacement Effect under different Assumptions on Selection on Unobservables

Outcome	R^2 max	$\hat{\delta}$	δ	$\hat{\beta}^*$	$\hat{\beta}^*$
	1.3	32.20	1	-14.88	-14.52
	1.3		2	-14.44	-13.86
Labor Earnings	1.3		3	-14.01	-13.18
	5	2.46	1	-9.37	-3.51
	5		2	-3.03	25.81
	5		3	3.73	-261.17
	1.3	190.69	1	-0.643	-0.425
	1.3		2	-0.642	-0.368
Years of Schooling	1.3		3	-0.641	-0.308
	5	15.83	1	-0.628	0.582
	5		2	-0.610	-9.162
	5		3	-0.591	-6.083
Baseline Controls				✓	✓
Mother's schooling					✓

The column labeled as $\hat{\delta}$ reports the estimate for δ for different values of R_{max} and assuming the true value of β^* is equal to 0. The results show that the degree of selection on unobservables would need to be greater than 2 to find a null displacement effect. Putting it differently, under different values of δ that vary between 1 and 3, I find smaller magnitudes for the displacement effect but they never become non-negative. In the only case that I find a positive displacement effect, or very negative effects that are not (economically) plausible, is on years of schooling under the assumption that $R_{max} = 5\tilde{R}$ and when including mother's schooling as a control, which is a very extreme case not even suggested by Oster (2019).

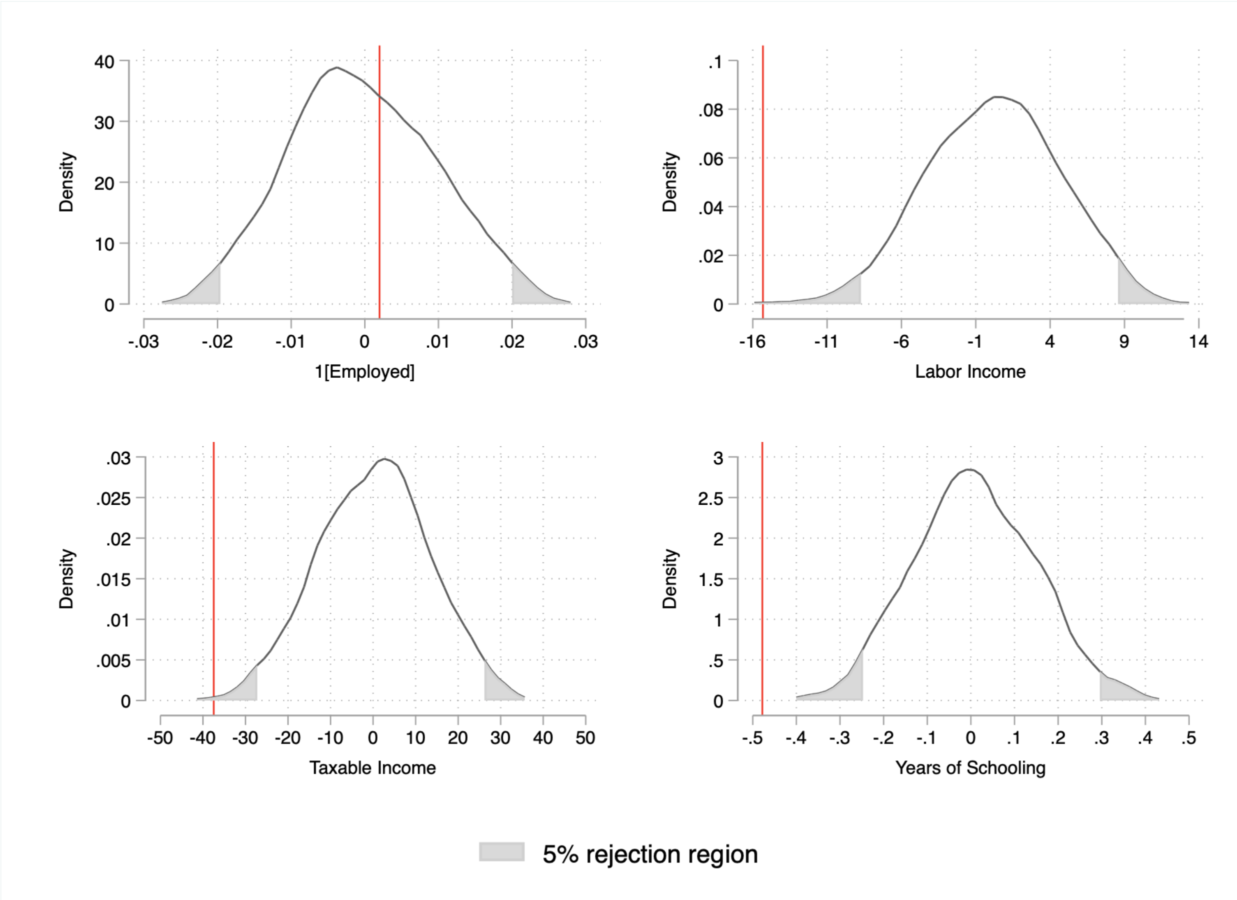
3.2 Alternative Standard Error Estimates

Table D.2: Conley Standard Errors

Outcome	Labor Income	Years of Schooling
Displacement coefficient	-15.134	-0.473
Clustered se by municipality of origin	6.098	0.111
Clustered se by slum	4.122	0.090
Conley se (cutoffs in km)		
2	1.089	0.113
3	1.074	0.115
4	1.098	0.114
5	1.123	0.114
6	1.158	0.113
7	1.199	0.114
8	1.242	0.115
9	1.276	0.115
10	1.305	0.115
11	1.333	0.115
12	1.351	0.116
13	1.364	0.117
14	**1.371**	**0.117**

Notes: This table reports estimates of Conley Standard errors on income and schooling for different distance cutoffs (Conley, 1999). The procedure to estimate the estimates comes from Thiemo Fetzer. For more details see here.

Figure D.1: Results robust to permuting treated clusters



Notes: The figure plots permutation distributions for my main outcomes. I perform 500 replications, in each of them I permute the treated clusters (slums) randomly within municipalities of origin. Red lines correspond to point estimates in my baseline specifications from Tables **??** and **??**, and the gray area correspond to the 5% rejection region.

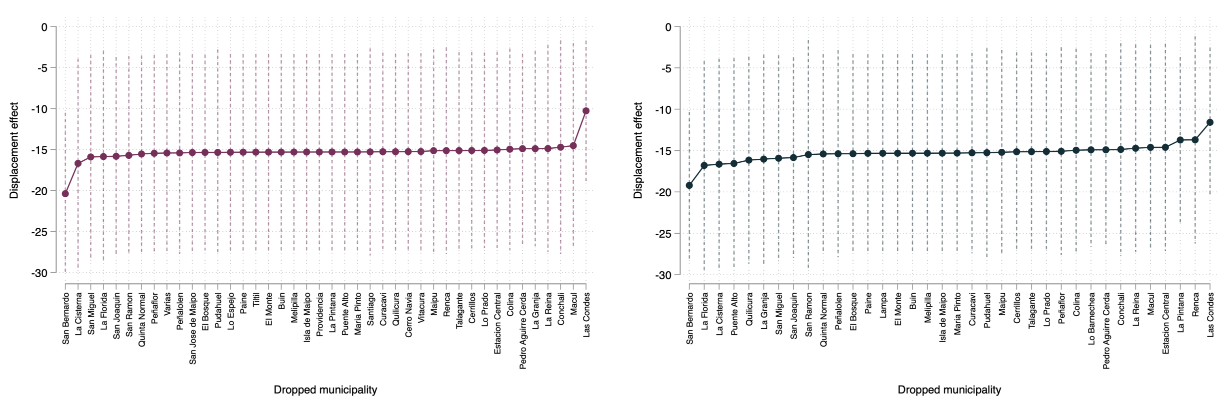
3.3 Other Robustness Checks

Table D.3: Slums' characteristics and non-displaced children earnings

Outcome	Labor Income	Labor Income	Labor Income	Labor Income
	(1)	(2)	(3)	(4)
Size (# families)	0.039		0.025	
	(0.047)		(0.050)	
Area (hectares)	-2.906		-1.240	
	(3.632)		(3.759)	
Military Name	-3.435	0.330	1.049	6.112
	(7.320)	(7.174)	(7.864)	(8.444)
Distance to river	-6.109	2.639	-3.618	8.611
	(3.715)	(6.204)	(4.046)	(7.931)
Density (fam/hect)		0.047		0.007
		(0.075)		(0.093)
Municipality of Origin FE	✓	✓	✓	✓
Mother's schooling			✓	✓
R^2	0.116	0.116	0.112	0.111
p-value joint significance	0.2864	0.917	0.719	0.506
Observations	164,610	156,292	146,139	138,936

Notes: This table reports regressions of labor earnings on a set of slums' characteristics in the sample of non-displaced children. Clustered standard errors at municipality of origin level. 10%*, 5%**, 1%***. All regressions include year of intervention fixed effects. Baseline controls include: female, mother head of household, single head of household, number of siblings, first-born dummy, and cohort fixed effects. P-value of joint significance corresponds to the joint hypothesis that all slum characteristics do no predict the outcome.

Figure D.2: Results robust to dropping each municipality once from sample. Results for Labor Income.



(a) Municipalities of origin

(b) Municipalities of destination

Notes: The figure plots the displacement coefficient from baseline regression in (1) on labor income and its 95% confidence interval, dropping each municipality of origin one by one (panel (a)), or each municipality of destination one by one (panel (b)). Clustered standard errors by municipality of origin. All regressions include year of intervention fixed effects. Baseline controls include: female, mother head of household, single head of household, number of siblings, first-born dummy, and cohort fixed effects.

Table D.4: Results robust to dropping municipalities that only expelled/only received families

	(1) Baseline	(2) W/o muni. expelled	(3) W/o muni. received	(4) W/o both
Outcome:	Labor Income CLP$1,000			
Displaced	-15.314**	-14.779**	-17.528**	-17.698**
	(6.098)	(6.597)	(8.058)	(8.248)
R^2	0.127	0.123	0.132	0.130
N	620,329	409,228	399,293	343,781
Outcome:	1[Employed]			
Displaced	0.002	0.006	-0.003	-0.002
	(0.014)	(0.015)	(0.017)	(0.017)
R^2	0.108	0.109	0.108	0.111
N	620,329	409,228	399,293	343,781
Outcome:	Years of Schooling			
Displaced	-0.643***	-0.641***	-0.820***	-0.833***
	(0.137)	(0.149)	(0.139)	(0.142)
R^2	0.114	0.118	0.128	0.129
N	30,882	20,464	20,042	17,252
Municipality of origin FE	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓

Notes: Regressions for children aged 0 to 18 at baseline and matched to the RSH. Clustered standard errors at the municipality level. 10%*, 5%**, 1%***. All regressions include year of intervention fixed effects. Baseline controls include: female, mother head of household, single head of household, number of siblings, first-born dummy, and cohort fixed effects.

Table D.5: Results robust to controlling for attrition probabilities

	(1) Baseline	(2) Polynomial	(3) Polynomial	(4) Polynomial	(5) Re-weight RSH match. rate
Outcome:			Labor Income		
Displaced	-15.314**	-14.177**	-13.610**	-15.458**	-15.195**
	(6.098)	(6.782)	(6.515)	(5.968)	(6.089)
Non-displaced mean	155.24	155.42	155.42	155.42	165.2
R^2	0.127	0.127	0.127	0.127	0.128
Outcome:			1[Employed]		
Displaced	0.003	0.004	0.005	0.004	0.004
	(0.009)	(0.017)	(0.016)	(0.017)	(0.014)
Non-displaced mean	0.670	0.670	0.670	0.670	0.695
R^2	0.108	0.108	0.108	0.108	0.114
Outcome:			Years of Schooling		
Displaced	-0.643***	-0.633***	-0.614***	-0.639***	-0.640***
	(0.137)	(0.155)	(0.144)	(0.135)	(0.138)
Non-displaced mean	11.37	11.37	11.37	11.37	11.37
R^2	0.114	0.114	0.115	0.115	0.114
P-score polynomial	0	1	2	3	0
Municipality of origin FE	✓	✓	✓	✓	✓
Year of displacement FE	✓	✓	✓	✓	✓

Notes: Regressions for children aged 0 to 18 at baseline and matched to the RSH. Clustered standard errors by municipality of origin. 10%*, 5%**, 1%***. All regressions include year of intervention fixed effects. Baseline controls include: female, mother head of household, single head of household, number of siblings, first-born dummy, and cohort fixed effects. Columns 2 to 4 include as a control an estimate of the probability of finding a slum in the archival data. Column 5 re-weights the data by the inverse the probability of being found in the RSH data as a function of demographics.

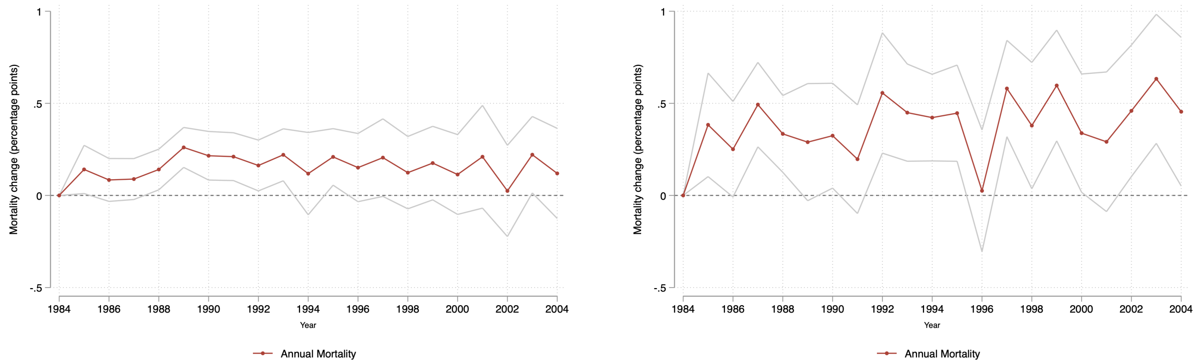
4. Displacement Effect on Other Family Members

Table E.1: Annual Mortality of Adults

Outcome	Mother died (1)	Mother died (2)	Father died (3)	Father died (4)
Panel A.	Full Sample			
Displaced	0.0014	0.0021*	0.0042**	0.0047**
	(0.0009)	(0.0011)	(0.0019)	(0.0019)
R^2	0.0148	0.0149	0.0177	0.0178
Non-displaced mean	0.007	0.007	0.011	0.011
%Var. w.r.t. non-disp.	20.0	30.0	38.2	42.2
Cumulative effect from 1985 to 2019	0.049	0.074	0.153	0.173
Observations	587,062	587,062	478,359	478,359
Individuals	18,080	18,080	15,709	15,709
Panel B.	Households with children			
Displaced	0.0007	0.0010	0.0039**	0.0042**
	(0.0010)	(0.0012)	(0.0018)	(0.0019)
R^2	0.0112	0.0113	0.0153	0.0155
Non-displaced mean	0.006	0.006	0.010	0.010
%Var. w.r.t. non-disp.	11.7	16.7	39.0	42.0
Cumulative effect from 1985 to 2019	0.024	0.035	0.141	0.153
Observations	531,650	531,650	435,527	435,527
Individuals	16,149	16,149	14,122	14,122
Municipality of origin FE	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓
Municipality of destination FE		✓		✓

Notes: Regressions for women and men that became homeowners at the time of intervention. Clustered standard errors by municipality of origin. 10%*, 5%**, 1%***. Outcome is annual mortality, and to account for survival the data is set up as in Deryugina and Molitor (2020). Regression is $Died_{it} = \alpha + \beta Displaced_{s\{i\}} + X_i'\theta + \psi_o + \gamma_t + \varepsilon_{it}$.All regressions include year of intervention fixed effects and calendar year fixed effects from 1985 to 2019. Baseline controls include: marital status, mapuche last-name, head of household dummy, number of children at baseline, and cohort fixed effects. Columns 2 to 4 include as a control an estimate of the probability of finding a slum in the archival data.

Figure E.1: Annual Mortality of parents 1985 to 2004



(a) Mothers

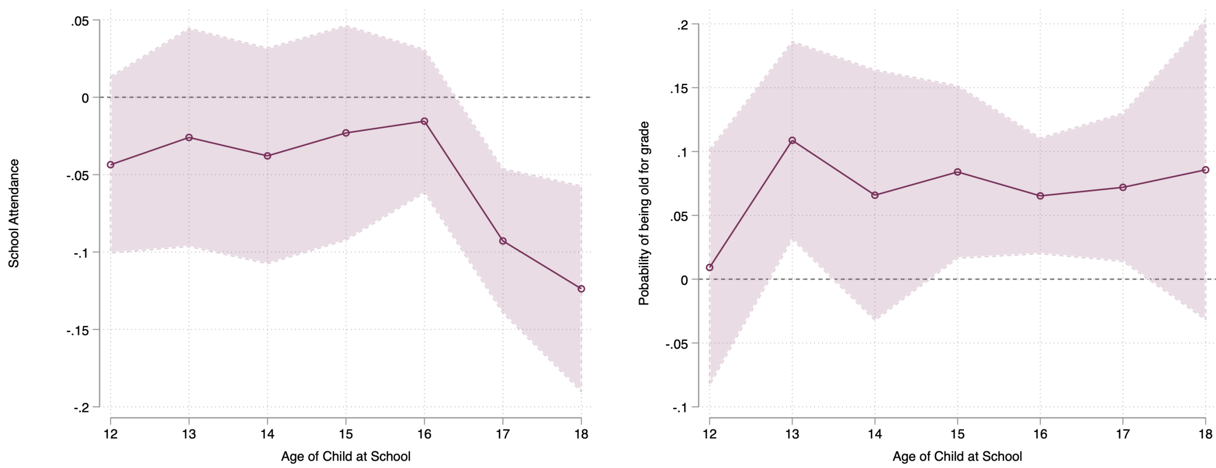
(b) Fathers

Notes: The figure plots the coefficients β_τ and their 95% confidence intervals from regression $Died_{it} = \sum_{\tau=1985}^{2019} \beta_\tau 1(t = \tau) \cdot Displaced_{s\{i\}} + X_i'\theta + \psi_o + \gamma_t + \varepsilon_{it}$. Plotted coefficients until 2004 for better exposition. I follow Deryugina and Molitor (2020) to set-up the data. Panel (a) estimates displacement effect on annual mother's mortality and panel (b) does the same for fathers. These regressions are for households with children at the time of the intervention.

Figure E.2: Displacement effects on School Attendance: Children born 1 to 5 years after intervention

(a) School Attendance

(b) Probability of being old for corresponding grade



Notes: Regressions for children born 1 to 5 years after the intervention and matched to school enrollment data. Clustered standard errors at the slum level. Controls include: female, mother head of household, married head of household, number of siblings, first-born dummy, head of household's marital status unknown, and year of birth fixed effects. Figures plot coefficients β_τ and their 95% confidence intervals from regression: $y_{it} = \sum_{\tau=12}^{18} \beta_\tau Displaced * 1[Age\ at\ school = \tau] + \sum_{\tau=12}^{18} \delta_\tau 1[Age\ at\ school = \tau] + \psi_o + X_{it}'\gamma + u_{it}$.

Table E.2: Adults' labor market outcomes, head of households

	(1)	(2)	(3)	(4)
Outcome:	1[Employed]	Total Income	Labor Income	Retirement Income
Panel A.	All head of households in RSH			
Displaced	0.059***	-12.603***	1.914	-24.621***
	(0.014)	(2.530)	(3.657)	(4.065)
Non-displaced mean	0.385	100.05	77.35	72.43
R^2	0.230	0.308	0.204	0.151
N	275,811	275,811	275,811	275,811
Individuals	14,947	14,947	14,947	14,947
Panel B.	Parents younger than 65 yo			
Displaced	0.036***	-14.281***	-8.796*	-17.151***
	(0.013)	(3.050)	(4.841)	(3.527)
Non-displaced mean	0.602	105.64	128.23	33.19
R^2	0.127	0.304	0.158	0.080
N	120,648	120,648	120,648	120,648
Individuals	9,905	9,905	9,905	9,905
Panel C.	Parents older than 65 yo			
Displaced	0.056***	-12.904***	1.636	-24.926***
	(0.016)	(2.603)	(3.473)	(4.207)
Non-displaced mean	0.286	97.52	53.78	90.61
R^2	0.156	0.320	0.148	0.066
N	155,163	155,163	155,163	155,163
Individuals	12,252	12,252	12,252	12,252
Municipality of origin FE	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓

Notes: Regressions for head of households matched the RSH data. Clustered standard errors by municipality level. 10%*, 5%**, 1%***. Controls include: female head of household, married head of household, marital status unknown, age at intervention, and cohort fixed effects. All regressions include year of intervention fixed effects.

Table E.3: Displacement effects for children born to treated families

Outcome	Employed	Labor Income	Taxable Income	Years of Schooling	HS Graduate	College Attendance
	(1)	(2)	(3)	(4)	(5)	(6)
Displaced	0.026	-0.074	-19.408	-0.475***	-0.051***	-0.002
	(0.021)	(6.771)	(16.787)	(0.130)	(0.015)	(0.020)
Non-displaced mean	0.59	123.20	705.31	12.32	0.80	0.16
% Variation w.r.t. non-disp.	4.4	-0.06	-2.8	-1.1	-6.4	-1.25
R^2	0.093	0.110	0.137	0.076	0.064	0.049
Observations	94,129	94,129	19,346	4,218	4,218	4,218
Individuals	4,665	4,665	4,367			
Municipality of origin FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes: Regressions for children born 1 to 5 years after intervention and matched to the RSH data that report non-missing schooling. Clustered standard errors by municipality of origin. 10%*, 5%**, 1%***. Baseline controls include: female, mother head of household, married mother at birth, age of mother at birth, number of siblings, Mapuche last-name, cohort fixed effects, year of treatment fixed effects. Schooling regressions include mother's schooling as a control variable.

5. Additional Results on Mechanisms

Table F.1: Parents mortality and children's outcomes

	Mother dies after displacement				Father dies after displacement			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age bracket	0-2	3-7	8-12	13-18	0-2	3-7	8-12	13-18
Panel A: Labor Income								
Displaced	-17.538**	-11.755**	-17.510*	-13.864	-16.809**	-13.407***	-18.940**	-14.278
	(6.852)	(4.461)	(8.771)	(8.806)	(7.073)	(4.379)	(8.737)	(8.732)
Parent died within 5 years	-114.609***	-69.038	13.160	-37.504*	-22.661	-21.545	-25.782**	-19.648
	(35.438)	(65.519)	(16.707)	(20.174)	(43.164)	(19.923)	(9.835)	(15.292)
Displaced* Parent died within 5 years	172.088**	59.639	-16.486	10.158	33.072	3.816	15.541	20.751
	(80.538)	(66.925)	(24.231)	(24.750)	(49.693)	(22.384)	(13.985)	(21.179)
Parent died 6-10 years	-2.653	47.554*	-9.327	1.883	43.731	-46.654***	-11.108	-4.266
	(63.517)	(24.102)	(28.407)	(10.674)	(41.338)	(11.724)	(18.484)	(16.876)
Displaced* Parent died 6-10 years	-3.881	-63.188*	-23.023	-8.319	-49.629	67.814***	33.205	-0.684
	(66.647)	(33.079)	(28.649)	(13.095)	(43.713)	(17.787)	(23.061)	(17.251)
Panel B: Years of schooling								
Displaced	-0.841***	-0.571***	-0.602***	-0.619**	-0.847***	-0.584***	-0.650***	-0.615**
	(0.172)	(0.133)	(0.102)	(0.287)	(0.169)	(0.131)	(0.101)	(0.291)
Parent died within 5 years	-0.019	-1.215	0.222	0.369	-1.248*	-1.904***	-1.591**	-0.572
	(0.209)	(1.045)	(0.428)	(0.779)	(0.664)	(0.564)	(0.690)	(0.403)
Displaced* Parent died within 5 years	0.786**	0.762	-1.448***	-1.905	0.148	1.443**	0.772	-0.256
	(0.374)	(1.204)	(0.488)	(1.280)	(0.679)	(0.652)	(0.778)	(0.552)
Parent died 6-10 years	-1.661	-0.871*	-0.441	-0.897	-0.431	-0.191	-0.907**	-0.757
	(1.280)	(0.446)	(0.652)	(1.052)	(0.855)	(0.327)	(0.379)	(0.595)
Displaced* Parent died 6-10 years	1.140	0.221	-0.519	0.269	0.518	0.142	1.101**	0.008
	(1.496)	(0.571)	(0.801)	(1.061)	(0.789)	(0.428)	(0.440)	(0.659)
Panel C: 1[Employment]								
Displaced	-0.001	0.007	-0.005	0.014	-0.001	0.004	-0.004	0.012
	(0.027)	(0.016)	(0.021)	(0.019)	(0.028)	(0.015)	(0.022)	(0.020)
Parent died within 5 years	-0.587***	-0.395**	0.083	-0.102	0.052	-0.072	0.165**	0.061
	(0.040)	(0.189)	(0.100)	(0.062)	(0.164)	(0.075)	(0.066)	(0.058)
Displaced* Parent died within 5 years	0.623***	0.392*	-0.035	0.114	-0.077	0.042	-0.170**	-0.121*
	(0.208)	(0.212)	(0.118)	(0.111)	(0.173)	(0.096)	(0.075)	(0.066)
Parent died 6-10 years	0.155	0.227**	-0.135	0.012	0.038	-0.019	-0.035	-0.067
	(0.134)	(0.108)	(0.093)	(0.100)	(0.093)	(0.096)	(0.047)	(0.043)
Displaced* Parent died 6-10 years	-0.147	-0.191	0.028	-0.026	0.030	0.085	0.055	0.095*
	(0.166)	(0.119)	(0.093)	(0.116)	(0.104)	(0.102)	(0.060)	(0.054)
% Displ. Parents died within 5 years	0.2	0.3	0.4	0.7	0.8	1.1	1.7	2.0
% Displ. Parents died in years 6-10	0.3	0.4	0.8	1.3	1.5	1.7	2.3	3.4
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Regressions for children of ages 0 to 18 at baseline matched to the RSH data that report non-missing schooling. Clustered standard errors by municipality of origin. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include: female, mother head of household, married head of household, head of household's marital status unknown, age of mother at birth, number of siblings, birth order, and year of birth fixed effects.

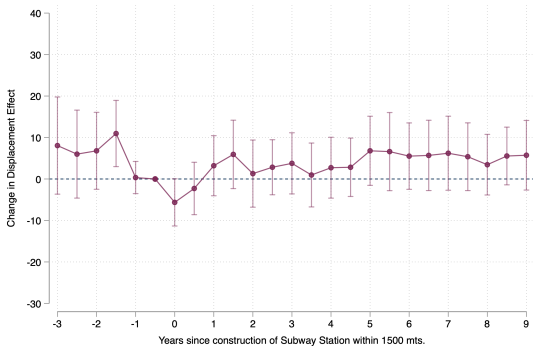
Table F.2: Displacement Effect and Subway Rollout between 2007 and 2019 by groups

Distance to new station	1.5KM	1.5 KM	1.5 KM	1.5 KM
Population	Women	Men	Age<45	Age≥45
	(1)	(2)	(3)	(4)
Panel A.	Labor Income			
Displaced	-23.940**	-16.243*	-19.366**	-27.292**
	(9.789)	(8.092)	(8.348)	(10.182)
Subway Station	-12.542	-1.173	-6.466	-15.045**
	(7.499)	(7.786)	(6.713)	(6.916)
Displaced*Subway	19.324**	5.417	11.663	21.183**
	(9.121)	(7.751)	(7.093)	(9.652)
Non-displaced mean				
R^2	0.029	0.033	0.128	0.127
Panel B.	Employment			
Displaced	-0.017	0.027*	0.002	-0.003
	(0.026)	(0.014)	(0.016)	(0.026)
Subway Station	-0.011	-0.003	-0.010	0.002
	(0.022)	(0.015)	(0.017)	(0.019)
Displaced*Subway	0.015	-0.017	-0.001	0.010
	(0.026)	(0.018)	(0.017)	(0.023)
Non-displaced mean				
R^2	0.014	0.010	0.109	0.107
Observations	361,203	259,126	500,504	119,825
% Displaced individuals affected	30.0	30.0	30.0	30.0
% Non-displaced individuals affected	45.5	45.5	45.5	45.5
Municipality of origin FE	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓

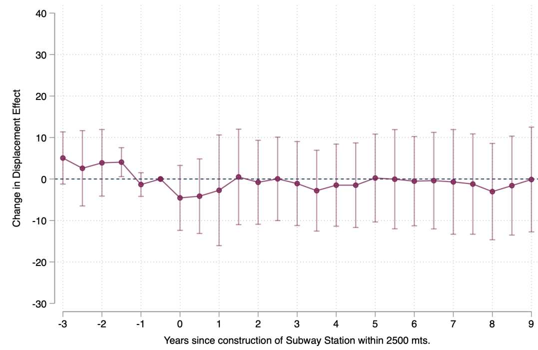
Notes: Regressions for children of ages 0 to 18 at baseline matched to the RSH data that report non-missing schooling. Clustered standard errors by municipality of origin. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include: female, mother head of household, married head of household, head of household's marital status unknown, age of mother at birth, number of siblings, birth order, and year of birth fixed effects.

Figure F.1: Roll out of subway stations between 2007 and 2019 and change in displacement effect

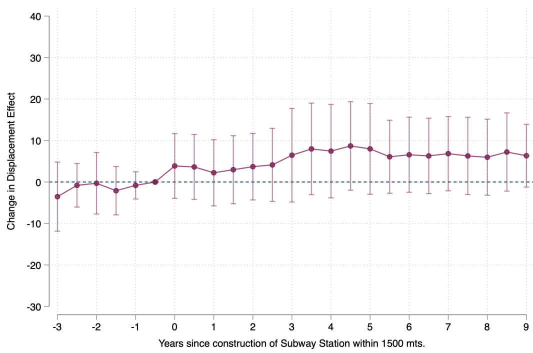
(a) Formal Earnings, distance=1.5KM



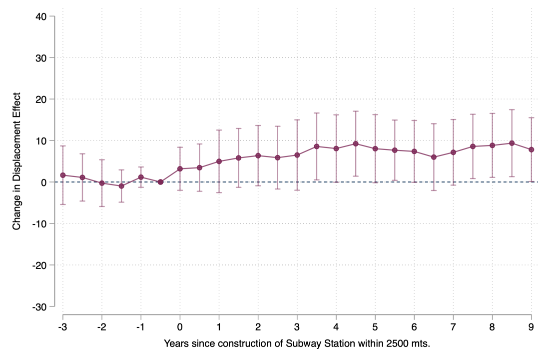
(b) Formal Earnings, distance=2.5KM



(c) Informal Earnings, distance=1.5KM



(d) Informal Earnings, distance=2.5KM



Notes: Regressions for children aged 0 to 18 at baseline matched to the RSH data that report non-missing schooling. Clustered standard errors by municipality of origin. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include: female, mother head of household, married head of household, head of household's marital status unknown, age of mother at birth, number of siblings, birth order, and year of birth fixed effects. Formal Earnings means earnings when working with a formal contract, and informal earnings without a contract.

Table F.3: Assignment to Fragmented Projects and *Displaced* Families' characteristics at baseline

Outcome	Mixed (1)	Mixed (2)	HHI (3)	HHI (4)
HH's age	-0.000	-0.001**	0.003	0.005***
	(0.001)	(0.001)	(0.002)	(0.001)
Female HH	0.002	0.005	-0.002	-0.013
	(0.005)	(0.006)	(0.012)	(0.019)
# children	0.001	0.015*	0.005	-0.024
	(0.003)	(0.008)	(0.009)	(0.013)
Married HH	-0.012*	-0.014**	0.024	0.032
	(0.006)	(0.005)	(0.018)	(0.025)
Marst Unknown	-0.007	-0.037**	0.012	0.064**
	(0.007)	(0.014)	(0.023)	(0.030)
Mapuche HH	0.012	0.012	-0.023	-0.005
	(0.012)	(0.010)	(0.040)	(0.031)
Mother's Schooling		-0.001		-0.001
		(0.003)		(0.007)
Test of joint significance of baseline controls				
F	1.549	0.538	2.006	1.951
$p > F$	0.201	0.745	0.093	0.102
R^2	0.511	0.522	0.529	0.608
Observations	13,519	10,830	13,519	13,519
Municipality of origin FE	✓	✓	✓	✓
Sample	All	RSH	All	RSH

Notes: Clustered standard errors at municipality level. 10%*, 5%**, 1%***. All regressions include year of intervention fixed effects.

Table F.4: Location Attributes at Origin

Location Attributes by Census District	Non-displaced mean (1)	Displaced Mixed mean at origin (2)	Displaced Not-mixed mean at origin (3)	Difference (2)-(1) (within munic.) (4)	Difference (3)-(1) (within munic.) (5)
Schooling HH	7.24	7.54	7.27	0.75	0.23
				(0.79)	(0.79)
Unemployed HH	0.18	0.18	0.21	-0.01	0.01
				(0.02)	(0.03)
HS Dropout students	0.33	0.32	0.32	-0.03	-0.03
				(0.03)	(0.03)
Schools per census district	3.89	3.57	3.93	-0.13	0.63
				(0.90)	(0.91)
Schools per 1000 students	1.19	0.84	0.92	-0.54	0.12
				(0.86)	(1.74)
Pub. Schools per 1000 students	1.00	0.68	0.86	-0.53	0.17
				(0.93)	(1.61)
Priv. Schools per 1000 students	0.18	0.14	0.04	-0.03	-0.05
				(0.12)	(0.18)
Family Care Centers per 1000 HH	0.01	0.01	0.01	0.00	0.01
				(0.01)	(0.02)
Hospitals per 1000 HH	0.03	0.02	0.02	0.00	-0.03
				(0.02)	(0.03)
Distance to (closest) metro station in km	7.95	9.89	8.25	-0.64	1.32
				(0.38)	(1.18)
Commuting time to Work (min)[a]	42.25	42.14	43.65	-0.11	1.40
				(0.84)	(0.83)
Commuting time to Study (min)[a]	32.92	33.14	31.87	0.22	-1.05
				(0.61)	(0.87)
Observations	53	90	17	143	70
# Slums	47	66	17	113	62
# New Projects	47	34	9	77	54

Notes: Each observation is a slum-neighborhood pair. Within difference corresponds to a regression of each location attribute on a displacement dummy conditional on municipality of origin. Clustered standard errors by municipality of origin. 10%*, 5%**, 1%***. All location attributes correspond to population averages by census district level in 1982. (a) Commuting times to work and to study are measured as the weighted average in minutes that takes the average person in each municipality to go to work/study using public transportation. Since these two variables are measured at the municipality level, the difference in column (3) does not include municipality fixed effects.

Table F.5: Displacement and Mixed Projects by Age

Age bracket	0-2 (1)	3-7 (2)	8-12 (3)	13-18 (4)
Panel A.	Outcome: Labor Income			
Displaced	-17.383*	-4.895	-10.703	-12.546
	(8.812)	(5.950)	(9.941)	(10.708)
Displaced* Mixed	0.028	-8.458	-8.309	-1.736
	(7.101)	(5.585)	(5.313)	(6.875)
R^2	0.124	0.119	0.137	0.158
Observations	90,824	198,471	198,789	132,245
Panel B.	Outcome: Years of Schooling			
Displaced	-0.682*	-0.313*	-0.216	-0.099
	(0.362)	(0.181)	(0.184)	(0.330)
Displaced* Mixed	-0.182	-0.307*	-0.467**	-0.614**
	(0.348)	(0.179)	(0.188)	(0.269)
R^2	0.102	0.082	0.097	0.099
Observations	4,580	9,929	9,827	6,546
Municipality of origin FE	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓

Notes: Regressions for children aged 0 to 18 at baseline matched with the RSH data that report non-missing schooling. Clustered standard errors by municipality of origin in parenthesis. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include: female, mother head of household, married head of household, head of household's marital status unknown, age of mother at birth, number of siblings, birth order, and year of birth fixed effects. Schooling regressions include mother's education as a covariate.

Table F.6: Displacement Effect, Change in Location Attributes, and fragmentation on Main Outcomes

Outcome	Labor Income		1[Employed]		Years of Schooling	
	(1)	(2)	(3)	(4)	(5)	(6)
Displaced	-3.800	5.339	0.008	0.032	0.039	0.103
	(6.726)	(9.558)	(0.014)	(0.025)	(0.125)	(0.158)
Change in Location Attributes						
* ΔHH Years of schooling	2.435	1.363	0.008**	0.006***	-0.002	-0.061
	(1.614)	(1.362)	(0.004)	(0.002)	(0.041)	(0.048)
* Δ#Private schools/child	1.861	-5.508	-0.003	-0.023	-0.135	-0.513*
	(6.027)	(8.138)	(0.024)	(0.023)	(0.277)	(0.290)
* Δ#Public schools/child	-1.785	0.874	0.001	-0.004	-0.043	0.203**
	(2.208)	(2.604)	(0.007)	(0.008)	(0.085)	(0.093)
* ΔDistance to Subway	-0.548**	-0.510	-0.002***	-0.003	0.016	0.062**
	(0.264)	(0.651)	(0.0007)	(0.002)	(0.011)	(0.030)
* ΔWaiting Time	0.215	-3.370	0.002	0.005	0.131***	-0.138
	(0.854)	(2.026)	(0.003)	(0.007)	(0.048)	(0.103)
* Distance from origin	-0.280	-0.130	0.001*	0.000	-0.009	-0.007
	(0.178)	(0.301)	(0.0005)	(0.007)	(0.007)	(0.012)
* ΔHealth Care Centers	-0.005	0.003	-0.000**	0.003	0.000**	0.000***
	(0.006)	(0.013)	(0.000)	(0.322)	(0.002)	(0.000)
* ΔHospitals	0.006***	0.005	0.000*	-0.205	0.000*	0.000**
	(0.002)	(0.007)	(0.000)	(0.131)	(0.0001)	(0.000)
Change in Composition of Neighbors						
* Mixed	-4.681	-9.614**	0.010	-0.007	-0.685***	-0.733***
	(4.069)	(4.175)	(0.011)	(0.012)	(0.201)	(0.216)
R^2	0.128	0.129	0.108	0.109	0.143	0.147
Non-displaced mean	155.24	155.24	0.67	0.67	11.37	11.37
Municipality of origin FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Municipality of destination FE		✓		✓		✓
Observations	620,329	620,329	620,329	620,329	26,871	26,871

Notes:This table shows results for $Y_{it} = \alpha + \beta Displaced_{s\{i\}} + \gamma Displaced_{s\{i\}} \cdot \Delta Attribute_{do} + \psi_o + \psi_\tau + X_i'\theta + \varepsilon_{it}$. All attributes are measured at the census district level which corresponds to a smaller level of aggregation than municipalities. Regressions for children aged 0 to 18 and matched to the RSH data. Clustered standard errors by municipality of origin in parenthesis. 10%*, 5%**, 1%***. Controls include: female, mother head of household, married head of household, number of siblings, birth order and cohort fixed effects. Schooling regressions include mother's education as a control.

6. Persistence of a Displacement Effect

In this section I discuss how persistent is the effect I find by looking at attrition rates by year 2016. Using data from the Chilean Electoral Records in 2016, I can observe individuals addresses and check whether they are still living in the destination locations.

Table A.8 shows that attrition after 30 years is surprisingly low: 60% of the head of households in my sample remain in the destination locations and 67% of them are in the same destination municipality. There are no differences between the displaced and non-displaced. Only 3% of the displaced head of households returned to their municipalities of origin pre-intervention. The figures are lower for children but still considerable: 58% of children are in their destination municipality and 41% reports to have an address at their parents' neighborhood of destination. These numbers are big but not unexpected, because one of the requisites for families in the Urban Marginality Program was to remain in their destination projects between 10 and 15 years after the intervention. Families had to pay the cost of the housing unit in installments, which prevented them from selling the house during the first years. To become a homeowner they had to pay all the installments. Families could choose to leave and stop paying, but they would lose the property deed.

Since many children remain in the same destination neighborhoods, I look at the characteristics of these neighborhoods today. I report my estimates in Table A.14. Panel A shows that displaced children relative to non-displaced are more likely to live in neighborhoods where the population has lower income and they are more likely to be poor. This last variable is labeled as *Quintile* in the table and is a poverty index used by the Ministry of Social Development to classify households for social assistance. The lower the index the more vulnerable the family is considered. In addition, displaced children live in neighborhoods where the population has lower schooling but higher employment.

Panel B reports differences in the attributes of the neighborhoods themselves. As of today, in the projects for the displaced families live individuals that have lower income, and lower schooling rates. These differences are economically important but have big standard errors, so I cannot reject the null of zero difference. In panel C I compare the projects in my sample to other neighborhoods in Greater Santiago. On average, all the projects in my sample (both for the displaced and for non-displaced families) are poorer neighborhoods than the average neighborhood in the city. However, the projects for the non-displaced are not as poor as for the displaced (measured by income or schooling).

These last results, together with the attrition estimates indicate that families remain in the same places they were sent to and their neighborhoods are of low quality. This suggests that it was not easy for families to leave these bad environments. However, since families became homeowners in the new locations, I do not know if they wanted to leave and/or how easy was for them to migrate.

7. Moving out of a Slum versus Neighborhood Effects

In this last section I discuss the total effect of this housing program on children's long-term earnings. For the purpose of this exercise, assume the outcome of child i in neighborhood z can be expressed as follows,

$$y_{iz} = \theta_z + \mu_i + \epsilon_{iz},$$

where θ_z is a neighborhood effect, μ is the effect of moving out of a slum, or the effect of slum upgrading (housing), which is independent of the neighborhood z,and ϵ_{iz} is a family shock or family taste for neighborhood z. The average outcome in neighborhood z can be written as

$$\bar{y}_z = \theta_z + \bar{\mu} + \bar{\epsilon}_z.$$

In my setting, I can conclude that $\bar{\epsilon}_z = 0$, because there is no selection of families into treatment and there is limited choice of the destination neighborhood. Hence, any total effect on children's outcomes would be decomposed into a neighborhood effect and a moving out effect.

The setting I study allows us to identify the average neighborhood effect as the displacement effect($\hat{\beta}$ from equation (1)), but the moving out or housing effect is not identified because both the treated and comparison groups got housed as part of the intervention.

The lack of administrative data for slums dwellers in the 1980s does not allow us to estimate μ because the slums were not an administrative unit as neighborhoods were; however, I can estimate un upper bound to this effect using current data. In the Social Registry data (RSH) I are able to observe households' types of dwelling. For the purpose of this exercise, I select all the households that ever lived in a slum between 2007 and 2019 and followed them across time, I can observe whether they move out of a slum to a house or an apartment at each point in time. I look at the earnings trajectories of the universe of head of households with children between 0 to 18 years of age that ever lived in a slum and estimate the following event study regression,

$$Y_{it} = \tilde{\alpha} + \tilde{\beta}\text{Mover} + \sum_{\tau=-10}^{16} \mu_\tau \text{Mover} * 1[t = \tau] + X'_{it}\gamma + \psi_o + \varepsilon_{it}, \tag{4}$$

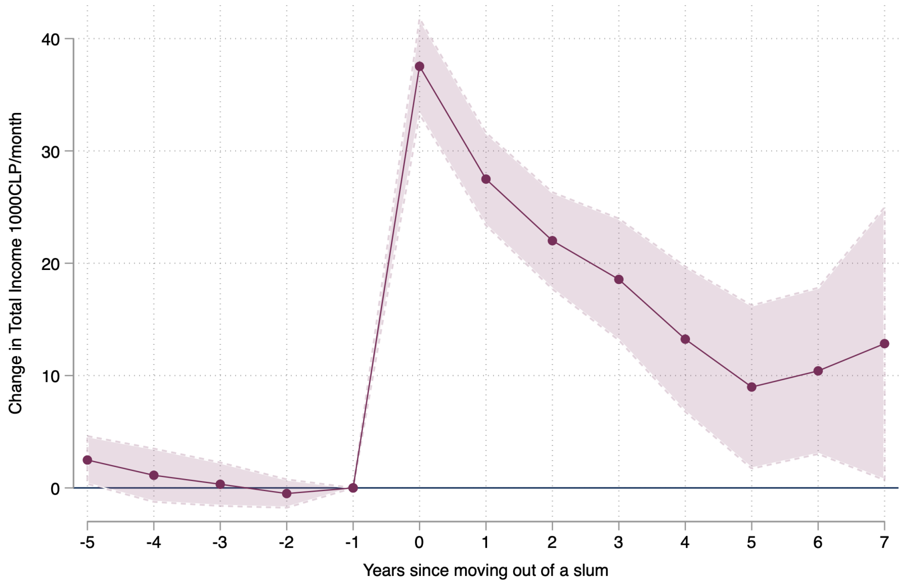
where Y_{it} is the earnings of individual i at time t. $Mover$ is a dummy that takes the value of 1 if a family living in a slum ever moved to a house or an apartment. X_{it} is a set of demographic characteristics, such as age, gender, nationality, number of children and indigenous identity, ψ_o are municipality fixed effects, and ε_{it} is the error term. I cluster the standard errors at the municipality level.

The goal of estimating this equation is to understand how adults' earnings change after they move out of a slum to a house. Importantly, I do not claim that the μ_τ coefficients estimate the causal effect of moving out from a slum in this setting, because there are several reasons why a family would move that I as researchers do not observe, such as motivation, access to housing subsidies, waiting time for a subsidy, and many others. The only goal of this exercise is to put a bound on μ.

The results to estimating equation (3) are plotted in Figure G.1. The results show that adults' earnings increase in the first year after moving, and the change is decreasing in time. The cumulative extra income after nine years is about CLP$1.4 MM, which is equivalent to US$1,974 extra income for low-income families.

To translate this number into children's earnings, I assume that the whole amount of extra income is transferrable to children when they are 18 years old. This would be equivalent to increasing a child's monthly earnings in CLP$5.89 between the ages of 18 and 60. Hence, if

Figure G.1: Earning Dynamics of Slum Dwellers after moving out of a Slum



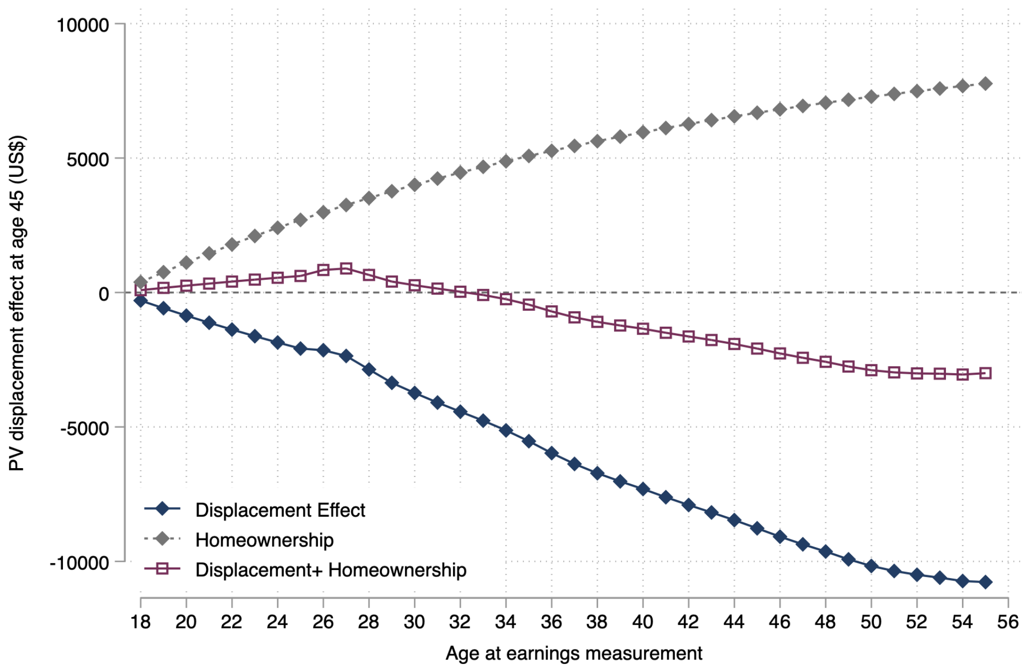
Notes: This figures plots the coefficients μ_τ from equation (7) and their 95% confidence intervals.

$\hat{\mu} = 5.89$, the total effect of the displacement on children's earnings would be

$$\bar{y}_z = \hat{\beta} + \hat{\mu} = -15.13 + 5.89 = -9.24$$

This number is equivalent to saying that displaced children's earnings are 6% lower than the non-displaced if I account for the effect of housing. I repeat this exercise as a present value calculation and show the results in Figure G.2. What the figure shows is that any cumulative positive effect of housing on children's long-term earnings is not able to counteract the negative effect of a bad neighborhood in the long-term.

Figure G.2: Present value of displacement effect at age 45



8. Forced Displacements versus Voluntary Moves

In this section I explore whether there are differences between forced and voluntary moves. In 1985 the Ministry of Housing changed the evaluation process to select slum families, the authorities made families apply for a housing unit with the same characteristics of the Program for Urban Marginality, and the housing units were located in the same peripheral municipalities. Two features are distinctive in this case, first, families had to apply, and second, they were asked which municipalities of destination they preferred, families were asked to list up to three different options.

I collected information for 1,601 slum dwellers of a universe of 4,500[1] that were selected to obtain a housing unit in this program, and the location of their new housing unit. All of them became homeowners in 1985. I found their children using the same procedure as for

[1] I am in the process of finding the rest of the slum dwellers in the Archives.

the baseline sample of displaced and non-displaced parents in this paper. Unfortunately, I do not have exact information of families' original slums, but I do have information about their municipality of origin. I call these families "movers," because they chose to participate in this program, as opposed to the displaced families that were forced to move.

To study if there are differences across the displaced and the movers, I compare their children's outcomes relative to the non-displaced. By definition the mover families are a selected group slum dwellers, thus, I do not interpret the estimate as causal but as informative about families' choices. Thus, if families were willing to participate in the program and had a say about their final destinations, I would expect that, compared to the non-displaced children, the coefficient should be less negative than the displacement effect or even positive. I run the following regression to compare coefficients,

$$Y_{it} = \alpha + \beta Displaced_{s\{i\}} + \delta Mover_{s\{i\}} + \psi_o + X_{it}'\theta + \varepsilon_{it}, \tag{5}$$

where Y_{it} is current outcome for individual i at time t, such as labor income, employment status or years of schooling, $s(i)$ indexes the slum of origin for individual i's family. The variable $Displaced_{s\{i\}}$ takes the value of 1 if an individual's family lived in a displaced slum and 0 otherwise, and $Mover_{s\{i\}}$ takes the value of 1 if an individual's family participated in the selection process to get a housing unit, and 0 otherwise, ψ_o are municipality of origin fixed effects that control for any initial differences between families living in slums located in different municipalities, such as access to public services or higher quality neighborhoods. X_{it} are baseline demographic controls. Notice that as opposed to equation (1) I cannot include year of intervention fixed effects because all movers received a housing unit in 1985, hence, to account for temporal variation, I include age of intervention fixed effects in some of my estimations and check whether the results change.

The null hypothesis is that $\beta < \delta$. I report the results for labor earnings and years of education in Table G.1. My results show that I cannot reject the null hypothesis on both earnings and education. The estimates for β are of the same magnitude than in the baseline results, and the estimates for δ are smaller in magnitude and not statistically significant. In fact, they become positive when I add age of intervention fixed effects, while $\beta's$ do not change much. In conclusion, I take these results as suggestive of the fact that forced movements imply worse outcomes for the children in forced movements, as opposed to when families have some control on their final destinations.

Table G.1: Forced Moves versus Voluntary Moves

Panel A.	Labor Income		Years of Schooling	
	(1)	(2)	(3)	(4)
Displaced	-20.196***	-16.435**	-0.913***	-0.746***
	(5.869)	(6.090)	(0.128)	(0.131)
Mover	-3.010	12.487	-0.097	0.625**
	(5.940)	(8.709)	(0.144)	(0.249)
R^2	0.124	0.124	0.114	0.118
Non-displ. mean	155.24	155.24	11.37	11.37
Observations	674,644	674,644	33,663	33,663
Municipality of origin FE	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓
Age at move FE		✓		✓

Notes: Regressions for children aged 0 to 18 at baseline matched to the RSH data that report non-missing schooling. Clustered standard errors by municipality of origin in parenthesis. 10%*, 5%**, 1%***. Baseline controls include: female, mother head of household, married head of household, head of household's marital status unknown, age of mother at birth, number of siblings, birth order, and year of birth fixed effects.

9. Variables Definitions

Variable Name	Description
Outcomes and Treatment	
Labor Income	source: RSH. Self-reported labor earnings measured in CLP$ per month. Original variable corresponds to the sum of all earnings in the last year at the time of the interview. It includes earnings from formal and informal employment, and excludes pensions and transfers. Data available biannually from 2007 to 2019.
Employed	source: RSH. Person reports to be employed at the time of the interview. It includes any type of employment, formal or informal. Data available biannually from 2007 to 2019.
Taxable Income	source: GRIS Mutuales. Monthly administrative records on taxable earnings for all workers that contribute to Social Security. Data available monthly from 2016 to 2019.
Contract	source: RSH. Conditional on employment, person reports to work with a formal contract.
Temporary Worker	source: RSH. Conditional on employment, person reports to work on a fixed term.
Years of Education	source: RSH. Completed years of schooling. Constructed based on grade completion and levels. A person can appear multiple times in the RSH with differences in this variable across years. I use the minimum value after the age of 25.
High school graduate	source: RSH. Person reports to have successfully completed high-school.

College Attendance	source: RSH. Person reports to attend at least one year of tertiary education. This includes 2-3 year colleges or 5-year colleges.
Displaced	source: Archives and authors calculations. Based on Archival data, MINVU (1979, 1984), Molina (1986), and Morales and Rojas (1986), I construct the displacement dummy at the slum level.

Covariates

Year of Intervention	source: Archives and authors calculations. Based on Archival data, MINVU (1979, 1984), Molina (1986), and Morales and Rojas (1986).
Municipality of Origin	source: Archives and authors calculations. Based on Archival data, MINVU (1979, 1984), Molina (1986), and Morales and Rojas (1986).
Slum of Origin	source: Archives and authors calculations. Based on Archival data, MINVU (1979, 1984), Molina (1986), and Morales and Rojas (1986).
Municipality of Destination	source: Archives and authors calculations. Based on Archival data, MINVU (1979, 1984), Molina (1986), and Morales and Rojas (1986).
Project of Destination	source: Archival records and electoral records in 2016. I updated the name of the projects using current names reported in families' addresses in 2016 that I observe in the electoral records.
Date of Birth	From birth bertificates
Age at intervention	Year of intervention minus year of birth
Female	From birth certificates

Mother head of household	I proxy head of household's gender using the gender of the person who received the property deed as it appears in the Archival Record.
Head of Household marital status	From marriage certificates I identify if a person is married or widowed at the time of the intervention. I cannot conclude a person is single is I did not find a marriage certificate because the oldest the couple the less likely their marriage certificate is to be available in the Social Registry website.
Age of mother at birth	From birth certificates, year of intervention minus mother's year of birth.
Number of Siblings	Number of children from the same couple minus one. I are not always able to observe half-siblings if parents re-married, because I only observe the last marriage certificate.
Mother's education	source: RSH. Constructed the same way as years of education. I correct this variable by weighting the observations by the inverse of the probability of being found in RSH $\hat{p}$. I compute this probability as the fitted values of a logit regression of the probability of being found in RSH on displaced, dead before 2007 and a full set of demographic controls at the time of the intervention. Then, I weight each observation by $1/\hat{p}$ if mother was displaced, and $1/(1-\hat{p})$ if mother was not-displaced.
Mapuche last-name	source: Archival Records and Mapuche Data Project. I identify each last-name as mapuche if I find it in the list collected by the *Mapuche Data Project*. Data available here.

Slum/Neighborhood Characteristics

Area	source: MINVU (1979, 1984). Land used by each slum measured in hectares.
# Families	source: MINVU (1979, 1984), Molina (1986). Number of families per slum.
Military Name	Constructed by the authors. A slum is considered to have a military name if its name has a reference to any military name or date associated to a military event in the History of Chile.
Distance to River	Measures the distance in kilometers from a slum location to the closest riverbank in Greater Santiago. To geo-reference slums I use Morales and Rojas (1986), and rivers locations available here.
Census District	Smaller geographic unit than municipality. source: Census of Population of 1992. Shape files of the census of 1982 where not available in the National Institute of Statistics, thus, I use the corresponding census districts in 1992 because the differences between 1982 and 1992 in the Greater Santiago were minor.
HH's Schooling	source: Census of Population of 1982. Average years of schooling of all head of households between 18 and 65 years old by municipality, and by census district.
HH' Unemployment	source: Census of Population of 1982. Average unemployment rate of head of households between 18 and 65 years old by municipality, and by census district.
HS Dropout students	source: Census of Population of 1982. Share of the population that is not in high-school but should be as measured by their age. Measures at the level of municipality and census district.

# Schools	source: Ministry of Education. List of all schools in Chile, their location, type (private and public), and their year of inauguration. I keep all schools until year 1985. I measure number of schools per municipality and per census district, as well as the number of schools per 1,000 students by using as denominator the schooling population from the 1982 Census.
# Health Care Centers	source: Ministry of Health. List of all Public Family care Centers in Chile, their location, and their year of inauguration. I checked the years of inauguration one by one by calling each of the centers that had wrong dates. I keep all Health Care Centers until year 1985. I measure number of centers per municipality and per census district, as well as the number of centers per 1,000 households by using as denominator the total number of households per municipality/district in the 1982 Census.
# Hospitals	source: Ministry of Health. List of all Public Hospitals in Chile, their location, and their year of inauguration. I keep all hospitals built until 1985. I measure number of hospitals per municipality and per census district, as well as the number of hospitals per 1,000 households by using as denominator the total number of households per municipality/district in the 1982 Census.
Distance to Subway	source: Metro de Santiago. List of metro stations in Greater Santiago, their location, and year of construction. Distance is measured in kilometers as the distance between each slum/project of destination to the closest metro station built in or before 1985.

Waiting Time	source: Origin-Destination Survey, Santiago, 1977. Average waiting time in public transportation at the municipality level. Measured in minutes. Unfortunately not available at a smaller geographic level.
Commuting Time	source: Origin-Destination Survey, Santiago, 1991. Average commuting time in public transportation at the municipality level. Measured in minutes.

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