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

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Abstract

We examine the long-term effects of moving to a high-poverty neighborhood on children's outcomes, using evidence from a slum clearance program in Santiago, Chile, between 1979 and 1985. During the dictatorship, slum families were forced to relocate to public housing in low-income areas. Two-thirds were relocated to new housing projects on the city's periphery, while the rest received housing at their original locations. Our results reveal that, 35 years post-policy, on average, displaced children earn 14% less, receive 0.64 fewer years of schooling and have more labor informality as adults compared to those not displaced. Results by age suggest young children are more likely to suffer from neighborhood effects due to lower home values and lack of schools, whereas teenagers are affected by the disruption of their networks. Despite being less likely to live in their assigned neighborhoods, displaced children as adults live in higher-poverty areas.

Keywords: slum clearance, children, neighborhood effects, long-term, forced displacement.

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1 Introduction

Over 25% of the world's urban population currently live in slums (UN-Habitat, 2020). A common policy response in developing countries to high poverty and the large share of slum dwellers has been to clear and redevelop slums and resettle dwellers to low-income housing in city peripheries (Belsky et al., 2013).[1] Annually, an estimated 15 million people per year are forcibly removed from their homes for new infrastructure projects (Cernea and Mathur, 2008), and governments spend up to 0.9% of GDP on homeownership subsidies.[2] However, even when slum dwellers are re-housed, it remains unclear if eviction policies truly compensate them. Improved housing quality could be offset by losses in job proximity, social networks, and access to public services, such as schools and health provision, raising questions about the welfare outcomes of these relocation policies (Lall et al., 2006; Barnhardt et al., 2016).

In this paper, we examine the long-term impacts on children's education and future earnings of relocating to high-poverty neighborhoods. We focus on a large-scale slum clearance and urban renewal program, the Program for Urban Marginality (Programa para la Marginalidad Urbana), implemented during the Chilean dictatorship between 1979 and 1985. The program was large in scope, affecting more than 5% of the population of Greater Santiago, Chile's capital. All slum dwellers in the program became homeowners of similar housing units. While some slums were upgraded into neighborhoods, others were relocated to suburban areas. The program consisted of two types of intervention. In the first, when urban conditions permitted, a slum was upgraded into a proper neighborhood, and families remained in their original location (i.e., were non-displaced). In the second type, when upgrading was not possible, families were evicted and forced to move in groups to new public housing projects (i.e., were displaced).

We estimate a displacement effect by comparing the two groups: families who were displaced and those who were not, although both became homeowners. Thus, the key difference between groups was the disruption from relocating and the characteristics of their new locations. To identify the total impact of displacement on children, we first

[1] Examples of this policy can be found in Brazil (Dasgupta and Lall, 2009), India (Barnhardt et al., 2016), and Kenya (see here). Current discussions on neighborhood redevelopment occur in Denmark (see here). Building social housing on city peripheries was a common policy in many European cities during the 1950s and 1960s (Hall, 1997) and in Latin America more recently (Sabatini, 2006).

[2] Based on the OECD Affordable Housing Database. See here.

use the variation with respect to which slums were cleared (and families moved). The selection of slums into the displaced or non-displaced group depended on the feasibility of urban renewal and not on individual family characteristics, such as slum density, geographic location, and price of land. We analyze these characteristics to estimate a policy function as the probability of a slum being cleared versus being redeveloped, and then compare displaced and non-displaced children from slums with the same probability of being cleared.

The validity of our identification strategy depends on whether displaced and non-displaced children differ on observable characteristics. Conditional on the probability of a slum being cleared, we find no correlation between the selection of slums for eviction and children's demographic and socioeconomic characteristics, such as age, gender, or family composition, before the program. One limitation of our data is that we cannot observe parental income or employment before the intervention. However, we use records for the Municipality of Santiago, located in the downtown Santiago, to partially address this gap. Though the sample is small, we find that displaced children are more likely to have employed fathers, and conditional on the probability of a slum being cleared, the predicted employment of fathers in the full sample does not vary between displaced and non-displaced children.

In addition to being forcibly moved, displaced families were assigned specific destinations. This variation in destination allows us to study place effects by identifying the determinants of the displacement effect on children's future earnings. Displaced families were disproportionately moved to low-income municipalities in neighborhoods mostly located on the city's periphery. Although on average these new areas were characterized by high poverty rates, a low provision of public goods, and a lack of public transportation, the degree of change varied between the destinations and origins. This variation allows us to identify which neighborhood characteristics account for displacement effects. The fact that displaced families had no choice in when or where they moved and were relocated to a specific location limits potential selection at destination. Moreover, we provide evidence that displaced families' demographics do not systematically predict the attributes of their destination locations.

Using archival records and administrative data, we create a novel dataset that follows children and parents from displaced (cleared) and non-displaced (redeveloped) slums 20 to 40 years after the policy ended. We determine where families were sent, match children with their families, and then match individuals with data on employment, labor earnings, and years of schooling. Our final sample contains 12,409 families with children treated between 1979 and 1985, observed from 2007 to 2019. The final dataset includes data on 33,669 children, all aged between 0 and 18 years old at the time of the policy. Of these, 24,277 come from cleared and redeveloped slums, with a similar probability of being cleared (common support).

We find that displacement is detrimental for children aged 0 to 18 at baseline. Compared with non-displaced children, displaced children earn 14% less per month, on average. This negative effect on earnings is not associated with lower employment but with the quality of employment, as they are less likely to contribute to social security and are more likely to be temporary workers. We also find that displacement reduces children's educational attainment: a displaced child loses 0.64 years of education and is 18% less likely to graduate from high school relative to a non-displaced child. We find no evidence that our results are driven by improvements in the comparison group. Additionally, when estimating the displacement effect by the age at which earnings are measured (in adulthood from ages 25 to 55), we find that the total earnings loss for a displaced child is around US$13,300. This loss is larger than the cost of the house received by the average family in our sample (US$10,148 in 2018, on average).

We next study heterogeneous displacement effects by age at intervention and find that all the children in our sample experience a negative displacement effect on earnings. The effect is most pronounced in young children aged 0 to 14 years old at the time of the intervention. Within this group, 0 to 8 year olds face a more negative effect on earnings, especially on formal earnings (taxable wages and employed with a contract). These results are consistent with what previous work has called an "exposure effect" of neighborhoods (Chetty et al., 2016; Chyn, 2018; Laliberté, 2021).

To rule out selection on unobservables, we perform two series of robustness checks. First, we estimate a displacement effect on earnings using an instrumental variable (IV) approach, where we instrument displacement with the original assignment to treatment determined by Chile's Ministry of Housing in 1979. We can only do this for the sample of children treated after 1980, but in this sample, our results show very similar estimates on earnings between propensity score and IV methods. Second, we examine attrition due to missing individuals in the administrative data, finding that our results are unlikely to be driven by differential matching rates between displaced and non-displaced children.

Several mechanisms could explain the negative displacement effect on children aged 0 to 18 at the time of the intervention. In our sample, most of the negative effect on earnings is due to movements between, rather than within, municipalities, and thus we estimate a distribution of displacement effects on children's future earnings by municipality of origin. We find ample variation: while the average effect on earnings is negative, some children fared better. This leads us to explore granular changes to neighborhood characteristics to understand the determinants of the displacement effect, such as changes to public service access, segregation, transportation access, and disruption of social networks.

We find that the displacement effect on children's future labor earnings is a function of several changes in their environments. In our sample, home value positively correlates with earnings, but it does not explain the displacement effect on children. In fact, conditional on home value, the displacement effect becomes more negative. On the contrary, when we control for project size (number of housing units in the new neighborhood), distance from origin, and network disruption (measured as the share of original slum families at destination), the displacement effect on earnings decreases by 50%. The effect also decreases by 50% when controlling for changes in neighborhood characteristics, such as access to schools, access to labor markets (measured as the distance to the central business district, CBD), and property prices. When controlling for both sets of determinants, the effect decreases by 60%.

These results indicate that in our setting, the displacement effect on earnings is a function of both disruption and place effects. To provide more evidence on the role of the two components, we study mechanisms by age at baseline. Our results suggest that the displacement effect on young children (ages 0 to 14) is more likely to be associated with a place effect, as their earnings are more responsive to changes in home value and access to schools. In addition, the negative displacement effect on teenagers (ages 15 to 18) is likely due to the disruptive move, as their earnings respond more to their social network becoming disorganized.

Finally, we find that the Urban Marginality Program had persistent effects on families' locations. Thirty years after the program ended, 60% of household heads remain in the same destination municipality. In contrast, only 30% of their children, who are now adults, reside in the same municipality. Moreover, the neighborhoods where these adult children from non-displaced families currently live are poorer compared to those of adults from non-displaced families. These results also vary by age: the younger the child was during the program, the more likely they are, as adults, to live further away from their parents' destination neighborhood (2 kms farther away) and in poorer neighborhoods than their non-displaced counterparts.

This paper contributes to several strands of literature. First, it contributes to the literature studying slums as a particular type of urban poverty (Marx et al., 2013). Slum clearance and housing upgrading programs were common in developed countries (LaVoice, 2023; 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 the developing country context, little evidence has been provided for the effects of slum clearance policies on individuals since tracking slum dwellers is challenging. Most of the literature has focused on property rights (Field, 2007; Franklin, 2020), or improvements on-site (Galiani et al., 2017, Harari and Wong, 2021). Barnhardt et al. (2016) and Picarelli (2019) present findings most similar to our paper as they find that the distance from origin matters. However, our study is distinct in that we follow children in the long run and in the context of a forced move. We can also shed light on the negative consequences of building public housing in low-quality neighborhoods on individuals' long-term outcomes.[3]

Second, the paper contributes to the literature in economics and sociology studying the impact of neighborhoods on economic outcomes and intergenerational mobility, finding heterogeneous results by outcome and age (Sampson, 2008; Galster, 2012; Chetty et al., 2016; Chetty and Hendren, 2018; Chyn, 2018; Pinto, 2022; Mogstad and Torsvik, 2021; Chyn and Katz, 2021).[4] Recent papers emphasize the mechanisms that

[3] Barnhardt et al. (2016) and Picarelli (2019) find a negative relationship between distance and adults' outcomes. We also find that distance from origin and distance from labor market opportunities negatively impact children. This finding also aligns with the spatial mismatch hypothesis (Kain, 1968; Andersson et al., 2018).

[4] Mogstad and Torsvik (2021) and Chyn and Katz (2021) conduct extensive literature reviews on neighborhood effects. Results from the Moving to Opportunity (MTO) show positive effects on children's future earnings and college attendance. Nakamura et al. (2022) find different effects on children and adults due to different comparative advantages. Chyn (2018) finds more positive effects on

shape neighborhood effects, such as schools (Laliberté, 2021), peers (Damm and Dustmann, 2014), or public investment (Derenoncourt, 2022). We add to this literature by exploring mechanisms by age at intervention, finding that the displacement negatively affects all children between 0 and 18 years. However, mechanisms differ by age group, as children under 15 years are more likely to be affected by place effects, and teenagers are more likely to be affected by a disruption effect. Along these lines, our findings also relate to the literature studying how shocks during childhood affect adult outcomes (Currie and Almond, 2011; Heckman, 2006).[5]

Finally, we contribute to the literature on the effects of forced displacements.[6] Intergenerational effects on income and human capital have been documented in various settings (Becker et al., 2020; Nakamura et al., 2022; Bauer et al., 2013), although their direction depends on the characteristics of displaced populations, features of the destinations (e.g., cultural similarity between the displaced and receivers), and age at displacement. Our setting contributes to the understanding of displacements with a novel focus on group movements, that allows us to explore the mechanisms that protect children from the disruptive effects of displacement.

The rest of the paper is organized as follows. Section 2 describes the historical background and the program. Section 3 explains the data collection process, and Section 4 presents the empirical framework. Section 5 presents the baseline results on income and schooling. Section 6 discusses the expected theoretical effects of the displacement, and Section 7 presents the mechanisms. Section 8 discusses the displacement's total effect on earnings, and Section 9 concludes.

2 The Program for Urban Marginality

In the late 1970s, Chile experienced high levels of urban poverty after decades of urbanization. In Greater Santiago, the country's main metropolitan area, approximately 15% of the population lived in a slum (INE, 1970; INE, 1982), defined as a squatter

earnings than the MTO for all age groups. Finally, two recent papers study housing and neighborhoods in developing countries: Camacho et al. (2022) for Colombia and Carrillo et al. (2023) for South Africa.

[5] This literature has mostly focused on early childhood shocks before the age of five. However, we find that disruptions can also create long-lasting effects on teenagers.

[6] Conflicts and natural disasters are common causes of displacement. For a detailed literature review, see Becker and Ferrara (2019).

settlement without access to drinking water, electricity, or sewage (MINVU, 1979).[7] These slums were geographically ubiquitous. After the Pinochet dictatorship began in 1973, any attempt to create a new slum faced a strong military response.[8]

Motivated by this housing crisis, between 1979 to 1985, Chile's Ministry of Housing and Urban Development (MINVU) implemented the Program for Urban Marginality, a massive slum clearance and urban renewal policy. Proponents of this program believed the most effective way to end poverty involved housing poor families by making them homeowners, regardless of the attributes of the new housing units or neighborhoods (Murphy, 2015). At the onset of the program, the government conducted a census of slums and targeted 340 of them to be cleared.[9] According to Molina (1986) and Morales and Rojas (1986), by 1985, between 40,000 and 50,000 families participated in the program, accounting for 5% of Greater Santiago's population. The average cost of a housing unit was US$10,148, and the program's average total annual cost was US$63 million, approximately 0.25% of Chilean GDP in 1982.[10]

The program had two features. First, it aimed to build public housing for low-income families where land was cheap. Second, it aimed to provide the families with housing in locations where they could afford it. With these goals, the MINVU implemented two different types of interventions. Whenever conditions permitted, 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.e., were not displaced). If this was not possible, the families would be evicted from their original location and receive a housing unit in a different one (i.e., were displaced). All families in the same slum would receive the same treatment, and all would become homeowners.[11]

The features of each intervention are as follows. Non-displaced families accounted

[7] The median slum had around 250 families, with an average size of 5.2 persons per family.

[8] From 1973 to 1990, Chile was under a military dictatorship headed by Augusto Pinochet. The slums originated as land seizures between 1960 and 1973.

[9] Some slums families had received housing starting in 1977, but they did not own these homes and were renting instead. At the onset of the program in 1979, these families were included in the group set to become homeowners, and we include them in our sample. Other evictions occurred between 1976 and 1978 known as the Operaciones Confraternidad I, II, and III. These evictions were politically motivated, thus we do not include them in our analysis Celedón, 2019).

[10] Own calculations from archival data on average home values and subsidies. This number is similar to the current expenditure in homeownership subsidies in Chile (see here).

[11] Since both groups were granted property rights to the new housing unit they received, we cannot study the effect of property rights and land security on labor market outcomes. Field (2007) provides a good example of the effects of granting property rights to slum dwellers on labor force participation.

for one-third of the total number of families in the program. In some cases, these families were provided with an apartment in housing projects constructed very close to their original location. For others, the slum's land was subdivided among residents, with each family receiving a "starting-kit unit."[12] These new neighborhoods were provided with all of the basic services of a formal neighborhood (water, electricity, and sewage).

Displaced families represented two-thirds of the total number of families in the program. These families were evicted and moved in groups to public housing projects located in the city's peripheral sectors. They received a house or an apartment in these new neighborhoods and became the owners of a new housing unit. The land used by the slum was then cleared and used for a different purpose.[13] The destination neighborhoods were not prepared to receive the large number of displaced families (Molina, 1986; Aldunate et al., 1987). A large fraction lacked access to public transportation and public goods and services, such as schools and health care centers, and many were located in former rural areas recently added to the metropolitan limits.

Funding for the homes came from a direct government subsidy that was designed to cover 75% of the construction cost but was capped at 200 UF (inflation-adjusted index).[14] That is, a family would receive a subsidy equal to the minimum between 200 UF and 75% of the value of the new housing unit. The remaining amount corresponded to a copay that was paid in monthly installments to the MINVU over a term of 12 years.[15] Although the policy's design considered the previous rule for the subsidy, in our data we find evidence suggesting there was discretion: some housing projects had a subsidy capped at 200 UF, which was above the 75% value of the new unit.[16]

Decisions regarding the implementation were made directly by the MINVU at the central level. Santiago lacked a citywide government; instead, 30 local municipalities

[12]A starting-kit unit consisted of a living room, bathroom, and kitchen. Families would add bedrooms to the kit, completing the home.

[13]All families would be evicted; if they did not want to move, they would be excluded from the program. According to social workers, most families did not refuse the subsidy because it was their only chance to become homeowners.

[14]UF stands for "Unidad de Fomento," an inflation-indexed unit of account, published by the Central Bank of Chile. The average home value in our sample is 254 UF, equivalent to US$10,148 in 2018.

[15]Sometimes we observe that families pay over a term of 25 years, but their subsidies are lower.

[16]One example of this is slum dwellers who were moved from the Rio Mapocho riverbank in 1982 due to a flood. These families received a house with a value of 220 UF but a subsidy of 200 UF, possibly due to the emergency associated with their displacement. However, we find no systematic evidence of families' demographics predicting the subsidy amount nor the home value (Table A.7).

were responsible for managing each territory. Under this governance structure, citywide policies such as social housing were determined by central government. Moreover, the dictatorial regime of Pinochet appointed all local-level authorities. Hence, government directives were uniformly followed at the municipal level (González et al., 2021).

Displaced families could not participate in the decisions made by the MINVU, and given the political circumstances, they could not oppose the policy. Instead, they were assigned to new locations based on the current availability of finished projects across the city. This also implied that in some cases, displaced families of a single slum were assigned to more than one housing project.[17] Destination municipalities could also not influence how the program was implemented in their territories. As Labbé et al. (1986) explain, "municipalities have not had a direct responsibility regarding the location and quantity of the displaced families, as construction and relocation did not have to be approved by the municipality of destination."

The decision to clear a slum stemmed from various circumstances that prevented families from staying in their original locations. These ranged from slums being too close to freeways to being on a riverbank with high risk of flooding during the winter. Other circumstances were related to features of the land itself, such as public property, a slum's density (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."

An example of how the MINVU decided to clear a slum is presented by Murphy (2015) for Las Palmeras, a slum in a low-income municipality. Originally, the MINVU officially planned to build housing for families on the original location. However, by 1981, the high density of Las Palmeras made it impossible to allocate plots inside the slum in a way that guaranteed a minimum size for all the plots. Thus, the authorities decided to include Las Palmeras among the displaced. In late 1983, residents were moved to a new neighborhood built on the municipality's outskirts, and the former slum became a park. A second example is the slum dwellers located in the riverbank

[17]Housing projects were not planned specifically to house families of any given slum. We interviewed social workers who accompanied families during the eviction processes, in most cases, they reported that displacement depended on which public housing projects were available to receive families at a given point in time.

of the Mapocho River, who were displaced in 1982 after it flooded. More than 3,000 families from the slums El Ejemplo, El Esfuerzo, and El Trabajo—originally located in Las Condes, a rich municipality—were relocated to La Pintana and San Ramón, two low-income municipalities in the south of the city.

Using data on slums' characteristics from the MINVU's slum censuses, we find the same patterns established by historians. We report means by intervention in Table 1. Columns (1) and (2) show that displaced slums are denser as they house fewer families in smaller land areas. They are closer to rivers, which proxy for risky locations, and the property prices that surround displaced slum areas are higher. We classify slums' names as either military or not, as a proxy for support for the dictatorial regime, and find that displaced slums are less likely to have a military-related name. We also find displaced slums are located in areas with higher average schooling and closer to downtown Santiago. Finally, treatment did not occur homogeneously across time, as more relocations occurred after 1982 due to a financial crisis. In column (3), we find the same patterns in municipalities. Last, in column (4) we compute the linear probability of displacement as a function of slums' characteristics. We find that time of treatment is the most predictive variable, but all slums' characteristics show the expected correlations with displacement.

Figure 1 plots the urban limits of Greater Santiago and its municipalities. Panel (a) depicts the location of slums in 1979, showing they were located throughout with no particular concentration in any municipality. Panels (b) and (c) show the location of the housing projects built to receive slum dwellers in 1985. The neighborhoods where housing projects for the displaced were built are represented by purple squares, and housing projects for the non-displaced are represented by blue triangles. Two important conclusions can be drawn from this figure: the new housing projects were disproportionately built in the peripheral areas of the city, and public housing projects were farther from job opportunities (in gray scale).

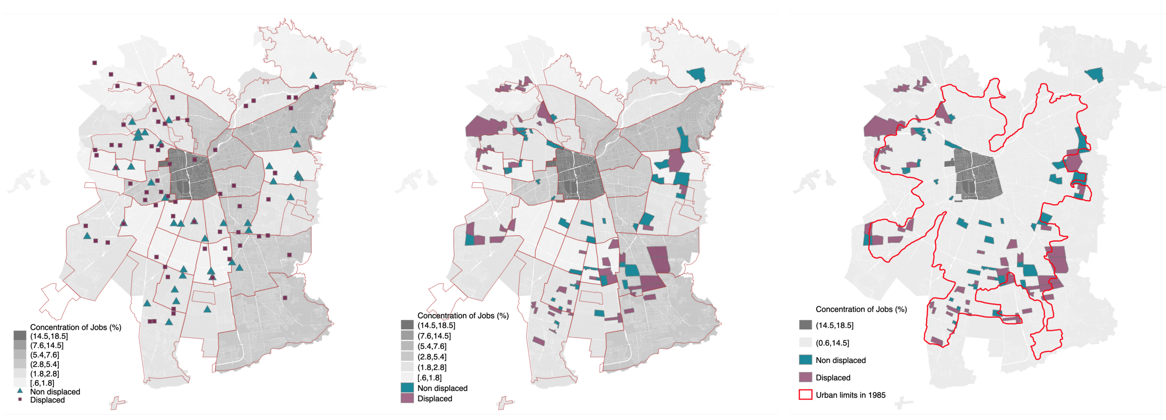
Aldunate et al. (1987) surveyed 592 displaced families. They reported their new homes had better housing conditions. However, they claimed their new neighborhoods were worse than their slums of origin, citing fewer job market opportunities and limited access to transportation, education, and health care services. They also perceived the new neighborhoods as more dangerous (see Appendix D for a summary).

Table 1: Slums' characteristics prior to intervention

	Non-displaced mean	Displaced mean	Difference (within municip.)	P(displacement)
	(1)	(2)	(3)	(4)
Area (hectares)	7.15	5.99	-1.26	0.006
			(1.14)	(0.016)
No. families	313.80	284.02	-112.5	-0.0003
			(67.16)	(0.0003)
Military name	0.26	0.16	-0.09	-0.124
			(0.09)	(0.109)
Distance to river (km)	1.74	1.59	-0.28	-0.027
			(0.21)	(0.049)
Log(price)	14.74	14.80	0.06	0.165
			(0.04)	(0.205)
Population's schooling	7.36	7.85	0.75	0.041
			(0.34)	(0.032)
Distance to CBD (km)	11.47	9.86	-0.79	-0.025
			(0.35)	(0.021)
Treated before 1982	0.88	0.48	-0.36	-0.317
			(0.09)	(0.095)
No. slums	43	73	116	116

Notes: The table shows summary statistics for non-displaced and displaced slums in sample. Slums' locations and characteristics constructed from FLACSO (1975, 1982, 1986), MINVU (1979), newspapers, and Population Census of 1982. Prices and population's schooling measured at the census district level where a slum is located. Column (3) reports the difference on each attribute within municipality of origin, and column (4) reports coefficients from a linear regression of the probability a slum is cleared (displacement) on slums' attributes. Robust standard errors in parenthesis. 10%*, 5%**, 1%***.

Figure 1: Eviction policies 1979–1985: Location of families living in slums



(a) Slum location before 1979 **(b)** Housing projects after 1985 **(c)** Housing projects and urban limits

Notes: Red lines represent the urban limits of Greater Santiago and grey lines its municipalities. Municipalities are colored in gray scale to depict the concentration of jobs across the city. These figures show the change in the location of families living in slums in 1979 (panel (a)) and their final destination in 1985 (panels (b) and (c)). Purple squares represent families living in slums that were moved out from their original location to a new neighborhood; blue triangles represent the families in slums that were not evicted but received a housing unit in their original location. The figures also show how the dispersion of the location of these families decreases and how they are relocated to the periphery of the city after the policy. For context, consider that the richest municipalities of Santiago at that time (and today) are the ones located in the northeast of this map and the poorer municipalities are located in the south and northwest 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 the population censuses of 1982 and 1992.

3 Data

In this section, we summarize the data collection process. We construct a novel dataset that tracks parents and their children, slum of origin, and destination neighborhood. We then match these individual records to administrative data on labor market outcomes.[18]

3.1 Archival data: Slums and homeowners

We digitize two slum censuses conducted by the MINVU in 1979 and 1980 that contain information on slum names, slum locations, and destination neighborhoods. We classify each slum as displaced or non-displaced and the final destination of displaced families. We then complement these data with information collected by Molina (1986) and Morales and Rojas (1986), who compiled a full list of slums, locations, and destination neighborhoods by year.

Next, we find the families in the program and collect archival data from the Regional Housing and Urban Planning Service and historical records kept by the Municipality of Santiago.[19] These records correspond to the lists of homeowners and their spouses who received a property deed through the program. We collect data for 20,781 unique recipients of social housing, representing 45% of the total number of recipients (Molina, 1986). We focus on individuals in Greater Santiago, excluding rural municipalities, and for internal validity, we exclude municipalities without variation in treatment (all families were either displaced or not displaced). This leaves us with 19,126 unique recipients of social housing from 14 different municipalities.[20]

The archival data contain information on the recipients of the property deed (heads of the household) and their spouses, full names, national identification numbers (NIDs), and new addresses. These records are grouped by year of eviction/urban renewal and destination neighborhood, and we match them to their slum of origin using the 1979 and 1980 slum censuses. We drop households with a missing NID due to mistakes or older versions that we cannot validate using contemporaneous data.[21] We end up with

[18] For a detailed description of the data collection process and variables definitions see Chapter 3 in Rojas Ampuero (2022).

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

[20] We exclude rural municipalities since most of the neighborhoods' characteristics that can be measured in the 1980s are only available in urban areas.

[21] To validate NIDs, we use data from Chilean electoral records in 2016.

16,936 families with at least one of the spouses having a valid NID. Because missing NIDs were more common among older or single individuals, in our matched data we are more likely to observe younger heads of households and married couples.

Compared with the entire program, our sample shows a higher likelihood of including displaced families (69% versus 65%). Rojas Ampuero (2022) compares the slums of families identified in the archives versus those documented by Morales and Rojas (1986), based on that we conclude that our dataset primarily includes families from larger and more urban slums, as measured by the distance to the CBD. We believe that this type of attrition might bias our results upward.[22]

3.2 Matching process: Children sample

Our next steps consist of locating the children of each family. We work with Genealog Chile and web scrape birth and marriage certificates for the Chilean population who were aged 18 and older in 2016.[23] The birth certificates contain full name at birth, birth date, NID number, and parents' full names. We match homeowners' archival data with their children using their NID. If the birth certificate did not contain at least one parent's NID, we match using a first name, a middle name, and two last names.[24]

We identify 47,044 children of 15,124 unique families (1,810 families did not have a child). Of these, 33,820 children come from a slum with a probability of being cleared within the common support range (which we detail in the next section). Of this subset, 24,277 are children aged 0 to 18 years at the time of the intervention (9,810 families). This constitutes our estimation sample. However, due to attrition from the loss of NID numbers, our matched sample likely over-represents younger children as we are missing the oldest heads of households.

[22] Rojas Ampuero (2022) chapter 3 makes a full comparison between the slums of the families in our sample and those in the full program, addressing concerns about selection due to attrition. She also estimates the probability of finding a slum as a function of its characteristics that we later use in the robustness checks section.

[23] We web scrape certificates from Chile's Civil Registration and Identification Service.

[24] In most Spanish-speaking countries, people have two last names. A child's first last name (in order from left to right) corresponds to the father's first last name, while the second last name is the mother's first last name. Hence, both paternal last names from the parents are transmitted to their children. For example, assume that María Pérez Rojas has a child with Juan Rodríguez González. Their child's family name will be "Rodríguez Pérez." See Rojas Ampuero (2022) for a full explanation of the process.

Table 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)
Demographics at intervention					
Displaced	0.796	0.807	0.790	0.039***	-0.019**
	[0.403]	[0.395]	[0.408]	(0.012)	(0.009)
Female	0.506	0.545	0.447	0.126***	-0.156***
	[0.500]	[0.498]	[0.497]	(0.006)	(0.007)
Age	8.188	8.127	7.868	-0.003****	-0.008***
	[4.860]	[4.877]	[4.790]	(0.001)	(0.001)
No. children	3.873	3.919	3.788	0.011***	-0.014***
	[1.817]	[1.831]	[1.751]	(0.002)	(0.002)
Firstborn	0.364	0.355	0.372	-0.013**	0.014*
	[0.481]	[0.479]	[0.483]	(0.006)	(0.008)
Oldest sibling	11.698	11.725	11.354	0.000	0.001
	[5.792]	[5.814]	[5.707]	(0.001)	(0.001)
Youngest sibling	5.191	5.150	5.025	0.002*	-0.001
	[4.216]	[4.201]	[4.121]	(0.001)	(0.002)
HH age	34.804	34.810	34.577	0.000	0.001
	[7.152]	[7.168]	[7.042]	(0.001)	(0.001)
Female HH	0.318	0.315	0.298	-0.006	-0.028***
	[0.466]	[0.464]	[0.458]	(0.009)	(0.009)
Married HH	0.794	0.795	0.807	-0.013**	0.023**
	[0.405]	[0.403]	[0.395]	(0.006)	(0.011)
Father Mapuche	0.050	0.053	0.053	0.036***	0.027*
	[0.219]	[0.224]	[0.224]	(0.010)	(0.014)
Treated before 1982	0.534	0.525	0.536	-0.021***	-0.001
	[0.499]	[0.499]	[0.499]	(0.008)	(0.007)
Variables measured after 2007					
Died before 2007	0.006	-	-	-0.822***	-0.636***
	[0.076]	-	-	(0.009)	(0.016)
Mother's schooling 0-6[a]	0.580	0.596	0.567		
	[0.494]	[0.491]	[0.496]		
Mother's schooling > 6	0.340	0.339	0.360		
	[0.474]	[0.473]	[0.480]		
Mother's schooling unknown	0.080	0.065	0.073		
	[0.271]	[0.246]	[0.261]		
# times in RSH	16.516	20.090	16.359		
	[9.804]	[6.595]	[9.636]		
Individuals	24,277	19,953	16,030	24,277	24,277
Matching rate		82.2%	66.0%	82.2%	66.0%

Notes: The table shows 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), probability of dying before 2007, and municipality of origin fixed effects. Standard errors are bootstrapped with 200 replications in parentheses. 10%*, 5%**, 1%***. Standard deviations are in brackets. Adjusted R^2 for regressions in (4) and (5) are 0.065 and 0.054 correspondingly. (a) Mother's years of schooling is observed in the sample of mothers found in the RSH and conditional on a mother being alive after year 2007.

3.3 Measuring outcomes: Matching to administrative data

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

The second source of administrative data is the Gestión de Reportes e Información para la Supervisión de Mutuales (GRIS), 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 listed in this database is formally employed. We observe monthly data on taxable income from July 2016 to December 2019.

3.4 Municipality and district attributes

We measure location attributes by municipality and census district, from the 1982 Census of Population, which contain data on education and employment status. We add 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, we obtain publicly available data from Greater Santiago's subway system on subway stations built in Santiago. Finally, we obtain neighborhood-level property prices from newspaper listings from 1978 to 1985 that we collect and digitize.

3.5 Estimation sample and summary statistics

Our estimation sample includes children from municipalities with both displaced and non-displaced slums in urban areas, who lived in slums equally likely to be cleared (e.g., common support between cleared and redeveloped slums). It includes all children who, as adults, were at least 25 years old at the time of income/employment measurement.[25]

[25] This is the minimum age we observe in our sample matched to the RSH data.

Table 2 presents summary statistics of the children in the full sample at the time of the intervention (column (1)). The table shows that 79.6% come from displaced families. Half are female, and the average age is 8.18 years. Families have four children on average, and 36% are firstborn. Their parents are 35 years old on average at baseline, 32% come from a female-headed household, 79% have parents who are married at the time of the intervention, and 5% have an indigenous father (father has a Mapuche last name). Only 0.6% of the children in our baseline sample died before 2007. In addition, 82% of the baseline sample appears at least once in the RSH (column (2)) and 66% in the GRIS (column (3)). In the RSH we match slightly more children from displaced families, with a share of 80.7%, and in the GRIS we match slightly fewer children, with a share of 79%.

In the last two columns, we regress the probability of being found in each of the two datasets on a set of demographic characteristics observed at baseline. Two demographic variables are critical for matching: age and gender. Age is determined by data availability; as the table shows, the newer the data, the less likely we are to match with older children. For gender, we find that females are over-represented in the RSH and under-represented 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, we expect more women to be in the RSH than in the GRIS. Since in Chile the formal labor force participation rate for women is only 50%, it is not surprising that fewer women are in the GRIS. Last, no child deaths are reported in either datasets; however, deaths are too rare to account for all non-matched individuals.

These summary statistics, combined with the attrition rates from the archives, imply that our matched RSH sample of children would correspond to 44% of children from slums with common support in urban areas.[26] In this group, children who were displaced, young, or female are over-represented. However, our concern for bias in estimates arises from the disproportionate presence of young children and females if these characteristics are unbalanced between the displaced and non-displaced, or if

[26]According to Molina (1986), the total number of families in the program was 40,491, of whom 75% were located in urban municipalities. Of these, we estimate that 65% come from slums with common support. Based on these shares, the number of families in the program with common support in urban areas would have been 19,739. In our archival sample, there are 10,660 families in urban areas with common support, 54% of the total.

they affect these groups differently. In the next section we show that this is not the case.

Finally, and not surprisingly, we conclude that the individuals in our sample are poor, as they have lower incomes than the universe of those in the RSH (see Figure A.2). In 2018 the population in the RSH reports a median monthly salary of CLP$183,998 (∼ US$250). The median monthly salary in our sample is lower, CLP$178,855 (∼ US$240). These numbers are low compared to estimates for the full Chilean population. For example, the median monthly salary for a Chilean worker in 2018 is CLP$450,000 (∼ US$600), more than twice the median salaries in the RSH.[27]

4 Empirical Strategy

4.1 Identifying a displacement effect

To estimate the impact of the forced displacement on children, we exploit the fact that treatment was determined at the slum level and not based on individual families' demographics. The empirical strategy involves comparing the children of displaced families with those of non-displaced families that come from slums with the same probability of being cleared. The process of selecting slums into displaced and non-displaced did not depend on households' characteristics but rather on the feasibility of renewal on-site.

Under the assumption that we know and observe the slums' characteristics that determine treatment, we can compute the probability of a slum being cleared as a function of its urban characteristics. Then, can compare the outcomes of children in a set where they have the same propensity of being displaced. Thus, any differences between children in the displaced and non-displaced group are attributed to the eviction process and subsequent relocation to a new housing project.

We estimate a linear model to study the displacement's impact on children, using

[27] This discrepancy between national estimates and the RSH data occurs due to under-reporting (the income data we use are self-reported), a higher proportion of informality in the RSH compared with the rest of the population, and lags in the updates of the RSH data, as they are self-reported. In our sample period, around 70% of the total Chilean population is registered in the RSH and report higher informality compared with the full labor force. Informality pre-Covid in Chile was about 20% (CASEN, 2017), while in the RSH, 40% of adults report working without a contract.

the following specification:

$$Y_i = \alpha + \beta Displaced_{s\{i\}} + \psi_o \times p(X_s, \psi_o) + X_i'\theta + \varepsilon_i, \qquad (1)$$

where Y_i is the average outcome for individual i in adulthood, such as labor income, employment status, or years of schooling,[28] and $s(i)$ indexes the slum of origin for individual i's family. The variable $Displaced_{s\{i\}}$ equals 1 if individual i'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. $p(X_s, \psi_o)$ is the propensity score that is a function of slums' characteristics X_s. These include number of families, area in hectares, property prices, period of treatment, and municipality of origin fixed effects since the propensity to clear slums varied by municipality of origin. For precision, in equation (1) we add baseline controls for individual and family characteristics at the time of the intervention, X_i, that include gender, child's year of birth fixed effects, female head of household, married head of household, head of household's age, father is Mapuche (indigenous), and year of intervention fixed effects (1979 to 1985) that control for aggregate temporal differences across the six years this housing program was in effect. We use bootstrapped standard errors.[29]

In addition, the estimation of a propensity score model requires the unconfoundness assumption to hold, which means that conditional on the propensity score, the outcome Y is independent of displacement, and also overlap, which means that we compare displaced and non-displaced children in the common support of the propensity score (Rosenbaum and Rubin, 1983). Note that our propensity score is only a function of slums' characteristics (s), not individuals' (i), because the policy function was at the slum level and not at the individual level.

We implement the method in four steps. First, we estimate the propensity score $\hat{p}(X_s, \psi_o)$ at the slum level using a logit function, slum characteristics from Table 1, and municipality of origin fixed effects. This requires treatment variation within municipalities. Second, we impose common support. Based on the propensity score densities

[28] Average outcomes are computed for age-adjusted employment and earnings outcomes.

[29] We also compute other clustering such as clustering at the level of slum of origin and Conley standard errors, which we discuss in the next section.

by treatment in Figure A.1, we keep slums where $0.2 < \hat{p}(X_s, \psi_o) < 0.97$. Third, we generate dummies for each quintile of distribution of the estimated propensity score by slum. Finally, we regress the outcomes of interest on the displacement dummy and a full set of propensity score dummies that include propensity score quintile dummies, municipality of origin fixed effects, and their interactions ($\psi_o \times p(X_s, \psi_o)$ in equation (1)). This ensures that we compare displaced and non-displaced children within the same municipality and in the same quintile of the propensity score estimate.

4.2 Evaluation of the identification strategy

The validity of our research design depends on whether the decision to displace a slum was uncorrelated with the characteristics of families, conditional on the probability that their slum was cleared. Under the assumption that conditional on the policy function, $p(X_s, \psi_o)$, the covariance between $Displaced_{s\{i\}}$ and ε_i is 0, the coefficient β estimates the displacement's causal effect on children's outcomes. We first compare the demographics of the displaced and non-displaced children at the time of the intervention.

Without conditioning on $p(X_s, \psi_o)$, the demographics of displaced and non-displaced children look very similar but not identical. Table 3, Panel A, column (1) reports means for several demographics for the non-displaced. Column (2) shows that conditional on municipality fixed effects, ψ_o, there are no statistical differences between both groups for 14 out of 16 observables. The main difference is that displaced children are less likely to come from families with a married head of household (6.5% less). Column (3) compares children, re-weighting the sample by the inverse probability of being treated ($1/\hat{p}$ for displaced and $1/(1-\hat{p})$ for non-displaced children). The adjusted differences do not show statistical differences between either group in any of the demographics in Panel A. This is reassuring as we do not force balance because our propensity score does not include individual-level characteristics.

Table 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.)	Difference (inv. weight)	Non-displaced mean	Difference (within municip.)	Difference (inv. weight)	Non-displaced mean	Difference (within municip.)	Difference (inv. weight)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Demographics									
Female	0.505	0.002	0.006	0.546	0.000	0.008	0.451	-0.006	0.010
		(0.005)	(0.006)		(0.006)	(0.006)		(0.009)	(0.011)
Age	8.202	-0.047	-0.204	8.268	-0.161	-0.335	7.880	-0.082	-0.290
		(0.253)	(0.432)		(0.255)	(0.438)		(0.251)	(0.420)
Firstborn	0.371	-0.009	0.008	0.356	-0.002	0.013	0.375	-0.005	0.018
		(0.011)	(0.013)		(0.011)	(0.010)		(0.012)	(0.016)
No. children	3.747	0.137	-0.024	3.838	0.078	-0.079	3.686	0.109	-0.114
		(0.094)	(0.122)		(0.092)	(0.119)		(0.103)	(0.152)
Oldest sibling	11.567	0.078	-0.518	11.771	-0.103	-0.754	11.272	0.012	-0.751
		(0.352)	(0.524)		(0.346)	(0.494)		(0.357)	(0.580)
Youngest sibling	5.296	0.078	-0.075	5.311	-0.192	-0.176	5.109	-0.151	-0.071
		(0.352)	(0.309)		(0.194)	(0.289)		(0.195)	(0.308)
HH age	35.081	-0.366	-0.939	35.178	-0.453	-0.985	34.908	-0.439	-1.309**
		(0.445)	(0.611)		(0.449)	(0.609)		(0.453)	(0.624)
Female HH	0.284	0.031	-0.028	0.285	0.028	-0.039	0.267	0.032	-0.022
		(0.033)	(0.080)		(0.035)	(0.087)		(0.031)	(0.071)
Married HH	0.846	-0.054***	-0.047	0.847	-0.054***	-0.046	0.862	-0.062***	-0.051*
		(0.014)	(0.028)		(0.016)	(0.030)		(0.013)	(0.026)
Father Mapuche	0.045	0.008	0.002	0.048	0.006	0.001	0.046	0.011	0.001
		(0.005)	(0.009)		(0.006)	(0.009)		(0.007)	(0.011)
Extended family in slum	0.158	0.042*	-0.002	0.160	0.047	-0.002	0.155	0.039*	0.003
		(0.023)	(0.028)		(0.023)	(0.029)		(0.023)	(0.027)
Mother's education[a]									
$0-6$ years	0.565	0.018	-0.009	0.593	0.005	-0.019	0.556	0.014	-0.023
		(0.021)	(0.023)		(0.020)	(0.017)		(0.021)	(0.027)
>6 years	0.360	-0.025	-0.009	0.352	-0.016	0.001	0.377	-0.023	0.002
		(0.020)	(0.018)		(0.020)	(0.016)		(0.022)	(0.021)
Unknown	0.075	0.006	0.018	0.055	0.012*	0.018**	0.067	0.009	0.021
		(0.007)	(0.012)		(0.006)	(0.007)		(0.008)	(0.014)
B. Houses received by families									
Home value	260.341	-8.363	19.179	260.384	-8.401	20.925	259.956	-8.246	19.787
		(10.436)	(33.851)		(10.859)	(34.975)		(10.392)	(33.137)
Monthly payment	2.391	-0.095	-0.034	2.403	-0.092	-0.096	2.376	-0.092	-0.024
		(0.274)	(0.436)		(0.290)	(0.421)		(0.273)	(0.420)
C.Father's employment[b]									
Employee	0.680	0.000	0.008	0.682	0.000	0.010	0.686	0.000	0.009
		(0.003)	(0.007)		(0.004)	(0.008)		(0.004)	(0.007)
Self-employed	0.221	0.001	-0.004	0.222	0.000	-0.005	0.215	0.000	-0.003
		(0.003)	(0.005)		(0.004)	(0.006)		(0.004)	(0.004)
Children		24,277			19,953			16,030	
Families		9,810			9,091			7,624	
Slums		64			64			63	
Municipalities		14			14			14	

Notes: Columns (1), (4), and (7) report means for non-displaced children conditional on their municipality of origin in each corresponding sample. Columns (2), (5), and (8) report the within difference that corresponds to the coefficient *Displaced* in equation (1) conditional on municipality of origin fixed effects; and Columns (3), (6), and (9) report the difference between displaced and non-displaced after weighting for the inverse probability of each child's slum being cleared. (a) Mother's years of schooling is observed in the sample of mothers found in the RSH and conditional on a mother being alive after year 2007. (b) Father's employment is the out-of sample prediction using estimates from Municipality of Santiago (see text and table A.4). Variables home value, and total debt are measured in UF (inflation adjusted index) and adjusted by year of intervention (see Data Appendix for variable definitions). Standard errors are clustered by slum of origin in columns (2), (5) and (8), and bootstrapped standard errors in columns with 200 replications in (3), (6), and (9). 10%*, 5%**, 1%***.

The results in Panel A are very similar for the children we matched to the RSH (columns (4)–(6)) and for those we matched to the GRIS (columns (7) and (9)). For only one and two demographics, there are differences between groups. Since age determines matching to administrative data, we find in the RSH that displaced mothers are slightly less likely to be present, and in the GRIS sample, heads of displaced households are younger. Though statistically significant, the differences in levels are not large.

Panel B of Table 3 reports differences in home values measured in UF (indexed by inflation).[30] Once we re-weight the sample, the results indicate that displaced families received houses that are 7.4% more expensive, with 1.4% lower monthly payments. This could indicate overcompensation for moving, as monthly payments are smaller. However, none of these differences are statistically different from zero.

We do not observe parents' earnings at baseline, a caveat of our identification strategy. However, the lack of statistical difference in monthly payments between groups suggests that household earnings are similar among displaced and non-displaced families. This is based on administrative MINVU guidelines, which state that monthly payments should be proportional to a household's total earnings.[31]

To provide more evidence on parents' labor market outcomes, we use program records for the Municipality of Santiago, located in the center of Greater Santiago.[32] These records are kept separately by the local government and report employment status at baseline. We focus on fathers' employment because they were more likely to work outside the house and be breadwinners. In Appendix Table A.4, we perform the same exercise as in Table 3 for children living in slums in the Municipality of Santiago. We find that the family structures of displaced and non-displaced children are similar to the full sample. However, parents in the displaced group are younger and are 7.4 percentage points more likely to be employed.

We use this information to predict fathers' employment status by regressing a dummy that indicates if a father works and children and family characteristics (gender, family size, age at intervention, age of siblings, head of household marital status,

[30]We adjust home values by year of treatment because after 1982, the houses built by the program became cheaper due to the financial crisis.

[31]The archival documents under Decree 2552 stated "the initial resulting monthly payment cannot be greater than 20% of the joint monthly earnings between the beneficiary and their spouse."

[32]The municipality is also the capital of the Greater Santiago Metropolitan area. It is also known as Downtown, and the CBD is located there.

mother is head of household, and father is Mapuche). Then, we use the coefficient estimates to predict employment status in our full sample of children. The bottom Panel C of Table 3 shows the differences in predicted employment between groups, and we find null differences on fathers' predicted employment between displaced and non-displaced children. Note that this also serves as a joint test of significance of demographics.

5 Results

5.1 Displacement effect on Labor market outcomes

We start our analysis by examining the earnings and employment of individuals (aged 0 to 18 at baseline) who are 25 to 60 years old at the time their income is measured, with non-missing education. The main outcomes for earnings are 1) self-reported labor earnings in the RSH between 2007 and 2019 and 2) taxable wages from social security contributions in the GRIS between 2016 and 2019. Self-reported earnings measure income from both formal and informal employment. They include wage income and proprietors' labor income but exclude pensions and transfers.[33] Taxable wages only measure formal earnings, meaning that if we do not observe an individual in the GRIS, their formal wages were zero in the corresponding period, and thus we can impute zeros. Both labor earnings and taxable wages are measured in 1,000 Chilean pesos per month (CLP$1,000/month).[34] In addition, employment is reported in the RSH and includes both formal and informal employment. We compute one observation per individual by collapsing each outcome after controlling for age and semester-year dummies.

Table 4 shows that displacement has a negative effect on earnings (Panels A and B) and a null effect on employment (Panel C). Column (1) reports the difference in outcomes between displaced and non-displaced children conditional on the municipality of origin and year of intervention fixed effects. Column (2) includes baseline controls for precision, and column (3) adds slums' characteristics before the intervention. This last column indicates that displaced children have lower future earnings compared with those who were not displaced. The coefficient of –21.121 in column (3) of Panel A is statistically significant at 1%, meaning that displaced children, as adults, earn an

[33] We do not impute zeros for individuals who we do not find in the matched sample, and we retain zeros for individuals who reported their earnings as such.

[34] CLP$1,000 corresponds to approximately US$1.5 in 2019.

average of 13.9% less per month than the non-displaced (see the row labeled "Percent effect").

Panel B, column (2) shows a more negative coefficient on taxable wages of –37.958, which is statistically significant at the 1% level. In addition, the percentage effect shows a very similar result of 13.4% lower earnings for the displaced group. In contrast, Panel C shows no effect on employment. The similarity in percentage effects for both types of earnings is important, as it goes against the concern about differential bias in self-reported outcomes. This implies that both groups under-report their earnings in the RSH at similar rates.

In column (4) we re-weight observations by the inverse probability of being treated at the slum level. Compared to column (3), the results are less negative but still economically meaningful: as adults, displaced children earn 11% less per month than those who were not displaced. Column (5) estimates the p-score model non-parametrically as described in Section 4. The coefficients are very similar to column (3), but the percentage effects are more negative: the displacement effect on total labor earnings is –14%, and on taxable wages it is –14.7%. This column is our preferred specification as it flexibly accounts for differences in the outcomes of displaced and non-displaced children with the same probability of their slum being cleared. We include column (6) as a robustness check, estimating a block-propensity score estimate, which requires us to impose common support of the propensity score within municipalities of origin. This approach is more restrictive because it requires several treatment units per block (municipality), significantly reducing our sample size. However, the results are very stable across columns.[35]

For comparison, all columns in Table 4, except column (4), report Conley standard errors in brackets (Conley, 1999) to account for any spatial dependence across slums that are close to each other.[36] The table shows that Conley standard errors deliver very similar results to bootstrapping by slum of origin. Thus, in all of the following estimations, we report bootstrapped standard errors.

[35] In Appendix Table A.5 we replicate the analysis of Table 4 using the sample of column (6), and find that the results are very stable.

[36] We calculate Conley standard errors for all regressions using a 8-km cutoff distance. This distance of 8 km is selected because it maximizes the standard errors for our main outcomes, as shown in Table B.4. For estimating the standard errors, we consider different cutoffs ranging from 2 km to 15 km. The upper bound is set to 15 km as this includes the largest municipality in Santiago in terms of square kilometers.

Table 4: Displacement effect on labor income and employment

Model	OLS	OLS	OLS	Inv-weight	P-score	Block p-score
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A.	Outcome: Self-reported earnings (CLP$1,000/month)					
Displaced	-18.645	-17.389	-21.121	-16.514	-21.207	-16.351
	(5.055)***	(4.600)***	(4.758)***	(4.869)***	(6.659)***	(4.749)***
	[5.317]***	[4.763]***	[4.808]***		[8.035]	[4.816]
Non-displaced mean	166.96	166.96	166.96	166.80	151.293	183.94
Percent effect	**-11.7**	**-10.9**	**-13.9**	**-10.9**	**-14.0**	**-9.9**
Adjusted R^2	0.009	0.128	0.129	0.133	0.131	0.134
Panel B.	Outcome: Taxable wages from social security (CLP$1,000/month)					
Displaced	-39.092	-34.161	-37.958	-35.461	-31.967	-46.358
	(8.638)***	(7.360)***	(8.380)***	(7.513)***	(13.711)**	(13.331)***
	[9.075]***	[7.719]***	[8.735]**		[12.192]***	[9.476]***
Non-displaced mean	291.713	291.713	291.713	281.191	238.178	297.190
Percent effect	**-13.8**	**-12.1**	**-13.4**	**-14.1**	**-14.7**	**-16.0**
Adjusted R^2	0.014	0.059	0.059	0.073	0.060	0.064
Panel C.	Outcome: 1[Employed]					
Displaced	-0.007	-0.006	-0.006	-0.004	0.001	-0.005
	(0.011)	(0.010)	(0.013)	(0.010)	(0.015)	(0.015)
	[0.012]	[0.011]	[0.012]		[0.018]	[0.012]
Non-displaced mean	0.644	0.644	0.644	0.627	0.654	0.664
Percent effect	**-1.1**	**-0.9**	**-0.9**	**-0.6**	**0.2**	**-0.7**
Adjusted R^2	-0.000	0.117	0.117	0.134	0.119	0.116
Individuals	19,953	19,953	19,953	19,953	19,953	13,728
Municipality of origin FE	✓	✓	✓	✓	✓	✓
Baseline controls		✓	✓	✓	✓	✓
Slum characteristics			✓	✓	✓	✓

Notes: Regressions for children aged 0 to 18 at baseline that are matched to the RSH and GRIS data, and that report non-missing schooling. All regressions control for year of intervention fixed effects. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children per couple, firstborn dummy, Mapuche first last name dummy, and year of birth fixed effects. Slums' characteristics include area, number of families, military name, distance to river, and log of property prices at origin. Propensity score matching results include a full set of municipality of origin fixed effects interacted with the propensity score percentile dummies. The different set of controls specified in the corresponding column are used in the propensity score estimation. Standard errors are clustered by slum of origin in parentheses in columns (1) to (4), standard errors of the propensity score results are bootstrapped with 200 replications in columns (5) and (6), and Conley standard errors are in brackets. 10%*, 5%**, 1%***. The row labeled as "Percent effect" stands for "percentage variation with respect to non-displaced mean."

Displaced children's lower future earnings are related to higher informality. Table 5, Panel A shows that as adults, displaced children are 5.8 percentage points more likely to work in temporary jobs, which is 10.6% more than non-displaced children. They are also 3.2 percentage points less likely to contribute to social security, which is 6.6% less than the non-displaced. Even though working with a contract shows no statistical difference between displaced and non-displaced children, the estimate is large: displaced children are 2.6 percentage points less likely to work with a contract as an adult, equivalent to a 6.9% lower probability relative to non-displaced children. This is similar to the percentage effect on the probability of contributing to social security.

In Panel B we split self-reported earnings between formal and informal sources (with and without a contract). The results show that most of the negative effect is due to lower earnings in the formal labor market, consistent with the negative estimate on taxable wages. In addition, the percentage effect is larger on self-reported formal earnings than on taxable wages.

5.2 Schooling outcomes

Next, we study the displacement effect on schooling outcomes. The results, shown in Panel C of Table 5, indicate that displaced children obtain 0.641 fewer years of schooling than non-displaced children. We find that the higher the education level, the more the negative percentage effect appears: displaced children are 18% less likely to graduate from high school, 27.5% less likely to attend a two-year college (for technical degrees such as mechanics, electrical technology), and 10.5% less likely to attend a five-year college (for professional degrees such as medicine, engineering, economics), though this last finding is not different from zero. Overall, these results suggest that displacement affects children's educational attainment by reducing their likelihood of graduating high school, and hence their likelihood of attending college is even lower.

The negative effect on years of education can explain about 46% of the negative effect on earnings that we find in our sample. According to CASEN (2017), one extra year of education for the population that finishes high school increases earnings by about 10%.[37] The displacement effect on earnings is –14%, while the effect on education is

[37] CASEN stands for Encuesta de Caracterización Socioeconómica (Socioeconomic Characterization Survey), and it is similar to the US Current Population Survey.

Table 5: Displacement effect on employment and education outcomes

Outcome	Displacement effect	Mean non-displaced	Percent effect (%)	P-value/ Sharp p-value
Panel A. Employment				
Employed=1 (sr)	0.001	0.654	0.2	0.964; 0.313
	(0.015)			
Contract=1 (sr)	-0.026	0.377	-6.9	0.193; 0.095
	(0.020)			
Temp worker=1 (sr)	0.058***	0.549	10.6	0.000; 0.001
	(0.016)			
Contributes to SS=1	-0.032**	0.486	-6.6	0.046; 0.039
	(0.016)			
Panel B. Income				
Formal earnings (sr)	-16.678***	104.532	-16.0	0.010; 0.015
	(6.455)			
Informal earnings (sr)	-4.530	46.761	-9.7	0.195; 0.095
	(3.496)			
Taxable wages	-31.967**	238.178	-14.7	0.020; 0.023
	(13.711)			
Panel C. Education				
Years of schooling (sr)	-0.641	11.645	-5.7	0.000; 0.001
	(0.108)***			
HS graduate=1 (sr)	-0.120	0.670	-17.9	0.000; 0.001
	(0.018)***			
2-year college=1 (sr)	-0.042	0.153	-27.5	0.0002; 0.001
	(0.011)***			
5-year college=1 (sr)	-0.006	0.057	-10.5	0.545; 0.279
	(0.009)			

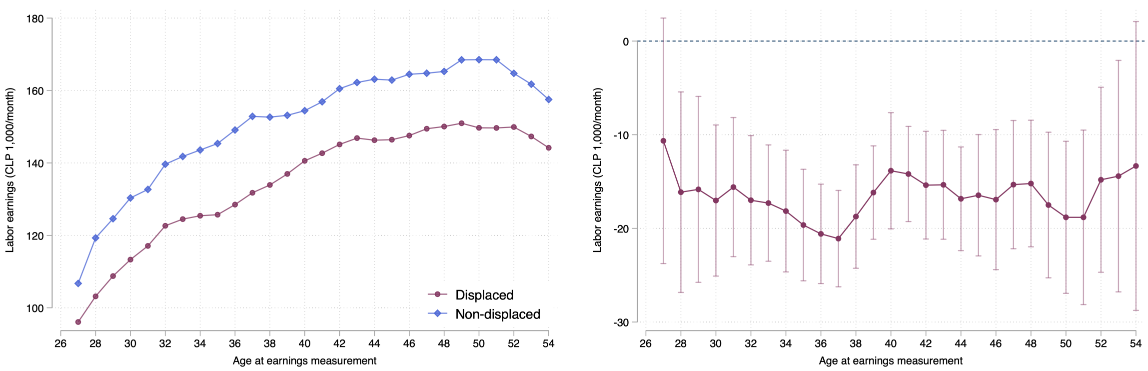
Notes: This table shows propensity score estimates equivalent to column (5) in Table 4, for children aged 0 to 18 at baseline that are matched to the RSH and GRIS data that report non-missing schooling. Bootstrapped standard errors with 200 replications in parenthesis, 10%*, 5%**, 1%***. Column (4) reports p-values and sharp p-values for the hypothesis that each coefficient is equal to zero. Sharp p-values are the corrected p-values for multiple hypotheses comparison based on Anderson (2008)'s method.

–0.64 years of education. Hence the decrease in years of schooling accounts for about 46% of the total effect on earnings.[38]

[38] We repeat this exercise using a mediation analysis, and our results are similar. Our results indicate that 55% of the total effect on earnings is mediated by the reduction in years of schooling relative to non-displaced children.

Figure 2: Displacement effects on labor market outcomes by age at earnings measurement: Children aged 0 to 18 at baseline

(a) Labor income trajectories (CLP$1,000/month) **(b)** Displacement effect on labor income



Notes: Regressions for children aged 0 to 18 at baseline that are matched to the RSH data, and report non-missing schooling. Standard errors are bootstrapped with 200 replications. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, father Mapuche, firstborn dummy, year of birth fixed effects, and year of intervention fixed effects. Figures (a) and (c) plot the predicted trajectories for the displaced and non-displaced children between ages 27 to 54 from the from the regression $y_{it} = \sum_{\tau=27}^{54} \beta_\tau Displaced * 1[Age = \tau] + \sum_{\tau=26}^{54} \delta_\tau 1[Age] + \psi_o + \hat{p}(X_s, \psi_o) \times \psi_o + X'_{it}\gamma + u_{it}$. Figures (b) and (d) plot coefficients β_τ and their 95% confidence intervals. Other outcomes can be found in A.4.

5.3 *Labor market outcomes across the age cycle*

We take advantage of the RSH's panel structure to estimate a displacement effect on children's future earnings across the age cycle (Figure 2). We find that across the entire age distribution, the income trajectories of displaced children are below those of the non-displaced, with the difference in earnings already being negative by age 28 and widening with age. Figure A.4 presents employment trajectories and displacement effects on formal and informal earnings separately. The results show that the negative effects are reflected mostly on formal earnings and formal employment (with a contract), though for older ages, the difference on informal earnings widens between displaced and non-displaced children.

5.4 *Robustness checks*

5.4.1 *Improvements in the comparison group*

We first investigate the possibility of a displacement effect determined by an improvement of the comparison group rather than a negative effect on the displaced group. If non-displaced families and their children saw an improvement in their neighborhoods, especially in richer municipalities after the expulsion of low-income families, the negative displacement effect we find might not be a negative effect on the displaced but a positive effect on the comparison group.[39] To test for this hypothesis, we perform two exercises. In the first exercise, we divide the non-displaced group into two: those who lived in slums near a displaced slum (at the origin) before the treatment and those in slums without a displaced slum nearby. The rationale for this is that the first group should have seen a bigger improvement in neighborhood quality if the cleared area was rebuilt. Table B.1 shows that non-displaced children who live within 0.5 or 1 km of a displaced slum have higher earnings as adults relative to non-displaced children without nearby displaced slums. The effect is small and not statistically different than zero, but more importantly, its inclusion does not change the effect on the displaced children.

In the second exercise, we drop municipalities one by one (Figure B.1) and find that our results are not driven by any particular municipality. We were mainly interested in dropping the richest municipalities since they were net expellers (i.e., expelled more families than they received) and might have seen the biggest improvements in land prices after the forced evictions. Our results do not indicate that dropping municipalities like Las Condes or La Reina (the richest in our sample) change our effects by a large magnitude.

5.4.2 Attrition

A second concern is related to whether differential attrition due to selection from the national archives, or from matching to administrative data, could bias our results. To address this, we estimate the probability that a slum is found in the archives as a function of slum and neighborhood characteristics by origin.[40] We include estimates of the propensity of being found in the archives as a polynomial in our baseline regressions and find no evidence of differential attrition driving our results (see Appendix Table B.5). A different approach to handling missing data is to compute Lee bounds (Lee, 2009). We compute tightened Lee bounds by municipality of origin and demographic controls (age, gender, female-headed household, and number of children per couple). Our findings reveal that our baseline estimate of the displacement effect is within bounds.

[39] A fraction of places where slums were originally located were used to build parks or new public goods, especially in municipalities that collected higher revenues.

[40] Chapter 3 in Rojas Ampuero (2022) describes the how the slums in our sample are different from slums we did not find in the archives.

Furthermore, both the upper and lower bounds are negative and statistically different from zero on self-reported earnings and taxable wages (Table B.3).[41]

5.4.3 Selection on unobservables

In the previous sections we provided evidence of no selection on observables, conditional on the policy function. However, some concerns arise if the slum propensity score and baseline controls do not account for all the selection types in our sample. For example, we do not observe other characteristics of slum dwellers at baseline, such as their relationship with local authorities or the difficulties they had when they left their original location. Political considerations are also relevant, for example, due to selection into treatment because of political opposition to the dictatorial regime.

To account for potential selection on unobservables, we perform two exercises. First, we use data from the 1980 slum census conducted by the MINVU, which reports a list of all remaining slums to be cleared and their assigned treatment. We find that about 20% of slums assigned to be non-displaced end up being displaced, especially after the 1982 financial crisis. Thus, we use this assignment as an instrument for displacement in the sample of slums cleared after 1980, under the assumption that the original assignment was determined by urban conditions and not by slum family characteristics.[42] Table B.2 shows that the IV coefficient is very similar to our propensity score estimate on total labor earnings and is more negative on informal earnings.

Second, we perform two more exercises, where we follow Oster (2019)'s procedure and run permutation tests on our main outcomes. We would need an extreme degree of selection on unobservables relative to the baseline controls—even larger than what Oster (2019) suggests—to conclude that our displacement effects on earnings and schooling are zero or even positive (see Appendix B.1). Finally, permutation tests show no evidence of selection (see Figure B.2).

[41] The procedure can be found in Tauchmann (2014). To compute tightened bounds, it is necessary to have treatment variation for each value of the control variables. Therefore, we create dummies for each of the continuous covariates.

[42] Baum-Snow (2007) is an example of a research paper that uses this type of identification strategy.

5.5 Displacement effect by age at intervention

The displacement effects may vary by age at intervention, as has been shown in previous settings (Chetty et al., 2016; Chyn, 2018; Laliberté, 2021; Nakamura et al., 2022). This pattern has been called a *childhood exposure effect* of neighborhoods, meaning 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 exposed than teenagers, and thus we expect a more negative displacement effect for young children in our setting.

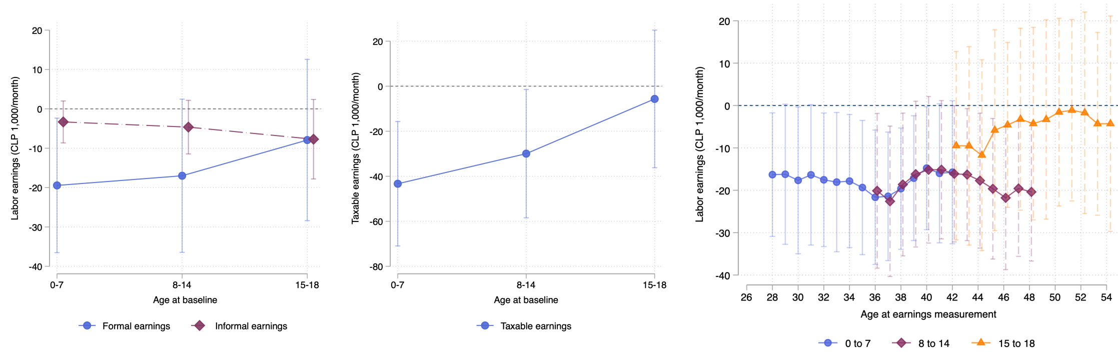
We test whether the displacement effect varies by age at baseline. To do so, we stratify our sample by age at intervention into three groups: 0–7, 8–14, and 15–18 years.[43] We find evidence of an exposure effect on labor income, driven by formal earnings. Figure 3, panels (a) and (b) show that the displacement effect on formal self-reported earnings and taxable wages is more negative for children under 15 years old. We also reject the equality of coefficients between teenagers and younger children. Panel (a) also shows that the age gradient on informal earnings is the opposite to an exposure effect, as teenagers face a more negative effect (though we cannot reject the equality of coefficients). Finally, panel (c) takes advantage of our dataset's panel structure and plots displacement effects across the age cycle. This confirms our aggregate findings: teenagers in our sample face a less negative effect on earnings for all the adulthood ages we observe, though still negative. For children under 15 years old at baseline, the displacement effects become more negative with age.

Our results confirm the established concept of neighborhood exposure effects. The richness of our data allows us to differentiate these effects by types of earnings, and we find that the negative exposure effect predominately influences the formal portion of children's future labor earnings. In addition, the negative displacement effect on formal and informal earnings for teenagers suggests that the disruption effects of moving are not negligible for this age group. We explore the distinction between disruption and neighborhood effects in more detail in the mechanisms section.

[43] We choose these three 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 suggest a break on labor earnings and taxable wages at age 14, and another break in taxable wages between ages 6 and 7. See Appendix Figure A.5 for more details.

Figure 3: Displacement effects on earnings by age at baseline

(a) Labor earnings **(b)** Taxable earnings **(c)** Labor earnings by age



Notes: Regressions for children aged 0 to 18 at baseline that are matched to the RSH data, and report non-missing schooling. Standard errors are bootstrapped with 200 replications. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, father Mapuche, firstborn dummy, year of birth fixed effects, and year of intervention fixed effects. Figures (a) and (b) plot coefficients β_τ and their 95% confidence intervals from regression (1) stratified by age group, and figure (c) plots coefficients $\beta_{\tau g}$ and their 95% confidence intervals from $y_{it} = \sum_{g=1}^{3}\sum_{\tau=27}^{54} \beta_{\tau g} Displaced * 1[Age = \tau, Group = g] + \sum_{g=1}^{3}\sum_{\tau=27}^{55} \delta_{\tau g} 1[Age = \tau, Group = g] + \psi_o + \hat{p}(X_s, \psi_o) \times \psi_o + u_{it}$, where g stands for an age group in [0,7], [8-14], or [15-18] at the time of intervention.

5.6 Displacement effect by demographic groups

The displacement effects by demographic group may vary (Figure A.3), but we do not find systematic differences across other demographic characteristics. We find more negative displacement effects on earnings and schooling for women and firstborn children. However, we cannot reject differences between groups.

6 Negative Displacement Effect: Potential Mechanisms

We find that on average, displacement negatively affects the labor earnings of individuals in our sample. Based on families' impressions after relocation, in Table A.2 we compare the characteristics of locations between children who were displaced and those who were not (columns (6) and (7)). The table shows that displaced children are relocated to locations that have lower education, more unemployment, and fewer schools; are farther away from the CBD, and have lower property prices in surrounding areas. Overall, these results indicate that the average displaced child in our sample ends up in a worse-quality neighborhood. Based on these results, in the following paragraphs we discuss the 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 a loss of social networks. It is expected to be non-positive, as has been shown by Chetty et al. (2016). Moving may impact children because adapting to new environments is costly due to changes in schools or social environments.

A place or neighborhood effect is associated with the attributes of the assigned location. Displaced families received a bundle of treatments, becoming homeowners of new housing units in isolated, lower-quality areas with low access to transportation, and their neighbors changed as a result of mixing individuals in the new locations (one housing project could receive families from multiple slums). The effect of homeownership is not present in our estimates because the comparison group also received a new housing unit. However, the value of the houses might differ depending on the location of the new units. Thus, property prices or asset values could also explain our results.

Isolation and lack of services are geographical characteristics of neighborhoods.[44] Based on the theory of spatial mismatch (Kain, 1968) and the short-term evidence of Aldunate et al. (1987), we expect the that the lack of employment and lower access to transportation will impact displaced children either directly or through their parents. For example, heads of households reported losing their jobs after being displaced and having a hard time finding employment in the destination location. This would imply a decrease in earnings within the household after relocation, consistent with previous work by Takeuchi et al. (2007), where the benefits of slum relocation depend on how easy it is for adults to change jobs.[45]

In addition, destination municipalities had less public infrastructure than the original slum locations, such as fewer schools and less access to public transportation. This can also differentially impact the value of homes, depending on the new locations. As Molina (1986) explains, on average, destination municipalities had fewer resources and did not invest in new public infrastructure upon new families' arrivals. For example, these municipalities did not start investing in transportation to a substantive degree until the 2000s, and thus displaced families remained isolated for years after the in-

[44] Galster (2012) classifies neighborhood characteristics into four categories: social-interactive, environmental, geographical, and institutional.

[45] Note that this is after considering that families became homeowners. Housing stability can positively impact children and adults who move out of slums or who receive upgraded housing (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 those who were not displaced.

tervention. This might have been reinforced by all families in the program becoming homeowners, which could lead to reduced mobility (DiPasquale and Glaeser, 1999).[46]

Displaced families experienced a change in their neighbors for two reasons. First, people who already lived in the destination locations had, on average, lower schooling than the population in the original location (Table A.2, column (7)). Second, some families from the same slum were split into different neighborhoods and mixed with other displaced families, in large housing projects (relative to their original slum). These new projects mixed poor individuals with even poorer individuals and featured small housing units.[47] Such a concentration of poverty 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 and fractionalized projects increase social conflict between neighbors because families are unfamiliar with each other. Thus, if families had preferences for neighborhood composition, as Takeuchi et al. (2007) suggest, being sent to large public housing projects, and losing their original networks, could negatively affect children's outcomes.

7 Mechanisms

In this section we investigate the mechanisms behind our baseline results on earnings. We start by studying the impact of changes in neighborhood environments on children's future earnings. We then explore the difference between disruption and neighborhood effects by studying children of different ages at baseline. Finally, we examine where children in our sample are presently located.

7.1 Movements between municipalities and destination locations

The displacement effect in our sample is mainly driven by moves across municipalities. Around two-thirds of the displaced families received housing in a municipality different from their origin. In Table 6 we stratify displacement in relocations within or between

[46] This contrasts to the case of most US cities, where 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 the liberalization of land use regulations during the Pinochet dictatorship.

[47] Families reported that their new apartments were smaller than expected. Some of these testimonies can be found in contemporary newspapers (Morales and Rojas, 1986).

Table 6: Displacement effect between and within municipalities

Outcome	Labor earnings (1)	Taxable wages (2)	Formal earnings (3)	Informal earnings (4)	Years of schooling (5)
Displaced within munic. (β_1)	-15.178**	1.149	-9.618	-5.560	-0.279**
	(7.144)	((15.042)	(7.944)	(4.224)	(0.125)
Displaced between munic. (β_2)	-21.825***	-35.356***	-17.400***	-4.425	-0.678***
	(6.490)	(13.538)	(6.303)	(3.452)	(0.105)
Non-displaced mean					
Observations	19,953	19,953	19,953	19,953	19,953
Municipality of origin FE	✓	✓	✓	✓	✓
Baseline controls	✓	✓	✓	✓	✓
$H_0 : \beta_1 = \beta_2$	0.108	0.000	0.072	0.638	0.000

Notes: Propensity score estimates equivalent to column (5) in Table 4 in the sample of children aged 0 to 18 at baseline that are matched to the RSH, and report non-missing schooling. Bootstrapped standard errors with 200 replications in parenthesis. 10%*, 5%**, 1%***. Baseline controls include the following: year of intervention fixed effects, female, mother head of household, married head of household, head of household's age, number of children, firstborn dummy, father Mapuche, and year of birth fixed effects. Last row reports the p-value of the null hypothesis of equality of coefficients.

municipalities. Our findings reveal that the displacement effect between municipalities is more than twice as negative on both total earnings and schooling, with even larger effects on formal earnings. This suggests that changes in children's environments play a role in explaining our main results.

Given that destination municipalities were poorer on average, we study which characteristics of the new locations or projects are the most relevant to explain the variation on children's future labor earnings. We start by stratifying our sample by municipality of origin and estimate a displacement effect for each municipality. Here, each coefficient should be understood as the displacement effect of leaving municipality o relative to staying. Figure A.7 presents the distribution of the estimates on labor earnings, showing great variation by municipality. Some children did better, but most did worse in terms of earnings, with large variations in the degrees of these effects.

To determine which location characteristics explain these patterns, we correlate the estimates by municipalities of origin with the contemporaneous (at baseline) average changes in location attributes by origin. That is, we collapse the location attributes at destination by municipality of origin. The validity of this exercise relies on the idea that displaced families were forced to move to a particular location. Qualitative evidence from social workers who worked with the families in the eviction processes causes us to believe that the assignment was as good as random. This is because according to them, the MINVU assigned families to locations based on unit availability.[48]

[48]We test whether families' demographics predict the attributes at destination. We run regressions

Figure 4 presents the results of this exercise. Panel (a) shows a positive correlation between the displacement effect and home value. However, the variation in home value is very low, and thus it does not explain the variation in children's future earnings. Examining other changes in children's environments, we find expected positive correlations between earnings and the share of individuals from their original slum community as a proxy for networks, the number of schools in the new areas, and log of property prices in surrounding areas at destination. We also find a negative correlation between children's future earnings and distance from origin—a pattern established in previous work (Barnhardt et al., 2016; Picarelli, 2019)—and it is consistent with the results of Table 6. Finally, we find a positive correlation between earnings and neighborhood size, mainly driven by housing projects with over 500 units. This last result challenges the theory of overcrowded neighborhoods having a negative impact due to having worse infrastructure and higher density (Newman, 1973). However, for projects with fewer than 500 units, the correlation is negative. This may indicate non-monotonic effects of size or omitted variable bias, as size might correlate with other neighborhood attributes such as property prices or distance to urban centers.[49]

In general, the correlations we study go in the expected direction and show that the displacement effect on children's future earnings is a function of the different changes experienced by families.[50] To explore which of the changes are the most relevant, in Table 7 we investigate how the displacement estimate decreases when location changes are included. Column (1) shows our baseline result on labor earnings, and column (2) adds neighborhood changes (schools, distance to CBD, and property prices). The estimates show the expected signs, with more schools and higher prices positively correlating with earnings, and longer distances to the CBD (as a proxy for labor market access) negatively correlating with earnings. Additionally, including these neighborhood changes reduces the displacement effect from –14% to –7.8%. In column (3) we add home value. Interestingly, conditional on home value, the displacement effect is slightly more negative than in column (2), but it does not change the estimates of neighborhood change

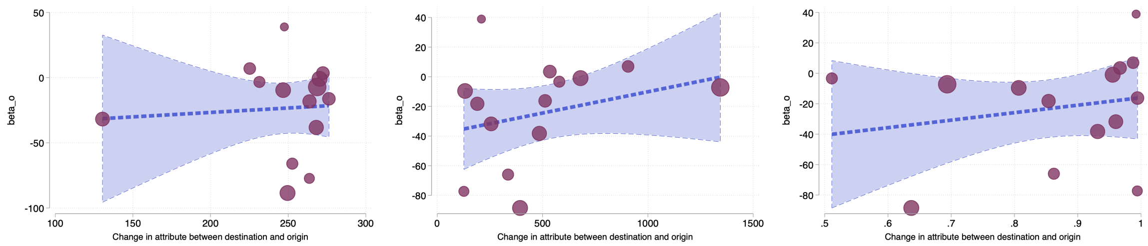
of several location attributes on a set of family demographics (Table A.7). In our analysis of the education dummies, we do not reject the null of joint significance in 12 out 15 outcomes. Similarly, when testing all of household characteristics, we do not reject the null in 11 out of these cases.

[49] Larger social housing projects were built in cheaper areas and farther from the city center.

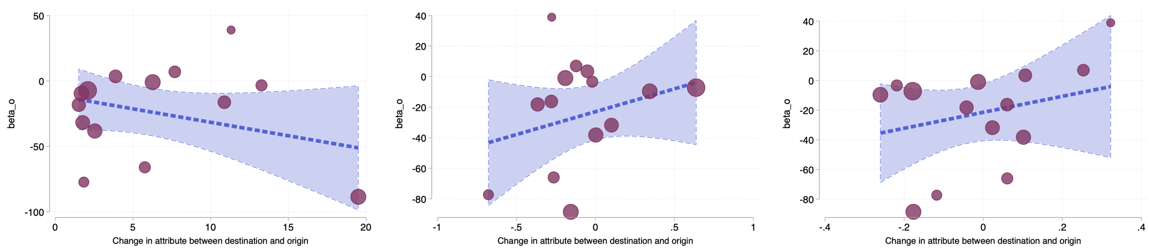
[50] Appendix Figure A.6 shows more correlations with other changes in attributes. We do not plot them in the main figure as the correlations are closer to zero or proxy for similar variables.

Figure 4: Relationship between displacement effect and changes in location attributes

(a) Home value (UF index) **(b)** Neighborhood size (units/project) **(c)** Share of original slum community



(d) Distance from origin (km) **(e)** Δ number of schools per student **(f)** Δ log(property prices)



Notes: The figures plot displacement coefficients on self-reported labor income stratified by municipality of origin (Figure A.7), against average changes in location attributes by municipality of origin. Regressions for children who were 0 to 18 years old at baseline that are matched to the RSH data that report non-missing schooling, from municipalities with displaced and non-displaced populations. The number of municipalities of origin is 14. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, father Mapuche, firstborn dummy, year of birth fixed effects, and year of intervention fixed effects. Correlations are weighted by the number of observations in each cell (number sample children in municipality of origin). Other correlations can be found in Figure A.6.

coefficients. In fact, the coefficient on schools per student becomes more positive.

In columns (3) and (4) we repeat the exercise, but studying changes related to the displacement itself (project size, network share, and distance from origin).[51] The results indicate that project size and distance from origin negatively correlate with earnings, and these correlations are statistically significant. Moreover, the network share, measured as the fraction of slum families from the original slum in the destination neighborhood, correlates positively with earnings. However, this particular correlation is not statistically significant. By including these determinants, the displacement effect decreases to –5.8%, and the results are robust to including home value (column (4)).

Finally, in columns (5) and (6) we combine all neighborhood changes from previous columns and find that including them reduces the displacement effect to –4.4%. All the estimates except neighborhood size are not different from zero but are stable across specifications. Column (6) adds home value as a control, which positively impacts

[51]Note that project size can be interpreted as a place change because slums contained fewer families than public housing projects.

Table 7: Displacement effect and change in location attributes on earnings

Outcome	Self-reported labor earnings (2007-2019)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Displaced	-21.207***	-11.782	-12.553	-8.819	-12.053	-6.636	-10.752
	(6.659)	(7.622)	(8.066)	(9.254)	(9.143)	(9.191)	(9.548)
Δ# schools/child		2.907*	3.555**			1.963	2.172
		(1.621)	(1.663)			(2.168)	(2.295)
ΔDistance to CBD		-1.136*	-1.103**			-0.703	-0.934
		(0.601)	(0.560)			(0.825)	(0.849)
ΔProperty prices		3.348	3.344			2.134	1.470
		(4.032)	(3.524)			(4.114)	(3.976)
Project size (#units)				-0.009**	-0.013***	-0.008*	-0.011**
				(0.004)	(0.005)	(0.004)	(0.005)
Share network (0-100)				0.085	0.077	0.102	0.084
				(0.103)	(0.106)	(0.135)	(0.124)
Distance from origin (km)				-0.623*	-0.417	-0.378	-0.019
				(0.365)	(0.402)	(0.562)	(0.675)
Home value (UF)			0.054		0.088*		0.098*
			(0.046)		(0.053)		(0.036)
Non-displaced mean				151.293			
Adj. R^2	0.131	0.131	0.131	0.131	0.131	0.131	0.131
Percent effect (%)	**-14.0**	**-7.8**	**-8.3**	**-5.8**	**-8.0**	**-4.4**	**-7.1**
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓
Baseline controls	✓	✓	✓	✓	✓	✓	✓
Slums characteristics	✓	✓	✓	✓	✓	✓	✓
Observations	19,953	19,953	19,953	19,953	19,953	19,953	19,953

Notes: This table shows results for coefficients β and γ from regression $Y_i = \alpha + \beta Displaced_{s\{i\}} + \sum \gamma \Delta Attribute_o + \psi_o + \hat{p}(X_s, \psi_o) \times \psi_o + X_i'\theta + \varepsilon_i$. All changes in attributes (Δ) are measured at the census district level, which corresponds to a smaller level of aggregation than municipalities. This table shows propensity score estimates equivalent to column (5) in Table 4, for children aged 0 to 18 are matched to the RSH data. Bootstrapped standard errors in parentheses. 10%*, 5%**, 1%***. Controls include the following: year of intervention fixed effects, female, mother head of household, married head of household, head of household's age, number of children, firstborn dummy, father Mapuche, and year of birth fixed effects. The row labeled as "Percent effect" stands for "percentage variation with respect to non-displaced mean."

earnings, and compared to column (5), most coefficients remain similar in magnitude. However, distance from origin decreases substantially, while distance to the CBD has a larger (but noisy) impact on earnings. This indicates that conditional on home value, children's future earnings are more responsive to their distance from labor market opportunities and are less responsive by the distance they were relocated (though these two variables have high correlation for children sent to peripheral areas). Finally, compared to column (5), the schools and network share estimates become larger.

7.1.1 Formal and informal earnings

We continue the analysis by studying if different versions of children's future earnings respond similarly to changes in location attributes. Table A.8 reports presents data analogous to column (6) from Table 7 on formal and informal earnings and years of schooling. The results indicate that both taxable wages and formal earnings (columns (1) and (2)) respond in the same way as total earnings to neighborhood changes. However, the correlation between formal earnings and the change in local property prices is negative. This pattern is also found for years of education (column (4)).

This last result is surprising as better economic conditions are expected to positively impact earnings. However, our results indicate that the positive impact of higher prices is more pronounced on informal earnings. This could imply that the children in our sample tend to have lower informal earnings in areas far from job opportunities (distance to the CBD) yet potentially higher earnings in areas with high property prices, which are also far from their families' original location. It is important to note, though, that this latter correlation is not statistically significant. The negative correlation between years of education and higher property prices might suggest that children are more likely to drop out of school in areas with better economic prospects but more remote. As a result, informality is higher due to the higher opportunity cost of schooling. This pattern has been found in other settings, for example, Atkin (2016) shows how in Mexico, children's schooling is reduced due to a positive trade shock, and the negative relationship between schooling and informality has also been previously established (Lopez Garcia, 2015). To better understand which children are more likely to respond to these changes, we study heterogeneity by age at displacement in the next section.

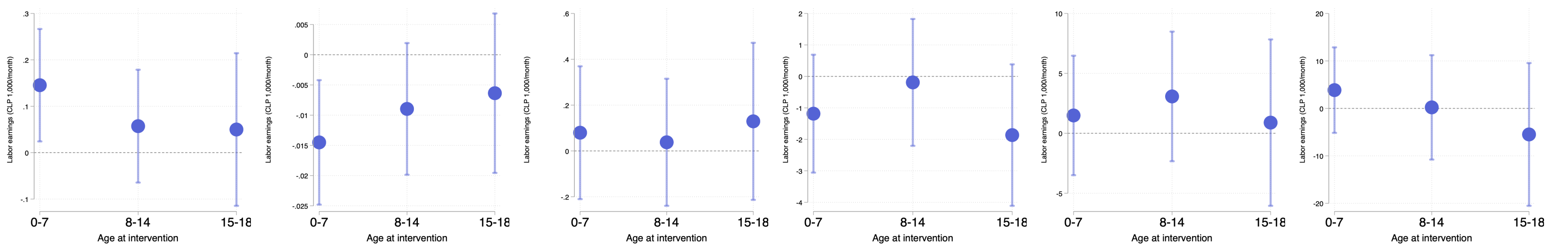
7.2 Disruption versus neighborhood effects

In this section we provide suggestive evidence of the two components of the displacement effect affecting children: a disruption effect and a neighborhood effect. While we do not have separate variation for the two components, we provide suggestive evidence on this question by examining the determinants of the displacement by age at baseline. The rationale for this exercise is the evidence we find in Section 5.3.1, of an exposure effect with a potential non-negligible disruption effect for teenagers. Given this, we would expect younger children's future earnings to respond more negatively to changes in neighborhood attributes and teenagers' future earnings to be more responsive to disruptive changes in social networks, for example. To provide evidence for these hypotheses, we run regression (6) in Table 7, stratified by age group at baseline (0–7, 8–14, 15–18). Figure 5 reports the coefficients for each location control and age group.

Figure 5: Mechanisms of displacement effect by age at intervention

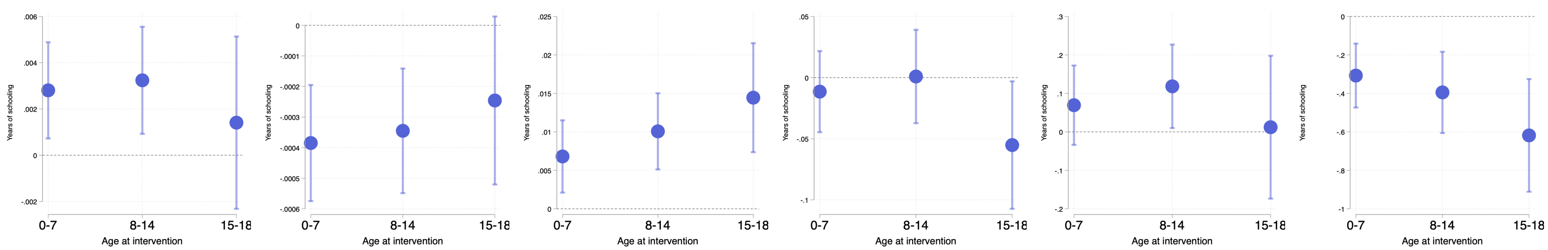
A. Labor earnings

(a) Home value **(b)** Neighborhood size **(c)** % original slum **(d)** Δ distance to CBD **(e)** Δ # schools/student **(f)** Δ log prices



B. Years of schooling

(g) Home value **(h)** Neighborhood size **(i)** % original slum **(j)** Δ distance to CBD **(k)** Δ # schools/student **(l)** Δ log prices



Notes: The figures plot equivalent coefficients from column (7) in Table 7 and their 95% confidence intervals for self-reported labor earnings in panel A and years of education in panel B stratified by age groups at baseline ([0,7], [8-14], and [15-18]). Regressions for children who were 0 to 18 years old at baseline that are matched to the RSH data that report non-missing schooling, from municipalities with displaced and non-displaced populations. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, father Mapuche, firstborn dummy, year of birth fixed effects, and year of intervention fixed effects.

Panel A replicates column (6) in Table 7 by age on total labor earnings. The results are noisy given the large number of coefficients we estimate, but they suggest an age gradient on several displacement determinants. On the one hand, young children, especially those under 8 years old, benefit more from a higher home value and from having more schools in their destination neighborhoods. However, they are more affected by larger neighborhoods. On the other hand, those who were displaced as teenagers have a larger benefit of moving with their original slum network (panel (c)), but their earnings are lower the farther they are from the city center. Finally, the conditional effect of moving to a pricier area shows that young children benefit and teenagers do not.

In Panel B we do the same exercise on years of education, and the age gradient is even clearer than on children's future labor earnings. Place characteristics are more likely to show effects for children under 15 years old at baseline, but moving with their original network is a protective factor against a negative displacement effect, especially for teenagers. In addition, as shown in Table A.8, property prices negatively correlate with years of education, and we find the same pattern for age, with the effects being more negative for teenagers. This suggests that teenagers are the most likely to drop out from school. Appendix Figure A.8 examines informality and high school completion, showing that higher property prices increase informal earnings for all children but have a greater impact on teenagers' likelihood of graduating from high school compared to younger children.

Overall, our results suggest that the negative displacement effect on the adult labor earnings of young children is likely attributable to neighborhood attributes. In addition, for teenagers, the negative effect seems to stem more from the disruption of moving. However, higher economic opportunities, measured as higher property prices, negatively affect teenagers by reducing schooling and increasing informality.

7.3 Children's long-run locations

Our previous analysis shows that children's future labor earnings are affected through changes in their environments when they relocate. The next step is investigating where children live today. We start by examining the likelihood of parents in our sample to remaining in their assigned neighborhoods. We estimate a displacement effect on current locations between 2016 and 2019 as well as on the poverty rate of these neighborhoods.[52]

Table 8, Panel A shows that compared to non-displaced, displaced parents are less likely to live in their assigned municipality (column (1)) or in their assigned neighborhood (column (2)). Even though these effects are not statistically significant, they are sizable (–20%). In addition, they are less likely to live in their municipality of origin (column (3)), and if they move, they live 2.2 kms farther away but are still close (6.7 kms). This indicates that they live in neighboring municipalities. In terms of poverty rates, displaced parent's neighborhoods are poorer, but the difference is very small and not different from zero, perhaps because they are more likely to move.

We continue the analysis by examining the current locations of children, now adults, in Panel B of Table 8. Children are less likely to live in their originally assigned neighborhood compared to their parents. Only 30% of these adult children live in their assigned municipality, and fewer than 23% remain in their assigned neighborhood. The differences are small and not statistically significant; however, displaced children live 2.164 kms farther away from their parents' neighborhood and live in poorer areas. In Panel C we study these patterns by age at baseline and find that children who were under 15 years of age at baseline now live farther away from their assigned neighborhood. Contrary to their parents, they move to higher-poverty areas compared to those who were not displaced (column (5)). This suggests that exposure to worse environments has persistent effects on location characteristics.

A caveat of our analysis is that our data on locations start in 2016, by which time the individuals in our study, have an average age of 42.6 years. Thus, we lack information on when they moved out of their parents' house. To provide suggestive evidence that these individuals stayed in their assigned locations until they at least turned 18, we examine the probability of parents selling their homes. We have information for 20% of families who were assigned to neighborhoods in the northern areas of Santiago. The results, in Table A.9, show that 10% of families in this restricted sample have sold their house by 2019, after 25 years on average. In addition, displaced families are 36% less likely to sell (column (1)), and if they do, they sell at a lower price (–3.5%). These results are suggestive but not different from zero, probably because our sample

[52] The RSH reports location information at the neighborhood level for a random sample of individuals. About 40% of the observations in RSH include a current location at the neighborhood level.

Table 8: Displacement effect on children's and parents' locations between 2016 and 2019

	Probability of living in			Distance	Poverty index
	assigned municipality	assigned neighborhood	municipality of origin	from assigned neighborhood	in current neighborhood
	(1)	(2)	(3)	(4)	(5)
A. Parents in RSH					
Displaced	-0.132	-0.113	-0.236**	2.208**	-0.886
	(0.106)	(0.091)	(0.093)	(0.934)	(0.642)
Non-displaced mean	0.668	0.555	0.688	4.468	55.316
Percent effect	**-19.7**	**-20.3**	**-34.3**	**49.4**	**-1.6**
Observations	3,661	3,661	3,661	3,661	3,661
B. Children in RSH					
Displaced	-0.078	-0.075	-0.164***	2.164**	-2.290***
	(0.081)	(0.056)	(0.070)	(0.852)	(0.688)
Non-displaced mean	0.347	0.231	0.392	8.385	60.875
Percent effect	**-20.9**	**-32.5**	**-41.8**	**25.8**	**-3.8**
C. Children in RSH by age					
Displaced 0-7 (β_1)	-0.113	-0.093	-0.170**	2.653***	-2.629***
	(0.081)	(0.061)	(0.070)	(0.870)	(0.753)
Displaced 8-14 (β_2)	-0.058	-0.062	-0.161**	1.904**	-2.822***
	(0.083)	(0.054)	(0.073)	(0.919)	(0.868)
Displaced 15-18 (β_3)	-0.035	-0.059	-0.152**	1.469	0.321
	(0.084)	(0.057)	(0.064)	(1.098)	(1.127)
Observations	9,975	9,975	9,975	9,975	9,975
Municipality of origin FE	✓	✓	✓	✓	✓
Baseline controls	✓	✓	✓	✓	✓
$H_0 : \beta_1 = \beta_2$	0.025	0.104	0.613	0.036	0.824
$H_0 : \beta_1 = \beta_3$	0.110	0.364	0.452	0.216	0.004
$H_0 : \beta_2 = \beta_3$	0.487	0.918	0.721	0.652	0.010

Notes: This table shows inverse propensity score weighted estimates equivalent to column (4) in Table 4. We cannot estimate equation (5) due to a small sample. Regressions for children aged 0 to 18 at baseline and their parents that are matched to the RSH, and report non-missing schooling. Standard errors clustered by slum of origin in parenthesis. 10%*, 5%**, 1%***. Last three rows report the p-values of the null hypotheses of equality of coefficients in pairs from panel C.

is small.[53]

We next explore the influence of transportation improvements on the displacement effect. Given that some children remain in their assigned neighborhoods, perhaps improvements in public transportation reduce the earnings gap between the displaced and non-displaced. To test this, we examine the impact of new metro lines introduced in Santiago between the years 2010 and 2019.[54] We analyze whether the construction of

[53] The sample consists of a 20% random sample of the families that we found in the archives who received a house in the northern areas of Santiago. We partnered with Santiago's Real Estate Registrar to track a random sample of families' addresses in our archival data.

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

a new station close to families' assigned locations impacts displaced and non-displaced children differently. Appendix Figure A.10, panel (a) shows suggestive evidence of an improvement on displaced children's future earnings when a subway station is constructed within 1.5 km of their parents' assigned neighborhood. This effect is mainly driven by an increase in formal earnings and a decrease of the probability of being a temporary worker (panel (b)). Our results are small and noisy since only 9 out of 49 housing projects in our sample are affected by the subway, and they imply a 12% reduction of the negative displacement effect on earnings. [55]

8 Total Displacement Effect on Children and Discussion

8.1 Total earnings lost due to displacement

We use the age estimates on earnings presented in Figure 2, panel (b) to calculate the present value of the loss of earnings due to displacement. Taking age displacement effects from 25 to 55 years, and using an annual discount rate of 4%, the average displaced child in our sample loses CLP$7 million by the age of 45 (relative to a non-displaced child). This is equivalent to US$10,090, and the amount is practically the same as the cost of the housing unit received by a family through the program (equivalent to US$10,103).[56] In aggregate terms, the total loss for children is equivalent to the construction of 12 subway stations or the maintenance of 300 primary schools per year.[57] We consider this estimate to be a lower bound because it is computed on self-reported earnings and does not account for the direct effect of displacement on schooling and its externalities, such as increased criminal activity.

See the maps in Figure A.9 for the geographic variation.

[55]Because we exploit late improvements to the subway infrastructure, we cannot rule out larger effects of new subway infrastructure before 2007, as the largest improvement in the subway system occurred before 2007, when Line 4 was built. This line connected the south of Santiago with the CBD.

[56]Using taxable wages, the loss is larger and equal to US$17,000 by the age of 45.

[57]We compute the aggregate loss as the individual loss times the number of children in our sample. The cost of building subway stations is available from Metro de Santiago, and the cost of schools can be found here.

8.2 From slums to poor neighborhoods: Trap or stepping stone?

Previous literature about slums argues that slum dwellers are caught in a poverty trap because living in slums puts additional burdens on families. Factors include health outcomes, access to financial and labor markets, or access to services (Marx et al., 2013). In our context, families are relocated from slums to public housing, raising the question of the consequences of moving to a poor neighborhood. While we cannot rule out positive consequences of moving from a slum to formal housing, our results show that displaced children, compared to those who stay in better locations and receive a house, perform worse in terms of education and earnings. Furthermore, their earnings loss is not compensated by the value of the housing asset received by their families. Our results on current locations also show that families are not necessarily escaping a poverty trap, as parents are likely to remain in their assigned neighborhoods, perhaps because they became homeowners. And even though children are more likely to move, displaced children's current neighborhoods have higher poverty than those of non-displaced children, especially those of young children. This sheds light on the long-term consequences of moving children to remote areas that perpetuate a poverty trap, especially as children spend more time in less favorable environments.[58]

9 Conclusions and Policy Alternatives

This paper presents new evidence on the long-term consequences of being displaced and growing up in a low-quality neighborhood. In our setting, families did not choose their final locations, allowing us to disentangle the mechanisms that mediate the displacement effect as a function of place. We find evidence of a negative displacement effect for young children and teenagers. The results suggest that young children are more likely to be affected through negative neighborhood effects or lack of services, while teenagers are affected through disruptions in their social networks and a decline in informal earnings.

Our results also show that forcing families to move negatively affects children be-

[58]This can also have intergenerational effects for other family members. Appendix Section C shows displacement negatively affects children born after treatment and the grandchildren of displaced families, who have lower schooling higher probability of dropping out of school.

cause their new neighborhoods are of low quality.[59] One policy alternative to displacing families to the periphery could be to provide housing on-site (UN-Habitat, 2020). However, this may not be feasible for multiple reasons, such as high urban density that impedes public housing construction, the high price of land, or the impossibility of providing services on-site (running water, electricity, sewage). Under those circumstances, monetary compensation for displacement could be an option (Lall et al., 2006), but assessing compensation amounts may be challenging. Thus, if displacement is the only solution, a more effective policy would be to directly provide families with public services. Furthermore, steps must be taken to minimize the disruption faced by families and children. This can be achieved by preserving social networks to keep communities together, offering support for the challenges associated with transitioning to formal housing, and involving communities in the eviction processes.[60]

Finally, an important aspect of our setting is that families were forced to move to places that ended up being poverty traps, potentially worse than their original slums, with increased segregation and low mobility. In the end, this led to negative consequences for children's economic development. Our paper contributes to understanding the effects of these policies on individuals; however, due to the scope of these programs, future research should consider the general equilibrium effects of slum clearance on neighboring individuals, their communities, and compensation schemes.

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[59] A valid question is whether the program was beneficial for families overall, but answering this requires understanding the impact of slum upgrading on children, which is beyond the scope of our current data.

[60] See research by the World Bank here.

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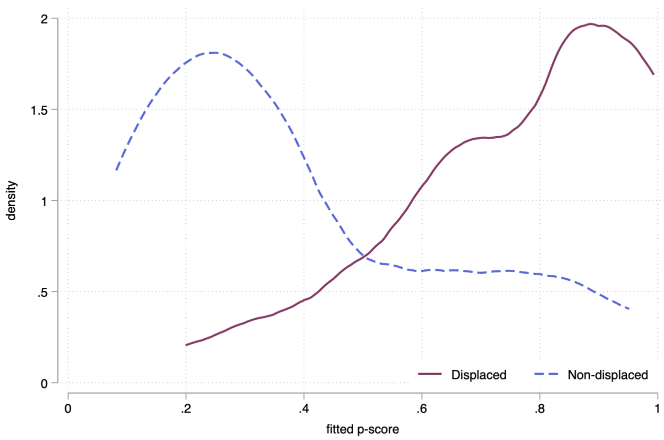
Online Appendix

A Additional Figures and Tables

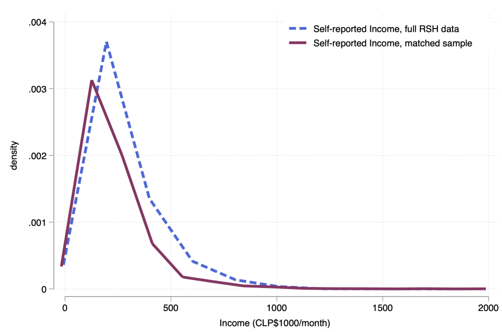
Figure A.1: Distribution of the probability of slum clearance



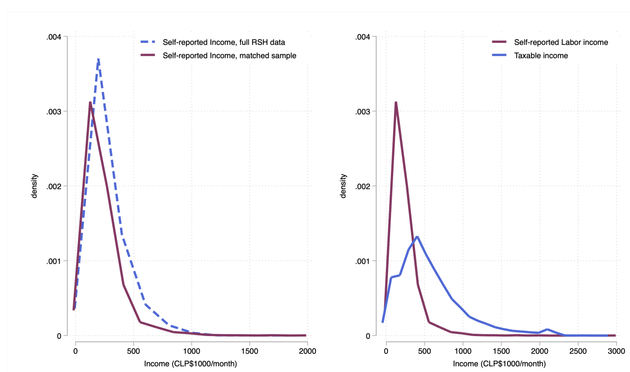
Notes: This figure plots the fitted values of a logit regression that includes controls from column (3) in Table A.1 by treatment.

Figure A.2: Labor income distribution across different samples

(a) Income distribution in the RSH and matched sample

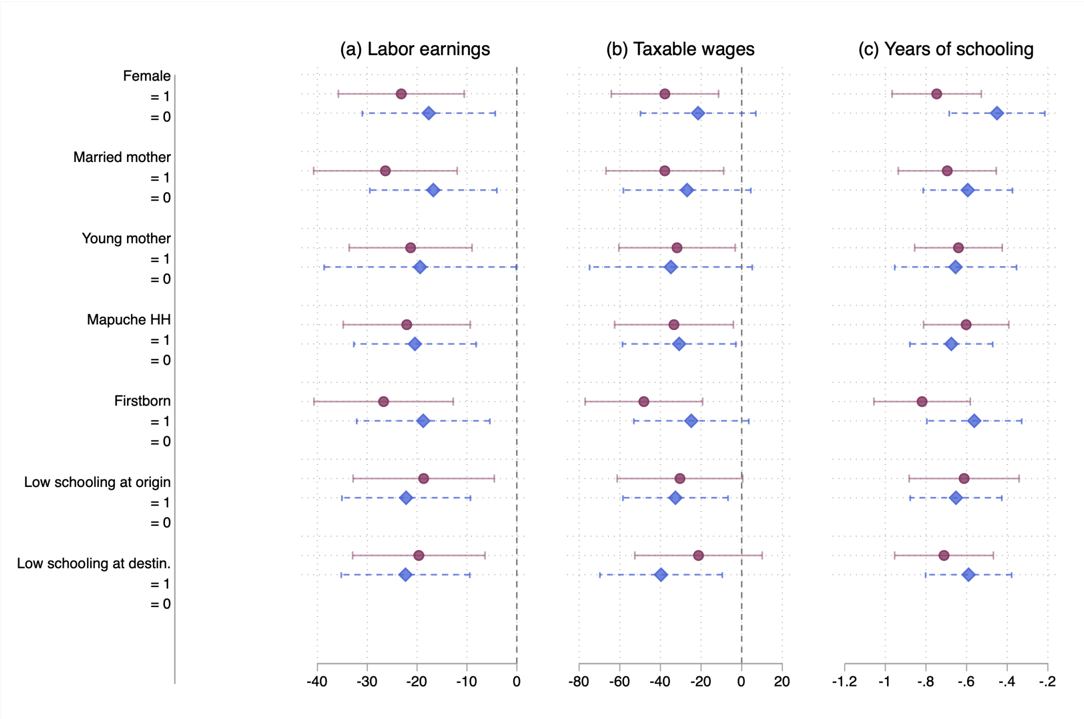


(b) Income distribution in matched sample



Notes: Income data for the year 2018. Matched sample stands for children aged 0 to 18 at baseline who are matched with the RSH data, and who are 21 or older in 2018. "Full RSH" corresponds to all individuals aged 21 to 60 in the RSH in year 2018 in Greater Santiago.

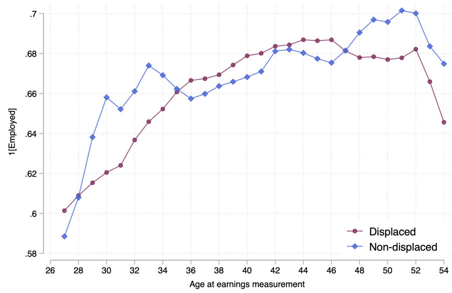
Figure A.3: Displacement effect by demographic groups on earnings and education



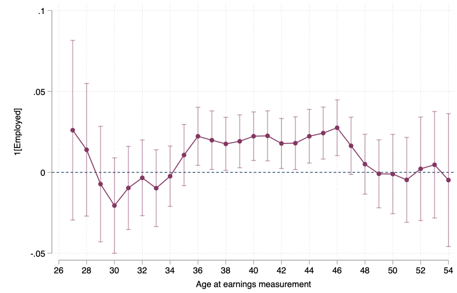
Notes: Propensity score estimates and their 95% confidence intervals equivalent to column (5) in Table 4 stratified by demographic variables in the sample of children aged 0 to 18 that are matched to the RSH, and report non-missing schooling. Standard errors are bootstrapped with 200 replications. Controls include the following: female, mother head of household, single head of household, number of siblings, Mapuche lastname, cohort fixed effects, and time fixed effects. Married 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 stands for municipalities of origin/destination where the population's average schooling is below the sample median.

Figure A.4: Displacement effects on labor market outcomes by age at earnings measurement

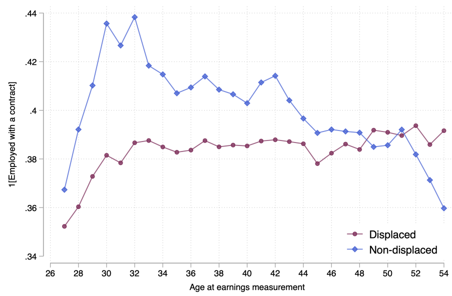
(a) Employment Trajectories



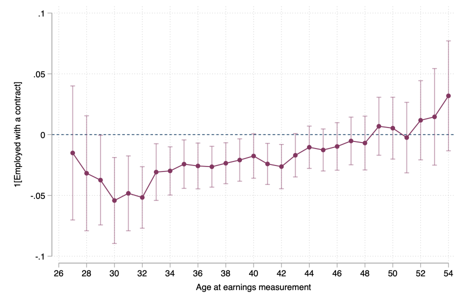
(b) Displacement effect on employment



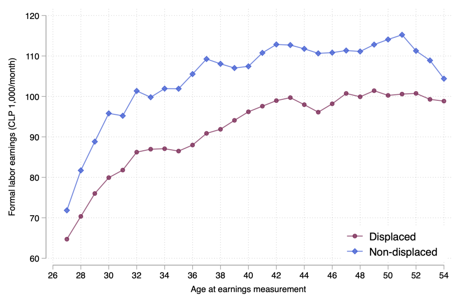
(c) Formal employment trajectories



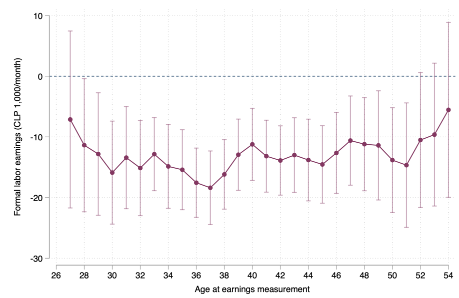
(d) Displacement effect on formal employment



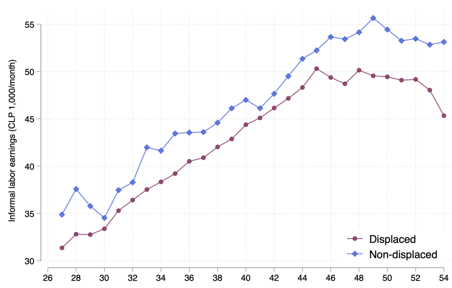
(e) Formal earnings trajectories



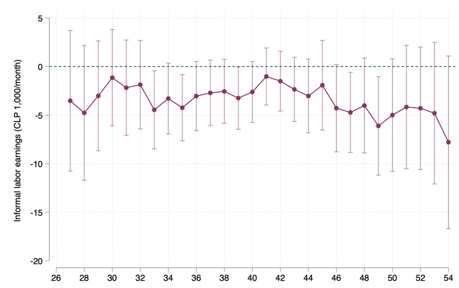
(f) Displacement effect on formal earnings



(g) Informal earnings trajectories



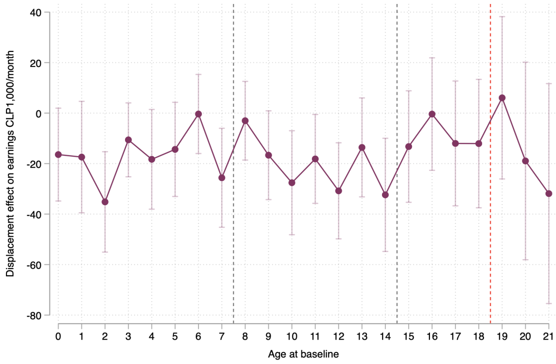
(h) Displacement effect on informal earnings



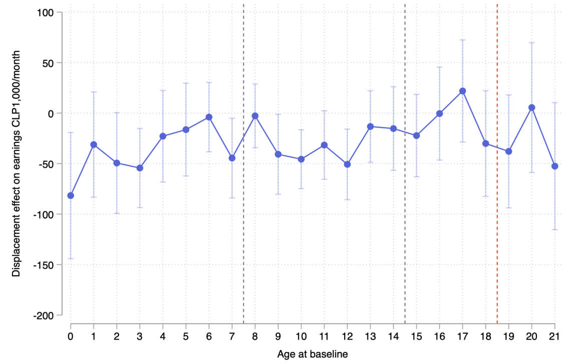
Notes: Regressions for children aged 0 to 18 at baseline that are matched to the RSH data, and report non-missing schooling. Bootstrapped standard errors with 200 replications. Controls include the following: female, mother head of household, married head of household, number of siblings, firstborn dummy, head of household's marital status unknown, and year of birth fixed effects. We estimate $y_{it} = \sum_{\tau=27}^{54} \beta_\tau Displaced * 1[Age = \tau] + \sum_{\tau=26}^{54} \delta_\tau 1[Age] + \psi_o + \hat{p}(X_s, \psi_o) \times \psi_o + X_{it}'\gamma + u_{it}$. Figures (a), (c), (e) and (g) plot the predicted trajectories for the displaced and non-displaced children between ages 27 to 54. Figures (b), (d), (f), and (h) plot coefficients β_τ and their 95% confidence intervals.

Figure A.5: Displacement effect by age at intervention and structural break

(a) All labor earnings (2007-2019)



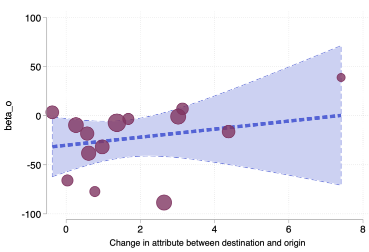
(b) Taxable earnings (2016-2019)



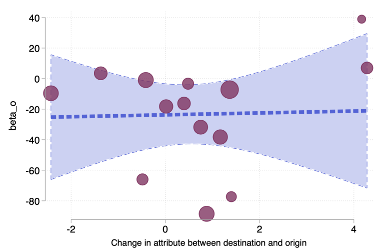
Notes: Regressions for children who are 0 to 21 years old the time of intervention and are matched with the RSH or the GRIS data. Bootstrapped standard errors. Controls include the following: Controls include the following: female, mother head of household, married head of household, number of siblings, firstborn dummy, head of household's marital status unknown, and year of birth 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 black vertical lines indicate that the p-value of the structural break test at the corresponding age is smaller than 0.1. Dotted red vertical lines are a reference for children older than 18 at baseline.

Figure A.6: Relationship between displacement effect and changes in location attributes

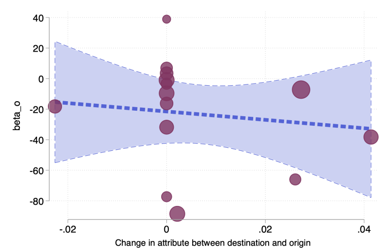
(a) Δ distance to CBD



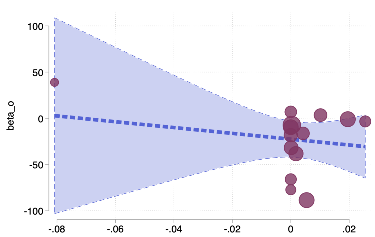
(b) Δ distance to subway



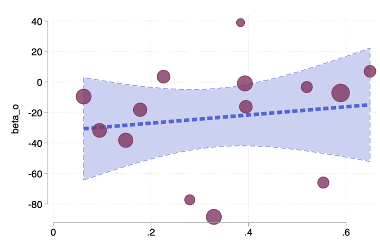
(c) ΔHospitals per family



(d) ΔPrimary care centers per family

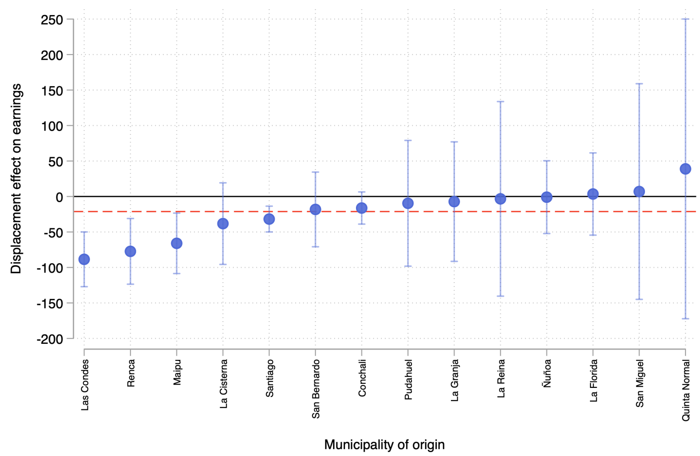


(e) Neighborhood fragmentation



Notes: The figures plot displacement coefficients on self-reported labor income stratified by municipality of origin (Figure A.7), against average changes in location attributes by municipality of origin. Regressions for children who were 0 to 18 years old at baseline that are matched to the RSH data that report non-missing schooling, from municipalities with displaced and non-displaced populations. The number of municipalities of origin is 14. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, father Mapuche, firstborn dummy, year of birth fixed effects, and year of intervention fixed effects. Correlations are weighted by the number of observations (children) in each cell.

Figure A.7: Distribution of displacement effects on labor earnings by municipality of origin

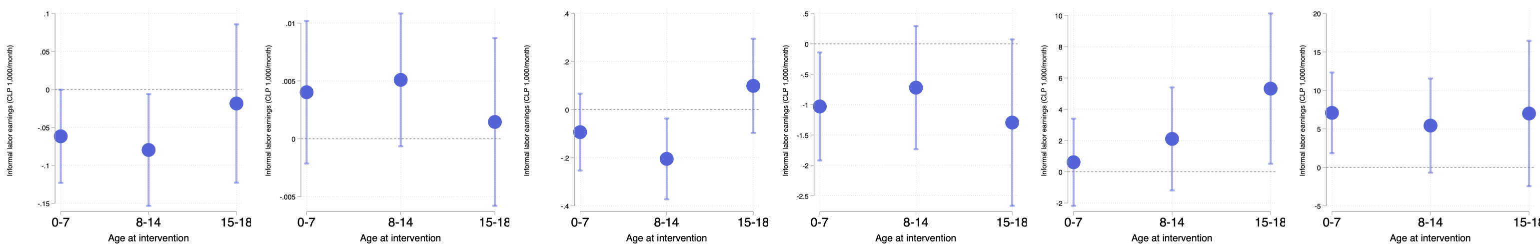


Notes: The figure shows regressions stratified by municipality of origin. The sample includes children who were 0 to 18 years old at the time of the intervention, matched to the RSH, and from municipalities with both displaced and non-displaced populations. The number of municipalities of origin is 14. Coefficients are estimated from a regression stratified by municipality of origin $y_i = \sum_{o=1} \beta_o Displaced_{s\{i\}} * 1[Origin = o] + \hat{p}(X_s, \psi_o) \times \psi_o + X'_{io}\theta + \varepsilon_i$. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, father Mapuche, firstborn dummy, year of birth fixed effects, and year of intervention fixed effects. Red horizontal line is the average displacement effect in full sample of children. Figure reports β_o and its 95% confidence intervals. Bootstrapped standard errors with 200 replications.

Figure A.8: Mechanisms of displacement effect by age at intervention

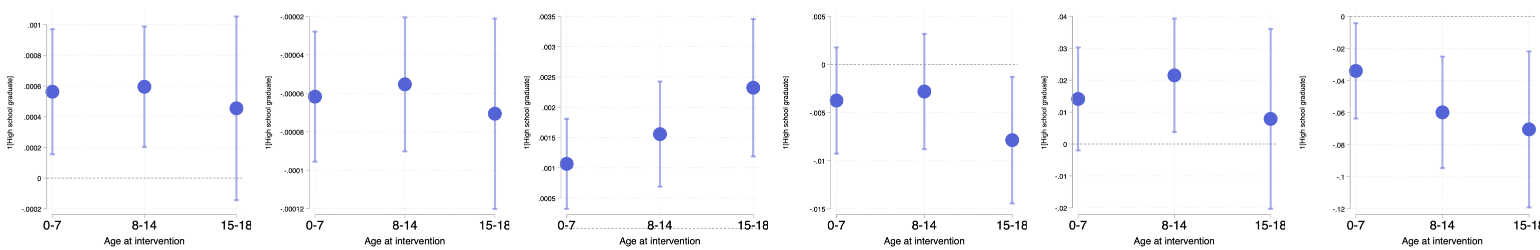
A. Informal labor earnings

(a) Home value **(b)** Neighborhood size **(c)** % original slum **(d)** Δ distance to CBD **(e)** Δ # schools/student **(f)** Δ log prices



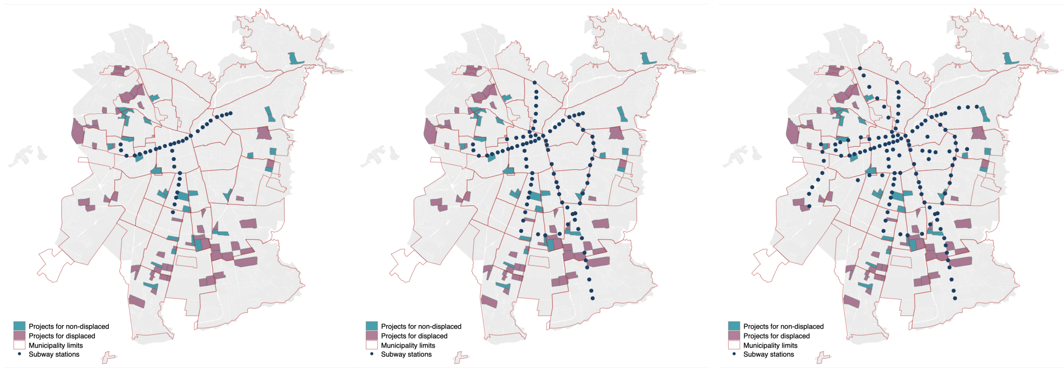
B. High school graduate

(g) Home value **(h)** Neighborhood size **(i)** % original slum **(j)** Δ distance to CBD **(k)** Δ # schools/student **(l)** Δ log prices



Notes: The figures plot the coefficients and their 95% confidence intervals of the correlations between self-reported informal labor earnings and high school completion, and changes in location attributes stratified by age groups at baseline ([0,7], [8-14], and [15-18]). Regressions for children who were 0 to 18 years old at baseline that are matched to the RSH data that report non-missing schooling, from municipalities with displaced and non-displaced populations. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, father Mapuche, firstborn dummy, year of birth fixed effects, and year of intervention fixed effects. See Data Appendix for variable definitions.

Figure A.9: Location of public housing projects and subway stations

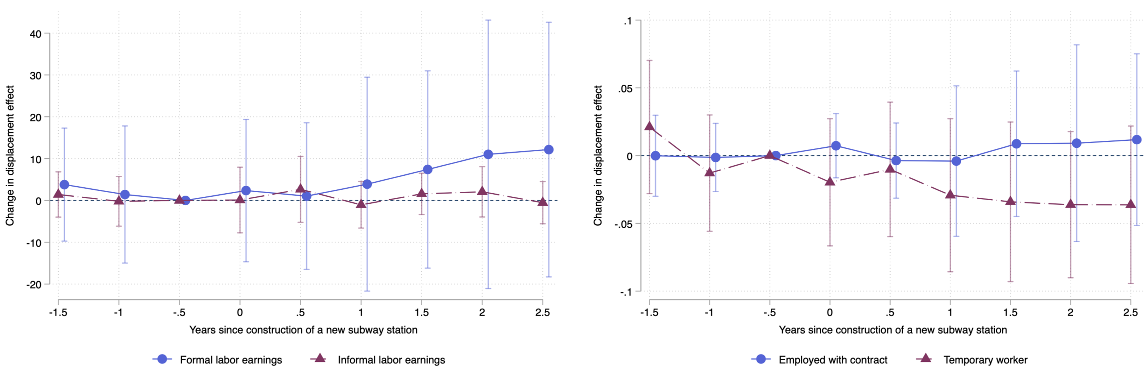


(a) Subway in 1980 **(b)** Subway in 2006 **(c)** Subway in 2019

Notes: This figure shows the rollout of subway stations in Greater Santiago from 1980 to 2019. Red lines represent the urban limits of Greater Santiago and its municipalities in 2019. 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.

Figure A.10: Change in displacement effect as a consequence of access to subway

(a) Labor earnings (CLP$1,000/month) **(b)** Employment by type



Notes: Distance measured in meters. Each coefficient and its 95% confidence interval in figures (a) and (b) correspond to estimates of γ_2 from regression $Y_{it} = \alpha + \beta Displaced_{s\{i\}} + \gamma_1 Subway_{\lambda\{\tau\}} + \gamma_2 Displaced_{s\{i\}} \cdot Subway_{\lambda\{\tau\}} + \psi_o + \hat{p}(X_s, \psi_o) \times \psi_o + X_i'\theta + \delta_t + \varepsilon_{it}$. Table reports A.11 coefficient estimates and their standard errors. Regressions for children who were 0 to 18 years old at baseline that are matched to the RSH data that report non-missing schooling. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, father Mapuche, firstborn dummy, year of birth fixed effects, year of intervention fixed effects, and calendar year fixed effect. Bootstrapped standard errors with 200 replications.

Table A.1: Determinants of the probability of displacement at the slum level

	Probability of slum clearance (displacement)				
	(1)	(2)	(3)	(4)	(5)
Area (in hectares)	0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Families (/100)	-0.011*	0.002	0.004	-0.011	0.007
	(0.006)	(0.011)	(0.013)	(0.007)	(0.012)
Distance to river	-0.045	-0.087	-0.044	-0.019	-0.034
	(0.036)	(0.055)	(0.041)	(0.028)	(0.046)
Military name	-0.204	-0.177	-0.107	-0.110	-0.114
	(0.136)	(0.126)	(0.117)	(0.114)	(0.114)
Log(property prices)	0.069	0.307	0.092	-0.026	0.128
	(0.111)	(0.280)	(0.284)	(0.131)	(0.293)
Treated before 1982			-0.400***	-0.452***	-0.407***
			(0.127)	(0.106)	(0.121)
Population's schooling				0.014	0.027
				(0.036)	(0.046)
Schools/student				-0.019***	-0.022***
				(0.004)	(0.007)
Distance to CBD				-0.004	-0.008
				(0.004)	(0.031)
Adj. R^2	0.030	0.096	0.212	0.191	0.214
Observations	116	116	116	116	116
Municipality of origin FE		✓	✓		✓

Notes: Regressions for the linear probability of slum clearance (versus redevelopment) on slums' characteristics, in the sample of slums found in the National Archives. Robust standard errors in parenthesis. 10%*, 5%**, 1%***.

Table A.2: Location attributes before and after intervention for children in estimation sample

Location attributes by census district	Displaced mean (1)	Non-displaced at origin (2)	Difference (within municip.) (3)	Inv. weight difference (4)	Displaced mean at destination (5)	Difference (within municip.) (6)	Inv. weight difference (7)
Schooling HH	7.664	7.780	-0.448 (0.541)	-0.240 (0.556)	6.525	-1.208(0.326)**	-1.200 (0.336)***
HS dropout students	0.115	0.109	0.009 (0.009)	0.001 (0.011)	0.136	0.033 (0.016)**	0.024 (0.013)*
Schools per census district	2.846	5.210	-1.122 (0.738)	-0.519 (1.110)	2.443	-2.654 (0.695)***	-1.078 (0.975)
Family care centers per 1,000 HH	0.001	0.002	-0.002 (0.002)	0.000 (0.001)	0.006	0.007 (0.005)	0.008 (0.004)*
Hospitals per 1,000 HH	0.002	0.000	0.004 (0.003)	0.005 (0.004)	0.021	0.025 (0.018)	0.021 (0.013)
Distance to closest school (km)	0.531	0.400	0.032 (0.593)	0.081 (0.112)	0.512	0.082 (0.054)	0.100 (0.064)
Distance to closest subway (km)	4.890	5.082	0.492 (0.545)	0.866 (1.450)	5.940	1.141 (0.921)	1.806 (1.439)
Distance to Downtown	9.990	10.721	-0.479 (0.583)	0.820 (1.732)	12.789	2.752 (0.950)***	3.643 (1.725)**
Property prices[a]	14.707	14.714	-0.014 (0.059)	-0.052 (0.085)	14.669	-0.023 (0.081)	-0.054 (0.071)
Neighborhood fragmentation[b]	0.000	0.000	-	-	0.513	0.455 (0.085)***	0.519 (0.052)***
Observations (Children)	19,319	4,958		24,277			24,277
No. slums	47	17		64			64
No. new projects	33	17		48			48

Notes: Within difference corresponds to a regression of each location attribute on a displacement dummy conditional on municipality of origin. Inv. weight difference is the difference after weighting for the inverse probability of being treated. Clustered standard errors in parenthesis. 10%*, 5%**, 1%***. All location attributes correspond to population averages by census district level in 1982. (a) Measured as the average of log(property prices) in a buffer of 2 kms. (b) Fragmentation measured as an HHI that uses as shares the fraction of families from slum of origin in the total number of families in the new neighborhood.

Table A.3: Summary statistics for the full sample of families

Variables	Full sample				Families with children			
	Displaced mean (1)	Non-displaced mean (2)	Difference (within municip.) (3)	Inverse weight diff. (4)	Displaced mean (5)	Non-displaced mean (6)	Difference (within municip.) (7)	Inverse weight diff. (8)
A. Demographics at baseline								
Head of household age	34.900	35.294	-0.474	-1.144	35.163	35.652	-0.574	-1.320**
			(0.620)	(0.708)			(0.557)	(0.652)
Female HH	0.360	0.333	0.017	-0.054	0.357	0.327	0.019	-0.047
			(0.029)	(0.077)			(0.030)	(0.073)
Married HH	0.747	0.811	-0.058***	-0.045*	0.763	0.833	-0.063***	-0.053**
			(0.016)	(0.026)			(0.016)	(0.023)
Couple	0.867	0.908	-0.035***	-0.019	0.867	0.910	-0.036***	-0.015
			(0.013)	(0.021)			(0.013)	(0.024)
Widowed HH	0.014	0.013	0.001	0.005	0.015	0.014	0.001	0.005
			(0.003)	(0.005)			(0.003)	(0.005)
Marital status unknown	0.133	0.092	0.035***	0.019	0.133	0.090	0.036***	0.015
			(0.013)	(0.021)			(0.013)	(0.024)
Father mapuche	0.051	0.044	0.009*	-0.001	0.051	0.044	0.009*	-0.003
			(0.005)	(0.008)			(0.005)	(0.010)
# children	2.578	2.491	0.083	-0.014	2.686	2.614	0.067	-0.078
			(0.083)	(0.09)			(0.073)	(0.081)
No. children	0.040	0.047	-0.008	-0.022	-	-	-	-
			(0.012)	(0.018)			-	-
Age of youngest child	6.505	6.674	-0.309	-0.573*	6.505	6.674	-0.309	-0.573*
			(0.286)	(0.342)			(0.286)	(0.343)
Age of oldest child	11.456	6.064	-0.204	-1.092*	11.456	11.500	-0.204	-1.092*
			(0.451)	(0.582)			(0.451)	(0.582)
B. Demographics measured between 2007 and 2019								
Female's schooling < 6	0.333	0.331	0.006	0.034	0.337	0.343	0.000	0.024
			(0.024)	(0.050)			(0.024)	(0.048)
Female's schooling > 6	0.246	0.268	-0.022	0.004	0.246	0.266	-0.020	0.008
			(0.017)	(0.022)			(0.018)	(0.023)
Female's schooling unknown	0.424	0.402	0.016	-0.038	0.417	0.391	0.020	-0.033
			(0.028)	(0.066)			(0.029)	(0.065)
Male's schooling < 6	0.063	0.076	0.015	0.002	0.085	0.074	0.013	0.004
			(0.011)	(0.018)			(0.011)	(0.017)
Male's schooling > 6	0.058	0.067	-0.017*	-0.031*	0.060	0.071	-0.015	-0.026*
			(0.009)	(0.017)			(0.01)	(0.015)
Male's schooling unknown	0.852	0.851	0.002	0.029	0.855	0.855	0.002	0.022
			(0.017)	(0.045)			(0.018)	(0.031)
Female in the RSH	0.531	0.562	-0.021	0.037	0.536	0.566	-0.020	0.034
			(0.025)	(0.066)			(0.026)	(0.064)
Male in the RSH	0.149	0.151	-0.003	-0.032	0.145	0.145	-0.002	-0.022
			(0.017)	(0.037)			(0.018)	(0.031)
Observations	8,435	2,225	10,660	10,660	8,097	2,120	10,217	10,217

Notes: Columns (3) and (7) report the within difference that corresponds to the coefficient *Displaced* in equation (1) conditional on municipality of origin fixed effects; and Columns (4) and (8) report the difference between displaced and non-displaced after weighting for the inverse probability of each family's slum being cleared. Standard errors clustered by slum of origin. 10%*, 5%**, 1%***. Families with children are all families with at least one child at the time of the intervention regardless of the age the child.

Table A.4: Comparing displaced and non-displaced children aged 0 to 18 in slums from municipality of Santiago

	Father's occupation non-missing		HH's NID non-missing	
	Non-displaced mean	Difference (within municip.)	Non-displaced mean	Difference (within municip.)
	(1)	(2)	(3)	(4)
A. Demographics children from Santiago				
Female	0.486	0.028	0.494	0.003
		(0.037)		(0.036)
Age	7.903	-0.015	7.706	-0.275
		(0.393)		(0.335)
Firstborn	0.303	-0.003	0.273	0.053
		(0.040)		(0.030)
Oldest sibling	12.297	-0.018	12.539	1.126*
		(0.597)		(0.538)
Youngest sibling	3.989	0.033	3.796	-0.138
		(0.363)		(0.262)
No. children	4.977	0.024	5.278	-0.470
		(0.185)		(0.164)
HH age	36.719	-3.516***	35.176	-1.987***
		(0.699)		(0.479)
Female HH	0.229	0.288***	0.592	-0.031
		(0.033)		(0.035)
Married HH	0.756	0.034	0.773	-0.077**
		(0.037)		(0.032)
Father Mapuche	0.058	0.032	0.059	0.025
		(0.024)		(0.019)
Mother's schooling 0-6[a]	0.503	-0.046	0.510	-0.020
		(0.042)		(0.033)
Mother's schooling > 6	0.234	0.036	0.322	-0.017
		(0.034)		(0.035)
Mother's schooling unknown	0.263	0.010	0.167	0.037
		(0.034)		(0.026)
Father employed	0.737	0.074**	0.691	0.116**
		(0.037)		(0.044)
Father dependent worker	0.480	0.140***	0.468	0.136**
		(0.037)		(0.046)
Father independent worker	0.257	-0.067*	0.223	-0.020
		(0.034)		(0.039)
Father retired	0.034	-0.016	0.029	-0.020
		(0.014)		(0.014)
Individuals	1,697		1,346	
Families	603		572	
B. Predicted employment in full sample				
Father dependent worker	0.680	0.000	0.633	-0.006
		(0.003)		(0.004)
Father independent worker	0.221	0.001	0.225	0.005
		(0.003)		(0.003)
Children	24,277		24,106	
Families	9,810		9,738	

Notes: Within difference corresponds to the coefficient *Displaced* in equation (1) in the sample of slums from Municipality of Santiago. Mother's years of schooling is observed in the sample of mothers found in the RSH conditional on being alive by 2007. Bootstrap standard errors in parenthesis. 10%*, 5%**, 1%***. (a) Mother's years of schooling is observed in the sample of mothers found in the RSH and conditional on a mother being alive after year 2007.

Table A.5: Displacement effect on labor income and employment in sample with common support for block p-score

Model	OLS (1)	OLS (2)	OLS (3)	P-score (4)	Block p-score (5)
Panel A.	Outcome: 1[Employed]				
Displaced	-0.021	-0.021*	-0.019	-0.002	-0.005
	(0.014)	(0.012)	(0.014)	(0.017)	(0.013)
Adjusted R^2	-0.005	0.115	0.115	0.116	0.116
Panel B.	Outcome: Self-reported earnings (CLP$1,000/month)				
Displaced	-21.705***	-21.426***	-24.069***	-24.044***	-16.351***
	(6.324)	(5.415)	(4.830)	(6.735)	(5.481)
Adjusted R^2	0.010	0.133	0.133	0.134	0.134
Panel C.	Outcome: Taxable wages from social security (CLP$1,000/month)				
Displaced	-42.279***	-41.267***	-44.872***	-38.871***	-46.358***
	(11.001)	(8.974)	(8.570)	(15.055)	(9.596)
Adjusted R^2	0.015	0.064	0.063	0.064	0.064
Individuals	13,728	13,728	13,728	13,728	13,728
Municipality of origin FE	✓	✓	✓	✓	✓
Baseline controls		✓	✓	✓	✓
Slum characteristics			✓	✓	✓

Notes: Regressions for children aged 0 to 18 at baseline that are matched to the RSH and GRIS data, and that report non-missing schooling, in sample with common support in all municipalities of origin (column (6) in Table 4). Clustered standard errors in parenthesis in columns (1) to (4), bootstrapped standard errors in columns (5) and (6). 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, firstborn dummy, father Mapuche, and year of birth fixed effects. Slums' characteristics include area in hectares, number of families, military name, distance to river in km, and log of property prices at origin. Location attributes at origin include the population's schooling, number of schools, and distance to subway. Propensity score matching results include a full set of municipality of origin fixed effects interacted with the propensity score percentile dummies. The different set of controls specified in the corresponding column are used in the propensity score estimation. The row labeled as "Percent effect" stands for "percentage variation with respect to non-displaced mean."

Table A.6: Displacement effect on employment and education outcomes

Outcome	Displacement effect	Mean non-displaced	Percent effect (%)
Panel A. Demographic outcomes			
Ever married	-0.003	0.697	-0.4
	(0.016)		
Age at first marriage	-0.127	24.608	-0.5
	(0.281)		
Teen parent	0.038**	0.346	11.0
	(0.016)		
# Children	0.065	4.8	1.4
	(0.050)		
On welfare (2015-2019)	-0.023*	0.449	-5.1
	(0.012)		
$Welfare (2015-2019)	-1.226	91.827	-1.3
	(14.856)		
Convicted (2000-2010)	0.003	0.014	58.8
	(0.010)		
Panel B. Types of occupations/industries			
Employer	-0.003*	0.007	-42.9
	(0.002)		
Self-employed	0.016	0.216	7.4
	(0.018)		
Caregiver	-0.004	0.068	-5.9
	(0.006)		
Manufacturing	0.010	0.034	29.4
	(0.006)		
Construction	0.030***	0.049	61.2
	(0.005)		
Services	-0.009	0.116	-7.8
	(0.007)		
Panel C. Household characteristics			
Homeowner	-0.026*	0.545	-4.8
	(0.013)		
Renter	-0.002	0.090	-2.2
	(0.011)		
Transferred property	0.019	0.360	5.3
	(0.017)		
Squatter	0.004**	0.008	50.0
	(0.002)		
Household size	-0.028	3.834	-0.7
	(0.074)		
Parent in household	-0.023	0.201	-11.4
	(0.017)		

Notes: This table shows propensity score estimates equivalent to column (5) in Table 4, for children aged 0 to 18 at baseline that are matched to the RSH and GRIS data that report non-missing schooling. Bootstrapped standard errors in parenthesis, 10%*, 5%**, 1%***.

Table A.7: Assignment location attributes and displaced families' characteristics at baseline

Attributes at destination	Home value (UF) (1)	Distance from origin (2)	Adult pop. schooling (3)	# schools/ 1,000 students (4)	Log property prices (5)	Distance to CBD (6)	Primary care centers (7)	Unemployment rate (8)
Female HH	-1.544	0.030	-0.002	0.066	0.023	0.130	0.003	0.008
	(1.591)	(0.108)	(0.001)	(0.050)	(0.016)	(0.105)	(0.002)	(0.006)
# Children	0.180	-0.003	-0.000	-0.009	0.006	-0.018	-0.000	0.002
	(0.225)	(0.022)	(0.000)	(0.007)	(0.005)	(0.022)	(0.000)	(0.002)
Married HH	0.855*	-0.104*	0.001	0.010	-0.007	-0.107***	-0.001	-0.002
	(0.452)	(0.055)	(0.001)	(0.021)	(0.009)	(0.038)	(0.001)	(0.004)
HH age	0.128	-0.015**	0.000	-0.002	-0.002	-0.015*	-0.000	-0.001
	(0.120)	(0.007)	(0.000)	(0.005)	(0.001)	(0.008)	(0.000)	(0.001)
Mapuche HH	1.831	-0.131	0.000	-0.048	-0.022	-0.121	-0.003	-0.004
	(1.452)	(0.087)	(0.001)	(0.051)	(0.014)	(0.101)	(0.002)	(0.004)
HH schooling > 6	0.671	0.015	-0.000	-0.014	-0.012	-0.060	-0.000	-0.006
	(0.460)	(0.047)	(0.001)	(0.015)	(0.011)	(0.046)	(0.001)	(0.004)
HH schooling > 12	-1.466	0.213*	-0.002	0.090	-0.000	0.183	0.004	-0.002
	(1.972)	(0.110)	(0.001)	(0.067)	(0.006)	(0.128)	(0.003)	(0.003)
HH schooling unknown	0.292	-0.082*	0.000	-0.063*	0.008	-0.063	-0.002	0.004
	(1.019)	(0.068)	(0.001)	(0.033)	(0.009)	(0.076)	(0.002)	(0.003)
Adjusted R^2	0.750	0.923	0.718	0.499	0.699	0.750	0.784	0.619
Observations				8,435				
P-value of F-test of joint significance of education dummies								
Attribute in Δ	0.018	0.246	0.208	0.170	0.417	0.180	0.445	0.295
P-value of F-test of joint significance of households' characteristics								
Attribute in Δ	0.210	0.009	0.319	0.116	0.258	0.131	0.570	0.201
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓	✓
Year of intervention FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Standard errors clustered by slum of origin. 10%*, 5%**, 1%***. Attributes in columns (3) to (8)are measured at the census district level in 1982; schools, hospitals and subway are measured in 1985.

Table A.8: Displacement effect and change in location attributes on earnings

Outcome	Taxable wage	Formal earnings	Informal Earnings	Schooling
	(1)	(2)	(3)	(4)
Displaced	11.528	-9.013	-1.739	-0.394**
	(22.714)	(10.841)	(5.158)	(0.182)
Δ# schools/child	9.194*	0.609	1.563	0.083*
	(4.841)	(2.463)	(1.213)	(0.043)
ΔDistance to CBD	-2.177	0.050	-0.983**	-0.011
	(1.692)	(0.933)	(0.463)	(0.017)
ΔProperty prices	-14.805	-5.072	6.542***	-0.374***
	(9.379)	(4.374)	(2.357)	(0.077)
Project size (#units)	-0.027**	-0.015***	0.004	-0.000***
	(0.011)	(0.005)	(0.003)	(0.000)
Share network (0-100)	0.628	0.197	-0.113	0.009***
	(0.260)	(0.126)	(0.077)	(0.003)
Distance from origin (km)	-1.443	-0.371	0.352	-0.008
	(1.374)	(0.712)	(0.339)	(0.011)
Home value (UF)	0.214*	0.160***	-0.062**	0.003***
	(0.113)	(0.064)	(0.031)	(0.001)
Adj. R^2	0.061	0.068	0.045	0.114
Non-displaced mean	238.178	104.532	46.761	11.645
Percent effect (%)	**4.8**	**-8.6**	**-3.7**	**-3.4**
Municipality of origin FE	✓	✓	✓	✓
Baseline controls	✓	✓	✓	✓
Slums attributes	✓	✓	✓	✓
Observations	19,953	19,953	19,953	19,953

Notes: This table shows results equivalent to column (7) in Table 7 on different employment outcomes. Bootstrapped standard errors in parentheses. 10%*, 5%**, 1%***. The row labeled as "Percent effect" stands for "percentage variation with respect to non-displaced mean."

0.38–1.8

Table A.9: Displacement effect on the probability of selling home by 2019

	Home ever sold	Conditional on selling: Log(Price)	Year sold	# years after treatment
	(1)	(2)	(3)	(4)
Displaced	-0.023	-0.028	1.300	0.957
	(0.027)	(0.296)	(4.060)	(3.948)
Adj. R^2	0.074	0.323	0.244	0.262
Non-displaced mean	0.115	9.549	2010.045	27.037
Percent effect	**-20.0**	**-0.3**	**0.06**	**3.5**
Observations	2,326	237	237	237
Municipality of origin FE	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓

Notes: Inverse propensity score estimates in a sample of 20% of families in archives who received a home in a municipality located in the Northern areas of Greater Santiago. Number of slums of origin is 35, and number of municipalities of origin is 12. Baseline controls include: female headed household, number of children in family, married head of household, head of household's age, Mapuche head of household, head of household year of birth fixed effect, and year of intervention fixed effects. Robust standard errors in parenthesis. 10%*, 5%**, 1%***.

Table A.10: Displacement effect and subway rollout between 2007 and 2019

Distance to new station	Labor Earnings: 1.25 km	1.5 km	1.75 km	2 km
	(1)	(2)	(3)	(4)
Displaced	-35.844***	-23.048*	-20.341**	-20.898*
	(11.313)	(11.668)	(9.993)	(10.596)
Subway station	-34.114***	-27.420***	-20.229*	-20.674*
	(8.460)	(8.871)	(10.271)	(10.267)
Subway station * post	-4.032	-7.128	-1.401	-1.056
	(3.866)	(5.394)	(4.708)	(4.412)
Displaced*Subway	42.638***	15.681	8.239	6.483
	(10.969)	(11.553)	(12.515)	(11.520)
Displaced*Subway* post	-0.828	2.799	-2.509	-3.531
	(4.559)	(5.460)	(4.919)	(4.652)
# obs treated	19,247	32,404	39,173	40,767
# neighborhoods treated	8	9	11	14
Adj. R^2	0.139	0.139	0.139	0.139
Observations	400,952	400,952	400,952	400,952
Municipality of origin FE	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓

Notes: Regressions for children aged 0 to 18 at baseline that are matched to the RSH data, and report non-missing schooling. Standard errors clustered by slum of origin. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, firstborn dummy, father Mapuche, and year of birth fixed effects.

Table A.11: Displacement effect and subway rollout between 2007 and 2019

	Labor Earnings			Temp. worker	Contract
	Total	Formal	Informal		
	(1)	(2)	(3)	(4)	(5)
Displaced	-23.048*	-15.593	-7.455*	0.054	-0.019
	(11.668)	(11.787)	(4.154)	(0.048)	(0.031)
Subway station	-27.420***	-14.241	-13.179***	0.044	-0.013
	(8.871)	(8.745)	(3.081)	(0.035)	(0.029)
Subway station*Post	-7.128	-3.423	-3.705***	0.011	0.000
	(5.394)	(6.108)	(1.096)	(0.010)	(0.013)
Displaced*Subway	15.681	7.890	7.791	-0.072*	0.024
	(11.553)	(10.760)	(5.047)	(0.041)	(0.034)
Displaced*Subway*Post	2.799	2.016	0.782	-0.018	0.002
	(5.460)	(6.252)	(1.484)	(0.011)	(0.014)
Adj. R^2	0.139	0.074	0.041	0.074	0.064
Observations	400,952	400,952	400,952	400,952	400,952
# obs treated	32,404	32,404	32,404	32,404	32,404
Municipality of origin FE	✓	✓	✓	✓	✓
Baseline controls	✓	✓	✓	✓	✓
Slums attributes	✓	✓	✓	✓	✓

Notes: Propensity score regressions for children aged 0 to 18 at baseline that are matched to the RSH data, and report non-missing schooling. Bootstrapped standard errors in parenthesis. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, firstborn dummy, father Mapuche, and year of birth fixed effects. Variable subway defined as having a new subway station within 1.5 km.

B Robustness Checks

Table B.1: Displacement effect and spillovers

Outcome	Self-reported labor earnings			Taxable wages		
	Baseline			Baseline		
	(1)	(2)	(3)	(4)	(5)	(6)
Displaced	-21.207***	-21.362***	-21.857***	-31.967**	-34.360***	-29.458*
	(7.133)	(5.879)	(8.213)	(13.661)	(12.902)	(17.250)
Non-displaced $< 0.5km$		-3.372			-52.077*	
		(14.373)			(29.462)	
Non-displaced $< 1km$			-1.617			6.248
			(9.996)			(23.381)
R^2	0.134	0.134	0.134	0.057	0.057	0.057
Observations	19,953	19,953	19,953	19,953	19,953	19,953

Notes: Propensity score regressions for children aged 0 to 18 at baseline that are matched to the RSH data and that report non-missing schooling. Bootstrapped standard errors with 200 replications in parenthesis. 10%*, 5%**, 1%***. This table splits the non-displaced group at baseline into two: Non-displaced without a displaced slum nearby (omitted category, and non-displaced with a displaced slum around 1 km or less. All regressions control for year of intervention fixed effects. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, firstborn dummy, father Mapuche, and year of birth fixed effects.

Table B.2: Displacement effect instrumented by original assignment

Outcome	Labor earnings	Taxable wage	Formal earnings	Informal earnings
	(1)	(2)	(3)	(4)
A. OLS				
Displaced	-17.421***	-26.478*	-13.553**	-3.868
	(5.491)	(14.293)	(6.397)	(2.571)
Adj. R^2	0.127	0.056	0.063	0.044
B. Inv-weight pscore				
Displaced	-13.611**	-25.823***	-15.535**	1.924
	(6.588)	(9.472)	(6.061)	(1.721)
Adj. R^2	0.137	0.074	0.071	0.047
C. IV				
Displaced	-16.205***	-20.585	-10.425	-5.779**
	(5.597)	(15.473)	(5.545)	(2.559)
Adj. R^2	0.127	0.056	0.063	0.044
Municipality of origin FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Slums' features	✓	✓	✓	✓
Observations	14,056	14,056	14,056	14,056

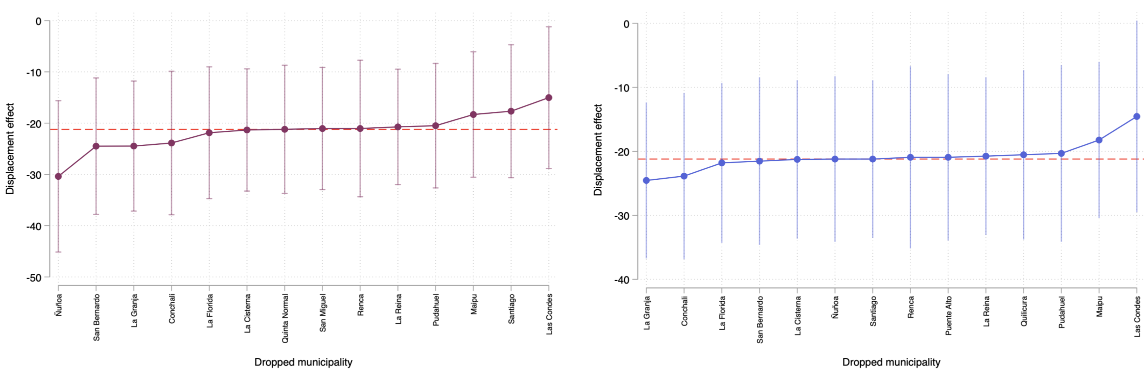
Notes: Regressions for children aged 0 to 18 at baseline that are matched to the RSH and GRIS data, and that report non-missing schooling treated between 1981 and 1985. Bootstrapped standard errors with 200 replications in parenthesis in panel A, and clustered standard errors by slum of origin in panels B. and C. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, firstborn dummy, father Mapuche, and year of birth fixed effects. Slums' characteristics include area in hectares, number of families, military name, distance to river in km, and log of property prices at origin.

Table B.3: Lee bounds for displacement effect on earnings

Outcome	Labor Income				Formal wages			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Displacement Effect								
Displaced	-25.153				-32.488			
	(9.883)***				(19.581)*			
Non-displaced mean	213.98				323.465			
Panel B. Lee Bounds								
Lower	-40.966	-34.558	-33.361	-34.841	-95.129	-89.221	-92.906	-86.743
	(5.588)***	(6.499)***	(4.271)***	(4.492)***	(10.235) ***	(9.393)***	(8.078)***	(8.668)***
Upper	-7.267	-7.138	-6.653	-7.387	-25.110	-23.293	-18.770	-20.033
	(3.419)**	(3.877)*	(3.563)*	(3.666)**	(8.549)***	(8.018)***	(7.395)**	(7.822)**
Municipality of origin	✓	✓	✓	✓	✓	✓	✓	✓
Gender & age		✓	✓	✓		✓	✓	✓
Mother HH			✓				✓	
# Children				✓				✓
Observations (selected)	17,687	17,687	17,687	26,124	17,687	17,687	17,687	26,124
Observations	21,489	21,489	21,489	21,489	21,489	21,489	21,489	21,489

Notes: Sample does no include individuals treated prior to 1979 because there is no variation in treatment. All regressions include a dummy for year of treatment prior to 1981. Clustered standard errors by slum of origin in panel A, bootstrap standard errors with 250 replications in panel B.

Figure B.1: Results on earnings robust to dropping each municipality once from sample



(a) Municipalities of origin

(b) Municipalities of destination

Notes: The figure plots the displacement coefficient from equation (1) for 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)). Standard errors clustered by slum of origin. All regressions include year of intervention fixed effects. Baseline controls include the following: female, mother head of household, single head of household, number of siblings, firstborn dummy, and cohort fixed effects.

Table B.4: Conley standard errors

Outcome	Labor income	Taxable wages
Displacement coefficient	-21.207	-31.967
Clustered se by slum of origin	7.077	11.035
Bootstrapped se	6.659	13.711
Conley se (cutoffs in km)		
1	6.922	11.072
2	6.863	11.083
3	6.937	11.225
4	7.025	11.337
5	7.072	11.391
6	7.357	11.654
7	7.577	11.893
8	7.731	12.065
9	7.817	12.192
10	7.937	**12.208**
11	**8.035**	12.192
12	8.034	12.044
13	7.995	11.921
14	7.904	11.899
15	7.805	11.869

Notes: This table reports estimates of Conley Standard errors on labor earnings for different distance cutoffs (Conley, 1999). The estimation procedure comes from Thiemo Fetzer. For more details see here.

B.1 Displacement effect coefficient and sensitivity to omitted variable bias

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

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

$$Y_{it} = \alpha + \beta Displaced_{s\{i\}} + \psi_o + \varepsilon_{it} \tag{2}$$

$$Y_{it} = \tilde{\alpha} + \tilde{\beta} Displaced_{s\{i\}} + \tilde{\psi}_o + X'_{it}\theta + \tilde{\varepsilon}_{it}, \tag{3}$$

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

Table B.5: Displacement effect with a control function and by sub-periods

	Baseline	Polynomial ARNAD	Period 1979-1984	Before 1982
	(1)	(2)	(3)	(4)
Panel A.	Outcome: Labor Income			
Displaced	-21.207***	-21.093***	-22.356***	-14.503*
	(6.659)	(8.087)	(6.244)	(7.922)
Adj. R^2	0.131	0.131	0.131	0.132
Panel B.	Outcome: Taxable wages			
Displaced	-31.967**	-24.487	-32.026***	-33.305**
	(13.711)	(16.675)	(14.243)	(14.874)
Adj. R^2	0.059	0.060	0.060	0.062
Observations	19,953	19,953	19,227	10,474
Municipality of origin FE	✓	✓	✓	✓
Baseline controls	✓	✓	✓	✓

Notes: Propensity score regressions for children aged 0 to 18 at baseline that are matched to the RSH and the GRIS and report non-missing schooling. Bootstrapped standard errors in parenthesis. 10%*, 5%**, 1%***. All regressions include year of intervention fixed effects. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, firstborn dummy, father Mapuche, and year of birth fixed effects. Column (1) is the baseline regression, columns (2) and (3) control for the probability of finding a child in the RSH or finding a slum in the Archives, respectively. Column (3) restricts the sample to years 1979 to 1984, and column (4) restricts the sample to years 1979 to 1982.

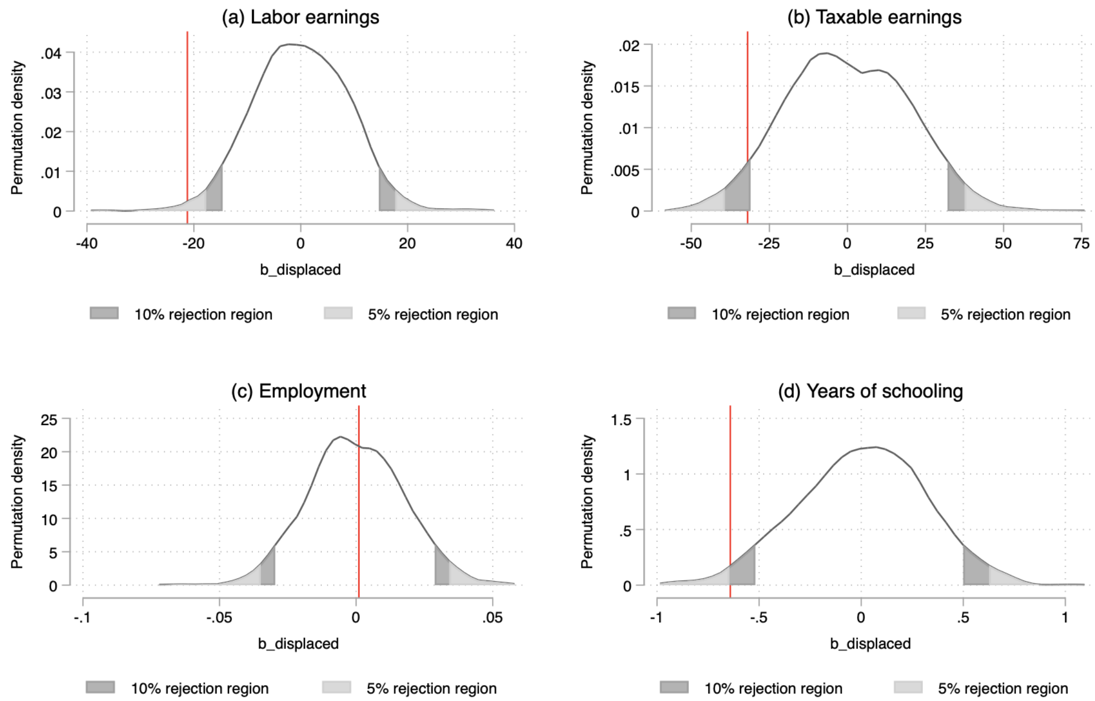
0 otherwise. The variable ψ_o are municipality of origin fixed effects. The matrix X_{it} includes baseline controls for individuals' and families' characteristics, such as gender, child's year of birth, female head of household, married head of household, head of household's age, birth-order dummies, mother's schooling, and year of intervention fixed effects (1979-1985). Under the assumption that X_{it} is uncorrelated with displacement, we would expect that $\beta = \tilde{\beta}$.

Following Oster (2019) we 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,

$$\beta^* \sim \tilde{\beta} + \delta(\tilde{\beta} - \beta)\frac{R_{max} - \tilde{R}}{\tilde{R} - R}, \tag{4}$$

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

Figure B.2: Permutation tests



Notes: Distribution of permutations tests on main outcomes performed in 1000 replications. Red lines indicate the average displacement effect on main sample of children aged 0 to 18 at baseline. Gay areas indicate 10% and 5% rejection regions.

the observable variables. For example, $|\delta| = 1$ implies that the degree of selection on unobservables is equally important as the observables.

Then, we use equation (5) to bound the true value for β^*. First, we estimate β, β^*, R and $\tilde{R}$ from equations (3) and (4). Second, we vary the values of δ and R_{max}, we choose $R_{max} = 1.3\tilde{R}$, as recommended by Oster (2019), and we also choose $R_{max} = 5\tilde{R}$ as a more conservative case. Then we vary the value of δ to be 2 or 3. For example, Altonji et al. (2005) assume $\delta = 1$. Our results are in B.6.

The column labeled as $\hat{\delta}$ reports the estimate for δ for different values of R_{max} 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. In other words, under different values of δ that vary between 1 and 3, we find smaller magnitudes for the displacement effect, but they never become non-negative. The only case where we find a less negative displacement effect, is when $R_{max} = 1.3\tilde{R}$

and $\delta = 2$, and for the very extreme case $R_{max} = 5\tilde{R}$ not even suggested by Oster (2019), our results are very stable.

Table B.6: Displacement effect under different assumptions on selection on unobservables

Outcome	R^2 max	$\hat{\delta}$	δ	$\hat{\beta}^*$	$\hat{\beta}^*$
	1.3	1.822	1	-28.738	-29.121
	1.3		2	-5.979	-7.590
Labor Earnings	1.3		3	-13.557	-13.995
	5	0.138	2	-17.162	-17.280
	5		3	-17.241	-17.354
	1.3	0.951	1	7.716	-15.285
	1.3		2	-60.239	-64.515
Taxable wages	1.3		3	-50.205	-53.312
	5	0.072	2	-43.107	-45.239
	5		3	-42.926	-45.032
	1.3	1.289	1	-0.639	-0.288
	1.3		2	-0.643	-0.756
Years of schooling	1.3		3	-0.642	-0.667
	5	0.100	2	-0.642	-0.610
	5		3	-0.641	-0.609
Included controls:					
Baseline controls				✓	✓
Mother's schooling					✓

C Displacement Effect on Other Family Members

Table C.1: Adults' labor market outcomes, heads of households in the RSH

Outcome	(1) 1[Employed]	(2) Total income	(3) Labor income	(4) Retirement income
Panel A.	Parents younger than 65 years old			
Displaced	0.053**	-5.935	-5.698	-9.314
	(0.022)	(9.994)	(7.492)	(6.463)
Non-displaced mean	0.530	92.904	93.387	40.949
Adj. R^2	0.127	0.127	0.127	0.021
Individuals	6,362	6,362	6,362	6,362
Panel B.	Parents older than 65 years old			
Displaced	0.061**	-9.582	5.116	-18.031**
	(0.023)	(10.048)	(3.703)	(9.010)
Non-displaced mean	0.276	80.782	20.544	106.223
R^2	0.054	0.083	0.080	0.041
Individuals	6,022	6,022	6,022	6,022
Municipality of origin FE	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓

Notes: Inverse propensity score regressions for head of households matched to the RSH data. Standard errors clustered by slum of origin. 10%*, 5%**, 1%***. Controls include the following: 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 C.2: Displacement effects for children born to treated families

Outcome	Employed (1)	Labor income (2)	Taxable income (3)	Years of schooling (4)	HS graduate (5)	College attendance (6)
Displaced	0.021	-9.921	-43.148***	-0.590*	-0.091*	-0.005
	(0.035)	(12.055)	(13.792)	(0.345)	(0.054)	(0.038)
Non-displaced mean	0.558	158.444	341.494	11.701	0.773	0.304
Adj. R^2	0.067	0.087	0.037	0.071	0.053	0.030
Observations	3,069	3,069	3,069	3,069	3,069	3,069
Municipality of origin FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓

Notes: Inverse propensity score regressions for children born 1 to 5 years after the intervention that are matched to the RSH data, and report non-missing schooling. Clustered standard errors by slum of origin. 10%*, 5%**, 1%***. Baseline controls include the following: female, mother head of household, married mother at birth, age of mother at birth, number of siblings, father Mapuche, cohort fixed effects, year of treatment fixed effects.

Table C.3: Displacement effects for grandchildren in schooling ages (2003-2019)

Outcome	School Attendance (1)	School Attendance (2)	Dropout (3)	Dropout (4)	Old for grade (5)	Old for grade (6)
Displaced	-1.000***	-0.492	0.005*	0.002	0.020*	0.004
	(0.344)	(0.297)	(0.003)	(0.003)	(0.011)	(0.007)
Non-displaced mean	87.453	87.453	0.085	0.085	0.128	0.128
Adj. R^2	0.082	0.172	0.569	0.604	0.102	0.262
Observations			361,780			
Grandchildren (children)			42,874			
Children (parents)			19,084			
Parents (grandparents)			8,995			
Municipality of origin FE	✓	✓	✓	✓	✓	✓
Family controls	✓	✓	✓	✓	✓	✓
School FE		✓		✓		✓

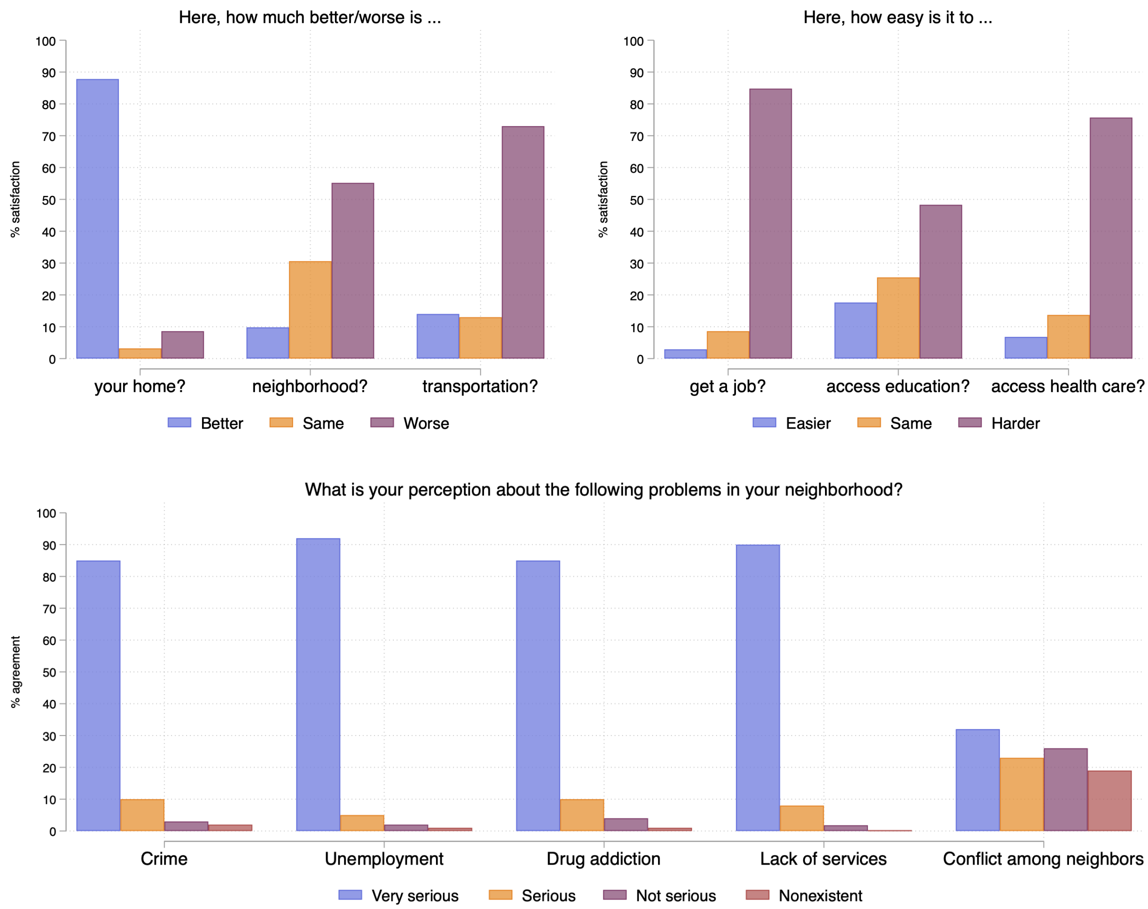
Notes: Propensity score regressions for grandchildren born to displaced and non-displaced children in estimation sample that are matched to the MINEDUC data. Bootstrapped standard errors in parenthesis. 10%*, 5%**, 1%***. Baseline controls include the following: female, grandmother head of household, married grandmother at birth, age of grandmother at birth, number of siblings, Mapuche last name, cohort fixed effects, year of treatment fixed effects.

D Eviction Policies

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

Notes: Examples of neighborhoods from Hidalgo (2019).

Figure D.2: Summary of evaluation of the Program for Urban Marginality (Aldunate et al., 1987)



Notes: Summary of results found by Aldunate et al. (1987). The authors interviewed 592 displaced slum dwellers that were relocated into four new neighborhoods.