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 Chile between 1979 and 1985. During the dictatorship, slum-dwelling families were forced to relocate to low-income areas. Two-thirds of them were relocated to housing projects on the city's periphery, while one-third received housing at their original locations. We find that 35 years post-policy, displaced children receive 0.81 fewer years of schooling, earn 9% less, and experience higher labor informality compared to non-displaced children. Distance from origin, disrupted social networks, and lower home values explain the negative displacement effects.

Keywords: slum clearance, children, neighborhood effects, forced displacement.
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1 Introduction

Due to rapid urbanization and a lack of affordable housing, 25% of the world's urban population currently lives in slums (UN-Habitat, 2020). Common policy approaches targeting informal settlements include on-site slum upgrading (Harari and Wong, 2024), sites and services programs (Michaels et al., 2021), urban redevelopment (Gechter and Tsivanidis, 2024), and slum relocation. However, the informal nature of slums complicates our understanding of these policies, especially on individuals. The challenges posed by lack of data and selection bias make evaluating relocation policies particularly difficult. Moreover, tracking slum residents over time to assess the long-term impacts on families and children's human capital and labor market outcomes result in additional challenges.

This paper addresses these issues by examining the long-term impacts of slum relocation on children's education and future earnings. 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, the capital of Chile. All participating slum-dwelling families became homeowners of similar housing units. While some slums were upgraded into neighborhoods, others were relocated to suburban areas, involving two types of interventions. In the first, when urban conditions permitted, the slum was upgraded into a proper neighborhood, and families remained in their original location (i.e., non-displaced). In the second type, when upgrading was not possible, the slum was cleared and families were evicted and forced to move in groups to new public housing projects (i.e., displaced).

We collect archival records of slum dwellers that we match to administrative data to create a novel dataset that follows children and parents from non-displaced (redeveloped) and displaced (relocated) slums from 20 to 40 years after the policy ended. We take advantage of the fact that slum-dwelling families received a property deed associated with a unique national identifier. Using these identifiers, we can 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 30,680 children aged 0 to 18 at the time of treatment who were treated between 1979 and 1985 and who we observe as adults from 2007 to 2019.

We use variation in treatment to estimate a displacement effect that compares displaced children to non-displaced children. An important identification concern is that displaced and non-displaced slum residents were different. The selection of slums for displacement or non-displacement was based on the feasibility of urban renewal rather than on individual family characteristics, such as slum density, geographic location, and price of land. To address this concern, we leverage the program's selection rule and our rich dataset to estimate a policy function that estimates the probability of a slum being relocated versus being redeveloped. We then compare displaced and non-displaced children from slums with the same probability of being relocated. Conditional on the probability of a slum being relocated, we find no correlation between the selection of slums for displacement and children's demographic and socioeconomic characteristics, such as age, gender, family composition, or household employment before the program's implementation.

We find that displacement is detrimental for children. Compared with non-displaced children, displaced children earn 8.9% 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 work with a contract or contribute to social security. We also find that displacement reduces children's educational attainment: a displaced child loses 0.81 years of education and is 21.6% less likely to graduate from high school relative to a non-displaced child. 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$11,000, which is larger than the cost of the house received by the average family in our sample (US$10,148 in 2018, on average). We also show that our results are not driven by selection into administrative data, nor by the selection of slums found in the archival records.

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 for young children aged 0 to 12 years old at the time of the intervention. Within this group, 0- to 5-year-olds face the most negative effect 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).

In addition to being forcibly moved, displaced families were assigned specific destinations, mostly in low-income municipalities on the city's periphery. These areas were generally characterized by high poverty rates and low provision of public goods, but the degree of change varied between the destinations and origins. This variation allows us to study place effects by investigating which neighborhood characteristics predict children's future earnings. Importantly, displaced families had no choice in their relocation, limiting potential selection at destination. We also show that family demographics do not systematically predict the attributes of their destination locations.

Using archival records, we confirm that displaced families were relocated to peripheral neighborhoods with higher unemployment rates and greater distances from the city center compared to non-displaced families, who remained in their original locations. Consequently, displaced families received homes of 12% lower value compared to non-displaced.

In our sample, most of the negative effect on earnings is due to new destination locations, as opposed to be driven by improvements in the non-displaced group. To understand which attributes of destinations are most determinant, we estimate a distribution of displacement effects on children's future earnings by municipality of origin. We find considerable variation in impact levels, but in most cases children faced a negative displacement effect regardless of their municipality of origin. This leads us to explore granular changes to neighborhood characteristics to understand the determinants of the displacement effect, such as changes to public services access, segregation, transportation access, and disruption of social networks.

We find that more than 80% of the displacement effect on earnings can be explained by changes in 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 the destination). A small share of the total effect is explained by changes in broader neighborhood characteristics, such as access to schools, unemployment rates, and property prices. Many of these attributes correlate with the home value that families received, as those sent to more peripheral areas received cheaper houses. When we control for home value, it explains 28% of the displacement effect itself. We also find an age gradient on the mechanisms studied. In particular, young children at baseline benefit more from higher home values, smaller project sizes, and more schools at destination, but network disruption affects young children and teenagers in similar magnitudes.

Next, we investigate current individuals' neighborhoods. We find that the program had persistent effects on families' locations. Thirty years after the program ended, 60% of parents remain in the same destination municipality, and 45% of their children, who are now adults, reside in the same municipality. In addition, their neighborhoods are 7% poorer compared to those of non-displaced children.

Finally, based on these results, we examine whether new subway infrastructure helps decrease the earnings gap between displaced and non-displaced children in their adulthood. Exploiting the rollout of subway stations in Santiago after 2006, we find that having a new subway station close to families' destination locations reduces the displacement effect between 65% and 77%, depending on the distance to a new station. The effects are mainly driven by increases in formal earnings and a decrease in the likelihood of being a temporary worker.

This paper contributes to several strands of literature. First, it contributes to the literature that evaluates policies that target slums. Because tracking slum dwellers is challenging, most previous work has focused on evaluating policies on places and estimating indirect effects on individuals. Examples include Michaels et al. (2021), who study a "sites and services" program in Tanzania, finding positive long-term impacts; Harari and Wong (2024), who study urban renewal on-site in Indonesia, finding lower land values and more informality in redeveloped areas; and Gechter and Tsivanidis (2024), who find large positive aggregate effects from redevelopment in India.[1] Almost no research has provided evidence on the effects of slum clearance and relocation policies on individuals' human capital, and there is even less evidence of their effects on children. We find that displacement versus on-site redevelopment is harmful because it disrupted slum networks and relocated families far away from

[1]Another similar literature has evaluated the effects of land titling (Field, 2007; Franklin, 2020) and improvements on-site but without clearing slums (Galiani et al., 2017), all focusing on adults. More recently, Sims (2023) studies the effect of homeownership on children's educational investments.

their original locations.[2]

This paper also contributes to the literature studying the impact of neighborhoods on intergenerational mobility, which finds heterogeneous results by outcome and age (Chetty et al., 2016; Chyn, 2018).[3] Recent studies like Camacho et al. (2022), for Colombia, and Agness and Getahun (2024) for Ethiopia, study the effects of housing on children. We complement this literature by studying intergenerational impacts in the context of slums, one of the main forms of shelter in developing countries, and of key interest for policymakers. We contribute to the understanding of the mechanisms behind neighborhood effects on children, by exploiting relocation with variation in destination locations, and group movements. These features help us decomposing the total displacement effect (Damm and Dustmann, 2014).[4]

Finally, we contribute to the literature that studies cities in the developing world (Glaeser and Henderson, 2017; Bryan et al., 2020). This literature emphasizes the challenges faced by developing countries due to rapid urbanization and the determinants of slum proliferation. For example, Henderson et al. (2021) model the evolution of slums within a city, and Gonzalez-Navarro and Undurraga (2023) study slum formation in the context of immigration. We contribute to this literature by studying the consequences of a city-wide housing relocation program on individuals. This type of policy is one of the main instruments to tackle the lack of affordable housing, but for which we do not have much causal evidence in the long run.

The rest of the paper is organized as follows. Section 2 describes the historical context 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 presents the mechanisms. Section 7 discusses the

[2]Barnhardt et al. (2016) and Picarelli (2019) study housing lotteries in India and South Africa, respectively, and find a negative relationship between distance from origin and adults' labor market outcomes. This finding also aligns with the spatial mismatch hypothesis (Kain, 1968; Andersson et al., 2018).

[3]Mogstad and Torsvik (2021) and Chyn and Katz (2021) conduct extensive literature reviews on neighborhood effects, but most papers study the developed world. Another recent paper by Carrillo et al. (2023) studies displacement effects on education in the context of the Apartheid in South Africa.

[4]Our findings also relate to the literature studying how shocks during childhood affect adult outcomes (Currie and Almond, 2011; Heckman, 2006). 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.

displacement's total effect on earnings, and Section 8 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 capital, approximately 15% of the population lived in a slum (INE, 1970; INE, 1982), defined as a squatter settlement without access to drinking water, electricity, or sewage (MINVU, 1979).[5] These slums were geographically ubiquitous, and after the Pinochet dictatorship began in 1973, any attempt to create a new slum faced a strong military response.[6]

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 believed the most effective way to end poverty was to make poor families homeowners, regardless of the attributes of the new housing units or neighborhoods (Murphy, 2015). At the program's onset, the government conducted a census of slums and targeted 340 of them to be cleared.[7] 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 program had two goals: to build public housing for low-income families where land was cheap, and to provide them with housing in affordable locations. 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 on-site housing (i.e., were not displaced). If this was not possible, they 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

[5] The median slum had around 200 families, with an average size of 5.2 persons per family.

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

[7] 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, they 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. Because these evictions were politically motivated, we do not include them in our analysis (Celedón, 2019).

homeowners.[8]

The features of each intervention are as follows. Non-displaced families accounted for one-third of the total number of families in the program. In some cases, they were provided with an apartment in housing projects constructed very close to their original location, while for others, the slum's land was subdivided among residents, with each family receiving a "starting-kit unit."[9] These new neighborhoods were provided with all of the basic services of a formal neighborhood (water, electricity, and sewage). On-site housing was constructed quickly and in stages, with families remaining on the same sites during the process.

Displaced families accounted for two-thirds of the total number of families in the program. They were evicted and moved in groups to public housing projects located in the city's peripheral sectors, where they became owners or either a house or an apartment. The land was then cleared and repurposed.[10]

Funding for the homes came from a direct government subsidy designed to cover 75% of the construction cost but was capped at 200 UF (inflation-adjusted index).[11] 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 or 25 years. Families were not allowed to sell the house until they paid for all the installments. 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.[12]

[8]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.

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

[10]All families would be evicted, and 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.

[11]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.

[12]This number is based on our own calculations from archival data on average home values and subsidies, and it is comparable to the current expenditure in homeownership subsidies in Chile (see the OECD's website for more details).

Decisions regarding the implementation were made directly by the MINVU at the central level.[13] Displaced families could not participate in decision-making, and given the political circumstances, they could not oppose the policy (Rodríguez and Icaza, 1993). 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.[14] 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, ranging 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) note, "other criteria included the reputation of the municipality of origin, their land values, and the speculation about future prices."

One 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 in the original location. However, by 1981, the slum's high density made it impossible to allocate plots inside the slum in a way that guaranteed a minimum size for all the plots, and therefore MINVU decided to include Las Palmeras residents among the displaced. In late 1983, they were moved to a new neighborhood built on the municipality's outskirts, and the former

[13] Santiago lacked a citywide government; instead, 30 local municipalities were responsible for managing their respective territories. Under this governance structure, citywide policies such as social housing were determined by the 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).

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

slum became a park. Another example is the slum dwellers located on the riverbank of the Mapocho River, who were displaced in 1982 after it flooded. More than 3,000 families from the El Ejemplo, El Esfuerzo, and El Trabajo slums—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 slum characteristics collected by Morales and Rojas (1986) and from the MINVU's slum censuses, we find the same patterns established by previous researchers. We report means by intervention in columns (1) and (2) of Table 1, and column (3) reports the simple difference between treatments. Panel A shows that displaced slums are denser as they house fewer families in smaller land areas. They are located in more elevated areas with higher slopes, are closer to rivers or canals, and have a higher risk of flooding. They are also closer to the central business district (CBD), but the difference between the treatments is small. Additionally, in Panel A we classify slums' names as either military related or not related as a proxy for support for the dictatorial regime, finding that displaced slums are less likely to have a military-related name.[15]

In Panel B we report attributes of the census districts where slums were originally located to proxy for neighborhood characteristics. We find that displaced slums are located in areas with higher average schooling, lower unemployment rates, and slightly higher property prices but fewer schools. In column (4) we report the difference in slum characteristics within municipalities of origin. Municipality fixed effects do not systematically reduce the difference in slum characteristics, indicating that even within municipalities, the urban attributes of slums determined their probability of relocation. This finding is consistent with the discussion by Rodríguez and Icaza (1998).

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 families in 1985. The neighborhoods where housing projects were built for the displaced are represented by purple areas,

[15] We classify the name of each slum as being military related if it refers to any military historical event, such as wars or the coup d'etat of September 11 of 1973, or names of heroes of the country who were in the military.

and housing projects for the non-displaced are represented by blue areas. Two important conclusions can be drawn from this figure: the new housing projects were disproportionately built in the city's peripheral areas, and public housing projects were farther from job opportunities (in gray scale).

After 1985, Aldunate et al. (1987) surveyed 592 displaced families, who reported that they thought their homes were better than their previous ones. However, they reported that the quality of their new neighborhoods was worse than the slums, citing fewer job market opportunities and limited access to transportation, education, and health care services. They also perceived their new neighborhoods as more dangerous and lacking public services (see Figure A.1 for a summary).

3 Data

In this section, we summarize the data collection process. We first 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 schooling and labor market outcomes.[16]

3.1 Slum census and archival data

We digitize two slum censuses conducted by the MINVU in 1979 and 1980 that contain data on slum names, slum locations, and destination neighborhoods. Each slum is classified as either displaced or non-displaced, and we record the final destination of families from displaced slums. We then complement these data with information collected by Molina (1986), Benavides et al. (1982), and Morales and Rojas (1986), who compiled a full list of slums, locations, and destination neighborhoods by year.

Next, we find families in the program by obtaining archival data from the Metropolitan Regional Housing and Urban Planning Service of Santiago and historical records kept by the Municipality of Santiago.[17] These records correspond to the lists of homeowners and their spouses who received a property deed through the program.

[16]For a detailed description of the data collection process and variable definitions, see Section 1 of the supplementary material to this paper.

[17]Each region of Chile (equivalent to a state) has an Urban Development and Housing Service (SERVIU), run by the MINVU, and administers housing policies at the local level.

We focus on individuals in Greater Santiago from 14 urban municipalities with variation in treatment, that is, municipalities with displaced and non-displaced slums. We attempt to collect all the surviving households records, yielding 16,947 unique recipients of social housing. These families come from 99 different slums and were assigned to 73 different destination projects.

The families we find represent 61.5% of the total number of recipients in urban areas.[18] Of these, 69% are displaced and 31% are non-displaced, which implies a slightly higher proportion of displaced families compared to the original program (Molina, 1986).[19] One reason for this higher proportion is that larger destination neighborhoods often contain multiple slums of origin, while non-displaced slums typically correspond one-to-one with destination projects. In the following sections, we discuss how this could affect our results and provide robustness checks when needed.

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), new addresses, and total cost of the new property in UF. These records are grouped by year of relocation/redevelopment and destination neighborhood, and we match them to their slum of origin using the 1979 and 1980 slum censuses.

3.2 Matching process: Children's sample

Our next step consists 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.[20] The birth certificates contain the children's 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.[21] We identify 47,072 children of 15,136 unique families in 99

[18]We exclude rural municipalities since most of the neighborhood characteristics that can be measured in the 1980s are only available in urban areas. And because most urban areas only received displaced families.

[19]Based on the numbers in Molina (1986), two-thirds of families were displaced by the end of 1984.

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

[21]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

slums. Of these, 33,669 were between 0 and 18 years old at the time of treatment.

Using birth and marriage certificates, we measure demographics at the time of the intervention. We observe gender, date of birth, number of children per couple, parents' age, and marital status. Because we observe individuals' full names, we can identify indigenous status based on last names. Using the Mapuche Data Project, we identify last names that are Mapuche, the largest indigenous group in Chile. Finally, we measure parents' formal employment at the slum level between 1975 and 1980, using historical records from Chile's Superintendency of Pensions.[22]

3.3 Measuring outcomes: Matching to administrative data

We match children and parents to several administrative data sources using NID numbers. Our main 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 provides 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 it. 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. We find 81.5% of children and 79% of adults from the archival sample in the RSH.

To validate self-reported earnings with administrative records, we merge individuals to the Gestión de Reportes e Información para la Supervisión de Mutuales (GRIS), an information system managed by Chile's Superintendency of Social Security. GRIS collects data on all workers in the formal sector who contribute to social security each month. Hence, any worker listed in it is formally employed. We observe monthly data on taxable income from July 2016 to December 2019.

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 supplementary material for a full explanation of the process.

[22]The Superintendency of Pensions does not provide researchers with individual-level data. However, since we have access to individuals' NID, they can provide us with aggregate data by groups. Thus, for the list of adults with NIDs in our sample, we requested the average formal employment rates before treatment by slum, gender, and household head status.

3.4 Municipality and neighborhood attributes

Using locations of slums and destination projects, we measure location attributes by municipality and census district from the 1982 Population Census, which contains 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 compute a neighborhood-level property price index from newspaper listings from 1978 to 1985 that we collect and digitize.[23]

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 family demographics. The empirical strategy involves comparing the children of displaced families with those of non-displaced families who come from slums with the same probability of being relocated. The process of selecting slums into displaced and non-displaced groups did not depend on households' characteristics but rather on the feasibility of renewal on-site.

Under the assumption that we know and observe the slum characteristics that determine treatment, we can compute the probability of a slum being relocated as a function of its urban characteristics. Then, we 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 groups 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 the following specification:

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

[23] See supplementary data appendix for a detailed description of each variable.

where Y_i is the average outcome for individual i in adulthood, such as labor income, employment status, and years of schooling,[24] 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. $p(X_s)$ is the propensity score that is a function of slum characteristics X_s. 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, Mapuche last name, head of household formal employment by slum, 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 cluster the standard errors by slum of origin; however, in Section 5.6 we show robustness to other clustering methods.[25]

In addition, estimating 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. Moreover, the overlap condition means that we can compare displaced and non-displaced children within the common support of the propensity score (Rosenbaum and Rubin, 1983). Note that our propensity score is only a function of slum characteristics (s), not individual characteristics (i), because the policy function was at the slum level rather than the individual level.

Equation (1) implies that we match on the propensity score, which requires first estimating the propensity score function (Abadie and Imbens, 2016). We choose matching instead of propensity score re-weighting because it offers greater flexibility and is more effective in cases where the overlap of the common support is imperfect (Busso et al., 2014). In the next section we show robustness of our results to different versions of the propensity score method.

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

[25] Additional clustering methods, such as Conley and bootstrapped standard errors, are discussed in the next section.

4.2 Propensity score estimation

To estimate the probability of relocation, we use data from Morales and Rojas (1986), who compiled the most complete sample of slums and their characteristics in urban areas. In these data we observe 222 slums with information on their characteristics (Table 1). We estimate the probability of relocation using a logit function on the following set of characteristics: density (families per hectare), military name, elevation, slope, proximity to a river or canal, flooding risk, distance to the CBD, population's schooling, unemployment rate, and number of schools per census district. We exclude the price index from the propensity score because it might reflect expectations of future land prices due to slum clearance, as well as municipality-of-origin fixed effects since differences between slums within the same treatment remained within municipalities.[26] The estimates of this exercise are presented in Table B.1, column (1).[27]

We use the estimates from the previous regression to predict the probability of slum relocation in our archival sample of 99 slums. This approach increases statistical power and reduces selection on observables, as the slums in the archives are less peripheral and show more similarity between treatments. Figure B.1 presents the results of the estimation. Panel (a) shows the propensity score densities by treatment in Morales and Rojas (1986)'s sample, and panel (b) depicts the same figure for slums in our archival sample. Importantly, in both figures there is common support. In Appendix B, we discuss the differences between samples in more detail.

We implement the propensity score method in four steps. First, we estimate the propensity score $\hat{p}(X_s)$ at the slum level using a logit function. Second, we restrict the sample to have common support. Based on the propensity score densities by treatment in Figure B.1, panel (b), we keep slums where $0.15 < \hat{p}(X_s) < 0.70$: from the 99 slums in our archival sample, 90 are in the common support. Third, we generate dummies for each decile of the distribution of the estimated propensity score. Last,

[26] In Section 5.6 we perform robustness checks where we include these variables in the propensity score. The results are very similar to the main results on children's outcomes.

[27] We choose the results in column (1) for our propensity score. In column (2) of Table B.1 we add the price index as a control. Including this variable does not change the estimates of the coefficients of other slums' attributes. The estimate of the price coefficient is negative and not statistically different from zero. Other variables like elevation, population' schooling, and schools per district are more predictive of the probability of slum relocation.

we estimate equation (1) on the outcomes of interest where $\hat{p}(X_s)$ is included as a full set of propensity score dummies interacted with municipality-of-origin fixed effects. This ensures that we compare displaced and non-displaced children within the same municipality in the same decile of the propensity score estimate.[28]

Finally, to provide evidence that our matching procedure guarantees a balanced sample of slum characteristics before the intervention, in column (5) of Table 1 we report the difference between displaced and non-displaced slum attributes controlling for decile dummies of the estimated propensity score. Our results show that matching generates a balanced sample of slums in the common support.

4.3 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 were at least 25 years old at the time of the income/employment measurement as adults.[29] Table 2 presents summary statistics for children at the time of the intervention. Column (1) shows that in the full archival sample, 69.4% of children come from displaced families. These families average four children each. Of these children, 37% are firstborn, half are female, and the average age is 8.14 years. Parents are, on average, 34.7 years old on average at baseline. Moreover, 33% of the children come from a female-headed household, and 89% have parents who are married or cohabiting at the time of the intervention. Additionally, 9% have an indigenous Mapuche last name, and all children come from slums where 40% of head of households were formally employed before treatment. Finally, only 0.6% of the children died before 2007.

Column (2) shows the same summary statistics for children who lived in slums within the common support of the propensity score, showing average demographics very similar to those in the full archival sample in column (1). Column (3) also provides the same statistics but specifically for children in the RSH within the common

[28] A more strict approach would be to perform a block propensity score by municipality of origin (Heckman et al., 1998). In our data this is not possible, as we would require a larger number of slums per municipality to estimate a different propensity score density in each municipality of origin.

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

support, where 81.6% of the children are in the RSH. These children are more likely to be female, younger, and displaced. Importantly, no slum is omitted from columns (2) and (3), ensuring that all selection is based on demographics within the same slum.

Column (4) estimates the linear probability of a child being in the common support as a function of demographics at baseline (column (2) relative to column (1)). The coefficients on most demographics are small, but that related to adults' formal employment indicates that slums with higher levels of formal employment are more likely to be excluded from the sample. In column (5) we regress the probability of being found in the RSH for children in the common support (column (3) relative to column (2)). As previously mentioned, 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 will match with older children. For gender, we find that women are over-represented in the RSH, consistent with the fact that women are more likely to be in the lower part of the income distribution and to request social benefits. Last, while no child deaths are reported in the RSH after 2007, they are too rare to account for all non-matched individuals.

The above summary statistics for the matched RSH sample with common support in urban areas indicate this is a sample of children that were more likely to be displaced, young, or female.[30] 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 groups, or if they affect these groups differently. In the next subsection we show that this is not the case.

4.4 Evaluation of the identification strategy

The validity of our research design depends on whether the decision to displace a slum was uncorrelated with family characteristics, conditional on the probability that their slum was cleared. Under the assumption that conditional on the policy function $p(X_s)$, 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 demo-

[30] Based on our attrition rates from the archives, this sample would correspond to 50.2% of children from slums with common support in urban areas.

graphics of the displaced and non-displaced children at the time of the intervention.

Table 3, columns (1) and (2) report means for the demographics of children in the sample with common support for the non-displaced and displaced groups, respectively. Column (3) reports the difference between groups conditional on $p(X_s) \times \psi_o$. Based on these adjusted differences, displaced and non-displaced children with similar probabilities of experiencing relocation have similar demographics at baseline, with no statistical differences between both groups for 18 out of 19 observables. The only variable that shows a statistically significant difference from 0 is the number of children per couple; however, the estimate is small in economic terms (0.177/3.773).

The results are very similar and even more balanced for the children matched to the RSH (columns (4)–(6)). Displacement, gender, and age were the main determinants for matching children to the RSH. However, the baseline demographics are not unbalanced between treatment groups, indicating that the over-representation of displaced individuals in the RSH is not due to their demographic characteristics. Overall, children in the RSH do not appear systematically different from those in the full sample, except for a 4 percentage point over-representation of women. Importantly, all 90 slums are retained in our matched sample.

Notice that when we estimated the propensity score we did not target balance in children's demographics, but on slums' characteristics before treatment. Thus, this table provides evidence that the methodology we use ensures balancedness in moments not targeted by the method in the first place.

5 Results

5.1 Displacement effect on new location attributes

To estimate the program's displacement effects on new neighborhood attributes, we analyze the densities of various characteristics in the relocation areas of both displaced and non-displaced households. Figure 2 illustrates these densities, with panel (a) reporting unemployment rates. The analysis shows that displaced families were more likely to be relocated to areas where the unemployment rate is 3 percentage points higher, or 15% higher compared to those of non-displaced households. Panel (b) plots densities for the prices of properties surrounding the new public housing projects, and on average, displaced families were relocated to areas with lower price values. In addition, their homes were 12% cheaper, on average (panel (c)). Most displaced families received a house that cost 220 UF, and while the variance in cost for non-displaced families is larger, most non-displaced received houses above 250 UF.

These patterns, consistently align with the fact that compared to non-displaced families, displaced families were relocated farther from the city center, by an average of 2.5 kilometers (panel (d)), and even farther away from their slums of origin, by 8.6 kilometers (panel (e)).

5.2 Displacement effect on labor market outcomes

We continue our analysis by examining the earnings and employment of individuals with non-missing education (aged 0 to 18 at baseline) who were 25 to 55 years old at the time of income measurement. The main outcomes studied are self-reported labor earnings and self-reported employment (including both formal and informal employment) in the RSH between 2007 and 2019. Self-reported earnings measure income from both formal and informal employment, which include wage income and proprietor labor income but exclude pensions and transfers.[31] Labor earnings are measured in 1,000 Chilean pesos per month (CLP$1,000/month).[32] 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 (Panel A) and a null effect on employment (Panel B). Column (1) reports the difference in outcomes between displaced and non-displaced children conditional on the municipality of origin and baseline controls. Column (2) adds slum characteristics before the intervention and indicates that displaced children have lower future earnings compared with those who were not displaced. The coefficient of –16.011 in column (2) of Panel A is statistically significant at 1%, meaning that displaced children, as adults, earn an average of 9.9% less per month than non-displaced children (see the row labeled "Percent effect").

[31] We do not impute zeros for individuals absent from the matched sample, and we retain zeros for those who reported zero earnings.

[32] CLP$1,000 corresponds to approximately US$1.50 in 2019.

In column (3) we drop slum attributes and include fixed effects of the deciles of the estimated propensity score $\hat{p}_s$. Compared to column (2), the results are very similar, both in levels and in percentage points. Finally, in column (4) we estimate the displacement effect from equation (1), where we fully saturate the model by incorporating the interactions between $\hat{p}_s$ and municipality-of-origin fixed effects, ψ_o. The coefficient of –14.038 on labor earnings implies that displaced children earn 8.9% less than non-displaced children when they are adults. It is also slightly smaller in absolute value than the coefficients in columns (2) and (3) and has a larger standard error, but it is still significant at the 5% level. 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 being relocated.

For comparison, all columns in Table 4 report Conley standard errors in brackets to account for any spatial dependence across slums that are close to each other (Conley, 1999).[33] The Conley standard errors deliver very similar results to clustering by slum of origin. Thus, in all of the following estimations, we report clustered standard errors.

Table 5 presents results on displacement's effect on employment and education outcomes. Panel A shows that as adults, displaced children are 6.8 percentage points more likely to work without a contract and 6.3 percentage points more likely to work in temporary jobs, which is 16.6% less and 11.4% more than non-displaced children, respectively. They are also 3.7 percentage points less likely to contribute to social security, which is 7.2% less than the non-displaced.

In Panel B we split self-reported earnings into formal and informal sources (with and without a contract). The results show that the negative effect observed in Panel A is due to lower earnings in the formal labor market (–16.2%), but the effect is positive on informal earnings (8.2%). However, these extra informal earnings do not compensate for the loss in formal earnings, and the total displacement effect is negative (column (4) of Table 4). In the last row of Panel B we include the displacement effect on taxable wages. We observe taxable wages from social security

[33] We use a 7-km cutoff distance to calculate Conley standard errors for all regressions. This distance is selected because it maximizes the standard errors for our main outcome, labor earnings, as shown in Table A.1. For estimating the standard errors, we consider different cutoffs ranging from 1 to 15 km. The upper bound is set to 15 km as this includes the largest municipality in Santiago in terms of square kilometers. We also report bootstrapped standard errors for comparison in the same appendix table.

contributions in the GRIS between 2016 and 2019, which, by definition, measure formal earnings. Consistent with the negative effect of displacement on formal self-reported earnings, we find an even larger effect of –21.6% on taxable wages, indicating that displaced children are not more likely than non-displaced children to underreport their earnings in the RSH.[34]

5.3 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 0.813 fewer years of schooling than non-displaced children. We find that the negative percent effect on outcomes increases with higher levels of education: displaced children are 21.6% less likely to graduate from high school, 31.3% less likely to attend a two-year college (for technical degrees such as mechanics and electrical technology), and 68.5% less likely to attend a five-year college (for professional degrees such as medicine, engineering, and economics). 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 almost all of the negative effect on labor earnings that we find in our sample. According to CASEN (2017), one extra year of education for those who finish high school increases their earnings by about 10%.[35] The displacement effect on earnings is –8.9%, while the effect on education is –0.81 years of education. Hence, the decrease in years of schooling accounts for all of the total effect on earnings.[36]

5.4 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 3). We find that across

[34] Discrepancies between reported earnings in the RSH and the GRIS can be attributable to several factors, such as underreporting, the timing of the report, or the cohort of children observed in the different datasets.

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

[36] We repeat this exercise using a mediation analysis, and our results are similar.

the entire age distribution, the income trajectories of displaced children are below those of the non-displaced, with a negative earnings difference already by age 26. Figure A.3 presents employment trajectories and displacement effects on formal and informal earnings separately. The results show that the negative effects are reflected in formal earnings and formal employment (with a contract), though for older ages, the difference in informality is reduced between displaced and non-displaced children.

5.5 Attrition and sample selection

In Section 4.4 we provided evidence of no selection on observables in the samples of children from both the archival data and the RSH. As discussed in Section 3, displaced children are more likely to be found in the RSH than non-displaced children. This variable itself can be viewed as an outcome given that individuals are more likely to enroll in the RSH to qualify for governmental benefits, which is consistent with our finding that displaced children have lower future earnings. However, a concern arises if this differential matching rate is driving the difference in children's labor earnings and not the displacement itself.

To show that differential attrition is not driving our results, we compute Lee bounds (Lee, 2009). In Table C.1 we compute lower and upper bounds for the displacement effect that replicate the models in Table 4 on total labor earnings, formal labor earnings, taxable wages, and education.[37] Based on the results, we do not find that differential attrition explains our results. In most cases, both the upper and lower bounds are negative and statistically different from zero, and they always contain the displacement effect for the corresponding sample.

While differential attrition from the archival sample to the RSH does not explain the displacement effect, it only addresses the potential displacement effect for the children found in the archives, not for all children in the program, as 39% of them were not found. If we believe that Morales and Rojas (1986)'s sample is the closest to the universe of slums, then in our archival sample displaced slums are over-represented.[38]

[37]Regular Lee bounds cannot be computed using controls. Therefore, to proceed with the estimation, we manually compute bounds by running each econometric model after dropping the differential displaced non-attriters in the upper and lower part of the outcome distribution, following McKenzie and Sansone (2019)'s procedure.

[38]In our sample we observe 58.6% of slums as displaced, while in Morales and Rojas (1986)'s

Thus, in a similar spirit to Lee bounds but applied at the slum level, we compute two estimates for the displacement effect under two extreme cases: 1) assuming that the unlocated slums fall within the upper part of the earnings distribution (or relevant outcome) of our children sample or 2) they fall within the lower segment of the distribution. The results, presented in Table C.2, show that our estimates are very similar in magnitude to the average displacement effect observed in our full estimation sample or are more negative.

5.6 Robustness checks

5.6.1 Variations to the propensity score method and subsamples

In this section, we show that our baseline results are robust to changes in the propensity score method and to different subsamples. Table A.2, columns (2) and (3) present robustness of the baseline results on earnings and education, even when the common support for the propensity score is reduced. Both the displacement effect in levels and in percentage terms are very similar to the baseline result in column (1). In column (4) we estimate our propensity score model by including municipality fixed effects in the propensity score function, and the results are robust. Finally, in column (5) we estimate the displacement effect by inverse propensity score re-weighting, and the conclusions remain the same.

We next examine if the displacement effect is robust to changing which municipalities are included in the sample. In Figure D.1 we drop municipalities one by one and find that our results are not driven by any particular municipality of origin nor destination. We are mainly interested in dropping the richest municipalities of origin since they were net expellers (i.e., expelled more families than they received) and might have seen the largest improvements in land prices after the forced evictions. However, 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.

sample, which is closer to the universe, 56% of slums are displaced. We are more likely to find larger slums, based on the number of families, and slums characterized by lower density and with closer proximity to the city center. This pattern holds true for both displaced and non-displaced slums. Based on our predicted densities of the propensity score, in our archival sample, displaced slums with a low probability of being relocated are over-represented compared to the universe.

5.6.2 *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 unobserved selection in our sample. For example, we do not observe other characteristics of slum families at baseline, such as their relationship with local authorities or the difficulties they faced when leaving 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 several 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.[39] Table D.1 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 find that 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 D.1). Finally, permutation tests show no evidence of selection (see Figure D.2).

5.7 *Displacement effect by age at intervention*

The displacement effect 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 is known as a *childhood exposure effect* of neighborhoods, meaning that the longer a child spends in a new environment, the larger the expected neighborhood

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

effect. 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, stratifying our sample by age at intervention into three groups: 0–5, 6–12, and 13–18 years.[40] We find evidence of an exposure effect on labor income, driven by formal earnings. Specifically, Figure 4, panels (a) and (b) show that the displacement effect on formal self-reported earnings and taxable wages is more negative for children under 13 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 of an exposure effect, as teenagers face a null effect, while younger displaced children experience a positive effect (though we cannot reject the equality of coefficients). Finally, panel (c) takes advantage of our dataset's panel structure to plot displacement effects across the age cycle, confirming our aggregate findings: teenagers experience a less negative effect on earnings during adulthood, though the effect remains negative. For children under 13 years old at baseline, the displacement effects become more negative with age.

The results confirm the established concept of neighborhood exposure effects. The richness of our data allows us to differentiate these effects by types of earnings, revealing that the negative exposure effect predominately influences children's future formal earnings. In addition, the negative displacement effect on formal earnings, coupled with the null effect on informal earnings, for teenagers suggests that the disruption effects of relocation are not negligible for this age group. In Section 6, we explore whether these earnings patterns have different causes by age at baseline.

5.8 Displacement effect by demographic groups

While the displacement effects by demographic group may vary (Figure A.2), we do not find systematic large differences across other demographic characteristics. We do find more negative displacement effects on earnings for men and indigenous children, but we cannot reject differences between groups. Finally, we find evidence of a more

[40] We choose these three groups after performing a structural break test for each age from 0 to 18, aiming to detect any changes in the slope at each individual age. F-tests suggest a break in labor earnings and taxable wages at age 13 or 14, and another break in years of education between ages 5 and 6. See Appendix Figure A.4 for more details.

negative effect for children from slums where adults had lower unemployment rates, measured by formal employment before treatment, their earnings, and education.

6 Mechanisms

In this section we investigate the mechanisms behind our baseline results on earnings. Based on families' impressions after relocation and the lower-quality attributes of destination neighborhoods, we study which changes in neighborhood attributes explain the average displacement effect on earnings. We then examine the current locations of children in our sample, followed by exploring whether improvements in the transportation system reduce the displacement effect.

6.1 Improvements in the comparison group

We start our analysis of mechanisms by investigating 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 rather a positive effect on the comparison group.[41]

To test this hypothesis, 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. We hypothesize that the former group would experience more significant improvements in neighborhood quality if the cleared areas were rebuilt. Table 6 shows that non-displaced children living within 1, 1.5, or 2 kms of a displaced slum earn more as adults relative to non-displaced children without nearby displaced slums, though the differences are small and not statistically different than zero. More importantly, including these results does not greatly change the observed negative effects on the displaced children.

[41] 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. Data on land value by neighborhoods show that cleared areas saw a larger increase in land value across time after the treatment, compared to redeveloped and relocation areas. See Figure A.7.

The previous exercise serves two purposes. First, it tests for the existence of spillover effects on the comparison group; although detected, the results are noisy. Second, because the displacement effect remains negative and significant after splitting the comparison group into two, we cannot attribute the entire difference between displaced and non-displaced children to improvements in the outcomes of the non-displaced. Instead, it may be related to the characteristics of the destination locations to which families were relocated.

6.2 Attributes of destination locations

Given that destination municipalities were poorer on average, we study which characteristics of the new locations or projects are most relevant in explaining the variation in 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.5 presents the distribution of the estimates on labor earnings, showing variation by municipality. In most cases, the displacement effect is negative.

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 families in the relocation processes leads us to believe that the assignment was as good as random, as they stated that the MINVU assigned families to locations based on unit availability. To provide quantitative evidence for this, we test whether family demographics predict the attributes at destination. We run regressions of several location attributes on a set of family demographics (Table A.4) from our sample of families who moved, and we do not find evidence that family characteristics predict their final destinations.

Figure 5 presents the results of correlating the displacement effect with changes in attributes. Panel (a) shows that children who were sent farther away from their original locations face a more negative displacement effect. This is a pattern established in previous work for adults (Barnhardt et al., 2016; Picarelli, 2019). 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 social networks (panel (b)), and with the change in log of property prices in surrounding areas at destination (panel (g)). We also find a small correlation between families' home values and the number of schools in the new areas.

Finally, we find a small positive correlation between earnings and neighborhood size, mainly driven by housing projects with over 1,000 units (panel c). This last result challenges the theory of overcrowded neighborhoods having a negative impact due to worse infrastructure and higher density (Newman, 1973). However, for projects with fewer than 1,000 units, the correlation is negative. This may indicate non-monotonic effects of size or omitted variable bias, as size could correlate with other neighborhood attributes like property prices or distance to urban centers, especially as in the context of this paper, larger social housing projects were built in cheaper areas and farther from the city center.

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. To explore which of the changes are most relevant, in Table 7 we investigate how the displacement estimate decreases when location changes are included. Column (1) shows our baseline results on labor earnings, and column (2) controls for determinants of the displacement itself (project size, network share, and distance from origin).[42] The results indicate that project size and distance from origin negatively correlate with earnings, but only project size is statistically different from zero. Moreover, the network share, measured as the fraction of slum families from the original slum in the destination neighborhood, positively correlates with earnings. By including these determinants, the negative displacement effect decreases from –8.9% to –1.2%, implying that these variables explain 87% of the average displacement effect on children's future earnings.

In column (3) we repeat the exercise but add changes in neighborhood attributes (schools, unemployment rate, property prices, and distance to the CBD) as controls.

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

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. However, the correlation with the unemployment rate is positive and small. By including these neighborhood changes, the displacement effect decreases by 0.1 percentage points, suggesting that these changes explain very little of the variation in the displacement effect on earnings in our sample.

In column (4) we combine all the changes and find very similar results to those in column (2). The determinant that is reduced the most is distance from origin, which is expected since it correlates with the changes in neighborhood characteristics, especially distance to the CBD. Additionally, the change in the unemployment rate shows the expected negative correlation with children's future earnings. Overall, all these determinants explain 88% of the displacement effect.

We began our analysis by showing that the homes received by the displaced families were 12% cheaper than those received by non-displaced families, due to being built in cheaper areas of the city and farther from the city center. Given that home value correlates with the changes in attributes experienced by families, in column (5) we control for it. We find that home value positively impacts earnings, and compared to column (4), most coefficients remain similar in magnitude. However, distance from origin changes sign and becomes noisier.

We use the results in columns (4) and (5) to conduct an accounting exercise that decomposes the displacement effect by determinant, following the procedure proposed by Gelbach (2016). This procedure states that the total displacement effect in column (1) is the sum of the contributions arising from each of the neighborhood attributes we consider in column (5) and from a residual contribution not captured by neighborhood changes. The observed contribution of each attribute is obtained by multiplying the corresponding coefficient in column (5) with the corresponding change in attribute due to displacement, that is, the correlation between the displacement dummy and the neighborhood attribute. We report these auxiliary correlations in column (6) of Table 7 and the decomposition results in Figure 6.

The results in Figure 6 show that more than 60% of the displacement effect may be associated with variations in project size and the disruption of families' original slum networks. Before controlling for home value, both distance from origin and distance to the CBD explain around 18% of the total displacement effect on earnings, and home value explains 28% of the total effect. The changes in neighborhood characteristics such as schools, unemployment, and prices explain a very small portion of the displacement effect, in part due to the low variation of these variables in our sample, as shown in the auxiliary regressions (column (6) of Table 7).

6.3 Changes in neighborhood attributes by age at intervention

We previously showed an age gradient in the exposure effect of displacement, especially on formal earnings. In this section, we further explore whether an age gradient exists in the determinants of the displacement effect. To do so, we run regression (5) in Table 7, stratified by age group at baseline (0–5, 6–12, 13–18). Figure 7 reports the coefficients for each location attribute and age group (equivalent to column (5) of Table 7). We find an age gradient in the main determinants of the displacement effect: larger destination projects affect younger children more than adolescents, and a higher home value benefits children below 13 years of age the most. Relocating families with their entire slum network benefits all children in our sample, though the confidence intervals are wide. Finally, while more schools and lower unemployment rates benefit younger children the most, the standard errors are large, and thus we cannot reject the equality of coefficients.

6.4 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 these children currently live. We start by examining the likelihood of the parents in our sample remaining in their assigned neighborhoods. We estimate a displacement effect on current locations between 2017 and 2019 as well as on the poverty rate of these neighborhoods.[43]

Table 8, Panel A shows that compared to non-displaced parents, displaced parents

[43]The RSH reports location data at the neighborhood level for a random sample of individuals, with about 40% of the observations including a current location at this granularity. However, the data are only considered reliable after 2017.

are not less likely to live in their assigned municipality (column (1)) but are less likely to live in their assigned neighborhood (column (2)). Even though this last estimate is not statistically significant, it is sizable (–35%). They are also less likely to live in their municipality of origin (column (3)), and if they move within Greater Santiago, they live 1.7 kms farther away but within 5 kms, indicating they probably live in neighboring municipalities. In terms of poverty rates, displaced parents' current neighborhoods are 6% poorer than those of non-displaced parents. Additionally, while displaced parents are less likely to live in their assigned neighborhoods, they are not more likely than non-displaced parents to sell their homes (see Appendix Table A.6).[44]

We continue the analysis by examining the current locations of children, now adults, in Panel B. We find that children are less likely to live in their originally assigned neighborhood compared to their parents. Only 44% live in their assigned municipality, and fewer than 30% remain in their assigned neighborhood. The differences are large but not statistically significant, most likely because we are underpowered given the missing information in these variables. Additionally, displaced children live 1.9 kms farther away from their parents' neighborhood of assignment and live in areas that are 7% poorer. In Panel C we study these patterns by age at baseline and find no systematic differences by age at intervention. All children, regardless of their age, are more likely to live in higher-poverty areas as adults (column (5)).

We next explore the influence of transportation improvements on the displacement effect. Given that some children remain in their assigned neighborhoods or not very far from them, improvements in public transportation may 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 2010 and 2019.[45] We analyze whether the construction of a new station close to families' assigned locations impacts displaced and non-displaced children differently. We exploit the timing and the location of the new subway stations to estimate the effects using an event-study approach, interacted

[44]We examine the probability of parents selling their homes, using data on 40% of families assigned to neighborhoods in the northern areas of Santiago. The results, shown in Table A.6, indicate that 6% of these families sold their house by 2019, after an average 25 years, with no statistical differences between displaced and non-displaced families. The sample consists of 40% of families found in the archives who received a house in the northern areas of Santiago. We partnered with Santiago's Real Estate Registrar to track families' addresses in our archival data.

[45]Three new lines were introduced during this time period, in 2010, 2011, 2017, and 2019. See the maps in Figure A.8 for the geographic variation.

with displacement. These results are only suggestive as the location of new subway stations is not random.

Figure 8 presents the results. Panel (a) shows that constructing a subway station within 1.5 km of their parents' assigned neighborhood leads to an improvement in displaced children's future earnings, primarily due to increases in formal earnings and decreases in the probability of being a temporary worker (panel (b)). The changes result in a 65–100% reduction in the negative displacement effect on earnings for children near these new subway stations (see Table A.7).[46] This results is consistent with recent literature studying the effects of transportation infrastructure on informality (Zárate, 2024).

7 Total displacement effect on children and discussion

7.1 *Total earnings lost due to displacement*

We use the age estimates on earnings presented in Figure 3, 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%,[47] by the age of 52, the average displaced child in our sample loses CLP$7 million (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 in our sample (equivalent to US$10,103).[48] 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.[49] 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

[46]Because we exploit late improvements to the subway infrastructure, we cannot rule out larger effects of new subway infrastructure before 2007. The largest improvement in the subway system occurred at the beginning of the 2000s, when Line 4 was built. This line connected the south of Santiago with the CBD, where many of the housing projects we study were built.

[47]We use an annual discount rate of 4%, which is comparable to the yield on 10-year Chilean government bonds at the end of 2018.

[48]Using taxable wages, the loss is larger and equal to US$26,000 by the age of 52. However, this does not include the positive effect on informal earnings.

[49]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.

displacement on schooling and its externalities, such as increased criminal activity.

7.2 Comparison of estimates with other settings

Our results show that relative to non-displaced children, displaced children have 0.81 fewer years of education, earn 9% lower income, and are 17% more likely to work in the informal labor market. Our setting is unique, occurring in a developing country where families are moved to high-poverty areas. This complicates comparisons with other studies, which typically examine movements from high- to low-poverty areas.

With these caveats in mind, we compare the magnitude of our estimates with other studies by computing an elasticity defined as the percentage change in earnings when neighborhood quality changes by 1%. The results, presented in Table A.8, indicate that the implied earnings elasticity in our setting ranges from 0.6 to 0.75. This is very similar to the elasticity reported by Chyn (2018) (0.72) and larger than that reported by Chetty et al. (2016) (0.41). It is also in the range of the implied estimate by Barnhardt et al. (2016) for India (when neighborhood quality is measured as urbanicity). However, the primary distinction of our study is its focus on long-term outcomes for children as they transition into adulthood, rather than immediate circumstances.

Our results also imply larger elasticity estimates on schooling outcomes and are very similar to previous studies in developing contexts. For example, in developed contexts, previous research finds minimal effects on high school completion rates but large effects on college enrollment. Our findings are similar (in negative terms) to those of Camacho et al. (2022) for Colombia, who find that children from families who win a housing lottery to move to better areas have a 17% higher probability of graduating from high school, with an implied elasticity of 1.73. Our implied elasticities for the same outcome range between 1.44 and 1.82, while studies like Chyn (2018) have an implied elasticity of 0.36 for high school graduation but 1.26 for college attendance.[50] The children in our sample have lower educational attainment but not lower employment. The total effect on earnings is mediated by lower education that

[50] If the average return on completing high school is lower than that of attending college, this could explain the variations we observe in earning outcomes. Specifically, we see a 10% return in Chile, compared to the 16% return implied by the estimates of Chyn (2018).

is compensated by higher informal earnings.

7.3 *From slums to poor neighborhoods: Trap or stepping stone?*

Previous literature on slums suggests that slum dwellers are caught in a poverty trap due to the additional burdens of living in such environments, including poor health outcomes, limited access to financial and labor markets, and restricted access to services (Marx et al., 2013). In our context, families are relocated from slums to public housing, raising the question of what are the consequences of moving to a poor neighborhood. While we cannot rule out positive the 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, the loss in earnings is not offset by the value of the housing asset received.

Our results on current locations also show that families do not necessarily escape a poverty trap but the opposite, as parents are likely to remain in their assigned neighborhoods, perhaps because they became homeowners. Additionally, even though displaced children are more likely to move, the neighborhoods they move to have higher poverty than those of non-displaced children. This sheds light on the long-term consequences of relocating families and children to remote areas that perpetuate poverty traps, especially as they spend more time in less favorable environments. Thus, consistent with previous research, the Program for Urban Marginality transformed the issue from homelessness to one of poor public housing (Aravena and Sandoval, 2005).

8 Conclusion: Policy alternatives

This paper presents new evidence on the long-term impact of displacement and growing up in high-poverty neighborhoods. 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 that displacement negatively impacts young children and teenagers.

Our results also show that forcing families to relocate negatively affects children, as their new neighborhoods are of low quality.[51] One policy alternative to relocating families to the periphery could be to provide on-site housing (UN-Habitat, 2020). However, this may not always be feasible due to factors such as high urban density, which impedes public housing construction, the high price of land, or the challenges of providing essential services on-site (running water, electricity, sewage). Under these conditions, monetary compensation for displacement could be an option (Lall et al., 2006), though determining compensation amounts may be challenging. Thus, if displacement remains the only solution, a more effective policy would be to directly provide families with public services. Furthermore, measures should be implemented to minimize the disruption to families and children, such as preserving social networks to keep communities together, providing support for the challenges associated with transitioning to formal housing, and involving communities in the eviction process.[52]

Finally, an important aspect of our setting is that families were forced into locations that often turned out to be poverty traps—potentially worse than their original slums—resulting in increased segregation, reduced mobility, and negative effects on children's economic development. Our paper contributes to understanding the implications of these policies on individuals. However, due to the scope of these programs, future research should consider the general equilibrium effects of slum relocation on neighboring individuals, their communities, and the efficacy of compensation schemes.

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[51] A valid question is whether the program was beneficial for families overall. However, answering this requires understanding the impact of slum upgrading on children, which is beyond the scope of this paper.

[52] See research by the World Bank.

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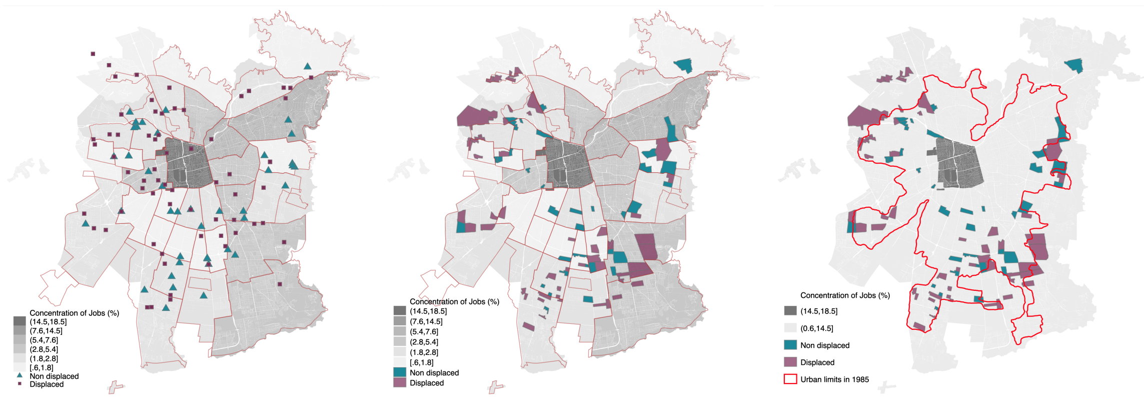
Figures and Tables

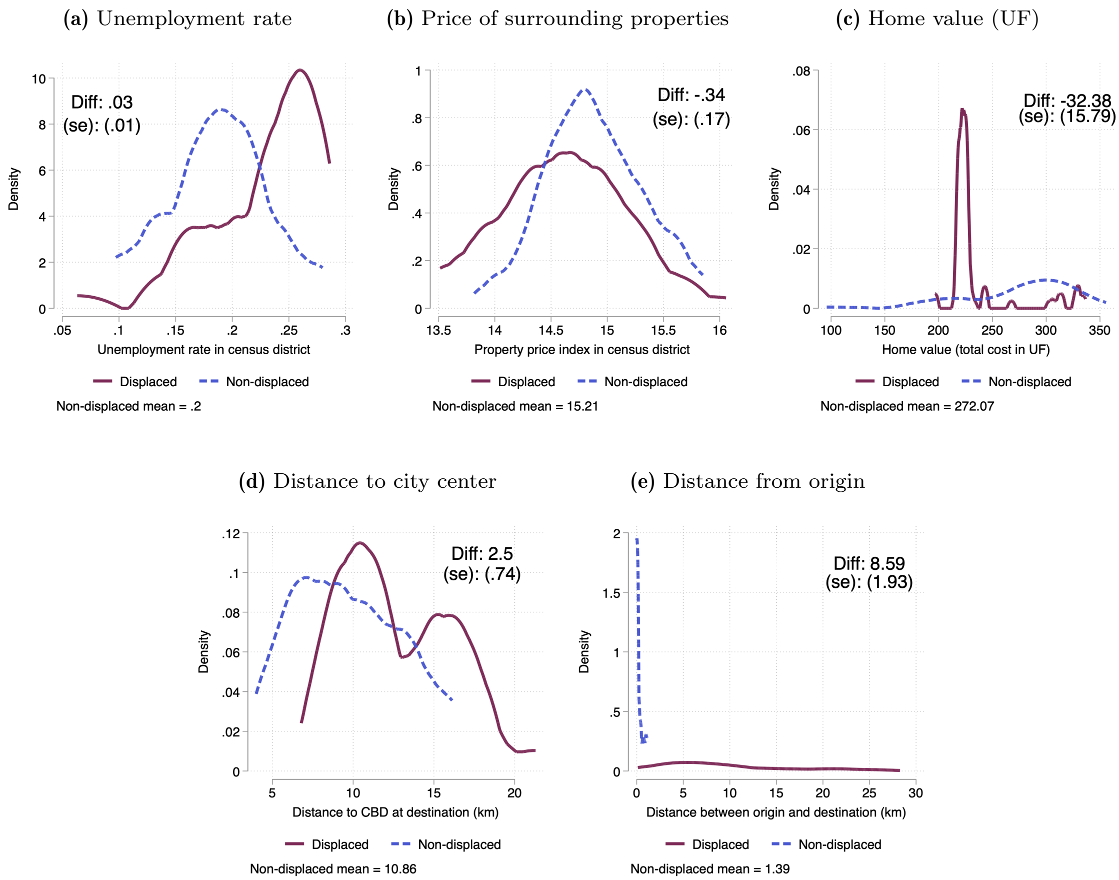
Figure 1: Eviction policies 1979–1985: Locations of families living in slums



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

Notes: The figure shows changes in the locations of families living in slums in 1979 (panel (a)) and their final destinations in 1985 (panels (b) and (c)). Red lines represent the urban limits of Greater Santiago, and municipalities are colored in gray scale to depict the concentration of jobs across the city. Purple squares represent families living in slums who were moved out from their original location to a new neighborhood, while blue triangles represent those in slums who were not evicted but received a housing unit in their original location. The figures also show that post-policy, the dispersion of the locations of these families decreases and they are relocated to the city's periphery. For context, the richest municipalities of Santiago at that time (and today) are those located in the northeast of the map and poorer municipalities in the south and northwest, which is exactly where the new public housing projects were built. The data used to construct this map come from MINVU (1979), Molina (1986), Benavides et al. (1982), Morales and Rojas (1986), and the population censuses of 1982 and 1992.

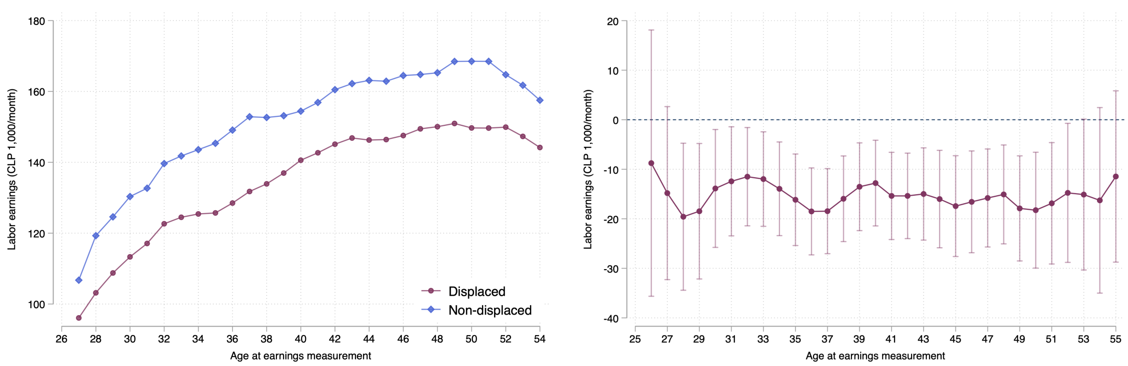
Figure 2: Density of neighborhood attributes after relocation



Notes: The figure shows densities by treatment for the average neighborhood attributes for each pair of slum of origin and project of destination in the archival sample (N = 110 unique pairs of slum-project of destination). Each subfigure's footnotes indicate the mean difference between treatments for all households in the sample, conditional on the propensity score (N = 15,613). We compute the average for all households within the common support of the sample regardless of whether a child is present.

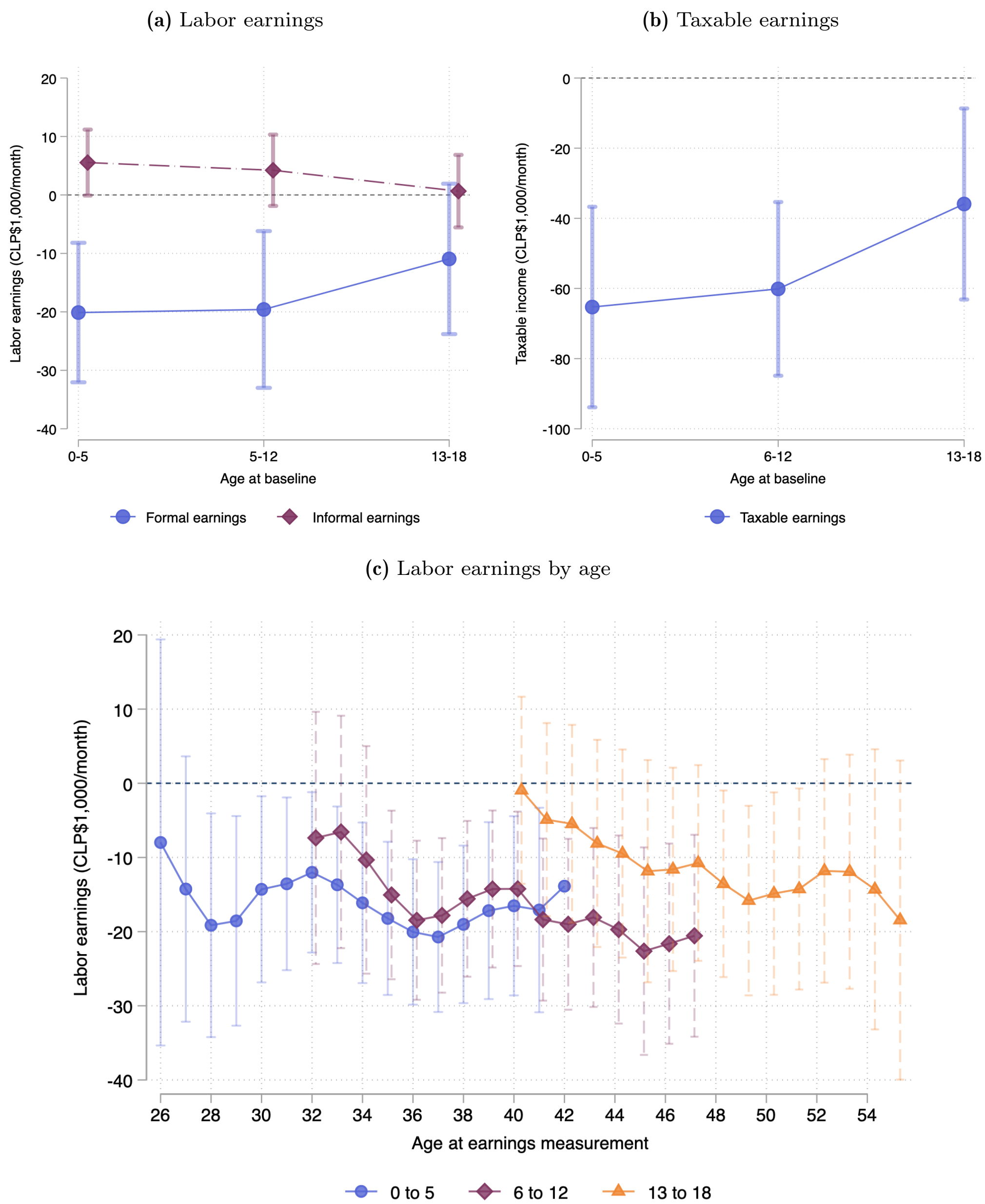
Figure 3: Displacement effects on labor market outcomes by age at earnings measurement: Children aged 0 to 18 at baseline

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



Notes: The figure shows regressions for children aged 0 to 18 at baseline who are matched to the RSH data. Panel (a) plots the predicted trajectories for displaced and non-displaced children between ages 27 and 55 from the regression $y_{it} = \sum_{\tau=27}^{55} \beta_\tau Displaced * 1[Age = \tau] + \sum_{\tau=26}^{55} \delta_\tau 1[Age] + \psi_o + \hat{p}(X_s) + \hat{p}(X_s) \times \psi_o + X'_{it}\gamma + u_{it}$. Panel (b) plots coefficients β_τ and their 95% confidence intervals, and other outcomes can be found in Figure A.3. Standard errors are clustered by slum of origin. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, Mapuche last name, firstborn dummy, head of household formal employment, year-of-birth fixed effects, and year-of-intervention fixed effects.

Figure 4: Displacement effects on earnings by age at baseline



(a) Labor earnings

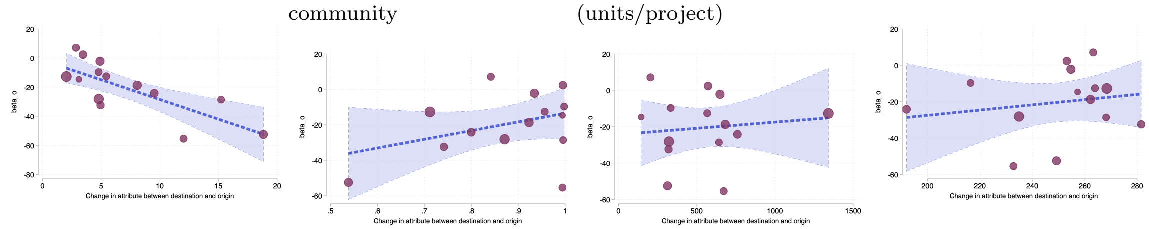
(b) Taxable earnings

(c) Labor earnings by age

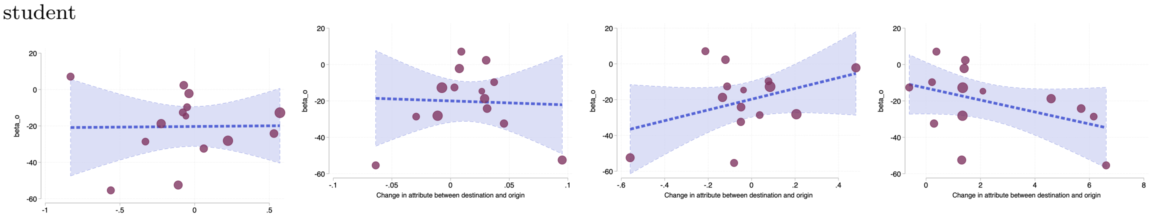
Notes: The figure shows regressions for children aged 0 to 18 at baseline who are matched to the RSH data. Panels (a) and (b) plot coefficients β_τ and their 95% confidence intervals from regression (1) stratified by age group, and panel (c) plots coefficients $\beta_{\tau g}$ and their 95% confidence intervals from $y_{it} = \sum_{g=1}^{3}\sum_{\tau=26}^{55}\beta_{\tau g} Displaced * 1[Age = \tau, Group = g] + \sum_{g=1}^{3}\sum_{\tau=26}^{55}\delta_{\tau g}1[Age = \tau, Group = g] + \psi_o + \hat{p}(X_s) + \hat{p}(X_s)\times\psi_o + X_{it}'\gamma + u_{it}$, where g stands for an age group in [0,5], [6–12], or [13–18] at the time of the intervention. Standard errors are clustered by slum of origin. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, Mapuche last name, firstborn dummy, head of household formal employment, year-of-birth fixed effects, and year-of-intervention fixed effects.

Figure 5: Relationship between displacement effect and changes in location attributes

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

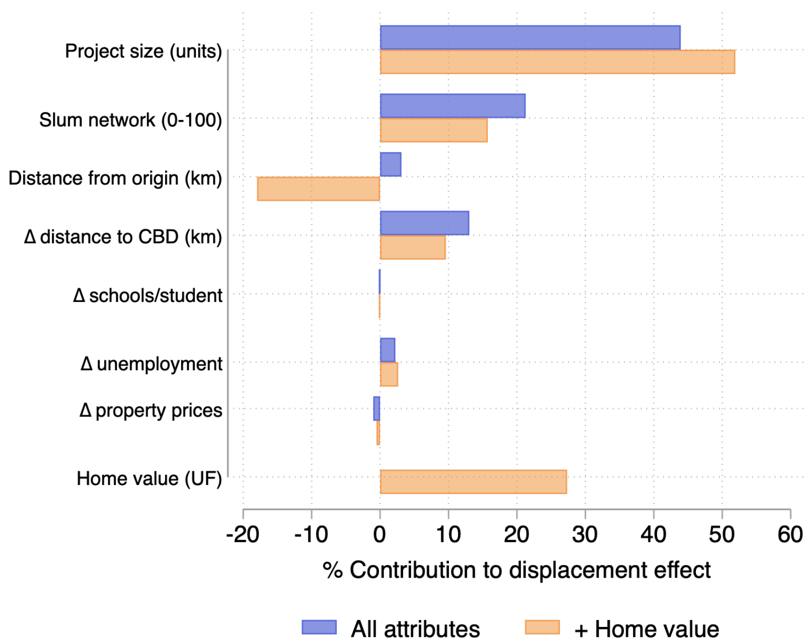


(e) Δ number of schools per student **(f)** Δ unemployment rate **(g)** Δ log(property prices) **(h)** Δ distance to CBD



Notes: The figures plot displacement coefficients on self-reported labor income, stratified by municipality of origin (Figure A.5), against changes in location attributes at destination averaged by municipality of origin. These regressions are for children who were 0 to 18 years old at baseline and matched to the RSH data from 14 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, Mapuche last name, firstborn dummy, head of household formal employment, year-of-birth fixed effects, and year-of-intervention fixed effects. Correlations are weighted by the number of observations in each cell (number of children in the sample in each municipality of origin).

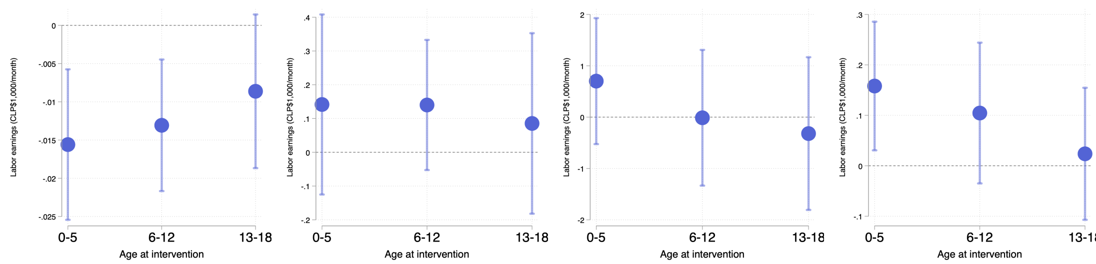
Figure 6: Decomposition of the displacement effect



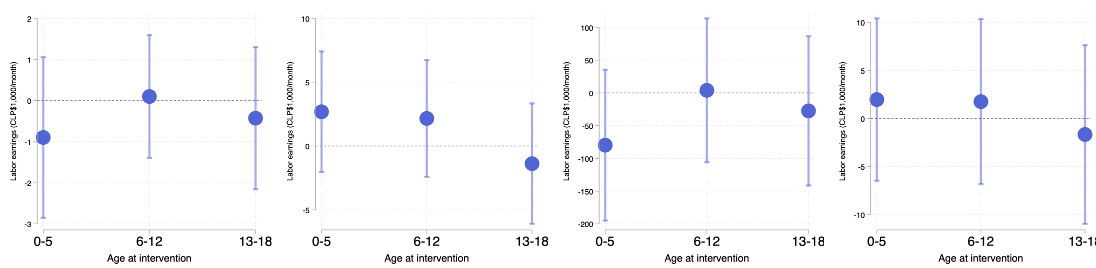
Notes: The figure plots a decomposition of results using the Gelbach (2016) method, based on columns (4)–(6) of Table 7.

Figure 7: Mechanisms of displacement effect on earnings by age at intervention

(a) Project size **(b)** % original slum **(c)** Distance from origin **(d)** Home value



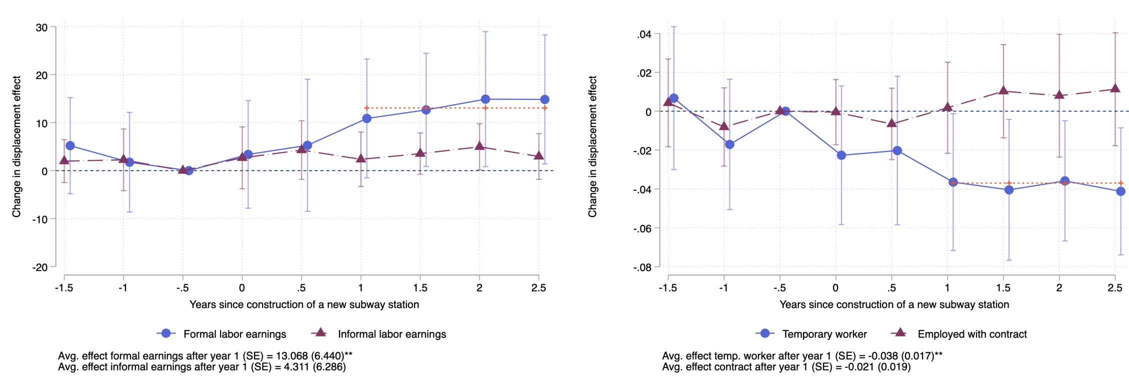
(e) Δ distance to CBD **(f)** Δ # schools/student **(g)** Δ Unemployment **(h)** Δ log prices



Notes: The figures plot equivalent coefficients from column (4) of Table 7 and their 95% confidence intervals for self-reported labor earnings, stratified by age groups at baseline ([0,5], [6–12], and [13–18]). These regressions are for children who were 0 to 18 years old at baseline and matched to the RSH data. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, Mapuche last name, firstborn dummy, head of household formal employment, year-of-birth fixed effects, and year-of-intervention fixed effects. Standard errors are clustered by slum of origin.

Figure 8: Change in displacement effect due to subway access

(a) Labor earnings (CLP$1,000/month) **(b)** Type of employment



Notes: Each coefficient and its 95% confidence interval in panels (a) and (b) correspond to the estimates of γ_2 from regression $Y_{it} = \alpha + \beta Displaced_{s\{i\}} + \gamma_{1\tau} Subway_{\lambda\{\tau\}} + \gamma_{2\tau} Displaced_{s\{i\}} \cdot Subway_{\lambda\{\tau\}} + \psi_o + X_i'\theta + \delta_t + \varepsilon_{it}$. Treatment corresponds to the variable $Subway_{\lambda\{\tau\}}$, defined as distance to the subway below 1,500 meters, and τ corresponds to years relative to the subway station opening. Table A.7 reports average effects for the post-period, and Figure A.9 presents these effects for the treatment defined as distance to the subway below 2,000 meters. Clustered standard errors by slum of origin are in parentheses.

Table 1: Slum characteristics before intervention

	Displaced mean (1)	Non-displaced mean (2)	Difference (3)	Difference within municipality (4)	Difference conditional on propensity score (5)
Panel A. Slum attributes					
Families/hectare	81.96	68.42	13.55	9.47	0.04
			(11.02)	(12.67)	(11.33)
Military name	0.11	0.17	-0.06	-0.04	0.00
			(0.05)	(0.05)	(0.05)
Elevation (mas)	581.51	582.00	-0.49	-14.43***	-9.45
			(12.34)	(4.80)	(13.58)
Slope (degrees)	3.07	2.55	0.52**	0.26	-0.18
			(0.26)	(0.24)	(0.25)
Close to river/canal (<100 m)	0.06	0.03	0.04	0.01	0.00
			(0.03)	(0.03)	(0.03)
Flooding risk	0.09	0.02	0.07**	0.03	0.00
			(0.03)	(0.02)	0.02
Distance to CBD	9.93	10.39	-0.47	-0.41	-0.05
			(0.61)	(0.39)	(0.70)
Panel B. Census district attributes					
Population's schooling	7.89	7.09	0.80***	0.32*	-0.09
			(0.25)	(0.19)	(0.17)
Unemployment rate	0.18	0.21	-0.02***	-0.01*	0.00
			(0.01)	(0.01)	(0.01)
Number of schools	3.96	4.54	-0.58	-0.40	-0.11
			(0.46)	(0.39)	(0.46)
Log surrounding prices	14.80	14.72	0.08*	0.03	-0.03
			(0.04)	(0.02)	(0.04)
Number of slums	92	130	222	222	190
Number of municipalities	14	14	14	14	14

Notes: The table shows summary statistics for non-displaced (redeveloped) and displaced (relocated) slums in Morales and Rojas (1986)'s sample with non-missing attributes or locations. Slum locations and characteristics are constructed from Benavides et al. (1982), Morales and Rojas (1986), MINVU (1979), newspapers, and the Population Census of 1982. Elevation, slope, and flooding risk data are obtained from Geoportal. Prices, unemployment, number of schools, and population's schooling are measured at the census district level where a slum was located. Column (3) reports the simple difference in each attribute between displaced and non-displaced slums, column (4) shows the difference between groups within municipalities of origin, and column (5) shows the difference between displaced and non-displaced slums controlling for the propensity score at the slum level in the sample with common support (see text for explanation of how the propensity score is estimated). Robust standard errors are in parentheses. 10%*, 5%**, 1%***.

Table 2: Summary statistics for children aged 0 to 18 at baseline

	Full sample (1)	Common support (2)	In RSH (2007-2019) (3)	P(in supp) (4)	P(in RSH) (5)
Demographics at intervention					
Displaced	0.694	0.680	0.695	-0.019	0.059***
	[0.461]	[0.467]	[0.460]	(0.056)	(0.008)
Female	0.503	0.504	0.544	0.000	0.126***
	[0.500]	[0.500]	[0.498]	(0.002)	(0.005)
Age	8.141	8.189	8.133	0.001*	-0.003****
	[4.864]	[4.856]	[4.856]	(0.001)	(0.001)
No. children	3.841	3.834	3.880	-0.003*	0.010***
	[1.795]	[1.779]	[1.794]	(0.002)	(0.002)
Firstborn	0.368	0.366	0.357	-0.009**	-0.013**
	[0.482]	[0.482]	[0.479]	(0.004)	(0.005)
Oldest sibling	11.620	11.663	11.692	0.000	0.000
	[5.805]	[5.734]	[5.744]	(0.001)	(0.001)
Youngest sibling	5.172	5.207	5.156	-0.001	0.000
	[4.219]	[4.217]	[4.201]	(0.001)	(0.001)
HH age	34.737	34.777	34.782	0.000	0.000
	[7.120]	[7.063]	[7.083]	(0.000)	(0.000)
Female HH	0.330	0.334	0.331	0.007	-0.010
	[0.470]	[0.472]	[0.470]	(0.009)	(0.007)
Married HH	0.798	0.801	0.802	0.031**	-0.012
	[0.402]	[0.399]	[0.398]	(0.011)	(0.008)
Cohabit HH	0.091	0.091	0.093	0.032*	0.002
	[0.288]	[0.288]	[0.291]	(0.018)	(0.011)
Single HH	0.111	0.109	0.104		
	[0.314]	[0.399]	[0.305]		
Mapuche last name	0.086	0.087	0.091	0.001	0.034***
	[0.280]	[0.281]	[0.287]	(0.007)	(0.007)
HH's formal employment	0.395	0.392	0.390	-0.794*	-0.182**
	[0.079]	[0.081]	[0.080]	(0.405)	(0.068)
Variables measured after 2007					
Died before 2007	0.006	0.006	-	0.007	-0.819***
	[0.075]	[0.074]	-	(0.014)	(0.011)
Mother's schooling[a]	5.921	5.936	5.836		
	[3.402]	[3.402]	[3.362]		
Mother's schooling unknown	0.080	0.079	0.063		
	[0.272]	[0.268]	[0.243]		
Mother is in the RSH	0.859	0.858	0.872		
	[0.348]	[0.349]	[0.334]		
Father is in the RSH	0.660	0.663	0.678		
	[0.474]	[0.473]	[0.467]		
# times in the RSH	16.389	16.339	20.021		
	[9.803]	[9.816]	[6.665]		
Individuals	33,669	30,680	25,032	33,669	30,680
Families	13,732	12,448	11,466		
Number of slums	99	90	90	99	90
Matching rate rel. to (2)			81.6%		

Notes: The table shows summary statistics for children aged 0 to 18 at baseline. Column (1) reports summary statistics for the full sample of children from archival records, column (2) for children in slums in the common support, and column (3) for children matched at least once to the RSH data in slums in the common support. Column (4) estimates a linear regression of the probability of being in the common support (column (1) relative to (2)) on a full set of demographics at baseline, treatment (displacement), probability of dying before 2007, and municipality-of-origin fixed effects. Column (5) estimates a linear regression of the probability of being found in the RSH (column (3) relative to (2)) on the same set of covariates. Standard errors are clustered by slum of origin in parentheses, and standard deviations are in brackets. Adjusted R^2 for regressions in columns (4) and (5) are 0.247 and 0.063, respectively.

[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 the year 2007. 10%*, 5%**, 1%***.

Table 3: Comparing displaced and non-displaced children aged 0 to 18 at baseline

	All children 0 to 18 in common support			Children matched to the RSH		
	Non-displaced mean	Displaced mean	Conditional difference	Non-displaced mean	Displaced mean	Conditional difference
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Demographics						
Female	0.499	0.507	-0.004	0.540	0.546	-0.007
			(0.011)			(0.008)
Age	8.248	8.131	0.164	8.271	8.043	0.116
			(0.413)			(0.400)
Firstborn	0.365	0.366	-0.018	0.352	0.360	-0.013
			(0.018)			(0.019)
No. children	3.773	3.865	0.177**	3.863	3.886	0.099
			(0.070)			(0.075)
Oldest sibling	11.644	11.614	0.488	11.795	11.584	0.490
			(0.523)			(0.542)
Youngest sibling	5.306	5.141	-0.090	5.273	5.088	-0.055
			(0.339)			(0.329)
HH age	35.107	34.523	0.025	35.194	34.495	0.055
			(0.720)			(0.707)
Mother age	33.457	32.859	-0.191	33.430	32.814	-0.157
			(0.575)			(0.546)
Father age	35.518	35.145	0.430	35.616	35.120	0.516
			(0.742)			(0.764)
Female HH	0.303	0.341	-0.049	0.302	0.336	-0.054
			(0.050)			(0.054)
Married HH	0.846	0.783	-0.018	0.849	0.786	-0.014
			(0.012)			(0.015)
Cohabit HH	0.081	0.093	0.006	0.079	0.096	0.011
			(0.013)			(0.014)
Single HH	0.073	0.124	0.012	0.073	0.118	0.003
			(0.014)			(0.014)
Mother age at first birth	24.762	24.277	-0.253	24.804	24.314	-0.160
			(0.209)			(0.205)
Mapuche last name	0.073	0.093	0.009	0.078	0.096	0.007
			(0.009)			(0.011)
HH formal employment[a]	0.431	0.377	0.022	0.429	0.376	0.023
			(0.026)			(0.025)
Mother's schooling[b]	6.211	5.834	-0.022	6.044	5.783	0.076
			(0.200)			(0.221)
Child mortality last 5 years[c]						
below 28 days	0.006	0.004	-0.002	0.007	0.004	-0.001
			(0.001)			(0.002)
below 1 year	0.019	0.015	0.004	0.019	0.016	0.006
			(0.004)			(0.004)
Children	9,823	20,857	30,680	7,632	17,400	25,032
Families	4,009	8,439	12,448	3,564	7,902	11,466
Slums	39	52	90	39	52	90
Municipalities		14			14	

Notes: Column (1) reports means for non-displaced children at baseline and column (2) for displaced children. Column (3) reports the difference between groups, adjusted by the probability of slum clearance ($\hat{p}_s \times \psi_o$) in the full sample of children aged 0 to 18 at baseline from families found in the archival sample and in the common support of the propensity score. Columns (4)–(6) repeat the exercise for children found in the RSH. Standard errors are clustered by slum of origin in parentheses. 10%*, 5%**, 1%***. [a]Household's formal employment is measured at the slum level using historical data from the Superintendence of Pensions. [b]Mother's years of schooling is observed in the sample of mothers found in the RSH and is conditional on a mother being alive after the year 2007. [c]Child mortality measures whether a mother in the sample had a child born alive who died in the first year, in the five years before treatment.

Table 4: Displacement effect on labor income and employment

Panel A.	Outcome: Self-reported earnings (CLP$1,000/month)			
	(1)	(2)	(3)	(4)
Displaced	-17.743	-16.011	-16.315	-14.038
	(3.424)***	(3.505)***	(3.619)***	(5.384)**
	[4.036]***	[3.934]***	[3.973]***	[5.145]***
Non-displaced mean	161.995	161.995	158.300	158.300
Percent effect	**-10.9**	**-9.9**	**-10.3**	**-8.9**
Adjusted R^2	0.122	0.122	0.123	0.124
Panel B.	Outcome: 1[Employed]			
Displaced	-0.007	-0.010	-0.005	-0.015
	(0.007)	(0.008)	(0.008)	(0.012)
	[0.009]	[0.009]	[0.009]	[0.013]
Non-displaced mean	0.647	0.647	0.641	0.641
Percent effect	**-1.1**	**-1.5**	**-0.8**	**-2.3**
Adjusted R^2	0.114	0.115	0.114	0.115
Individuals	25,032	25,032	25,032	25,032
Municipality-of-origin FE	✓	✓	✓	✓
Baseline controls	✓	✓	✓	✓
Slum characteristics		✓		
$\hat{p}_s$			✓	✓
$\hat{p}_s \times \psi_o$				✓

Notes: The table shows regressions for children who are aged 0 to 18 at baseline and are matched to the RSH data. Standard errors are clustered by slum of origin in parentheses (90 clusters), and Conley standard errors are in brackets. 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 last name dummy, household's formal employment, year-of-intervention fixed effects and year-of-birth fixed effects. Slum characteristics include families per hectare, military name, closeness to rivers/canals, slope, risk of flooding, average schooling and unemployment by census district, number of schools per census district, and distance to the CBD. The row labeled as "Percent effect" stands for percentage variation with respect to the non-displaced mean. The non-displaced mean in columns (3) and (4) is computed conditional on $\hat{p}_s$. 10%*, 5%**, 1%***.

Table 5: Displacement effect on employment and education outcomes

Outcome	Displacement effect	Mean non-displaced	Percent effect (%)	P-value/ Sharp p-value
Panel A. Type of employment				
Contract = 1	-0.068***	0.409	-16.6	0.000; 0.001
	(0.011)			
Temp worker = 1	0.063***	0.555	11.4	0.000; 0.001
	(0.014)			
Contributes to SS = 1	-0.037***	0.514	-7.2	0.005; 0.002
	(0.013)			
Panel B. Income				
Formal earnings	-17.952***	110.845	-16.2	0.003; 0.001
	(5.765)			
Informal earnings	3.913*	47.455	8.2	0.099; 0.011
	(2.351)			
Taxable wages	-56.619***	261.850	-21.6	0.000; 0.001
	(11.630)			
Panel C. Education				
Years of schooling	-0.813***	11.235	-7.2	0.000; 0.001
	(0.146)			
HS graduate = 1	-0.138***	0.639	-21.6	0.000; 0.001
	(0.027)			
2-year college = 1	-0.036***	0.115	-31.3	0.000; 0.001
	(0.008)			
5-year college = 1	-0.035***	0.051	-68.6	0.000; 0.001
	(0.007)			

Notes: The table shows propensity score estimates equivalent to column (4) of Table 4 for children aged 0 to 18 at baseline who are matched to the RSH data. Clustered standard errors by slum of origin are in parentheses. Column (4) reports p-values and sharp p-values for the hypothesis that each coefficient is equal to zero. Sharp p-values are corrected p-values for multiple hypotheses comparison based on Anderson (2008)'s method. 10%*, 5%**, 1%***.

Table 6: Displacement effect and spillovers on non-displaced children

Outcome:	Self-reported earnings (CLP$1,000/month)			
	(1)	(2)	(3)	(4)
Displaced	-14.038**	-13.796**	-12.650**	-12.046*
	(5.384)	(5.585)	(5.193)	(6.288)
Non-displaced $< 1km$		3.291		
		(9.877)		
Non-displaced $< 1.5km$			11.475	
			(16.005)	
Non-displaced $< 2km$				7.541
				(11.250)
Non-displaced mean	158.300	156.643	156.624	156.656
Percent effect	**-8.9**	**-8.8**	**-8.1**	**-7.7**
Adjusted R^2	0.128	0.128	0.128	0.128
Observations	25,032	25,032	25,032	25,032

Notes: The table shows regressions for children aged 0 to 18 at baseline who are matched to the RSH data, and reports non-missing schooling equivalent to column (4) of Table 4. The 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 a radius of 1, 1.5, or 2 km. Standard errors clustered by slum of origin are in parentheses. 10%*, 5%**, 1%***. In Table A.3 we report equivalent results on formal earnings and years of education.

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

Outcome:	Self-reported labor earnings (2007-2019)					Auxiliary
	(1)	(2)	(3)	(4)	(5)	(6)
Displaced	-14.038**	-1.830	-13.932**	-1.798	-3.499	
	(5.384)	(8.935)	(5.419)	(8.711)	(8.973)	
Project size (#units)		-0.011***		-0.011***	-0.013***	560.567***
		(0.003)		(0.004)	(0.004)	(94.341)
Share network (0-100)		0.132*		0.177*	0.131	-16.872***
		(0.075)		(0.105)	(0.111)	(3.785)
Distance from origin (km)		-0.181		-0.044	0.253	9.970***
		(0.464)		(0.561)	(0.615)	(1.394)
ΔDistance to CBD			-0.287	-0.533	-0.393	3.433***
			(0.611)	(0.702)	(0.741)	(0.754)
Δ# schools/child			1.750	1.239	1.618	0.013
			(1.174)	(1.673)	(1.961)	(0.134)
ΔUnemployment			0.299	-0.258	-0.307	1.200
			(0.472)	(0.522)	(0.520)	(1.100)
ΔProperty prices			5.252	2.782	1.463	0.050
			(3.402)	(3.289)	(3.379)	(0.086)
Home value (UF)					0.107*	-35.852***
					(0.061)	(10.852)
Non-displaced mean	158.300	158.300	58.300	158.300	158.300	
Percent effect	**-8.9**	**-1.2**	**-8.8**	**-1.1**	**-2.2**	
Adj. R^2	0.124	0.125	0.124	0.125	0.125	
Observations	25,032	25,032	25,032	25,032	25,032	25,032

Notes: The table shows results for coefficients β and γ from regression $Y_i = \alpha + \beta Displaced_{s\{i\}} + \gamma \Delta Attribute_o + \psi_o + \hat{p}_s + \hat{p}_s \times \psi_o + X_i'\theta + \varepsilon_i$. Column (6) estimates the correlation between each attribute and $Displaced_{s\{i\}}$. The table also shows propensity score estimates equivalent to column (4) of Table 4 for children aged 0 to 18 matched to the RSH data. Clustered standard errors by slum of origin are in parentheses, and the row labeled as "Percent effect" stands for percentage variation with respect to the non-displaced mean. 10%*, 5%**, 1%***.

Table 8: Displacement effect on children's and parents' locations between 2017 and 2019

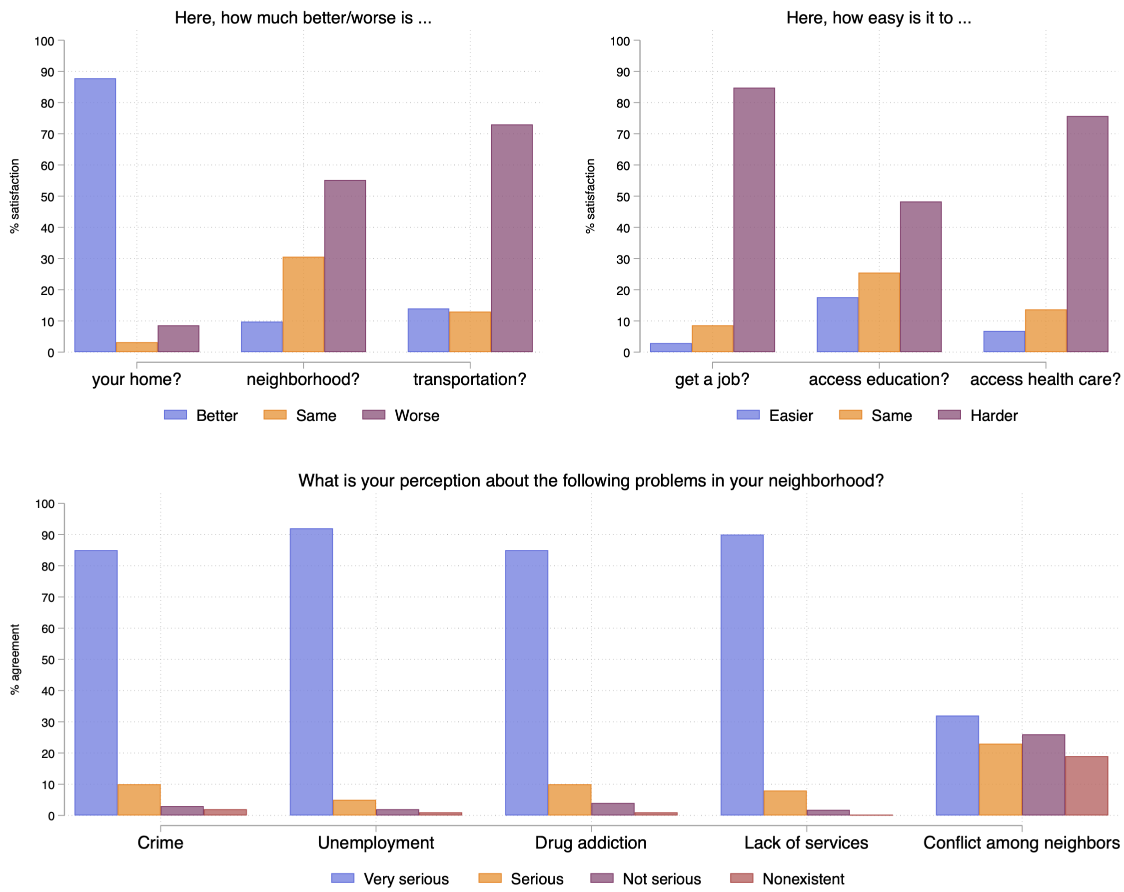
	Probability of living in ...			Distance	% poor
	assigned municipality	assigned neighborhood	municipality of origin	from assigned neighborhood	in current neighborhood
	(1)	(2)	(3)	(4)	(5)
Panel A. Parents in the RSH					
Displaced	0.030	-0.186	-0.162	1.719	0.033***
	(0.126)	(0.134)	(0.129)	(1.651)	(0.009)
Non-displaced mean	0.599	0.536	0.599	4.260	0.509
Percent effect	**5.0**	**-34.7**	**-27.0**	**40.4**	**6.5**
Observations	10,392	10,392	10,392	8,952	10,392
Panel B. Children in the RSH					
Displaced	0.057	-0.123	-0.131	1.880	0.026***
	(0.105)	(0.083)	(0.091)	(1.935)	(0.007)
Non-displaced mean	0.436	0.343	0.422	6.550	0.499
Percent effect	**13.1**	**-35.9**	**-31.0**	**28.7**	**6.8**
Panel C. Children in the RSH by age					
Displaced 0–5 (β_1)	0.039	-0.129	-0.157*	1.867	0.028***
	(0.106)	(0.085)	(0.092)	(1.951)	(0.008)
Displaced 6–12 (β_2)	0.061	-0.116	-0.128	1.892	0.024***
	(0.106)	(0.083)	(0.091)	(1.949)	(0.007)
Displaced 13–18 (β_3)	0.069	-0.131	-0.097	1.832	0.025***
	(0.106)	(0.083)	(0.091)	(1.935)	(0.008)
Observations	12,968	12,968	12,968	11,017	12,968
Test $\beta_1 = \beta_2$	0.250	0.467	0.107	0.941	0.362
Test $\beta_1 = \beta_3$	0.293	0.966	0.016	0.858	0.510
Test $\beta_2 = \beta_3$	0.720	0.552	0.172	0.897	0.913

Notes: The table shows regressions for all adults (Panel A) and children aged 0 to 18 at baseline (Panels B and C) who are matched to the RSH data, and reports a non-missing location between 2017 and 2019. The regressions are equivalent to column (4) of Table 4. Standard errors clustered by slum of origin are in parentheses, and the row labeled as "Percent effect" stands for percentage variation with respect to the non-displaced mean. "Distance from assigned neighborhood" is computed for the sample of individuals who remain in Greater Santiago through 2017. The last three rows report p-values for equality tests of coefficients in Panel C. 10%*, 5%**, 1%***.

Online Appendix

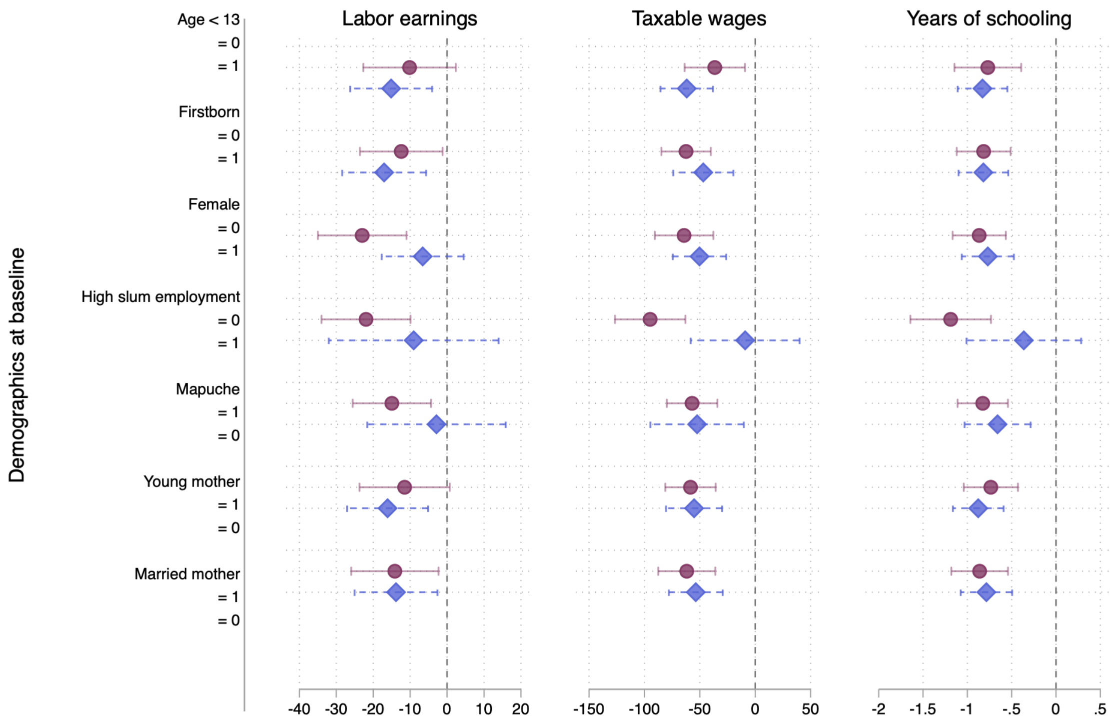
A Additional figures and tables

Figure A.1: Summary of the evaluation of the Program for Urban Marginality: Results from Aldunate et al. (1987)



Notes: The figure presents a summary of results found by Aldunate et al. (1987). The authors interviewed 592 displaced slum families who were relocated into four new neighborhoods.

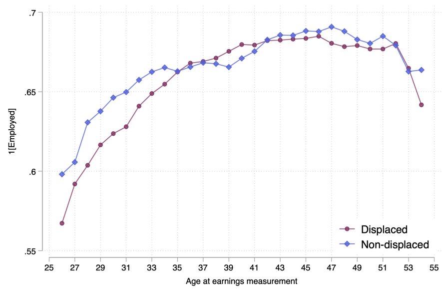
Figure A.2: Displacement effect by demographic groups on earnings and education



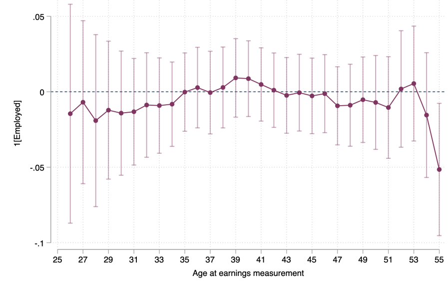
Notes: The figure shows propensity score estimates and their 95% confidence intervals, equivalent to column (4) of Table 4, stratified by demographic variables for the sample of children aged 0 to 18 matched to the RSH data. Standard errors are clustered by slum of origin. "Married mother" is measured at the time of the intervention, "young mother" stands for mothers younger than 25 (sample median) at the time their child is born, and "high slum employment" stands for slums where the average formal employment rate of heads of households is above the sample median at baseline.

Figure A.3: Displacement effects on labor market outcomes by age at earnings measurement

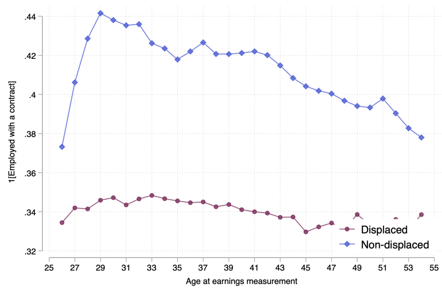
(a) Employment trajectories



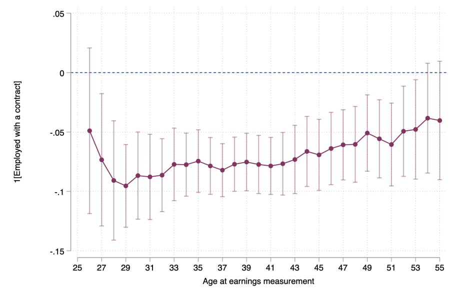
(b) Displacement effect on employment



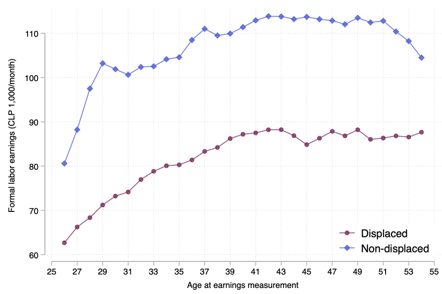
(c) Formal employment trajectories



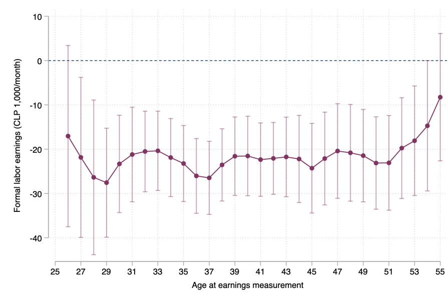
(d) Displacement effect on formal employment



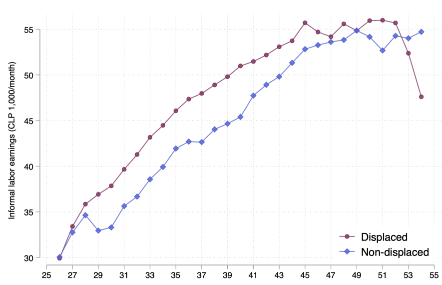
(e) Formal earnings trajectories



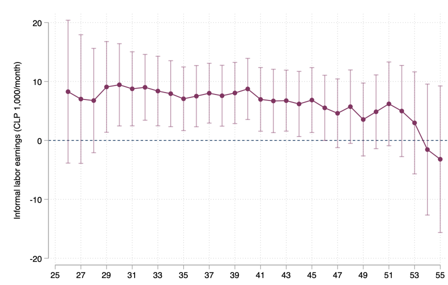
(f) Displacement effect on formal earnings



(g) Informal earnings trajectories



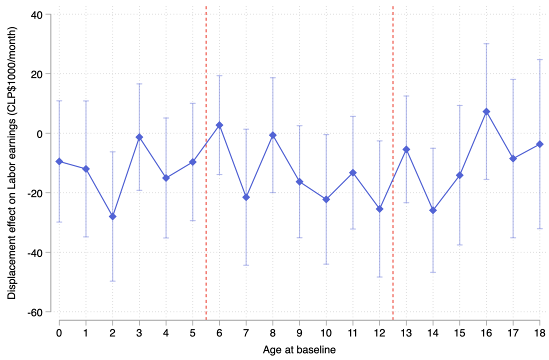
(h) Displacement effect on informal earnings



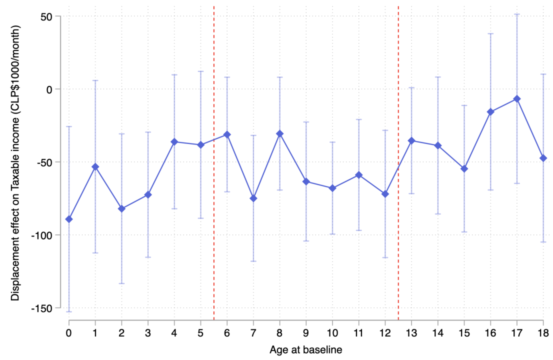
Notes: The figures show regressions for children aged 0 to 18 at baseline who are matched to the RSH data. Panels (a), (c), (e), and (g) plot the predicted trajectories for displaced and non-displaced children between ages 27 and 54 from the regression $y_{it} = \sum_{\tau=27}^{54} \beta_\tau Displaced * 1[Age = \tau] + \sum_{\tau=26}^{54} \delta_\tau 1[Age] + \psi_o + \hat{p}(X_s) + \hat{p}(X_s) \times \psi_o + X'_{it}\gamma + u_{it}$, for different outcomes. Panels (b), (d), (f), and (h) plot coefficients β_τ and their 95% confidence intervals for corresponding outcomes. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, Mapuche last name, firstborn dummy, head of household formal employment, year-of-birth fixed effects, and year-of-intervention fixed effects.

Figure A.4: Displacement effect by age at intervention

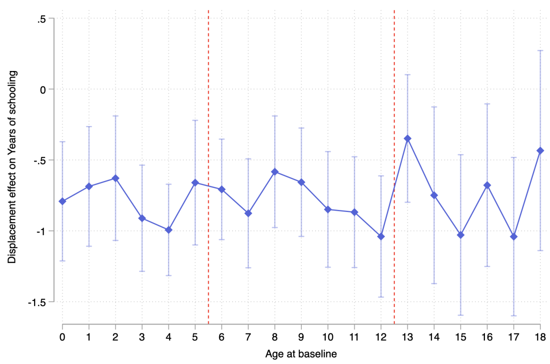
(a) Labor earnings (2007-2019)



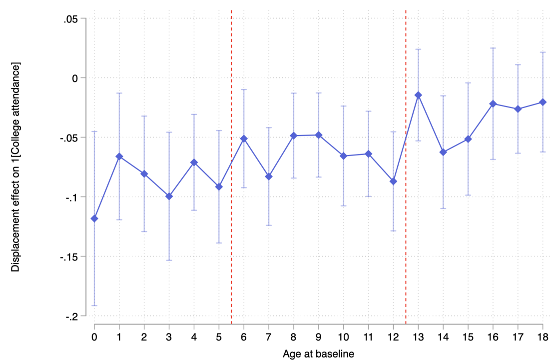
(b) Taxable wages (2016-2019)



(c) Years of Education

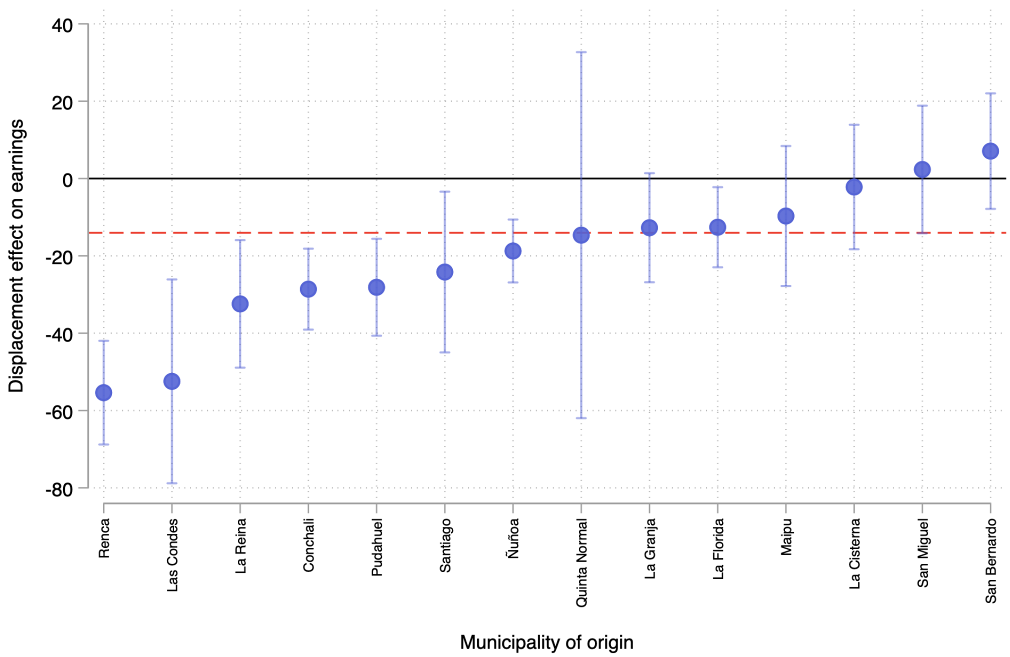


(d) College attendance



Notes: The figure plots the displacement coefficient and its 95% confidence interval derived from estimating equation (1), stratified by age at intervention. Dotted red vertical lines indicate that the p-value of the structural break test at the corresponding age is smaller than 0.1 for most outcomes. The regressions are for children who are 0 to 18 years old at the time of the intervention and are matched with the RSH data. Standard errors are clustered by slum of origin.

Figure A.5: Distribution of displacement effects on labor earnings by municipality of origin

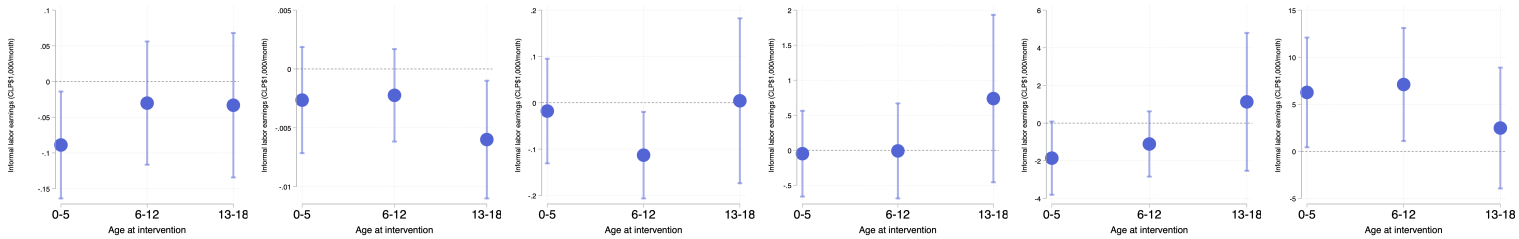


Notes: The figure shows regressions stratified by municipality of origin. The sample includes children who were 0 to 18 years old at the time of the intervention, matched to the RSH data, and from 14 municipalities with both displaced and non-displaced populations. The coefficients are estimated from a regression stratified by municipality of origin $y_i = \sum_{o=1} \beta_o Displaced_{s\{i\}} * 1[Origin = o] + \hat{p}(X_s) \times \psi_o + X'_{io}\theta + \varepsilon_i$. Sue to the low number of slums per municipality, the interaction $\hat{p}(X_s) \times \psi_o$ is not identified in all municipalities of origin. Therefore, we use an inverse propensity score re-weighting method to run this regression. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, Mapuche last name, firstborn dummy, head of household formal employment, year-of-birth fixed effects, and year-of-intervention fixed effects. The red horizontal line represents the average displacement effect in the full sample of children (column (4) of Table 4). β_o and its 95% confidence intervals are reported, and clustered standard errors by slum of origin are in parentheses.

Figure A.6: Mechanisms of displacement effect by age at intervention

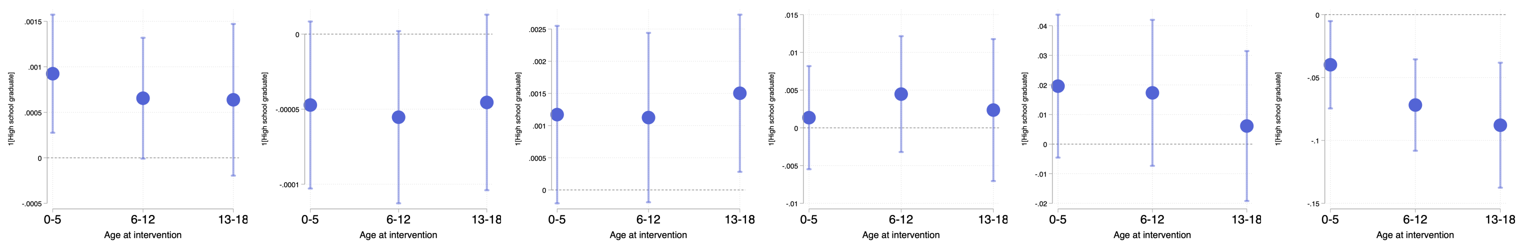
A. Informal labor earnings

(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

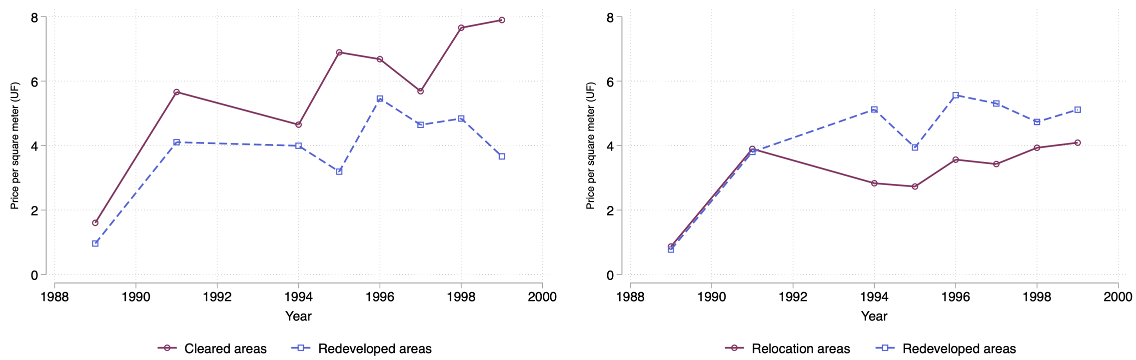
(g) Home value **(h)** Neighborhood size **(i)** % original slum **(j)** Δ distance to CBD **(k)** Δ # schools/student **(l)** Δ log prices



Notes: The figures plot equivalent coefficients from column (4) of Table 7 and their 95% confidence intervals for self-reported informal labor earnings (panel (a)) and high school completion (panel (b)), stratified by age groups at baseline ([0,5], [6–12], and [13–18]). The regressions are for children who were 0 to 18 years old at baseline and matched to the RSH data. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, Mapuche last name, firstborn dummy, head of household formal employment, year-of-birth fixed effects, and year-of-intervention fixed effects. Standard errors are clustered by slum of origin.

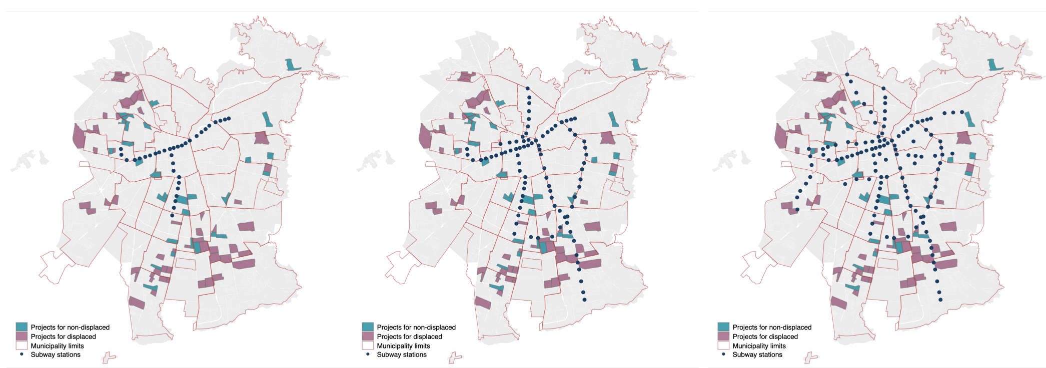
Figure A.7: Difference in land value across time by treatment

(a) Neighborhoods of origin **(b)** Neighborhoods of destination



Notes: The figures plot the difference in land value by square meters, measured in UF, in areas were slums and neighborhoods were located. We use historical data from Trivelli (1989–2000). Panel (a) plots the difference between cleared areas (families were displaced) and redeveloped areas (families were not displaced). Panel (b) plots the difference between relocation areas (areas that received displaced families) and redeveloped areas. Differences control for the number of offers and total squared meters offered in each zone.

Figure A.8: Location of public housing projects and subway stations

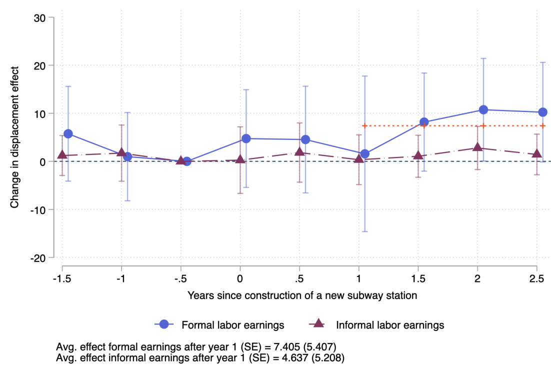


(a) Subway in 1980 **(b)** Subway in 2006 **(c)** Subway in 2019

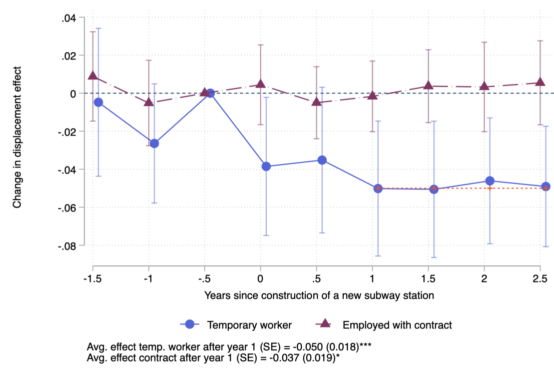
Notes: The figures show the rollout of subway stations in Greater Santiago from 1980 to 2019. Red lines represent the urban limits of Greater Santiago and its municipalities in 2019, while colored areas correspond to neighborhoods created by the Program for Urban Marginality between 1979 and 1985. Purple areas correspond to projects that received displaced families, and green areas correspond to projects for non-displaced families. Blue circles are locations of subway stations at each moment in time. The data used to construct this map come from MINVU (1979), Molina (1986), Benavides et al. (1982), Morales and Rojas (1986), and Metro de Santiago.

Figure A.9: Change in displacement effect due to subway access

(a) Labor earnings (CLP$1,000/month)



(b) Employment by type



Notes: Each coefficient and its 95% confidence interval in panels (a) and (b) correspond to estimates of γ_2 from regression $Y_{it} = \alpha + \beta Displaced_{s\{i\}} + \gamma_{1\tau} Subway_{\lambda\{\tau\}} + \gamma_{2\tau} Displaced_{s\{i\}} \cdot Subway_{\lambda\{\tau\}} + \psi_o + X_i'\theta + \delta_t + \varepsilon_{it}$. Treatment corresponds to the variable $Subway_{\lambda\{\tau\}}$, defined as distance to subway below 2,000 meters, and τ corresponds to years relative to the subway station opening. Clustered standard errors by slum of origin are in parentheses.

Table A.1: Conley standard errors

Outcome	Labor income	Taxable wages
Displacement coefficient	-14.038	-56.619
Clustered se by slum of origin	5.384	11.630
Bootstrapped se	5.066	14.450
Conley se (cutoffs in km)		
1	5.031	11.360
2	5.050	11.567
3	5.104	11.643
4	5.124	11.636
5	5.125	11.855
6	5.143	12.017
7	**5.145**	12.182
8	5.122	12.375
9	5.107	12.522
10	5.085	12.605
11	5.094	12.606
12	5.109	12.666
13	5.101	12.718
14	5.088	12.746
15	5.081	12.773

Notes: The 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 Fetzer's website. Bootstrapped standard errors are computed with 200 replications.

Table A.2: Robustness of displacement effect to changes in propensity score method and support

Model	Baseline	$p_1 < p < p_{99}$	$p_5 < p < p_{95}$	P-score+FE	Inv-weight
	(1)	(2)	(3)	(4)	(5)
Panel A. Self-reported earnings (CLP$1,000/month)					
Displaced	-14.038	-14.676	-12.732	-13.185	-15.696
	(5.384)**	(5.424)***	(5.521)**	(4.870)***	(3.600)***
Non-displaced mean	158.300	153.709	164.115	164.761	159.714
Percent effect	**-8.9**	**-9.5**	**-7.8**	**-8.0**	**-9.8**
Adjusted R^2	0.125	0.124	0.125	0.125	0.122
Panel B. Taxable wages from social security (CLP$1,000/month)					
Displaced	-56.619	-43.290	-51.380	-28.622	-28.504
	(11.630)***	(9.059)***	(6.836)***	(11.817)**	(8.465)***
Non-displaced mean	261.850	250.348	291.931	272.342	279.856
Percent effect	**-21.6**	**-17.2**	**-17.6**	**-10.5**	**-10.2**
Adjusted R^2	0.058	0.059	0.061	0.057	0.056
Panel C. 1[Employed]					
Displaced	-0.015	-0.001	-0.021**	-0.005	-0.004
	(0.012)	(0.009)	(0.009)	(0.009)	(0.008)
Non-displaced mean	0.641	0.638	0.641	0.642	0.645
Percent effect	**-2.3**	**-0.2**	**-3.3**	**-0.8**	**-0.6**
Adjusted R^2	0.115	0.115	0.118	0.114	0.115
Panel D. Years of schooling					
Displaced	-0.813***	-0.870***	-0.602***	-0.412***	-0.643***
	(0.146)	(0.152)	(0.095)	(0.134)	(0.142)
Non-displaced mean	11.235	11.202	11.540	11.446	11.297
Percent effect	**-7.2**	**-7.7**	**-5.2**	**-3.6**	**-5.7**
Adjusted R^2	0.112	0.113	0.118	0.108	0.094
Individuals	25,032	24,846	21,356	24,463	25,032
Number of slums	90	88	80	90	90

Notes: Column (1) is equivalent to the results in column (4) of Table 4. Column (2) drops slums in the 1% lower and upper part of the common support distribution, while column (3) drops slums in the 5% lower and upper part of the common support distribution. Column (4) adds municipality-of-origin fixed effects to the propensity score estimation, and column (5) estimates the displacement effect by propensity score re-weighting.

Table A.3: Displacement effect and spillovers

	Baseline (1)	(2)	(3)	(4)
Panel A. Self-reported formal labor earnings				
Displaced	-17.952***	-17.362***	-16.468***	-15.348**
	(5.765)	(5.973)	(5.853)	(6.850)
Non-displaced $< 1km$		8.022		
		(9.921)		
Non-displaced $< 1.5km$			12.263	
			(14.298)	
Non-displaced $< 2km$				9.855
				(10.308)
R^2	0.071	0.071	0.071	0.071
Non-displaced mean	110.845	108.492	109.088	108.265
Panel B. Schooling				
Displaced	-0.813***	-0.698***	-0.804***	-0.821***
	(0.146)	(0.133)	(0.140)	(0.155)
Non-displaced $< 1km$		1.565***		
		(0.235)		
Non-displaced $< 1.5km$			0.072	
			(0.534)	
Non-displaced $< 2km$				-0.029
				(0.324)
R^2	0.116	0.116	0.116	0.116
Non-displaced mean	11.235	11.189	11.213	11.188
Observations	25,032	25,032	25,032	25,032

Notes: The table shows regressions for children aged 0 to 18 at baseline who are matched to the RSH data non-missing schooling equivalent to column (4) of Table 4. The 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 a radius of 1, 1.5, or 2 km. Standard errors clustered by slum of origin are in parentheses. 10%*, 5%**, 1%***.

Table A.4: Assignment location attributes and displaced families' characteristics at baseline

	Different characteristics of projects or districts of assignment								
	Home value (log UF)	Distance from origin	Project size	Adult pop. schooling	# schools/ 1,000 students	Log property prices	Distance to CBD	Primary care centers	Unemployment rate
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
# Children	0.001	-0.016	2.265	-0.002	-0.008	0.005	-0.030	-0.001	-0.000
	(0.001)	(0.018)	(3.810)	(0.002)	(0.009)	(0.004)	(0.022)	(0.001)	(0.000)
Married	0.005	-0.092	5.347	-0.002	-0.030	-0.022	-0.159*	-0.006	0.002
	(0.006)	(0.078)	(15.861)	(0.010)	(0.042)	(0.020)	(0.086)	(0.005)	(0.001)
Cohabit	-0.000	0.044	14.615	0.020	0.018	-0.007	0.011	-0.005	-0.001
	(0.002)	(0.047)	(9.606)	(0.012)	(0.019)	(0.010)	(0.050)	(0.004)	(0.001)
Age	0.000	-0.006	0.721	0.001	-0.001	-0.001	-0.010	-0.000**	0.000
	(0.000)	(0.005)	(1.159)	(0.001)	(0.003)	(0.001)	(0.006)	(0.000)	(0.000)
Age youngest child	0.000	-0.011	1.807	0.001	-0.005	-0.000	-0.013	-0.000	-0.000
	(0.000)	(0.007)	(1.600)	(0.001)	(0.004)	(0.000)	(0.009)	(0.000)	(0.000)
Mapuche last name	0.006	-0.114	27.178	0.008	-0.046	-0.020*	-0.145	-0.002	0.001
	(0.006)	(0.080)	(16.670)	(0.013)	(0.045)	(0.011)	(0.089)	(0.007)	(0.001)
Formal employment	0.012*	0.036	-54.980**	-0.027	0.021	-0.010	-0.140*	-0.005	0.001
	(0.007)	(0.053)	(21.566)	(0.022)	(0.027)	(0.012)	(0.073)	(0.009)	(0.002)
Adjusted R^2	0.432	0.970	0.763	0.808	0.363	0.405	0.738	0.851	0.655
Observations					8,439				
P-values for test of joint insignificance of baseline characteristics in regressions above									
Attribute in levels	0.331	0.171	0.122	0.510	0.035	0.213	0.081	0.554	0.386
Attribute in Δ	-	-	-	0.128	0.128	0.111	0.346	0.496	0.121
Municipality-of-origin FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year-of-intervention FE	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The sample includes all mover families with children in the common support. Covariates measured for head of households. In addition to municipality and year-of-intervention fixed effects, all regressions control for propensity score decile dummies and their interactions with municipality-of-origin fixed effects. Standard errors are clustered by slum of origin. 10%*, 5%**, 1%***.

Table A.5: Displacement effect and change in location attributes on earnings

Outcome	Taxable wages	Formal earnings	Informal earnings	Contract	Schooling	HS grad
	(1)	(2)	(3)	(4)	(5)	(6)
Displaced	13.820	-16.218	7.711**	-0.052***	-0.480	-0.109**
	(17.487)	(10.032)	(3.411)	(0.020)	(0.323)	(0.052)
Project size (#units)	-0.052***	-0.009*	-0.003*	-0.000	-0.0002	-0.000***
	(0.012)	(0.005)	(0.002)	(0.000)	(0.000)	(0.000)
Share network (0-100)	0.440	0.197*	-0.064	0.001*	0.010**	0.001**
	(0.266)	(0.113)	(0.051)	(0.000)	(0.004)	(0.001)
Distance from origin (km)	-1.751	0.646	-0.281	0.000	-0.002	0.002
	(1.348)	(0.526)	(0.322)	(0.002)	(0.014)	(0.002)
Δ# schools/child	-6.003	2.797	-0.952	0.005	0.132*	0.017
	(3.654)	(1.882)	(0.894)	(0.005)	(0.074)	(0.011)
ΔDistance to CBD	0.075	-0.943	0.230	0.000	-0.009	0.001
	(2.232)	(0.704)	(0.309)	(0.002)	(0.021)	(0.003)
ΔUnemployment	-2.278*	-0.692	0.335	-0.0025*	-0.0162	-0.0024
	(1.222)	(0.516)	(0.342)	(0.0015)	(0.017)	(0.0026)
ΔProperty prices	-35.160***	-4.416	5.705**	-0.031***	-0.456****	-0.075***
	(9.763)	(3.317)	(2.784)	(0.010)	(0.136)	(0.020)
Home value (UF)	0.089	0.135*	-0.034	0.000	0.003*	0.001**
	(0.123)	(0.078)	(0.037)	(0.000)	(0.002)	(0.000)
Non-displaced mean	238.178	104.532	46.761	0.377	11.645	0.670
Adj. R^2	0.064	0.067	0.038	0.068	0.114	0.090
Observations	25,032	25,032	25,032	25,032	25,032	25,032

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

Table A.6: Displacement effect on the probability of selling home by 2019

	Home ever sold	Inheritance	Conditional on selling Log(Price)	Year sold	# years after treatment
	(1)	(2)	(3)	(4)	(5)
Displaced	0.003	0.019	-0.066	0.896	1.358
	(0.010)	(0.013)	(0.195)	(1.671)	(1.541)
Adj. R^2	0.034	0.051	0.011	0.022	0.044
Non-displaced mean	0.047	0.140	9.595	2008.885	26.487
Percent effect	**6.4**	**13.6**	**-0.69**	**0.04**	**5.1**
Observations	4,537	4,537	273	273	273

Notes: Due to our small sample, we compute inverse propensity score estimates in the archival sample of families who received a home in a municipality located in the northern areas of Greater Santiago. The data include 48 slums of origin and 12 municipalities of origin. Baseline controls include the following: female-headed household, number of children in family, married head of household, head of household's age, Mapuche head of household, head of household formal employment, head of household year-of-birth fixed effects, and year-of-intervention fixed effects. Clustered standard errors by slum of origin are in parentheses. 10%*, 5%**, 1%***.

Table A.7: Displacement effect and subway rollout between 2007 and 2019

	Labor Earnings				
	Total	Formal	Informal	Temp. worker	Contract
	(1)	(2)	(3)	(4)	(5)
Panel A. Distance = 1.5 km (9 neighborhoods treated)					
Displaced	-15.156***	-21.906***	6.750***	0.076***	-0.074***
	(4.174)	(4.057)	(2.186)	(0.015)	(0.012)
Subway station	-23.894**	-8.091	-15.804***	0.078***	-0.044
	(11.565)	(11.689)	(3.150)	(0.024)	(0.033)
Displaced*Subway	-0.420	-10.995	10.574**	-0.054*	0.015
	(13.896)	(13.070)	(5.244)	(0.029)	(0.034)
* $T < -0.5$	3.580	0.702	2.878	0.014	-0.007
	(4.734)	(5.002)	(2.426)	(0.017)	(0.011)
* $T = -0.5$ (omitted)					
* $T \in [0, 1]$	7.786	4.311	3.474	-0.021	-0.004
	(6.653)	(6.286)	(3.161)	(0.019)	(0.009)
* $T > 1$	16.531**	13.068**	3.463	-0.038**	0.007
	(6.874)	(6.440)	(2.370)	(0.017)	(0.014)
Adj. R^2	0.132	0.074	0.036	0.075	0.066
Panel B. Distance = 2 km (14 neighborhoods treated)					
Displaced	-13.671***	-20.845***	7.174***	0.073***	-0.075***
	(4.106)	(4.317)	(2.328)	(0.014)	(0.013)
Subway station	-26.582**	-17.901	-8.680**	0.023	-0.034
	(12.447)	(11.657)	(3.791)	(0.033)	(0.021)
Displaced*Subway	-1.890	-3.591	1.701	0.007	0.014
	(11.556)	(10.868)	(4.018)	(0.033)	(0.021)
* $T < -0.5$	2.118	0.438	1.679	0.005	-0.005
	(4.225)	(4.628)	(2.209)	(0.017)	(0.010)
* $T = -0.5$ (omitted)					
* $T \in [0, 1]$	5.676	4.637	1.038	-0.037*	-0.000
	(5.301)	(5.208)	(3.284)	(0.019)	(0.010)
* $T > 1$	8.880	7.405	1.475	-0.050***	0.002
	(6.369)	(5.407)	(2.107)	(0.018)	(0.010)
Adj. R^2	0.132	0.074	0.036	0.075	0.066
Observations	501,173	501,173	501,173	501,173	501,173

Notes: The table shows matching propensity score regressions for children who were aged 0 to 18 at baseline and matched to the RSH data in the panel dataset from 2007 to 2019. Standard errors clustered by slum of origin are in parentheses. Baseline controls include the following: female, mother head of household, married head of household, head of household's age, number of children, firstborn dummy, Mapuche last name, head of household formal employment, year-of-intervention fixed effects, and year-of-birth fixed effects. 10%*, 5%**, 1%***.

Table A.8: Comparison of displacement/mover estimates across studies

Study	Setting	% Δ earnings	% Δ neighborhood Quality	$\|Elasticity\|$
	(1)	(2)	(3)	(4)
Panel A. Earnings estimates				
Chetty et al. (2016)[a]	MTO (children 7–13 in exp. group)	+14%	-34% (Poverty)	0.41
Chyn (2018)[b]	Public demolition in Chicago (children 7–18)	+16%	-22.2% (Poverty)	0.72
Barnhardt et al. (2016)[c]	Housing lottery in Ahmedabad (adults in India)	-14.5%	-37.5% (Urbanicity); -8.1% (Housing Value)	0.38–1.8
This paper[d]	Program for Urban Marginality (children 0–18 in Chile)	-8.9%	-11.9% (Housing value) +15% (Unemployment)	0.6–0.75
Panel B. Schooling estimates				
Chetty et al. (2016)[a]	MTO (children 7–12 in Exp. group)	+15% (College att.)	-34% (Poverty)	0.44
Chyn (2018)[b]	Public demolition in Chicago (children 7–18)	-8.1% (HS dropout)	-22.2% (Poverty)	0.36
		28% (College att.)	-22.2% (Poverty)	1.26
Barnhardt et al. (2016)[c]	Housing lottery in Ahmedabad (children in India)	-2.25% (schooling)	-37.5% (Urbanicity); -8.1% (Housing value)	0.06–0.27
Camacho et al. (2022)[c]	Free housing program (children in Colombia)	5.7% (schooling)	-9.8% (Distance to schools)	0.58
		17% (HS grad)	-9.8% (Distance to schools)	1.73
This paper[e]	Program for Urban Marginality (children 0–18 in Chile)	-7.2% (schooling)	-11.9% (Housing value); +15% (Unemployment)	0.48–0.61
		-21.6% (HS grad)	-11.9% (Housing value); +15% (Unemployment)	1.44–1.82
		-49.9% (College att.)	-11.9% (Housing value); +15% (Unemployment)	3.33–4.2

Notes: The results come from tables in each corresponding paper: [a]Tables 2 and 3, [b]Tables 2 and 3, [c]Tables 5 and 6, [d] Tables 1 and 5, and [e] Tables 4 and 5.

B Propensity score estimation

We estimate the propensity score by running a logistic regression of the probability of relocation versus redevelopment on a set of slum characteristics before the program started. To do so, we use data from Morales and Rojas (1986), who compiled the largest sample of slums by treatment that participated in the program between 1979 and 1985. We complement their data using the 1979 slum census conducted by MINVU, the list of displaced slums collected by Molina (1986), and the location of slums compiled by Benavides et al. (1982). With all these sources, we are able to characterize 222 slums. This is not the complete universe because many lack a location or changed their names after 1973—making tracking slums across time more difficult—and the sample only includes slums in urban municipalities.

Given the uncertainties surrounding the total number of families who participated in the program and the distribution of slums across treatments, this sample of 222 slums represents our most comprehensive effort to analyze and compare their characteristics. For example, Molina (1986) documents that in 1979, MINVU targeted 50,000 families in 340 slums, of whom 70% would be displaced. However, based on the author's data collection, only 40,000 families were treated. Additionally, Morales and Rojas (1986) find that more than 300 slums were treated after 1985, with 60% of them being displaced but only in urban areas. Another feature of the data collected by Morales and Rojas (1986) is that many of the slums they considered as non-displaced were split in various smaller slums and included projects that were finished later in the 1980s. With these caveats in mind, we still use their dataset because it is the most complete in terms of slum characteristics, though the non-displaced slums are over-represented compared to other historical sources.

We use the characteristics in Table 1 to estimate the probability of relocation versus redevelopment. Table B.1, column (1) shows the estimates for all covariates, excluding the price index for surrounding property prices, which might reflect expectations of future relocations and is therefore omitted from our main specification. For comparison, in column (2) we include the price index and find the opposite sign, but it is not statistically significant. Overall, its inclusion does not change the coefficients nor the signs of other determinants of relocation. In columns (3) and (4) we add municipality-of-origin fixed effects. As we observed in Table 1, the differences in slum characteristics remained within municipalities. When we include them in the regression, local characteristics of slums (e.g., elevation) become more predictive of relocation, and characteristics of the census districts (e.g., population's schooling) become less relevant (the coefficients decrease). In the robustness check section, we use the estimates in column (3) to show the robustness of the displacement effect to different versions of the propensity score (see Table A.2, column (4)).

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

Outcome	Pr(slum is cleared and relocated = 1)			
	(1)	(2)	(3)	(4)
Families/hectare	0.002	0.002	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)
Military name	-0.197	-0.201	-0.271	-0.256
	(0.414)	(0.414)	(0.531)	(0.531)
Elevation (mas)	-0.004*	-0.003	-0.019***	-0.020***
	(0.002)	(0.003)	(0.007)	(0.007)
Slope (degrees)	0.058	0.078	0.103	0.119
	(0.096)	(0.103)	(0.100)	(0.101)
Close to river/canal	0.796	0.838	0.220	0.195
	(0.726)	(0.722)	(0.769)	(0.770)
Flooding risk	0.632	0.664	0.292	0.238
	(0.842)	(0.844)	(0.920)	(0.924)
Distance to CBD	0.072	0.066	0.097	0.084
	(0.044)	(0.044)	(0.075)	(0.077)
Population's schooling	0.449**	0.470**	0.138	0.150
	(0.213)	(0.221)	(0.234)	(0.239)
Unemployment rate	2.910	2.325	-7.221	-7.173
	(5.938)	(5.936)	(7.568)	(7.607)
Schools per district	-0.126**	-0.128**	-0.141*	-0.123*
	(0.058)	(0.058)	(0.073)	(0.075)
Log surrounding prices		-0.578		-1.098
		(0.813)		(1.191)
Municipality-of-origin FE			✓	✓
Observations	222	222	222	222

Notes: The table shows logit regressions for the linear probability of slum clearance (versus redevelopment) on slum characteristics, in the sample of slums in Morales and Rojas (1986). Robust standard errors are in parentheses. 10%*, 5%**, 1%***.

We use the results in column (1) to estimate the propensity score in our archival sample, which corresponds to 99 slums. Table B.2, columns (1)–(3) show characteristics of the slums in this sample. While we find the same patterns as in the full sample of slums, the differences are much smaller, meaning the slums in the archival sample are more similar to each other across treatments. These differences are due to displaced slums in the archival sample being less dense, having more military names, having a lower slope, and being less peripheral, as measured by distance to the CBD. Overall, they look more similar to the non-displaced group. This motivates us to estimate the propensity score in the full sample of slums for two reasons: to gain statistical power and to reduce sample selection. We estimate the propensity score in the full sample and use the estimates in Table B.1, column (1) to predict the propensity score in the sample of slums in the archives.

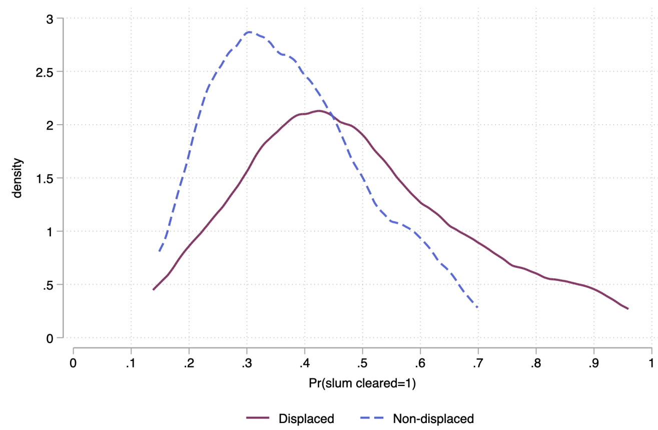
Table B.2: Slum characteristics in estimation sample prior to the intervention

	Slums in Archives			Slums in common support		
	All	Displaced	Non-displaced	All	Displaced	Non-displaced
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Slum attributes						
Families/hectare	66.294	65.010	68.168	67.213	66.492	68.168
	[53.933]	[53.96]	[54.582]	[55.313]	[56.412]	[54.582]
Military name	0.182	0.172	0.195	0.167	0.137	0.205
	[0.388]	[0.381]	[0.401]	[0.375]	[0.348]	[0.409]
Elevation	574.798	568.845	583.22	571.744	562.157	584.282
	[81.242]	[82.845]	[79.161]	[81.347]	[81.188]	[80.876]
Slope	2.709	2.780	2.610	2.65	2.707	2.574
	[1.518]	[1.632]	[1.355]	[1.558]	[1.706]	[1.359]
Close to river or canal (<100 m)	0.030	0.034	0.024	0.022	0.020	0.026
	[0.172]	[0.184]	[0.154]	[0.148]	[0.140]	[0.160]
Flooding risk	0.020	0.034	0.00	0	0	0
	[0.141]	[0.184]	[0.00]	[-]	[-]	[-]
Distance to CBD	9.429	9.072	9.934	9.401	9.087	9.811
	[3.648]	[3.675]	[3.593]	[3.649]	[3.803]	[3.443]
Panel B. Census districts attributes						
Population's schooling	7.670	7.799	7.488	7.438	7.397	7.493
	[1.967]	[2.254]	[1.478]	[1.648]	[1.760]	[1.510]
Unemployment rate	0.190	0.193	0.185	0.196	0.203	0.187
	[0.059]	[0.066]	[0.048]	[0.054]	[0.058]	[0.048]
Number of schools	3.733	3.820	3.610	3.606	3.560	3.667
	[2.900]	[3.024]	[2.747]	[2.892]	[2.984]	[2.804]
Log surrounding prices	14.801	14.820	14.773	14.767	14.765	14.771
	[0.360]	[0.370]	[0.348]	[0.331]	[0.315]	[0.355]
Number of slums	99	58	41	90	51	39
Number of municipalities	14	14	41	14	14	14

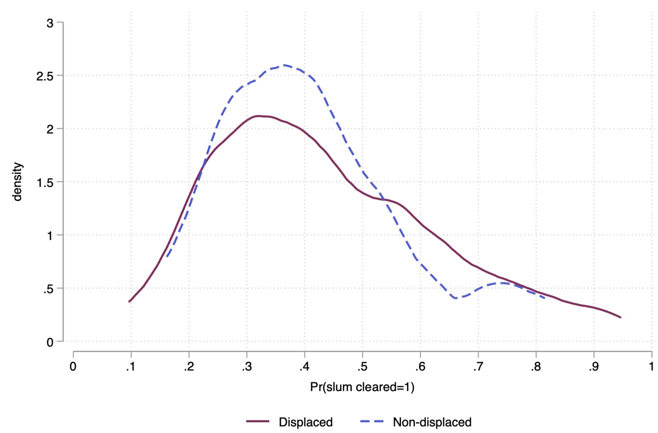
Figure B.1 shows the $\hat{p}_s$ densities for displaced slums in purple and non-displaced slums in blue. Panel (a) shows estimates for the full sample, while panel (b) shows those for slums in the archives. Based on the figures and the previous discussion, the densities in the archival sample are more similar than in the full sample because slums in the former are more similar to each other in their observables. Additionally, common support is observed in both panels. Table B.2, columns (3)–(4) show slum characteristics after imposing common support in the archival sample: nine slums are dropped, with seven in the displaced group and two in the non-displaced group. While the sample of slums in the common support looks similar to the full archival sample, none of the kept slums have a positive flooding risk, which is a predictor of displacement.

Figure B.1: Distribution of the probability of displacement by treatment

(a) Full sample



(b) Archival sample



Notes: Panel (a) plots the fitted values of a logit regression that includes controls from regression (1) of Table B.1 by treatment. Panel (b) uses the estimates in column (1) of Table B.1 to predict the probability of relocation in the sample of slums in the archives.

C Attrition

We examine sensitivity to attrition through different checks. First, we estimate Lee bounds in the sample of children matched to the RSH data (Lee, 2009). This approach makes a monotonicity assumption and then adjusts for differential attrition between treatment and control. Since the probability of finding a child in the RSH is higher for the displaced group than the non-displaced group, we assume that some individuals would attrit if they end up in the non-displaced slums but not if they end up in the displaced slums, and not vice versa. Given that the RSH concentrates the lower part of the income distribution in Chile and we hypothesize that displacement is negative for children, the monotonicity assumption appears plausible in our context.

The matching rate to the RSH for the displaced group is 83.4% and 77.7% for the non-displaced group; thus, the difference in matching rates is 5.7 percent. We therefore trim $5.7/83.4 = 7$ percent of the displaced observations, with the lower bound occurring when we trim observations with the highest earnings (or corresponding outcome) and the upper bound when we trim observations with the lowest earnings. Because our specifications require us to control for baseline characteristics and the interactions between propensity score dummies and municipality-of-origin fixed effects, we perform the trimming manually instead of relying on the command in Stata or R.

Table C.1 presents the results for different outcomes in each panel. Column (1) corresponds to the case where the only controls are municipality-of-origin fixed effects, column (2) add baseline demographics, and column (3) estimates the regression by propensity score re-weighting. Column (4) is the equivalent to our propensity score matching baseline result in Table 4, column (4) that fully controls for the interaction between propensity score dummies and municipality-of-origin fixed effects.

In addition to trimming, in the bottom of each panel, we include Imbens and Manski (2004) confidence intervals for the Lee bounds. These account for sampling variability and the potential selection bias from differential attrition.

The second exercise we conduct is trimming our sample by slum. In a similar spirit as with Lee bounds, we ask what would be the distribution of slums in our sample if we had a similar proportion of displaced and non-displaced slums, as in Morales and Rojas (1986). This requires trimming 25% of the displaced slums in our archival sample because they are over-represented compared to those in Morales and Rojas (1986). We also make two extreme assumptions. First, the 25% "excess" displaced slums in our archival sample are those with average children's outcomes in the upper part of the outcome distribution (lower bound). Second, the 25% excess displaced slums in our archival sample are those with average children's outcomes in the lower part of the outcome distribution (upper bound). Note that we assume there is no selection on the children we find in the RSH but on the slums. Thus, we trim 13 slums, and the results are presented in Table C.2. The upper bound is very similar to our baseline displacement effect, while the lower bound is expected to be more negative if the trimmed children are those from slums with the highest earnings (or corresponding outcome).

Table C.1: Robustness of displacement impact to attrition by individual

	Model			
	OLS	OLS	Inv-p-score	P-score
	(1)	(2)	(3)	(4)
Panel A. Self-reported earnings (CLP$1,000/month)				
Displacement effect	-17.635	-17.743	-15.696	-14.038
	(3.573)***	(3.424)***	(3.600)***	(5.384)**
Upper bound	-5.069	-6.566	-9.138	-2.427
	(4.081)	(3.779)*	(3.873)**	(5.669)
Lower bound	-49.680	-47.934	-49.742	-47.178
	(2.901)***	(2.847)***	(3.431)***	(5.858)***
Imbens and Manski (2004) CI	[-56.045,-11.169]			
Panel B. Formal wages (CLP$1,000/month)				
Displacement effect	-32.303	-31.252	-28.622	-56.619
	(8.656)***	(8.327)***	(8.465)***	(11.630)***
Upper bound	-20.068**	-19.676**	-21.762**	-44.853
	(8.505)	(8.544)	(9.609)	(12.633)
Lower bound	-102.464	-98.684	-97.367	-114.603
	(7.608)***	(7.336)***	(8.217)***	(9.234)***
Imbens and Manski (2004) CI	[-120.00, -28.577]			
Panel C. Formal earnings(CLP$1,000/month)				
Displacement effect	-17.049	-17.107	-15.289	-17.952
	(3.904)***	(3.769)***	(3.842)***	(5.952)***
Upper bound	-8.975**	-8.672**	-11.868**	-8.781
	(4.074)	(3.920)	(4.778)	(6.054)
Lower bound	-51.457	-50.588	-52.174	-54.282
	(3.375)***	(3.311)***	(4.337)***	(6.170)***
Imbens and Manski (2004) CI	[-59.761,-15.749]			
Panel D. Outcome: Years of schooling				
Displacement effect	-0.640	-0.681	-0.643	-0.813
	(0.140)***	(0.133)***	(0.142)***	(0.146)***
Upper bound	-0.182	-0.233**	-0.315**	-0.432***
	(0.123)	(0.115)	(0.123)	(0.146)
Lower bound	-1.069	-1.084	-1.140	-1.144
	(0.137)***	(0.130)***	(0.125)***	(0.133)***
Imbens and Manski (2004) CI	[-1.179, -0.227]			
Selected individuals	23,814	23,814	23,814	23,814
Trimming portion	7%	7%	7%	7%
Municipality-of-origin FE	✓	✓	✓	✓
Baseline controls		✓	✓	✓

Notes: Column (1) corresponds to the case where the only controls are municipality-of-origin fixed effects. Column (2) adds baseline demographics. Column (3) estimates the regression by propensity score re-weighting, and column (4) is the equivalent to our propensity score matching result in column (4) of Table 4 that fully controls for the interaction between propensity score dummies and municipality-of-origin fixed effects. Clustered standard errors by slum of origin are in parentheses, and Imbens and Manski (2004)'s confidence intervals are produced by Stata's leebounds command. The analysis includes a total of 90 unique slums. 10%*, 5%**, 1%***.

Table C.2: Robustness of displacement impact to attrition by slum

	Model			
	OLS	OLS	Inv-p-score	P-score
	(1)	(2)	(3)	(4)
Panel A. Self-reported earnings (CLP$1,000/month)				
Upper bound	-15.459***	-15.125***	-19.240***	-12.740**
	(3.321)	(3.144)	(3.824)	(5.587)
Lower bound	-22.427***	-21.585***	-23.744***	-15.300***
	(3.913)	(3.626)	(3.516)	(5.449)
Individuals	21,419	21,419	21,419	21,419
Panel B. Formal wages (CLP$1,000/month)				
Upper bound	-28.508***	-26.956***	-28.135***	-21.772
	(7.745)	(7.337)	(9.212)	(13.657)
Lower bound	-42.324***	-39.874***	-41.653***	-66.261***
	(8.876)	(8.531)	(8.943)	(10.356)
Individuals	20,181	20,181	20,181	20,181
Panel C. Formal earnings(CLP$1,000/month)				
Upper bound	-15.686***	-15.164***	-19.026***	-17.242***
	(3.709)	(3.578)	(4.825)	(6.193)
Lower bound	-22.034***	-21.791***	-25.261***	-23.525***
	(3.946)	(3.771)	(4.183)	(6.297)
Individuals	21,395	21,395	21,395	21,395
Panel D. Years of schooling				
Upper bound	-0.459***	-0.493***	-0.653***	-0.435***
	(0.120)	(0.109)	(0.138)	(0.105)
Lower bound	-0.775***	-0.786***	-0.817***	-0.857***
	(0.152)	(0.139)	(0.122)	(0.148)
Individuals	20,641	20,641	20,641	20,641
Unique slums	77	77	77	77

Notes: Column (1) corresponds to the case where the only controls are municipality-of-origin fixed effects. Column (2) adds baseline demographics. Column (3) estimates the regression by propensity score re-weighting, and column (4) is the equivalent to our propensity score matching result in column (4) of Table 4 that fully controls for the interaction between propensity score dummies and municipality-of-origin fixed effects. Sample size varies by outcome because we trim 13 slums, not individuals. Consequently, the size of these trimmed slums also varies depending on the outcome. Standard errors are clustered by slum of origin. 10%*, 5%**, 1%***.

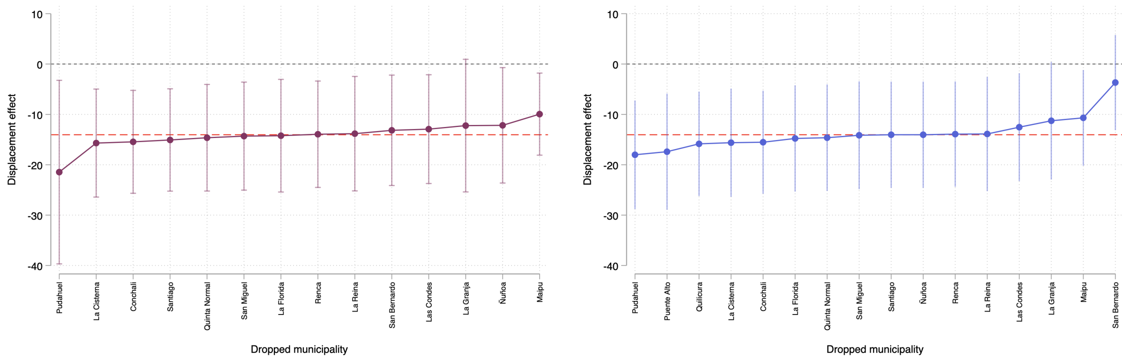
D Additional robustness checks

Table D.1: Displacement effect instrumented by original assignment

Outcome	Labor earnings (1)	Taxable wages (2)	Formal earnings (3)	Informal earnings (4)	Years of schooling (5)
Panel A. OLS					
Displaced	-15.614***	-12.891	-14.136***	-1.478	-0.596***
	(4.494)	(8.932)	(4.486)	(1.582)	(0.199)
Adj. R^2	0.118	0.057	0.063	0.036	0.089
Panel B. Propensity score matching					
Displaced	-11.159**	-6.655	-10.022**	-1.137	-0.338*
	(4.233)	(9.948)	(4.689)	(1.739)	(0.173)
Adj. R^2	0.118	0.063	0.063	0.036	0.095
Panel C. Instrumental variable					
Displaced	-17.851***	-2.560	-13.067***	-4.783*	-0.453**
	(5.086)	(12.159)	(4.666)	(2.480)	(0.218)
Adj. R^2	0.118	0.057	0.063	0.036	0.089
Observations	16,838	16,838	16,838	16,838	16,838

Notes: The table shows regressions for children who were aged 0 to 18 at baseline, matched to the RSH data, and treated between 1981 and 1984. Standard errors are clustered by slum of origin in parentheses. All regressions include municipality-of-origin fixed effects and baseline controls, which include the following: female, mother head of household, married head of household, head of household's age, number of children, firstborn dummy, Mapuche last name, head of household formal employment, year-of-intervention fixed effects, and year-of-birth fixed effects. 10%*, 5%**, 1%***.

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

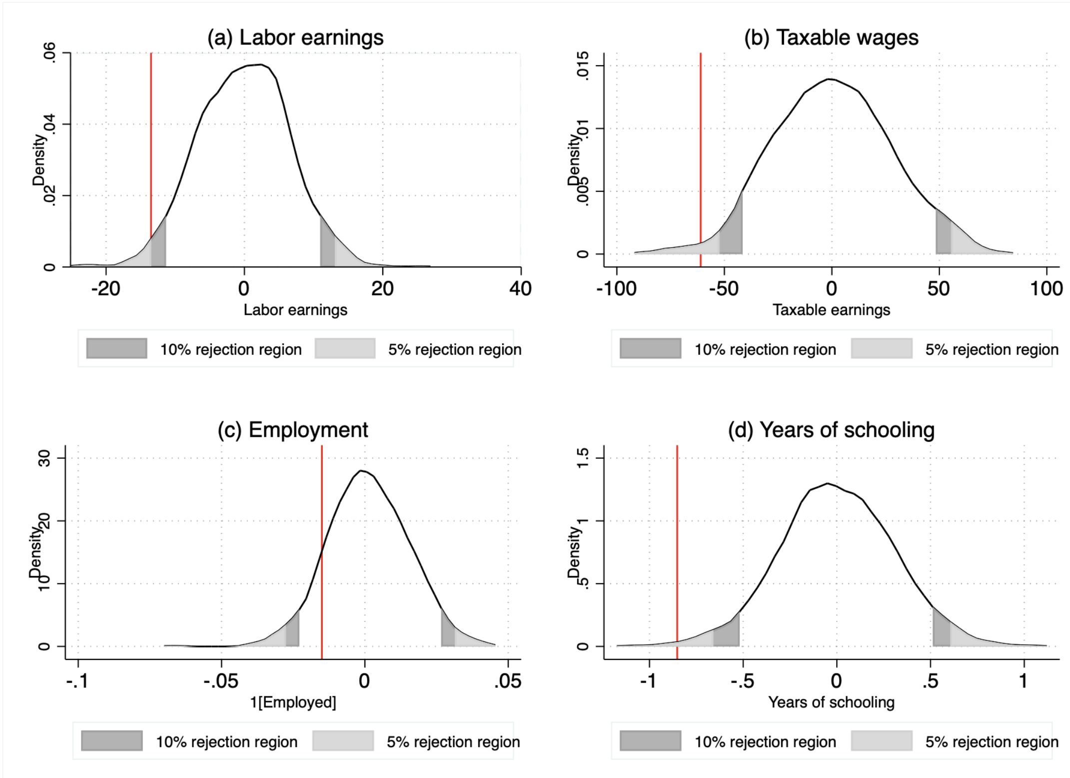


(a) Municipalities of origin

(b) Municipalities of destination

Notes: The figures plot the displacement coefficient in column (4) of Table 4 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 are clustered by slum of origin.

Figure D.2: Permutation tests



Notes: The figures show the distribution of permutations tests on main outcomes performed in 1,000 replications. Red lines indicate the average displacement effect equivalent to column (4) of Table 4. Gray areas indicate 10% and 5% rejection regions.

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

In this 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 the current outcome for individual i at time t, such as labor income or years of schooling, and $s(i)$ indexes the slum of origin for individual i's family. The variable $Displaced_{s\{i\}}$ equals 1 if an individual's family lived in a displaced slum and 0 otherwise. The variable ψ_o are municipality-of-origin fixed effects. The matrix X_{it} includes baseline controls for individual and family 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 accounted for:

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

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

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). We then vary the values of δ and R_{max}, choose $R_{max} = 1.3\tilde{R}$—as recommended by Oster (2019)—and choose $R_{max} = 3\tilde{R}$ as a more conservative case. Last, we vary the value of δ to be 1, 2, or 3. For example, Altonji et al. (2005) assume that $\delta = 1$. D.2 presents the results.

Table D.2: Displacement effect under different assumptions for selection on unobservables

Outcome	R^2 max	$\hat{\hat{\delta}}$	δ	$\hat{\beta}^*$
Labor earnings	1.3	-9.101	1	-15.706
	1.3		2	-17.403
	1.3		3	-19.131
	3	-1.371	1	-25.723
	3		2	-38.932
	3		3	-54.061
Taxable wages	1.3	-17.263	1	-60.454
	1.3		2	-64.373
	1.3		3	-68.385
	3	-2.709	1	-83.809
	3		2	-115.608
	3		3	-153.899
Years of schooling	1.3	-16.983	1	-0.868
	1.3		2	-0.924
	1.3		3	-0.981
	3	-2.633	1	-1.202
	3		2	-1.666
	3		3	-2.242
Included controls:				
Baseline controls				✓