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

Fernanda Rojas-Ampuero[†] Felipe Carrera[‡]

August 4, 2025

Abstract

We examine the difference in long-term impacts of two policies that target urban slums, relocation versus redevelopment on-site, on children's future outcomes. We use evidence from a slum clearance program in Chile between 1979 and 1984, where two-thirds of slum-dwelling families were relocated to housing projects on the city's periphery, and one-third received housing through on-site redevelopment at their original locations. We find that 40 years post-policy, displaced children receive 0.65 fewer years of schooling, earn 10% less, and experience higher labor informality compared to non-displaced children. Longer distances from jobs, disrupted social networks, and relocation to lower-opportunity areas explain the negative displacement effects. As adults, displaced children live in higher poverty areas, but new transportation infrastructure helps reduce the gap between displaced and non-displaced individuals.

Keywords: slum clearance, children, neighborhood effects, forced displacement.
JEL No: H75, I38, J13, N36, O18, R23.

*A previous version of this paper circulated as Rojas-Ampuero's job market paper titled "Sent Away: The Long-Term Effects of Slum Clearance on Children and Families." We are grateful to the editor, co-editor, and anonymous referees for detailed comments that significantly improved the paper. We thank Dora Costa, Michela Giorcelli, and Adriana Lleras-Muney for their continual support and guidance throughout this research project. We also thank Martha Bailey, Natalie Bau, Simon Board, Gilles Duranton, Jesse Gregory, Matthew Khan, Juliana Londoño-Vélez, Rodrigo Pinto, Laura Schechter, Jeffrey Smith, Raimundo Undurraga, Maisy Wong, and participants in various seminars for helpful comments. We thank Ethan Duong, Sebastian Figari, Lucas García, Maeve Giza, Katherine Jones, Alissa Lioznova, Alejandra Martínez, Francisco Norambuena, Vicente Rojas Ampuero, and Yuting Zhou for their help with the data work, digitization process, and statistical analysis. We gratefully acknowledge the contribution of Genealog Chile and Coderhub in the construction of the dataset used in this project. We thank Chile's Superintendence of Social Security for granting access to its administrative data. We also thank Felipe González and Nicolás Figueroa for sponsoring us with Chile's Ministry of Social Development. This research uses information from the Registry of Social Information (RIS). We thank the Undersecretary of Social Evaluation, owner of the RIS, for the authorization to use the databases under the provisions of Exempt Resolution No. 412, of 2019. All the results of this study are our sole responsibility and interpretation and do not commit said Undersecretary in any way. Rojas-Ampuero benefited from facilities and resources provided by the California Center for Population Research at UCLA (CCPR), which receives core support (P2C-HD041022, NICHD). Finally, the authors thank the UCLA Ziman Center for Real Estate's Rosalinde and Arthur Gilbert Program in Real Estate, Finance and Urban Economics, the 2019 LAI-Tinker Field Research Grant, and the Treiman Fellowship, CCPR at UCLA, for generous funding. † Rojas-Ampuero, corresponding author, University of Wisconsin-Madison. Email: rojasampuero@wisc.edu.
‡ Carrera, Reed College. Email: fcarrera@reed.edu.

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 that target informal settlements include on-site slum upgrading (Harari and Wong, 2025), 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 the 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 results in additional challenges.

This paper addresses these issues by examining the difference in long-term impacts of two widely used policy instruments—slum relocation versus redevelopment on-site—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 1984. The program was large in scope, affecting more than 5% of the population of Greater Santiago, the capital of Chile. Through the program, participating slum-dwelling families became homeowners of similar housing units through 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).

To evaluate the long-term effects, we collect archival records of slum dwellers and match them to administrative data to create a novel dataset that follows children and parents from displaced and non-displaced 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 labor earnings and years of schooling. Our final sample contains 33,624 children aged 0–18 who were treated between 1979 and 1984 and who we observe as adults from 2007 to 2023. We estimate that this sample represents 58% of the children aged 0–18 in the original program.

We use variation in the two treatments to estimate a displacement effect, defined as the difference between children from displaced and non-displaced families. 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 as the probability of a slum being relocated versus redeveloped. We then compare displaced and non-displaced children from slums with the same probability of being relocated. Conditional on this probability, we find no correlation between the selection of slums for displacement and children's pre-program characteristics, such as age, gender, family composition, and household employment.

We find that displacement is detrimental for children's outcomes. Compared with non-displaced children, displaced children earn 10.4% less per month, on average. This negative difference 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 in the formal sector. Displacement also reduces children's educational attainment: a displaced child loses 0.65 years of education and is 17% 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 60), we find that the total earnings loss for a displaced child is around US$18,965 at age 60, which is almost twice as large as the cost of the house received by the average family in our sample (US$10,500). We also show that our results are robust to correcting for attrition in the selection of slums found in the archival records and into administrative data.

We next study heterogeneous displacement effects by age at intervention and find that the effect is most pronounced for children aged 0–12 years old at the time of the intervention. Within this group, 0- to 5-year-olds face the most negative effect on formal earnings (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).

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. Importantly, displaced families had no choice in their relocation, limiting potential selection at destination. In our sample, we find that family demographics do not systematically predict the attributes of their destination locations.

To characterize the destination municipalities, we compute upward mobility measures by municipalities as in Chetty and Hendren (2018a), defined as the average income rank of children whose parents are in the 25th percentile of the national income distribution. We find that displaced families were relocated to low-opportunity areas that exhibit lower upward mobility compared to non-displaced families. Additionally, displaced families were relocated to larger public housing projects located farther away from the Central Business District (CBD). Consequently, the homes they received were 13% lower in value compared to non-displaced families, though the housing infrastructure was the same for both treatments.

In our sample, most of the negative effect on earnings is due to new destination locations and network disruption, but we do not rule out non-displaced children benefiting from on-site redevelopment. To further investigate the role of neighborhood change in driving these outcomes, we follow an approach similar to that of Carrillo et al. (2023). We find that children who experience larger negative changes in municipal upward mobility experience larger decreases in their future earnings, with the reduction mainly driven by young children (0- to 5-year-olds) at baseline.

Although upward mobility is a summary measure of neighborhood quality, the variation we observe in the data accounts for only 10% of the average displacement effect. Thus, we explore more granular changes in neighborhoods experienced by displaced families. We find that distance from the slum of origin, increased distance to the CBD, as a proxy for access to employment, the disruption of slum networks, and the size of new public housing projects are good predictors of the difference in earnings between displaced and non-displaced children in our sample. Together with neighborhood quality, these changes explain 64.5% of the point estimate of the displacement effect.

Next, we investigate whether the program had persistent effects on families' locations. We find that 40 years after the program ended, 69% of displaced parents remain in the same destination municipality (compared to a baseline of 59%). Among displaced children, 61% still reside in the same municipality as adults, which is 33% more than non-displaced children. In addition, their neighborhoods are 3% poorer. These results are consistent with the spatial mismatch hypothesis, suggesting that displaced children are "stuck" in low-opportunity areas (Kain, 1968; Kain, 2004).

Finally, motivated by the lower residential mobility of displaced families and lower access to employment in peripheral neighborhoods, we examine whether new subway infrastructure helps decrease the earnings gap between displaced and non-displaced children. 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 negative displacement effect on earnings by 20%–40% in the 10 years after the arrival of a new line. These effects are driven by increased formal employment of individuals working near the new subway stations.

This paper contributes to several strands of literature. First, it adds to the literature evaluating 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 and find positive long-term impacts; Harari and Wong (2025), who study urban renewal on-site in Indonesia and document lower land values and more informality in redeveloped areas; and Gechter and Tsivanidis (2024), who find large positive aggregate effects from redevelopment in India. Almost no research has investigated the effects of slum clearance policies on individuals' human capital.[1] We focus on children and find that relocation versus on-site redevelopment is harmful because it disrupted networks and relocated families far away from their original locations (Barnhardt et al., 2016).

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).[2] Recent studies, such as Camacho et al. (2022) for Colombia and Agness and Getahun (2024) for Ethiopia, study the effects of housing on children. We complement this literature in the context of slums, one of the main forms of shelter in developing countries. We contribute to the understanding of the mechanisms by exploiting variation in destination locations (Damm and Dustmann, 2014) and group movements.

Finally, we contribute to the literature studying cities in the developing world (Glaeser and Henderson, 2017; Bryan et al., 2020), which emphasizes the challenges faced by developing countries due to rapid urbanization and proliferation of slums. For example, Henderson et al. (2021) model the evolution of slums within a city, and Gonzalez-Navarro

[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.

[2] Mogstad and Torsvik (2021) and Chyn and Katz (2021) conduct extensive literature reviews on neighborhood effects, but most of their evidence is from the developed world.

and Undurraga (2023) study slum formation in the context of immigration. We contribute to this literature by studying the consequences of a citywide housing relocation program on individuals. This is a common policy response to tackle the lack of affordable housing, yet there is little causal evidence on its long-run effects (Buckley et al., 2016).

The rest of the paper is organized as follows. Section 2 describes the historical context. Section 3 explains the data collection process, and Section 4 presents the empirical strategy. Section 5 presents the baseline results on income and schooling. Section 6 discusses the mechanisms. Section 7 presents a discussion, 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). 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.[3]

Motivated by this housing crisis, between 1979 and 1984, Chile's Ministry of Housing and Urban Development (MINVU) implemented the Program for Urban Marginality, a massive slum clearance and urban renewal policy. Advocates of the program believed that the most effective way to end poverty was to make poor families homeowners (Murphy, 2015), and the ultimate goal was to clear all slums in the city. At the program's onset, the government conducted a census of slums and targeted 340 of them to be cleared, corresponding to a total of 51,797 families.[4] 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

[3]From 1973 to 1990, Chile was under a military dictatorship headed by Augusto Pinochet. The slums originated as land seizures between 1950 and 1973.

[4]Numbers come from Table 4 in Molina (1986). 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).

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 homeowners.

The features of each intervention are as follows. Non-displaced families accounted for one-third of the total number of families. 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."[5] 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 mostly in the city's peripheral sectors, where they became owners of either a house or an apartment. The land used by the slum was then cleared and repurposed.[6]

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).[7] 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.[8]

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

[6]All families would be evicted, and if they did not want to move, they would be excluded from the program. According to conversations with social workers, most families did not refuse the subsidy because it was their only chance to become homeowners. See the photos in Figure A.2 for an example.

[7]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 2023.

[8]This number is based on our own calculations from archival data on average home values and subsidies, and comparable to estimates in Molina (1986). It is also comparable to the current expenditure in homeownership subsidies in Chile (see the Organisation for Economic Co-operation and Development's website for more details).

Displaced and non-displaced families received houses that were similar in quality and size. Figures A.1 and A.2 show examples of slums in Santiago in the 1970s and the types of houses provided in the destination neighborhoods. Slum dwellers did not choose the type of housing they received but expressed a preference for houses over apartments, as they could be extended (Aldunate et al., 1987). All houses included sewage, electricity, and water, and unit cost varied by location: the more peripheral and larger the project, the lower the cost. In our data we find that the housing units received by displaced families were valued 13% lower than those received by non-displaced families, although housing infrastructure was the same for both treatments.

Decisions regarding the implementation were made directly by the MINVU at the central level.[9] 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. This implied that in some cases, displaced families of a single slum were assigned to more than one housing project; hence, the original slum network was split.[10] 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, or electricity. Land value also mattered; as Rodríguez and Icaza (1993) note, "other criteria included the reputation of the municipality, 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)

[9]Santiago lacked a citywide government. Instead, 30 local municipalities were responsible for managing their respective territories, but 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).

[10]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.

for Las Palmeras, a slum in a low-income municipality. At first, 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 that guaranteed a minimum size per plot, and therefore the 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 slum became a park. Another example involves slum dwellers located on the riverbank of the Mapocho River, who were displaced in 1982 after it flooded. More than 3,000 families from El Ejemplo, El Esfuerzo, and El Trabajo slums—originally located in Las Condes, a wealthy 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 both types of slums had a similar number of families, but displaced slums were denser as they housed fewer families in smaller land areas. They were located in lower-elevation areas with steeper slopes, were closer to rivers or canals, and had a higher risk of flooding. They were also located nearly 1 kilometer (km) closer to the CBD. Additionally, in Panel A we classify slum names as either military related or not as a proxy for support for the dictatorial regime, and we find that displaced slums were less likely to have a military-related name.[11]

Panel B reports attributes of the census districts where slums were originally located to proxy for neighborhood characteristics. We find that displaced slums were located in areas with higher average schooling, lower unemployment rates, slightly higher surrounding property prices, and fewer schools. All these differences are consistent with the historical evidence (Rodríguez and Icaza, 1993).

Figure 1 plots the urban boundaries of Greater Santiago and its municipalities. Panels (a) and (c) depict the location of slums in 1979, showing they were located throughout with no particular concentration in any municipality. Panels (b) and (d) show the location of the housing projects built to receive slum families in 1985. The neighborhoods where housing projects were built for displaced families are represented by purple areas and those for

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

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

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.3 for a summary).

3 Data

We construct a novel dataset that tracks parents and their children, slum of origin, and destination neighborhood. We then match these individual records to administrative data on schooling and labor market outcomes.[12]

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.[13] These records correspond to the lists of homeowners and their spouses who received a property deed through the program. We focus on individuals in Greater Santiago from 14 urban municipalities with variation in treatment (i.e., municipalities with displaced and non-displaced slums). We attempt to collect all the surviving households records, yielding 17,527 unique recipients of social housing with a valid national

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

[13] 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.

identification number (NID). These families come from 98 different slums and were assigned to 73 different destination projects, treated between 1979 and 1984.

The archival data contain information on the recipients of the property deed (heads of household) and their spouses, full names, 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 slum censuses.

Based on the administrative records reported in Molina (1986), around 40,491 families were treated by 1984, of whom approximately 27,419 received a home in urban municipalities. Thus, the families in our sample represent 64% of slum dwellers in the program in urban areas (17,527/27,419), though their slums represent 42% of the slums in the program (98/233).[14] In our archival sample, 12,173 (71%) are displaced and 5,353 (30%) are non-displaced, as opposed to 18,789 (69%) versus 8,630 (31%) in Molina (1986). Thus, we have differential attrition rates by treatment: we find 65% of displaced households but only 62% of non-displaced households in urban areas. This higher proportion among displaced families is due to the presence of larger slums and larger destination neighborhoods (the first row in Table 1). Large destination projects often contained multiple slums of origin, while non-displaced slums typically corresponded one-to-one with destination projects.

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.[15] The birth certificates contain the children's full name at birth, birth date, NID, 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.[16] We identify 15,032

[14]We use Molina (1986) as our primary source for household records because the totals per project we find in the archives coincide one-to-one with her numbers; however, the author does not provide a list of non-displaced slums, only the aggregates. Therefore, we use Morales and Rojas (1986) as our primary source for slum-level data. However, their totals per slum differ from Molina (1986) because they collect data from newspapers, which may be more prone to measurement error. Additionally, their number of non-displaced slums is overestimated, as they count subdivisions of larger slums as separate slums, hence the number 98/233 is likely a lower bound for our matching rate.

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

[16]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, each parent's paternal last name is transmitted to their children. For example,

families with at least one child (the rest did not have children), corresponding to a total of 46,310 children in 98 slums. Of these, 33,624 were aged 0–18 years at the time of treatment. This is our baseline sample. Figure C.1 shows a summary of the data collection process.

Using the 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, marital status, and place of birth. 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.[17] Finally, we measure parents' formal employment at the slum level between 1975 and 1980, using historical records from Chile's Superintendency of Pensions.[18]

3.3 Measuring outcomes: Matching to administrative data

We match our full sample of children and parents to several administrative data sources using NIDs. Our main source of data is the Social Household Registry (Registro Social de Hogares, RSH), 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 90% of all Chilean households voluntarily enroll in it. We have access to biannual data from June 2007 to December 2023, which includes information on self-reported income, employment status, and schooling, as well as family composition and dwelling characteristics.

We also merge individuals with the Administradora de Fondos de Cesantía (AFC). The AFC is an employer-employee dataset used by the Superintendency of Pensions to administer unemployment insurance for all workers in the private sector. Hence, any worker in the system is formally employed in the private sector.[19] We observe monthly data on taxable income from November 2002 to December 2023. We use this dataset to measure formal employment; thus, if a person is not in the AFC, we can confidently say she is not

assume that María Pérez Rojas has a child with Juan Rodríguez González. Their child's family name will be Rodríguez Pérez. See the supplementary material for a full explanation of the process.

[17]The Mapuche Data Project is a collective effort to collect historical information about the Mapuche population. The available data can be accessed here.

[18]The Superintendency of Pensions does not provide researchers with individual-level data. However, since we have access to individuals' NIDs, 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.

[19]The AFC represents approximately 90% of formal employment and 72% of total employment in Chile.

formally employed and her taxable wage is zero.

We find 91.2% of children in the archives in the RSH, with a matching rate of 92.2% for displaced children and 88.7% for non-displaced children (Table 2, Panel B). These, combined with the attrition from the archives, imply that the final matching rate from the full program to the RSH is 60% for displaced children (0.92*0.65) and 55% for non-displaced children (0.89*0.62). In section 5 we discuss how these differences may bias our results.

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. In addition, we obtain publicly available data from Greater Santiago's subway system on subway stations built in the city. Finally, we compute a neighborhood-level property price index from newspaper listings from 1978 to 1985 that we collect and digitize. We estimate the residuals of a hedonic regression that accounts for property size and type of dwelling, and then compute the property index as the logarithm of the average residuals within a 2 km buffer around the centroid of each slum or destination project.

4 Empirical strategy

4.1 Identifying a displacement effect

To estimate the impact of 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 children of displaced families with those of non-displaced families who come from slums with the same probability of being relocated. Slum assignment to relocation or on-site upgrading did not depend on household characteristics but rather on the feasibility of upgrading the slum 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 relocation. Thus, any differences between children in the displaced and non-displaced groups are attributed to the eviction and relocation processes.

Note that the comparison we make is between two treatments, relocation versus upgrade, allowing us to estimate a displacement effect. This estimate is not the effect of the program, as we would need a control group of families who remained in slums, but we do not observe given the nature of our data. Nevertheless, the displacement effect is still of policy interest because it compares the effects of two widely used policies that target urban slums.

We estimate a linear model using the following specification:

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

where Y_i is the average outcome for individual i in adulthood, such as labor income, employment status, and years of schooling. $s(i)$ 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 if a non-displaced slum. 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, which is a function of slum characteristics X_s (see Table 1). We include the interaction $\psi_o \times p(X_s)$, to flexibly capture differences in relocation probabilities within municipalities. 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's formal employment by slum, and year-of-intervention fixed effects (1979 to 1984) that control for aggregate temporal differences across the years this housing program was in effect.[20] We cluster the standard errors by slum of origin; however, in Section 5 we show robustness to other clustering methods.

Estimating a propensity score model requires the unconfoundness assumption to hold, meaning 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 is at the slum level rather

[20]In our archival sample, we do not have variation in treatment for the year 1984, as we only found displaced individuals in the archives and did not find records of families treated in 1985. Thus, we combine 1984 with 1983 when estimating year fixed effects since we only observe displaced families in 1984.

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 the control function approach where we control directly for $\hat{p}(X_s)$ and its interactions with ψ_o, instead of nearest neighbor or 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.

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 by treatment status. In these data, we observe 233 slums with information on their characteristics (columns (1) and (2) of Table 1). We estimate the probability of relocation using a logit function on slum characteristics, but to avoid overfitting, we use a LASSO model where we include all the variables in Table 1 (see Appendix B for a full description). We exclude from the model the price index because it could reflect expectations of future land prices due to slum clearance.

LASSO selects density, elevation, slope, flooding risk, and average schooling in the census district. Interestingly, most of these variables reflect the feasibility of providing sewage, electricity, and water on-site. The estimates from this exercise are presented in column (4) of Table B.1, and the densities of the fitted values by treatment are reported in Figure B.1. The estimates of the propensity score vary between 0.1 and 0.9, and as expected, displaced slums have a higher propensity of relocation compared to non-displaced slums. Importantly, there is common support between 0.20 and 0.65. Column (4) of Table 1 reports the difference in slum characteristics after controlling for the fitted values of the propensity score, $\hat{p}(X_s)$, within the common support. The results show balance in slum characteristics between treatments, with 12 slums excluded from the full sample due to high estimated values of $\hat{p}(X_s)$.

Columns (5) and (6) of Table 1 show characteristics for the 98 slums in our archival sample, and column (7) reports the simple difference between treatments. In the archival sample, the differences between treatments are smaller than in the full sample, suggesting that these slums are more similar to each other. The data show that displaced slums are more likely to be larger in terms of the number of families, located closer to the CBD, and at lower risk of flooding. As noted in the data section, the families in the archival records come from larger destination projects, which is consistent with the presence of larger slums.

Because the slums in our sample are not a random sample of the universe of slums in the program, we use the estimates from the LASSO regression in the full sample of 233 slums to predict the probability of slum relocation in our archival sample of 98 slums. This approach aims to increase statistical power and reduce selection on observables. Figure B.2, panel (a) presents the densities of the fitted values of the propensity score in our archival sample. As expected, because displaced and non-displaced slums are more similar to each other, the predicted densities are also more similar between treatments. In particular, they do not include slums with high probabilities of treatment above 0.7, implying that when we impose common support, only 4 out of the 98 slums are excluded—mainly those with a high risk of flooding.

Additionally, to account for the non-randomness of our archival sample, we compute sampling weights estimated as the inverse probability of finding a slum in the archives, stratified by treatment.[21] Panel (b) of Figure B.2 plots the re-weighted propensity score densities. The weighted archival sample is more similar to the full sample, as it places higher weight on displaced slums with a high probability of relocation and on non-displaced slums with a low probability of relocation. Later in the paper, we return to the use of these weights as a robustness check for our baseline results on children.

We implement the propensity score method in three steps. First, we estimate the propensity score $\hat{p}(X_s)$ at the slum level using a LASSO-logit function in the sample of 233 slums. Second, we restrict the sample to have common support in the 98 slums in the archives. Based on the propensity score densities by treatment in Figure B.2, we keep slums where $0.23 < \hat{p}(X_s) < 0.60$: from the 98 slums in our archival sample, 94 are in the common support. Third, we run equation (1) on the outcomes of interest where $p(X_s)$ is included as a continuous variable $\hat{p}(X_s)$ and interacted with municipality-of-origin fixed effects ψ_o. This ensures that we compare displaced and non-displaced children within the same municipality with similar values of the probability of relocation.[22]

[21] We describe the construction of the weights in Appendix B.

[22] A more strict approach would be to perform a block propensity score by municipality of origin (Heckman et al., 1998). This is not possible with our data, as it would require a larger number of slums per

Finally, to provide evidence that our matching procedure guarantees a balanced sample of slum characteristics before the intervention, in columns (4) and (8) of Table 1 we report the difference between displaced and non-displaced slum attributes controlling for the estimated propensity score. The results show that matching generates a more balanced sample of slums in both the full and archival samples.

4.3 Estimation sample and summary statistics

The estimation sample includes children from municipalities with both displaced and non-displaced slums in urban areas, drawn from the sample of households in the archives. Table 2 presents summary statistics for children at the time of the intervention. Column (1) reports statistics for the full sample of children aged 0–18 at baseline. Thirty-seven percent are firstborn, 50% are female, their average age is 8.12 years, their parents are 34.8 years old at baseline, and families have an average of four children. Additionally, 33% come from female-headed households, 88% have parents who are married or cohabiting at the time of the intervention, 6% have a Mapuche last name, and they come from slums where 40% of heads of households were formally employed before treatment and 46% migrated from outside the Greater Santiago area before age 18. Finally, 2% had parents who lost a child under five before treatment.

Among the children in our sample, 91.2% are found in the RSH. Table A.1 shows that displacement and gender predict the probability of finding a child in the administrative data. We find 0.9 percentage points more displaced children and 1.4 percentage points more female children. This is consistent with the fact that women are more likely than men to request social benefits. Our concern about bias in the estimates arises from the over-representation of female children, particularly if the gender distribution is unbalanced between the treatment groups or if gender affects outcomes differently. In the next subsection we show that this is not the case.

The children in our baseline sample are also representative of those living in slums in 1982. Table A.2 reports demographics for children aged 0–18 in the 1982 Population Census who lived in Greater Santiago. Of these children, 19% lived in a slum, and among them, 61% attended school, and their average age was 8.3 years. Their parents were, on average, 37 years old, and 88.8% were married or cohabiting. Importantly, children living in slums

municipality to estimate a different propensity score density in each municipality of origin.

came from households that were more vulnerable compared to the rest of the population in Santiago, as they have lower educational attainment, and their parents had lower levels of employment and education, and were more likely to live in female-headed households.

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 relocated. Under the assumption that conditional on the policy function $p(X_s)$ and municipality of origin o, the covariance between $Displaced_{s\{i\}}$ and ε_i is zero, the coefficient β estimates the displacement's causal effect on children's outcomes as the difference between relocation and upgrade on-site within origin o.

We first compare the demographics of displaced and non-displaced children at the time of the intervention. Columns (2) and (3) of Table 2, Panel A report means for the demographics of children in the sample with common support for the non-displaced and displaced groups, respectively. Column (4) reports the difference between groups conditional on the propensity score and municipality of origin ($\hat{p}(X_s) + \psi_o + \hat{p}(X_s) \times \psi_o$). Based on these adjusted differences, displaced and non-displaced children with similar probabilities of relocation have similar demographics at baseline, with no statistical differences between both groups for 16 out of 19 observables. Displaced children are 5.6 percentage points more likely to have married parents at baseline and 1.1 percentage point more likely to have a Mapuche (Indigenous) last name. While this latter difference is large, the share of children with a Mapuche last name is small. The final statistically significant difference, though small, is mother's schooling, which should be interpreted with caution, as it is measured after 2007 and is affected by attrition.[23]

The results are very similar and even more balanced for children matched to the RSH (columns (5)–(7)). The difference in marital status remains significant, but the other two differences disappear and become smaller in absolute value. The difference in mother's age also becomes slightly significant. As noted earlier, female children are over-represented in the RSH; however, baseline demographics are not unbalanced between treatment groups, indicating that this over-representation is not due to their demographic characteristics.

[23] Rojas-Ampuero (2022) shows that displacement positively affects parents' mortality, and thus the difference in years of education measured in the long run is subject to a displacement effect.

Overall, children in the RSH do not appear systematically different from those in the full sample. Importantly, all 94 slums are retained in our matched sample.

Note that when estimating the propensity score, we targeted balance in slum characteristics before treatment, not in children's demographics. Thus, this table provides evidence that our methodology also ensures balance in baseline demographic variables not explicitly targeted by the method.

5 Results

5.1 Displacement effect on new location attributes

To estimate the program's displacement effects on new location 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 estimates of upward mobility by municipality of destination.[24] The analysis shows that displaced households were more likely to be relocated to areas where upward mobility is 1.41 points lower, or 14% less, compared to those of non-displaced households.

We observe even larger differences in other neighborhood attributes. Panel (b) plots densities for distance to the CBD, showing that displaced households are 2.85 km farther away from the CBD, from a baseline of 14.7 km. Panel (c) shows that they also experience longer commuting times. These patterns consistently align with the fact that compared to non-displaced households, displaced households were relocated to lower-opportunity areas: they ended up in locations with 19% higher unemployment rates (panel (d)), 7.8 km farther from their slums of origin (panel (e)), and in larger public housing projects (panel (f)). Table A.4 summarizes these differences.

5.2 Displacement effect on labor market outcomes

We continue our analysis by examining the earnings and employment of individuals with non-missing education information (aged 0–18 at baseline) who were 25–60 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

[24] Upward mobility is computed as the average income rank of children born between 1985 and 1990, whose parents are in the 25th percentile of the national income distribution. These estimates are computed as in Chetty and Hendren (2018a). See Appendix Section E for a full description of the methodology.

RSH between 2007 and 2023. Self-reported earnings measure income from both formal and informal employment, which include wage income and proprietor labor income but exclude pensions and transfers.[25] Labor earnings are measured in 1,000 Chilean pesos per month (CLP$1,000/month), equivalent to approximately US$1 per month. We compute one observation per individual by collapsing each outcome after controlling for age and semester-year dummies.

Table 3 shows that displacement has a negative effect on earnings (Panel A) and a null effect on employment (Panel B). Column (1) reports the simple difference in outcomes between displaced and non-displaced children conditional on year-of-treatment fixed effects,[26] indicating that displaced children earn CLP$30,622 less per month, which is 12.5% less than non-displaced children (see the row labeled "Percent effect"). Column (2) adds municipality-of-origin fixed effects ψ_o, showing the importance of comparing children within the same municipality; the estimate is reduced to –24.787. In column (3), we include the estimated propensity score $\hat{p}(X_s)$ as a control, which further reduces the negative displacement coefficient. Compared to column (1), the results decrease substantially from –30.622 to –24.377, equivalent to a 10.2% decrease in the earnings of displaced children compared to non-displaced children. Column (4) adds the interaction between municipality fixed effects and the propensity score, yielding results that are very similar to those of column (3). Taken together, comparing children with similar probabilities of relocation within the same origin reduces selection by about 20%.

Finally, in column (5) we estimate the displacement effect from equation (1), where we add baseline demographics, and the results are very similar to those of column (4). The coefficient of –24.992 on labor earnings, statistically significant at the 1% level, implies that displaced children earn 10.4% less than non-displaced children in adulthood. This column is our preferred specification as it flexibly accounts for differences in the outcomes of displaced and non-displaced children with similar probabilities of being relocated within a municipality of origin.

For comparison, all columns in Table 3 report Conley standard errors in brackets to account for any spatial dependence across slums that are close to each other (Conley,

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

[26] Treatments were not balanced across time, as relocations were more common after 1982.

1999).[27] The Conley standard errors yield very similar results to clustering by slum of origin. Thus, in all of the following estimations, we report clustered standard errors.

Table 4 presents the results of displacement effects on employment and education outcomes. Panel A shows that as adults, displaced children are 3.5 percentage points less likely to work with a contract and 3.1 percentage points more likely to work in temporary jobs, equivalent to 9.6% less and 4.8% more than non-displaced children, respectively. They are also 2 percentage points less likely to work in the formal sector, defined as being found in the AFC, which corresponds to 5.6% less than non-displaced children.

Panel B splits self-reported earnings into formal and informal sources (with and without a contract). The results show that the negative effect observed in Panel A of Table 3 is mostly driven by lower earnings in the formal labor market (–14.7%), while the effect is smaller and not significant on informal earnings (–2.8%). The last row of Panel B includes the displacement effect on taxable wages, which are observed through social security contributions in the private sector in the AFC between 2007 and 2023. These contributions, by definition, measure formal earnings.[28] Consistent with the negative effect of displacement on formal self-reported earnings, we find an even larger displacement effect of –43.622, which is similar in percentage points to the effects on the previous measures of earnings. The –12.4% displacement effect on taxable wages indicates that displaced children are not more likely than non-displaced children to under-report their earnings in the RSH.[29]

5.3 Schooling outcomes

Next, we study the displacement effect on schooling outcomes. The results, shown in Panel C of Table 4, indicate that displaced children obtain 0.648 fewer years of schooling than non-displaced children. The negative percent effect increases with higher levels of

[27] We use a 4-km cutoff distance to calculate the 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.5. For estimating the standard errors, we consider different cutoffs ranging from 1 to 10 km. The upper bound is set to 10 km as this includes the largest municipality in Santiago in terms of square km. We also report bootstrapped standard errors for comparison in the same appendix table.

[28] We observe data in the AFC starting in 2002 but use the same years as in the RSH for comparability purposes. Additionally, the AFC system started in 2002 but did not include all firms immediately. Instead, firms and their workers joined the system gradually until 2008, when all private firm workers in Chile had access to the unemployment insurance system.

[29] Discrepancies between reported earnings in the RSH and the AFC can be attributable to several factors, such as under-reporting, or the timing of the report. The AFC is a monthly dataset, while the RSH is biannual.

education: displaced children are 15.5% less likely to graduate from high school, 25.2% less likely to attend a two-year college (for technical degrees such as mechanics and electrical technology), and 39.2% 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 education by reducing their likelihood of graduating high school, and hence their likelihood of attending college is even lower.

5.4 *Labor market outcomes across the age cycle*

We take advantage of the panel structure of the RSH and the AFC to estimate a displacement effect on children's future earnings across the age cycle (Figure 3). We find that across the entire age distribution, the income trajectories of displaced children are below those of non-displaced children, with a negative earnings difference as early as age 27, both for self-reported earnings (upper panels) and taxable wages (lower panels). However, the effect on formal wages decreases with age.

Figure A.6 presents employment trajectories and displacement effects on formal and informal earnings separately. The results confirm the findings of Figure 3, as the negative effects are reflected in formal earnings and formal employment (with a contract), and the difference in informality is reduced between displaced and non-displaced as they age.

5.5 *Attrition and sample selection*

A main concern regarding the validity of our results is the representativeness of our sample, especially given the attrition present in the archives. As explained in the data section, the construction of our baseline sample is subject from two levels of attrition: at the slum level (archival data) and at the individual level (administrative data). In this section, we discuss how each of these sources of attrition could affect our baseline results.

5.5.1 *Sampling weights*

In Section 4.2, we discussed how the slums in the archival data were not representative of the full sample of slums in the program. To overcome this, we computed sampling weights so that the distribution of the propensity score estimates in the archival sample was similar to the full sample of slums (Figure B.2, panel (b)).

We apply these sampling weights by slum to our baseline sample of children and perform two exercises. First, we check whether the balance of demographics in Table 2 changes when we re-weight the sample (Table A.3). Second, we estimate the displacement effect on children's earnings as adults in the re-weighted sample (Table C.1). Table A.3 shows that in the weighted sample, the demographics of children at baseline are even more balanced between treatments, with only parental marital status remaining statistically different from zero. As shown in Table C.1, the displacement effect in the weighted sample is more negative and very similar to our baseline estimate in Table 3, column (5).

5.5.2 Lee bounds

As discussed in Section 3, the combination of attrition from archival data to the RSH leads to a final matching rate of 60% for displaced children and 55% for non-displaced children in the RSH. Hence, to show that differential attrition is not driving our results, we compute Lee bounds (Lee, 2009) by trimming the 8.3% of excess attriters among displaced children ((60–55)/60). Table C.2 presents lower and upper bounds for the displacement effect that replicate the models in Table 3 on total labor earnings, taxable wages, and schooling.[30] While attrition is high in our sample, these results do not suggest that differential attrition explains our findings. In all cases, the upper and lower bounds are negative, in most cases statistically different from zero, and they always contain the displacement effect for the corresponding sample. We also use sampling weights as in the previous subsection and find very similar results. Alternatively, we repeat this exercise using slums as the level of observation to account for the over-representation of displaced slums in our archival sample and find robust results (see discussion in Appendix C and results in Table C.3).

5.6 Robustness checks

5.6.1 Variations to the propensity score method and subsamples

We examine the robustness of the estimated displacement effect to changes in the propensity score method and to restrictions on common support. Table A.6 presents estimates under a

[30] 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 parts of the outcome distribution, following McKenzie and Sansone (2019)'s procedure.

range of alternative specifications. Column (2) presents robustness of the baseline result on earnings and education when we estimate the displacement effect using inverse propensity score re-weighting. Columns (3)–(5) present robustness of the results when we trim the common support of the propensity score. Both the displacement effect in levels and in percentage terms are very similar to the baseline estimate.

Column (6) excludes three municipalities with low overlap of the propensity score between treatments, and column (7) excludes from the sample cells with no variation in treatment, where a cell is defined as the combination of a municipality of origin and whether the propensity score is above or below the median (see Figure B.3). Column (8) estimates the propensity score using all slum characteristics, without using LASSO. Finally, column (9) uses estimates of the propensity score from the sample of slums not found in the archives (out-of-sample estimation).

Next, we examine whether 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 wealthiest 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 wealthiest in our sample) change our effects by a large magnitude.

5.6.2 Selection on unobservables

In the previous sections we provided evidence of no selection on observables, conditional on the policy function and municipalities of origin. However, some concerns arise if our identification strategy does not account for unobserved selection. For example, we do not observe other characteristics of slum families at baseline, such as their relationship with local authorities. Political considerations are also relevant due to potential 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 were ultimately 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.[31] Table D.1 shows that the instrumental variable coefficient is very similar to our propensity score estimate on 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.2). 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; Nakamura et al., 2022; Carrillo et al., 2023). 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 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 age groups: 0–5, 6–12, and 13–18.[32] 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.

Additionally, panel (a) shows no age gradient in informal earnings, and 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 small negative effect on earnings in adulthood, while for children under 13 years old at baseline, the displacement effects become more negative with age.

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

[32] We choose these three groups after performing a structural break test for each age from 0–18, 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 Figure A.7 for more details.

In Figure A.4, we include a separate group of individuals who were aged 19–21 at the time of the intervention. We do this to test for selection, as older individuals who were less likely to live with their parents should not have been affected by displacement. However, because they were older, we are less likely to find them working in the RSH or the AFC, so the estimates for this group are noisier. We find no effects of displacement for this age group on taxable wages and a negative but noisy selection effect on formal earnings. If we bound the effects by selection as in Chetty and Hendren (2018a), we still find a negative displacement effect for the youngest children in our sample.[33]

The results suggest the existence of exposure effects. The richness of our data allows us to differentiate these effects by types of earnings, revealing that the negative exposure effect primarily influences children's future formal earnings. One explanation is that informality is a negative function of education (e.g., Perry et al., 2007); thus, if displacement reduces schooling, this may help explain the results. Another possibility is that informal employment tends to be more local, so displaced individuals may face reduced access to formal job opportunities. In Section 6, we explore these differences in more detail.

5.8 *Displacement effect by demographic groups*

While the displacement effects by demographic group may vary (Figure A.5), we find no systematic large differences across other demographic characteristics other than gender and age. We find more negative displacement effects on total labor earnings for men than for women, but not on taxable wages, suggesting that displaced boys earn less than non-displaced boys as adults in the informal sector.

6 Mechanisms

In this section we investigate the mechanisms behind the baseline results on earnings. Due to 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, followed by exploring whether improvements in the transportation system reduce the displacement effect.

[33] This refers to the difference in coefficients between each age group and the 19–21 age group.

6.1 Attributes of destination locations

Because the displacement effect is the difference between two treatments, the negative effect could reflect non-displaced children benefiting from improved locations after clearance and redevelopment.[34] While we do not rule out this possibility, in this subsection we provide evidence that the displacement effect on earnings may instead be due to changes in the environments of displaced children.[35]

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.8 presents the distribution of the estimates on labor earnings, showing variation by municipality. We then correlate these different displacement effects with a measure of change in neighborhood quality for children from origin o. If a non-zero correlation is found, it indicates that the displacement effect is likely a function of neighborhood change. The validity of this exercise relies on the idea that displaced families were forced to move to a particular location.[36]

To measure neighborhood quality at the municipality level, we follow the methodology of Chetty and Hendren (2018a), and calculate upward mobility measures by neighborhood of residence. Upward mobility is computed as the average rank in the national income distribution for a child with parents in the 25th percentile of the income distribution, for children born between 1985 and 1990, averaged by municipality of residence. We estimate these using the RSH data for the whole Chilean population (see Appendix Figure E.3).

Panel (a) of Figure 5 presents the results of correlating the displacement effect with the change in upward mobility between destination and origin municipalities. Larger changes in upward mobility (more negative) are associated with larger (more negative) displacement effects. Thus, a negative displacement effect is more likely to be associated with a reduction in a neighborhood's upward mobility. Similarly, panel (b) presents the results of correlating

[34] 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.10.

[35] We find evidence of spillover effects on non-displaced children. Results are in Tables A.7 and A.8.

[36] 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.9) from our sample of families who moved, finding no evidence that family characteristics predict their final destinations.

the adult earnings of displaced children with the level of upward mobility in destination municipalities. The result is a positive relationship (i.e., displaced children relocated to areas with higher upward mobility have higher earnings as adults on average). The figure also shows that the predicted earnings of displaced children using the demographics of non-displaced children (gray triangles) display almost zero correlation with destination characteristics. Figure A.9 shows similar patterns when measuring neighborhood quality as distance to the CBD. These results suggest that the variation explaining the displacement effect may be due to relocation.

We generalize the results in Figure 5 by estimating an equation similar to equation (1) but in the spirit of Chetty and Hendren (2018a), where we replace the displacement dummy for the change in a child's environment, using the following equation:

$$Y_i = \alpha + \delta \Delta \text{Upward Mobility}_{do'} + \gamma \text{Upward Mobility}_{o'} + \psi_o + p(X_s) + \psi_o \times p(X_s) + X_i'\theta + \varepsilon_i, \tag{2}$$

where ΔUpward Mobility$_{do'}$ is the difference between upward mobility between municipality of destination d and municipality of origin o'. We add upward mobility in the municipality of origin o' as a baseline control. Importantly, o' and d are smaller geographical divisions than o, so the origin fixed effects ψ_o are identified.[37] All other variables are measured as in equation (1). The parameter of interest is δ, which measures the effect of increases in neighborhood quality on children's earnings, relative to the origin. Note that ΔUpward Mobility$_{do'} = 0$ for non-displaced children; hence, δ is identified from the changes experienced by displaced children.

Table 5 presents the results of estimating equation (2). Column (1) shows that moving to a better neighborhood, measured as an increase of 1 in upward mobility, increases children's earnings in CLP\$1,918 per month. The average reduction in upward mobility in our sample is −1.284 (see auxiliary regressions in Table A.10), implying a decrease of CLP\$2,463 per month. This effect is statistically significant but small, as it represents only 10.4% of the average displacement effect after conditioning on neighborhood quality at origin ($\frac{-2{,}463}{-23{,}824} = 10.3\%$). The variation in upward mobility in our sample is also small,

[37]Strictly speaking, o is measured as municipalities in 1980, while o' and d are measured as municipalities in 1985, after a reform in municipal divisions that took place in 1980. Greater Santiago went from 17 municipalities to over 30.

likely because municipalities might be too large a representation of a neighborhood, or because the children in our sample might be affected by other location attributes not captured by upward mobility. Hence, to account for more granular neighborhood measures, we investigate whether other changes in children's environments explain the variation in earnings. We focus on three sets of variables that have been documented in the literature and are also motivated by families' impressions in Aldunate et al. (1987): remoteness of the new locations (Barnhardt et al., 2016; Picarelli, 2019), disruption of slum networks, and characteristics of the new public housing projects, such as project size and home value (Newman, 1973). We omit other variables like unemployment or number of schools because they highly correlate with measures of upward mobility at the municipality level.

Columns (2)–(6) of Table 5 show that children relocated far from their origin (column (2)) or farther from the CBD (column (3)) experience a reduction in future earnings. Children who move with their entire slum network also perform better compared to those whose slum is split (column (4)). Finally, children relocated to larger destination neighborhoods (column (5)) or whose new homes have a lower value (column (6)) experience a decrease in their adult labor earnings.

These estimates have the expected signs and contribute to the effect of the change in upward mobility on children's earnings, suggesting they better explain the variation in our data. Importantly, in our setting, displacement is a function of different changes that correlate with each other. For example, municipalities with lower upward mobility tend to be more peripheral (Figure E.3) and have a greater share of families living in large public housing projects with lower-value homes. Thus, identifying the treatment effect of each component separately would require additional quasi-experimental variation. Nevertheless, we can perform an accounting exercise as proposed by Gelbach (2016).

In column (7), we include all changes in location attributes simultaneously. The resulting estimates are noisier and smaller but remain in the expected direction. These imply that the difference in earnings between displaced and non-displaced children accounted for by these changes is –15.356, or 64.5% of the displacement effect after controlling for neighborhood quality at origin.[38] Based on these results, we find that 22.5% of the point estimate of the displacement effect is explained by distance from the slum of origin and distance

[38] The computation is the sum of the products between column (7) of Table 5 and the row of displacement coefficients in the auxiliary regressions in Table A.10.

to the CBD, 32% by neighborhood size and home value, and 10% by network disruption (see Table A.10 for the full decomposition). This implies that displacement entails both a disruption effect and a lack of opportunity, measured by changes in neighborhoods.[39]

Finally, Table A.12 shows estimations of column (7) of Table 5 for different types of children's earnings as adults and schooling. The results show that formal earnings and educational attainment are more sensitive to changes in location attributes. In particular, earnings with a contract and taxable wages are lower for children relocated farther away from the CBD or when their slum network was split.

To understand why formal earnings are more sensitive to location, we use data from current employment surveys (ENE, 2023) to compare commuting patterns between formal and informal workers. We find that in Greater Santiago, formal workers commute longer distances compared to informal workers (19.9 km versus 8.8 km). In addition, among formal workers, 85.5% work in a different municipality than their residence, and for informal workers this number is only 14.4%. This suggests that informal work is highly local. This pattern is not unique to Greater Santiago, and it has been documented in other Latin American contexts such as Mexico City (Zárate, 2024) and Bogotá (Tsivanidis, 2025). If access to better employment is far from peripheral neighborhoods, displaced children will be affected negatively the farther they are relocated. This is consistent with our results and with the spatial mismatch hypothesis, suggesting that displaced children are "stuck" in places that offer worse employment opportunities (Kain, 2004).

6.2 Changes in neighborhood attributes by age at intervention

We previously showed an age gradient of displacement. In this section, we further explore whether an age gradient exists in the neighborhood effects. To do so, we run regression (1) in Table 5, stratifying the change in upward mobility by age group (0–5, 6–12, and 13–18).[40] Figure 6, panel (a) reports coefficients δ by age group, and panel (b) does the same but only includes displaced individuals. We find an age gradient in the effect of a change in neighborhood quality on children's earnings. Children below the age of 6 (blue circles) are

[39]In Table A.11, we examine the distance variables only and find that the displacement effect is larger the farther children relocate away from their origin. In contrast, the negative effect does not increase for children who relocate to more remote areas, measured by distance to the CBD. However, these two variables are highly correlated, and hence it is not possible to estimate their separate effects in our setting.

[40]This exercise is very similar to Carrillo et al. (2023), who study the effect of relocation in the context of apartheid in South Africa.

most affected by a change in neighborhood quality, and we can reject the equality of coefficients between the first group and the other two.[41] The effect remains almost unchanged but is reduced in levels if we run column (7) stratified by age (purple diamonds) instead of column (1) (i.e., after controlling for other changes in location attributes). Although access to employment and networks are important determinants of children's earnings, the overall quality of a neighborhood is especially important for younger children, consistent with what the previous literature has documented (Chetty et al., 2016; Carrillo et al., 2023).[42]

6.3 Children's long-run locations

Our previous analysis shows that children's future labor earnings are affected through changes in their environments when they relocate. The next step is investigating where these children currently live. To do so, we estimate a displacement effect on current locations between 2016 and 2023 and on the poverty rate of these neighborhoods.

We start by examining the likelihood that the parents in our sample will remain in their assigned neighborhoods. Table 6, Panel A shows that displaced parents are more likely to live in their assigned municipality compared to non-displaced parents (column (1)); although the estimate is noisy, the percent effect is sizable (17.9%). They are also equally likely to live in their assigned neighborhood (column (2)) and do not return to their municipalities of origin (column (3)). These results suggest that displaced parents have lower mobility, as they are more likely to remain in their assigned municipalities, and when they do move, they move nearby (column (4)). In terms of poverty rates, displaced parents' current neighborhoods are 1.8% poorer than those of non-displaced parents, but this effect is not statistically significant.

Additionally, displaced households are not more likely than non-displaced households to sell their homes (see Table A.13). This is likely due to the constraints imposed by the program, as families could not sell until they have paid for the full amount, which, according to our data, was on average 27 years after treatment.[43]

[41]In Figure A.11 we add children aged 19–21 and find null effects for this group.

[42]We test whether there is an age gradient on distance to CBD and networks. While we do not find an age pattern, all children are affected similarly. This is not shown in the paper.

[43]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.13, indicate that only 5% of these families sold their house by 2019, after 27 years on average, with no statistical differences between displaced and non-displaced families. We partnered with Santiago's Real Estate Registrar to track families' addresses in our archival data.

We continue the analysis by examining the current locations of children, now adults, in Panel B of Table 6. Displaced children are much more likely than non-displaced children to live in their municipality of assignment (column (1)). Consequently, they do not return to their municipalities of origin (column (3)), and they live closer to their parents' neighborhood of assignment (column (4)). Finally, as adults, displaced children live in neighborhoods that are 2.7% poorer than those of non-displaced children (column (5)). Panel C presents these patterns by age at baseline and shows no systematic differences.

6.4 Improvements to transportation

Motivated by our previous results on the impacts of displacement on children's earnings and the lower spatial mobility of displaced families, we explore the influence of transportation improvements on reducing the displacement effect. Specifically, we examine the impact of new metro lines introduced in Santiago between 2010 and 2023 to study whether the construction of a new station close to a family's assigned location impacts displaced and non-displaced children differently.[44] To do so, we exploit the timing and location of the new subway stations, interacted with a child's displacement status, in the form of a triple-difference event-study regression. We estimate the following equation:

$$Y_{it} = \sum_{\tau=-3}^{10} \gamma_\tau Displaced_{s\{i\}} \cdot Subway_d \cdot 1[t=\tau] + \sum_{\tau=-3}^{10} \lambda_\tau Subway_d \cdot 1[t=\tau] + \delta Subway_d$$
$$+ \beta Displaced_{s\{i\}} + \psi_o + p(X_s) + \psi_o \times p(X_s) + X_i'\theta + \alpha_t + \varepsilon_{it}, \quad (3)$$

where $Subway_d$ is a dummy variable that equals 1 if a new subway station is built within 2 km of a family's neighborhood of assignment d, and equals 0 if the station is built between within 2 and 5 km. This variable measures if the subway station is close or far from their neighborhood.[45] Coefficients α_t are calendar-year fixed effects, and all the other variables remain the same as in equation (1). The coefficients of interest are γ_τ, which measure the difference in outcome Y (wages or employment) between displaced and non-displaced children τ years after the arrival of a subway station that is close to a family's neighborhood.

[44]Four new lines were introduced during this time period, in 2010, 2011, 2017, 2019, and 2023. See the maps in Figure A.12 for the geographic variation.

[45]We choose 2 km as the cutoff based on estimates of Chile's Ministry of Transportation, considered to be the maximum walkable distance to a subway station, and an upper bound of 5 km to include as many neighborhoods as possible in the sample.

Figure 7 presents the results. It shows that constructing a subway station close to their parents' assigned neighborhood leads to an improvement in displaced children's future earnings, primarily due to increases in formal employment and taxable wages. The changes result in a 20%–40% reduction in the present value of the displacement effect on earnings for children near these new subway stations (see Table A.14).[46] Once again, we see that formal earnings are more sensitive to location changes, partly because the AFC data are more precise (measured monthly), making it more likely that we detect an effect.

Based on Zárate (2024)'s model, subway access impacts both formal and informal employment through two different channels: a supply effect, allowing individuals to commute longer to search for better employment, or a demand effect, as a new subway line brings more job opportunities to the new locations. In the AFC we observe employers' municipalities, so we can estimate effects on commuting distances for formal workers. Table A.15 presents the results. It shows that displaced children who are closer to the subway are more likely to remain in the municipality of assignment, more likely to work formally in their municipality of residence, and commute shorter distances. These results are consistent with a larger demand effect favoring displaced children more and are similar to Zárate (2024)'s findings for the case of a subway expansion in Mexico City. He finds that the arrival of the subway to peripheral neighborhoods creates more formal and informal jobs in the surrounding areas, but formal jobs increase more. For Santiago, Asahi (2015) finds that new subway lines positively impact employment in new destination municipalities.

7 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 60 years, and using an annual discount rate of 4%, the average displaced child in our sample loses US$10,202 by the age of 40 (relative to a non-displaced child).[47] This is practically the same as the cost of the housing unit received by a family through the

[46] The average age a child received the subway is 35. We compute the present value of the subway gain under two assumptions: the gains last for 10 years, or the gains last until a worker retires at age 60. See discussion in Section 7.1 for the computation of the present value.

[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.

program in our sample (equivalent to US$10,500). By the age of 60, the total loss is almost twice as large and equal to US$18,965. For children who have access to the subway infrastructure as adults, the loss could be reduced to US$11,360, under the assumption that the effects of the subway are permanent.

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.[48] Note that while this estimate is the difference in earnings between relocation versus upgrade, it does not account for the total change in earnings due to the program. However, this comparison is still relevant as it helps inform policy on which alternative might be preferred.[49]

7.2 Comparison of estimates with other settings

We compare the magnitude of our estimates with other studies in developing countries that provide low-income families with homeownership subsidies under different allocation schemes (see Table A.16 for comparisons). Most studies that estimate effects on earnings are for adults using lotteries that induce relocation. For example, Barnhardt et al. (2016), Kumar (2021), and Franklin (2020) study housing lotteries in India and South Africa. Our results are similar to that of Barnhardt et al. (2016), who find a decrease of 7.7% in household income for adults moving to isolated areas, though their neighborhoods are better quality. Belchior et al. (2024) study the Minha Casa, Minha Vida program in Brazil. They find negligible effects in their baseline sample but a 7.7% increase in earnings among disadvantaged households who have access to better employment opportunities. Our results for children are in the range of –15% on formal earnings and –10% on total earnings, which are similar or larger than those of previous settings.

More recent papers focusing on children study the effects on schooling outcomes in the short and medium term. Camacho et al. (2022) study the effects of public housing in Colombia and find that children from families who win a housing lottery and move to better areas have a 17% higher probability of graduating from high school, driven by access to better schools. Agness and Getahun (2024) study a similar lottery in Ethiopia; while the

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

[49]For example, one could argue that access to better housing causes positive health effects on children (Cattaneo et al., 2009). Thus, the total effect of the program in our setting is unknown.

lottery winners' destination neighborhoods are farther from the CBD, they have higher-quality amenities. The authors find positive effects on children's educational enrollment in the range of 4.5%–11% and a 10.5% increase in secondary school completion, driven by families who occupy their new homes. In this paper, we find that displaced children have a 17% lower probability of graduating high school and a larger negative effect on enrollment in tertiary education (25.2%–39.2%), effects that are similar to those of previous settings.

The total cost of the program we study is large—equivalent to 0.25% of Chile's GDP (Molina, 1986)—and comparable in scope to the housing programs analyzed by Barnhardt et al. (2016) and Camacho et al. (2022). Compared to the latter, however, the unitary cost of a home in our setting is half of that in the Colombian case. Slum clearance programs continue to be implemented in Chile at similar or higher costs (Gertler et al., 2025), though now in a democratic context in which families are more involved in the process.

8 Conclusion

This paper presents new evidence on the long-term impact of relocating to isolated neighborhoods relative to staying in central areas. The novelty of our paper lies in the construction of a dataset that tracks slum-dwelling families and their children to estimate the long-term impact of relocation versus upgrade. Our results show that relative to non-displaced children, displaced children complete 0.65 fewer years of education, earn 10.4% less income, and are 5.6% less likely to work in the formal labor market. The analysis of mechanisms suggests that forced relocation to large public housing projects negatively affects children, as their new neighborhoods are of lower quality. However, we also find that group relocation is desirable, as keeping slum networks together helps reduce the negative incidence of displacement, and access to infrastructure helps reduce the earnings gap between treatments.

Despite their high costs, international organizations generally advocate for on-site housing over relocation (UN-Habitat, 2020). Our findings support this view, suggesting that relocation without accompanying public infrastructure may have negative consequences. Nevertheless, more empirical evidence is needed to compare policy alternatives, including compensation schemes (Lall et al., 2006), disruption effects, and access to public services. Finally, given the scale of these programs, future research could explore their general equilibrium effects on surrounding communities.

References

Abadie, A. and Imbens, G. W. (2016). Matching on the estimated propensity score. *Econometrica*, 84(2):781–807.

Agness, D. and Getahun, T. (2024). "housing and human capital: Condominiums in ethiopia". *Working Paper*.

Aldunate, A., Morales, E., and Rojas, S. (1987). Evaluación social de las erradicaciones: Resultados de una encuesta. *Programa FLACSO*, (96).

Altonji, J., Elder, T., and Taber, C. (2005). Selection on observed and unobserved variables: Assessing the effectiveness of catholic schools. *Journal of Political Economy*, 113(1):151–184.

Anderson, M. L. (2008). Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects. *Journal of the American Statistical Association*, 103(484):1481–1495.

Asahi, K. (2015). *Impacts of Better Transport Accessibility: Evidence from Chile*. PhD thesis, The London School of Economics and Political Science.

Barnhardt, S., Field, E., and Pande, R. (2016). Moving to opportunity or isolation? Network effects of a randomized housing lottery in urban India. *American Economic Journal: Applied Economics*, 9(1):1–32.

Baum-Snow, N. (2007). Did Highways Cause Suburbanization? *The Quarterly Journal of Economics*, 122(2):775–805.

Belchior, C. A., Gonzaga, G., and Ulyssea, G. (2024). "who benefits from social housing? experimental evidence from a large-scale program in brazil". *Working Paper*.

Benavides, L., Morales, E., and Rojas, S. (1982). Campamentos y poblaciones de las comunas del Gran Santiago. Una síntesis informativa. *Documento de Trabajo Programa FLACSO-Santiago*, (154).

Britto, D., Fonseca, A., Pinotti, P., Sampaio, B., and Warwar, L. (2025). "intergenerational mobility in the land of inequality". *Working Paper*.

Bryan, G., Glaeser, E., and Tsivanidis, N. (2020). Cities in the developing world. *Annual Review of Economics*, 12(Volume 12, 2020):273–297.

Buckley, R. M., Kallergis, A., and Wainer, L. (2016). The emergence of large-scale housing programs: Beyond a public finance perspective. *Habitat International*, 54:199–209. Housing the Planet: Evolution of Global Housing Policies.

Busso, M., DiNardo, J., and McCrary, J. (2014). New evidence on the finite sample properties of propensity score reweighting and matching estimators. *The Review of Economics and Statistics*, 96(5):885–897.

Camacho, A., Duque, V., Gilraine, M., and Sanchez, F. (2022). The Effects of Free Public Housing on Children. *NBER Working Paper*, (30090).

Carrillo, B., Charris, C., and Iglesias, W. (2023). Moved to poverty? a legacy of the apartheid experiment in south africa. *American Economic Journal: Economic Policy*, 15(4):183–221.

Cattaneo, M. D., Galiani, S., Gertler, P. J., Martinez, S., and Titiunik, R. (2009). Housing, health, and happiness. *American Economic Journal: Economic Policy*, 1(1):75–105.

Celedón, A. (2019). Operación piloto: Santiago en tres actos. *Revista 180*, 43:1–12.

Chetty, R. and Hendren, N. (2018a). The impacts of neighborhoods on intergenerational mobility I: Childhood exposure effects. *The Quarterly Journal of Economics*, 133(3):1107–1162.

Chetty, R. and Hendren, N. (2018b). The impacts of neighborhoods on intergenerational mobility II: County-level estimates. *The Quarterly Journal of Economics*, 133(3):1163–1228.

Chetty, R., Hendren, N., and Katz, L. F. (2016). The effects of exposure to better neighborhoods on children: New evidence from the Moving to Opportunity experiment. *American Economic Review*, 106(4):855–902.

Chetty, R., Hendren, N., Kline, P., and Saez, E. (2014). Where is the land of opportunity? The geography of intergenerational mobility in the United States. *The Quarterly Journal of Economics*, 129(4):1553–1623.

Chyn, E. (2018). Moved to Opportunity: The long-run effects of public housing demolition on children. *American Economic Review*, 108(10):3028–56.

Chyn, E. and Katz, L. F. (2021). Neighborhoods Matter: Assessing the Evidence for Place Effects. *Journal of Economic Perspectives*, 35(4):197–222.

Conley, T. (1999). GMM Estimation with Cross Sectional Dependence. *Journal of Econometrics*, 92(1):1–45.

Damm, A. P. and Dustmann, C. (2014). Does growing up in a high crime neighborhood affect youth criminal behavior? *American Economic Review*, 104(6):1806–32.

Field, E. (2007). Entitled to work: Urban Property Rights and Land Labor Supply in Peru. *The Quarterly Journal of Economics*, 122(4):1561–1602.

Franklin, S. (2020). Enabled to work: The impact of government housing on slum dwellers in South Africa. *Journal of Urban Economics*, 118:103265.

Galiani, S., Gertler, P. J., Undurraga, R., Cooper, R., Martinez, S., and Ross, A. (2017). Shelter from the storm: Upgrading housing infrastructure in Latin American slums. *Journal of Urban Economics*, 98.

Gechter, M. and Tsivanidis, N. (2024). Spatial spillovers from high-rise developments: Evidence from the mumbai mills. *Working Paper*.

Gelbach, J. B. (2016). When do covariates matter? and which ones, and how much? *Journal of Labor Economics*, 34(2):509–543.

Gertler, P., Gonzalez-Navarro, M., Undurraga, R., and Urrego, J. A. (2025). "in-situ upgrading or population relocation? direct impacts and spatial spillovers of slum renewal policies". *Working Paper*.

Glaeser, E. and Henderson, J. V. (2017). Urban economics for the developing world: An introduction. *Journal of Urban Economics*, 98:1–5. Urbanization in Developing Countries: Past and Present.

González, F., Muñoz, P., and Prem, M. (2021). Lost in transition? The persistence of dictatorship mayors. *Journal of Development Economics*, 151:102669.

Gonzalez-Navarro, M. and Undurraga, R. (2023). Immigration and slums. *Working Paper*.

Harari, M. and Wong, M. (2025). Slum Upgrading and Long-run Urban Development: Evidence from Indonesia. *Working Paper*.

Heckman, J., Ichimura, H., Smith, J., and Todd, P. (1998). Characterizing Selection Bias Using Experimental Data. *Econometrica*, 66(5):1017–1098.

Henderson, J. V., Regan, T., and Venables, A. J. (2021). Building the city: From slums to a modern metropolis. *The Review of Economic Studies*, 88(3):1157–1192.

Hidalgo, R. (2019). *La Vivienda Social en Chile y la Construcción del Espacio Urbano en el Santiago del siglo XX*. RIL Editores.

Imbens, G. W. and Manski, C. F. (2004). Confidence intervals for partially identified parameters. *Econometrica*, 72(6):1845–1857.

Instituto Nacional de Estadísticas (INE) (1970). XIV Censo Nacional de Población y III de Vivienda.

Instituto Nacional de Estadísticas (INE) (1982). XV Censo Nacional de Población y IV de Vivienda.

Instituto Nacional de Estadísticas (INE) (2023). Encuesta Nacional de Empleo 2023.

Kain, J. F. (1968). Housing Segregation, Negro Employment, and Metropolitan Decentralization. *The Quarterly Journal of Economics*, 82(2):175–197.

Kain, J. F. (2004). A Pioneer's Perspective on the Spatial Mismatch Literature. *Urban Studies*, 41(1):7–32.

Kumar, T. (2021). The housing quality, income, and human capital effects of subsidized homes in urban india. *Journal of Development Economics*, 153:102738.

Labbé, F. J., Llévenes, M., et al. (1986). Efectos redistributivos derivados del proceso de erradicación de poblaciones en el Gran Santiago. *Estudios públicos*, (24).

Lall, S. V., Lundberg, M. K., and Shalizi, Z. (2006). Implications of alternate policies on welfare of slum dwellers: Evidence from Pune, India. *Journal of Urban Economics*, 63(2008):56–73.

Lee, D. S. (2009). Training, wages, and sample selection: Estimating sharp bounds on treatment effects. *Review of Economic Studies*, 76:1071–1102.

McKenzie, D. and Sansone, D. (2019). Predicting entrepreneurial success is hard: Evidence from a business plan competition in nigeria. *Journal of Development Economics*, 141.

Michaels, G., Nigmatulina, D., Rauch, F., Regan, T., Baruah, N., and Dahlstrand, A. (2021). Planning Ahead for Better Neighborhoods: Long-Run Evidence from Tanzania. *Journal of Political Economy*, 129(7):2112–2156.

MINVU (1979). Ministerio de Vivienda y Urbanismo: Campamentos Año 1979: Radicación-Erradicación.

Mogstad, M. and Torsvik, G. (2021). Family Background, Neighborhoods and Intergenerational Mobility. Working Paper 28874, National Bureau of Economic Research.

Molina, I. (1986). El Programa de Erradicación de Campamentos en la Región Metropolitana de Santiago (1979-1984): Implicancias Socioeconómicas y Espaciales.

Morales, E. and Rojas, S. (1986). Relocalización socio-espacial de la pobreza: Política estatal y presión popular, 1979-1985. *Programa FLACSO*, (280).

Murphy, E. (2015). *For a Proper Home: Housing Rights in the Margins of Urban Chile, 1960-2010*. University of Pittsburgh Press.

Nakamura, E., Sigurdsson, J., and Steinsson, J. (2022). The Gift of Moving: Intergenerational Consequences of a Mobility Shock. *Review of Economic Studies*, 89(3):1557–1592.

Newman, O. (1973). *Defensible space: Crime prevention through urban design*. Collier Books New York.

Oster, E. (2019). Unobservable Selection and Coefficient Stability: Theory and Evidence. *Journal of*

Business & Economic Statistics, 37(2):187–204.

Perry, G., Maloney, W., Arias, O., Fajnzylber, P., Mason, A., and Saavedra-Chanduvi, J. (2007). Informality: Exit or exclusion. *The World Bank: Washington, D.C.*

Picarelli, N. (2019). There Is No Free House. *Journal of Urban Economics*, 111:35–52.

Rodríguez, A. and Icaza, A. M. (1993). Procesos de expulsión de habitantes de bajos ingresos del centro de Santiago, 1981–1990. *Proposiciones Ediciones SUR*, 22.

Rojas-Ampuero, F. (2022). *Sent Away: The Long-Term Effects of Slum Clearance on Children and Families.* PhD thesis, University of California, Los Angeles.

Rosenbaum, P. R. and Rubin, D. B. (1983). The Central Role of the Propensity Score in Observational Studies for Causal Effects. *Biometrika*, 70(1):41–55.

Trivelli, P. (2009). Mercado de suelo urbano area metropolitana de santiago, boletines 1989-2009.

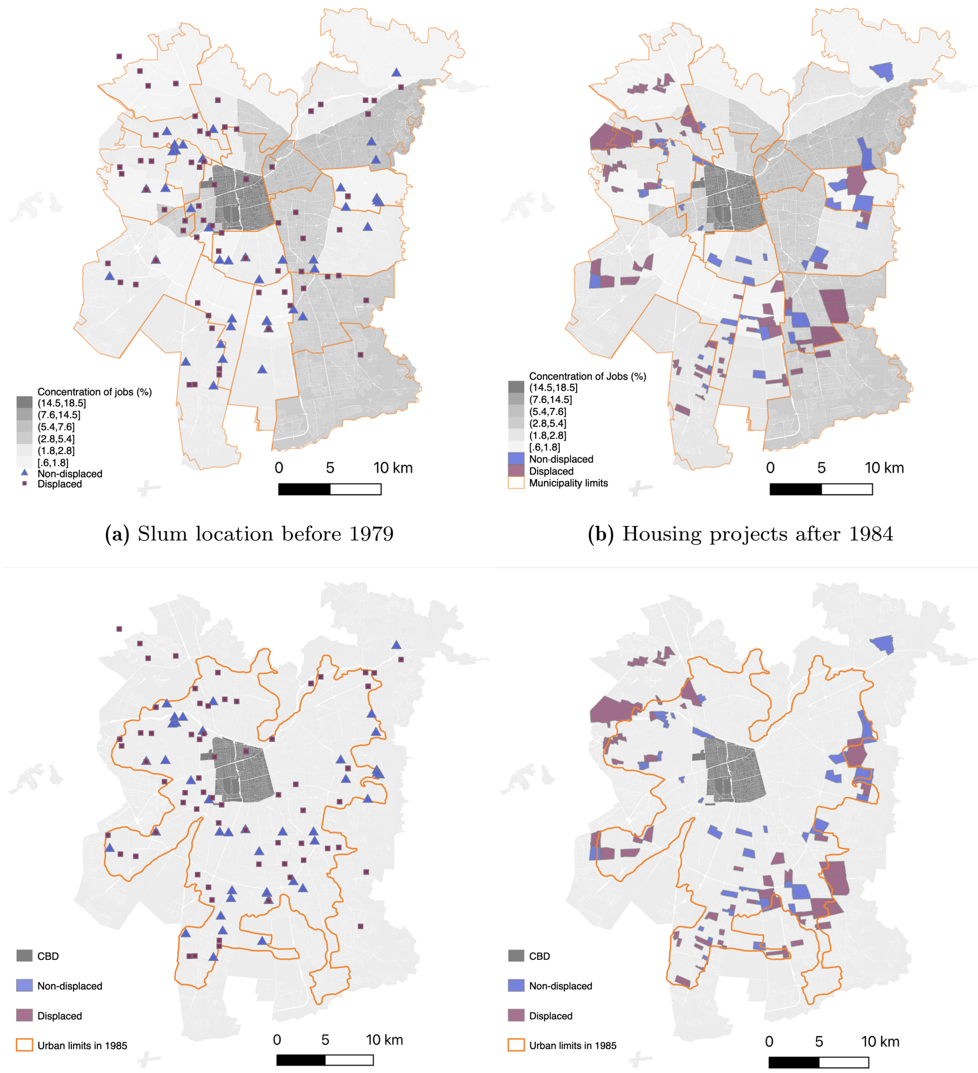
Tsivanidis, N. (2025). Evaluating the Impact of Urban Transit Infrastructure: Evidence from Bogotá's TransMilenio. *Conditionally Accepted at American Economic Review.*

UN-Habitat (2020). World Cities Report 2020: The Value of Sustainable Urbanization. Technical report, UN-Habitat.

Zárate, R. D. (2024). Spatial misallocation, informality and transit improvements: Evidence from mexico city. *Working Paper.*

Figures and Tables

Figure 1: Eviction policies 1979–1984: Locations of families living in slums



(a) Slum location before 1979

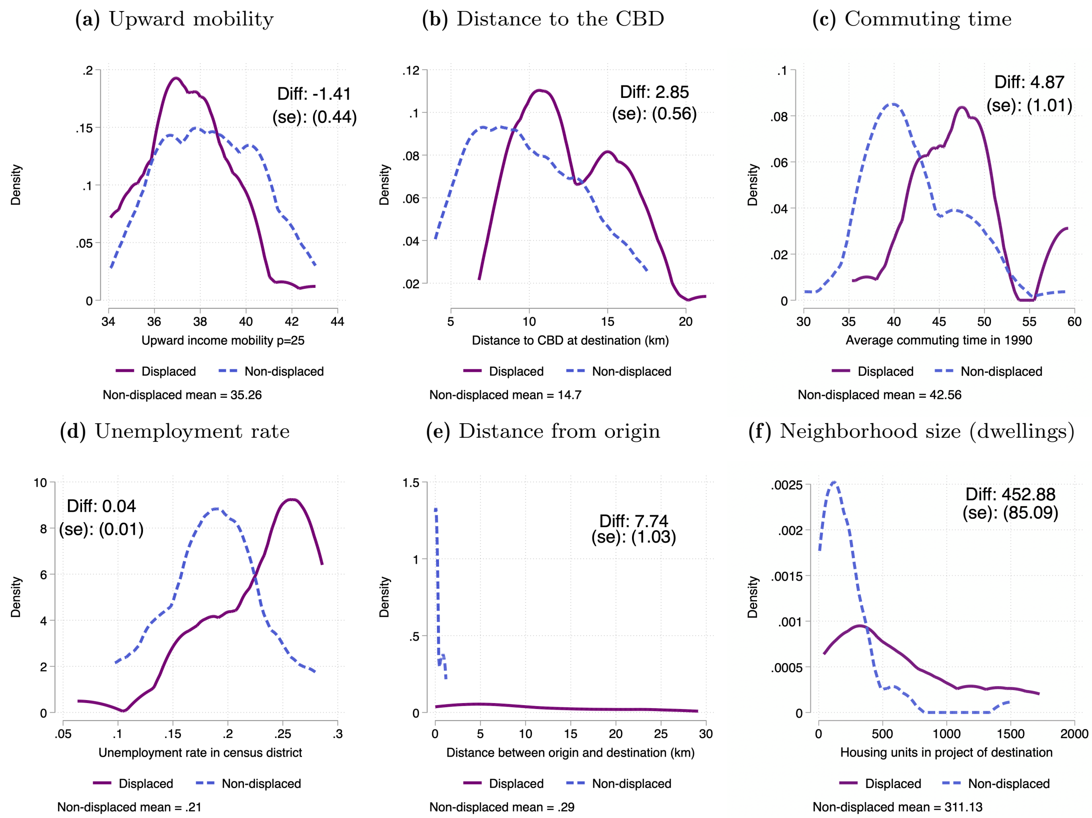
(b) Housing projects after 1984

(c) Slum location before 1979

(d) Housing projects after 1984

Notes: The figure shows changes in the locations of families living in slums in 1979 (panels (a) and (c)) and their final destinations in 1984 (panels (b) and (d)). The orange lines in the upper panels represent the municipality boundaries in 1980, while in the lower panels they indicate the urban boundaries of Greater Santiago. Municipalities are colored in grayscale 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 the 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 wealthiest 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.

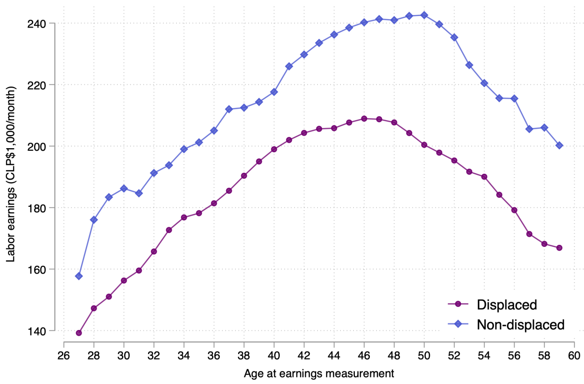
Figure 2: Density of neighborhood attributes after relocation



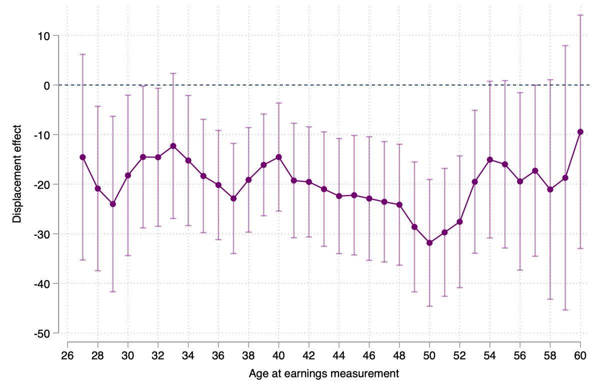
Notes: The figure shows densities by treatment for the average neighborhood attributes of each pair of slum of origin and destination project in the archival sample (N = 112 unique slum-project pairs). Each panel shows the average difference in treatments labeled as "Diff," and each footnote indicates the mean for the non-displaced households, conditional on the propensity score. The sample includes all households within the common support regardless of whether a child is present.

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

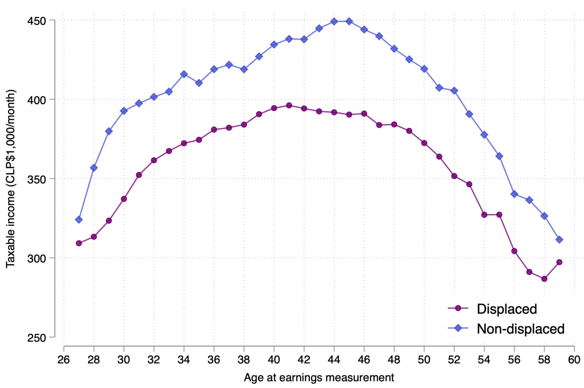
(a) Labor income trajectories (CLP$1,000/month)



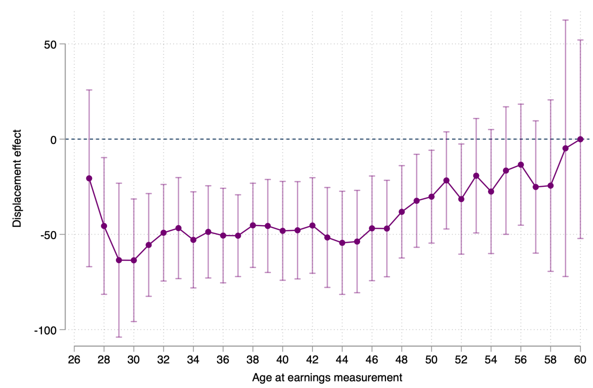
(b) Displacement effect on labor income



(c) Taxable wage trajectories (CLP$1,000/month)



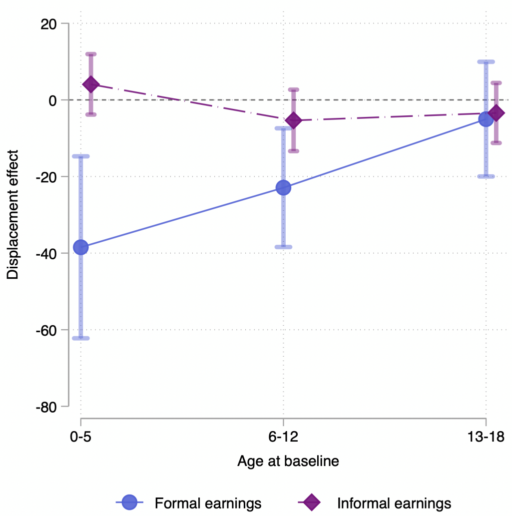
(d) Displacement effect on taxable wage



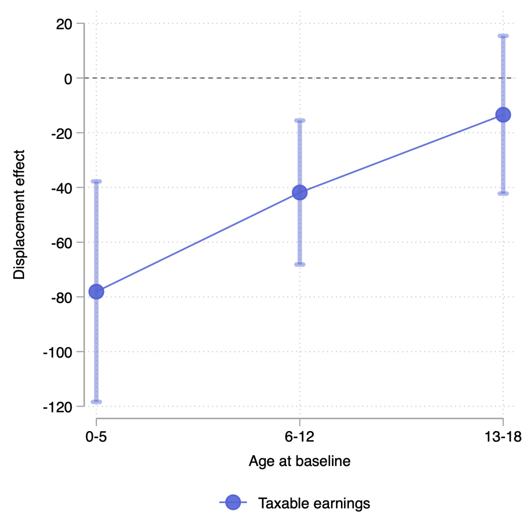
Notes: The figure shows regressions for children aged 0–18 at baseline who are matched to the RSH and AFC data. Panels (a) and (c) plot the predicted trajectories for displaced and non-displaced children between ages 25 and 60 from the regression $y_{it} = \sum_{\tau=25}^{60} \beta_\tau Displaced * 1[Age = \tau] + \sum_{\tau=25}^{60} \delta_\tau 1[Age] + \psi_o + \hat{p}(X_s) + \hat{p}(X_s) \times \psi_o + X_{it}'\gamma + u_{it}$. Panels (b) and (d) plot coefficients β_τ and their 95% confidence intervals for each corresponding outcome. Displacement effects by age on other employment outcomes are available in Figure A.6. Standard errors clustered by slum of origin are reported in parentheses. Coefficients for ages 25 and 26 are omitted in the figures because of the large confidence intervals. 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's formal employment, year-of-birth fixed effects, and year-of-intervention fixed effects.

Figure 4: Displacement effects on earnings by age at baseline

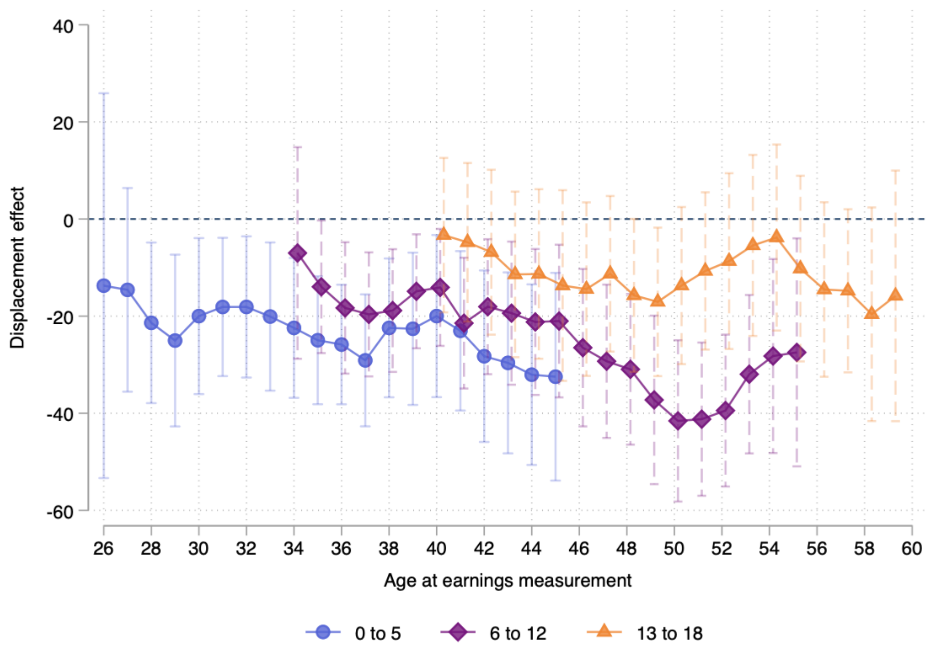
(a) Labor earnings



(b) Taxable earnings



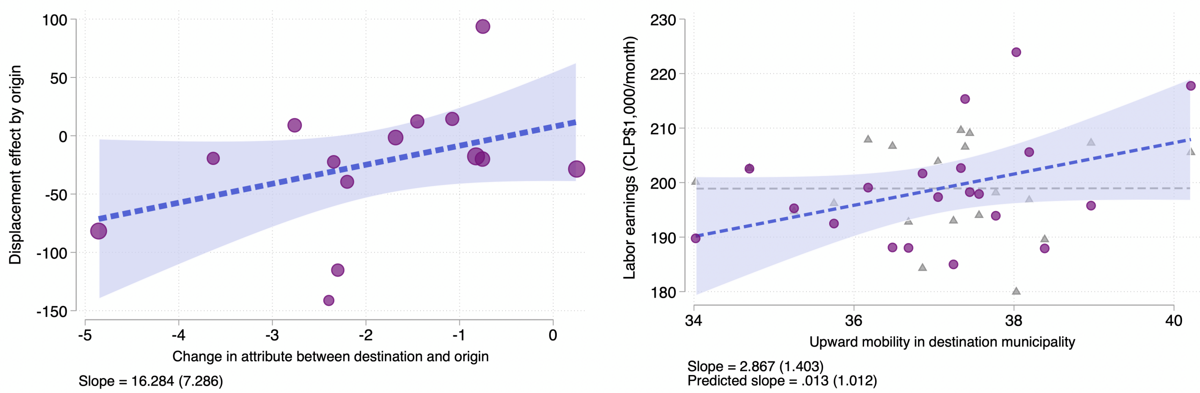
(c) Labor earnings by age



Notes: The figure shows regressions for children aged 0–18 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 $y_{it} = \sum_{g=1}^{3}\sum_{\tau=25}^{60}\beta_{\tau g} Displaced * 1[Age = \tau, Group = g] + \sum_{g=1}^{3}\sum_{\tau=25}^{60}\delta_{\tau g} 1[Age = \tau, Group = g] + \psi_o + \hat{p}(X_s) + \hat{p}(X_s)\times\psi_o + X_{it}'\gamma + u_{it}$, where g stands for an age group in [0,5], [6–12], or [13–18] at the time of the intervention. Standard errors clustered by slum of origin are reported in parentheses. 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's formal employment, year-of-birth fixed effects, and year-of-intervention fixed effects.

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

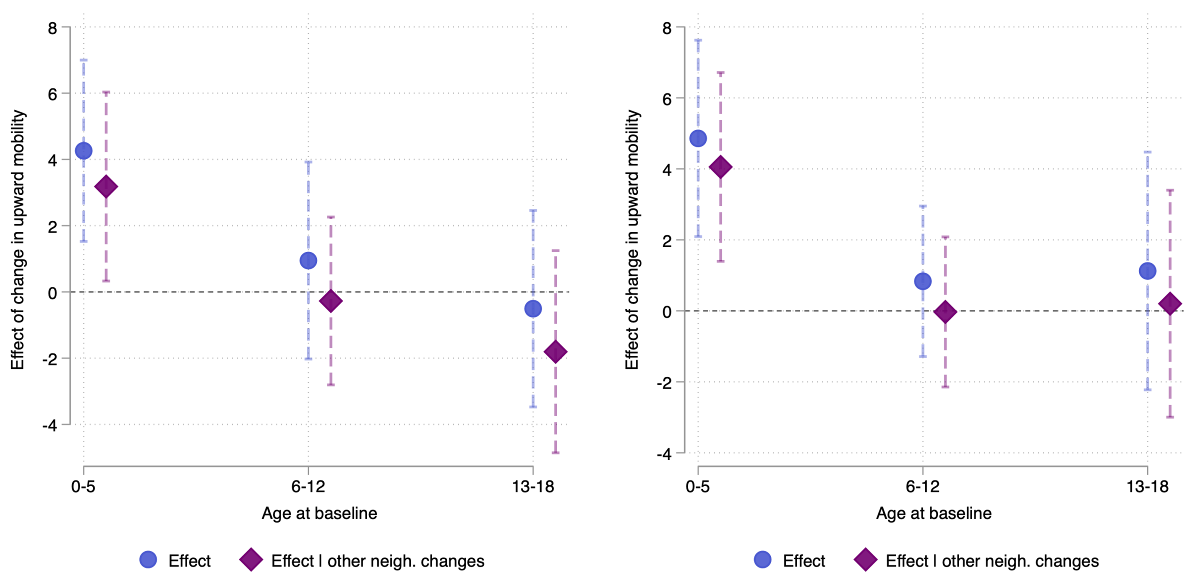
(a) Displacement effect **(b)** Movers' earnings



Notes: Panel (a) plots displacement coefficients on self-reported labor income, stratified by municipality of origin (Figure A.8), against average change in upward mobility between municipality of destination and origin for children from origin o. Each coefficient is weighted by the number of observations in each cell (number of children in the sample in each municipality of origin). Panel (b) plots displaced children's earnings in adulthood against upward mobility in destination municipalities in purple, divided into 20 centiles. "Slope" of 2.867 is the slope of the blue fitted line. Gray triangles correspond to displaced children's earnings predicted by non-displaced children's demographics at baseline. "Predicted slope" of 0.013 is the slope of the gray fitted line. Blue areas are the 95% confidence intervals of the corresponding blue fitted lines.

Figure 6: Effect of change in neighborhood quality on earnings by age at intervention

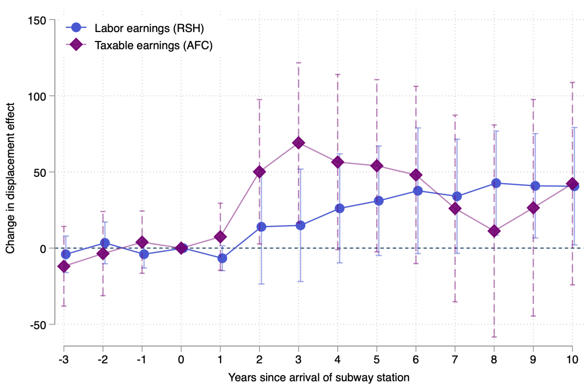
(a) Full sample **(b)** Displaced only



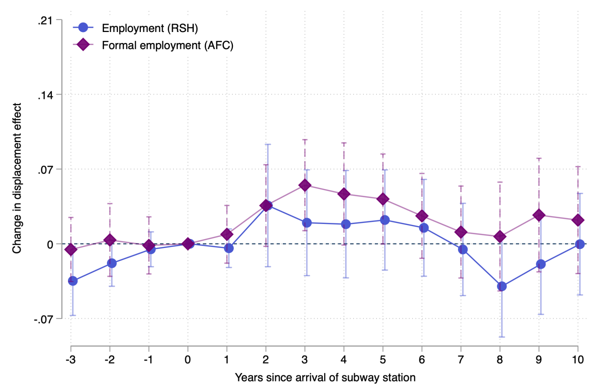
Notes: Panel (a) plots coefficients from equivalent specifications in columns (1) (blue circles) and (7) (purple diamonds) of Table 5, along with their 95% confidence intervals, for self-reported labor earnings, stratified by age groups at baseline ([0,5], [6–12], and [13–18]). Panel (b) repeats the same exercise in the sample of displaced children only.

Figure 7: Change in displacement effect due to subway access

(a) Earnings (CLP$1,000/month)



(b) Employment



Notes: Each coefficient and its 95% confidence interval in panels (a) and (b) correspond to the estimates of γ_τ from equation (3) on each corresponding outcome.

Table 1: Slum characteristics before the intervention

	Full sample of slums				Slums in the archives			
	Displaced mean	Non-displaced mean	Difference	Conditional difference	Displaced mean	Non-displaced mean	Difference	Conditional difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Slum attributes								
# Families	229.336	230.263	-0.926	-3.052	293.479	330.385	-39.906	-40.779
			(36.152)	(40.134)			(72.922)	(75.225)
Families/hectare	70.868	61.379	9.489	8.001	62.084	68.486	-6.401	-15.62
			(7.632)	(5.846)			(10.71)	(8.598)
Military name	0.137	0.191	-0.054	-0.037	0.19	0.225	-0.035	-0.023
			(0.049)	(0.05)			(0.085)	(0.086)
Elevation (mas)	570.873	586.305	-15.433	-1.365	568.483	585.3	-16.817	-16.605
			(11.301)	(9.567)			(16.579)	(13.702)
Slope (degrees)	2.833	2.643	0.190	0.236	2.799	2.567	0.232	0.213
			(0.229)	(0.242)			(0.303)	(0.311)
Close to river/canal (<100 m)	0.049	0.031	0.018	0.006	0.034	0.025	0.009	0.012
			(0.026)	(0.023)			(0.035)	(0.037)
Flooding risk	0.059	0.09	0.051**	-0.009	0.034	0.00	0.034	0.00
			(0.025)	(0.009)			(0.024)	(-)
Distance to CBD	9.838	10.289	-0.906*	-0.183	9.164	9.928	-0.764	-0.539
			(0.544)	(0.533)			(0.747)	(0.713)
Panel B. Attributes of the census district where a slum is located								
Population education attainment	7.799	7.164	0.635**	0.146	7.789	7.506	0.283	-0.021
			(0.245)	(0.197)			(0.379)	(0.315)
Unemployment rate	0.191	0.199	-0.009	0.003	0.195	0.184	0.011	0.018
			(0.007)	(0.007)			(0.011)	(0.011)
Number of schools	4.015	4.290	-0.275	0.483	3.854	3.650	0.204	0.141
			(0.420)	(0.436)			(0.586)	(0.599)
Log property prices	14.793	14.739	0.055	0.033	14.818	14.777	0.041	0.018
			(0.043)	(0.043)			(0.074)	(0.070)
Number of slums	102	131	233	221	58	40	98	94
Number of municipalities	14	14	14	14	14	14	14	14

Notes: Columns (1) and (2) show summary statistics for displaced (relocated) and non-displaced (redeveloped) 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. The number of families is presented for reference, but it is not accurate, as Morales and Rojas (1986) count subdivisions of larger slums as separate slums but measure density within the subdivision. Elevation, slope, and flooding risk data are obtained from Geoportal. Prices, unemployment, number of schools, and population education attainment 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, and column (4) reports the difference between groups conditional on the propensity score $\hat{p}(X_s)$ for slums in the sample with common support. Columns (5)–(8) repeat the exercise of the first four columns but for the sample of 98 slums found in the archival data. Robust standard errors are reported in parentheses. 10%*, 5%**, 1%***.

Table 2: Sample summary and balancing tests for children aged 0–18 at baseline

	Full sample of children in archives	Children 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)	(7)
Panel A. Demographics							
Female	0.503	0.499	0.505	0.004	0.517	0.517	-0.002
	[0.500]			(0.006)			(0.005)
Age	8.124	8.261	8.120	0.027	8.316	8.095	-0.127
	[4.854]			(0.236)			(0.233)
Firstborn	0.366	0.367	0.365	-0.005	0.355	0.360	0.001
	[0.482]			(0.010)			(0.011)
No. children	3.840	3.748	3.879	0.101	3.807	3.894	0.054
	[1.795]			(0.069)			(0.069)
Oldest sibling	11.524	11.562	11.552	0.146	11.693	11.562	-0.046
	[5.798]			(0.317)			(0.324)
Youngest sibling	5.094	5.264	5.052	-0.132	5.274	5.040	-0.170
	4.198			(0.186)			(0.185)
HH age	34.788	35.291	34.617	-0.572	35.357	34.625	-0.614
	[7.125]			(0.385)			(0.390)
Mother age	33.066	33.529	32.899	-0.561	33.599	32.889	-0.630*
	[6.951]			(0.343)			(0.346)
Father age	35.336	35.703	35.217	-0.168	35.752	35.208	-0.218
	[7.487]			(0.373)			(0.383)
Female HH	0.329	0.309	0.331	0.022	0.307	0.326	0.021
	[0.470]			(0.032)			(0.033)
Married HH	0.787	0.834	0.773	-0.056***	0.835	0.778	-0.055***
	[0.410]			(0.012)			(0.012)
Cohabit HH	0.091	0.081	0.089	0.006	0.080	0.090	0.008
	[0.288]			(0.008)			(0.009)
Widowed HH	0.011	0.008	0.013	0.004	0.008	0.013	0.004
	[0.105]			(0.003)			(0.003)
Mapuche HH	0.057	0.050	0.060	0.011*	0.051	0.061	0.009
	[0.232]			(0.006)			(0.006)
HH formal employment[a]	0.388	0.413	0.382	-0.028	0.412	0.381	-0.028
	[0.077]			(0.021)			(0.021)
HH born outside Santiago	0.461	0.460	0.457	0.004	0.458	0.458	0.004
	[0.498]			(0.022)			(0.021)
Mother's schooling[b]	5.973	6.261	5.883	-0.342*	6.119	5.826	-0.257
	[3.448]			(0.175)			(0.172)
Child mortality last 5 years[c]							
below age 1	0.016	0.022	0.013	-0.007	0.022	0.014	-0.007
	[0.131]			(0.005)			(0.006)
below age 5	0.020	0.026	0.017	-0.005	0.026	0.018	-0.005
	[0.148]			(0.006)			(0.006)
B. Matching rates							
In RSH	0.912	0.887	0.922	0.035***	1.00	1.00	-
	[0.283]			(0.005)			
In AFC	0.754	0.726	0.766	0.037***	0.818	0.831	0.009
	[0.431]			(0.008)			(0.006)
Children	33,624	10,291	21,082	31,373	9,131	19,429	28,560
Families	13,739	4,197	8,547	12,744	3,948	8,295	12,243
Slums	98	41	54	94	41	54	94
Municipalities	14		14			14	

Notes: Column (1) reports means for the sample of children in the archival data. Column (2) reports means for non-displaced children at baseline, and column (3) reports means for displaced children in the sample with common support of the propensity score, which excludes four slums. Column (4) reports the difference between groups, adjusted by the probability of slum clearance within a municipality of origin ($\hat{p}_s + \psi_o + \hat{p}_s \times \psi_o$). Columns (5)–(7) repeat the exercise for children found in the RSH. [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 child's mother had a child born alive who died below the age of 1 or 5, in the five years before treatment. One slum in the archival sample had families in both treatments, that is why the total number of slums is not 95. Standard deviations are reported in brackets, and standard errors clustered by slum of origin are reported in parentheses. 10%*, 5%**, 1%***.

Table 3: Displacement effect on labor income and employment

	(1)	(2)	(3)	(4)	(5)
Panel A. Outcome: Self-reported earnings (CLP$1,000/month)					
Displaced	-30.622	-25.787	-24.377	-24.750	-24.992
	(6.705)***	(5.557)***	(5.409)***	(6.086)***	(6.435)***
	[6.299]***	[5.545]***	[5.347]***	[5.974]***	[6.346]***
Adjusted R^2	0.009	0.010	0.009	0.011	0.086
Non-displaced mean	244.163	244.163	239.841	239.841	239.841
Percent effect	**-12.5**	**-10.6**	**-10.2**	**-10.3**	**-10.4**
Panel B. Outcome: 1[Employed]					
Displaced	0.003	0.000	0.003	0.002	0.001
	(0.006)	(0.007)	(0.007)	(0.008)	(0.007)
	[0.006]	[0.007]	[0.007]	[0.008]	[0.008]
Adjusted R^2	0.000	0.000	0.000	0.000	0.066
Non-displaced mean	0.606	0.606	0.632	0.632	0.632
Percent effect	**0.50**	**0.00**	**0.47**	**0.31**	**0.16**
Individuals	30,677	30,677	28,560	28,560	28,560
Slums	98	98	94	94	94
Year-of-treatment FE	✓	✓	✓	✓	✓
Origin FE (ψ_o)		✓	✓	✓	✓
Propensity score ($\hat{p}_s$)			✓	✓	✓
$\hat{p}_s \times \psi_o$				✓	✓
Baseline controls					✓

Notes: The table shows regressions for children aged 0–18 at baseline who are matched to the RSH data. The row labeled as "Percent effect" stands for percentage variation with respect to the non-displaced mean. ψ_o are municipality-of-origin fixed effects, and $\hat{p}_s$ is the fitted value of the propensity score by slum. The non-displaced mean in columns (3), (4) and (5) is computed conditional on $\hat{p}_s$ in the sample of children in the common support. 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, and year-of-birth fixed effects. Standard errors clustered by slum of origin are reported in parentheses, and Conley standard errors are reported in brackets. 10%*, 5%**, 1%***.

Table 4: Displacement effect on employment and education outcomes

	Displacement effect (1)	Mean non-displaced (2)	Percent effect (%) (3)	P-value/ Sharp p-value (4)
Panel A. Type of employment				
Contract = 1	-0.035***	0.363	-9.6	0.001; 0.001
	(0.011)			
Temp worker = 1	0.031***	0.648	4.8	0.000; 0.001
	(0.008)			
Formal employment = 1	-0.020***	0.355	-5.6	0.001; 0.001
	(0.006)			
Panel B. Income				
Formal earnings	-22.526***	153.093	-14.7	0.001; 0.001
	(6.333)			
Informal earnings	-2.465	86.748	-2.8	0.246; 0.026
	(2.114)			
Formal wages	-43.622***	352.013	-12.4	0.000; 0.001
	(10.836)			
Panel C. Education				
Years of schooling	-0.648***	11.353	-5.7	0.000; 0.001
	(0.117)			
HS graduate = 1	-0.101***	0.653	-15.5	0.000; 0.001
	(0.017)			
2-year college = 1	-0.031***	0.123	-25.2	0.000; 0.001
	(0.009)			
5-year college = 1	-0.020***	0.051	-39.2	0.000; 0.001
	(0.005)			

Notes: The table shows propensity score estimates equivalent to column (5) of Table 3 for children aged 0–18 at baseline who are matched to the RSH data. 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 hypothesis comparison, based on Anderson (2008)'s method. Standard errors clustered by slum of origin are reported in parentheses. 10%*, 5%**, 1%***.

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

	Self-reported earnings (CLP$1,000/month)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ Upward mobility$_{do'}$	1.918*	0.818	1.203	1.769*	1.314	1.732**	0.083
	(0.983)	(0.901)	(0.955)	(0.920)	(1.004)	(0.860)	(0.956)
Distance from origin		-1.002**					-0.586
		(0.405)					(0.484)
Δ distance to CBD$_{do'}$			-1.483***				-0.592
			(0.524)				(0.579)
Share slum network in d				18.261**			12.601
				(8.813)			(8.457)
Neighborhood size in d					-0.010**		-0.011**
					(0.004)		(0.005)
Home value						0.123***	0.094*
						(0.046)	(0.051)
Upward mobility$_{o'}$	5.543***	5.269***	5.451***	5.315***	5.581***	5.396***	5.118**
	(1.804)	(1.817)	(1.810)	(1.655)	(1.723)	(1.773)	(1.601)
Adjusted R^2	0.086	0.086	0.086	0.086	0.086	0.086	0.086
Change in earnings	-2.463	-6.791	-6.542	-5.595	-7.205	-4.236	-15.356
Observations	28,560	28,560	28,560	28,560	28,560	28,560	28,560

Notes: The table shows results for coefficients δ from regression $Y_i = \alpha + \delta \Delta Attribute_{o'd} + \gamma \text{Upward Mobility}_{o'} + \psi_o + \hat{p}_s + \hat{p}_s \times \psi_o + X_i'\theta + \varepsilon_i$. All regressions include baseline controls, as in column (5) of Table 3. Row labeled "Change in earnings" corresponds to the implied difference in earnings between displaced and non-displaced children due to $\Delta Attribute_{o'd}$, computed as the sum of the multiplication of the estimates in each column and estimates from auxiliary regressions in Table A.10. Standard errors clustered by slum of origin are reported in parentheses. 10%*, 5%**, 1%***.

Table 6: Displacement effect on children's and parents' locations between 2016 and 2023

	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	0.105	0.010	-0.276***	-0.416	0.010
	(0.074)	(0.060)	(0.060)	(0.765)	(0.009)
Non-displaced mean	0.587	0.339	0.569	4.047	0.580
Percent effect	**17.9**	**3.0**	**-48.5**	**-10.3**	**1.8**
Observations	17,823	17,823	17,823	15,325	17,823
Panel B. Children in the RSH					
Displaced	0.153***	0.015	-0.173***	-0.400	0.016***
	(0.055)	(0.040)	(0.038)	(0.505)	(0.006)
Non-displaced mean	0.457	0.487	0.587	6.958	0.586
Percent effect	**33.5**	**3.1**	**-29.5**	**-5.7**	**2.7**
Panel C. Children in the RSH by age					
Displaced 0–5 (β_1)	0.143**	0.017	-0.179***	-0.385	0.019***
	(0.056)	(0.043)	(0.038)	(0.547)	(0.006)
Displaced 6–12 (β_2)	0.155***	0.016	-0.180***	-0.319	0.016***
	(0.057)	(0.040)	(0.040)	(0.520)	(0.006)
Displaced 13–18 (β_3)	0.160***	0.012	-0.146***	-0.586	0.012
	(0.057)	(0.037)	(0.036)	(0.541)	(0.007)
Observations	26,454	26,454	26,454	22,934	26,454
Test $\beta_1 = \beta_2$	0.477	0.938	0.968	0.802	0.296
Test $\beta_1 = \beta_3$	0.464	0.811	0.084	0.567	0.120
Test $\beta_2 = \beta_3$	0.767	0.793	0.073	0.397	0.380

Notes: The table shows regressions for all adults (Panel A) and children aged 0–18 at baseline (Panels B and C) who are matched to the RSH data, and report a non-missing location between 2016 and 2023. The regressions are equivalent to column (5) of Table 3. 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 2016. "% poor in current neighborhood" corresponds to the proportion of individuals in a neighborhood who qualify for social assistance in the RSH data. Due to large attrition in the sample of parents, the estimates in Panel A are corrected by the inverse probability of dying before 2016 as a function of baseline demographics stratified by treatment. The last three rows report p-values for equality tests of coefficients in Panel C. Standard errors clustered by slum of origin are reported in parentheses. 10%*, 5%**, 1%***.

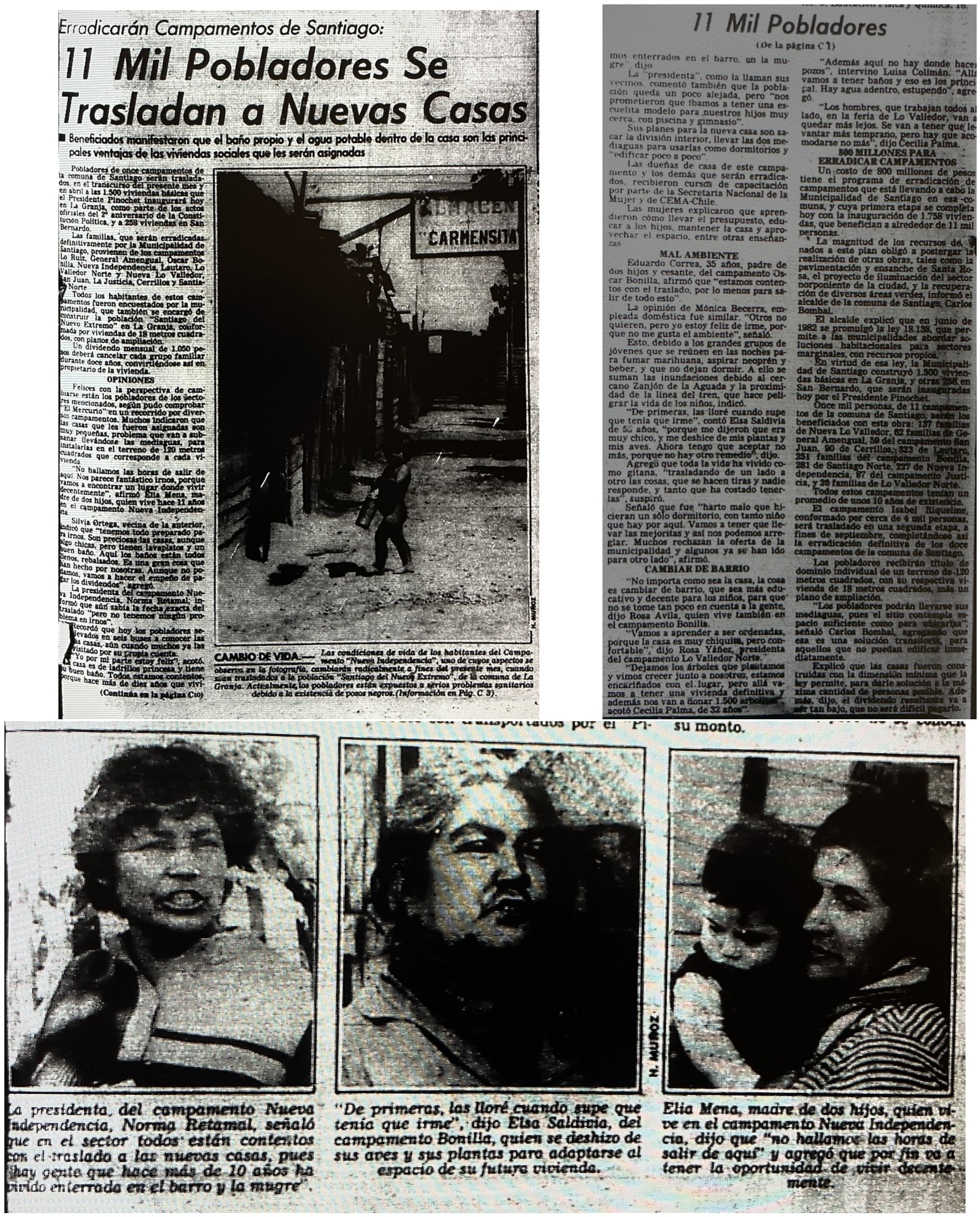
Online Appendix

A Additional figures and tables

Figure A.1: Example of a slum and neighborhoods of destination

Notes: These photos show examples of a slum and destination neighborhoods. The upper left panel shows a photo of the slum Nueva Habana (New Havana) in 1975, which was later upgraded into the formal neighborhood of Lo Hermida. The photos in the upper right panel are from Hidalgo (2019) and correspond to two examples of public housing projects received by displaced and non-displaced families as part of the Program for Urban Marginality. Finally, the photo in the bottom panel was taken by the authors in 2023 in the municipality of La Pintana, located in the southern part of Greater Santiago and characterized by its high share of public housing projects.

Figure A.2: Example of slum-dwelling families' impressions before relocation



Erradicarán Campamentos de Santiago:

11 Mil Pobladores Se Trasladan a Nuevas Casas

■ Beneficiados manifestaron que el baño propio y el agua potable dentro de la casa son las principales ventajas de las viviendas sociales que les serán asignadas

CAMBIO DE VIDA.— Las condiciones de vida de los habitantes del Campamento "Nueva Independencia", uno de cuyos aspectos se observa en la fotografía, cambiarán radicalmente a fines del presente mes, cuando sean trasladados a la población "Santiago del Nuevo Extremo", de la comuna de La Granja. Actualmente, los pobladores están expuestos a serios problemas sanitarios debido a la existencia de pozos negros. (Información en Pág. C 3).

11 Mil Pobladores

(De la página C 1)

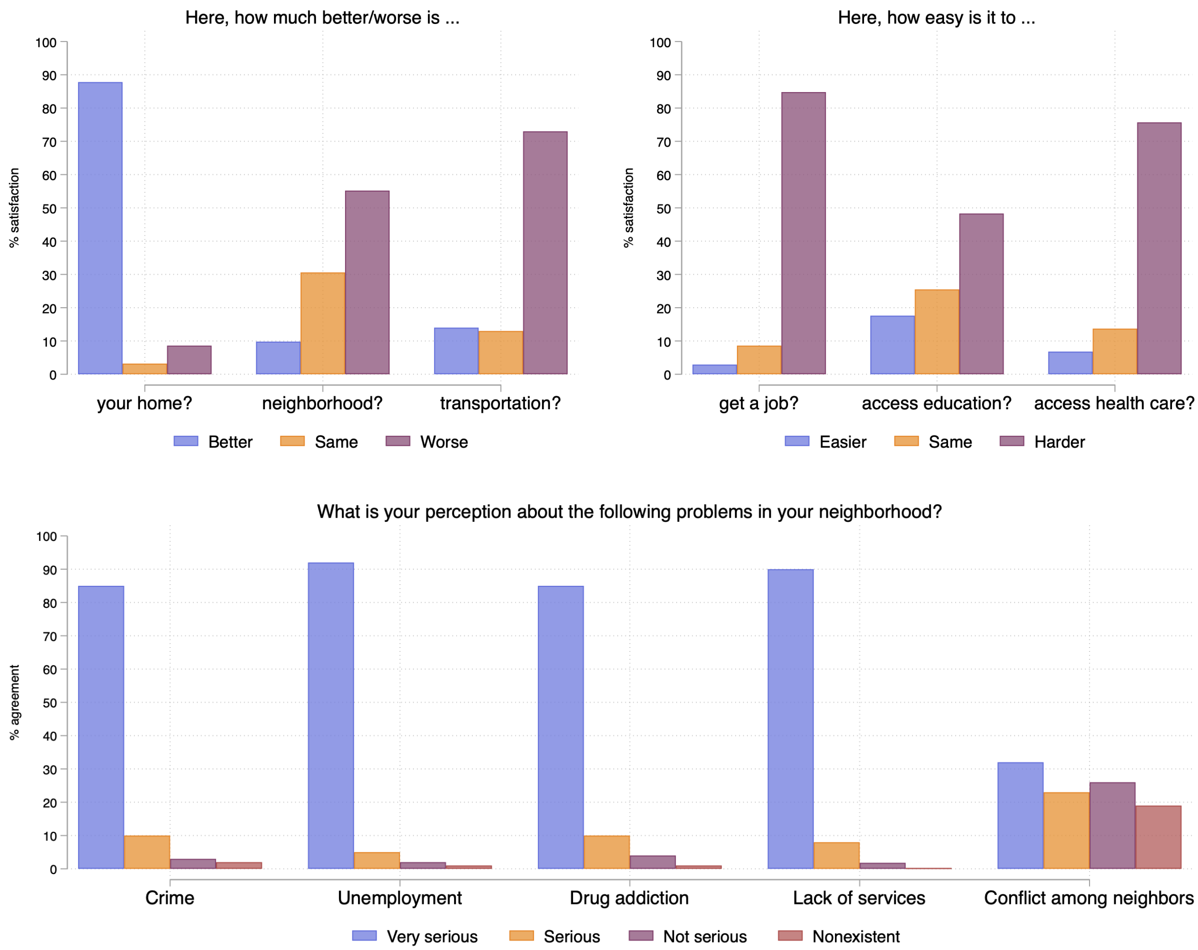
La presidenta, del campamento Nueva Independencia, Norma Retamal, señaló que en el sector todos están contentos con el traslado a las nuevas casas, pues hay gente que hace más de 10 años ha vivido enterrada en el barro y la mugre".

"De primeras, las lloré cuando supe que tenía que irme", dijo Elsa Saldivia, del campamento Bonilla, quien se deshizo de sus aves y sus plantas para adaptarse al espacio de su futura vivienda.

Elia Mena, madre de dos hijos, quien vive en el campamento Nueva Independencia, dijo que "no hallamos las horas de salir de aquí" y agregó que por fin va a tener la oportunidad de vivir decentemente.

Notes: The figure shows newspaper clippings from *El Mercurio* on July 09, 1982. The headline reads as "Eleven Thousand Slum Residents Move to Their New Homes." The photos at the bottom show testimonies from slum residents. Left: "The president of slum Nueva Independencia, Norma Retamal, stated that everyone in the area is happy about the move to the new houses, because there are people who have lived buried in mud and filth for more than 10 years." Center: "'At first I cried when I found out I had to leave,' said Elsa Saldivia from slum Bonilla, who got rid of her birds and plants to adapt to the space of her future home." Right: "Elia Mena, a mother of two who lives in the Nueva Independencia settlement, said, 'We can't wait to get out of here' and added that she will finally have the chance to live decently."

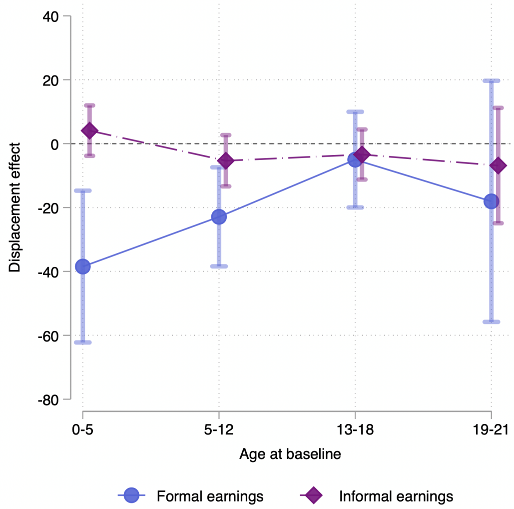
Figure A.3: Summary of the evaluation of the Program for Urban Marginality: Results from Aldunate et al. (1987)



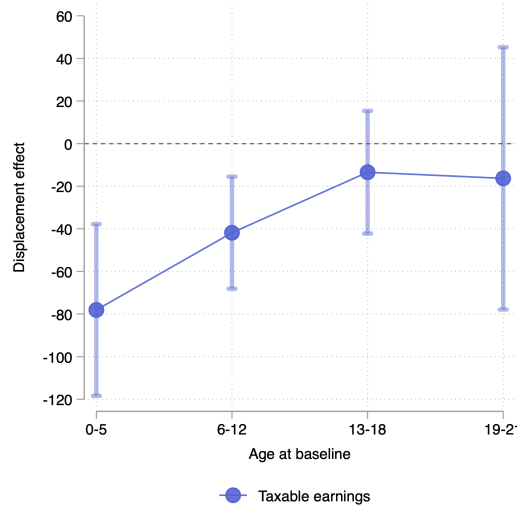
Notes: The figure presents a summary of results found by Aldunate et al. (1987). The authors interviewed 592 displaced slum families who were relocated into four new neighborhoods.

Figure A.4: Displacement effects on earnings by age at baseline

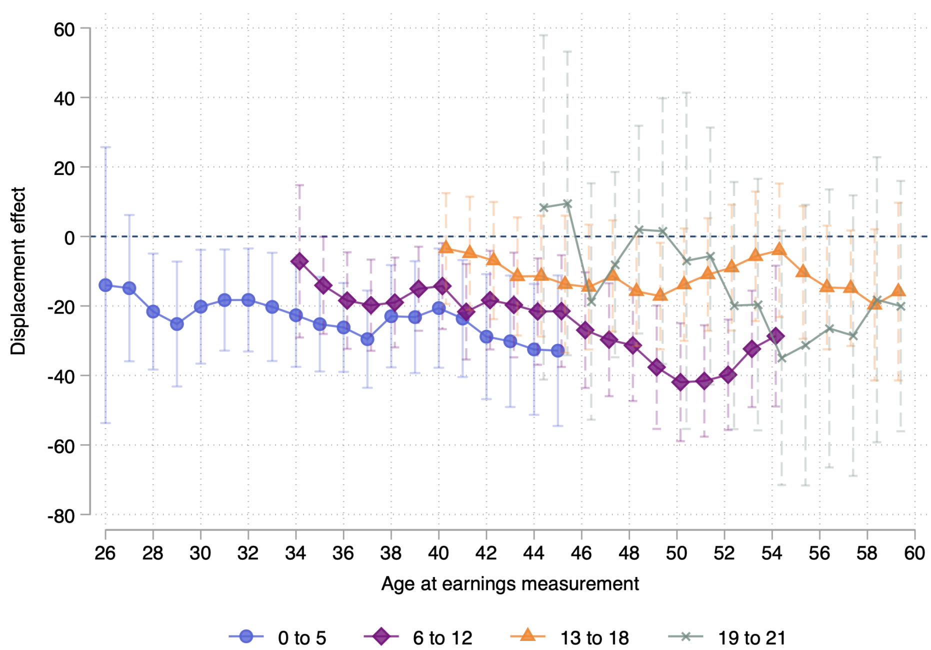
(a) Labor earnings



(b) Taxable earnings

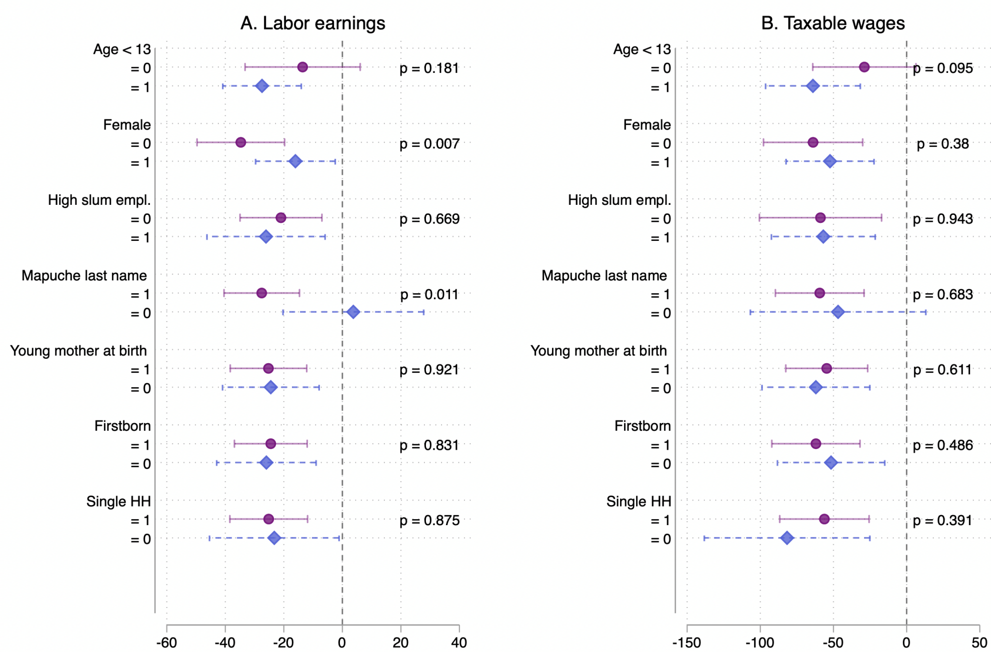


(c) Labor earnings by age



Notes: The figure shows regressions for children aged 0–21 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 $y_{it} = \sum_{g=1}^{4}\sum_{\tau=25}^{60} \beta_{\tau g} Displaced * 1[Age = \tau, Group = g] + \sum_{g=1}^{4}\sum_{\tau=25}^{60} \delta_{\tau g} 1[Age = \tau, Group = g] + \psi_o + \hat{p}(X_s) + \hat{p}(X_s) \times \psi_o + X'_{it}\gamma + u_{it}$, where g stands for an age group in [0,5], [6–12], [13–18], and [19–21] at the time of the intervention. 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's formal employment, year-of-birth fixed effects, and year-of-intervention fixed effects.

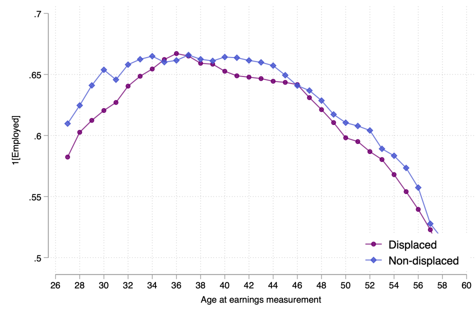
Figure A.5: Displacement effect by demographic groups on earnings and education



Notes: The figure shows displacement effect estimates and their 95% confidence intervals, equivalent to column (5) of Table 3, stratified by demographic variables for the sample of children aged 0–18 and who are matched to the RSH data. “Young mother” stands for mothers younger than 25 (sample median) at the time their first 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. P-values are reported for equality tests of coefficients.

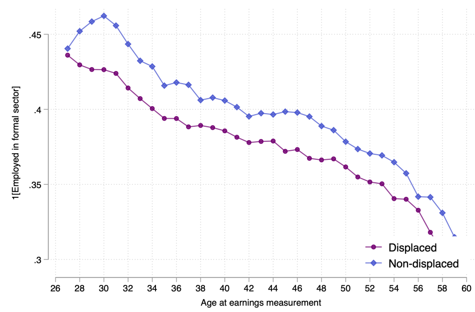
Figure A.6: Displacement effects on labor market outcomes by age at earnings measurement

(a) Employment trajectories

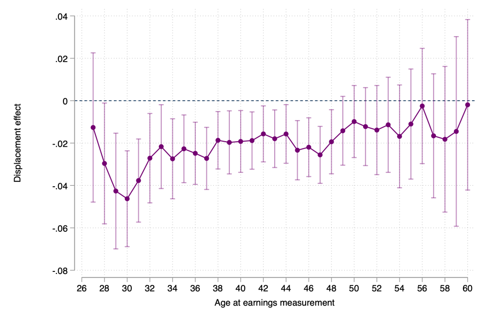


(b) Displacement effect on employment

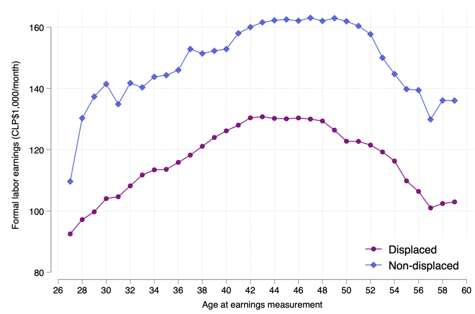
(c) Formal employment trajectories



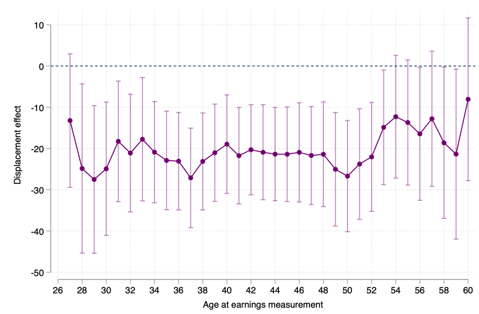
(d) Displacement effect on formal employment



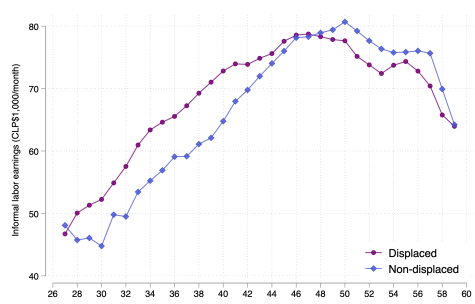
(e) Formal earnings trajectories



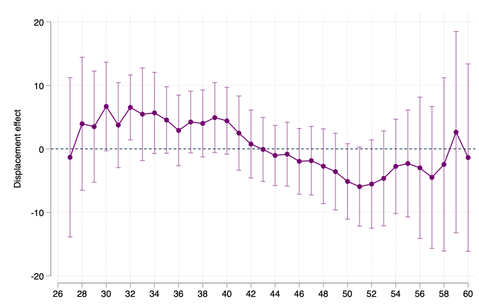
(f) Displacement effect on formal earnings



(g) Informal earnings trajectories

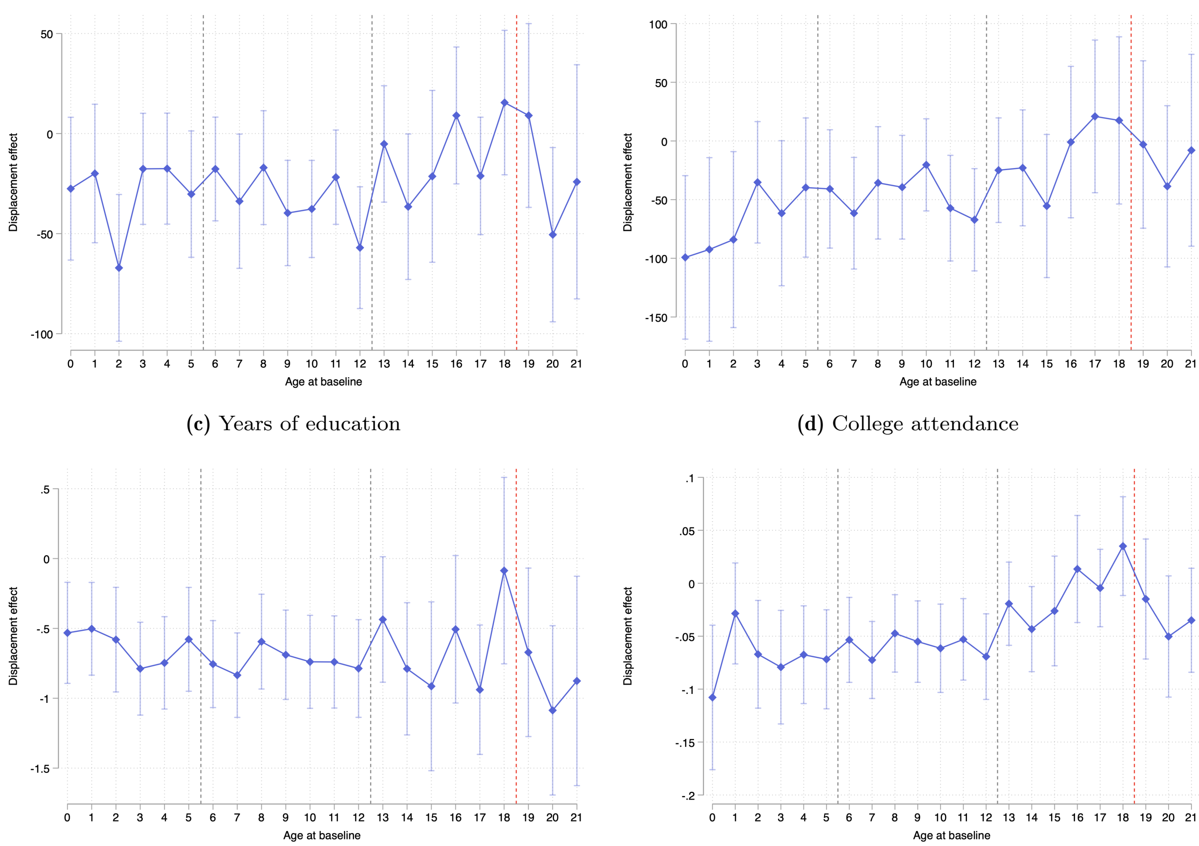


(h) Displacement effect on informal earnings



Notes: The figures show regressions for children aged 0–18 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 25 and 60 from the regression $y_{it} = \sum_{\tau=25}^{60} \beta_\tau Displaced * 1[Age = \tau] + \sum_{\tau=25}^{60} \delta_\tau 1[Age] + \psi_o + \hat{p}(X_s) + \hat{p}(X_s) \times \psi_o + X_i'\gamma + u_{it}$, 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's formal employment, year-of-birth fixed effects, and year-of-intervention fixed effects.

Figure A.7: Displacement effect by age at intervention



(a) Labor earnings

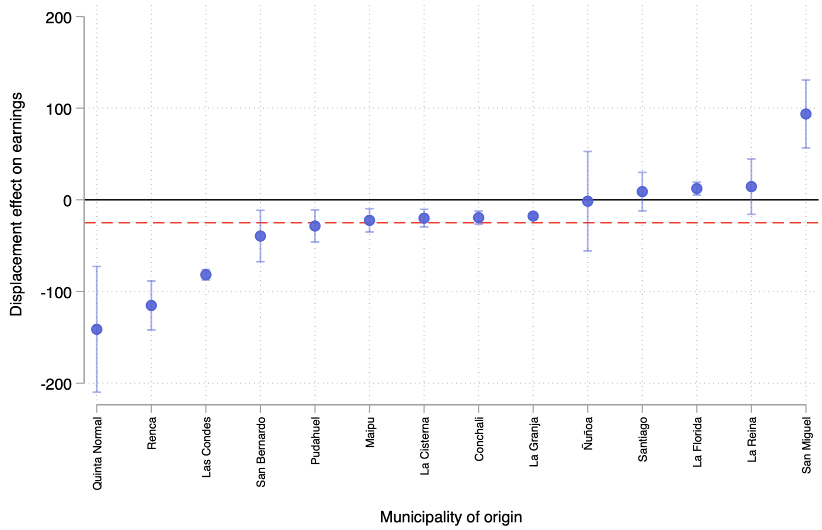
(b) Taxable wages

(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, for the sample of children aged 0–21 years old at the time of the intervention who are matched with the RSH data. The dotted gray vertical lines indicate that the p-value of the structural break test at the corresponding age is smaller than 0.1 for most outcomes (earnings, taxable wages, and schooling). The dotted red vertical line marks age 18 for reference.

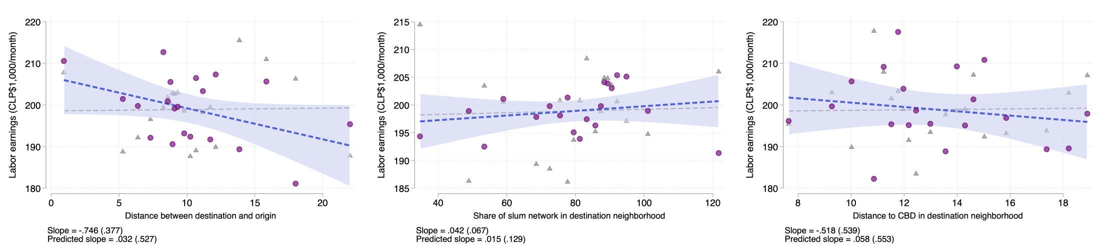
Figure A.8: Distribution of displacement effects on labor earnings by municipality of origin



Notes: The figure shows regressions stratified by municipality of origin. The sample includes children who were 0–18 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 $y_i = \sum_{o=1} \beta_o Displaced_{s\{i\}} * 1[Origin = o] + \hat{p} + \psi_o + X'_{io}\theta + \varepsilon_i$. Due 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's 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 (5) of Table 3). β_o and its 95% confidence intervals are reported, and standard errors clustered by slum of origin.

Figure A.9: Relationship between movers' earnings and destination location attributes

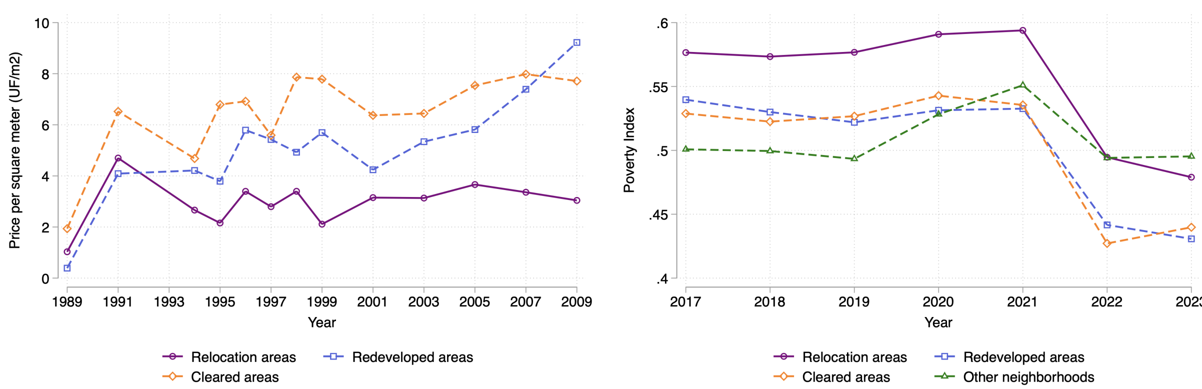
(a) Distance from origin (km) **(b)** % slum community **(c)** Distance to CBD



Notes: The figures plot displaced children's adult labor earnings against average neighborhood attributes at destination, divided into 20 centiles. These are equivalent to panel (b) of Figure 5. Purple dots are actual earnings of displaced children, and gray triangles are displaced children's predicted earnings using non-displaced children's baseline demographics. Blue lines correspond to the fitted correlations (slope), and gray lines correspond to the fitted correlations (predicted slope) using predicted earnings.

Figure A.10: Property values and poverty rates in treated neighborhoods across time

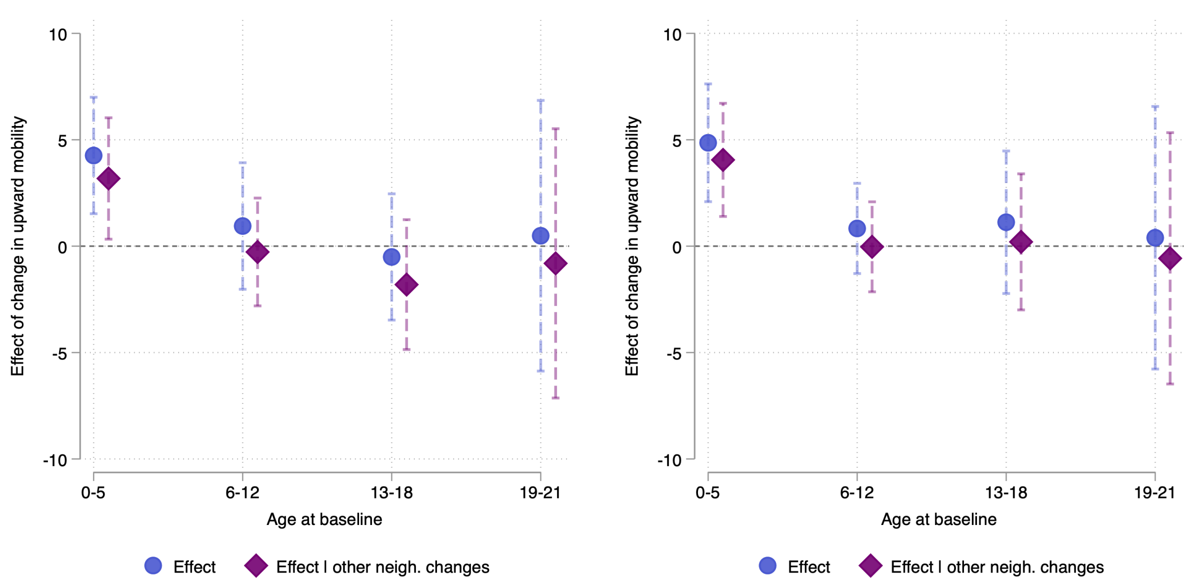
(a) Property values **(b)** Poverty rates



Notes: Panel (a) plots the average property value per square meter, measured in UF/m^2, in areas were slums and neighborhoods were located. We use historical data from Trivelli (1989–2009) by zone, which is similar to a census district in 1982 (Trivelli, 2009). To compute the averages, we control for the number of offers per zone. Panel (b) plots the poverty index per neighborhood using the RSH data. The poverty index is defined as the proportion of individuals per neighborhood who qualify for social assistance. Each treatment is defined as follows: 1) relocation areas are neighborhoods that housed displaced families; 2) redeveloped areas are neighborhoods where slums were redeveloped on-site; 3) cleared areas are neighborhoods from which displaced families were evicted; and 4) other neighborhoods include all other areas in Greater Santiago not classified into the previous categories.

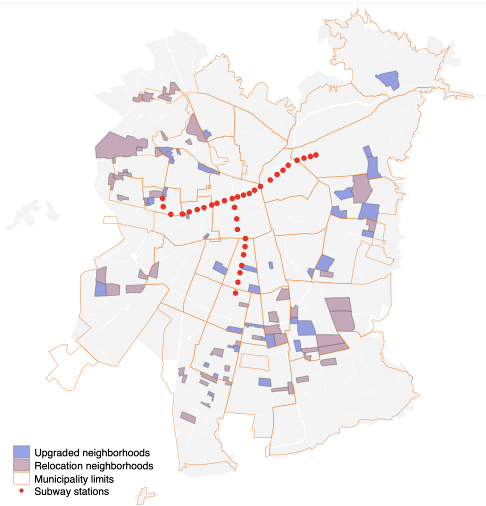
Figure A.11: Effect of change in upward mobility on earnings by age at intervention

(a) Full sample **(b)** Displaced only

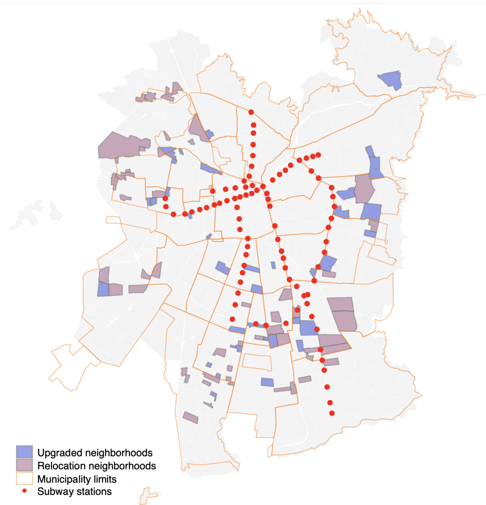


Notes: The figures plot coefficients from equivalent specifications in columns (1) (blue circles) and (5) (purple diamonds) of Table 5, along with their 95% confidence intervals, for the sample of children aged 0-21 matched to the RSH. Outcome is self-reported labor earnings, and estimates are stratified by age groups at baseline ([0,5], [6–12], [13–18], and [19–21]).

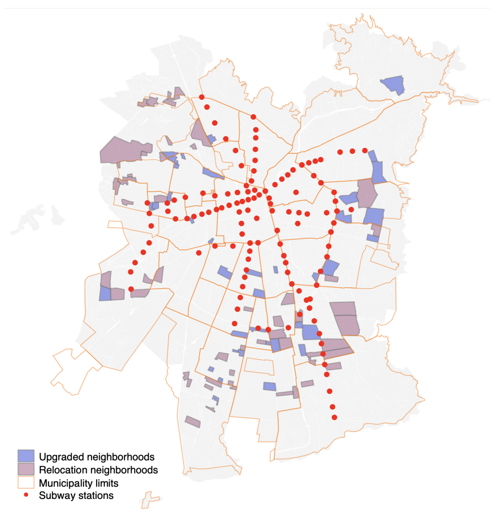
Figure A.12: Location of public housing projects and subway stations



(a) Subway before 2001



(b) Subway in 2006



(c) Subway in 2019

Notes: The figures show the rollout of subway stations in Greater Santiago from 1980 to 2019. Orange lines represent the urban boundaries of Greater Santiago and its municipalities in 2023, while the 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 blue 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.

Table A.1: Summary statistics for children aged 0–18 at baseline

	Full sample of children (1)	Children in common support (2)	Children in the RSH in common support (3)	P(child is found in the RSH) (4)
Demographics at intervention				
Displaced	0.694	0.672	0.680	0.033***
	[0.461]	[0.469]	[0.466]	(0.005)
Female	0.503	0.503	0.517	0.048***
	[0.500]	[0.500]	[0.500]	(0.004)
Age	8.124	8.166	8.166	0.000
	[4.854]	[4.822]	[4.830]	(0.001)
No. children	3.840	3.836	3.866	0.007***
	[1.795]	[1.791]	[1.798]	(0.001)
Firstborn	0.366	0.365	0.359	-0.018***
	[0.482]	[0.482]	[0.480]	(0.003)
Oldest sibling	11.524	11.555	11.604	
	[5.798]	[5.703]	[5.707]	
Youngest sibling	5.094	5.122	5.115	
	[4.198]	[4.188]	[4.179]	
HH age	34.788	34.838	34.859	-0.000
	[7.125]	[7.040]	[7.037]	(0.000)
Female HH	0.329	0.324	0.320	-0.012***
	[0.470]	[0.468]	[0.467]	(0.004)
Married HH	0.787	0.793	0.796	-0.012***
	[0.410]	[0.405]	[0.403]	(0.004)
Cohabit HH	0.091	0.086	0.087	
	[0.288]	[0.281]	[0.282]	
Widowed HH	0.011	0.011	0.011	-0.031
	[0.105]	[0.106]	[0.105]	(0.027)
Mapuche HH	0.057	0.057	0.058	0.013*
	[0.232]	[0.231]	[0.233]	(0.006)
Mapuche last name	0.086	0.084	0.086	
	[0.280]	[0.277]	[0.287]	
HH's formal employment[a]	0.388	0.392	0.391	-0.147***
	[0.077]	[0.073]	[0.072]	(0.031)
HH born outside Stgo	0.461	0.458	0.458	-0.001
	[0.498]	[0.498]	[0.498]	(0.004)
Child mortality[b]	0.020	0.020	0.021	
	[0.148]	[0.148]	[0.150]	
Variables measured after 2007				
Died before 2007	0.006	0.006	-	-0.923***
	[0.077]	[0.078]	-	(0.004)
Died after 2007	0.025	0.025	0.024	
	[0.156]	[0.156]	[0.155]	
Mother's schooling[c]	5.973	6.009	5.921	
	[3.448]	[3.460]	[3.422]	
in RSH	0.912	0.910	1.000	
	[0.283]	[0.286]	[-]	
in AFC	0.754	0.753	0.827	
	[0.431]	[0.431]	[0.378]	
Individuals	33,624	31,373	28,560	33,624
Families	13,739	12,744	12,243	13,739
Number of slums	98	94	94	98

Notes: The table shows summary statistics for children aged 0–18 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 in slums in the common support. Column (4) estimates a linear regression of the probability of being found in the RSH (column (3) relative to (1)) on the set of covariates with no missing values. Standard errors clustered by slum of origin are reported in parentheses, and standard deviations are reported in brackets. Adjusted R^2 for the regression in column (4) is 0.079. [a]Household's formal employment is measured at the slum level using historical data from the Superintendence of Pensions. [b]Child mortality measures whether a child's mother had a child born alive who died before the age of 5, in the five years before treatment. [c]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 A.2: Demographics of children aged 0–18 in 1982 Census

	All children (1)	Children in slums (2)	Children in formal housing (3)	Difference [(2)-(3))] (4)
Lives in a slum	0.190	1.000	0.000	
	[0.392]			
Female	0.494	0.490	0.495	-0.005
	[0.500]	[0.500]	[0.500]	(0.004)
Age	9.284	8.257	9.525	-1.268***
	[5.443]	[5.307]	[5.446]	(0.039)
In school	0.689	0.607	0.708	-0.102***
	[0.463]	[0.489]	[0.455]	(0.004)
No. children	4.148	4.619	4.017	0.603***
	[2.598]	[2.718]	[2.549]	(0.051)
HH size	5.720	5.732	5.717	0.014
	[2.211]	[2.338]	[2.181]	(0.017)
HH age	39.393	36.956	39.964	-3.008***
	[9.646]	[9.770]	[9.526]	(0.071)
Female HH	0.131	0.149	0.127	0.022***
	[0.337]	[0.356]	[0.333]	(0.003)
Married HH	0.861	0.781	0.879	-0.098***
	[0.346]	[0.414]	[0.326]	(0.003)
Cohabit HH	0.047	0.107	0.033	0.074***
	[0.212]	[0.309]	[0.179]	(0.002)
Widowed HH	0.032	0.034	0.032	0.002*
	[0.176]	[0.181]	[0.175]	(0.002)
HH's employment	0.688	0.619	0.704	-0.086***
	[0.463]	[0.486]	[0.456]	(0.003)
HH's schooling	7.980	5.825	8.485	-2.660***
	[4.188]	[3.205]	[4.231]	(0.025)
Individuals	123,102	23,386	99,716	123,102
Households	56,020	10,034	45,986	56,020

Notes: The table shows summary statistics for children aged 0–18 in a 10% sample of the 1982 Census of Population. Person weights are used. A household is defined to live in a slum if their dwelling is in any of the following categories: improvised hut made of light constructions, room in a high-density slum dwelling, or improvised dwelling. If a household did not enter the previous categories but their dwelling had a ground floor, or had no access to sewage or electricity, they were also considered to be living in a slum. Standard deviations are reported in brackets, and robust standard errors are reported in parentheses. 10%*, 5%**, 1%***.

Table A.3: Comparison of displaced and non-displaced children aged 0–18 at baseline in common support

	All children in archives				Children matched to the RSH			
	Non-displaced mean	Displaced mean	Conditional difference	Weighted cond. diff	Non-displaced mean	Displaced mean	Conditional difference	Weighted cond. diff.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Demographics								
Female	0.499	0.505	0.004	0.006	0.517	0.517	-0.002	0.000
			(0.006)	(0.006)			(0.005)	(0.005)
Age	8.261	8.120	0.027	0.028	8.316	8.095	-0.127	0.068
			(0.236)	(0.258)			(0.233)	(0.252)
Firstborn	0.367	0.365	-0.005	-0.007	0.355	0.360	0.001	-0.002
			(0.010)	(0.011)			(0.011)	(0.012)
No. children	3.748	3.879	0.101	0.115	3.807	3.894	0.054	0.073
			(0.069)	(0.072)			(0.069)	(0.071)
Oldest sibling	11.562	11.552	0.074	0.146	11.693	11.562	-0.046	0.042
			(0.317)	(0.339)			(0.324)	(0.346)
Youngest sibling	5.264	5.052	-0.132	-0.090	5.274	5.040	-0.170	-0.125
			(0.186)	(0.206)			(0.185)	(0.207)
HH age	35.291	34.617	-0.572	-0.464	35.357	34.625	-0.614	-0.499
			(0.385)	(0.419)			(0.390)	(0.421)
Mother age	33.529	32.899	-0.561	-0.506	33.599	32.889	-0.630*	-0.567
			(0.343)	(0.361)			(0.346)	(0.362)
Father age	35.703	35.217	-0.168	-0.077	35.752	35.208	-0.218	-0.113
			(0.373)	(0.414)			(0.383)	(0.419)
Female HH	0.309	0.331	0.022	0.004	0.307	0.326	0.021	0.002
			(0.032)	(0.035)			(0.033)	(0.036)
Married HH	0.834	0.773	-0.056***	-0.055***	0.835	0.778	-0.055***	-0.053***
			(0.012)	(0.011)			(0.012)	(0.012)
Cohabit HH	0.081	0.089	0.006	0.008	0.080	0.090	0.008	0.012
			(0.008)	(0.008)			(0.009)	(0.009)
Widowed HH	0.008	0.013	0.004	0.005	0.008	0.013	0.004	0.005*
			(0.003)	(0.002)			(0.003)	(0.003)
Mapuche HH	0.050	0.060	0.011*	0.005	0.051	0.061	0.009	0.003
			(0.006)	(0.006)			(0.006)	(0.006)
HH formal employment[a]	0.413	0.382	-0.028	-0.018	0.412	0.381	-0.028	-0.018
			(0.021)	(0.021)			(0.021)	(0.021)
HH born outside Santiago	0.460	0.457	0.004	-0.004	0.458	0.458	0.004	-0.003
			(0.022)	(0.023)			(0.021)	(0.021)
Mother's schooling[b]	6.261	5.883	-0.342*	-0.293	6.119	5.826	-0.257	-0.223
			(0.175)	(0.186)			(0.172)	(0.186)
Child mortality last 5 years[c]								
below age 1	0.022	0.013	-0.007	-0.008	0.022	0.014	-0.007	-0.008
			(0.005)	(0.006)			(0.006)	(0.006)
below age 5	0.026	0.017	-0.005	-0.005	0.026	0.018	-0.005	-0.006
			(0.006)	(0.006)			(0.006)	(0.006)
B. Matching rates								
In RSH	0.887	0.922	0.035***	0.031***	1.00	1.00	-	-
			(0.005)	(0.005)				
In AFC	0.726	0.766	0.037***	0.030***	0.818	0.831	0.009	0.005
			(0.008)	(0.009)			(0.006)	(0.007)
Children	10,291	21,082	31,373	31,373	9,131	19,429	28,560	28,560
Families	4,197	8,547	12,744	12,744	3,948	8,295	12,243	12,243
Slums	40	54	94	94	40	54	94	94
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 within a municipality of origin ($\hat{p}_s + \psi_o + \hat{p}_s \times \psi_o$) in the full sample of children aged 0–18 at baseline from families found in the archival sample and in the common support of the propensity score. Column (4) estimates the equivalent to column (3), re-weighting the sample by the inverse probability that a slum is found in the archives. Columns (5)–(8) repeat the exercise for children found in the RSH. Standard errors clustered by slum of origin are reported 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 child's mother had a child born alive who died below the age of 1 or 5, in the five years before treatment.

Table A.4: Location characteristics before and after treatment

	Non-displaced mean	Displaced mean at origin	Conditional difference	Displaced mean at destination	Conditional difference
	(1)	(2)	(3)	(4)	(5)
Upward mobility[a]	38.390	38.535	-0.056	37.303	-1.470***
			(0.447)		(0.441)
Schooling HH	7.370	7.502	-0.288*	6.530	-1.005***
			(0.163)		(0.237)
Primary Care Centers/1000 HH	0.009	0.006	-0.005**	0.009	-0.002
			(0.002)		(0.002)
Hospitals/1000 HH	0.003	0.002	-0.001	0.005	0.003
			(0.001)		(0.003)
Distance to CBD (km)	9.981	9.876	-0.399	12.977	2.929***
			(0.424)		(0.552)
Commuting time[b]	42.880	42.840	-0.535	48.454	4.889***
			(0.956)		(0.951)
Property prices[c]	14.742	14.706	0.010	14.701	0.089
			(0.060)		(0.069)
Home value (UF)	277.494	-	-	241.181	-36.530***
			-		(11.346)
Distance from origin (km)	0.290	-	-	9.635	-7.637***
			-		(0.954)
Individuals	10,291	21,082	31,373	21,082	31,373
Slums	40	54	94	54	94

Notes: Table reports means of the characteristics of the neighborhoods that children lived in before and after treatment. 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 within a municipality of origin ($\hat{p}_s + \psi_o + \hat{p}_s \times \psi_o$). Column (4) reports means for displaced children after relocation. Column (5) reports the difference between columns (4) and (1), adjusted by the probability of slum clearance within a municipality of origin ($\hat{p}_s + \psi_o + \hat{p}_s \times \psi_o$). (a) Measured at the municipality level. (b) Measured at the municipality level in 1990. (c) Measured within in a 2 km buffer around a slum or neighborhood of destination. Standard errors clustered by slum of origin are reported in parentheses. 10%*, 5%**, 1%***.

Table A.5: Conley standard errors

Outcome	Labor income	Taxable wages
Displacement coefficient	-24.992	-43.622
Clustered se by slum of origin	6.435	10.836
Bootstrapped se	3.994	10.372
Conley se (cutoffs in km)		
1	6.104	10.785
2	6.160	10.873
3	6.330	10.990
4	**6.346**	11.058
5	6.323	11.058
6	6.294	11.087
7	6.274	11.142
8	6.292	11.151
9	6.331	11.114
10	6.282	10.928

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.6: Robustness of displacement effect to changes in propensity score method and common support

Model	Baseline	Inv-weight	$p_1 < p < p_{99}$	$p_5 < p < p_{95}$	$p_{10} < p < p_{90}$	p_{rest}	p_{rest_2}	p_{full}	p_{out}
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Self-reported earnings (CLP$1,000/month)									
Displaced	-24.992***	-23.769***	-24.229***	-24.717***	-26.928***	-24.579***	-15.998***	-25.364***	-24.297***
	(6.435)	(5.514)	(6.420)	(6.670)	(6.143)	(6.354)	(4.863)	(5.146)	(6.402)
Non-displaced mean	239.841	243.689	238.381	237.232	214.382	246.856	223.994	252.227	242.096
Percent effect	**-10.4**	**-9.8**	**-10.2**	**-10.4**	**-12.5**	**-10.0**	**-7.1**	**-10.1**	**-10.0**
Adjusted R^2	0.086	0.085	0.086	0.087	0.087	0.086	0.083	0.088	0.086
Panel B. Taxable wages from formal employment (CLP$1,000/month)									
Displaced	-43.622***	-36.298***	-44.393***	-42.807***	-46.097***	-38.315***	-32.077***	-47.801***	-41.572***
	(10.836)	(10.233)	(10.972)	(11.281)	(10.578)	(10.821)	(9.501)	(11.444)	(10.453)
Non-displaced mean	352.013	437.082	346.171	346.108	334.808	352.052	339.199	407.851	424.696
Percent effect	**-12.4**	**-8.3**	**-12.8**	**-12.4**	**-13.8**	**-10.9**	**-9.4**	**-11.7**	**-9.8**
Adjusted R^2	0.083	0.083	0.083	0.085	0.086	0.081	0.084	0.083	0.084
Panel C. Years of schooling									
Displaced	-0.648***	-0.654***	-0.654***	-0.642***	-0.646***	-0.617***	-0.560***	-0.726***	-0.629***
	(0.117)	(0.103)	(0.119)	(0.120)	(0.109)	(0.115)	(0.111)	(0.118)	(0.114)
Non-displaced mean	11.353	11.773	11.266	11.193	10.856	11.318	11.214	11.818	11.638
Percent effect	**-5.7**	**-5.6**	**-5.8**	**-5.7**	**-6.0**	**-5.5**	**-4.9**	**-6.1**	**-5.4**
Adjusted R^2	0.100	0.095	0.100	0.099	0.099	0.098	0.105	0.103	0.101
Individuals	28,560	28,560	28,452	27,532	25,219	24,525	22,126	29,300	29,618
Number of slums	94	94	93	89	78	79	78	95	95

Notes: Column (1) is equivalent to the results in column (5) of Table 3. Column (2) estimates the displacement effect using inverse propensity score weighting. The sample in column (3) excludes slums in the bottom and top 1% of the common support distribution; column (4) excludes those in the bottom and top 5%; and column (5) excludes those in the bottom and top 10%. Column (6) excludes three municipalities with low overlap of the propensity score between treatments. Column (7) restricts the sample even more to cells with no variation in treatment, where a cell is defined as the combination between a municipality of origin and whether the propensity score is above or below the median (see Figure B.3 for the distribution of cells). Column (8) uses a different version of the propensity score estimated using all slum characteristics in Table 1, and column (9) predicts the propensity score in the archival sample using the estimates from the observations that were not in the archives (out-of-sample estimation).

Table A.7: Displacement effect and spillovers on non-displaced children

	Self-reported earnings (CLP$1,000/month)				
	(1)	(2)	(3)	(4)	(5)
Displaced	-24.131***	-23.337***	-23.812***	-22.521***	-21.842***
	(6.752)	(6.704)	(6.453)	(6.960)	(6.927)
Non-displaced $< 1km$	6.742			9.379	
	(16.763)			(15.940)	
Non-displaced $< 1.5km$		10.896			12.378
		(14.115)			(13.317)
Home value			0.080*	0.086*	0.085*
			(0.043)	(0.045)	(0.045)
Non-displaced mean	241.486	241.957	239.841	241.486	241.957
Percent effect	**-10.0**	**-9.6**	**-9.9**	**-9.3**	**-9.0**
Adjusted R^2	0.086	0.086	0.086	0.086	0.086
Observations	28,560	28,560	28,560	28,560	28,560

Notes: The table shows regressions for children aged 0–18 at baseline who are matched to the RSH data who report non-missing education. Regressions are equivalent to column (5) of Table 3. The table splits the non-displaced group at baseline into two: non-displaced children without a displaced slum nearby (omitted category) and non-displaced children with a displaced slum within a 1, 1.5, or 2 km radius. Standard errors clustered by slum of origin are reported in parentheses. 10%*, 5%**, 1%***.

Table A.8: Displacement effect and spillovers on non-displaced children

	Self-reported earnings (CLP$1,000/month)			
	(1)	(2)	(3)	(4)
Displaced	-21.784	-16.605	-15.656	-18.019
	(19.484)	(22.913)	(21.997)	(21.175)
Non-displaced $< 1km$		9.880		
		(16.536)		
Non-displaced $< 1.5km$			12.734	
			(13.555)	
Non-displaced $< 2km$				12.801
				(10.771)
Home value	0.084	0.098	0.098	0.087
	(0.064)	(0.071)	(0.068)	(0.068)
Displaced* Home value	-0.008	-0.022	-0.023	-0.011
	(0.075)	(0.083)	(0.080)	(0.078)
Non-displaced mean	239.841	241.486	241.957	241.756
Adjusted R^2	0.086	0.086	0.086	0.086
Observations	28,560	28,560	28,560	28,560

Notes: The table shows regressions for children aged 0–18 at baseline who are matched to the RSH data and who report non-missing education. Regressions are equivalent to column (5) of Table 3. The table splits the non-displaced group at baseline into two: non-displaced children without a displaced slum nearby (omitted category) and non-displaced children with a displaced slum within a 1, 1.5, or 2 km radius. Standard errors clustered by slum of origin are reported in parentheses. 10%*, 5%**, 1%***.

Table A.9: Assignment location attributes and displaced families' characteristics at baseline

	Different characteristics of projects or districts of assignment								
	Home value (log UF)	HH Schooling at destination	# schools/ 1,000 students	Primary care/ 1,000 HH	Hospitals/ 1,000 HH	Distance to subway (1980)	Distance to CBD	Neighborhood size	Upward mobility
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Female HH	-0.936	0.005	0.020	0.001*	0.003	0.084	0.211**	-22.990	-0.024
	(1.252)	(0.019)	(0.041)	(0.000)	(0.002)	(0.076)	(0.085)	(15.440)	(0.036)
# children	0.194	-0.001	-0.008	-0.000	-0.000	-0.015	-0.020	1.256	-0.016
	(0.179)	(0.004)	(0.006)	(0.000)	(0.000)	(0.016)	(0.023)	(2.893)	(0.010)
Married HH	1.013	-0.005	-0.027	0.000	0.000	-0.089	-0.077	15.683	0.067*
	(0.816)	(0.015)	(0.022)	(0.000)	(0.002)	(0.073)	(0.088)	(10.655)	(0.034)
HH age	0.142	0.001	-0.003	-0.000	-0.000	-0.020**	-0.031***	2.947*	0.006
	(0.139)	(0.002)	(0.005)	(0.000)	(0.000)	(0.008)	(0.010)	(1.713)	(0.005)
Mapuche HH	2.472**	-0.003	-0.033	0.000	-0.000	-0.071	-0.113	31.409*	0.052
	(1.216)	(0.019)	(0.040)	(0.000)	(0.002)	(0.098)	(0.129)	(17.136)	(0.063)
HH born outside Stgo.	-0.349	-0.004	-0.014	0.000	-0.002	-0.058	-0.084	-7.824	0.032
	(0.527)	(0.011)	(0.012)	(0.000)	(0.001)	(0.051)	(0.068)	(7.925)	(0.036)
Slum's formal employment	125.672	1.736	0.404	-0.090***	0.495*	17.610**	15.759	-3.127	1.724
	(78.123)	(1.751)	(1.167)	(0.033)	(0.285)	(8.040)	(10.242)	(1264.381)	(3.880)
Adjusted R^2	0.598	0.517	0.242	0.572	0.461	0.283	0.314	0.542	0.357
Observations					8,547				
P-values for test of joint insignificance of baseline characteristics in regressions above									
	0.347	0.932	0.254	0.096	0.208	0.165	0.047	0.463	0.317

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

Table A.10: Auxiliary regression of displacement and change in location attributes

Outcome:	Labor earnings (1)	Δ Upward mobility$_{do'}$ (2)	Distance from origin (3)	Δ distance to CBD$_{do'}$ (4)	Share slum network in d (5)	Neighborhood size in d (6)	Home Value (7)
Displaced	-23.824***	-1.284***	5.729***	3.365***	-0.182***	551.792***	-16.356
	(6.430)	(0.345)	(1.164)	(0.662)	(0.044)	(82.217)	(9.962)
Upward mobility$_{o'}$	4.124***	-0.504***	0.371	0.239	0.005	43.087**	0.125
	(1.518)	(0.123)	(0.278)	(0.226)	(0.020)	(19.100)	(2.420)
Adj. R^2	0.086	0.722	0.806	0.553	0.611	0.628	0.612
Observations	28,560	28,560	28,560	28,560	28,560	28,560	28,560
% of Displacement effect explained by variable	-	0.5	14.1	8.4	9.6	25.5	6.5

Notes: The table shows results for coefficients β from regression $Y_i = \alpha + \beta Displaced_{s\{i\}} + \gamma \text{Upward mobility}_{o'} + \psi_o + \hat{p}_s + \hat{p}_s \times \psi_o + X_i'\theta + \varepsilon_i$. The row labeled "% of Displacement effect explained by variable" corresponds to the share of the displacement effect explained by each variable, calculated by multiplying the estimate from column (7) of Table 5 by the coefficient β in each column. Standard errors clustered by slum of origin are reported in parentheses. 10%*, 5%**, 1%***.

Table A.11: Displacement and distance

	Self-reported earnings (CLP$1,000/month)				
	(1)	(2)	(3)	(4)	(5)
Δ distance to CBD	-1.600***	-1.019	-1.010**	-0.168	-0.270
	(0.522)	(0.628)	(0.508)	(0.594)	(1.046)
Distance from origin		-0.701		-0.888*	-0.895*
		(0.468)		(0.494)	(0.491)
(Δ distance CBD)$\times$(Distance origin)					0.007
					(0.062)
Distance to CBD at origin			2.863*	3.384**	3.402**
			(1.568)	(1.500)	(1.517)
Upward mobility$_{o'}$	4.897***	4.975***	5.042***	5.167***	5.200***
	(1.679)	(1.671)	(1.540)	(1.513)	(1.573)
Adjusted R^2	0.086	0.086	0.086	0.086	0.086
Individuals	28,560	28,560	28,560	28,560	28,560

Notes: The table shows results for coefficients δ from regression $Y_i = \alpha + \delta \Delta Attribute_{o'd} + \gamma Upward\ mobility_{o'} + \psi_o + \hat{p}_s + \hat{p}_s \times \psi_o + X_i'\theta + \varepsilon_i$, similar to those of column (7) of Table 5. Standard errors clustered by slum of origin are reported in parentheses. 10%*, 5%**, 1%***.

Table A.12: Displacement effect and change in location attributes on labor earnings

	Total earnings (1)	Formal (contract) (2)	Informal (no contract) (3)	Taxable wages (4)	Years of schooling (5)
Δ Upward mobility$_{do'}$	0.083	-0.266	0.349	-0.572	0.017
	(0.956)	(1.106)	(0.733)	(2.523)	(0.026)
Distance from origin	-0.586	-0.409	-0.177	-0.423	-0.003
	(0.484)	(0.411)	(0.222)	(0.777)	(0.008)
Δ distance to CBD$_{do'}$	-0.592	-0.734	0.142	-1.542	-0.023*
	(0.579)	(0.466)	(0.372)	(1.117)	(0.013)
Share slum network in d	12.601	12.658*	-0.057	34.147**	0.677***
	(8.457)	(7.583)	(3.992)	(16.178)	(0.148)
Neighborhood size in d	-0.011**	-0.012***	0.001	-0.025****	-0.000***
	(0.005)	(0.004)	(0.002)	(0.008)	(0.000)
Home value	0.094*	0.144***	-0.051**	0.224**	0.002*
	(0.051)	(0.048)	(0.023)	(0.092)	(0.001)
Upward mobility$_{o'}$	5.118**	5.374***	-0.255	8.880***	0.129***
	(1.601)	(1.491)	(0.643)	(3.107)	(0.025)
Adjusted R^2	0.086	0.050	0.026	0.083	0.103
Observations	28,560	28,560	28,560	28,560	28,560

Notes: The table shows results for coefficients δ from regression $Y_i = \alpha + \delta \Delta Attribute_{o'd} + \gamma Upward\ mobility_{o'} + \psi_o + \hat{p}_s + \hat{p}_s \times \psi_o + X_i'\theta + \varepsilon_i$, equivalent to column (7) of Table 5. Standard errors clustered by slum of origin are reported in parentheses. 10%*, 5%**, 1%***.

Table A.13: Displacement effect on the probability of selling home by 2019

			Conditional on selling		
	Home ever sold (1)	Inheritance (2)	Log(Price) (3)	Year sold (4)	# years after treatment (5)
Displaced	-0.007	0.009	-0.077	-1.735	-0.520
	(0.010)	(0.011)	(0.204)	(2.081)	(2.027)
Adj. R^2	0.028	0.045	-0.019	0.031	0.043
Non-displaced mean	0.047	0.143	9.607	2009.077	26.820
Percent effect	**-14.9**	**6.3**	**-0.8**	**-0.09**	**-1.9**
Observations	3,995	3,995	224	224	224

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 and central areas of Greater Santiago. The data include 45 slums of origin, 9 municipalities of origin, and 15 municipalities of destination. Baseline controls include the following: female-headed household, number of children, married head of household, head of household's age, Mapuche head of household, average slums' formal employment, head of household's year-of-birth fixed effects, and year-of-intervention fixed effects. Standard errors clustered by slum of origin are reported in parentheses. 10%*, 5%**, 1%***.

Table A.14: Effects of displacement and subway rollout on labor market outcomes, 2007–2023

	Total earnings (1)	Taxable wages (2)	Formal earnings (3)	Informal earnings (4)	Employed (5)	Formally employed (6)
Panel A. Baseline displacement effect						
Displaced	-18.797**	-19.367	-17.889**	-0.908	-0.007	-0.008
	(7.790)	(14.325)	(8.093)	(3.198)	(0.015)	(0.013)
Panel B. Triple difference-in-difference						
Displaced	-15.805	47.750**	-4.865	-10.940	0.014	0.059***
	(15.741)	(22.502)	(14.722)	(6.939)	(0.030)	(0.022)
Treated	-3.198	30.098	1.525	-4.723	0.008	0.055***
	(14.107)	(20.233)	(13.193)	(6.444)	(0.028)	(0.015)
Post	27.345**	15.498	24.713***	2.632	0.038**	0.006
	(11.977)	(11.554)	(9.322)	(4.650)	(0.016)	(0.009)
Displaced* Treated	8.527	-72.073***	-4.802	13.330*	-0.008	-0.083***
	(14.694)	(21.767)	(13.747)	(6.988)	(0.031)	(0.022)
Displaced* Post	-20.640	-34.760**	-22.480**	1.840	-0.035**	-0.022*
	(12.642)	(15.898)	(9.725)	(5.579)	(0.017)	(0.011)
Treated*Post	-20.415	-17.800	-19.720*	-0.694	-0.033*	-0.023**
	(13.011)	(15.566)	(11.181)	(5.269)	(0.017)	(0.011)
Displaced*Treated* Post	13.544	35.187*	15.793	-2.250	0.024	0.026*
	(14.057)	(20.667)	(12.264)	(6.190)	(0.020)	(0.014)
Adjusted R^2	0.099	0.083	0.058	0.029	0.093	0.080
Observations	303,818	303,818	303,818	303,818	303,818	303,818

Notes: The table shows regressions for children aged 0–18 at baseline who are matched to the RSH data in the panel dataset from 2007 to 2023, equivalent to equation (3). There are 74 slums in the sample. See the text for variable definitions. 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's formal employment, year-of-intervention fixed effects, and year-of-birth fixed effects. Standard errors clustered by slum of origin are reported in parentheses. 10%*, 5%**, 1%***.

Table A.15: Effects of displacement and subway rollout on location outcomes, 2007–2023

	Municipality of assignment (1)	Municipality of origin (2)	Formally employed in munic. of residence (3)	Commuting distance (4)
Panel A. Baseline displacement effect				
Displaced	-0.019	-0.330***	-0.002	2.024**
	(0.044)	(0.068)	(0.006)	(0.775)
Panel B. Triple difference-in-difference				
Displaced	-0.358**	-0.424***	-0.023*	3.542**
	(0.162)	(0.100)	(0.012)	(1.452)
Treated	-0.181**	-0.094	0.001	2.025*
	(0.088)	(0.075)	(0.009)	(1.164)
Post	0.066	-0.041	0.013**	-0.822
	(0.055)	(0.044)	(0.005)	(0.566)
Displaced* Treated	0.374**	0.090	0.020	-1.572
	(0.176)	(0.088)	(0.013)	(1.480)
Displaced* Post	0.009	0.042	-0.010*	0.821
	(0.080)	(0.053)	(0.006)	(0.686)
Treated*Post	-0.101*	0.016	-0.026***	1.292**
	(0.057)	(0.051)	(0.007)	(0.599)
Displaced*Treated* Post	0.026	-0.032	0.017**	-1.291
	(0.085)	(0.070)	(0.008)	(0.792)
Adjusted R^2	0.065	0.318	0.014	0.061
Observations	303,818	303,818	303,818	110,235

Notes: The table shows regressions for children aged 0–18 at baseline who are matched to the RSH data in the panel dataset from 2007 to 2024, equivalent to equation (3). Columns (1)–(3) use a sample of 74 slums, and column (4) uses a sample of 48 slums. See text for variable definitions. Commuting distance computed as the distance between the municipality of residence and the employer's municipality centroids. Employers' information is only available in the AFC. 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's formal employment, year-of-intervention fixed effects, and year-of-birth fixed effects. Standard errors clustered by slum of origin are reported in parentheses. 10%*, 5%**, 1%***.

Table A.16: Comparison of displacement/mover estimates across studies

Study	Setting (1)	% Δ outcome (2)	% Δ neighborhood quality (3)
Panel A. Labor market outcomes			
Barnhardt et al. (2016)	Housing lottery in Ahmedabad, India (adults)	-7.7% household income; -2.4 labor force participation	-37.5% urbanicity; -8.1% housing value
Picarelli (2019)	Relocation program in six main metropolitan areas in South Africa (adults)	0.94 labor supply index (no percent);	50% distance (km) to CBD
Franklin (2020)	Housing relocation program in Cape Town (adult slum dwellers)	18% earnings	1.3% distance (km) to CBD
Kumar (2021)	Housing lottery in Mumbai, India (adults)	16% earnings in the median range of household income; 10% employment	-1.75% employment, but varies by outcome
Belchior et al. (2024)	Social housing program in Brazil (adults)	-1.03% formal employment, 7.7% for disadvantaged sample; -0.05% earnings, 0.43% in disadvantaged sample	0.93% labor market access
This paper	Program for Urban Marginality (children 0–18 in Chile)	-10.4% earnings; 0.16% employment; -5.6% formal employment	-4.0% upward mobility; 19% distance (km) to CBD
Panel B. Schooling outcomes			
Barnhardt et al. (2016)	Housing lottery in Ahmedabad, India (children)	-2.25% years of schooling	-37.5% urbanicity; -8.1% housing value
Camacho et al. (2022)	Free housing program in Colombia (children)	5.7% years of schooling; 17% high school completion	-9.8% distance (km) to schools
Agness and Getahun (2024)	Housing lottery in Addis Ababa, Ethiopia (children)	4.5%-11% school enrollment; 10.5% secondary school completion; 16% post-secondary attendance	0.863 SD neighborhood quality index
This paper	Program for Urban Marginality (children 0–18 in Chile)	-5.7% years of schooling; -15.5% high school graduation; -32.2% college attendance	-4.0% upward mobility; 19% distance (km) to CBD

Notes: This table presents percent effect estimates of displacement or treatment effects from experiments that induce individuals to move to a new neighborhood. Percent effect is defined as the treatment effect divided by the mean of the control group, if available; otherwise, the main result is presented in standard deviations. Panel A presents results for labor market outcomes, and Panel B presents results for educational outcomes. Column (2) shows the percent effect on the relevant outcome, and column (3) shows the percent effect on neighborhood outcomes.

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 pre-program slum characteristics. 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 with the 1979 slum census conducted by MINVU (MINVU, 1979), a list of displaced slums collected by Molina (1986), and slum locations documented by Benavides et al. (1982). Together, these sources allow us to characterize 233 slums. However, this does not represent the complete universe of slums, as many lack location data or changed names after 1973—making it more difficult to track them over time—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 233 slums represents our most comprehensive effort to analyze and compare their characteristics. For example, Molina (1986) documents that in 1979, MINVU targeted 51,797 families in 340 slums, of whom 70% would be displaced. However, based on the author's data collection, only 40,491 families were treated by 1984. Additionally, Morales and Rojas (1986) find that more than 300 slums were treated by 1985, with 60% being displaced but only in urban areas. Another feature of their data is that many of the slums they considered as non-displaced were split into various smaller slums and included projects completed 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 and their locations, though the non-displaced slums are over-represented compared to other historical sources.

We use the slum characteristics in Table 1 to estimate the probability of relocation versus redevelopment. Columns (1) and (2) of Table B.1 report slum characteristics by treatment. Column (3) shows the results of estimating a logit regression of the probability of relocation, using all variables in the table as covariates except the price index for surrounding property prices, which is excluded from the main specification as it may reflect expectations of future relocations. The estimates show the expected signs: as discussed in the historical sources, relocation is more likely when a slum does not have a military name and has a lower elevation, higher slope, and higher flooding risk. Slums are also more likely to be relocated from wealthier neighborhoods, measured by population education attainment.

Many of the previous variables may be correlated with one another, so to avoid overfitting our propensity score model, we estimate the probability of relocation using a LASSO-logit regression, allowing the data to choose the main predictors of relocation. We include density and elevation as variables that are always included in the model (i.e., their penalties are never zero). The results are presented in column (4) of Table B.1. In addition to density and elevation), the LASSO model chooses slope, flooding risk, and population education attainment as predictors of relocation. Interestingly, most of these determinants are variables that determine the feasibility of providing basic services on-site, such as sewage or electricity.

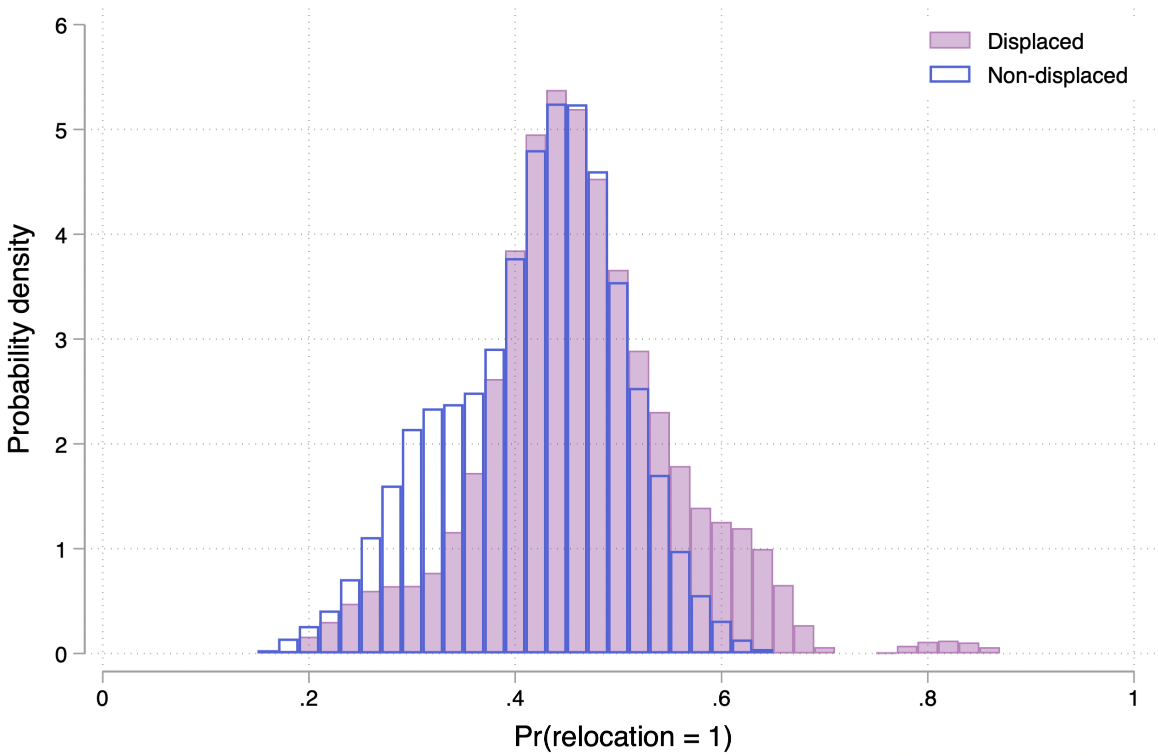
Table B.1: Slum characteristics and determinants of relocation

	Displaced mean (1)	Non-displaced mean (2)	Pr(relocation=1) (3)	Pr(relocation=1) (LASSO) (4)
Panel A. Slum attributes				
Families/hectare	70.868	61.379	0.003	0.003
			(0.003)	(0.003)
Military name	0.137	0.191	-0.298	
			(0.385)	
Elevation (mas)	570.873	586.305	-0.005**	-0.005*
			(0.002)	(0.003)
Slope (degrees)	2.833	2.643	0.137	0.122
			(0.093)	(0.085)
Close to river/canal (<100 m)	0.049	0.031	0.024	
			(0.818)	
Flooding risk	0.059	0.09	1.219	1.643
			(1.221)	(1.336)
Distance to CBD	9.838	10.289	0.024	
			(0.044)	
Panel B. Census district attributes				
Population education attainment	7.799	7.164	0.635***	0.392
			(0.199)	(0.258)
Unemployment rate	0.191	0.199	13.643**	
			(5.900)	
Number of schools	4.015	4.290	-0.057	
			(0.051)	
Log property prices	14.793	14.739		
Number of slums	102	131	233	233
Number of municipalities	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 education attainment are measured at the census district level where a slum was located. Column (3) reports the estimate of a logit regression of the probability of relocation on all slum covariates, and column (4) reports the estimates of a LASSO-logit regression of the probability of relocation, where density and elevation are always included in the model. Robust standard errors are reported in parentheses in column (3), and bootstrapped standard errors (200 reps.) are reported in column (4). 10%*, 5%**, 1%***.

We use the estimates in column (4) to predict the propensity score $\hat{p}(X_s)$. As shown in Figure B.1, the propensity scores vary between 0.18 and 0.9 in the full sample of slums, and

Figure B.1: Distribution of the probability of slum relocation versus redevelopment



Notes: The figure plots the fitted values from a LASSO-logit regression of the probability of slum relocation on slum attributes in Table 1, for the full sample of slums stratified by treatment. The LASSO estimation selects slum density, elevation, slope, flooding risk, and population education attainment as determinants of relocation.

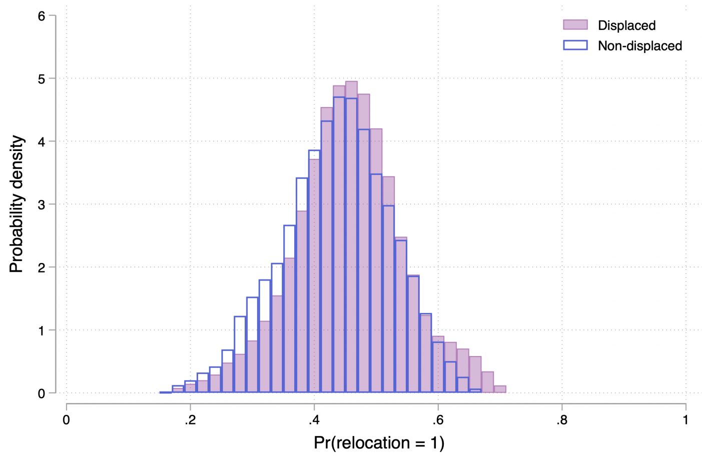
on average, the estimated probability of relocation is higher for displaced slums compared to non-displaced slums (in purple and blue, respectively). Importantly, the distributions overlap, guaranteeing there is common support between treatments in the range of 0.23 and 0.60.

Panel (a) of Figure B.2 shows the propensity score estimates in the sample of slums found in the archives. The densities between treatments are very similar to each other, and the range of values is almost the same between displaced and non-displaced slums ($0.18 < \hat{p}(X_s) < 0.7$). This pattern is very much expected, as the slums found in the archives are more similar to each other between treatments (recall columns (5)–(7) of Table 1). These results show that most of the slums missing from the archival sample are displaced slums with a high probability of relocation and non-displaced slums with a medium or low probability of relocation.

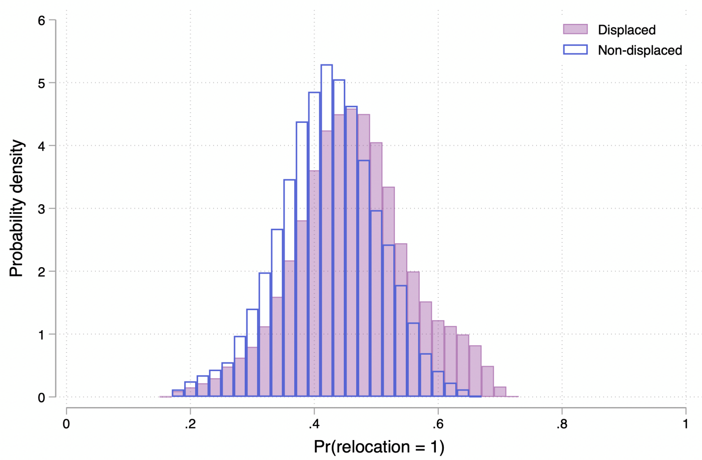
The similarity in density distributions does not invalidate our main empirical strategy, as we aim to compare children in families from slums with similar probabilities of relocation. Instead, it raises concerns about selection into the sample. To address this issue, we perform an exercise in which we re-weight the slums we find in the archives by their inverse probability of being found, stratified by type of treatment, so that our archival sample is more similar to the full sample of slums in terms of their probability of treatment. To do so, we run a logit regression of the likelihood of finding a slum in the archives on all the slum characteristics of Table B.1, excluding the price index, and estimate it separately for displaced and non-displaced slums. Recall that we are more likely to find larger slums and those closer to the CBD. We then compute weights as the inverse of the predicted probability.

Panel (b) of Figure B.2 plots the re-weighted propensity score densities by treatment. After re-weighting, there is a higher incidence of displaced slums with a higher probability of relocation, making the distribution more similar to the one in Figure B.1. The idea behind this exercise is to assign a higher weight to displaced slums with a higher probability of relocation so that the selected sample is more similar to the full sample. We use these slum-level sampling weights to perform robustness checks on our baseline specifications. See Appendix Section C for details on selection and attrition.

Figure B.2: Distribution of the probability of slum clearance in the archival sample



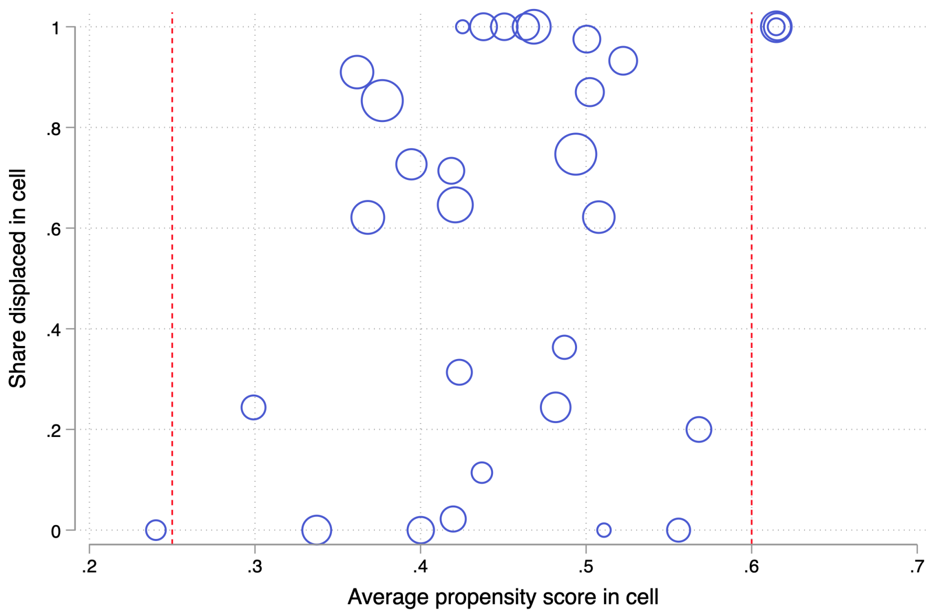
(a) Urban slums in the archives



(b) Urban slums in the archives (weighted)

Notes: Panel (a) plots the propensity score estimates of Table B.1, column (4) in the sample of 98 slums found in the archives. Panel (b) re-weights each observation by the inverse probability of finding a slum in the archives. The probability of finding a slum in the archives is computed using a logit model on slum characteristics and stratified by treatment (see text).

Figure B.3: Treatment variation by propensity score



Notes: The figure shows the proportion of displaced children per cell, ordered by the cell's average propensity score. A cell is defined as the combination between an origin municipality and a high (above-median) or low (below-median) propensity score in the baseline sample. Red lines indicate the boundaries of the common support. Each observation is weighted by the number of children in the corresponding cell in the baseline sample. In column (7) of Table A.6 we exclude from the estimation sample all cells in the figure with no variation in treatment, that is, all cells with a share of displaced children equal to 0 or 1.

C Attrition

C.1 Sample selection in archival sample of slums

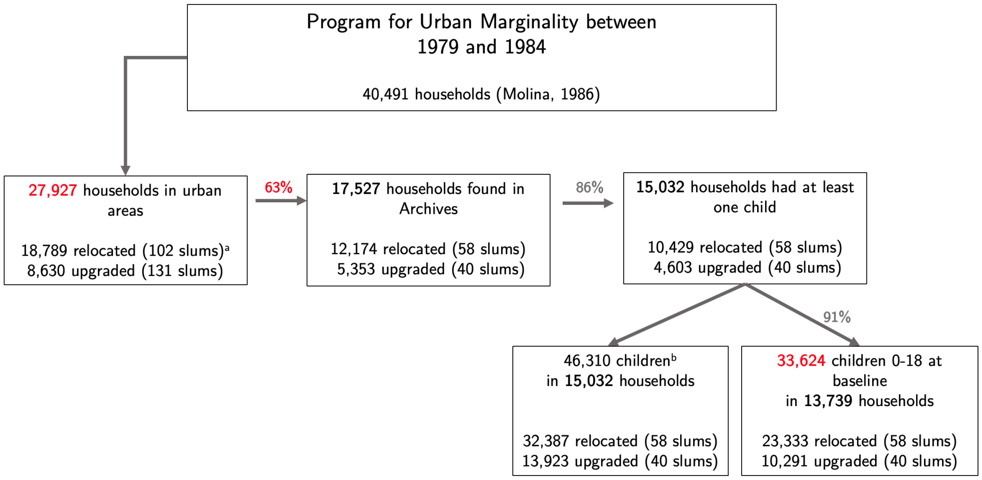
The first stage of attrition is the selection of slums into our sample. In the text and Data Supplement, we described the process of finding the homeownership data in the archives. Unfortunately, in our archival records, we did not find all the slums that were part of the Program for Urban Marginality, and we were more likely to find larger slums and larger destination projects that were less remote, as they were closer to the CBD. Figure C.1 summarizes the data collection process from the archival records.

Because the slums in our sample are not a random sample of those in the program, in the previous appendix section we constructed sample weights by treatment at the slum level, such that by using these sampling weights, we recover similar propensity score distributions to those in the full sample of slums. The sampling weights assign a higher weight to displaced slums with a high probability of treatment and to non-displaced slums with a low probability of treatment.

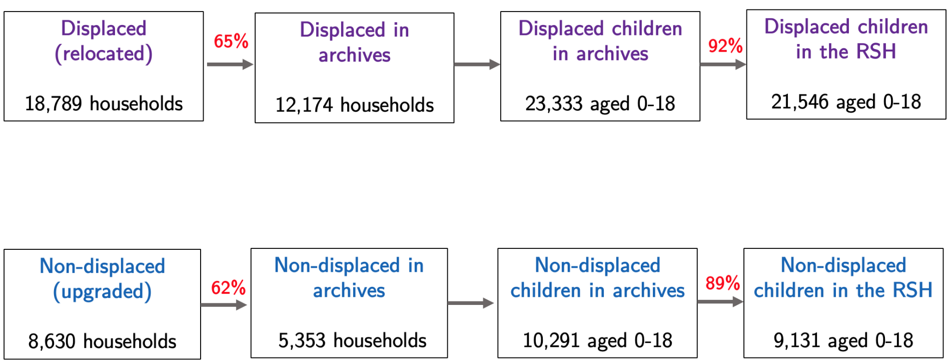
We estimate our baseline results on children's income, wages, and schooling in adulthood using sampling weights by slum. The results equivalent to Panel A of Table 3 are presented in Table C.1. The estimates are very similar to our baseline results and are slightly more negative, suggesting that selection into the sample does not explain our results.

Figure C.1: Summary of data collection and attrition rates

(a) Archival records



(b) Matching rates by treatment



Notes: Panel (a) compares the number of slums in our archival records to those reported by Molina (1986). We report the number of slums from Morales and Rojas (1986) instead, as Molina (1986) does not include the total number of non-displaced slums by the end of the program. Panel (b) shows attrition rates from the full program to the RSH administrative records by treatment. The sample includes any children born before treatment as well as children born after treatment until 2016. Percents in red represent matching rates, while percents in gray represent the fraction from the corresponding sample. Because not all families had children, we assume that the number of children we find is representative of the households in the archives.

C.2 Attrition in administrative data

The second stage of attrition in constructing our baseline sample is selection into the outcome variables due to differential matching rates by treatment with administrative data. In this section, 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 adjusts for differential attrition between treatment and control groups. 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 ended up in a non-displaced slum but not if they ended 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 from the archival sample of children to the RSH is 92.2% for the displaced group and 88.7% for the non-displaced group. However, because we are more likely to find displaced households in the archives, the total matching rate from the full program to the RSH is 60% for displaced children (0.92*0.65) and 55% for non-displaced children (0.89*0.62), as shown in Figure C.1, panel (b). We therefore trim (60–55)/60 = 8.3% of the displaced observations, with the lower bound occurring when trimming observations with the highest earnings (or corresponding outcome) and the upper bound when trimming those 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 trim manually instead of relying on the command in Stata or R.

Table C.2 presents the results for different outcomes in each panel. Column (1) estimates the regression by propensity score re-weighting. Column (2) is the equivalent to our propensity score matching baseline result in column (3) of Table 3 and uses the inverse probability of finding a slum in the archives as sampling weights (see discussion in the previous subsection).

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 potential selection bias from differential attrition.

Last, we trim our sample by slum. In a similar spirit as with Lee bounds and sampling weights, we consider what the distribution of slums would be in our sample if we had a similar proportion of displaced and non-displaced slums, as in Morales and Rojas (1986). This requires trimming 45% 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 45% "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 45% excess displaced slums 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, only on the slums. Thus, we trim 24 slums under this procedure, and the results are presented in Table C.3. 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: Displacement effect on labor income and schooling

	Self-reported earnings (CLP$1,000/month)				Taxable wages	Years of schooling
	(1)	(2)	(3)	(4)	(5)	(6)
Displaced	-31.673***	-31.351***	-27.188***	-26.162***	-48.960***	-0.609***
	(7.097)	(6.899)	(5.666)	(6.860)	(11.300)	(0.118)
Non-displaced mean	245.130	245.130	247.889	247.889	359.788	11.225
Percent effect	**-12.9**	**-12.8**	**-11.0**	**-10.6**	**-13.6**	**-5.4**
Adjusted R^2	0.082	0.082	0.083	0.084	0.083	0.109
Individuals	28,560	28,560	28,560	28,560	28,560	28,560
Baseline controls	✓	✓	✓	✓	✓	✓
Slum characteristics		✓				
ψ_o			✓	✓	✓	✓
$\hat{p}_s$			✓	✓	✓	✓
$\hat{p}_s \times \psi_o$				✓	✓	✓

Notes: The table shows regressions for children aged 0–18 at baseline who are matched to the RSH and AFC data, weighted by sampling weights at the slum level. Standard errors clustered by slum of origin are reported in parentheses. 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 "Percent effect" stands for percentage variation with respect to the non-displaced mean. The non-displaced means in columns (3)–(6) are computed conditional on $\hat{p}_s$. 10%*, 5%**, 1%***.

Table C.2: Robustness of the displacement effect to attrition by individual

	Model		
	Inv-p-score	P-score	P-score (sampling weight)
	(1)	(2)	(3)
Panel A. Self-reported earnings (CLP$1,000/month)			
Displacement effect	-23.769	-24.992	-26.162
	(5.514)***	(6.435)***	(6.860)***
Upper bound	-4.234	-5.142	-6.363
	(5.824)	(6.706)	(7.148)
Lower bound	-80.595***	-79.730***	-79.260***
	(4.279)	(5.339)	(5.712)
Imbens and Manski (2004) CI	[-73.732,-27.721]		
Panel B. Formal wages (CLP$1,000/month)			
Displacement effect	-36.298	-43.622	-48.960
	(10.233)***	(10.836)***	(11.300)***
Upper bound	-8.800	-14.974	-20.006*
	(10.353)	(11.290)	(11.607)
Lower bound	-148.049***	-152.885***	-154.828***
	(8.618)	(9.069)	(10.019)
Imbens and Manski (2004) CI	[-130.000, -29.793]		
Panel C. Outcome: Years of schooling			
Displacement effect	-0.654	-0.648	-0.609
	(0.103)***	(0.117)***	(0.118)***
Upper bound	-0.113	-0.115	-0.067
	(0.089)	(0.106)	(0.105)
Lower bound	-1.121***	-1.104***	-1.046***
	(0.102)	(0.120)	(0.120)
Imbens and Manski (2004) CI	[-0.993, -0.363]		
Selected individuals	28,560	28,560	28,560
Trimming portion	8.3%	8.3%	8.3%
Municipality-of-origin FE	✓	✓	✓
$\hat{p}(X_s)$		✓	✓
$\hat{p}(X_s) \times \psi_o$		✓	✓
Baseline controls	✓	✓	✓

Notes: Column (1) estimates the regression by propensity score re-weighting, and column (2) is the equivalent to our propensity score matching result in column (5) of Table 3 that fully controls for the interaction between propensity score dummies and municipality-of-origin fixed effects. Column (3) uses sampling weights by slum. Standard errors clustered by slum of origin are reported in parentheses, and Imbens and Manski (2004)'s confidence intervals are produced by Stata's leebounds command, tightened by municipality of origin, with a smaller trimming portion of 3.8% that considers differential matching rates in the administrative data only. The analysis includes a total of 94 unique slums. 10%*, 5%**, 1%***.

Table C.3: Robustness of the displacement effect to attrition by slum

	Model		
	Inv-p-score	P-score	P-score (sampling weight)
	(1)	(2)	(3)
Panel A. Self-reported earnings (CLP$1,000/month)			
Upper bound	-16.009***	-15.376**	-14.263*
	(5.940)	(6.943)	(7.606)
Lower bound	-40.222***	-37.607***	-36.725***
	(5.151)	(5.882)	(5.864)
Ind. upper	16,012	16,012	16,012
Ind. lower	23,376	23,376	23,376
Panel B. Formal wages (CLP$1,000/month)			
Upper bound	-16.011*	-30.961**	-36.179***
	(8.396)	(12.180)	(12.871)
Lower bound	-57.001***	-63.665***	-65.497***
	(11.806)	(14.280)	(14.677)
Ind. upper	15,916	15,916	15,916
Ind. lower	24,309	24,309	24,309
Panel C. Years of schooling			
Upper bound	-0.341***	-0.387***	-0.322***
	(0.108)	(0.104)	(0.109)
Lower bound	-0.890***	-0.918***	-0.842***
	(0.106)	(0.126)	(0.123)
Ind. upper	17,866	17,866	17,866
Ind. lower	22,928	22,928	22,928
Unique slums	70	70	70
Trimming portion (slums)	45%		

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

D Additional robustness checks

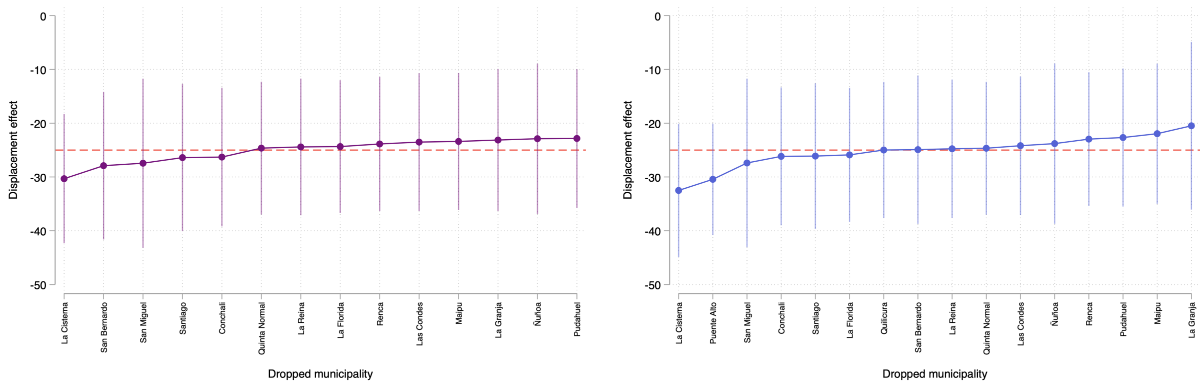
D.1 Selection on unobservables

Table D.1: Displacement effect instrumented by original assignment

	Labor earnings (1)	Formal wages (2)	Years of schooling (3)
Panel A. OLS			
Displaced	-22.007***	-23.937**	-0.547***
	(7.343)	(9.598)	(0.152)
Adjusted R^2	0.082	0.088	0.087
Panel B. Propensity score matching			
Displaced	-16.992**	-39.740***	-0.503***
	(7.237)	(8.846)	(0.141)
Adjusted R^2	0.084	0.089	0.092
Panel C. Instrumental variable			
Displaced	-25.878***	-11.942	-0.467***
	(8.883)	(11.697)	(0.177)
Adjusted R^2	0.082	0.088	0.087
Observations	18,985	18,985	18,985

Notes: The table shows regressions for children aged 0–18 at baseline, matched to the RSH and the AFC data, and treated between 1981 and 1984. Standard errors clustered by slum of origin are reported 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's 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

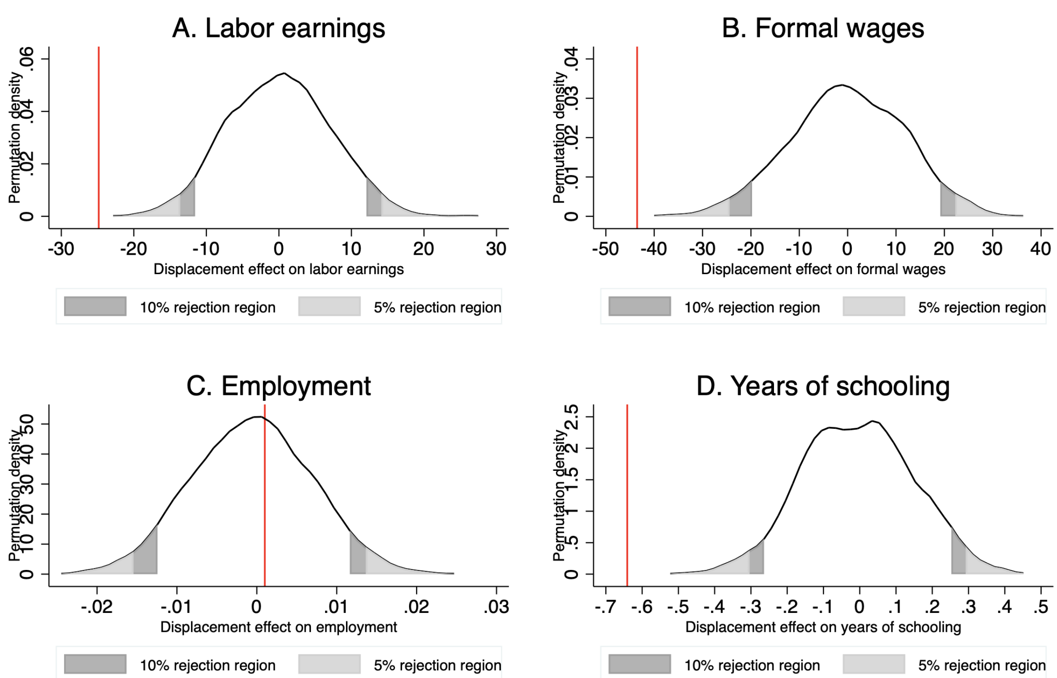


(a) Municipalities of origin

(b) Municipalities of destination

Notes: The figures plot the displacement coefficient in column (5) of Table 3 for labor income and its 95% confidence interval, dropping each municipality of origin one by one (panel (a)) or each municipality of destination one by one (panel (b)). Standard errors clustered by slum of origin are reported in parentheses.

Figure D.2: Permutation tests



Notes: The figures show the distribution of permutation tests on main outcomes performed in 1,000 replications. Red lines indicate the average displacement effect equivalent to column (5) of Table 3, and gray areas indicate 10% and 5% rejection regions.

D.2 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

$$Y_{it} = \alpha + \beta Displaced_{s\{i\}} + \psi_o + \varepsilon_{it}, \tag{4}$$

$$Y_{it} = \tilde{\alpha} + \tilde{\beta} Displaced_{s\{i\}} + \tilde{\psi}_o + X_{it}'\theta + \tilde{\varepsilon}_{it}, \tag{5}$$

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 is 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:

$$\beta^* \sim \tilde{\beta} + \delta(\tilde{\beta} - \beta)\frac{R_{max} - \tilde{R}}{\tilde{R} - R}, \tag{6}$$

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 $\delta = 1$. Table D.2 presents the results.

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

Outcome	R^2 max	$\hat{\delta}$	δ	$\hat{\beta}^*$
Labor earnings	1.3	212.78	1	-25.078
	1.3		2	-25.166
	1.3		3	-25.256
	3	32.810	1	-25.597
	3		2	-26.275
	3		3	-27.039
Formal wages	1.3	-222.234	1	-44.158
	1.3		2	-44.702
	1.3		3	-45.254
	3	-34.151	1	-47.360
	3		2	-51.534
	3		3	-56.226
Years of schooling	1.3	-59.098	1	-0.664
	1.3		2	-0.680
	1.3		3	-0.696
	3	-9.889	1	-0.758
	3		2	-0.884
	3		3	-1.030
Included controls:				
Baseline controls				✓
$\hat{p}(X_s) + \psi_o + \hat{p}(X_s) \times \psi_o$				✓

E Intergenerational mobility estimates in Chilean municipalities

In this section, we describe the methodology used to produce intergenerational mobility estimates by municipality in Chile, which we use to characterize place effects in Section 6 of the main text.

E.1 Data

We use earnings measures available in the RSH between 2016 and 2023. Although the RSH contains information from 2007 and 2023, a corrected measure of household income—referred to as "corrected income"—has been computed by the Ministry of Social Development since 2016. This measure uses individuals' self-reporting earnings and employment and is complemented with all other forms of administrative records the Ministry has access to, including taxable income from the Chilean Internal Revenue Service, formal income from the AFC, social security contributions for pensions and health services, and social benefits. Thus, corrected income is the most comprehensive measure of household income available in our data.

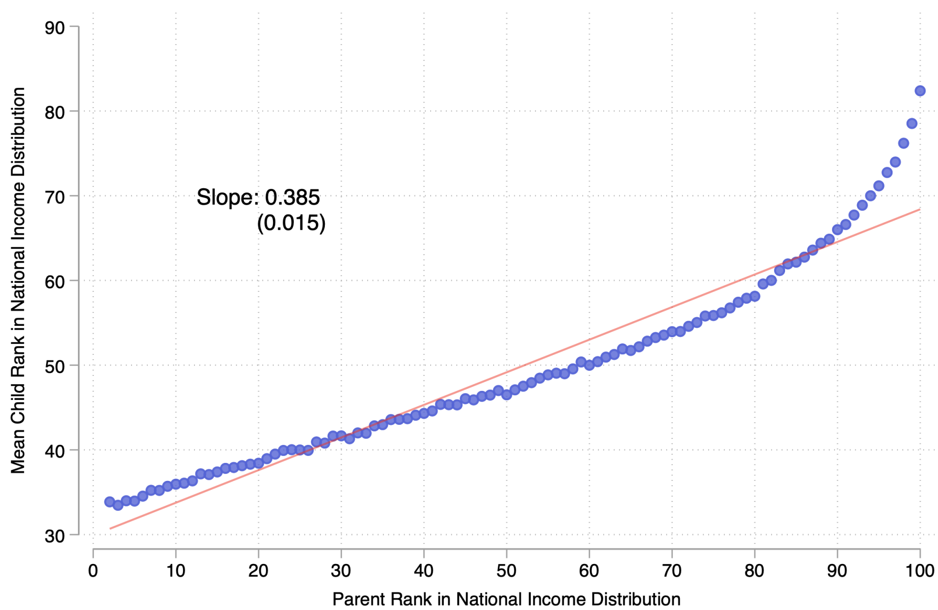
However, a caveat is that its availability beginning in 2016 restricts the cohorts and ages of children included our baseline sample. To address this, in some of the exercises presented in this section, we also use administrative records from the AFC to compute children's earnings dating back to 2002. In addition, because the RSH is used to allocate social benefits, it excludes 10% of the population, which are those with the highest earnings. As a result, our sample is not representative of very high-income households in Chile.

We select children born between 1985 and 1998 who we observe in the RSH data and for whom we observe their parents' municipalities of residence in the RSH when the children are between 0 and 30 years old. We identify parent-child links using the universe of birth records. Next, we compute the average corrected income for parents in the sample, and for children we compute two measures: average corrected income in the whole sample and average corrected income between ages 30–35. As expected, this last measure has more missing information given the constraints imposed by the years of available data, so it only includes children born between 1985 and 1993.

E.2 *Measures of intergenerational mobility*

We measure intergenerational mobility by estimating the correlation between parent and child income percentile rank. First, we use our baseline sample of children and parents to compute the ranks of corrected income, followed by averaging the children's income rank for each value of the parent's rank. Figure E.1 shows the result from this exercise. For comparability across birth cohorts, the figure includes only children born between 1985 and 1993 for whom we observe corrected income between ages 30 and 35. It shows that the rank-rank correlation between parents' and children's income in Chile is 0.385. This estimate is above that for the US (0.34; Chetty et al., 2014), which lies in the upper portion of the distribution among developed countries but below the estimate for Brazil (0.55; Britto et al., 2025), another Latin American country with historically high levels of inequality similar to that of Chile.

Figure E.1: Mean child income rank versus parent income rank



Notes: The figure shows children in Chile born between 1985 and 1993, whose income is measured at ages 30–35. Income measured as "corrected household income" is available in the RSH for children and parents between 2016 and 2023.

E.3 *Outcomes of permanent residents*

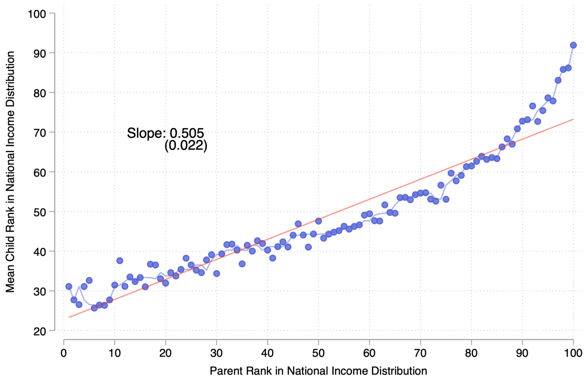
The goal of this exercise is to compute measures of intergenerational mobility that proxy for measures of neighborhood quality that we can use in equation (2). Hence, following

Chetty and Hendren (2018a), we compute intergenerational mobility estimates for children of parents who are permanent residents of each corresponding municipality. Thus, we keep parents we observe living in the same municipality during the period 2007–2023 when their children are young. Next, we compute rank-rank correlations by municipality of residence for children born between 1985 and 1990. Because we want the birth cohorts to be as close as possible to our baseline sample of children from slum-dwelling families, we keep children born between 1985 and 1990, who are the next closest group.[50] Additionally, because of power issues, we keep all children with income information after the age of 25 in the RSH.

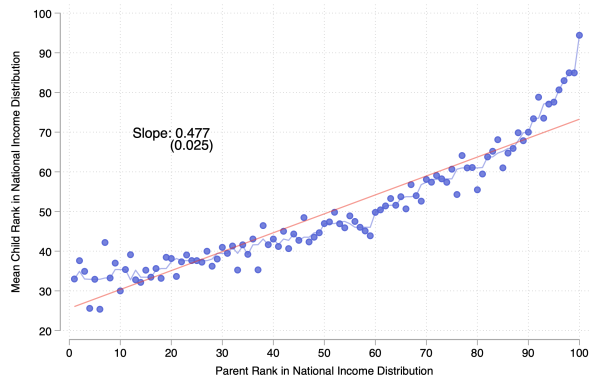
We characterize children's mean income rank, conditional on their parents' income rank, separately for each municipality. Figure E.2 shows the result from this exercise for four different municipalities in Greater Santiago. These municipalities were selected to reflect different socioeconomic statuses and illustrate the variation present in our data. Using these estimates, we next predict a child's average income rank conditional on her parent's rank being at the 25th or 75th percentile of the national income distribution. The results for Greater Santiago are plotted in the maps of Figure E.3. We refer to the measures in panel (a) as upward mobility estimates for permanent residents; these are the variables used in equation (2) in the main text.

[50] We also compute estimates for two other cohort groups, 1991–1995 and 1996–2000, and find very similar patterns.

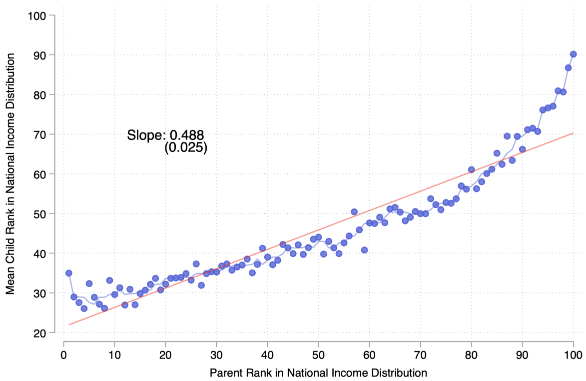
Figure E.2: Mean child income rank versus parent income rank for municipalities in Greater Santiago



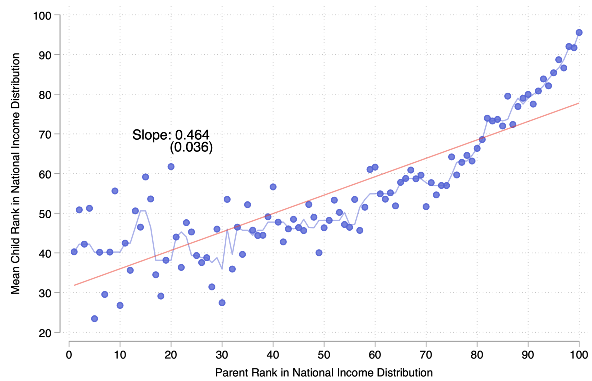
(a) San Bernardo



(b) Renca



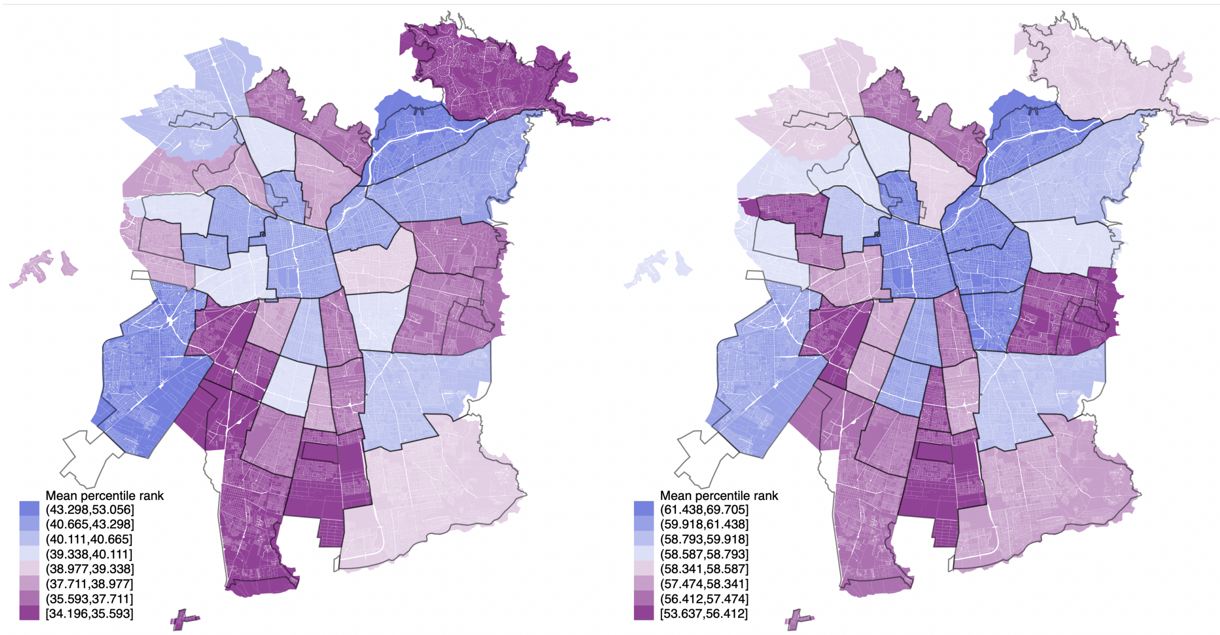
(c) La Pintana



(d) Las Condes

Notes: The figure shows intergenerational mobility estimates for children who were raised in Chilean municipalities and born between 1985 and 1993, with income measured at ages 30–35. Income measured as “corrected household income” is available in the RSH for children and parents between 2016 and 2023. Light blue lines correspond to a non-linear smoother with five spans. We show estimates for four different municipalities of varying levels of socioeconomic status. San Bernardo is a large municipality with a high proportion of middle-class families (panel (a)). Renca and La Pintana are municipalities with high levels of low-income populations (panels (b) and (c)), located in the northwest and southern areas of the city, respectively. Finally, Las Condes is a wealthy municipality located in the northeast of Santiago.

Figure E.3: Mean income ranks for children of permanent residents in Greater Santiago



(a) For children with parents at p=25 **(b)** For children with parents at p=75

Notes: The figure shows maps of intergenerational mobility estimates for children who were raised in Chilean municipalities and born between 1985 and 1990, whose parents are permanent residents. Panel (a) plots the mean income rank by municipality of residence for children whose parents are at the 25th percentile of the national income distribution. Panel (b) plots the mean income rank by municipality of residence for children whose parents are at the 75th percentile of the national income distribution.

E.4 Exposure effect of neighborhoods

We follow Chetty and Hendren (2018a) and Chetty and Hendren (2018b) to estimate the causal effect of neighborhoods in our sample of Chilean households and then use the estimates as controls in our baseline regressions. First, we estimate average neighborhood effects by age, using as inputs the rank-rank correlation estimates of upward mobility computed in the previous section. To do this, we select children of parents who move once in our sampling period and estimate the predicted rank difference for each moving child based on the municipalities of origin and destination. Due to lack of power, we include children born until 2000 and estimate the following equation:

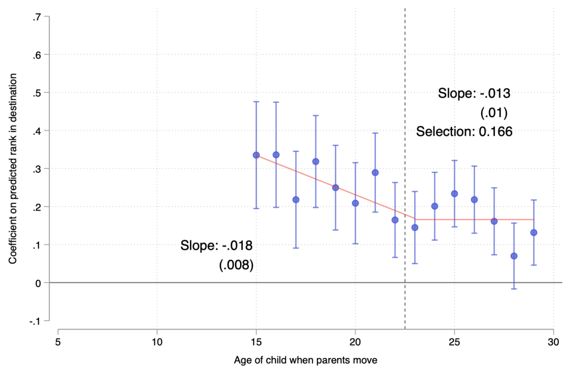
$$y_i = \sum_{s=1}^{3} I(S_i = s)(\alpha_s^1 + \alpha_2^2 \bar{y}_{pos}) + \sum_{m=7}^{30} (\chi_m^1 + \chi_m^2 p_i) + \sum_{m=7}^{30} b_m I(m_i = m)\Delta_{odps} + \sum_{s=1}^{3} I(s_i = s)\Delta_{odps} + \varepsilon_i, \tag{7}$$

where s indexes three different cohort groups (1985–1990, 1991–1995, and 1996–2000) and m is the age of a child when her family moves from municipality of origin o to municipality of destination d. Δ_{odps} is the change in upward mobility for a child whose family moves from o to d from cohort s and whose parents are in percentile p of the national income distribution for her corresponding cohort. As Chetty and Hendren (2018a) explain, this is the parametric version to estimate exposure effects b_m, where the first two terms of the specification control for origin quality and disruption effects. The third term represents the exposure effects of interest (b_m), and the fourth term consists of cohort interactions to control for differential measurement error across cohort groups.

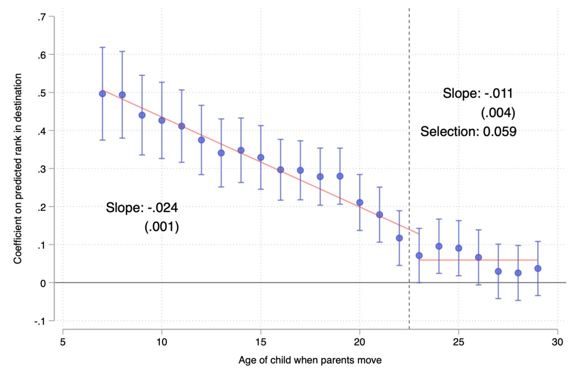
The results of estimating equation (7) are presented in panel (a) of Figure E.4. This panel uses corrected income in the sample of children, which limits the range of observable ages, starting at age 15. To address this, panel (b) presents the results of repeating the exercise using taxable wages from the AFC, which increases the number of observations and allows us to observe children who moved from age 7 onward. Importantly, both panels illustrate the established concept of the exposure effect of neighborhoods: children who move at younger ages to higher-quality municipalities—where quality is measured by upward mobility—experience greater increases in their adult earnings.

Finally, we estimate causal place effects by relating children's income rank at ages 30–35 to their predicted rank differences based on the age at which they moved, as suggested by the previous exposure effects. We follow the methodology of Chetty and Hendren (2018b) to estimate the causal effect of municipalities on children's earnings as adults. These effects, called μ_{p25} and μ_{p75}, are plotted in the maps presented in Figure E.5 for the municipalities of Greater Santiago.

Figure E.4: Exposure effect of neighborhoods



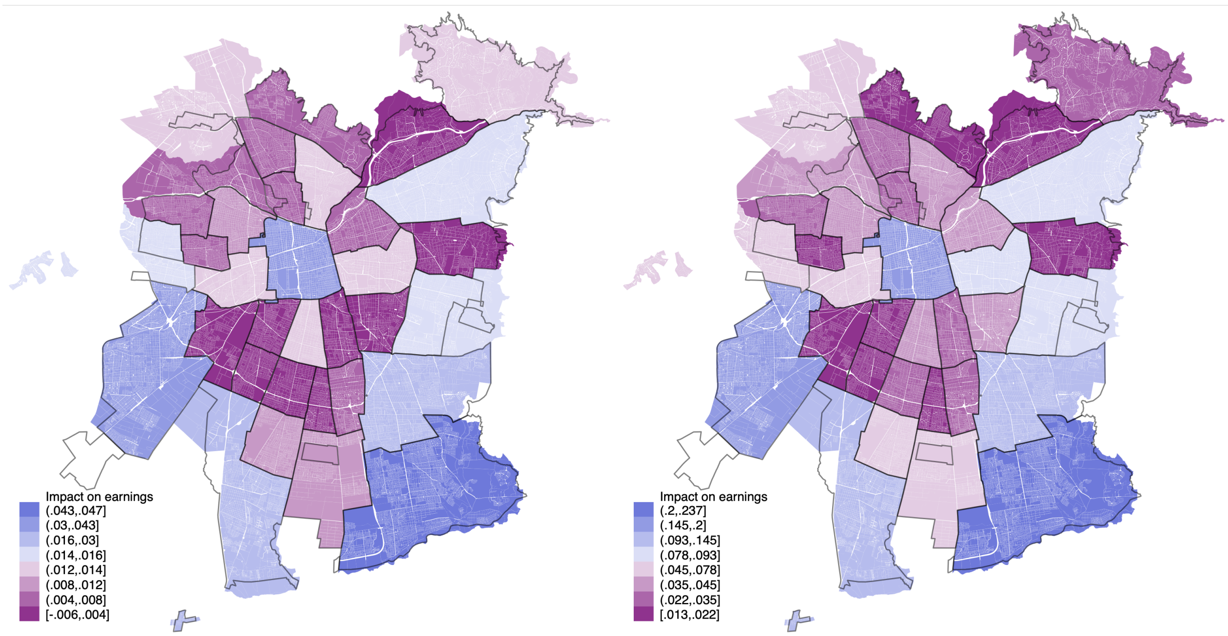
(a) Corrected income (RSH)



(b) Wages (AFC)

Notes: The figure shows exposure effects of neighborhoods, estimated in the full sample of children in the RSH and the AFC with corrected income data, whose parents are observed to have moved municipalities once between 2007 and 2023. See text for details about sample construction.

Figure E.5: Forecasts of causal effects on children's income by municipality in Greater Santiago



(a) For children with parents at p=25

(b) For children with parents at p=75

Notes: The figure shows estimates of the causal effect of moving to a municipality on children's earnings as adults for children of parents in the 25th income percentile (μ_{p25}) in panel (a), and for children of parents in the 75th income percentile (μ_{p75}) in panel (b). Details about the methodology are in Chetty and Hendren (2018b).