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SENT AWAY:
DISPLACEMENT, NEIGHBORHOODS, AND
CHILDREN'S OUTCOMES UNDER SLUM CLEARANCE POLICIES

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ABSTRACT

We examine the difference between 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 0.62 fewer years of schooling, earn 10.2% less, experience higher labor informality, and live in higher poverty areas compared to non-displaced children. Relocation to lower-opportunity areas and disruption of social networks explain the negative displacement effects.

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1 Introduction

Due to rapid urbanization and a lack of affordable housing, 25% of the world's urban population currently lives in slums (UN-Habitat, 2020). Common policy responses targeting informal settlements include on-site slum upgrading (Harari and Wong, 2025), sites and services programs (Michaels et al., 2021), urban redevelopment (Gechter and Tsivanidis, 2023), and slum relocation (Bryan et al., 2025). While the existing literature has examined the consequences of these policies for urban development, less is known about the effects of rehousing policies on individuals. Evaluating these policies poses challenges related to selection bias in treatment assignment and in the choice of relocation sites. Moreover, tracking slum residents over time to assess long-term impacts on human capital accumulation and labor market outcomes presents 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 formal 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 of rehousing policies, 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 households 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 earnings and years of schooling. Our final sample contains 33,611 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 10.2% 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 0.621 years of education and is 13.1% less likely to graduate from high school relative to a non-displaced child. Consequently, by ages 30–45, displaced children experience a 2.5-point decline in their income ranking, live in areas with a 3.4% higher poverty rate, and remain geographically close to their assigned neighborhoods.

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).

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 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.

To characterize the destination neighborhoods, we compute upward mobility measures by municipalities as in Chetty and Hendren (2018), 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 5% lower in value compared to non-displaced families, though the housing infrastructure was the same for both treatments.

To explore mechanisms, we leverage variation in the assigned neighborhoods of relocated families and examine how much of the displacement effect on earnings can be accounted for by changes in neighborhood attributes between origin and destination locations. We find that moving to a municipality with one point lower upward mobility reduces children's earnings rank in adulthood by 0.753 points. Accounting for all neighborhood changes experienced by displaced children explains between 33 and 49% of the total displacement effect on earnings. These include changes in upward mobility, access to the CBD, access to schools, the unemployment rate, and the size of the new project sites. The remaining portion can be interpreted as a disruption effect, which in our sample is largely explained by the disruption of slum-based social networks and the distance between origin and destination locations. Our results suggest that moving families together and to smaller relocation sites may decrease the negative impact of displacement.

Finally, we examine heterogeneity in the underlying mechanisms by age at intervention and find that neighborhood effects are most pronounced for children aged 0–5 at the time of the intervention. For this group, a one-point decrease in the measure of upward mobility reduces their income ranking by 1 point, compared with 0.6 points for older children. This finding is consistent with what previous work has described as an "exposure effect" of neighborhoods (Chetty et al., 2016; Chyn, 2018; Carrillo et al., 2023). In contrast, we find that disruption effects are stronger among middle-school-aged children, with the most negative impacts observed for those aged 9–14 at the time of treatment.

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 prior work has focused on studying the effects of these policies on places, estimating only indirect effects on individuals. Examples include Michaels et al. (2021) for Tanzania, Harari and Wong (2025) for Indonesia, Gechter and Tsivanidis (2023) for India, and Gertler et al. (2025) for contemporaneous Chile. Almost no research has investigated the effects of slum clearance policies on individuals' human capital, with the recent exception of Bryan et al. (2025) for Ethiopia.[1] We focus on children and find that relocation—relative to on-site redevelopment—is harmful because it disrupts social networks (Barnhardt et al., 2016) and moves families into lower-quality areas. Importantly, our estimates isolate the effect of rehousing distance by comparing outcomes across central and peripheral locations, and are thus informative about where to build new housing for low-income families.

This paper also contributes to the literature on neighborhood effects and intergenerational mobility, which finds heterogeneous results across outcomes and ages (Chetty et al., 2016; Chyn, 2018; Carrillo et al., 2023).[2] Recent studies, such as Camacho et al. (2022) for Colombia and Agness and Getahun (2024) for Ethiopia, examine the effects of housing on children. We complement this literature by focusing on slums, one of the most prevalent forms of shelter in developing countries.[3] We further contribute to understanding the mechanisms behind neighborhood effects by exploiting variation in destination locations (Damm and Dustmann, 2014) and group movements. Moreover, we construct measures of intergenerational mobility across municipalities in Chile—a measure that is difficult to estimate in low- and middle-income countries due to data limitations.

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 these countries due to rapid urbanization and the proliferation of slums. Examples include Henderson et al. (2021), who model the evolution of slums in a city, and Gonzalez-Navarro and Undurraga (2023), who study slums and 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).

This 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. Section 6 discusses the mechanisms, and Section 7 concludes.

[1] A related literature has evaluated the effects of land titling (Field, 2007) and on-site improvements without slum clearance (Galiani et al., 2017), both focusing on adults.

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

[3] An important set of papers, including Belchior et al. (2026), Franklin (2020), Kumar (2021), and Picarelli (2019), studies housing policies in developing countries; most of this literature focuses on adults and emphasizes different neighborhood characteristics when explaining mechanisms.

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.[4]

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.[5] According to Molina (1986) and Morales and Rojas (1986), by 1985, between 40,000 and 50,000 families participated in the program, accounting for 5% of Greater Santiago's population.

The program had two goals: to build public housing for low-income families where land was cheap and to provide them with housing in affordable locations. With these goals, the MINVU implemented two different types of interventions. Whenever conditions permitted, families would remain in their original location, and their slum would go through an urban renewal process to provide them with on-site housing (i.e., were not displaced). If this was not possible, they would be evicted from their original location and receive a housing unit in a different one (i.e., were displaced). All families in the same slum would receive the same treatment, and all would become 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,

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

[5] 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).

the slum's land was subdivided among residents, with each family receiving a "starting-kit unit."[6] 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.[7]

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).[8] 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.[9]

Displaced and non-displaced families received houses that were similar in quality and size (see, for example, Appendix Figures A.1 and A.2). 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

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

[7] All families would be evicted, and if they did not want the rehousing subsidy, 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 Appendix Figure A.2 for reference.

[8] 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.

[9] 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).

level.[10] 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.[11] 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) 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.

[10]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).

[11]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 at a given point in time.

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.[12]

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 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 Appendix Figure A.3 for a summary).

[12]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 from the army.

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.[13]

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 relocated 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.[14] 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 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

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

[14] 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.

urban areas (17,527/27,419), though their slums represent 42% of the slums in the program (98/231).[15] In our archival sample, 12,174 (69.5%) are displaced and 5,353 (30.5%) 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, Panel A). 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 to match our sample to birth and marriage certificates for individuals who were aged 18 and older in 2016.[16] 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.[17] We identify 15,032 families with at least one child (the rest did not have children), corresponding to a total of 46,297 children in 98 slums. Of these, 33,611 were aged 0–18 years at the time of treatment. This is our baseline sample (see Appendix Figure C.1 for a summary of the data collection).

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

[15] 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 98/231 is likely a lower bound for our matching rate.

[16] These are official certificates from Chile's Civil Registration and Identification Service.

[17] 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, 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.

identify last names that are Mapuche, the largest Indigenous group in Chile.[18] Finally, we measure parents' formal employment at the slum level between 1975 and 1980, using historical records from Chile's Superintendency of Pensions.[19]

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.[20] 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 the AFC, we can confidently say she is not employed in the formal sector, 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.

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

[19]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.

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

3.4 Municipality and neighborhood attributes

Using locations of slums and destination projects, we measure location attributes by municipality and census district from the 1982 Population Census, which contains data on education and employment status. We add historical records from the Ministry of Education on the number and location of schools. In addition, we obtain publicly available data from Greater Santiago's subway system on subway stations built in the city. We also 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.

Finally, we use the RSH data to compute municipality-level upward mobility estimates to measure neighborhood quality. 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 (2018) (see Appendix Section F for a full description of the methodology).

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 it 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 allow the relationship between the outcome and the propensity score to vary by municipality of origin. For precision, in equation (1) we add baseline controls for individual and family characteristics at the time of the intervention, X_i, that include gender, child's year-of-birth fixed effects, female head of household, married head of household, head of household's age, 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.[21] We cluster the standard errors by slum of origin.[22]

Estimating a propensity score model requires the unconfoundness assumption to hold, meaning that conditional on the propensity score, the potential outcomes are 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

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

[22] In Appendix Table A.3 we use Conley standard errors (Conley, 1999), and in all cases, clustering at the slum level yields the largest standard errors on our main outcomes.

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 discuss 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 231 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 of the methodology). We exclude from the model the price index because it could reflect expectations of future land prices due to slum clearance.

The estimates of the LASSO-logit model lead to fitted propensity score values between 0.05 and 1, and as expected, displaced slums have a higher propensity of relocation compared to non-displaced slums (see Appendix Table B.1 and Appendix Figure B.1). Importantly, there is common support between 0.1 and 0.78. 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 7 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 smaller in terms of the number of families, located closer to the CBD, and at higher 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 compared to the full sample of 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 231 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. As expected, because displaced and non-displaced slums are more similar to each other, the predicted densities are also more similar between treatments (see Appendix Figure B.2 panel (a)). In particular, they do not include slums with high probabilities of treatment above 0.78, and when we impose common support in this sample, only 3 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.[23] 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 (see Appendix Figure B.2). 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 231 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 (Appendix Figure B.2), we keep slums where $0.1 < \hat{p}(X_s) < 0.78$: from the 98 slums in our archival sample, 95 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.[24]

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

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

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

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 39% of heads of households were formally employed before treatment and 46% migrated from outside the Greater Santiago area before age 18. Finally, 2% had a mother who lost a child under five before treatment.

Among the children in our sample, 91.2% are found in the RSH. Displacement and gender predict the probability of finding a child in the administrative data (see Appendix Table A.1). 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.

Importantly, the children in our baseline sample are also representative of those living in slums in 1982 (see Appendix Table A.2). Among children aged 0–18 in the 1982 Population Census who lived in Greater Santiago, 19% lived in a slum, 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 came from households that were more vulnerable compared to the rest of the population in Santiago, as they had lower educational attainment, 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.

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 few statistical differences between the two groups. Displaced children are 3.5 percentage points less likely to have married or cohabiting parents at baseline and 1.5 percentage points 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.[25] We also include the p-value of a joint test of treatment significance for the 18 variables in Panel A, and find no statistical evidence that demographics jointly determine treatment.

The results are very similar and even more balanced for children matched to the RSH (columns (5)–(7)). The difference in marital status and mother's schooling become smaller in absolute value. 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. Overall, children in the RSH do not appear systematically different from those in the full sample. Importantly, all 95 slums are retained in our matched sample.

Note that when estimating the propensity score, we targeted balance in slum character-

[25] 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.

istics 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 children. Figure 2 illustrates these densities, with panel (a) reporting estimates of upward mobility by municipality of destination. The analysis shows that displaced children were more likely to be relocated to areas where upward mobility is 0.54 points lower, or 1.2% less, compared to those of non-displaced children.

We observe even larger differences in other neighborhood attributes. Panel (b) plots densities for distance to the CBD, showing that displaced children are relocated 2.62 km farther away from the CBD, from a baseline of 11.36 km. Panel (c) shows that they also experience longer commuting times. These patterns consistently align with the fact that, compared to non-displaced children, displaced children were relocated to lower-opportunity areas: they ended up in locations with 21% higher unemployment rates (panel (d)), 9 km farther from their locations of origin (panel (e)), and in larger public housing projects (panel (f)).

5.2 Displacement effect on children's adult outcomes

5.2.1 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 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.[26] Labor earnings are measured in 1,000 Chilean pesos per month (CLP$1,000/month), equivalent to approximately US$1 per month in 2024. We compute one observation per individual by collapsing each outcome after controlling for age and semester-year dummies.

Table 3 shows estimates of the displacement effect on labor market outcomes. Column (1) reports the estimated β from equation (1), and column (2) the mean of the outcome for non-displaced children.[27] The results in Panel A show that displacement reduces children's earnings in CLP$25,418 per month, which corresponds to 10.2% lower earnings relative to non-displaced children in their adulthood. The negative displacement effect on earnings is not because displaced children are less likely to be employed, but because they are more likely to work informally (without a contract) by 3.6 percentage points, or 9.1% relative to non-displaced children. The worse labor market outcomes of displaced children reflect that their earnings ranking (income percentile) in the national income distribution by the age of 30-45 is lower by 2.473 points.[28]

Panel B in Table 3 shows estimates of the displacement effect on outcomes in the AFC, where we observe taxable wages, which are observed through social security contributions in the private sector between 2007 and 2023. These contributions, by definition, measure formal earnings.[29] Consistent with the negative effect of displacement on self-reported earnings in Panel A, we find that displaced children are 1.9 percentage points less likely to work in the formal sector, and experience a more negative displacement effect on taxable wages equal to –CLP$43,969 per month, which corresponds to 11.5% lower formal earnings compared with non-displaced children. This result indicates that displaced children are not more likely than non-displaced children to under-report their earnings in the RSH.[30]

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

[27]In Appendix Table A.3, we show how the propensity score accounts for selection on observables for the outcomes of income and employment. While the propensity score does reduce selection on observables in about 25%, most selection is explained by the year of treatment in our sample.

[28]We lose some observations for this variable because we are less likely to find children of older cohorts before the age of 45 in the administrative data between 2007 and 2023. We choose the 30-45 interval because it maximizes the number of children with non-missing income at younger ages, but our results are robust to estimating income rankings using wider age brackets.

[29]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.

[30]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

5.2.2 *Schooling outcomes*

Next, we study the displacement effect on schooling outcomes. The results, shown in Panel C of Table 3, indicate that displaced children obtain 0.621 fewer years of schooling than non-displaced children. The negative percent effect increases with higher levels of education: displaced children are 13.1% less likely to graduate from high school, 28.5% less likely to attend a two-year college (for technical degrees such as mechanics and electrical technology), and 35.7% 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.

5.2.3 *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 shows the results on earnings and taxable wages. 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. We also find a similar age trajectory and displacement effect when looking separately at formal and informal earnings in the RSH (see Appendix Figure A.5).

We can 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).[31] This is practically the same as the cost of the housing unit received by a family through the program in our sample (equivalent to US$10,500). By the age of 60, the total loss is almost twice as large and equal to US$18,965.

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.[32] Note that while

RSH is biannual.

[31] 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.

[32] We compute the total loss as the individual loss multiplied by the number of children in our sample.

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.[33]

5.2.4 Children's long-run locations

Panel D of Table 3 shows that after treatment in 1985, displaced children were relocated to areas of lower quality in terms of upward mobility and the population's schooling. These are the regression results equivalent to Figure 2 after controlling for covariates. Additionally, panel D shows that displaced households received homes that were cheaper compared to non-displaced households. These results, and those in our previous analysis, suggest that children's future labor earnings and schooling are affected by displacement, likely because relocation changes children's environments. Hence, 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 proxy poverty at the neighborhood level as the average RSH index for the proportion of individuals per neighborhood who qualify for social assistance.[34]

Panel E of Table 3 shows that displaced children are 22.5% more likely than non-displaced children to live in their municipalities of assignment, and do not return to their municipalities of origin. Additionally, their current neighborhoods of residence are 3.4% poorer than those of non-displaced children. Overall, these estimates show that displaced children have lower spatial mobility and live in poorer areas in their adulthood. This result is reinforced by the fact that only 5% of families sold their house by 2019, and displaced households are not more likely than non-displaced households to sell their homes in the long run, suggesting families remain in the same areas they were relocated to (see Appendix Table A.8).[35]

The cost of building subway stations is available from Metro de Santiago, and the cost of maintaining schools can be found here.

[33] 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.

[34] The RSH reports location data at the neighborhood level for a random sample of individuals, with about 40% of the observations including a current location at this granularity. However, the data are only considered reliable after 2015.

[35] The fact that families are unlikely to sell their houses is probably due to the constraints imposed by the program, as households could not sell until they have paid for the full amount, which, according to our data, was on average 27 years after treatment.

5.2.5 *Displacement effect by demographic groups*

While displacement effects may vary across demographic groups, we find no systematic or large differences across most characteristics, with the exception of gender and age (see Appendix Figure A.4). We find more negative effects on labor earnings and income rank for boys than for girls, but not on taxable wages, suggesting that displaced boys earn less than non-displaced boys in adulthood primarily through employment in the informal sector.

We also find stronger negative effects for children who were younger at baseline (see Appendix Figure A.6). In the following sections, we examine age heterogeneity in greater detail to better disentangle the mechanisms underlying the displacement effect.

5.3 *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.

5.3.1 *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 (see Appendix Figure B.2, panel (b)).

We apply these sampling weights by slum to our baseline sample of children, and estimate the displacement effect on children's earnings as adults in the re-weighted sample (Appendix Table C.1). The results show the displacement effect in the weighted sample is more negative and very similar to our baseline estimates in Table 3, suggesting that attrition from the archives is not inducing a selection of individuals such that it would explain the observed negative displacement effect.

5.3.2 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 matched observations among displaced children ((60–55)/60). We estimate Lee bounds on total labor earnings, taxable wages, and schooling (see Appendix Table C.2).[36] 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 (see discussion in Appendix C).

5.4 Robustness checks

5.4.1 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 the common support (Appendix Table A.5). We find that our results for earnings and schooling are very similar when we estimate the displacement effect using inverse propensity score re-weighting, or when we trim the common support of the propensity score.

We also estimate the displacement effect on children's outcomes in a sample that excludes three municipalities with low overlap of the propensity score between treatments, or when we exclude from the main 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 Appendix Figure B.3). Our results for the displacement effect are very similar to those of Table 3.

Finally, we examine whether the displacement effect is robust to changing which municipalities are included in the sample (see Appendix Figure D.1). When we drop municipalities

[36] 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.

one by one, we find that our results are not driven by any particular municipality of origin.

5.4.2 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 upgraded on-site were ultimately relocated. Thus, we use this assignment as an instrument for displacement in the sample of slums cleared after 1980. We find that the instrumental variable coefficient is very similar to our propensity score estimate on earnings and schooling (Appendix Table D.1).

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 (Appendix Figure D.2).

6 Mechanisms

Due to lower-quality attributes of destination neighborhoods, in this section, we study whether changes in neighborhood attributes explain the average displacement effect on children's future earnings.[37]

[37] 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 (see, for example, Appendix Table A.6 and Appendix Figure A.9). We do not rule out this possibility; however, in this section, we provide evidence that the displacement effect on earnings may instead be due to changes in the environments of displaced children.

6.1 Disruption and neighborhood effects

The displacement effect can be separated into a disruption effect and a neighborhood effect. The disruption effect is defined as the impact of moving due to changes in environments and the loss of social networks. Moving may impact children because adapting to new environments is costly due to changes in schools and the disruption of their social environments. A place or neighborhood effect is associated with the attributes of the assigned location, which can include access to and quality of schools, access to labor markets, and peer effects.

We provide graphical evidence for the role of place effects in explaining the variation in the outcomes of displaced children. To do this, we first measure neighborhood quality at the municipality level. We follow the methodology of Chetty and Hendren (2018), 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 F.3).

Next, we study whether there is a relationship between the adult labor market outcomes of displaced children and the change in neighborhood quality between destination and origin. The validity of this exercise relies on the idea that displaced families were forced to move to a particular location.[38] If a non-zero correlation is found, it indicates that the displacement effect is likely a function of neighborhood change.

Figure 4 panel (a) presents the results of correlating the adult earnings of displaced children with the change in levels of upward mobility after controlling for municipality of origin fixed effects, the propensity score, and baseline demographics. The result is a positive relationship (i.e., displaced children relocated to areas with higher upward mobility have higher monthly 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.

[38] 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 (Appendix Table A.7) from our sample of families who moved, finding no systematic evidence that family characteristics predict their final destinations.

The next panels of Figure 4 repeat the previous exercise using as children's outcomes their earnings ranking at age 30-45 (panel (b)), and their neighborhood poverty rate in adulthood (panel (c)). The results show evidence of a strong and statistically significant relationship between displaced children's outcomes and the change in neighborhood quality as a consequence of relocation. These results suggest that the average displaced child who experiences a decrease in neighborhood quality of 1 point suffers a reduction of CLP$3,035 on her monthly earnings, her earnings ranking is reduced by 0.665 points, and she experiences a higher poverty rate index in adulthood of 0.741 points. These results suggest that the variation explaining the displacement effect may be due to relocation.

The previous results are useful in providing evidence of a relationship between displaced children's outcomes and the change in neighborhood quality experienced by relocated children. However, we would like to separate the effect of the neighborhood from the disruption. To do this, we augment equation (1) and estimate the following model:

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

where Y_i is either labor earnings or percentile rank for child i when she is an adult. ΔUpward Mobility$_{do'}$ is the difference between upward mobility between municipality of destination d and municipality of origin o'. This is our summary measure of neighborhood quality change. 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.[39] All other variables are measured as in equation (1). The parameters of interest are β' and δ. β' measures the displacement effect net of neighborhood change, and δ measures the effect of increases in neighborhood quality from changes in upward mobility 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 4 presents the results of estimating equation (2) on children's earnings and income percentile. By presenting the effects on income percentile we can interpret the coefficient δ similarly to an elasticity with respect to upward mobility. For comparison, column (1)

[39] 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.

presents the displacement effect β from Panel A in Table 3. Column (2) shows that children who experience an increase in neighborhood quality of 1 point, increase their earnings in CLP\$3,279 per month. This result is statistically different from zero and similar to the coefficient in panel (a) of Figure 4. Column (6) presents the corresponding estimate on the earnings percentile. This shows that children who experience an increase of one point in neighborhood quality, have a higher income ranking by 0.753 points. This result is statistically different from zero at 1%, and also very similar in magnitude to the correlation of panel (b) in Figure 4.

Notice that in both columns (2) and (6), the inclusion of ΔUpward mobility does not change the remaining displacement effect β' much, compared to our baseline results in columns (1) and (5). While these results show a role for upward mobility in explaining the displacement effect, especially on income ranking (column (5)), the variation may not be ideal because municipalities might be too large a representation of a neighborhood, or other, more local changes may be able to explain the variation in our sample. Because of this, in columns (3) and (7) we add as controls other changes in neighborhood quality experienced by children in the program. In particular, we add the change between destination and origin for the following attributes: commuting time, distance to the CBD, population's schooling, unemployment rate, number of schools in a municipality, and neighborhood size per 100 units.

The results in columns (3) and (7) show estimates that have the expected signs. For example, a longer distance to the CBD, more unemployment, and larger projects correlate negatively with children's earnings.[40] These results are consistent with previous literature (Aldunate et al., 1987; Barnhardt et al., 2016; Picarelli, 2019; Newman, 1973; Kain, 1968), and interestingly, they suggest that moving families to worse areas and to larger sites (projects) has negative impacts on children. But more importantly, by including additional neighborhood changes, the effect of neighborhood quality, δ, is stronger on both earnings and income percentile, similar to the results from Figure 4, suggesting that our summary measure of change in neighborhood quality (upward mobility) does a good job at predicting children's long-run outcomes conditional on other neighborhood changes. Additionally, the remaining displacement effect (β') is reduced substantially in absolute value and is no

[40]Many of the variables included in these regressions are highly correlated; as a result, some coefficients, such as those on commuting time and average schooling, have unexpected signs.

longer statistically significant for the outcome of income percentile (column (7)). These suggest that accounting for various changes in neighborhood quality explains between 33% (1-16.936/25.418) and 49% (1-1.265/2.473) of the total displacement effect on children's adult earnings in our sample.

The remaining percent could be attributed to a disruption effect. To understand whether disruption plays a role, we control for two additional changes experienced by displaced households that can be associated more with a disruption effect rather than a neighborhood effect. In particular, we include the change in networks as the difference between 1 and the fraction of slum-dweller families from the original slum relocated together in a new project site, and the distance between origin and destination.[41] The results are in columns (4) and (8) of Table 4.

Children who are relocated with a fraction of their network and who are housed farther away from their original locations experience a negative effect on their adult labor earnings, relative to those children housed in situ. Importantly, by controlling for the change in networks and distance, the coefficient on the change in upward mobility changes very little, suggesting that the effect of these variables does not operate through a neighborhood channel, but instead they reduce the remaining displacement effect to almost zero, especially for the outcome of income percentile (column (8)). These results are revealing and show that displacement in our sample is both a function of neighborhood change and a disruption effect.

Finally, we find suggestive evidence that the long-run effects of displacement can be partially mitigated by improvements in neighborhood quality at the assigned location. For instance, the construction of nearby metro stations in Santiago increases displaced children's adult earnings relative to non-displaced children, consistent with transportation access offsetting some of the costs of relocation to peripheral areas (see Appendix E).

6.2 Changes in children's environments 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).

[41] The network disruption can be observed because some slums were divided into groups, and families were relocated into different neighborhoods of destinations. Notice that non-displaced families did not experience a network disruption, and almost no change in distance between destination and origin projects (see Figure 2 for reference).

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 neighborhood effect for young children in our setting.

We further explore whether an age gradient exists in the neighborhood and disruption effects. To do so, we expand regression (2) by interacting the neighborhood and disruption effects with age-group categories at baseline (year of treatment), and we estimate the following equation:

$$\begin{aligned} Y_i = \alpha + \sum_{g=1}^{6} \beta'_g Displaced_{s\{i\}} \cdot 1\{Age = g\} + \sum_{g=1}^{6} \delta_g \Delta \text{Upward Mobility}_{do'} \cdot 1\{Age = g\} \\ + \gamma \text{Upward Mobility}_{o'} + \psi_o + p(X_s) + \psi_o \times p(X_s) + X'_i \theta + \varepsilon_i, \end{aligned} \tag{3}$$

where all variables are measured as in equation (2), and $g \in [1, 6]$ stands for one of six age groups at baseline (0-2, 3-5, 6-8, 9-11, 12-14, and 15-18).[42] The coefficients of interest are β'_g and δ_g, which measure differential effects of displacement and neighborhood change by age at the time of treatment. We hypothesize that the absolute value of δ_g decreases in g.

The results from estimating equation (3) are presented in Figure 5. The upper panels report results for labor earnings, while the lower panels report results for income percentile. The coefficients δ_g are shown as blue circles in panels (a) and (c), and the coefficients β'_g are shown as light green circles in panels (b) and (d). The results provide evidence of a neighborhood exposure effect. For children of age 5 or younger at the time of treatment, relocating to a municipality with higher upward mobility increases monthly earnings by CLP\$7,000 (panel (a)) and raises their income ranking at ages 30–45 by 1 percentile, relative to non-displaced children of the same age at the year of treatment. These effects are statistically significant at the 5% level. For children older than 5, the neighborhood effect decreases by age: close to zero for earnings and around 0.6 for income ranking. Additionally, the p-value for the difference between the effects for children below and above age 5 is 0.036 for labor earnings, suggesting statistically different effects by age. Finally, the age pattern of the disruption effect is not monotonic. It shows a more negative impact of displacement (net of neighborhood change) for middle-school-aged children, particularly those between

[42]We selected these groups to ensure sufficient observations in each category and to reflect typical schooling age groups.

ages 9 and 14, for both earnings and income ranking.

We investigate whether our results are robust to controlling for other neighborhood changes, as in columns (4) and (8) of Table 4, and plot the corresponding age coefficients in purple in panels (a) and (c) for δ_g, and in green in panels (b) and (d) for β'_g. We find that controlling for additional neighborhood changes does not alter the age pattern of neighborhood effects: younger children are the most negatively affected when relocating to lower-quality neighborhoods. And the disruption effects are reduced in magnitude once we control for network change and distance from origin, but the age pattern remains, particularly for labor earnings. Panel (b) shows that the children most affected by the disruption induced by relocation are those between the ages of 9 and 14. These children are likely to be in middle school at the time of displacement. This disruption effect has also been documented in other settings, such as the Moving to Opportunity experiment in the United States, where older children have been found to be negatively affected, even when moving to better neighborhoods (Chetty et al., 2016).

The results presented in this section reinforce the idea that displacement affects children through an exposure effect of neighborhoods, as children more exposed to worse environments, measured as average upward mobility, are the most affected in the longer run. However, older children are also affected by displacement due to a disruption effect that likely affected their social environments after relocation, with negative consequences on their long-term outcomes.

7 Conclusion

This paper presents new evidence on the long-term impacts of relocating to isolated neighborhoods, relative to remaining in central areas. The novelty of our study lies in the construction of a dataset that tracks slum-dwelling families and their children, allowing us to estimate the long-term effects of relocation versus on-site upgrading.

Our results show that, relative to non-displaced children, displaced children complete 0.621 fewer years of education, earn 10.2% less income, and are 5.1% 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 through two channels: exposure to lower-quality neighborhoods and the disruption of existing social networks. The negative neighborhood effects are larger for younger children, while disruption effects are more pronounced among middle-school-aged children.

Despite their high costs, international organizations such as United Nations generally advocate for on-site housing improvements over relocation (UN-Habitat, 2020). Our findings support this view, suggesting that relocation to lower-quality areas farther from the city center may have adverse consequences. Nevertheless, more empirical evidence is needed to compare alternative policy approaches, including compensation schemes (Lall et al., 2006) and expanded access to public services. Finally, given the scale of these programs, future research could examine their general equilibrium effects on surrounding communities and cities.

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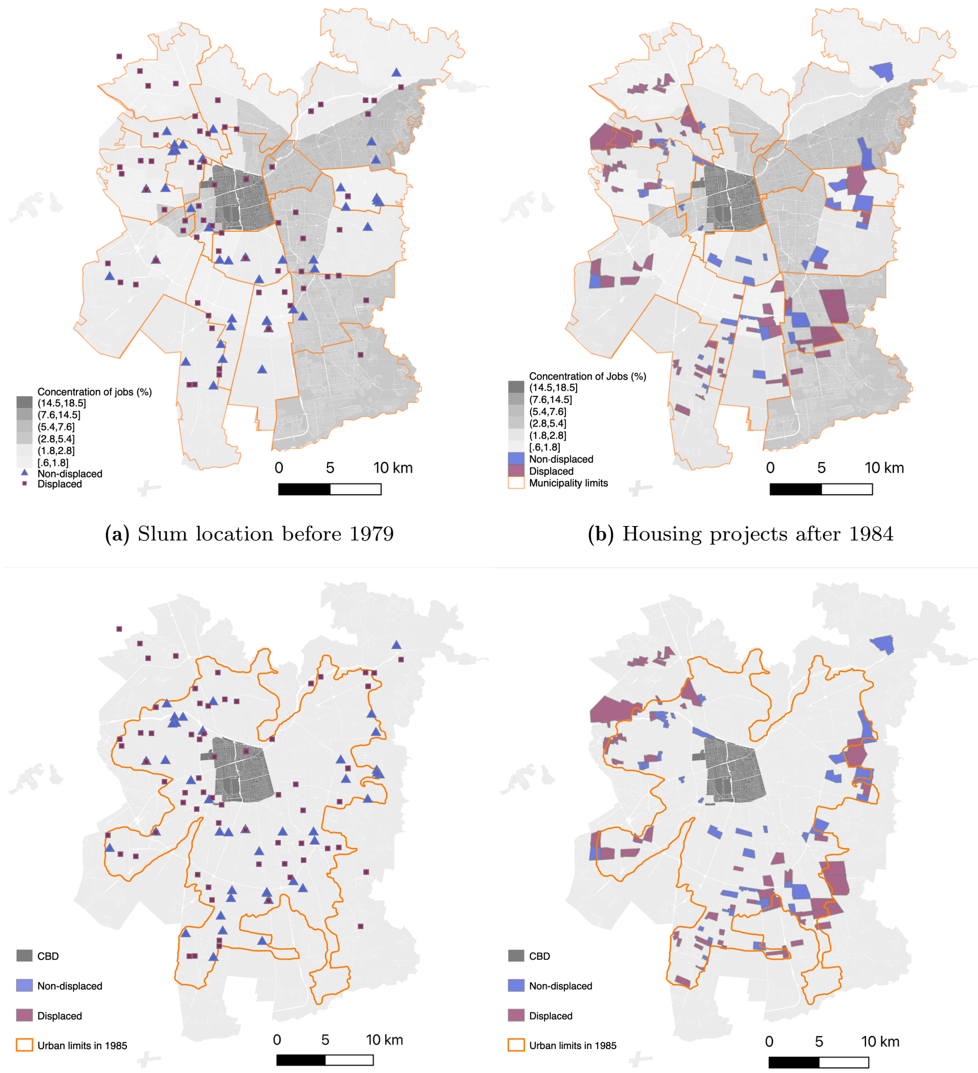
Figures and Tables

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



(a) Slum location before 1979

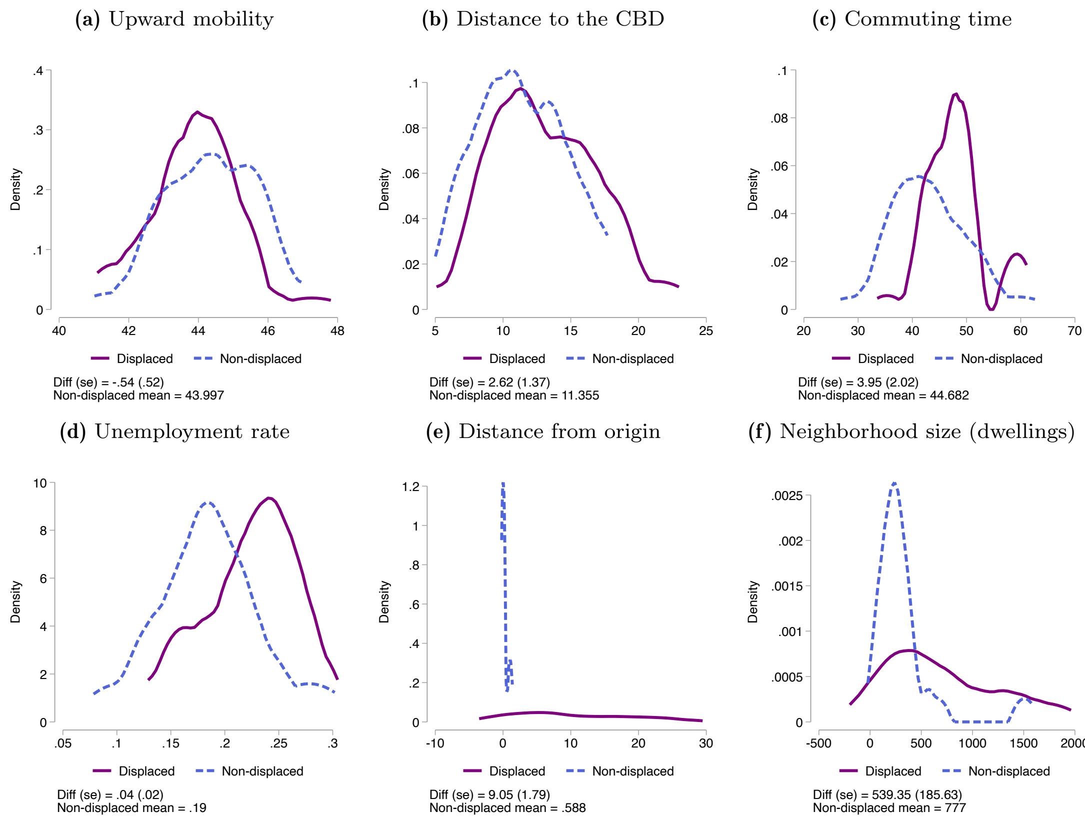
(b) Housing projects after 1984

(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.

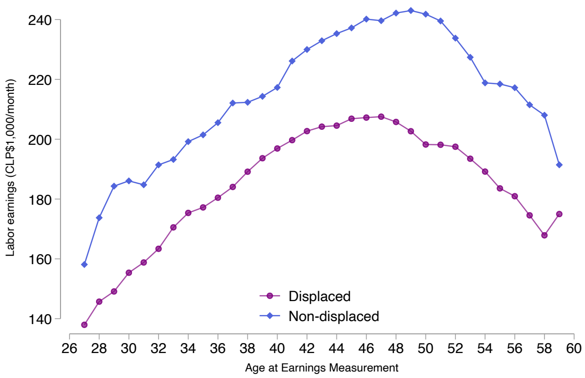
Figure 2: Density of neighborhood attributes after relocation



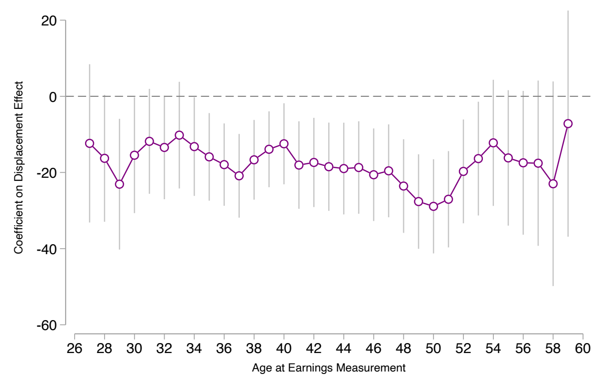
Notes: The figure shows densities by treatment for the average neighborhood attributes of each pair of slum of origin and destination project in the archival sample (N = 112 unique slum-project pairs). Each panel shows the average difference in treatments labeled as "Diff," and each footnote indicates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

Figure 3: Displacement effects on labor market outcomes by age at earnings measurement for children aged 0-18 at baseline

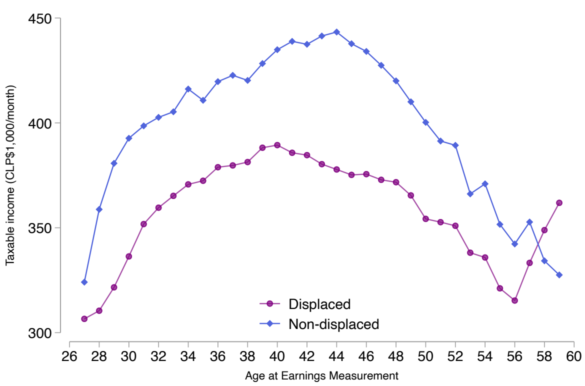
(a) Labor income trajectories (CLP$1,000/month)



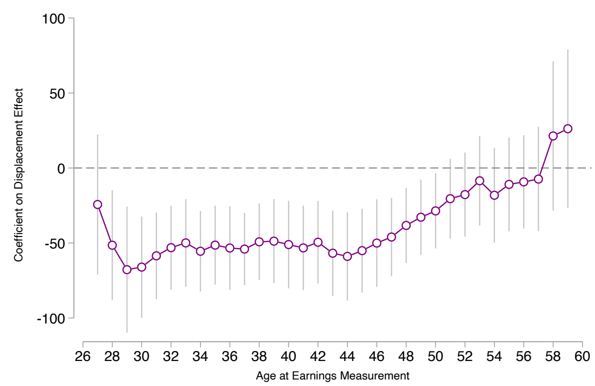
(b) Displacement effect on labor income



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



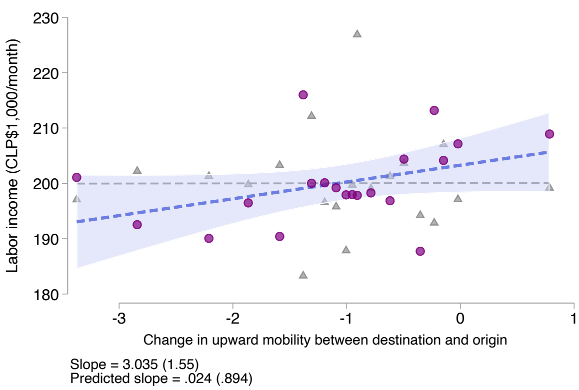
(d) Displacement effect on taxable wage



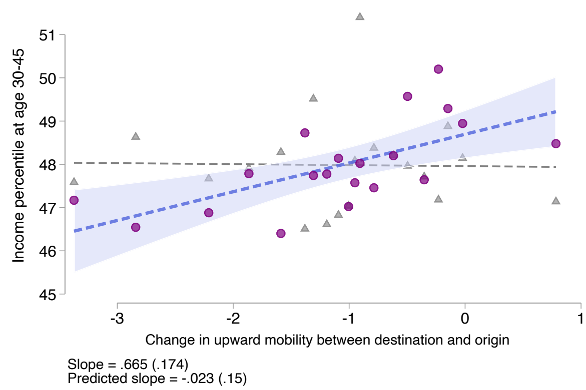
Notes: The figure shows regressions for children aged 0-18 at baseline who are matched to the RSH and AFC data. Panels (a) and (c) plot the predicted trajectories for displaced and non-displaced children between ages 25 and 60 from the regression $y_{it} = \sum_{\tau=25}^{60} \beta_\tau Displaced * 1[Age = \tau] + \sum_{\tau=25}^{60} \delta_\tau 1[Age] + \psi_o + \hat{p}(X_s) + \hat{p}(X_s) \times \psi_o + X_{it}'\gamma + u_{it}$. 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 Appendix Figure A.5. 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: Relationship between displaced children's outcomes and change in location attributes

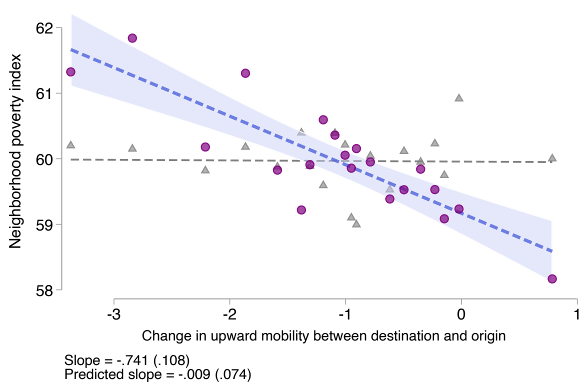
(a) Labor earnings



(b) Income percentile



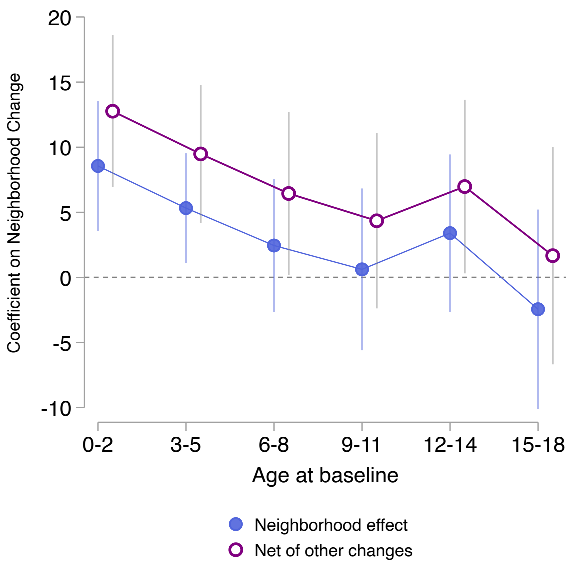
(c) Neighborhood poverty index



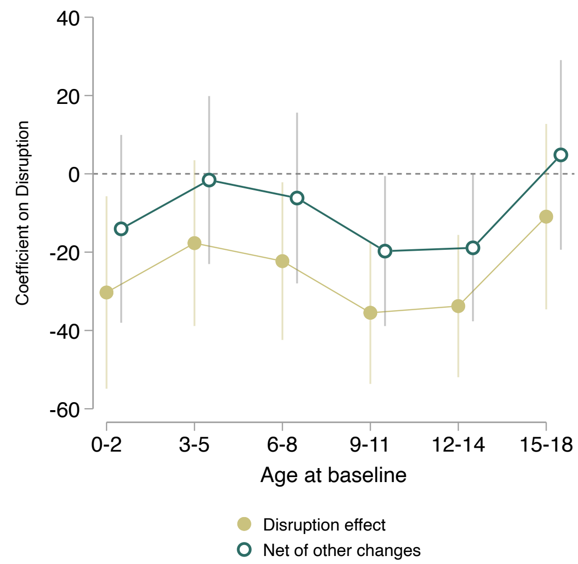
Notes: These figures plot displaced children's outcomes in adulthood against the change in upward mobility between destination and origin municipalities in purple, divided into 20 centiles. In each panel, both the outcome and change in upward mobility are residualized by the municipality of origin fixed effects, the predicted propensity score, and children's demographics at baseline, with the sample means added for better interpretability. "Slope" stands for 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" stands for the slope of the gray fitted line. Blue areas are the 95% confidence intervals of the corresponding blue fitted lines.

Figure 5: Effect of change in neighborhood quality on earnings by age at intervention

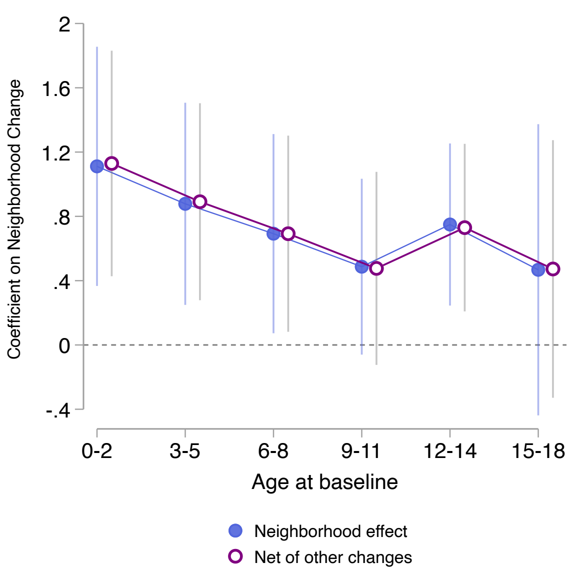
(a) Neighborhood effect on earnings



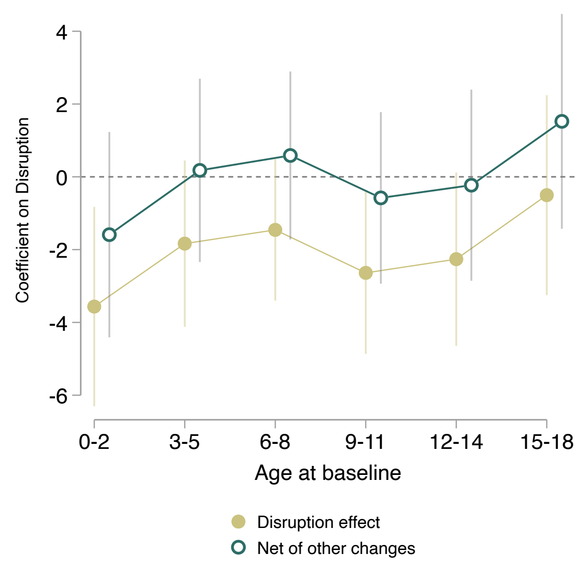
(b) Disruption effect on earnings



(c) Neighborhood effect on income rank at age 30-45



(d) Disruption effect on income rank at age 30-45



Notes: These figures report age estimates from equation (3) on labor earnings in the upper panels and on income ranking at age 30-45 in the bottom panels. Panels (a) and (c) plot age coefficients, δ_g, in blue circles, and panels (b) and (d) plot age coefficients, β'_g, in yellow circles. Purple circles and green circles correspond to estimates of δ_g and β'_g after controlling for other neighborhood and disruption changes as in columns (4) and (8) of Table 4. Similar figures for the outcome of poverty index can be found in Appendix Figure A.8.

Table 1: Slum characteristics before the intervention

	Panel A. Full sample of slums				*Panel B. 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	231.584	228.579	3.005	15.354	298.487	322.522	-24.034	-7.928
			(36.710)	(42.026)			(72.631)	(75.605)
Families/hectare	71.906	60.923	10.983	1.224	63.160	66.844	-3.685	-6.566
			(7.756)	(6.844)			(10.711)	(9.920)
Military name	0.141	0.189	-0.048	-0.035	0.193	0.220	-0.027	-0.017
			(0.049)	(0.049)			(0.084)	(0.085)
Elevation (mas)	572.667	585.652	-12.985	-4.117	569.684	583.220	-13.535	-15.790
			(11.448)	(10.994)			(16.565)	(15.417)
Slope (degrees)	2.819	2.656	0.163	-0.013	2.772	2.610	0.161	0.095
			(0.232)	(0.233)			(0.304)	(0.312)
Close to a river/canal (<100m)	0.051	0.030	0.020	-0.001	0.035	0.024	0.011	-0.005
			(0.027)	(0.022)			(0.035)	(0.032)
Flooding risk	0.061	0.008	0.053**	0.008	0.035	0.000	0.035	0.000
			(0.025)	(0.013)			(0.025)	(0.000)
Distance to CBD	9.459	10.288	-0.829	-0.041	9.147	9.934	-0.787	-0.549
			(0.548)	(0.546)			(0.741)	(0.717)
Panel B. Attributes of the census district where a slum is located								
Population educational attainment	7.826	7.161	0.665***	-0.109	7.807	7.488	0.320	-0.205
			(0.250)	(0.160)			(0.379)	(0.253)
Unemployment rate	0.191	0.200	-0.009	0.011*	0.194	0.185	0.010	0.022**
			(0.008)	(0.006)			(0.012)	(0.009)
Number of schools	4.086	4.273	-0.187	-0.297	3.886	3.610	0.277	0.235
			(0.425)	(0.450)			(0.586)	(0.606)
Log property prices	14.800	14.738	0.062	0.003	14.822	14.773	0.049	-0.003
			(0.044)	(0.044)			(0.073)	(0.068)
Number of slums	99	132	231	224	57	41	98	95
Number of municipalities	15	15	15	15	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: Summary statistics and balancing tests for children aged 0-18 at baseline

	Full sample	Children in common support			Children matched to the RSH		
	of children in archives	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	0.504	0.002	0.517	0.518	-0.004
	[0.500]			(0.006)			(0.006)
Age	8.124	8.261	8.119	0.260	8.319	8.090	0.183
	[4.854]			(0.221)			(0.219)
Firstborn	0.366	0.367	0.364	-0.012	0.355	0.360	-0.006
	[0.482]			(0.010)			(0.010)
No. children	3.840	3.748	3.874	0.107*	3.808	3.890	0.065
	[1.795]			(0.064)			(0.067)
Oldest sibling	11.524	11.562	11.563	.426	11.697	11.568	0.326
	[5.798]			(0.291)			(0.299)
Youngest sibling	5.093	5.264	5.054	0.086	5.276	5.038	0.060
	[4.197]			(0.185)			(0.184)
HH age	34.789	35.291	34.607	-0.148	35.361	34.610	-0.179
	[7.125]			(0.363)			(0.371)
Mother age	33.067	33.529	32.906	-0.183	33.603	32.891	-0.236
	[6.952]			(0.335)			(0.340)
Father age	35.336	35.703	35.249	0.037	35.757	35.235	-0.007
	[7.487]			(0.357)			(0.368)
Female HH	0.329	0.309	0.341	-0.010	0.307	0.337	-0.010
	[0.470]			(0.030)			(0.031)
Married or cohabit HH	0.878	0.915	0.864	-0.035***	0.915	0.870	-0.029***
	[0.328]			(0.011)			(0.011)
Widowed HH	0.011	0.008	0.013	0.001	0.008	0.012	0.001
	[0.105]			(0.002)			(0.002)
Mapuche HH	0.057	0.050	0.062	0.015***	0.051	0.063	0.014**
	[0.232]			(0.006)			(0.006)
HH formal employment[a]	0.388	0.413	0.375	-0.008	0.412	0.374	-0.008
	[0.077]			(0.019)			(0.019)
HH born outside Santiago	0.461	0.460	0.457	0.010	0.458	0.458	0.011
	[0.498]			(0.022)			(0.022)
Mother's schooling[b]	5.975	6.262	5.858	-0.343**	6.117	5.801	-0.267*
	[3.446]			(0.153)			(0.150)
Child mortality last 5 years[c]							
below age 1	0.018	0.024	0.016	-0.004	0.024	0.016	-0.005
	[0.139]			(0.004)			(0.004)
below age 5	0.023	0.029	0.021	-0.002	0.029	0.021	-0.004
	[0.158]			(0.005)			(0.005)
P-value for joint test of treatment significance[d]				0.263			0.499
B. Matching rates							
In RSH	0.911	0.886	0.921	0.034***	1	1	0
	[0.284]			(0.005)			(0)
In AFC	0.750	0.722	0.762	0.035***	0.814	0.827	0.008
	[0.433]			(0.009)			(0.008)
Children	33,611	10,291	21,744	32,035	9,120	20,035	29,155
Families	13,723	4,197	8,826	13,023	3,945	8,566	12,511
Slums	98	41	54	95	41	54	95
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 three 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. (d) P-value of the joint hypothesis of no significance of displacement effect for all the variables in panel A. Test performed using the Westfall-Young randomization procedure (Young, 2020). 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 children's adult outcomes

	Displacement effect (1)	Mean non-displaced (2)	Percent effect (%) (3)	P-value/ Sharp p-value (4)	Observations (5)
Panel A. Labor market outcomes in the RSH					
Labor earnings	-25.418***	250.503	-10.2	0.000; 0.001	29,155
	(6.109)				
Employed = 1	0.001	0.627	0.0	0.903; 0.230	29,155
	(0.007)				
Contract = 1	-0.036***	0.396	-9.1	0.000; 0.001	29,155
	(0.010)				
Percentile rank at age 30-45	-2.473***	55.620	-4.5	0.000; 0.002	25,747
	(0.709)				
Panel B. Formal labor market outcomes in the AFC					
Formal employment	-0.019***	0.374	-5.1	0.006; 0.005	29,155
	(0.007)				
Formal wages	-43.969***	385.392	-11.5	0.000; 0.001	29,155
	(11.740)				
Panel C. Education outcomes					
Years of schooling	-0.621***	11.740	-5.3	0.000; 0.001	29,155
	(0.117)				
HS graduate	-0.092***	0.705	-13.1	0.000; 0.001	29,155
	(0.017)				
2-year college	-0.039***	0.137	-28.5	0.000; 0.001	29,155
	(0.008)				
5-year college	-0.020***	0.056	-35.7	0.000; 0.031	29,155
	(0.005)				
Panel D. Neighborhood characteristics after treatment in 1985					
Upward mobility	-0.418	44.382	-1.0	0.126; 0.056	29,155
	(0.271)				
Average schooling	-0.919***	6.874	-13.4	0.009; 0.007	29,155
	(0.345)				
Home value (UF)	-15.974	316.912	-5.1	0.157; 0.064	29,155
	(11.219)				
Panel E. Neighborhood characteristics in adulthood (2016-2023)					
Lives in munic. of assignment	0.121**	0.533	22.5	0.043; 0.021	26,938
	(0.059)				
Lives in munic. of origin	-0.180***	0.612	-29.5	0.000; 0.001	26,938
	(0.047)				
Poverty rate index (0–100)	1.965***	57.115	3.4	0.004; 0.004	26,938
	(0.677)				

Notes: The table shows estimates for the displacement effect using equation (1) in column (1), and the mean for non-displaced children conditional on $\hat{p}(X_s)$ in column (2), in the sample of children aged 0-18 at baseline who are matched to the RSH data. All regressions control for baseline demographics, including: female dummy, 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. 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 4: Displacement effect and change in location attributes on children's outcomes

	Labor earnings				Income rank at age 30-45			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Displaced (β')	-25.418***	-25.095***	-16.936**	-9.391	-2.473***	-2.072***	-1.265	-0.074
	(6.109)	(6.791)	(6.849)	(7.302)	(0.709)	(0.746)	(0.839)	(0.893)
Δ upward mobility$_{do'}(\delta)$		3.279**	7.152***	7.044***		0.753***	0.915***	0.758***
		(1.598)	(2.334)	(2.209)		(0.218)	(0.222)	(0.202)
Δ commuting time			1.873***	1.637**			0.068	0.039
			(0.653)	(0.646)			(0.067)	(0.067)
Δ distance to CBD			-1.789*	-1.240			-0.022	0.101
			(0.912)	(0.982)			(0.093)	(0.087)
Δ HH schooling			-3.618	-2.950			0.287	0.411
			(2.794)	(2.489)			(0.350)	(0.326)
Δ HH unemployment			-1.055**	-1.083**			-0.014	0.014
			(0.495)	(0.505)			(0.053)	(0.052)
Δ# schools			-0.031	-0.043			-0.009	0.004
			(0.172)	(0.165)			(0.019)	(0.019)
Δ neighborhood size			-1.057***	-1.373***			-0.117**	-0.149***
			(0.364)	(0.399)			(0.048)	(0.050)
(1 - network slum)				-24.287**				-2.804***
				(10.597)				(0.987)
Distance from origin				-0.340				-0.143***
				(0.454)				(0.051)
Upward mobility$_{o'}$		9.629**	9.412***	9.412***		0.801**	0.834**	0.719**
		(4.280)	(3.490)	(3.061)		(0.398)	(0.382)	(0.352)
Adjusted R^2	0.087	0.087	0.087	0.087	0.141	0.142	0.142	0.142
Non-displaced mean	250.503	250.503	250.503	250.503	55.620	55.620	55.620	55.620
Observations	29,155	29,155	29,155	29,155	25,747	25,747	25,747	25,747

Notes: This table reports estimates for the displacement and neighborhood effects from equations (1) and (2) on labor earnings and income rank at age 30-45. The sample includes children aged 0-18 at baseline who are matched to the RSH data. All regressions control for baseline demographics, including: female dummy, 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. Upward mobility, commuting time, and number of schools are measured at the municipality level. Schooling HH and unemployment HH are measured at the census district level (smaller than a municipality). All other neighborhood characteristics are measured at the project/slum level. Standard errors clustered by slum of origin are reported in parentheses. 10%*, 5%**, 1%***.