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

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February 6, 2023

Abstract

We use evidence from a slum clearance program implemented in Santiago, Chile, between 1979 and 1985, to study the long-term effects of moving to a high-poverty neighborhood on children's earnings and schooling. During the country's dictatorship, the government mandated families' relocation to public housing in low-income areas. Two-thirds were relocated to new housing projects on the periphery of the city, and the rest received housing at their initial location. We construct a novel dataset that combines archival records with administrative data containing 16,548 homeowners matched to 45,750 children. To estimate a displacement effect, we compare the outcomes of displaced and non-displaced children 20 to 40 years after the end of the policy. We find negative effects: Displaced children have 9.4% lower earnings and 0.68 fewer years of education as adults compared to non-displaced children. Moreover, displaced children are more likely to later work in informal jobs. Destination characteristics mediate our results: Decreased social capital in destination locations reduces children's education, and their future labor earnings are also affected by worse labor market access and lower property prices. Building new infrastructure helps reduce the earnings gap between displaced and non-displaced children.

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

*Rojas-Ampuero is indebted to Dora Costa, Michela Giorcelli, and Adriana Lleras-Muney for their continual support and guidance throughout this research project. We thank Natalie Bau, Juliana Londoño-Vélez, Martha Bailey, Rodrigo Pinto, Daniel Haanwinckel, Victoria Barone, Matthew Khan, Raimundo Undurraga, and participants in the Applied Microeconomics Proseminar, the Economic History Proseminar at UCLA, WEAI student workshop, YES conference, SECHI, UEA conference, U Toronto Munk School, Notre Dame, FEN, PUC-Chile, IESE, CEMFI, Duke, UW-Madison, Northwestern Development Rookiefest, Opportunity Insights, NEUDC, and LACEA for helpful comments. The authors thank Sebastian Figari, Vicente Rojas Ampuero, Francisco Norambuena, Ethan Duong, Maeve Giza, Katherine Jones, Alissa Lioznova, Alejandra Martínez, and Yuting Zhou for their help with the data work and digitization process. We gratefully acknowledge the contribution of Genealog Chile and Coderhub in the construction of the dataset used in this project. We thank Chile's Superintendence of Social Security (SUSESO) for granting access to its administrative data. We especially thank Felipe González for sponsoring us with Chile's Ministry of Social Development. This research used information from the Registry of Social Information (RIS). The authors thank the Undersecretary of Social Evaluation, owner of the RIS, for the authorization to use the databases that are part of the RIS in accordance with the provisions of Exempt Resolution No. 412, of 2019. All the results of this study are the sole responsibility and interpretation of the authors and do not commit said Undersecretary in any way. Rojas-Ampuero benefited from facilities and resources provided by the California Center for Population Research at UCLA (CCPR), which receives core support (P2C-HD041022, NICHD). Finally, the authors thank the UCLA Ziman Center for Real Estate's Rosalinde and Arthur Gilbert Program in Real Estate, Finance and Urban Economics, the 2019 LAI-Tinker Field Research Grant, and the Treiman Fellowship, CCPR at UCLA, for generous funding.† Corresponding author, Harvard University. Email: frojasampuero@fas.harvard.edu. ‡ Reed College. Email: fcarrera@reed.edu.

1. Introduction

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

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

We use the variation between the two groups to estimate a displacement effect. While both groups of families became homeowners, the displaced were forced to move to a new location. Thus, what differed between groups was the disruption from having to move and the characteristics of their destination locations. We first use the variation with respect to which slums were moved to identify the total impact of displacement, since the selection of slums into the displaced or non-displaced group depended on the feasibility of urban renewal, not on individual family or slum population characteristics. Urban conditions such as slum density, geographic location, and price of land across municipalities determined whether slums were appropriate locations for building on-site public housing. We find that within municipalities, good predictors of displacement are property prices and proximity to rivers. However, we find no evidence that the choice of which slums to evict was correlated with the demographic or socioeconomic characteristics of the slum's families before the program. We also find no evidence that dis-

[1] Examples of this policy can be found in Brazil (Dasgupta and Lall, 2009), India (Barnhardt et al., 2016), and Kenya (see here). Many slum clearance programs around the world are implemented through forced relocations of the poor; for more details, see Goetz (2012). Historically, building social housing on city peripheries used to be a common policy in many European cities during the 1950s and 1960s (Power, 1993; Hall, 1997) and in Latin America (Sabatini, 2006). In countries like the US or Canada, public housing is not necessarily built in city peripheries but rather is usually located in poor areas of the city (Chyn, 2018; Oreopoulos, 2003).

placed and non-displaced slums differed in their access to public goods, the characteristics of their populations, or their access to labor markets.

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

We create a novel dataset that follows children and parents from displaced and non-displaced slums 20 to 40 years after the end of the policy. This dataset is constructed from archival records and administrative data. We determine where families were sent, match children with their families, and match individuals with data on employment, labor earnings, and years of schooling. Our final sample contains 16,548 families treated between 1979 and 1985 and observed from 2007 to 2019. The final data of children comprise 45,750, of whom 32,998 were between 0 and 18 years old at the time of the policy.

Our results show that displacement is detrimental for children aged 0 to 18 at baseline: Compared with non-displaced children, displaced children earn, on average, 9.4% less per month across their life cycle. This negative effect on earnings is not associated with lower employment but with the quality of employment: Displaced children are more likely to work without a formal contract and in temporary jobs. In addition, we find that displacement reduces children's educational attainment: A displaced child loses 0.7 years of education and is 17% less likely to graduate from high school relative to a non-displaced child. Our results are robust to controlling for the variables that predict displacement at the slum level (property values and slums' characteristics).

We next study heterogeneous displacement effects by age at intervention. We find that young children who were 0 to 14 years old at the time of the intervention are the most affected. Within this group, children who were 0 to 5 years old face a more negative effect on earnings because they are less likely to attend college compared with the non-displaced. The effect is especially negative on formal earnings. These results are consistent with what previous work has called an *exposure effect* of moving (Chetty et al., 2016; Chyn, 2018; Laliberté, 2021).

Several mechanisms could explain the negative displacement effect on children aged 0 to 18 at the time of intervention. These range from access to public services, segregation, and transportation access to a lack of social capital and cohesion at destination. We estimate a distribution of displacement effects on children's earnings by municipality of origin and find there is ample variation: While the average effect on earnings is negative, some children did better off. Thus, we explore granular changes to neighborhood characteristics to understand the determinants of the displacement effect. We find that in our sample, around 80% of the displacement effect on earnings can be explained by changes in poverty (measured as the population's schooling), distance to original locations, and decreased social capital. Negative changes in property prices make the displacement effect more negative, but changes in access to schools do not explain the variation in the effect.

These findings lead us to study two main mechanisms: lower social capital in destination locations and segregation and access to transportation. For the first mechanism, we study neighborhood fragmentation as a proxy for lack of social capital, measured as a normalized Herfindahl-Hirshman index. We find that the increase in neighborhood fragmentation in new locations explains a great share of the effect on earnings because it impacts years of schooling. To estimate the effect on children, we compare housing projects that mixed slums from different origins with those that did not. We find that the more fragmented the neighborhood (the more mixing), the more negative the displacement effect on children's education. This result is robust to including other neighborhood attributes and other measures of social capital, such as polarization within the neighborhood or the project's size. However, the negative effect of fragmentation can be counteracted by bigger social networks, measured as the share of families from a child's slum of origin in the new locations. Overall, we find that children from families that were not mixed and were displaced together do not face a negative displacement effect on earnings or schooling.

For the second mechanism, we find that segregation and access to transportation determines children's future labor earnings. Displaced families in destination neighborhoods had longer commuting times, longer distances to work, and lower commuter market access (CMA) for at least 20 years after the end of the policy, as public transportation in Santiago did not improve substantially until the 2000s. Thus, we use the rollout of new subway stations in Santiago between 2010 and 2019 to explore changes in infrastructure on children's labor market outcomes. We find that when a new metro station is built close to a family's destination neighborhood, the earnings difference between the displaced and non-displaced children reduces to between 33% and 75% depending on the proximity to the subway. This change is a consequence of higher employment of the displaced and higher earnings in both formal and informal labor markets.

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

Second, a large literature in economics and sociology studies the role of neighborhoods on individuals' economic outcomes and on intergenerational mobility (Sampson, 2008; Galster, 2012; Ludwig et al., 2013; Chetty et al., 2016; Chetty and Hendren, 2018; Chyn, 2018; Pinto, 2022; Mogstad and Torsvik, 2021; Chyn and Katz, 2021), with results for children varying by the examined outcome and age and with different results for children and adults.[2] We examine the effects of moving to a poor neighborhood on children over a longer period of time than many studies, and we find persistent effects for the individuals in the program.

Third, in the literature that studies the mechanisms that shape neighborhood effects, previous research has emphasized the roles of schools (Laliberté, 2021), peers (Damm and Dustmann, 2014), and public investment (Derenoncourt, 2022). We study the mechanisms by exploiting movements in groups and the variation in destination locations, finding that children's schooling and adult earnings respond to different neighborhood characteristics. We decompose the different mechanisms for each outcome: Children's education is more responsive to social capital and their future labor earnings are also determined by segregation and labor market access.[3] This last result is consistent with the literature on uneven geographical access to jobs and the spatial mismatch hypothesis (Kain, 1968; Kain, 2004; Andersson et al., 2018; Haltiwanger et al., 2020).

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

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

[3] The question regarding fragmentation and social cohesion is more common in the development economics literature that looks at indigenous reservoirs and forced displacements and how fragmentation and forced coexistence enhance (or not) economic development. See, for example, Dippel (2014) or Bazzi et al. (2019). Our paper is similar in that regard as displacement forced the coexistence of families from different slums of origin, but we do not have the ethnicity component.

2. Historical Background: The Program for Urban Marginality

In the late 1970s, Chile had high levels of urban poverty after decades of urbanization. In Greater Santiago, the country's main metropolitan area,[4] approximately 15% of the population lived in a slum (INE, 1970, 1982). A slum was defined as a squatter settlement without access to drinking water, electricity, or sewage (MINVU, 1979).[5] Besides housing a large fraction of the population, slums were geographically ubiquitous: Every municipality in the city contained at least one. After the beginning of the Pinochet dictatorship in 1973, any attempt to create a new slum faced a strong military response.[6]

Motivated by this housing crisis, between 1979 to 1985, Chile's Ministry of Housing and Urban Development (MINVU) implemented the Program for Urban Marginality, a massive slum clearance and urban renewal policy. Proponents of this program believed the most effective way to end poverty was to house poor families by making them homeowners regardless of the attributes of the new housing units or neighborhoods (Murphy, 2015). At the onset of the program in 1979, the government conducted a census of slums and targeted 340 slums to be cleared.[7] According to Molina (1986) and Morales and Rojas (1986), by 1985, between 40,000 and 50,000 families were involved in the program, accounting for 5% of the population of Greater Santiago. The cost of the program was low: The average housing unit cost US$7,700, and the program's average total annual cost was US$34 million, which was about 0.2% of Chilean GDP at the time.

The Program for Urban Marginality had two features. First, it aimed to build public housing for low-income families where land was cheap. Second, it aimed to provide families with housing in places where they could afford it. With these goals, MINVU implemented two different types of interventions for slum dwellers: Whenever conditions permitted, families would remain in their original location, and their slum would go through an urban renewal process to provide them with housing on-site (i.e., non-displaced group). If this was not possible, the slum's residents would be evicted from their original location, and families would receive a housing unit in a different location (i.e., displaced group). All families in the same slum would receive the same treatment, and all of them would become homeowners.[8]

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

[4]Santiago is the capital of Chile, and at the time it contained 34.8% of the country's population.

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

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

[7]Other evictions occurred between 1976 and 1978 and are considered a precedent for the Program for Urban Marginality. They were called Operaciones Confraternidad I, II, and III. These were politically motivated forced evictions, and hence we do not include them in our analysis (for more information, see Celedón, 2019).

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

one-third of the total number of families in the program, and their slums went through a process of urban renewal. In some cases, these families would get an apartment in projects constructed very close to their original site; in other cases, the slum's land was subdivided among all the residents, and families received a "starting-kit unit."[9] These new neighborhoods were provided with all of the basic services (water, electricity, and sewage). To pay for the new units, families received a 75% governmental subsidy.

The displaced families accounted for two-thirds of the total number of families. These families were evicted and moved in groups to public housing projects located in peripheral sectors of the city. They received a house or an apartment in these new neighborhoods and became the owners of a new housing unit that had a 75% governmental subsidy. The land used by the slum was then cleared and used for a different purpose.[10] The destination neighborhoods were not prepared to receive the large number of displaced families involved in this program (Molina, 1986; Aldunate et al., 1987). A large fraction lacked access to public transportation and public goods and services, such as schools and health care centers, and many were located in former rural areas recently added to the metropolitan area.

Decisions regarding the program's implementation were made directly at the central government level by MINVU. Santiago lacked a citywide government; instead, there were 34 local municipalities that managed each territory. Under this governance structure, citywide policies such as social housing were defined at the central government level. 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).

Families did not participate in the decisions made by MINVU, and given the political circumstances, they could not oppose the policy. Instead, displaced families were assigned to destination locations based on the current availability of finished housing projects across the city.[11] Destination municipalities could not influence how the Program for Urban Marginality 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 a variety of circumstances that prevented families from staying in their original locations. These circumstances ranged from slums being too close to freeways to being on a riverbank—especially the Mapocho River, which had a high risk of flooding during winter months. Other circumstances were related to features of the land

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

[10] All families would be evicted, and if they did not want to move, they would be excluded from the program. According to social workers, it was unheard of for families to not accept the subsidy because for most of them, it was their only chance to become homeowners.

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

itself, such as public versus private property, the density of a slum (number of families per site), and potential difficulties for the provision of sewage, water, and electricity. Land value also mattered; as Rodríguez and Icaza (1998) explain, "other criteria included the reputation of the municipality of origin, their land values, and the speculation about future prices." This explanation is consistent with the fact that evictions were more common than urban renewal projects in high-income municipalities.

A well-documented example of how MINVU decided to displace a slum is presented by Murphy (2015) for Las Palmeras, a slum in a low-income municipality. Originally, MINVU's official plan was to create a neighborhood for families on the original location. However, by 1981, the high density of Las Palmeras made it impossible to allocate plots inside the slum in a way that guaranteed a minimum size for all the plots. Thus, the authorities decided to include Las Palmeras among the slums to be displaced. In late 1983, residents were moved to a new neighborhood built on the outskirts of the municipality, and the former slum became a park. A second example is the slum dwellers located in the riverbank of the Mapocho River, who were displaced in 1982 after it flooded. More than 3,000 families from the slums El Ejemplo, El Esfuerzo, El Trabajo, and others—originally located in Las Condes, a rich municipality—were relocated to La Pintana and San Ramón, two low-income municipalities in the south of the city.[12]

Figure I plots the urban limits of Greater Santiago and its municipalities. Panels (a) and (c) depict the location of slums in 1979 and show that they were located everywhere without a particular concentration in any municipality. Panels (b) and (d) show the location of the housing projects built to receive slum dwellers in 1985. Neighborhoods where housing projects for the displaced were built are purple, and housing projects for the non-displaced are light blue. Two important conclusions can be drawn from this figure: the new housing projects were disproportionately built in the peripheral areas of the city, and public housing projects were farther from job opportunities (in gray scale).

After 1985, Aldunate et al. (1987) evaluated the program by surveying 592 families that were displaced in 1983, and Álvarez (1988) collected families' testimonies. The families in these studies reported liking their home better but that the quality of the new neighborhoods was worse than the slums in several respects: They had fewer job market opportunities, and it was harder to access transportation, education, and health care services. The families also perceived their new neighborhoods as more dangerous and lacking public services (see Appendix B for a summary of these results).

[12]Most of these families were relocated to El Castillo and La Bandera neighborhoods.

3. Data

In this section, we summarize the data collection process. We construct a novel dataset that tracks parents and their children, slum of origin, and destination neighborhood, and then match these individual records to administrative data on labor market outcomes.[13]

3.1 Archival data: Slums and homeowners

We digitize two slum censuses conducted by MINVU in 1979 and 1984 that contain information on slums' names, their locations, and destination projects. We classify each slum as displaced or non-displaced and the final destination of the displaced families. We then complement these data with information collected by Molina (1986) and Morales and Rojas (1986), who compiled a full list of slums, locations, and destination neighborhoods by year.

Next, we find the families in the program. We collect and digitize archival data from the Regional Housing and Urban Planning Service 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 for Urban Marginality. We collect data for 22,689 unique recipients of social housing, representing 56% of the total number of recipients (Molina, 1986).[15] We focus on individuals in Greater Santiago, excluding rural municipalities, leaving us with 20,620 unique recipients of social housing.[16]

The archival data contain information of the recipients of the property deed (heads of the household) and their spouses, full names, national identification numbers (NIDs), and new addresses. These records are grouped by year of eviction/urban renewal and project of destination (Figure C.1), and we match them to their slum of origin using the slum censuses of 1979 and 1984. Our matched sample contains 16,548 recipients with a valid NID. We lose people because some individuals did not have a valid NID due to mistakes or older versions.[17] Missing NIDs were more common for older people or for those who did not report having a spouse. Hence, in our matched data we are more likely to observe younger heads of households and married individuals.

Compared with the total program, we are more likely to find displaced families (70% versus 65%). In Appendix Section C.4 we discuss attrition by comparing the slums of the families we find in the archives versus Morales and Rojas (1986) and conclude that we find families from

[13]See Appendix C for a detailed description of the process and variables.

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

[15]We could not find all of the records; details can be found in Data Appendix.

[16]We exclude rural municipalities because most of the neighborhoods' characteristics we can measure in the 1980s are only available in urban areas.

[17]We could not validate them using contemporaneous data. We used data from Chilean electoral records in 2016 to validate full names and NID numbers. After the data were validated, we searched for people's birth certificates.

larger and more urban slums, as measured by the distance to the central business district. We believe this type of attrition might bias our results upward.[18]

3.2 Matching process: Children sample

The second stage is to find the children of each family. We worked with Genealog Chile and web scraped birth and marriage certificates for the Chilean population 18 and older in 2016.[19] The birth certificates contain full name at birth, date of birth, NID number, and parents' full names. We matched homeowners' archival data with their children using their NID. If the birth certificate did not contain at least one parent's NID, we matched using a first name, a middle name, and two last names.[20]

We find 45,750 children of 14,765 unique families (1,783 families did not have a child). Of these, 32,998 individuals are children aged 0 to 18 at the time of the intervention (13,853 families). This is our estimation sample. Because of attrition due to the loss of NID numbers, it is likely that in our matched sample, younger children will be overrepresented because we are losing the oldest heads of households.

3.3 Measuring outcomes: Matching to administrative data

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

The second source of administrative data is the Gestión de Reportes e Información para la Supervisión de Mutuales (GRIS), an information system managed by Chile's Superintendency of Social Security. This system collects data on all workers in the formal sector who contribute to social security each month. Hence any worker with a contract is in this database. We observe monthly data on taxable income starting from July 2016 to December 2019.

[18]In Appendix C.4 we make a full comparison between the slums of the families in our sample with the slums in the full program and discuss concerns about selection due to attrition. We also estimate the probability of finding a slum as a function of its characteristics.

[19]We web scraped certificates from Chile's Civil Registration and Identification Service.

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

3.4 Municipality and district attributes

We measure location attributes, such as education and employment, by municipality and by census district, which come from the 1982 Census of Population, in which we observe variables such as years of education and employment status. We combine these measures with historical records from the Ministry of Education and the Ministry of Health in 1985 or earlier on schools, hospitals, and family health care centers. In addition, we have information on subway stations built in Santiago and their opening dates and locations; these are publicly available from Greater Santiago's subway system. We measure property prices at the neighborhood level from newspaper listings that we collect and digitize from 1978 to 1985. Finally, we collect information on waiting times for public transportation at the municipality level from the historical records of Santiago's Origin-Destination Surveys in 1977 and from micro-level data for 1991, 2001, and 2012.

3.5 Estimation sample and summary statistics

In our estimation sample, we include all children who were at least 21 years old at the time of income/employment measurement.[21] Table I presents summary statistics of the children in our full sample at the time of the intervention (column (1)). The table shows that 68% of children come from families that were displaced. Half are female, and the average age is 8.22 years at the time of the intervention. They have approximately three siblings on average, and 37% are firstborn. Their parents are 35 years old on average at baseline, 31% come from a female-headed household, and 81% have parents who were married at the time of the intervention. Only 1% of the total number of children in our baseline sample died before 2007. The table also shows that 81% of our baseline sample appear at least once in the RSH (column (2)) and 67% at least once in the GRIS (column (3)). In the RSH we match slightly more children from displaced families, with a share of 70%, and in the GRIS we match slightly fewer children from displaced families, with a share of 67%.

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

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

These summary statistics, combined with the attrition rates from the archives, imply that our matched RSH sample of children corresponds to 40% of the total number of children in the program (0.81*0.49). In this group, children who were displaced, young, or female are overrepresented. The fact that we find more females and younger children will bias our estimates only if these characteristics are not balanced between the displaced and non-displaced or if they affect the displaced and non-displaced differently. In the next section we show that this is not the case.

Finally, not surprisingly, we conclude that the individuals in our sample are poor. They have lower incomes than the universe of individuals in the RSH (see Figure A.2). In 2018 the population in the RSH reports a median monthly salary of CLP$183.998 ($\sim$ US$250), and the median monthly salary in our sample is even lower: CLP$178.855 ($\sim$ US$240). These numbers are low compared to estimates for the full Chilean population since the median monthly salary for a Chilean worker in 2018 is CLP$450,000 ($\sim$ US$600), which is more than twice the median salaries in the RSH.[22]

4. Empirical Strategy

4.1 Identifying a displacement effect

To estimate the impact of forced displacement on children, we exploit the fact that within the same municipality, certain slums were chosen for eviction while others were not. Thus the empirical strategy we adopt is to compare the children of displaced families with children of non-displaced families conditional on the municipality of origin. Since the process of sorting slums into displaced and non-displaced did not depend on households' characteristics but instead on the feasibility of renewal on-site, non-displaced children serve as a comparison group for the displaced *within* the same municipality.[23] Any differences between children in the displaced group and the non-displaced group are attributed to the eviction process and subsequent relocation to a new project.

We estimate a linear model to study the impact of the displacements on children, using the

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

[23]Greater Santiago is administratively divided into 34 municipalities. On average, a municipality has a population of 200,000, and its area can vary between 20 km^2 and more than 100 km^2. Hence, it is smaller on average than a US county. A municipality is a geographical and political unit, and each municipality currently has an elected mayor. However, during the Pinochet dictatorship, mayors were directly appointed by the central government.

following specification:

$$Y_{it} = \alpha + \beta Displaced_{s\{i\}} + \psi_o + X'_{it}\theta + \varepsilon_{it}, \tag{1}$$

where Y_{it} is the current outcome for individual i at time t, such as labor income, employment status, or years of schooling; and $s(i)$ indexes the slum of origin for individual i's family. The variable $Displaced_{s\{i\}}$ equals 1 if an individual's family lived in a displaced slum and 0 otherwise. 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. For precision, we add baseline controls for individual and family characteristics, such as gender, child's year of birth, female head of household, married head of household, head of household's age, indigenous last name, birth-order dummies, and year of intervention fixed effects (1979 to 1985) that control for aggregate temporal differences across the six years this housing program was in effect. When the outcome is income or employment, we include semester fixed effects to account for common temporal shocks across individuals.

The treatment was at the slum level; however, within the same municipality, displaced and non-displaced slums could have been subject to common shocks or similar social policies. Thus, to account for any potential correlation among slum residents with the same origin, we cluster standard errors at the level of municipality of origin.[24]

4.2 Comparing displaced and non-displaced children at baseline

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

In Table II column (1) reports means for several demographics for the non-displaced. Column (2) shows that conditional on ψ_o, there are no statistical differences between both groups for 9 out of 11 observables, but displaced children come from families in which the head of household is less likely to be married (7% less) and are more likely to come from a household with an indigenous last name (Mapuche head of household).[25] This last difference is sizable relative to the non-displaced (0.02/0.05); however, the share of the population we identify as indigenous is small relative to our full sample (only 5%). Hence we do not expect this variable

[24]We compute other clustering, such as clustering by slum or Conley standard errors. We discuss these in more detail when we present our baseline results.

[25]See Appendix C for a definition of each variable.

to determine the displacement effect.[26]

As a measure of families' socioeconomic status, we measure mother's years of schooling. This variable is only available for children whose mothers we find in the RSH data (70% of our full sample of children). Hence, the variable might be subject to selection, especially if displacement impacts the likelihood of finding these mothers in the data.[27] That is why whenever we use this variable, we take a control variable approach and include an estimate of the likelihood of finding a mother in the RSH.[28] As expected, the children in our sample have mothers with very low education: On average, non-displaced mothers have 6.25 years of education. Displaced mothers have 0.28 fewer years of schooling relative to non-displaced mothers, but this coefficient is small and not statistically different from 0. The results are very similar for the children we matched to the RSH (columns (3) and (4)) and for the children we matched to the GRIS (columns (5) and (6)). This confirms that attrition by gender and age is not different between displaced and non-displaced children.

Overall, we conclude that these two groups are very similar in their observables conditional on municipality of origin. However, a concern arises because we do not have a measure of household income at the time of intervention.[29] We claim that part of the variation is already captured by the municipalities of origin since they were highly homogeneous units by socioeconomic status at the time the program occurred. To provide evidence for this claim, in Table A.3 we report the share of the variance of income and schooling that can be attributed to the variation within municipalities in several sources of data. The share varies from 21% to 28%, and thus at least one-fourth of the variation in outcomes is captured by the municipalities of origin, which reduces the concerns of potential bias in our estimates.

4.3 Slum characteristics and location attributes before and after the intervention

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

Panel A of Table III compares slum characteristics and shows that displaced slums are denser since they have more families but are located in plots of similar size (area) compared to non-displaced slums. They are also closer to rivers, and the prices of properties that surround the displaced slums are higher. In Appendix Table A.1, we estimate the probability of displacement as a function of slum characteristics. Across municipalities, density is a good predictor of

[26] In Appendix Table E.1 we compute these differences for the adults in our sample, and the differences across demographics are the same.

[27] This is exactly the case because displaced mothers are more likely to die than non-displaced mothers before 2007. This can thus widen the difference between the two groups. See the results in Appendix E.

[28] See Appendix C for details on the construction of the demographic variables.

[29] These data exist, but the Ministry of Social Development and Family does not share this information with researchers.

displacement, but once we control for municipality of origin fixed effects, good predictors of displacement are distance to rivers and property prices. This is consistent with the historical evidence presented in Section 2.[30]

Panel B of Table III compares the characteristics of the census districts in which the original slums were located. We compute the average for several location attributes at the census-district level, which corresponds to a smaller geographic level than a municipality. Conditional on municipality of origin, displaced and non-displaced families lived in neighborhoods that were very similar to each other. The only sizable difference is the number of schools per student, which is not statistically significant.[31]

Finally, we look at neighborhood characteristics after the intervention (columns (4) and (5)). Displaced families were moved to places where the population had higher levels of unemployment and lower levels of schooling, with fewer schools in total and per student. They also ended up farther from public transportation and had longer commuting times, and property prices were lower in their new neighborhoods. These results are consistent with the evidence provided by Aldunate et al. (1987). We also measure fragmentation, or the degree of fractionalization in destination neighborhoods, as displaced families ended in neighborhoods that mixed families from different slums. On average, displaced families ended up in a housing project that mixed families from two slums.[32]

4.4 Displaced families' characteristics and new location attributes

Our identification strategy relies on the idea that displaced and non-displaced families were quasi-randomly selected for eviction. However, since families did not choose to move to a particular location, a concern arises that certain types of families were systematically sent to worse locations, which could potentially explain any negative displacement effect we find in our sample. Qualitative evidence from interviews with social workers who worked with the families in the eviction process causes us to believe that the assignment was as good as random. According to the social workers, MINVU assigned families to locations based on the availability of units.

To provide statistical evidence for the assumption that there is no selection on observables in the displaced group, we test whether families' demographics predict destination location attributes. We run a regression of several location attributes on a set of family demographics at the time of the intervention in the sample of displaced families. Our results are reported in

[30] Our results on earnings are robust to including these predictor variables.

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

[32] Fragmentation is measured as an Herfindahl-Hirschman index normalized between 0 and 1 that uses as shares the fraction of families from each slum in each destination project. More details are provided in Section 7.

Table A.7. We report the F-test of joint significance of baseline controls and its corresponding p-value: For 10 out of 12 different location attributes, we do not reject the null of joint significance of controls. We interpret these results as evidence of the new locations being quasi-randomly assigned to displaced families.

5. Results

5.1 *Displacement effect on earnings and schooling*

5.1.1 *Labor market outcomes*

We start our analysis by looking at the earnings and employment of individuals (aged 0 to 18 at baseline) who are 18 to 60 at the time their income is measured, with non-missing education. The main outcome for earnings is self-reported labor income in the RSH, for which we have many observations, and the biggest matching rate. This variable measures income from both formal and informal employment and includes wage income and proprietors' labor income but excludes pensions and transfers.[33] Earnings are measured in 1,000 Chilean pesos per month (CLP$1,000/month).[34] Employment is reported in the RSH and includes both formal and informal employment.

Table IV shows negative effects of the displacement on earnings (panel A) and null effects on employment (panel B). Column (1) reports the difference in earnings and employment between displaced and non-displaced children conditional on the municipality of origin, and column (2) includes baseline controls for precision. This last column indicates that displaced children have lower future earnings compared with non-displaced children: The coefficient of –14.700 in column (2) panel A is statistically significant at 5%. This means that displaced children earn 9.4% less than the non-displaced on average per month (see row labeled "% Variation w.r.t non-displaced").

In contrast, panel B shows no effect on employment; if anything, the coefficient is positive but not statistically different from zero. The results in column (2) are robust to including slum and neighborhood of origin characteristics, as shown in columns (3)–(5). Overall, the displacement effect on earnings is more negative and equal to –10.2% in column (5), which includes all location controls. On employment, the coefficient remains positive and statistically different from zero. Thus, our preferred specification is column (2), and we will use it throughout the rest of the paper.

For comparison, Table IV reports Conley standard errors in brackets (Conley, 1999) to

[33]We do not impute zeros to people we do not find in the matched sample, and we keep zeros if individuals reported earnings as such.

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

account for any spatial dependence across slums that are close to each other.[35] The table shows that the municipality of origin is a conservative measure of clustering. Thus, in all of the following estimations, we report clustered standard errors by municipality of origin.[36]

Displaced children's lower earnings are related to higher informality. Table V shows that, as adults, displaced children are 4.1 percentage points less likely to work with a contract (column (3)), which is equivalent to a 9.8% lower probability relative to non-displaced children. They are also 4 percentage points more likely to work in temporary jobs (column (4)), which is 6.6% more than non-displaced children. This is also reflected in columns (5) and (6), where we split earnings between formal and informal sources. The results show that all the negative effect is due to lower earnings in the formal labor market, and we find no differences between displaced and non-displaced children's informal earnings.

In column (7) we estimate a displacement effect on taxable income, which is only available for individuals who contribute to social security (GRIS sample); thus these are the earnings of workers with formal jobs between 2016 and 2019. We find a bigger displacement effect in magnitude (displaced children earn CLP$36.738 less per month as adults) but smaller in relative terms (-6.4%). The result in column (7) serves the purpose of showing the same result as in (5) but from an administrative source and not from a self-reported source as in the RSH, which poses a concern if the displaced and non-displaced children underreport earnings at different rates.[37] Thus, even if the displaced are more likely to underreport their earnings, we still see a negative 6.4% displacement effect on taxable earnings.

We next estimate a displacement effect on children's earnings and employment as adults across the age cycle (Figure II). Across the entire age distribution, the income trajectories of displaced children are below those of the non-displaced, with minor differences in employment, as in the pooled regressions. The difference in earnings starts at age 28, while the difference in employment closes after age 28. In Figure A.5 we show that the differences in employment in the first part of the age distribution are due to differences in school attendance: Non-displaced children are more likely to be out of the labor force because they are attending school, but after the age of 30, the differences in earnings arise because they increase their formal earnings.

[35] We compute Conley standard errors for all regressions at the cutoff distance of 14 km. We choose 14 km because it is the distance that maximizes standard errors for our main outcomes, as shown in Table D.5. We estimate the standard errors at different cutoffs between 2 km and 15 km; we limit the upper bound to 15 km because a cutoff of 15 km would include the largest municipality in Santiago, measured in square kilometers.

[36] Another option is to cluster standard errors at the level of intervention by slum; however, clustering by slum does not account for the potential correlation between slums within the same municipality. As a robustness check, we compute clustered standard errors by slum in our baseline regressions and find that the standard errors are smaller than when clustering by municipality. The results can be found in Appendix Table D.5.

[37] Intuitively, if the displaced children are poorer, they might be more likely to underreport earnings to be eligible for social benefits.

5.1.2 *Educational outcomes*

Our estimates show that displacement negatively impacts children's educational outcomes. Column (1) of Table VI indicates that displaced children have 0.68 fewer years of schooling than non-displaced children. We find that the displacement effect on high school graduation and college attendance are more negative than on years of schooling. The results show that displaced children are 18% less likely to graduate from high school, 26% less likely to attend a two-year college (technical degree such as mechanics, electrical technology), and 38% less likely to attend a five-year college (professional degree such as medicine, engineering, economics) relative to the non-displaced. Overall, these results suggest that displacement affects children's educational attainment by reducing their likelihood of getting their high school diploma, and hence their likelihood of attending college is even lower.

The negative effect on years of education can explain about 70% of the negative effect on earnings that we find in our sample. According to CASEN (2017),[38] one extra year of education for the population that finishes high school increases earnings by about 10%. The displacement effect on earnings is –9.4%, while the effect on education is –0.68 years of education. Hence the decrease in years of schooling accounts for about 70% of the total effect on earnings.[39]

5.1.3 *Occupations, industries, and other demographics*

Appendix Table A.4 examines the occupations and industries where children end up working. The table shows that displaced children are less likely to be employers or employees as adults, but they are 13.5% more likely to be independent workers, which is consistent with higher informality in the labor market. In terms of industries, they are more likely to work in construction and manufacturing jobs compared to non-displaced children.

Overall, we find that displaced children are a more vulnerable population. Table A.5 shows they have a 17% higher probability of becoming parents as teenagers and end up having more children (but the coefficient is small). They are also 11% more likely to be on welfare, receive higher amounts of governmental subsidies compared to non-displaced children, and are 28% more likely to be incarcerated as adults. Interestingly, they are less likely to be renters (Table A.6), not because they are more likely to own a house but because they are more likely to live in a transferred property (probably from their parents).

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

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

5.2 *Robustness checks*

We perform several robustness checks. A first concern arises due to the fact that non-displaced families and their children saw an improvement in their neighborhoods, especially in richer municipalities after the expulsion of low-income families.[40] Hence, the negative displacement effect we find might not be a negative effect on the displaced but a positive effect on the comparison group. To test for this hypothesis, we perform two exercises. In the first exercise, we divide the non-displaced group into two groups: those who prior to treatment lived in slums with a displaced slum nearby (in the origin) and those who lived in slums without a displaced slum nearby. The rationale for this is that the first group should have experienced a bigger improvement in neighborhood quality if the cleared area was rebuilt. Table D.1 shows that non-displaced children who live within 0.5 or 1 km of a displaced slum have higher earnings as an adult relative to non-displaced children without nearby displaced slums (coefficient is not significant), but this does not change the effect on the displaced children.

In the second exercise, we drop the richest municipalities that were net expellers (i.e., they expelled more families than they received), as they might see the biggest improvements in land prices after the forced evictions. By doing this, we do not find evidence of a displacement effect being driven by improvements for the comparison group (Table D.6). In fact, our results are not driven by any particular municipality in our sample (Figure D.1).

A second concern is related to whether differential attrition due to selection from the national archives or from matching to administrative data could bias our results. To address this, we estimate the probability that a slum is found in the archives as a function of slum and neighborhood characteristics by origin (C.3). We include estimates of the propensity of being found as a polynomial in our baseline regressions and do not find evidence of differential attrition driving our results (see Appendix Table D.7). A different approach to missing data is to compute Lee bounds (Lee, 2009). We compute tightened Lee bounds by municipality of origin and demographic controls (age and gender) and find that our baseline estimate of the displacement effect is within the bounds (Table D.4).

Third, in the previous sections we provided evidence of no selection on observables. However, some concerns arise if the demographic variables we are measuring do not account for all of the selection types in our sample. For example, we do not observe other characteristics of slum dwellers at baseline, such as their relationship with local authorities or the difficulties that each slum's residents had when they left their original location. Political considerations are also relevant, for example, due to selection into treatment because of political opposition to the dictatorial regime. Some of these concerns were addressed when we included control variables at the slum level in columns (3)–(5) in Table IV but not all of them. Thus, to account for the degree of selection of unobservables in our setting, we follow Oster (2019)'s procedure. We

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

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, we perform two other robustness checks in different subsamples, controlling for mother's education and restricting the sample to 1979–1982 (before the financial crisis experienced by Latin American countries in 1982). Our results on earnings and employment are robust to these checks (Tables D.2 and D.7).

5.3 Heterogeneous displacement effects

5.3.1 Displacement effect by age at intervention

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

We test whether the displacement effect varies by age at baseline. To do so, we stratify the displacement dummy in equation (1) by age at intervention into four groups: 0 to 5, 6 to 10, 11 to 14, and 15 to 18.[41] We choose these four groups after performing a structural break at each age from 0 to 18 to test whether there is a change in the slope at each single age. F-tests suggest three breaks on labor earnings at age 5, 10, and 15.[42]

We find evidence of an exposure effect on earnings but not on employment (Figure III). Children younger than 15 in our sample face a more negative displacement effect on earnings, but we cannot reject that the coefficients are different across age groups. The pattern is clearer for taxable income, where the biggest effect is for the youngest children (0 to 5), and for the effects on earnings across the age cycle by groups (Figure A.4).

We find mixed results for schooling outcomes. The results in panel (a) of Figure III do not show an exposure effect on years of schooling or high school graduation because children of all ages face a negative displacement effect of similar magnitude (~ 0.7 years). If anything, in these two variables we observe the opposite of an exposure effect; however, we cannot reject the equality of coefficients across age groups. Where we do see a more negative effect for the youngest children is on college attendance. Panel (f) shows a negative relation between age at intervention and college attendance. The coefficients are negative and different from 0 for children younger than 15, with the biggest negative coefficient for children younger than 5.

We interpret these results for schooling outcomes as a cohort effect: Overall, younger children

[41]In Appendix Figure A.6 we include children aged 19–21 for whom we should not expect a causal effect of the displacement. If we find a difference, it might be attributed to selection.

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

are more likely to go to college when they are older and even more so in the non-displaced group. Hence, displacement is preventing older children from finishing high school and the younger children from attending college. This last result is consistent with the finding of a more negative effect on formal labor earnings for the youngest group of children (0 to 5 years old at baseline) who are the most likely to attend college in our sample.

5.3.2 Displacement effect by demographic groups

The effects of the displacement may vary by demographic group. We find gender differences in employment (first panel in Figure A.3). Women are less likely to be employed (not significant), and men are more likely to be employed as a consequence of the displacement. However, this higher employment does not translate into higher labor earnings for men. This result is consistent with results wherein men on average are more likely to have temporary jobs, which might pay lower wages (Table A.10).

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

6. Negative Displacement Effect: Potential Mechanisms

We find that on average, individuals in our sample face a negative displacement effect. In this section we discuss the mechanisms that could mediate this effect.

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

A place or neighborhood effect is associated with the location attributes that families were assigned to. Families in the displaced group received a bundle of treatments. They became homeowners of new housing units in isolated, lower-quality areas with low access to transportation, and their neighbors changed as a result of mixing individuals in the new locations. The effect of homeownership is not present in our estimates because the comparison group also received a new housing unit in an upgraded neighborhood. However, the value of the houses might differ depending on the location of the new units; thus property prices or differential asset values could also explain our results.

Isolation and lack of services are geographical characteristics of neighborhoods.[43] Based on the theory of spatial mismatch (Kain, 1968, 2004) and the short-term evidence of Aldunate et al. (1987), we expect the lack of employment and lower access to transportation to impact displaced children directly or through their parents. Heads of households reported that they lost their jobs after the displacement and it was harder for them to find new employment in the destination location. This would imply a decrease in earnings within the household after relocation,[44] consistent with previous work by Takeuchi et al. (2007), where the benefits of slum relocation depend on how easy it is for adults to change jobs.

In addition, destination municipalities had less public infrastructure than the original slum locations, such as fewer schools and less access to public transportation (Table III, panel B column (5)). This can also impact the value of homes differentially, depending on the destination locations. As Molina (1986) shows, on average, destination municipalities had fewer resources and did not invest in new public infrastructure upon the arrival of the new families. For example, public investment in transportation did not occur to a substantive degree until the 2000s, and thus displaced families remained isolated for years after the intervention. This might have been reinforced by the fact that all families in the program became homeowners, which has the potential to reduce mobility (DiPasquale and Glaeser, 1999).[45]

Families in the displaced group experienced a change in their neighbors for two reasons. First, people who already lived in the destinations had, on average, lower schooling than the population at the origin (Table III, column (5)). And second, they were mixed with other displaced families in their destination neighborhoods. These changes correspond to the social-interactive attributes of neighborhoods. The new projects were mixing poor individuals with more poor individuals and had small housing units.[46] This concentration of the poor can

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

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

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

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

generate harmful local spillovers that exacerbate social problems (Case and Katz, 1991). This is consistent with Aravena and Sandoval (2005), who argue that mixed and fractionalized projects increase social conflict between neighbors because families do not know each other. Thus, if families had preferences for neighborhood composition, as Takeuchi et al. (2007) suggest, being mixed with new people and losing their original networks could have negative consequences for children's outcomes.

7. Mechanisms

7.1 Destination locations

An important fraction of the displacement effect on earnings can be attributed to destination locations. Based on the fact that destination municipalities were poorer on average, we start the analysis by looking at how including destination municipality fixed effects in regression (1) changes the displacement effect on earnings, employment, and education. Table VII shows the results.

Destination municipality fixed effects are identified because a municipality can expel and receive families at the same time. Thus, the fixed effects are identified as the difference in mean outcomes between individuals from origin o and destination d and the mean outcome of individuals from origin o. They measure all common attributes shared by families in the destination municipalities. Our results show that 99% of the displacement effect on earnings can be attributed to the variation in municipality characteristics at the destination (columns (1) and (2)) and 35% of the displacement effect on years of schooling (columns (5) and (6)).

The fixed effects do not tell us which characteristics of the new locations or projects are the most relevant to explain the variation on earnings, so we proceed by stratifying our sample by municipality of origin and estimate a displacement effect for each municipality. Here, each coefficient should be understood as the displacement effect of leaving municipality o relative to staying. Figure IV, panel (a) presents the distribution of the estimates on earnings and shows great variation by municipality. Some children did better, but most did worse in terms of earnings, with large variations in the degrees of these effects. In panel (b) we repeat the exercise and do the same by municipality of origin and destination, and the figure shows variation depending on where children were assigned.[47]

To determine which location characteristics explain these patterns on earnings, we correlate the estimates in Figure IV, panel (b) with the contemporaneous average changes in location attributes by origin-destination pair.[48] Figure V presents the results and shows that the dis-

[47] Note that destination municipalities are fewer, as displaced families were sent to neighborhoods concentrated on the periphery of the city. See Figure I.

[48] All changes in location characteristics are measured as the difference in the measure of an attribute between the census districts of destination and origin for the year 1985 or earlier, if available. Census districts are

placement effect on earnings correlates positively with the population's schooling (as a measure of overall poverty in the area) and with the number of schools per student at the destination. On the other hand, the correlation is negative with longer distances to the subway and longer distances from families' original locations. We also observe a positive correlation with changes in property prices, a negative correlation with neighborhood fragmentation, and a positive correlation with the share of displaced families from the same slum assigned to a new housing project.

Overall, these correlations go in the expected direction. To understand which of these changes is the most relevant, in Table VIII we combine all these variables as regressors on earnings. About 80% of the displacement effect on earnings can be attributed to changes in the population's schooling, distance from origin, and neighborhood fragmentation (column (2)). Changes in property prices are also important (column (3)): Children with a positive shock counteract the negative displacement effect, but negative shocks make it more negative. Note that schools are important too, but their effect is absorbed by the change in property prices (column (6)). Finally, distance to subway is not as relevant as distance to origin (and it has the opposite sign).[49] These results lead us to believe there are two sets of determinants that explain the variation in children's future labor earnings in our sample: social capital proxied by fragmentation and segregation as a consequence of isolation.

7.2 Composition of new neighborhoods and social capital

The first set of mechanisms we examine is the composition of new neighborhoods as a consequence of the displacement. When a slum was considered for eviction, all families in the slum would be displaced to a new project, and most of the new projects built for the displaced received families from different slums from different municipalities. Approximately 50% of the total number of projects received families from different slums, which accounts for 70% of displaced families and 80% of children in our sample. The mixing of families created fragmented neighborhoods, and a concern arises if this mixing was not random. Appendix Table A.8 shows that the characteristics of the mixed and non-mixed neighborhoods before displacement were very similar to those of the non-displaced slums. Thus, conditional on displacement, the assignment of project characteristics was as good as random.

A fragmented neighborhood might impact children differently than a non-fragmented one, as the former is associated with reductions in social cohesion among individuals because the imposition of living with new neighbors creates conflictive relationships (Aravena and Sandoval, 2005). Testimonies from two displaced slum dwellers exemplify this (Álvarez, 1988). One 20-

smaller than municipalities. This procedure aims to quantify the shocks experienced by the families on location attributes. Note that this method implies that shocks for the non-displaced are all zeros.

[49] As previously mentioned, in Chile students are not mandated to attend public schools in their district of residence. Moreover, families can also choose between public and subsidized private schools (voucher schools). Thus, we cannot rule out that children in our sample were not attending schools in their vicinity.

year-old male said, "Another thing that made us worse off is that two slums were combined here: Isabel Riquelme and Centenario. That started the fights and the gangs, because they distrusted each other and both groups wanted to be better than the other." Another man, 27 years old, said that "Besides us, they have brought other displaced from many slums. Confra is adjacent to the Santa Marta slum and the 21 de Mayo and La Portada housing projects. This is also different to living at Zanjon (original slum), because there you could see other realities. Here is the opposite, we are all sunk in a hole and there is no exit."

We measure fragmentation using a Herfindahl-Hirschman index normalized between 0 and 1, which uses as shares the fraction that each slum represents in the total number of families assigned to a new project. A value of 0 means no fragmentation (no mixing), while 1 corresponds to full fragmentation. We estimate the HHI indices for the universe of neighborhoods in the program.

Our results in Table VIII show a negative correlation between neighborhood fragmentation and children's future earnings. This result is robust to including other neighborhood attributes at the destination, such as property prices or distance from the original slum. According to these attributes, a fully fragmented project has an impact equivalent to 50% to 60% of the average displacement effect on children's future earnings (columns (2)–(6)).

Fragmentation is an important determinant of the displacement effect because it affects years of schooling. In Table A.12 we repeat the previous exercise on employment and schooling. The results imply that employment is a function of the overall changes in poverty (population's schooling and property prices); meanwhile, the effect on children's education is explained in great magnitude by the degree of neighborhood fragmentation, and this is true even when controlling the number of schools in the new locations.

Fragmentation is not the only change in neighborhood composition; thus, in Table IX we dig deeper. Mixing families in destination locations can also create polarized communities. Moreover, some families were moved with only a fraction of their original communities. We therefore measure neighborhood polarization as in Bazzi et al. (2019) and the share of the original network as the fraction of families from the original slum in each neighborhood of destination. We find that polarization correlates negatively with earnings, but its coefficient is not economically significant. The share of the original network has a positive but noisy effect. None of these variables, however, explains a great share of the displacement effect (columns (2) and (3)).

When combined with (column (4)), we see that fragmentation is still an important determinant of children's outcomes. More extensive original networks counteract the effect of a fractionalized neighborhood, and when including polarization, the effect of fragmentation becomes greater. This last result is not surprising because as Bazzi et al. (2019) show, the relation between fragmentation and polarization is highly collinear for low levels of both variables. This makes it difficult to isolate the effect of each variable separately. We find the same in our setting, so we focus on fragmentation as it explains a greater share of the displacement effect, but we cannot claim that our results are not driven by group polarization.[50]

We also include project size (column (6)) and other neighborhood characteristics, such as property prices and the number of schools (column (7)). Our results are noisier but stable. About half of the displacement effect on earnings can be attributed to being assigned to a fully fragmented neighborhood, but the stronger the original network, the smaller the displacement effect.

We next examine if children in non-fragmented neighborhoods face a negative effect. In Figure VI we stratify our sample in three levels ("no fragmentation," "low fragmentation," and "high fragmentation"). We find that displaced children in highly fragmented neighborhoods have a more negative effect, especially on years of schooling, and those in non-fragmented neighborhoods have a 0 displacement effect on both earnings and education. This indicates that, on average, children displaced with their full network and not mixed with other slums do not face a negative displacement effect, and this is robust to controlling for other neighborhood attributes at destination. This last result is very important because it relates to the variation we find in the distribution of displacement effects in our sample.

Finally, if fragmentation is a proxy for lower social capital in mixed neighborhoods, we ask whether its effect persists on neighbors' perceptions in the long term. To do this, we use individual-level data from Núñez et al. (2012), who interviewed families in different neighborhoods in Santiago in 2012 and asked them their perceptions about their current neighborhoods. We observe that individuals living in displaced neighborhoods, relative to those in upgraded neighborhoods, are less likely to trust their neighbors and are more likely to report their neighborhood as conflictive and divided (Table A.13).

7.3 Segregation and labor market access

In addition to the decrease in social capital in destination neighborhoods, our results indicate that isolation and property prices are other determinants of the displacement. Since the displacement occurred 40 years ago, we explore how labor market access has evolved between displaced and non-displaced neighborhoods. To do so, we use data from origin-destination surveys in 1991, 2001, and 2012 and commuter market access (CMA) from Asahi et al. (2022) to estimate equation (1) at the neighborhood level. We are interested in changes in commuting times, distance to jobs, and CMA between displaced and non-displaced across time.

Figure VII presents the results. We observe that in 1991, displaced families travel two more kilometers and commute for five more minutes relative to families living in non-displaced (upgraded) neighborhoods; this is six years after the program ended. The difference remains the same by 2001, but the trend is reversed in 2012. For CMA, we see a change after 2010, but

[50]See Appendix Figure A.7.

it is minor. These results suggest that displaced neighborhoods had lower labor market access for at least 20 years. The change in trends after 2001 is consistent with one of the biggest expansions of the subway infrastructure in Santiago after 2006, which was especially important for improving connectivity between southern Santiago and the rest of the city (Asahi et al., 2022; Pérez Pérez et al., 2022).

These results are relevant because the parents and children of our sample are likely to remain in their assigned neighborhoods. To show this, we estimate a displacement effect on current locations of parents and children after 2016, as the RSH data report locations for a random sample of individuals at the neighborhood level.[51] Table A.14 shows that displaced parents and children are less likely to live in their assigned neighborhoods relative to the non-displaced (but the estimates are noisy); however, 40 years after the policy ended, the baselines remain high: Conditional on finding them in the RSH, displaced parents have a 38% probability of living in their assigned neighborhood (53% for non-displaced) and a higher probability of living in the same municipality (56% versus 67% for non-displaced). For displaced children, the numbers are lower but still relevant: 44% live in the same municipality, and 20% are in their parents' assigned neighborhoods after 2016.

Since an important share of parents and children remain in their assigned neighborhoods, and the city of Santiago has improved its transportation infrastructure in the last 20 years, perhaps the improvements in public transportation reduce the earnings gap between the displaced and non-displaced. To test this, we examine the rollout of the new metro lines in Santiago during recent decades to see whether the construction of a new metro station close to families' assigned locations impacts the displaced and non-displaced differently. As our earnings data start in 2007, we exploit the variation in new subway stations between the years 2010 and 2019.[52]

To estimate the impact of access to the subway, we expand equation (1) to estimate the following event-study regression:

$$Y_{it} = \alpha + \beta Displaced_{s\{i\}} + \gamma_1 Subway_\lambda + \sum_{\tau=-5}^{8} \gamma_{2\tau} Displaced_{s\{i\}} \cdot Subway_{\lambda\tau} + \psi_o + X'_{it}\theta + \varepsilon_{it}, \tag{2}$$

where all variables are defined as in equation (1) and $Subway_{\lambda\tau}$ is a dummy equal to 1 if a subway station is available after year τ within distance λ from children's destination neighborhoods. If access to the subway reduces the future earnings gap between displaced and non-displaced children, we expect $\gamma_{2\tau}$ to be positive. We interpret the following results as suggestive because the location of subway stations is not random.

[51] About 40% of the observations in RSH include a current location at the neighborhood level. We find that about 45% of the individuals in our sample after 2016 have a non-missing observation.

[52] Three new lines were inaugurated during this time period, in the years 2010, 2011, 2017, and 2019. See the maps in Figure A.9 for the geographic variation.

Figure VIII shows a positive change in earnings and employment of having a new subway station built within 1 km. The effects on earnings are due to increases in formal earnings and a small effect on informal earnings. The effects we find are a function of the distance to the subway: The closer to a station, the greater the change in earnings because the coefficient γ_2 decreases with distances longer than 1.5 km (Table A.10). A back-of-the-envelope calculation suggests that access to the subway can reduce the future earnings gap between displaced and non-displaced children between 33% and 75%, depending on proximity.[53]

Our results on earnings are consistent with other studies in developing countries that find increases in earnings as a consequence of improvements in public infrastructure (Zárate, 2021; Pérez Pérez et al., 2022).[54] We find changes in employment but no changes in formal employment (contract), which also suggests more informality. Thus, in the context of our setting with a high share of low-income individuals, our results suggest that access to infrastructure improves earnings in both formal and informal labor markets.

Our results are significant because we exploit late changes to subway infrastructure and still see positive changes. Moreover, subway infrastructure impacts the city as a whole and can impact both the location of workers and the location of employers. Unfortunately, due to data limitations, we cannot study how individuals' commuting choices changed after 2007.[55]

8. Comparison with Other Settings and the Total Displacement Effect on Children

8.1 Comparison of estimates with other settings

Our results show that in our sample, displaced children have 0.7 fewer years of education relative to non-displaced children, earn 9.4% lower income, and are 10% more likely to work in the informal labor market. Our setting is very particular: It occurs in a developing country, and families are moved to high-poverty areas. This renders comparison with other settings difficult because most of the previous literature considers induced movements from high- to low-poverty areas.

With these caveats in mind, we compare the magnitude of our estimates with other studies by computing an elasticity defined as the percentage change in earnings when there is a 1% change in neighborhood quality. Table A.16 reports the results of this exercise. According to these numbers, the implied elasticity in our setting is 0.99. This is larger than the implied elasticity in studies in the US (in Chetty et al. (2016) it is 0.41, and in Chyn (2018) it is 0.72). It is also larger than the implied estimate in Barnhardt et al. (2016) for India (if neighborhood

[53] The results on earnings become smaller as λ increases. See Table A.15 and Figure A.10.

[54] In addition, Meneses (2021) finds that subway expansion enables students from more peripheral areas to access higher-quality schools in more central districts of the city.

[55] The origin-destination survey of 2019 was canceled and never conducted.

quality is measured as urbanicity) but with the difference that this paper focuses on adults and not children.

A first source of difference between our estimates and the other papers is that we include children younger than 7. We compute the corresponding elasticities for different ages and find 1.04 (ages 0 to 5), 0.95 (ages 6 to 10), 1.28 (ages 11 to 14), and 0.64 (15 to 18).[56] In all cases, the elasticities we estimate are greater than in the other studies. Thus, even if we focus on children older than 7, we find an elasticity of larger magnitude than in other studies.

A second source of difference across studies could be attributed to the level of development between countries (since cities in developing countries are more segregated and more unequal) and/or to the population under study because slum dwellers were poorer than the average low-income family in Greater Santiago. In addition, the individuals we study are likely to participate in the informal labor market and we find more negative effects on high school graduation. Previous papers do not find large effects on high school completion but do find effects on college enrollment. If, on average, the return to high school completion is smaller than the return to college attendance, this can explain our different results on earnings (–10% in Chile and 16% (Chyn, 2018).

Finally, a third source of difference could be nonlinearities in neighborhood effects since poorer families might be affected differently than richer families. This has been suggested by Chyn (2018) when comparing his setting with the MTO setting and by van Dijk (2019) in the context of public housing in Denmark.[57] Our results confirm this as the distribution of displacement effects by municipalities of origin is not uniform: Some children improve their earnings and some children are negative impacted. And it is not unexpected that children from the richest municipality at the time have one of the most negative impacts on earnings.[58]

8.2 Total earnings lost due to displacement

We use the age estimates on earnings presented in Figure II to calculate the present value of the loss of earnings as a consequence of displacement. Assuming the effects are constant between ages 21 to 25 and after 55 up to 60, and using an annual discount rate of 4%, the average child in our sample loses CLP$4.9 million by the age of 45 (relative to a non-displaced child). This is equivalent to US$7,000, and the amount is comparable to the cost of the housing unit received by a family through the Program for Urban Marginality. In aggregate terms, the total loss for children is equivalent to the construction of 12 subway stations or the maintenance of 300

[56] Corresponding changes in earnings by age groups are –9.9%, –9%, –12.2%, and –6.1%. In all cases, we divide by –9.5% to compute the elasticity.

[57] This is confirmed by our estimated elasticities. The implied elasticity in Chyn (2018) is bigger than the elasticity in Chetty et al. (2016). The percentage change in earnings is very similar in both studies, but in the first case, families experienced a smaller change in neighborhood quality as measured by the poverty rate. See Table A.16.

[58] Children from the municipality of Las Condes in Figure IV.

primary schools per year.[59] We consider this estimate to a be lower bound because it does not account for the direct effect of displacement on schooling and its externalities, such as increased criminal activity.

9. Conclusions and Policy Alternatives

This paper presents new evidence on the long-term consequences of being displaced and growing up in a low-quality neighborhood. In our setting, families did not choose their final locations, which facilitates us to disentangle the mechanisms that mediate the displacement effect as a function of place. We find evidence that social capital and segregation are important determinants of neighborhood effects and can have long-lasting consequences. The families in the displaced group are more likely to be isolated for at least 20 years after the end of the program; many of them remain in their assigned locations, and current residents are more likely to report lower levels of trust in their neighbors.

As our results show that displacement negatively affected children because their new neighborhoods were of low quality,[60] one policy alternative to displacing families to the periphery would be to provide housing on-site, which has been proposed by the World Bank and the United Nations in recent years (UN-Habitat, 2020). However, this is not always feasible for multiple reasons, such as high urban density that impedes public housing construction, the high price of land, or the impossibility of providing services on-site (running water, electricity, sewage). Under these circumstances, one option would be to compensate families monetarily for displacement, as proposed by Lall et al. (2006).

However, this paper does not estimate the effect of the program but instead the effect of the forced move as a function of place. Also, it might be difficult to assess compensation amounts, and this type of compensation may not solve poor households' problems if it does not translate into access to services or less segregation.[61] Thus, if displacement is the only solution, one option is to provide families with the necessary public services they need to foster their economic development, such as schools, health care centers, and access to public transportation. This means that to effectively foster families' and children's development,

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

[60] While this paper studies the long-term effects of neighborhoods on children's economic outcomes, a valid question is whether the program was good or bad for families. To answer this question, in addition to estimating a displacement effect, we would need to know the effect of slum upgrading on children. Unfortunately, the nature of our data does not allow us to answer this question because our comparison group was also provided with housing. Moreover, administrative data for slum dwellers are not available for the 1980s because slums were not administrative units.

[61] Dasgupta and Lall (2009) discuss several reasons for this, such as that poor populations may face significant problems of social cohesion, low levels of empowerment, and fewer social networks, which may translate into more difficulties for them in coordinating the provision of public services. This is consistent with our results for decreased social capital.

displacement should be accompanied by the provision of public services that counteract the negative effects of disruption.[62]

Another policy alternative is to allow communities to participate in the eviction processes and families to choose their final destination. Policy advocates argue that successful eviction processes should include families in the process.[63] Under the hypothesis that families have more information and greater incentives to find a proper destination (and no information frictions), we could expect that a voluntary move is not as negative as a forced displacement and could potentially lead to increased social capital in new locations (as our results show decreased social capital in forced destination locations). Thus, more research should be devoted to these questions.

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

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

[63] See research on this matter supported by the World Bank here.

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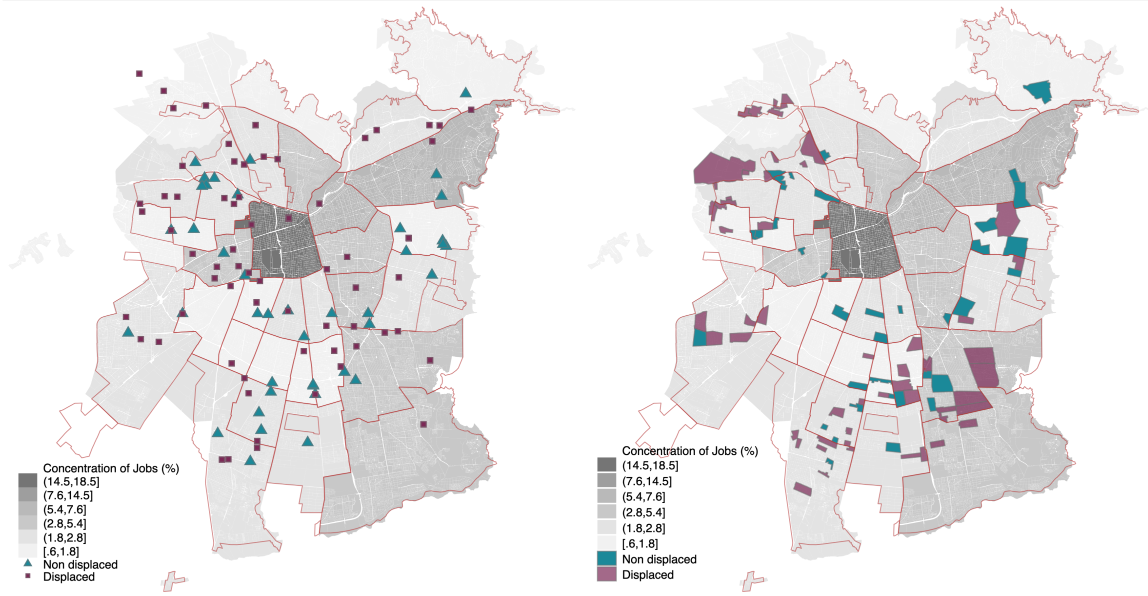
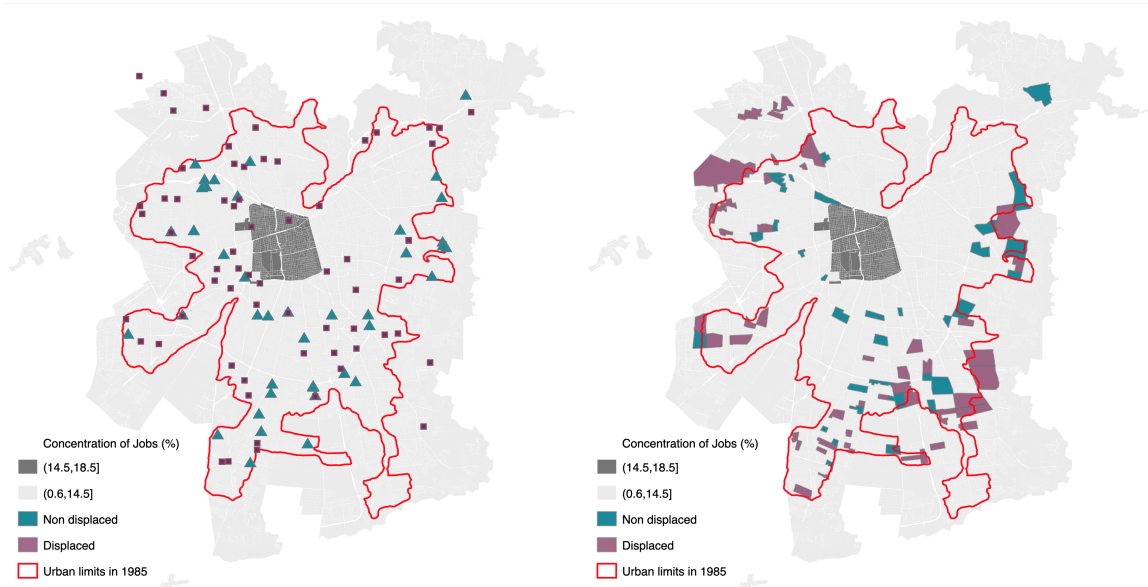
Figure I: Eviction policies 1979–1985: Location of families living in slums



(a) Slum location before 1979

(b) Public housing projects after 1985



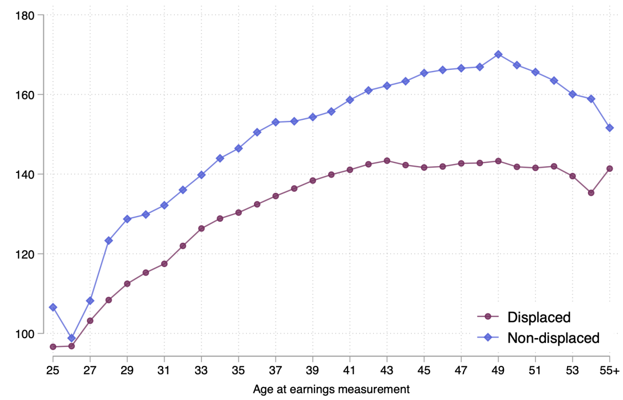
(c) Slum location before 1979

(d) Public housing projects after 1985

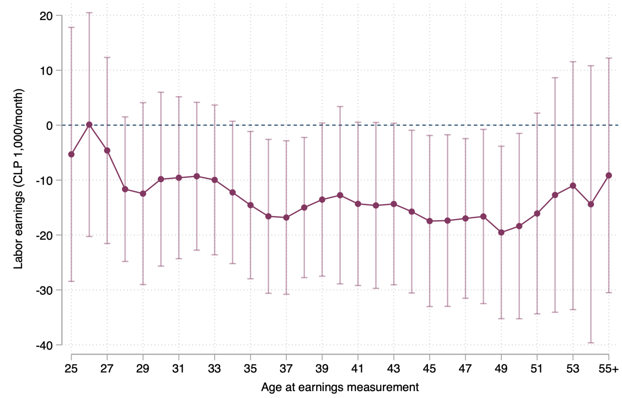
Notes: Red lines represent the urban limits of Greater Santiago and grey lines its municipalities. Municipalities are colored in gray scale to depict the concentration of jobs across the city. These figures show the change in the location of families living in slums in 1979 (panels (a) and (c)) and their final destination in 1985 (panels (b) and (d)). Purple squares represent families living in slums that were moved out from their original location to a new neighborhood; blue triangles represent the families in slums that were not evicted but received a housing unit in their original location. The figures also show how the dispersion of the location of these families decreases and how they are relocated to the periphery of the city after the policy. For context, consider that the richest municipalities of Santiago at that time (and today) are the ones located in the northeast of this map and the poorer municipalities are located in the south and northwest of the city, which is exactly where the new public housing projects were built. The data to construct this map come from MINVU (1979), Molina (1986), FLACSO (1982, 1986), and the population censuses of 1982 and 1992.

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

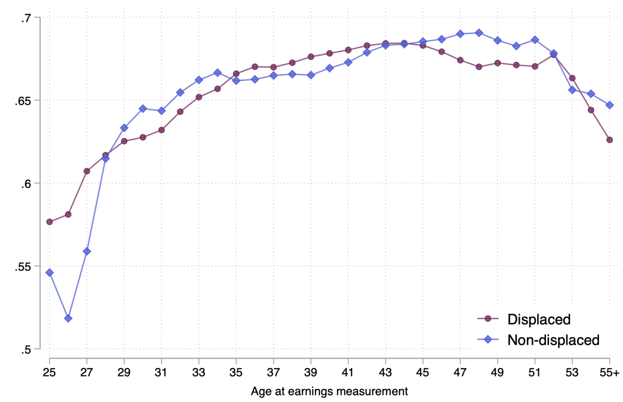
(a) Labor income trajectories (CLP$1,000/month)



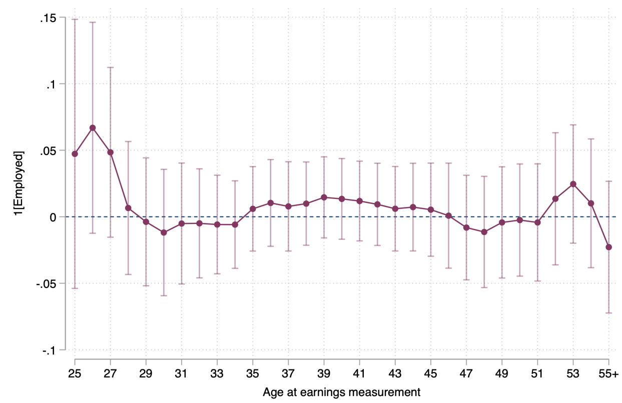
(b) Displacement effect on labor income



(c) Employment trajectories

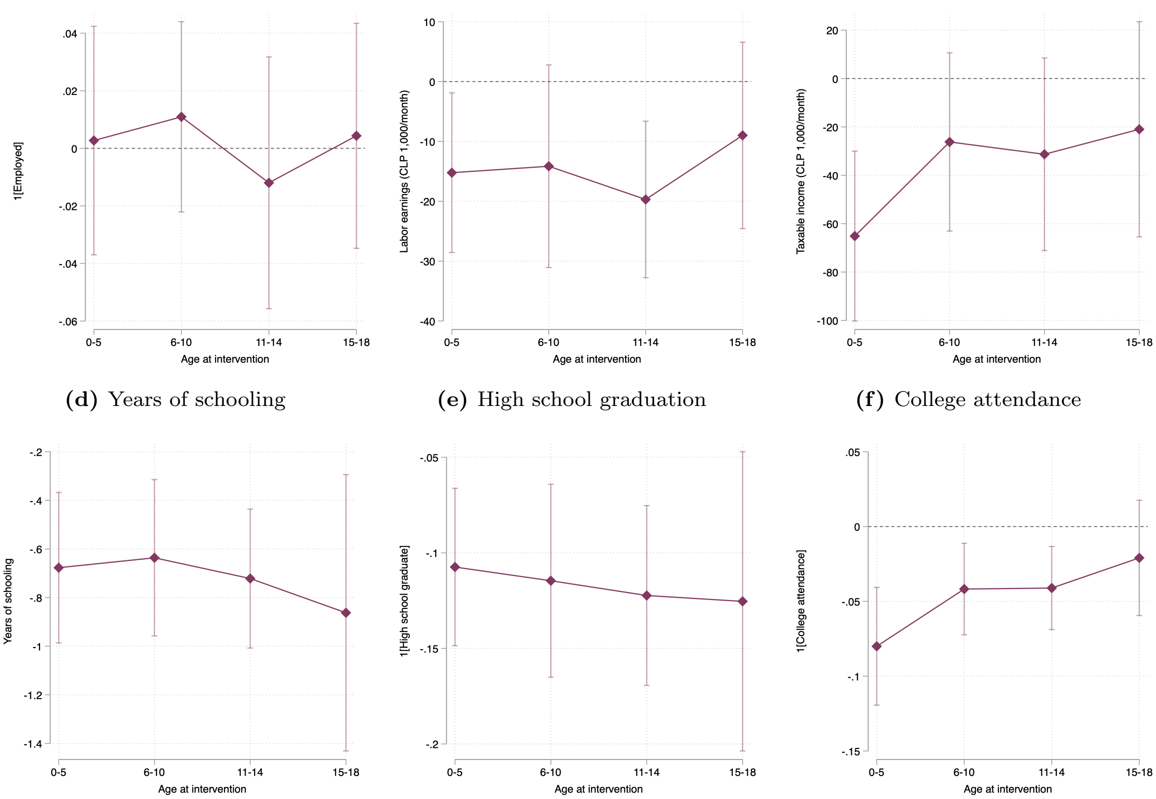


(d) Displacement effect on employment



Notes: Regressions for children aged 0 to 18 at baseline that are matched to the RSH data, and report non-missing schooling. Standard errors are clustered by municipality of origin. Baseline controls include the following: female, mother head of household, married head of household, head of households' marital status unknown, head of household's age, age of mother at birth, number of siblings, firstborn dummy, and year of birth fixed effects. Figures (a) and (c) plot the predicted trajectories for the displaced and non-displaced children between ages 25 to 55 from the previous regression. Figures (b) and (d) plot coefficients β_τ and their 95% confidence intervals from the regression $y_{it} = \sum_{\tau=25}^{55} \beta_\tau Displaced * 1[Age = \tau] + \sum_{\tau=25}^{55} \delta_\tau 1[Age] + \psi_o + X_{it}'\gamma + u_{it}$. Other outcomes can be found in A.5.

Figure III: Displacement effect on outcomes by age at intervention



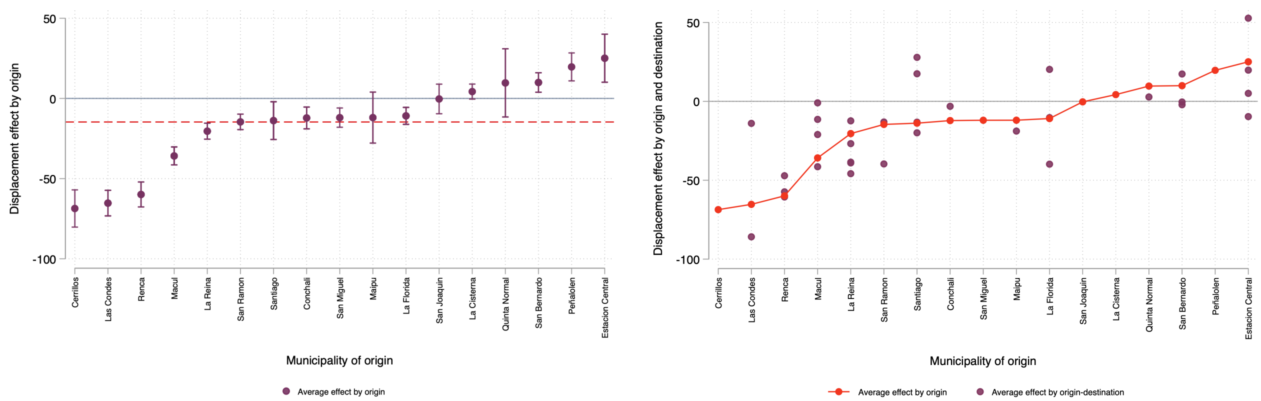
(a) Employment **(b)** Self-reported labor earnings **(c)** Taxable labor earnings

(d) Years of schooling **(e)** High school graduation **(f)** College attendance

Notes: Regressions for children aged 0 to 18 at baseline that are matched to the RSH or the GRIS. Standard errors are clustered by municipality of origin. Baseline controls include the following: female, mother head of household, married head of household, head of households' marital status unknown, head of household's age, age of mother at birth, number of siblings, firstborn dummy, and year of birth fixed effects. The figures plot coefficients β_τ and their 95% confidence intervals from regression $y_{it} = \sum_{\tau=1}^{4} \beta_\tau Displaced * 1[Age\ at\ baseline = \tau] + \sum_{\tau=1}^{4} \delta_\tau 1[Age\ at\ baseline = \tau] + \psi_o + X_{it}'\gamma + u_{it}$, where categories 1 to 4 are four age groups: 0–5, 6–10, 11–14, and 15–18. We plot the results on earnings from ages 0 to 18 in Figure A.6.

Figure IV: Distribution of displacement effect on earnings by municipalities

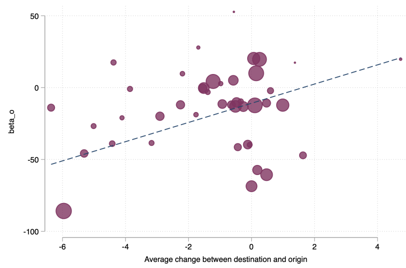
(a) By municipality of origin **(b)** By municipality of origin and destination



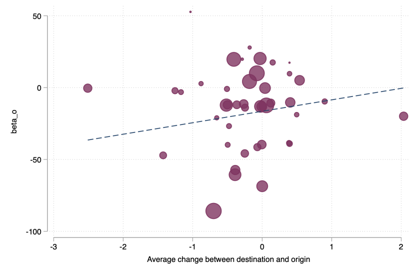
Notes: The figure shows regressions stratified by municipality of origin (a), and origin and destination (b). The sample includes children who were 0 to 18 years old at the time of the intervention, matched to the RSH, and from municipalities with both displaced and non-displaced populations. The number of clusters of origin is 17, and the number of clusters by origin and destination is 30. Regressions correspond to (a) $y_{it} = \sum_{o=1} \beta_o Displaced_{s\{i\}} * 1[Origin = o] + X'_{io}\theta + \varepsilon_{it}$ and (b) $y_{it} = \sum_{o=1,d=1} \beta_{od} Displaced_{s\{i\}} * 1[Origin = o, Destination = d] + X'_{io}\theta + \varepsilon_{it}$.

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

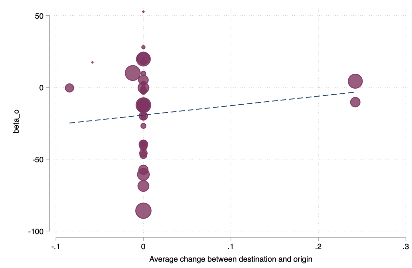
(a) Δ population's schooling



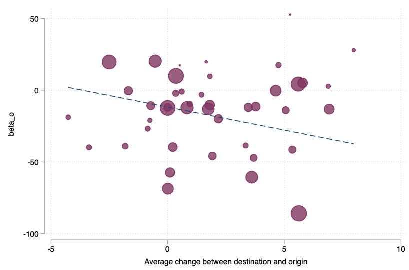
(b) Δ number of schools per student



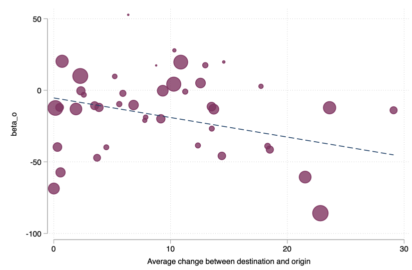
(c) Δ number of hospitals per household



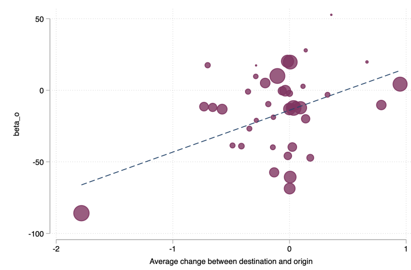
(d) Δ distance to subway



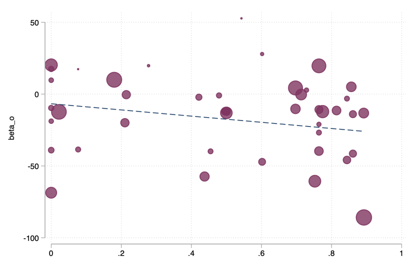
(e) Distance from origin



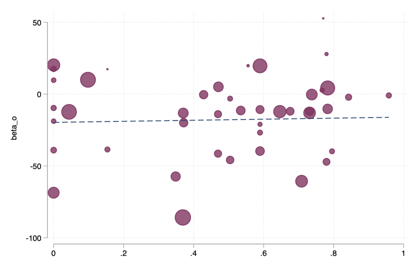
(f) Δ property prices



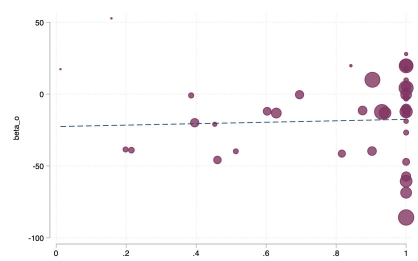
(g) Neighborhood fragmentation



(h) Neighborhood polarization



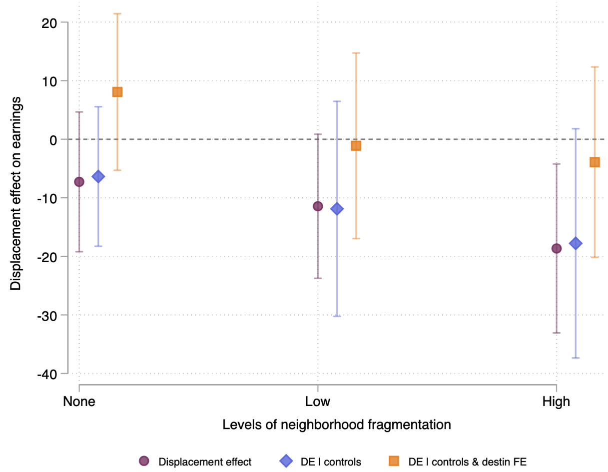
(i) Share of original community (slum)



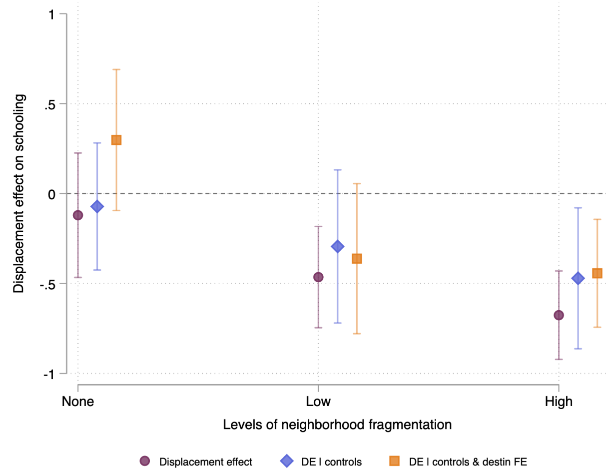
Notes: The figures plot displacement coefficients on labor income stratified by municipality of origin and destination (Figure IV (b)), against average changes in location attributes by municipality of origin. Regressions for children who were 0 to 18 years old at baseline that are matched to the RSH data that report non-missing schooling, from municipalities with displaced and non-displaced populations. The number of clusters of origin and destination is 42. Controls include the following: female, mother head of household, married head of household, number of siblings, firstborn dummy, head of household's marital status unknown, head of households' age, and year of birth fixed effects. Coefficients β_{od} are weighted by the number of observations in each cell. Figure A.8 repeats this exercise for years of schooling. See Data Appendix for variable definitions.

Figure VI: Displacement effect by levels of neighborhood fragmentation

(a) Earnings



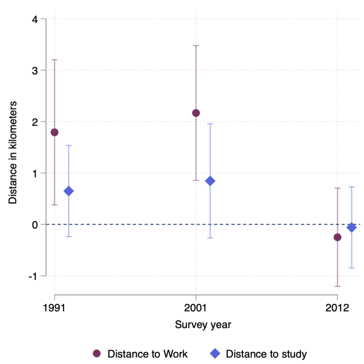
(b) Years of education



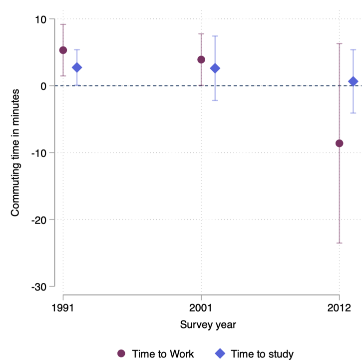
Notes: Regressions for children 0 to 18 years old at baseline that are matched with the RSH data. Standard errors are clustered by municipality of origin. Controls include the following: female, mother head of household, married head of household, number of siblings, firstborn dummy, head of household's marital status unknown, head of household's age, and year of birth fixed effects. Earnings regressions include semester fixed effects. The figures plot displacement coefficients and their 95% confidence intervals from an extended version of regression (1) in which the displacement dummy is stratified into three groups: no fragmentation (Findex=0), low fragmentation (Findex<0.5), and high fragmentation (Findex≥0.5). Each coefficient should be understood as the difference in outcomes between displaced children in the corresponding group relative to non-displaced children. "DE | controls & destin FE" stands for the displacement effect after controlling for municipality of destination fixed effects and other controls. Other controls include location attributes: property prices in 1985, number of schools per student, and population's schooling at destination neighborhoods.

Figure VII: Displacement effect on neighborhoods' accessibility across time

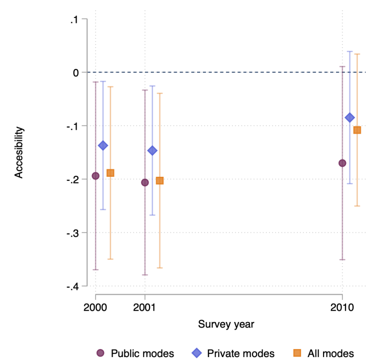
(a) Distance to place of work/study



(b) Commuting times



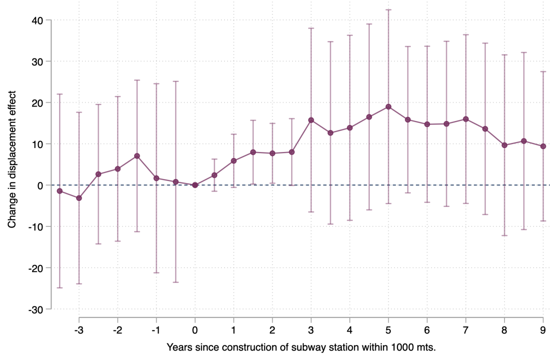
(c) Accessibility index



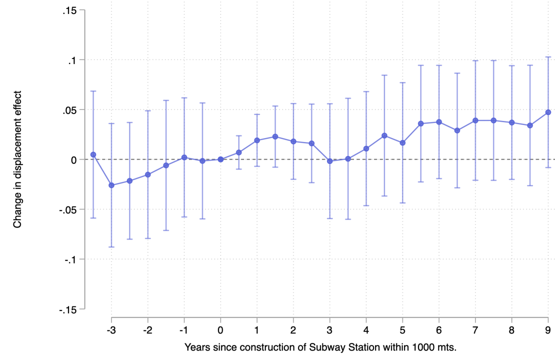
Notes: Regressions at the level of neighborhood, and include all neighborhoods in our sample. Standard errors are clustered by municipality of origin. The figures plot coefficients β_τ and their 95% confidence intervals from regression $y_{it} = \sum_{\tau=1991}^{2012} \beta_\tau Displaced * 1[Year = \tau] + \sum_{\tau=1991}^{2012} \delta_\tau 1[Year = \tau] + \psi_o + \gamma + u_{it}$, where i stands for neighborhood of destination. Each observation corresponds to the average by neighborhood. The data come from origin-destination surveys and Asahi et al. (2022).

Figure VIII: Rollout of subway stations between 2007 and 2019 and change in displacement effect within 1 km of a new subway station

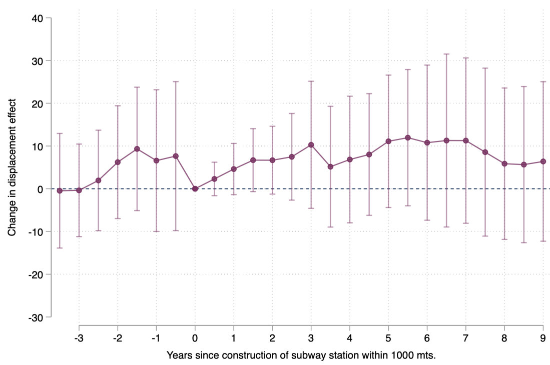
(a) Labor earnings



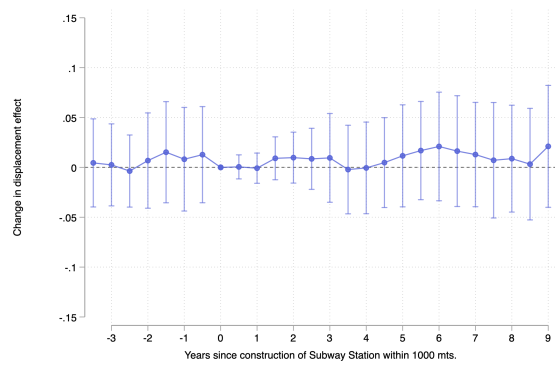
(b) Employment



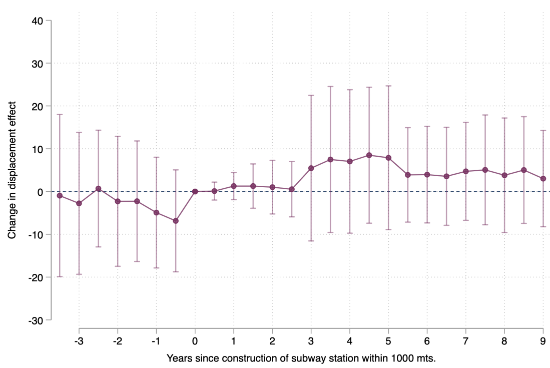
(c) Formal labor earnings



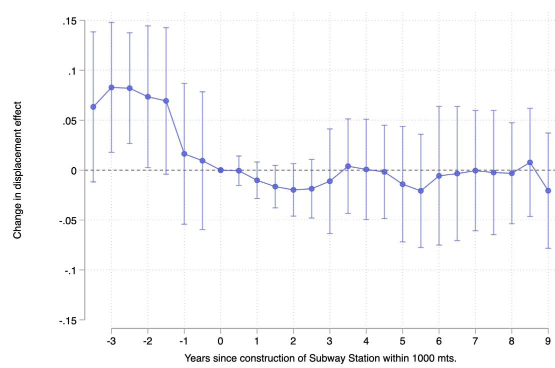
(d) Formal employment (contract)



(e) Informal labor earnings



(f) Temporary jobs



Notes: Regressions for children aged 0 to 18 at baseline that are matched to the RSH data, and that report non-missing schooling. Standard errors are clustered by municipality of origin. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include the following: female, mother head of household, married head of household, head of household's marital status unknown, head of household's age, age of mother at birth, number of siblings, birth order, and year of birth fixed effects. Each coefficient corresponds to $\gamma_{2\tau}$ in equation (2) from the text. Estimates for different distances to the subway are in Table A.15 and Figure A.10.

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

	Full sample	In RSH (2007-2019)	In GRIS (2016-2019)	P(in RSH)	P(in GRIS)
	(1)	(2)	(3)	(4)	(5)
Demographics at displacement					
Displaced	0.68	0.70	0.67	0.055***	-0.024***
	[0.47]	[0.46]	[0.47]	(0.010)	(0.005)
Female	0.50	0.54	0.45	0.127***	-0.152***
	[0.50]	[0.50]	[0.50]	(0.005)	(0.005)
Age	8.22	8.20	7.90	-0.001	-0.007***
	[4.85]	[4.86]	[4.78]	(0.001)	(0.001)
No. siblings	2.82	2.87	2.73	0.010***	-0.012***
	[1.79]	[1.81]	[1.73]	(0.002)	(0.001)
Firstborn	0.37	0.36	0.38	-0.017***	0.016**
	[0.48]	[0.48]	[0.48]	(0.004)	(0.006)
HH age	34.95	34.98	34.73	0.000	-0.000
	[7.03]	[7.05]	[6.95]	(0.001)	(0.001)
Female HH	0.31	0.31	0.29	-0.009	-0.027***
	[0.46]	[0.46]	[0.46]	(0.007)	(0.008)
Married HH	0.81	0.81	0.82	-0.005	0.010
	[0.40]	[0.40]	[0.39]	(0.009)	(0.014)
Marital status unknown	0.10	0.10	0.09	0.013	-0.033**
	[0.31]	[0.30]	[0.29]	(0.012)	(0.014)
Mother's age at birth	24.60	24.63	24.68	0.040***	0.019
	[5.65]	[5.67]	[5.64]	(0.011)	(0.012)
Mapuche HH	0.06	0.06	0.04	0.000	0.002**
	[0.23]	[0.23]	[0.20]	(0.001)	(0.001)
Demographics measured after 2007					
Died before 2007	0.01	0.00	0.00	-0.818***	-0.625***
	[0.07]	[0.00]	[0.00]	(0.009)	(0.016)
Mother's schooling	6.50	6.39	6.65		
	[3.52]	[3.49]	[3.54]		
Mother is in RSH	0.85	0.87	0.87		
	[0.35]	[0.34]	[0.34]		
Individuals	32,998	26,676	22,000	32,998	32,998
Matching rate		80.8%	66.7%	80.8%	66.7%

Notes: The table shows summary statistics for children aged 0 to 18 at baseline. Column (1) reports summary statistics for the full sample, column (2) for children matched at least once to the RSH, and column (3) for children matched at least once to the GRIS. Columns (4) and (5) estimate a linear regression of the probability of being found in the RSH or the GRIS (correspondingly), on a full set of demographics at baseline, treatment (displacement), probability of dying before 2007, year of intervention fixed effects, and municipality of origin fixed effects. Standard errors are clustered by municipality of origin in parentheses. 10%*, 5%**, 1%***. Standard deviations are in brackets. R^2 for regressions in (4) and (5) are 0.068 and 0.054 correspondingly.

Table II: Comparing displaced and non-displaced children aged 0 to 18 at baseline (year of intervention)

	All children 0 to 18		Children matched to RSH		Children matched to GRIS	
	Non-displaced mean	Difference (within municip.)	Non-displaced mean	Difference (within municip.)	Non-displaced mean	Difference (within municip.)
	(1)	(2)	(3)	(4)	(5)	(6)
Female	0.50	0.01	0.54	0.01	0.45	0.00
		(0.01)		(0.01)		(0.02)
Age	8.65	-0.32	8.71	-0.49	8.37	-0.35
		(0.28)		(0.30)		(0.28)
Firstborn	0.36	0.01	0.35	0.01	0.36	0.02
		(0.01)		(0.01)		(0.01)
No. siblings	2.73	0.13	2.81	0.11	2.66	0.08
		(0.13)		(0.12)		(0.12)
HH age	35.80	-0.58	35.92	-0.70	35.62	-0.66
		(0.44)		(0.46)		(0.43)
Mother's age at birth	25.02	-0.25	25.08	-0.27*	25.14	-0.34
		(0.15)		(0.15)		(0.17)
Female HH	0.31	-0.01	0.31	0.01	0.29	-0.001
		(0.02)		(0.03)		(0.02)
Married HH	0.84	-0.06***	0.85	-0.06***	0.86	-0.06***
		(0.02)		(0.02)		(0.02)
Widowed HH	0.01	0.001	0.01	-0.001	0.01	-0.001
		(0.003)		(0.003)		(0.003)
Mapuche HH	0.05	0.02**	0.05	0.02**	0.05	0.02**
		(0.01)		(0.01)		(0.01)
Mother's schooling	6.25	-0.27	6.08	-0.25	6.38	-0.33
		(0.21)		(0.22)		(0.24)
Individuals	32,998		26,675		22,000	
Families	13,447		12,294		11,638	
Slums	101		101		100	

Notes: Within difference corresponds to the coefficient *Displaced* in equation (1) conditional on the municipality of origin and year of intervention fixed effects. Mother's years of schooling is computed in the sample of mothers found in the RSH because of differential matching rates between displaced and non-displaced parents; the conditional difference is computed including an estimate of the probability of an individual's mother being found in the RSH (see Data Appendix for variable definitions). Standard errors are clustered by municipality of origin. 10%*, 5%**, 1%***.

Table III: Location attributes before and after intervention

Location attributes by census district	Non-displaced mean (1)	Displaced mean at origin (2)	Difference (within municip.) (3)	Displaced mean at destination (4)	Difference (within municip.) (5)
Panel A. Slum characteristics					
Area (hectares)	12.17	5.40	0.86 (0.81)		
No. families	292.98	247.53	46.60 (84.53)		
Military name	0.21	0.19	-0.03 (0.12)		
Distance to river (km)	1.74	1.40	-0.06 (0.32)		
No. slums	44	77	121		
Panel B. Location attributes					
Schooling HH	7.24	7.50	0.68 (0.72)	6.59	-0.69 (0.28)**
Unemployed HH	0.18	0.18	0.00 (0.02)	0.22	0.04 (0.01)***
HS dropout students	0.33	0.32	-0.01 (0.01)	0.36	0.04 (0.03)
Schools per census district	3.89	3.63	0.05 (0.78)	2.83	-1.31 (1.00)
Schools per 1,000 students	1.19	0.85	-0.44 (0.75)	0.64	-0.87 (0.86)
Pub. schools per 1,000 students	1.00	0.70	-0.43 (0.80)	0.58	-0.69 (0.85)
Priv. schools per 1,000 students	0.18	0.12	-0.03 (0.10)	0.06	-0.15 (0.11)
Family care centers per 1,000 HH	0.01	0.01	0.01 (0.01)	0.01	0.01 (0.01)
Hospitals per 1,000 HH	0.03	0.02	-0.01 (0.02)	0.01	-0.01 (0.02)
Distance to (closest) subway (km)	7.95	9.63	-0.37 (0.38)	9.84	2.49 (1.17)**
Commuting to work (min)[a]	42.25	42.38	0.13 (0.80)	47.47	5.06 (2.14)**
Commuting to study (min)[a]	32.92	32.94	0.02 (0.60)	32.82	0.64 (0.79)
Property prices[b]	14.81	14.90	0.06 (0.11)	14.69	-0.11 (0.14)
Neighborhood fragmentation	0.00	0.00	-	0.46	0.41 (0.05)***
Observations			160		160
No. slums			124		124
No. new projects			84		84

Notes: In panel A each observation is a slum, and in panel B each observation is a slum-neighborhood pair. Within difference corresponds to a regression of each location attribute on a displacement dummy conditional on municipality of origin. Standard errors are clustered by municipality of origin. 10%*, 5%**, 1%***. All location attributes correspond to population averages by census district level in 1982. [a]Measured as the weighted average in minutes that takes the average person in each municipality to go to work/study using public transportation; since these two variables are measured at the municipality level, the difference in column (3) does not include municipality fixed effects. [b]Only available for urban areas; the number of observations is 121.

Table IV: Displacement effect on labor income and employment

Panel A.	Outcome: Self-reported income (CLP$1,000/month)				
	(1)	(2)	(3)	(4)	(5)
Displaced	-15.753	-14.700	-14.978	-16.646	-16.028
	(6.835)**	(6.701)**	(4.969)***	(4.079)***	(4.626)***
	[1.541]***	[1.432]***	[1.182]***	[0.978]***	[1.044]***
Non-displaced mean	155.89	155.89	155.89	155.89	155.89
% Var. w.r.t. non-disp.	**-10.1**	**-9.4**	**-9.6**	**-10.5**	**-10.2**
R^2	0.018	0.125	0.126	0.126	0.127
Panel B.	Outcome: 1[Employed]				
Displaced	0.003	0.005	0.003	0.007	0.009
	(0.016)	(0.015)	(0.015)	(0.013)	(0.014)
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
Non-displaced mean	0.67	0.67	0.67	0.67	0.67
% Var. w.r.t. non-disp.	**0.4**	**0.7**	**0.3**	**0.9**	**1.0**
R^2	0.002	0.101	0.101	0.101	0.102
Municipality of origin FE	✓	✓	✓	✓	✓
Baseline controls		✓	✓	✓	✓
Slum characteristics			✓		✓
Location attributes origin				✓	✓
Observations	533,444	533,444	533,444	533,444	533,444
Individuals	26,675	26,675	26,675	26,675	26,675

Notes: Regressions for children aged 0 to 18 at baseline that are matched to the RSH data, and that report non-missing schooling. Standard errors are clustered by municipality of origin in parentheses (30 clusters), and Conley standard errors are in brackets. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects and semester fixed effects. Baseline controls include the following: female, mother head of household, married head of household, head of households' marital status unknown, head of household's age, age of mother at birth, number of siblings, firstborn dummy, and year of birth fixed effects. Slums' characteristics include area, number of families, military name, distance to river, and log of property prices at origin. Location attributes at origin include the population's schooling, number of schools, and distance to subway. The row labeled as "% Var. w.r.t. non-disp." stands for "percentage variation with respect to non-displaced mean."

Table V: Displacement effect on other labor market outcomes

Outcome	Labor income	Employed	Has a contract	Temp. worker	Formal income	Informal income	Taxable income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Displaced	-14.700	0.005	-0.040	0.037	-15.438	0.739	-36.738
	(6.701)**	(0.015)	(0.013)***	(0.018)*	(5.262)***	(2.039)	(15.706)**
Non-displaced mean	155.89	0.67	0.41	0.56	109.08	46.81	579.69
% Var. w.r.t. non-disp.	**-9.4**	**0.7**	**-9.8**	**6.6**	**-14.2**	**1.6**	**-6.3**
Observations	533,444	533,444	533,444	533,444	533,444	533,444	99,547
Individuals	26,675	26,675	26,675	26,675	26,675	26,675	22,000
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓
Baseline controls	✓	✓	✓	✓	✓	✓	✓

Notes: Regressions for children aged 0 to 18 at baseline that are matched to the RSH data that report non-missing schooling. Standard errors are clustered by municipality of origin (30 clusters). 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects and semester fixed effects. Baseline controls include the following: female, mother head of household, married head of household, head of households' marital status unknown, head of household's age, age of mother at birth, number of siblings, firstborn dummy, and year of birth fixed effects. The row labeled as "% Var. w.r.t. non-disp." stands for "percentage variation with respect to non-displaced mean."

Table VI: Displacement effect on schooling outcomes

Outcome	Years of schooling	1[HS graduate]	1[2y College attendance]	1[5y College attendance]
	(1)	(2)	(3)	(4)
Displaced	-0.683	-0.116	-0.031	-0.023
	(0.154)***	(0.021)***	(0.011)***	(0.007)***
Non-displaced mean	11.38	0.66	0.12	0.06
% Var. w.r.t. non-disp.	**-6.0**	**-17.6**	**-25.8**	**-38.3**
R^2	0.115	0.091	0.021	0.024
Municipality of origin FE	✓	✓	✓	✓
Baseline controls	✓	✓	✓	✓
Observations	26,675	26,675	26,675	26,675

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

Table VII: Displacement effect and municipality of destination

Outcome	Labor income		Employment		Years of schooling	
	(1)	(2)	(3)	(4)	(5)	(6)
Displaced	-14.700**	-1.205	0.005	0.036**	-0.683***	-0.460**
	(6.701)	(8.978)	(0.015)	(0.017)	(0.154)	(0.173)
R^2	0.125	0.127	0.101	0.102	0.115	0.19
Non-displaced mean	155.89	155.89	0.67	0.67	11.38	11.38
% Var. w.r.t. non-displaced	**-9.4**	**-0.8**	**0.7**	**5.4**	**-6.0**	**-4.0**
Municipality of origin FE	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
Municipality of destin. FE		✓		✓		✓
Observations	533,444	533,444	533,444	533,444	26,675	26,675

Notes: Regressions for children aged 0 to 18 that are matched to the RSH data, and report non-missing schooling. Standard errors are clustered by municipality of origin in parentheses. 10%*, 5%**, 1%***. Controls include the following: female, mother head of household, married head of household, head of household's age, number of siblings, birth order, and cohort fixed effects.

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

Outcome	Labor income						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Displaced	-14.700**	-3.122	-2.239	-2.707	-2.156	-2.043	-1.286
	(6.701)	(6.060)	(5.915)	(5.935)	(6.045)	(5.876)	(5.774)
* ΔHH years of schooling		2.938**	2.198	2.822**	2.872*	2.169	2.129
		(1.435)	(1.360)	(1.374)	(1.446)	(1.317)	(1.323)
* Fragmentation		-4.946	-5.954	-4.192	-6.918	-5.463	-7.432
		(4.330)	(4.495)	(4.333)	(4.534)	(5.544)	(4.817)
* Distance from origin		-0.514	-0.465*	-0.580*	-0.600*	-0.505*	-0.562*
		(0.308)	(0.259)	(0.311)	(0.336)	(0.266)	(0.282)
* ΔProperty prices			8.189***			7.769**	7.922***
			(3.090)			(3.077)	(2.845)
* Δ# schools/child				1.375**		0.799**	0.422
				(0.517)		(0.322)	(0.273)
* ΔDistance to subway					0.577		0.510
					(0.423)		(0.314)
R^2	0.125	0.126	0.126	0.126	0.126	0.126	0.126
Non-displaced mean	155.89	155.89	155.89	155.89	155.89	155.89	155.89
% Var. w.r.t. non-displaced	-9.4	-2.0	-1.4	-1.7	-1.4	-1.3	-0.8
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓
Observations	533,444	533,444	533,444	533,444	533,444	533,444	533,444

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

Table IX: Displacement and social capital

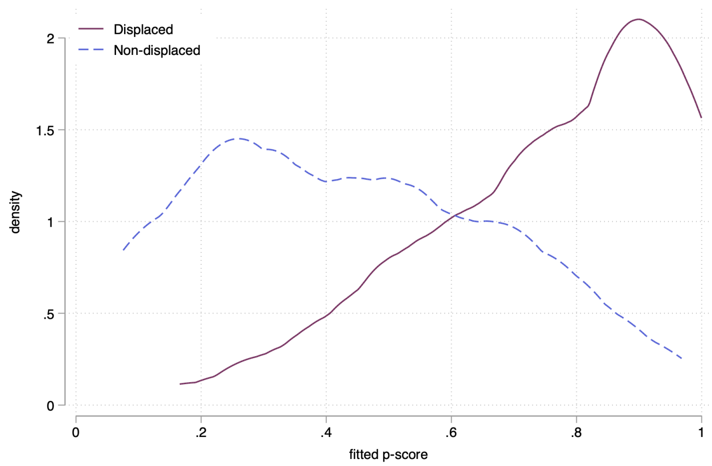
Panel A.	Outcome: Labor income						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Displaced	-9.565	-14.319**	-14.370*	-9.167	-11.462*	-8.320	-5.259
	(5.923)	(6.715)	(7.953)	(5.932)	(6.659)	(5.983)	(5.423)
Fragmentation index	-9.524**			-9.540**	-16.917**	-6.539	-8.209*
	(4.430)			(4.415)	(7.544)	(4.815)	(4.692)
Share of original network		3.500		3.612	2.619	7.418	7.289
		(6.763)		(7.199)	(7.486)	(8.273)	(8.783)
Polarization index			-0.665		12.419		
			(4.064)		(8.269)		
Project size (per 10 units)						-0.040	-0.028
						(0.034)	(0.037)
R^2	0.126	0.126	0.125	0.126	0.126	0.126	0.126
Non-displaced mean	155.89	155.89	155.89	155.89	155.89	155.89	155.89
Observations	533,444	533,444	533,444	533,444	533,444	533,444	533,444
Panel B.	Outcome: Years of schooling						
Displaced	-0.399**	-0.631***	-0.568***	-0.344*	-0.397**	-0.286	-0.266
	(0.172)	(0.156)	(0.170)	(0.186)	(0.166)	(0.208)	(0.211)
Fragmentation index	-0.527**			-0.530**	-0.701**	-0.435**	-0.446**
	(0.213)			(0.213)	(0.320)	(0.175)	(0.181)
Share of original network		0.471		0.477	0.456	0.717*	0.702
		(0.304)		(0.300)	(0.297)	(0.421)	(0.438)
Polarization index			-0.231		0.287		
			(0.210)		(0.357)		
Project size (per 10 units)						-0.001	-0.001
						(0.002)	(0.002)
R^2	0.116	0.115	0.116	0.116	0.117	0.117	0.117
Non-displaced mean	11.37	11.37	11.37	11.37	11.37	11.37	11.37
Observations	26,675	26,675	26,675	26,675	26,675	26,675	26,675
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓
Baseline controls	✓	✓	✓	✓	✓	✓	✓
Other neighborhood controls							✓

Notes: Regressions for children aged 0 to 18 at baseline that are matched to the RSH data, and report non-missing schooling. Standard errors are clustered by municipality of origin in parentheses. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include the following: female, mother head of household, married head of household, head of household's marital status unknown, age of mother at birth, head of household's age, number of siblings, birth order, and year of birth fixed effects. The average project size in the sample is 255 housing units.

Appendix and Supplementary Material

A Additional Figures and Tables

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

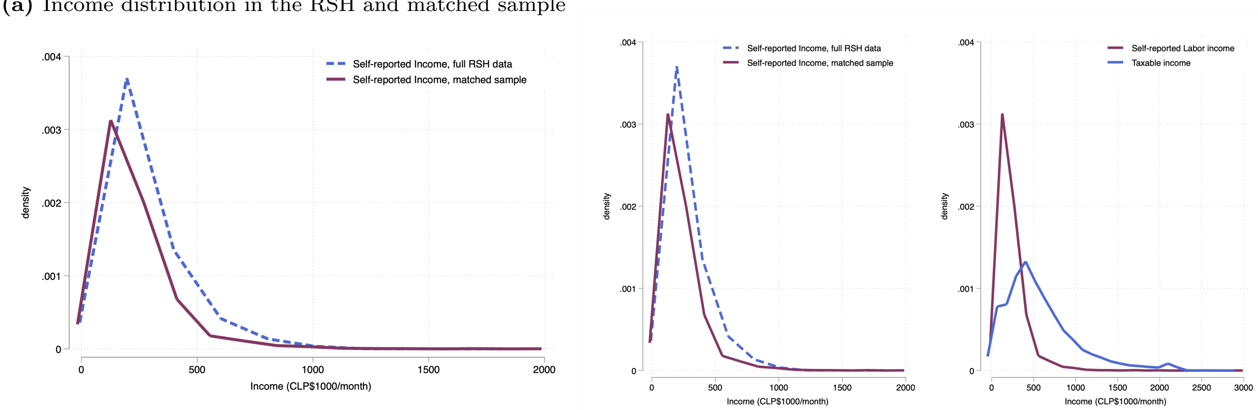


Notes: Figure plots the fitted values of a logit regression that includes controls from regression (4) in Table A.1 by treatment.

Figure A.2: Labor income distribution across different samples

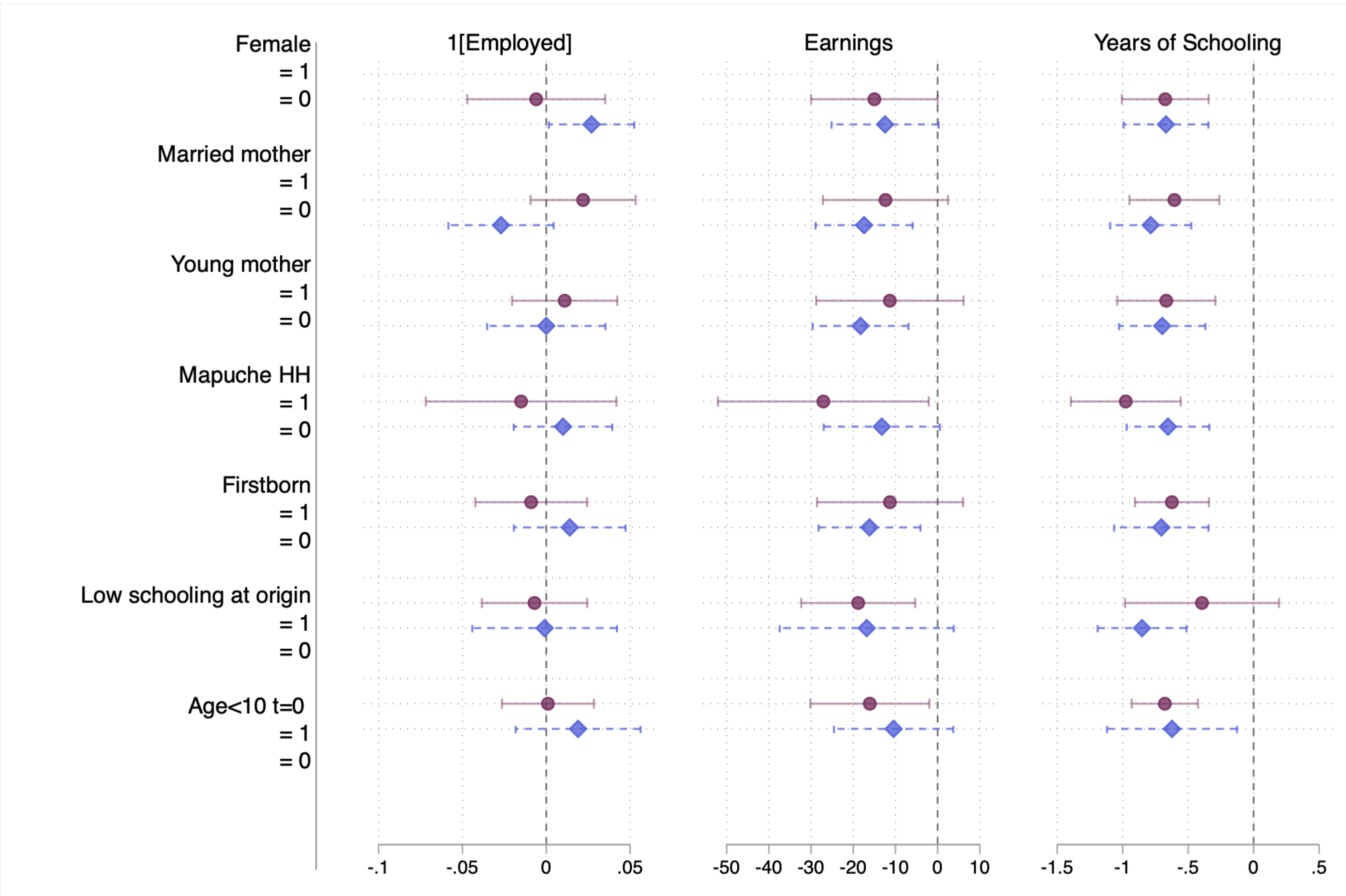
(a) Income distribution in the RSH and matched sample

(b) Income distribution in matched sample



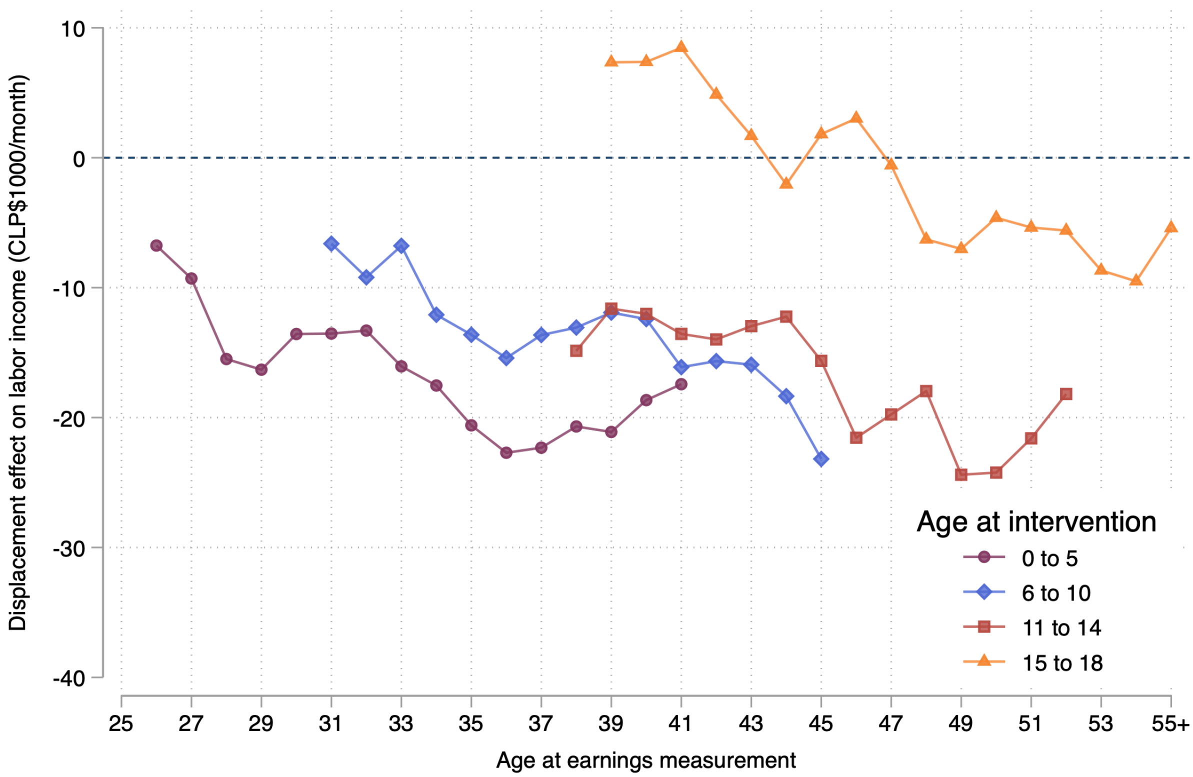
Notes: Income data for the year 2018. Matched sample stands for children aged 0 to 18 at baseline who are matched with the RSH data, and who are 18 or older in 2018. "Full RSH" corresponds to all individuals aged 21 to 60 in the RSH in year 2018 in Greater Santiago.

Figure A.3: Displacement effect by demographic groups on main outcomes



Notes: Regressions for children aged 0 to 18 that are matched to the RSH, and report non-missing schooling. Standard errors are clustered by municipality of origin. Controls include the following: female, mother head of household, single head of household, number of siblings, Mapuche lastname, cohort fixed effects, and time fixed effects. The figure plots the displacement coefficient and its 95% confidence interval resulting from estimating equation (1) stratified by demographic groups. Single mother is measured at the time of intervention, "young mother" stands for mothers younger than 25 (sample median) at the time their child is born, and "Low schooling" at origin stands for municipalities of origin where the population's average schooling is below the sample median.

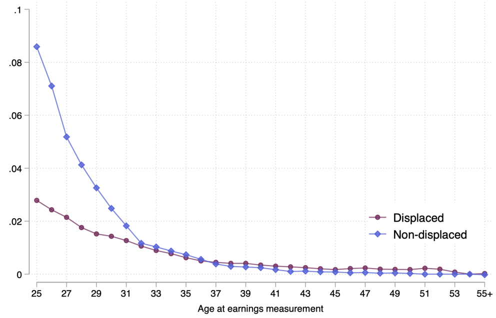
Figure A.4: Displacement effect on earnings by age at earnings measurement and cohort



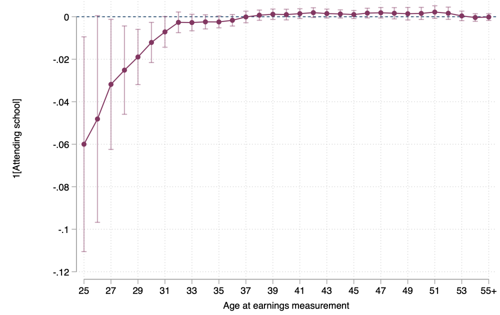
Notes: Regressions for children aged 0 to 18 at baseline that are matched to the RSH data, and report non-missing schooling. Standard errors clsutered by municipality of origin. Controls include the following: female, mother head of household, married head of household, number of siblings, firstborn dummy, head of household's marital status unknown, and year of birth fixed effects. Figure plots coefficients β_τ and their 95% confidence intervals from the regression: $y_{it} = \sum_{\tau=25}^{55} \beta_\tau Displaced * 1[Age = \tau] + \sum_{\tau=25}^{55} \delta_\tau 1[Age] + \psi_o + X_{it}'\gamma + u_{it}$, for each of the four groups by the age at intervention.

Figure A.5: Displacement effects on labor market outcomes by age at earnings measurement

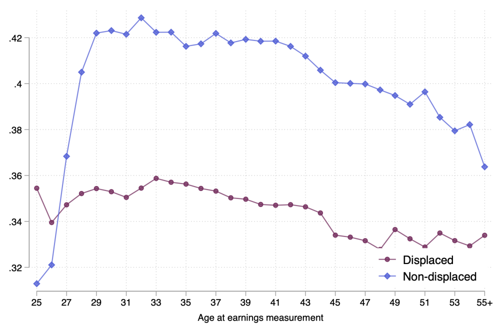
(a) Probability of studying



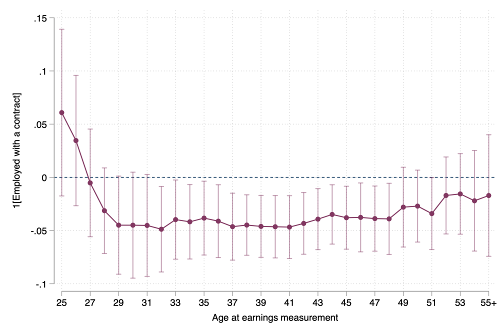
(b) Displacement effect on probability of studying



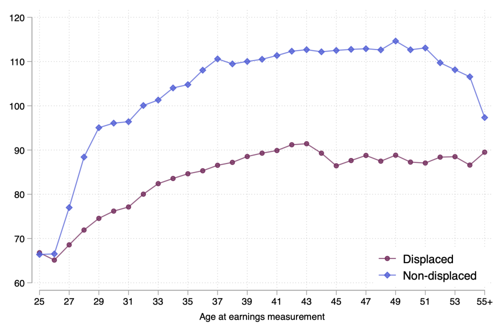
(c) Formal employment trajectories



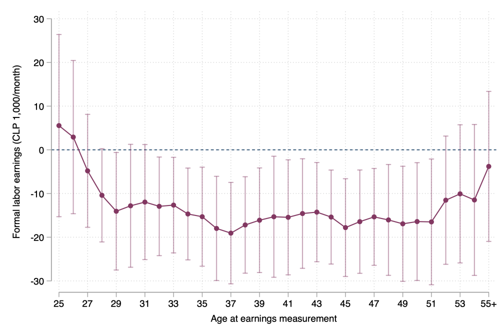
(d) Displacement effect on formal employment



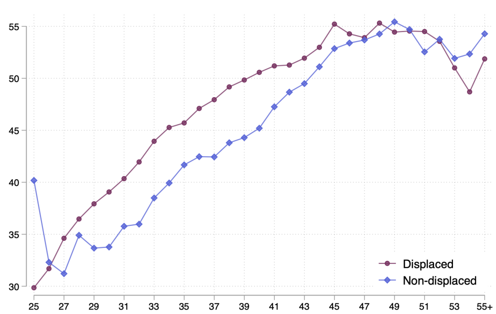
(e) Formal earnings trajectories (CLP$1,000/month)



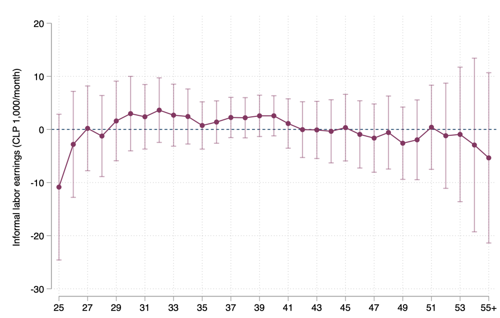
(f) Displacement effect on formal earnings



(g) Informal earnings trajectories (CLP$1,000/month)



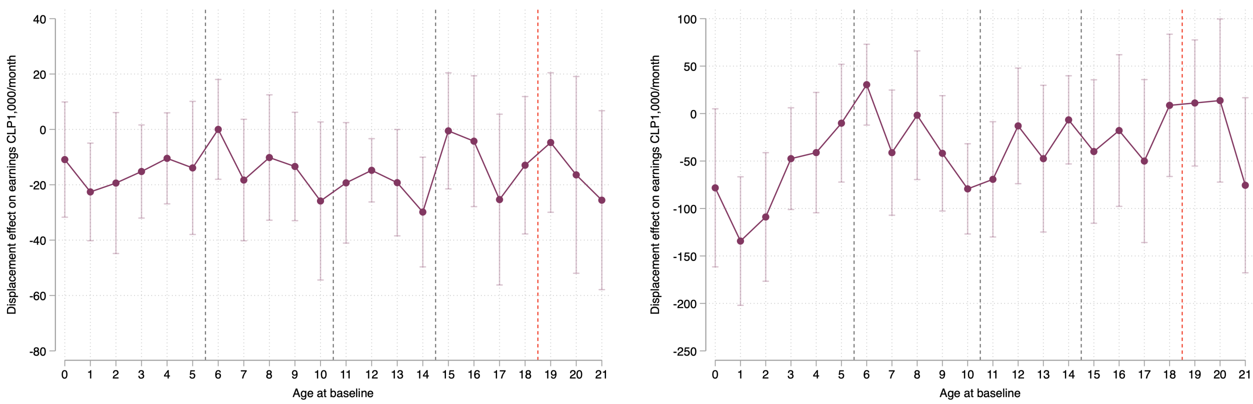
(h) Displacement effect on informal earnings



Notes: Regressions for children aged 0 to 18 at baseline that are matched to the RSH data, and report non-missing schooling. Standard errors clustered by municipality of origin. Controls include the following: female, mother head of household, married head of household, number of siblings, firstborn dummy, head of household's marital status unknown, and year of birth fixed effects. We estimate $y_{it} = \sum_{\tau=25}^{55} \beta_\tau Displaced * 1[Age = \tau] + \sum_{\tau=25}^{55} \delta_\tau 1[Age] + \psi_o + X_{it}'\gamma + u_{it}$. Figures (a), (c), (e) and (g) plot the predicted trajectories for the displaced and non-displaced children between ages 25 to 55. Figures (b), (d), (f), and (h) plot coefficients β_τ and their 95% confidence intervals.

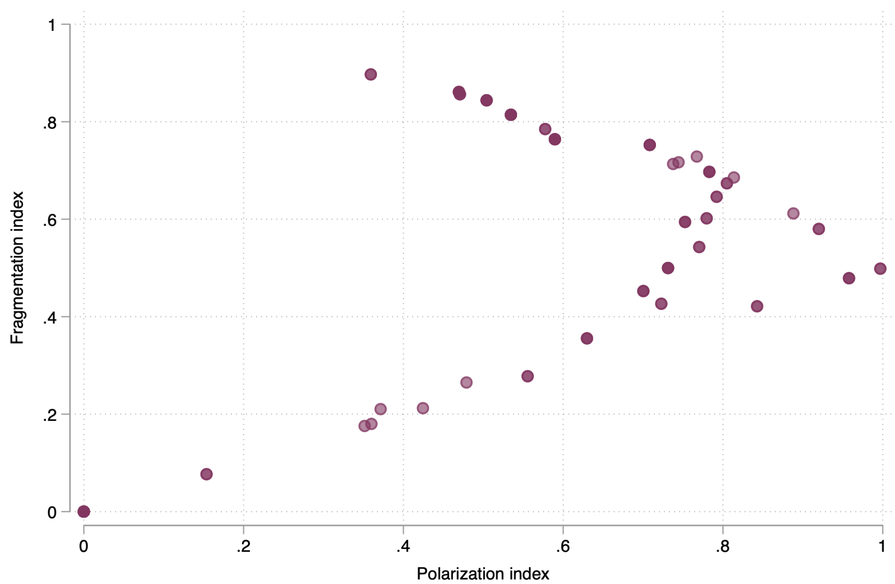
Figure A.6: Displacement effect by age at intervention and structural break

(a) All labor earnings (2007-2019) **(b)** Taxable earnings (2016-2019)



Notes: Regressions for children who are 0 to 21 years old the time of intervention and are matched with the RSH or the GRIS data. Standard errors clustered by municipality of origin. Controls include the following: female, mother head of household, firstborn dummy, single head of household, number of siblings, mapuche last-name, cohort fixed effects, and time fixed effects. The figure plots the displacement coefficient and its 95% confidence interval resulting from estimating equation (1) stratified by age at intervention. Dotted black vertical lines indicate that the p-value of the structural break test at the corresponding age is smaller than 0.1. Dotted red vertical lines is a reference for children older than 18 at intervention.

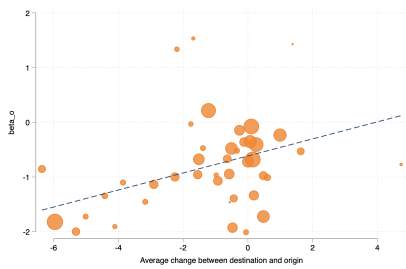
Figure A.7: Relationship between fragmentation and polarization for neighborhoods in the program



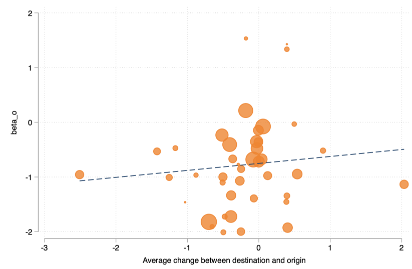
Notes: Each circle corresponds to a housing project (neighborhood of destination) in the Program for Urban Marginality between 1979 and 1985. Fragmentation and polarization measured based on data from Molina (1986) and MINVU (1979, 1984). See text and Data Appendix for variables definitions.

Figure A.8: Displacement effect on years of schooling by municipality of origin and changes in location attributes

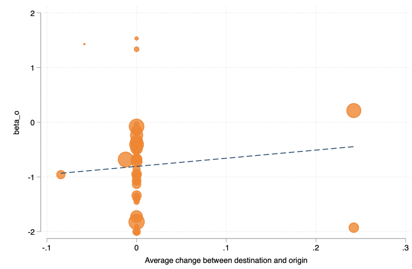
(a) Δ population's schooling



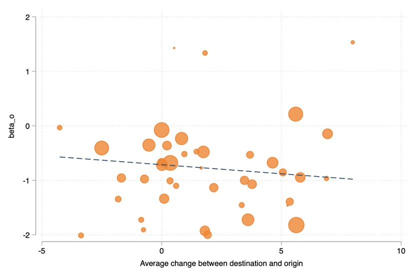
(b) Δ number of schools per student



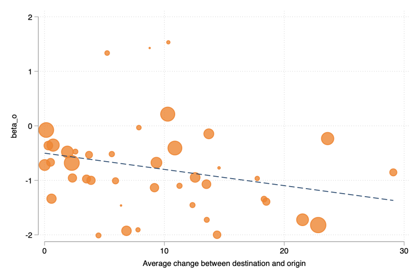
(c) Δ number of hospitals per household



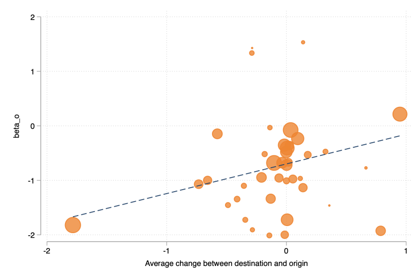
(d) Δ distance to subway



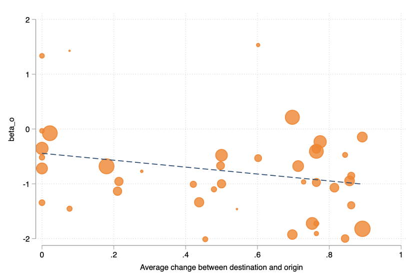
(e) Distance from origin



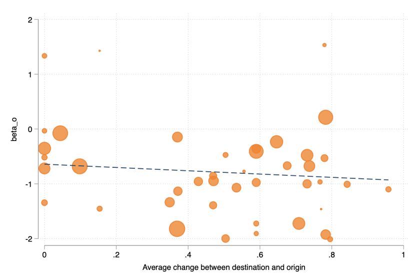
(f) Δ property prices



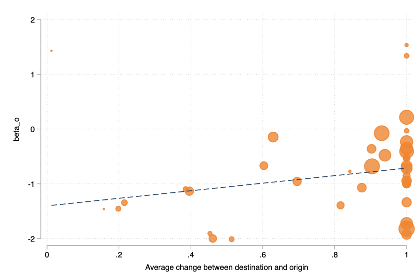
(g) Neighborhood fragmentation



(h) Neighborhood polarization

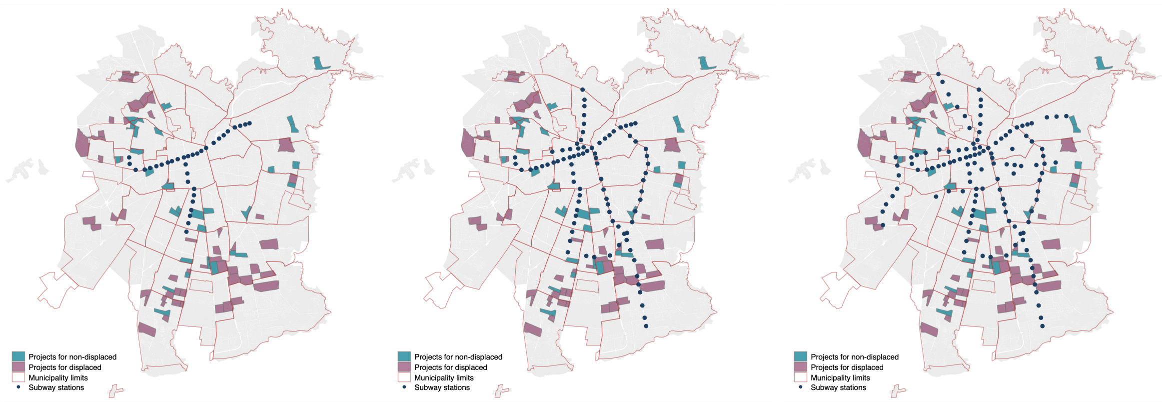


(i) Share of original community (slum)



Notes: The figures plot displacement coefficients on years of education stratified by municipality of origin and destination against average changes in location attributes. Coefficients are estimated using the following regression: $y_{it} = \sum_{o=1,d=1} \beta_o Displaced * 1[Origin = o, Destination = d] + X'_{iot}\gamma + u_{iodt}$, where o indexes the municipality of origin, and d municipality of destination for child i. Changes in attributes (x-axis) are computed as $\bar{\Delta}_{od} = \sum_{o=1,d=1}^{30} \Delta_{iod}$. Regressions for children aged 0 to 18 at baseline that are matched to the RSH data, and report non-missing schooling. Controls include the following: female, mother head of household, married head of household, number of siblings, firstborn dummy, head of household's marital status unknown, and year of birth fixed effects. Coefficients β_{od} are weighted by the number of observations in each cell. Figure V repeats the exercise for earnings.

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

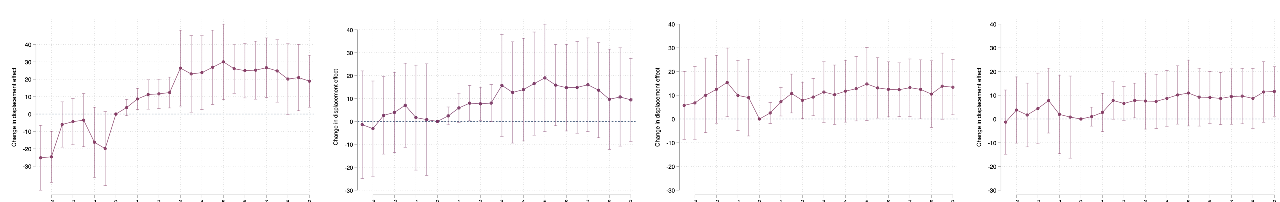


(a) Subway in 1980 **(b)** Subway in 2006 **(c)** Subway in 2019

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

Figure A.10: Roll out of subway stations between 2007 and 2019 and change in earnings

(a) Station is within 0.8 KM **(b)** Station is within 1 KM **(c)** Station is within 1.5 KM **(d)** Station is within 2 KM



Notes: Results of equation (2) for different values of λ. Children aged 0 to 18 at baseline that are matched to the RSH, and report non-missing schooling. Standard errors clustered by municipality of origin. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include the following: female, mother head of household, married head of household, head of household's marital status unknown, age of mother at birth, number of siblings, birth order, and year of birth fixed effects.

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

Outcome	Probability of displacement					
Sample	All slums in archives				Matched	Matched & urban
	(1)	(2)	(3)	(4)	(5)	(6)
Area (in hectares)	-0.016***	0.012	-0.015**	0.017	0.037*	0.038
	(0.005)	(0.017)	(0.006)	(0.017)	(0.021)	(0.026)
Families (/100)	0.024*	0.006	0.024*	0.004	-0.019	-0.021
	(0.012)	(0.020)	(0.012)	(0.019)	(0.025)	(0.028)
Distance to river	-0.073	-0.118**	-0.068	-0.100*	-0.125**	-0.118*
	(0.042)	(0.045)	(0.040)	(0.048)	(0.049)	(0.061)
Military name	-0.145	-0.047	-0.162	-0.065	-0.044	-0.007
	(0.106)	(0.113)	(0.110)	(0.108)	(0.121)	(0.126)
Log(property prices)	0.122	0.307**	0.155	0.290*	0.273	0.181
	(0.105)	(0.127)	(0.161)	(0.138)	(0.223)	(0.266)
Population's schooling			-0.006	0.016	0.011	0.022
			(0.034)	(0.036)	(0.043)	(0.051)
Schools/student			0.032	0.027	0.048	0.065
			(0.052)	(0.054)	(0.068)	(0.075)
Distance to subway			0.003	0.010**	0.021	0.015
			(0.006)	(0.005)	(0.016)	(0.016)
R^2	0.108	0.254	0.116	0.274	0.319	0.301
Sample mean	0.64	0.64	0.64	0.64	0.64	0.61
Observations	133	133	133	133	120	111
Municipality of origin FE		✓		✓	✓	✓

Notes: Regressions for the linear probability of displacement on slums' characteristics. Standard errors are clustered by municipality of origin in parenthesis. Because of the small number of observations, we use use the definitions of municipalities before 1980, which correspond to 19 unique urban municipalities of origin. 10%*, 5%**, 1%***. "Matched" stands for the slums in the final sample of children, and "urban" stands for slums in urban municipalities.

Table A.2: Summary statistics for children at the time of intervention by gender

	Women 0 to 18		Men 0 to 18	
	Non-displaced mean	Difference (within municip)	Non-displaced mean	Difference (within municip)
Age	8.60	-0.20	8.71	-0.45
		(0.30)		(0.30)
Firstborn	0.35	0.01	0.37	0.01
		(0.01)		(0.02)
# Siblings	2.74	0.17	2.72	0.09
		(0.12)		(0.15)
HH age	35.76	-0.55	35.83	-0.61
		(0.39)		(0.52)
Mother's age at birth	25.02	-0.31**	25.03	-0.14
		(0.14)		(0.22)
Female HH	0.31	-0.01	0.31	-0.003
		(0.03)		(0.03)
Married HH	0.85	-0.07***	0.84	-0.05***
		(0.02)		(0.01)
Widowed HH	0.01	0.00	0.01	0.00
		(0.004)		(0.003)
HH marital status unknown	0.08	0.01	0.08	0.02
		(0.01)		(0.02)
Mapuche HH	0.05	0.02**	0.05	0.02*
		(0.01)		(0.01)
Mother's schooling	6.24	-0.35	6.27	-0.30
		(0.26)		(0.22)
Individuals	16,565		16,433	

Notes: Within difference corresponds to the coefficient of *displaced* in equation (1) conditional on municipality of origin and year of intervention fixed effects. All children in matched sample from age 0 to 18 at baseline. Standard errors clustered by municipality of origin in parenthesis. 10%*, 5%**, 1%***.

Table A.3: Variance decomposition of outcomes within municipalities

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

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

Table A.4: Displacement effect on types of occupations/industries

Outcome	Occupation				Industry		
	Employer	Independent worker	Employee	Caregiver	Manufacture	Construction	Services
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Displaced	-0.004***	0.026***	-0.029*	0.003	0.008*	0.017***	-0.005
	(0.001)	(0.009)	(0.014)	(0.005)	(0.004)	(0.003)	(0.005)
Non-displaced mean	0.004	0.193	0.472	0.065	0.040	0.038	0.116
% Var. w.r.t. non-disp.	**-100**	**13.5**	**-6.1**	**4.6**	**21.1**	**44.7**	**-4.3**
R^2	0.003	0.019	0.094	0.058	0.035	0.093	0.077
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓	✓
Observations	533,444	533,444	533,444	533,444	533,444	533,444	533,444

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

Table A.5: Displacement effect on demographic outcomes

Outcome	Ever married	Age at first marriage	Teen parent	#Children	On welfare (2015-2019)	$Welfare (2015-2019)	Incarcerated (2000-2010)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Displaced	-0.007	-0.078	0.058	0.116	0.029	13.338	0.006
	(0.012)	(0.251)	(0.015)***	(0.031)***	(0.013)**	(5.085)**	(0.003)**
Non-displaced mean	0.66	24.67	0.34	2.42	0.27	58.29	0.021
% Var. w.r.t. non-disp.	**0.9**	**-3.9**	**16.8**	**4.1**	**10.7**	**22.9**	**28.57**
R^2	0.064	0.049	0.098	0.041	0.136	0.047	0.031
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓	✓
Observations	26,675	26,675	26,675	26,675	267,074	267,074	26,230
Individuals	26,675	26,675	26,675	26,675	25,433	25,433	26,230

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

Table A.6: Displacement effect on household characteristics

Outcome	Homeowner	Renter	Transfer	Squatter	Doubled-up	HH size	Parent in
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Displaced	-0.004	-0.019**	0.019	-0.001	-0.001	0.029	-0.010
	(0.019)	(0.007)	(0.018)	(0.002)	(0.015)	(0.062)	(0.016)
Non-displaced mean	0.51	0.12	0.35	0.01	0.29	3.87	0.20
% Var. w.r.t. non-disp.	**-0.8**	**-15.8**	**5.4**	**-10.0**	**-0.3**	**0.7**	**-5.0**
R^2	0.064	0.049	0.098	0.041	0.031	0.057	0.060
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓	✓
Observations	533,444	533,444	533,444	533,444	533,444	533,444	533,444
Individuals	26,675	26,675	26,675	26,675	26,675	26,675	26,675

Notes: Regressions for children aged 0 to 18 at baseline that are matched to the RSH data, and report non-missing schooling. Standard errors clsutered by municipality of origin in parenthesis. 10%*, 5%**, 1%***. All regressions control for year of intervention fixed effects. Baseline controls include the following: female, mother head of household, married head of household, head of household's marital status unknown, head of household's age, age of mother at birth, number of siblings, firstborn dummy, and year of birth fixed effects. Row labeled as *% Var. w.r.t. non-disp.* stands for "percentage variation with respect to non-displaced mean." "Transfer" means current house/apartment is not owned but it has been transferred from a third party. "Parent in" means at least one of the parents lives in the house.

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

Location Atributtes	Population's schooling (1)	Unempl. rate (2)	% Rural (3)	# Primary care cent./1,000HH (4)	# Hospitals/ 1,000HH (5)	# schools/ 1,000 stud. (6)	# Pub. schools/ 1,000 stud. (7)	# Priv. schools/ 1000 stud. (8)	Fragment. index (9)	Polarization index (10)	Prices (in logs) (11)	Distance from origin (12)
HH's age	0.000	-0.000	-0.002	-0.003*	-0.003*	-0.003	-0.002	-0.004	-0.001	-0.000	-0.001	-0.017**
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)	(0.004)	(0.001)	(0.001)	(0.001)	(0.008)
Female HH	-0.011	0.020	-0.002	0.019	0.011	0.040	0.041	0.024	-0.000	0.003	0.000	0.033
	(0.018)	(0.018)	(0.009)	(0.027)	(0.020)	(0.038)	(0.033)	(0.046)	(0.007)	(0.006)	(0.008)	(0.089)
Married HH	0.008	0.008	-0.019	0.016	-0.031	-0.035	-0.030	-0.040	-0.012	-0.010	0.001	-0.218*
	(0.017)	(0.020)	(0.020)	(0.021)	(0.026)	(0.030)	(0.028)	(0.028)	(0.008)	(0.007)	(0.005)	(0.125)
Widowed HH	0.054	-0.060	0.053	0.082	0.056	0.041	0.027	0.076**	0.008	0.014	0.023	0.282
	(0.056)	(0.049)	(0.054)	(0.068)	(0.057)	(0.026)	(0.027)	(0.032)	(0.015)	(0.017)	(0.016)	(0.345)
Marital status unknown	0.018	-0.023	-0.013	0.018	-0.034	-0.029	-0.028	-0.025	-0.007	-0.008	-0.000	-0.186
	(0.021)	(0.025)	(0.021)	(0.019)	(0.023)	(0.021)	(0.019)	(0.021)	(0.007)	(0.007)	(0.006)	(0.159)
# children	0.007	-0.006	-0.004	-0.015*	-0.009*	-0.005	-0.005	-0.005	-0.001	0.001	-0.002	-0.016
	(0.006)	(0.008)	(0.005)	(0.008)	(0.005)	(0.009)	(0.008)	(0.010)	(0.003)	(0.002)	(0.002)	(0.033)
Mapuche HH	0.009	0.006	-0.028*	-0.022	-0.056*	-0.070	-0.060	-0.080	0.002	0.001	-0.013*	-0.110
	(0.018)	(0.016)	(0.016)	(0.020)	(0.029)	(0.060)	(0.053)	(0.063)	(0.011)	(0.007)	(0.006)	(0.151)
R^2	0.600	0.588	0.775	0.571	0.666	0.397	0.500	0.325	0.523	0.559	0.721	0.766
Observations	11,327	11,327	11,327	11,327	11,327	11,327	11,327	11,327	11,327	11,327	11,327	11,327
Test of joint significance of baseline controls												
F	0.550	0.905	0.599	1.977	2.013	2.750	2.236	1.466	2.666	1.546	2.317	1.044
$p > F$	0.788	0.518	0.751	0.099	0.094	0.029	0.065	0.225	0.033	0.198	0.057	0.427
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year of intervention FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Standard errors clustered by municipality of origin. 10%*, 5%**, 1%***. Attributes in columns 1, 2 and 3 are measured at the census district level in 1982; schools, hospitals and subway are measured in 1985. Neighborhood fragmentation and neighborhood polarization are measured at the project level based on data from Molina (1986) and MINVU (1979) (See text for details).

Table A.8: Location attributes at origin by mixed and not mixed neighborhoods

Location Attributes by Census District	Non-displaced mean (1)	Displaced mixed mean at origin (2)	Displaced not-mixed mean at origin (3)	Difference (2)-(1) (within munic.) (4)	Difference (3)-(1) (within munic.) (5)
Schooling HH	7.24	7.54	7.27	0.75	0.23
				(0.79)	(0.79)
Unemployed HH	0.18	0.18	0.21	-0.01	0.01
				(0.02)	(0.03)
HS dropout students	0.33	0.32	0.32	-0.03	-0.03
				(0.03)	(0.03)
Schools per census district	3.89	3.57	3.93	-0.13	0.63
				(0.90)	(0.91)
Schools per 1,000 students	1.19	0.84	0.92	-0.54	0.12
				(0.86)	(1.74)
Pub. schools per 1,000 students	1.00	0.68	0.86	-0.53	0.17
				(0.93)	(1.61)
Priv. schools per 1,000 students	0.18	0.14	0.04	-0.03	-0.05
				(0.12)	(0.18)
Family care centers per 1,000 HH	0.01	0.01	0.01	0.00	0.01
				(0.01)	(0.02)
Hospitals per 1,000 HH	0.03	0.02	0.02	0.00	-0.03
				(0.02)	(0.03)
Distance to (closest) metro station in km	7.95	9.89	8.25	-0.64	1.32
				(0.38)	(1.18)
Commuting time to work (min)[a]	42.25	42.14	43.65	-0.11	1.40
				(0.84)	(0.83)
Commuting time to study (min)[a]	32.92	33.14	31.87	0.22	-1.05
				(0.61)	(0.87)
Observations	53	90	17	143	70
# Slums	47	66	17	113	62
# New projects	47	34	9	77	54

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

Table A.9: Displacement effect on schooling outcomes by age at intervention

Age group	0-5	6-10	11-14	15-18
	(1)	(2)	(3)	(4)
Panel A. Outcome: Years of schooling				
Displaced	-0.741	-0.644	-0.488	-0.822
	(0.155)***	(0.113)***	(0.176)***	(0.396)**
Non-displaced mean	11.89	11.57	11.11	10.32
% Var. w.r.t. non-disp.	**-6.2**	**-5.6**	**-4.4**	**-8.0**
R^2	0.090	0.086	0.105	0.089
Panel B. Outcome: High school graduate				
Displaced	-0.122	-0.122	-0.076	-0.130**
	(0.018)***	(0.021)***	(0.027)***	(0.053)***
Non-displaced mean	0.75	0.68	0.61	0.50
% Var. w.r.t. non-disp.	**-16.3**	**-17.9**	**-12.5**	**-0.26**
R^2	0.072	0.069	0.086	0.067
Panel C. Outcome: College attendance				
Displaced	-0.089	-0.039	-0.025	-0.027
	(0.022)***	(0.014)***	(0.016)	(0.019)
Non-displaced mean	0.23	0.17	0.14	0.10
% Var. w.r.t. non-disp.	**-38.7**	**-22.9**	**-17.9**	**-27.0**
R^2	0.041	0.031	0.034	0.035
Municipality of origin FE	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓
Observations (Individuals)	8,665	9,271	5,422	3,317

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

Table A.10: Displacement effect on labor market outcomes by gender

Outcome	Labor income (1)	Employed (2)	Has a contract (3)	Temp. worker (4)	Taxable income (5)	Formal income (6)	Informal income (7)
Panel A. Women							
Displaced	-15.479*	-0.010	-0.050**	0.037	-38.213**	-16.813**	1.334
	(7.845)	(0.021)	(0.020)	(0.022)	(16.997)	(7.264)	(2.040)
Non-displaced mean	109.69	0.55	0.32	0.64	523.09	77.65	32.04
% Variation w.r.t. non-disp.	-14.1	-1.8	-15.6	5.8	-7.3	-21.7	4.2
Observations	312,828	312,828	312,828	312,828	46,930	312,828	312,828
Individuals	14,480	14,480	14,480	14,480	8,626	14,480	14,480
Panel B. Men							
Displaced	-13.240**	0.026*	-0.025	0.035*	-34.423*	-13.261**	0.021
	(6.104)	(0.013)	(0.015)	(0.018)	(19.121)	(4.830)	(3.782)
Non-displaced mean	220.77	0.84	0.53	0.44	631.28	154.23	67.54
% Variation w.r.t. non-disp.	-6.0	3.1	-4.7	8.0	-5.5	-8.6	0.0
Observations	220,616	220,616	220,616	220,616	52,617	220,616	220,616
Individuals	12,195	12,195	12,195	12,195	9,264	12,195	12,195
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓	✓

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

Table A.11: Displacement effect on schooling outcomes by gender

Outcome	Years of schooling (1)	1[HS graduate] (2)	1[2y college] (3)	1[5y college] (4)
Panel A. Women				
Displaced	-0.679***	-0.118***	-0.025*	-0.022**
	(0.172)	(0.024)	(0.013)	(0.009)
Non-displaced mean	11.43	0.67	0.12	0.05
% Variation w.r.t. non-disp.	-5.9	-17.6	-20.8	-44.0
R^2	0.121	0.095	0.021	0.029
Individuals	14,480	14,480	14,480	14,480
Panel B. Men				
Displaced	-0.682***	-0.111***	-0.039**	-0.024***
	(0.159)	(0.022)	(0.012)	(0.007)
Non-displaced mean	11.32	0.65	0.12	0.06
% Variation w.r.t. non-disp.	-6.0	-17.1	-32.5	-40.0
R^2	0.118	0.094	0.029	0.026
Individuals	12,195	12,195	12,195	12,195
Municipality of origin FE	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓

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

Table A.12: Displacement effect and change in location attributes on main outcomes

Outcome	Employment (1)	Contract (2)	Temp. worker (3)	Years of schooling (4)
Displaced	0.022	-0.009	-0.020	-0.306*
	(0.012)	(0.015)	(0.013)	(0.165)
* ΔHH years of schooling	0.006**	0.007*	-0.011***	0.036
	(0.002)	(0.003)	(0.004)	(0.052)
* Fragmentation	-0.005	-0.016	0.049***	-0.500*
	(0.014)	(0.016)	(0.018)	(0.261)
* Distance from origin	0.000	-0.002	0.002**	-0.004
	(0.001)	(0.001)	(0.001)	(0.007)
* Δ property prices	0.026***	0.005	0.013	0.067
	(0.007)	(0.011)	(0.010)	(0.206)
* Δ# schools/child	0.000	-0.003	0.008	0.016
	(0.003)	(0.005)	(0.005)	(0.062)
R^2	0.101	0.066	0.076	0.116
Non-displaced mean	0.67	0.41	0.56	11.37
Municipality of origin FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
Observations	533,444	533,444	533,444	26,675

Notes: This table shows results for $Y_{it} = \alpha + \beta Displaced_{s\{i\}} + \gamma \Delta Attribute_{do} + \psi_o + \psi_\tau + X_i'\theta + \varepsilon_{it}$. All changes in attributes are measured at the census district level which corresponds to a smaller level of aggregation than municipalities. Regressions for children aged 0 to 18 that are matched to the RSH data, and report non-missing schooling. Standard errors clustered by municipality of origin in parenthesis. 10%*, 5%**, 1%***.

Table A.13: Displacement and social capital in the long run

Outcome	Chose neighborhood (1)	Conflictive neighbors (2)	Insecure neighborhood (3)	No trust in neighbors (4)	Trust own child with neighbor (5)	Divided neighborhood (6)
Displaced	-0.037	0.101	-0.099	0.046	-0.100**	0.209***
	(0.047)	(0.277)	(0.637)	(0.032)	(0.050)	(0.048)
R^2	0.160	0.062	0.071	0.111	0.161	0.224
Fragmentation Index	-0.027	0.877**	-0.442	0.083	-0.070	0.284***
	(0.060)	(0.420)	(0.975)	(0.051)	(0.067)	(0.069)
R^2	0.142	0.068	0.065	0.108	0.154	0.230
Non-displaced mean	0.780	1.586	1.652	0.116	0.341	0.296
Observations	1,184	1,184	1,184	1,184	1,184	1,184
# neighborhoods	43	43	43	43	43	43

Notes: Results of equation (1) on individuals' perceptions about their neighborhoods in 2012. Data come from Núñez et al. (2012). Each individual in this dataset is matched with a neighborhood in our sample, using current address. Standard errors clustered by municipality of residence in parenthesis. 10%*, 5%**, 1%***

Table A.14: Displacement effect on children's and parents' locations after 2015

Sample	Parents in RSH (2015-2019)				Children in RSH (2015-2019)			
Probability of living in	Same municipality (1)	Same neighborhood (2)	Distance from assigned neighb. (3)	Municipality of origin (4)	Same municipality (5)	Same neighborhood (6)	Distance from assigned neighb. (7)	Municipality of origin (8)
Displaced	-0.111	-0.184	1.329	-0.286***	-0.080	-0.121	1.560	-0.215
	(0.101)	(0.130)	(1.396)	(0.089)	(0.088)	(0.095)	(1.403)	(0.071)
Non-displaced mean	0.669	0.530	3.156	0.669	0.454	0.309	6.103	0.454
% Var. w.r.t. non-disp.	**-16.6**	**-34.7**	**42.1**	**-42.8**	**17.6**	**-39.2**	**25.6**	**-47.3**
R^2	0.196	0.228	0.145	0.452	0.001	0.008	0.002	0.304
Municipality of origin FE	✓	✓	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓	✓	✓
Observations	37,516	37,516	33,256	37,516	90,093	90,093	75,979	90,093

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

Table A.15: Displacement effect and subway rollout between 2007 and 2019

Outcome	Labor Earnings					
Distance to new station	0.7 km	0.8 km	1 km	1.2 km	1.5 km	2 km
	(1)	(2)	(3)	(4)	(5)	(6)
Displaced	-16.311*	-16.853**	-18.019**	-21.338**	-21.442**	-18.316**
	(8.020)	(8.020)	(7.620)	(8.986)	(9.148)	(7.854)
Subway station	-5.974	-4.886	-7.321	-10.409*	-10.662	-2.378
	(6.680)	(6.877)	(5.237)	(6.031)	(6.297)	(4.236)
Displaced*Subway	2.659	11.158*	13.657**	16.003**	14.532**	6.071
	(7.143)	(6.447)	(5.653)	(6.486)	(6.484)	(4.746)
Non-displaced mean	155.89	155.89	155.89	155.89	155.89	155.89
% Displaced affected by subway	2.2	11.9	13.7	26.6	36.98	53.13
% Non-displaced affected by subway	28.36	28.36	31.58	44.01	48.86	53.59
%Δ Displacement effect	16.3	66.2	75.8	75.0	67.8	33.1
R^2	0.126	0.126	0.126	0.126	0.126	0.126
Municipality of origin FE	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓

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

Table A.16: Comparison of earnings estimates across studies

Study	Setting	% Δ Earnings	% Δ Neighborhood Quality	$\lvert Elasticity \rvert$
	(1)	(2)	(3)	(4)
Chetty et al. (2016)[a]	MTO (children 7-13 in exp. group)	+14%	-34% (Poverty)	0.41
Chyn (2018)[b]	Public demolition in Chicago (children 7–18)	+16%	-22.2% (Poverty)	0.72
Barnhardt et al. (2016)c	Housing lottery Ahmedabad (adults in India)	-14.5%	-37.5% (Urbanicity)—-8.1% (Housing Value)	0.38–1.8
This paper[d]	Program for Urban Marginality (children 0–18 in Chile)	-9.4%	-9.5% (Schooling)	0.99

Notes: Results come from tables in each corresponding paper: [a]Tables 2 and 3; [b]Tables 2 and 3; [c]Tables 5 and 6; [d]Tables 3 and 4.

Table A.17: Comparison of schooling estimates across studies

Study	Setting	% Δ Years of Education	% Δ Neighborhood Quality	$\lvert Elasticity \rvert$
	(1)	(2)	(3)	(4)
Chetty et al. (2016)[a]	MTO (children 7-12 in Exp. group)	+15% (College Att.)	-34% (Poverty)	0.44
Chyn (2018)[b]	Public demolition in Chicago (children 7-18)	-8.1% (HS dropout)	-22.2% (Poverty)	0.36
		28% (College Att.)	-22.2% (Poverty)	1.26
Barnhardt et al. (2016)[c]	Housing lottery Ahmedabad (children in India)	-2.25% (schooling)	-37.5% (Urbanicity)—-8.1% (Housing Value)	0.06–0.27
This paper[d]	Program for Urban Marginality (children 0-18 in Chile)	-6.0% (schooling)	-9.5% (Schooling)	0.63
		-17.6% (HS grad)	-9.5% (Schooling)	1.85
		-32.8% (College att.)	-9.5% (Schooling)	3.45

Notes: Results come from tables in each corresponding paper: [a]Tables 2 and 3; [b]Tables 2 and 3; [c]Tables 5 and 6; [d]Tables 3 and 6.