Displacement and Mortality: Evidence from a Slum Clearance Program*

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Abstract

This paper examines the relationship between forced displacements and adult mortality. We use evidence from a slum clearance program implemented in Santiago, Chile, between 1979 and 1985, which mandated slum dwellers to relocate to public housing in low-income areas. Two-thirds of families were relocated to new housing projects on the periphery of the city, while the rest received housing at their initial location. We compare the outcomes of displaced and non-displaced adults from slums with the same probability of relocation and find that displacement increases mortality. Displaced individuals are 7% less likely to survive 40 years after the intervention and experience a reduction in longevity of 2.5 years. Among those who survive to the age of 65, displacement reduces long-term earnings by 18% and increases the likelihood of developing a disability. Mechanisms suggest that the mortality risk is higher for individuals relocated to areas with lower longevity, where access to formal jobs is more scarce, and for those whose networks were disrupted.

Keywords: slum clearance, mortality, segregation, neighborhood effects, forced relocation

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1 Introduction

Isolation and poverty correlate negatively with health outcomes, as individuals in poorer areas live shorter lives compared to adults in richer areas (Lleras-Muney et al., 2024). However, this relationship could be due to people sorting into lower-quality places rather than the causal effect of location on health (Deryugina and Molitor, 2020). In developing countries, these patterns can be exacerbated when the availability of neighborhoods for low-income individuals is limited and policy contributes to building housing in peripheral areas. A common policy to address the lack of housing for low-income individuals has been to build public housing on the outskirts of cities; however, the welfare consequences of these policies remain poorly understood. On the one hand, households obtain access to better-quality housing, but on the other, the locations come with worse access to labor markets and public services, thus, the total effect on health is ambiguous.

In this paper, we study the long-term effects of moving to a high-poverty area versus staying at central locations, on mortality. To study this question, we study a large-scale slum clearance and urban renewal program called the Program for Urban Marginality (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 formal 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 in the periphery of the city (i.e., were displaced).

We collect archival records of slum-dwelling families that we match to administrative records to create a novel dataset that follows individuals from non-displaced (redeveloped) and displaced (relocated) slums for 40 years after the policy ended. We take advantage of the fact that slum-dwelling families received a property deed associated with a unique national identifier. Using these identifiers, we can determine where families were sent, match individuals to death certificates, and then match individuals with data on employment, labor earnings, and retirement pensions. Our final sample contains

28,855 adults between the ages of 18 and 80, who were treated between 1979 and 1985, and whom we follow until 2023. Our sample represents 63% of the total number of individuals in the program in urban areas.

We use the same identification strategy proposed by Rojas-Ampuero and Carrera (2025). The authors use variation in the two treatments to estimate a displacement effect defined as the difference in health outcomes of individuals assigned to relocation (displaced) versus those re-housed on site (non-displaced). An important identification concern is that displaced and non-displaced slum residents may be different. The selection of slums for displacement or non-displacement was based on the feasibility of urban renewal on-site rather than on individual family characteristics. These determinants included slum attributes such as slum density, geographic location, and price of land. To address this concern, we leverage the program's selection rule and our rich dataset to estimate a policy function that estimates the probability of a slum being relocated versus redeveloped, and then compare displaced and non-displaced individuals from slums with the same probability of relocation. Conditional on the probability of a slum being relocated, we find no correlation between the selection of slums for displacement and families' demographic and socioeconomic characteristics, such as age, gender, family composition, or formal employment before the program's implementation.

Our results show that displacement increases the risk of mortality. Compared to the non-displaced, displaced individuals die 21% more year, which translate into a 7% lower survival rate 40 years after the end of the policy. The displacement effect is larger for men and older cohorts (older than 40 at baseline), compared to women or younger cohorts, respectively. We show that our estimates are unlikely to be driven by attrition from archival records or missing individuals that we cannot match to administrative records because of the lack of national identifiers.

We find that increases in mortality due to displacement are found in the entire age distribution, and are larger after the age of 50. We use these age estimates to compute a displacement effect on expected longevity, and our results indicate that on average, displaced individuals die 2.5 years younger.

We estimate displacement effects by causes of death and find that displacement increases the risk of dying in all four aggregate categories of causes of death we observe: cancer, cardiovascular disease, internal, and external causes of death. But the effects are especially large in the last two groups. While cancer and cardiovascular diseases are the main causes of death in the Chilean population during the last twenty years, displacement increases mortality due to causes that are less prominent in the population, such as respiratory disease and diabetes, which are likely to be related to socioeconomic determinants of health, and external causes of death, that include accidents and violent deaths, explain a fourth of the increased risk of dying among displaced men.

Displacement impacts individuals' employment and earnings. We find that displaced adults after retirement have 17% lower self-funded pensions compared to non-displaced retirees. These lower self-funded pensions indicate that displaced individuals were more likely to work informally or were unemployed for longer periods of time across their employment trajectories. However, we observe that subsidized pensions are larger among displaced retirees which compensate the lack of social security contributions among low-income individuals, and reduce the difference in pensions between treatments from -17% to -3.5%

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

We examine where families were relocated using archival and census records. The data confirm that displaced families were relocated to municipalities where the population have lower longevity in 1985 in more peripheral neighborhoods with higher unemployment rates and greater distances from the city center compared to non-displaced families, who remained in their original locations. Consequently, displaced families received homes of 30% lower value compared to non-displaced.

To study the mechanisms that determine the displacement effect by exploring which characteristics of the new locations explain the observed variation in adults' mortality. We find that positive changes in population longevity decrease the risk of mortality, but the effect is not large enough to explain the full displacement effect. Thus, we explore other changes in locations induced by the forced relocations. We find that a longer distance to the central business district (CBD) that proxies for access to employment, has a positive impacts on mortality, while keeping the slum network together in the destination neighborhood reduces the risk of dying. Finally, we find that access to more primary care health center in destination municipalities reduces mortality, but the effects are small and not statistically significant.

When we study changes in place attributes, we find that men and women's mortality respond differently to those changes. On the one hand, men's excess mortality among the displaced is associated with the lack of access to employment, measured by distance to the CBD. This result is consistent with the relationship between mortality and employment found in previous research (Schwandt and von Wachter, 2020). On the other hand, women's excess mortality due to relocation is more likely to be explained by changes in access to primary health care services, the disruption of slum networks, and lower longevity in destination areas. In this sense, women's mortality is more sensitive to local neighborhood changes, and men's mortality is to employment opportunities.

Finally, we study the labor market outcomes of adults that have not died by 2007, and who we match with administrative data between 2007 and 2023. We find that displaced individuals are more likely to be employed after the age of 65 compared to non-displaced adults, but their total income is lower. This is probably a form of compensation for their lower retirement pensions. We also find that the program had persistent effects on families' locations. Thirty years after the program ended, displaced adults remain in their municipality of assignment, they do not return to their municipality of origin, and their current neighborhoods are 2% poorer compared to those of non-displaced individuals.

This paper contributes to several strands of literature. First, it is related to the literature that studies policies that target slums (Marx et al., 2013). Due to the lack of data to follow individuals across time, most of prior research has focused on the effects of slum clearance on neighborhood quality (Michaels et al., 2021; Harari and Wong, 2021).[1] A few papers that study the effects of slum clearance policies on employment and health focus on improvements on site (Cattaneo et al., 2009; Galiani et al., 2017). Barnhardt et al. (2016), Picarelli (2019), and Kumar (2021) are the most similar to our paper as they compare movers and non-movers to estimate displacement effects, but their focus is on employment and earnings. This is the second paper that evaluates the effects of

[1] A large literature focuses on the effects of granting property rights on individuals labor market outcomes (Field, 2007; Franklin, 2020)

this slum program after Rojas-Ampuero and Carrera (2025), who evaluate effects on children. Instead, we study mortality and income effects associated to building public housing in low-quality neighborhoods in the long run.[2]

We also contribute to a recent literature that studies the role of place in health (Deryugina and Molitor, 2020; Finkelstein et al., 2021; Deryugina and Molitor, 2021). A recent paper by Currie et al. (2025) for Colombia studies the effect of moving to better neighborhoods on health. In this paper, we contribute to this literature by studying adults that are forced to move to worse and isolated neighborhoods. We study individuals by gender, age, and causes of death, and exploit the forced relocation nature of the program to study mechanisms. Our results suggest that different place characteristics impact men's and women's health differently.

Finally, this paper contributes to the literature on forced displacements. A large literature focuses on the labor market effects of displacement on both receiving populations and displaced individuals (e.g. Bauer et al., 2013; Becker et al., 2020; Nakamura et al., 2022).[3] A more recent literature has focused on forced moves and health outcomes, including Haukka et al. (2017), Bauer et al. (2019), and Valenzuela-Casasempere (2025). Most of these studies find negative effects on longevity. We contribute to this literature by studying the mechanisms that mediate our mortality effects, including employment and social networks.

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 mortality by age and causes of death. Section 6 discusses mechanisms, Section 7 presents long-term displacement effect on employment, and Section 8 concludes.

[2]Both Barnhardt et al. (2016) and Picarelli (2019) find a negative relationship between distance and adults' outcomes. This is also related to 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.

[3]See Becker and Ferrara (2019) for a full literature review on forced displacements.

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; INE, 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 participated in the program, accounting for 5% of the population of Greater Santiago. The average housing unit cost was US$10,148, and the program's average total annual cost was US$63 million, which corresponds to approximately 0.25% of the Chilean GDP in 1982.[8]

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

[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]Some slums families had received housing starting in 1977 but did not have the property on the new homes, they were renting. At the onset of the program in 1979 they were included in the group of families to become homeowners, thus we include them in our sample. But there were other evictions that occurred between 1976 and 1978, and were 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]Computation made by the authors based on average home value and subsidy from archival data. This number is similar to current expenditure in homeownership subsidies in Chile (see here).

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.[9]

The features of each intervention are as follows. Non-displaced families accounted for 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."[10] These new neighborhoods were provided with all of the basic services of a formal neighborhood (water, electricity, and sewage).

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. The land used by the slum was then cleared and used for a different purpose.[11] 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 limits.

Funding for the homes provided by the program came from a direct government subsidy that was designed to cover 75% of the cost of construction but capped at 200UF (inflation adjusted index).[12] That is, a family would receive a subsidy equal to the

[9] 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.

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

[11] 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.

[12] The average home value in our sample is 254UF equivalent to US$10,148 in 2018.

minimum between 200UF and 75% of the value of the new housing unit. The remaining amount corresponded to a copay that was paid in monthly installments to MINVU over a term of 12 years.[13] Although the design of the policy considered the previous rule for the subsidy, in our data we find evidence that suggests there was discretion: Some housing projects had a subsidy capped at 200UF, that was above the 75% value of the new housing unit.[14]

Displaced and non-displaced families received houses that were of the same quality and size. Figure B.1 left panel shows an example of a slum in Santiago, and the right panel shows two examples of the types of dwellings that families received in their destination neighborhoods, either a house (upper panel) or an apartment in a public housing building (lower panel). Slum-dwellers did not decide the type of dwelling, but they manifested to prefer houses over apartments, as they can be extended (Rodríguez and Icaza, 1998). While the houses received by both groups were very similar, the cost varied by location; the more peripheral and larger the project, the unit cost was cheaper. In our data we find that the housing units received by the displaced families are 13% lower in value compared to those for the non-displaced families.

Decisions regarding the program's implementation were made directly at the central government level by MINVU. Santiago lacked a citywide government; instead, there were 30 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 context, they could not oppose the policy. Instead, displaced families were assigned to destination locations based on the current availability of finished projects across the city. This also implied that, in some cases, displaced families of a single slum were assigned to more than one housing project.[15] Destination municipalities could not influence how

[13] In our data we observe that sometimes families pay over a term of 25 years, but their subsidies are lower.

[14] An example of this are slum dwellers that were moved from the Rio Mapocho riverbank in 1982 due to a flood. These families received a house with a value of 220UF, but a subsidy of 200UF, possibly due to the emergency situation associated to their displacement. However, we do not find systematic evidence of families' demographics predicting the subsidy amount nor the home value. See Table A.5.

[15] Housing projects were not planned specifically to house families of any given slum. We interviewed

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 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. The value of land 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 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 clear a slum and relocate its dwellers 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—originally located in Las Condes, a high-income municipality—were relocated to La Pintana and San Ramón, two low-income municipalities in the south of the city.[16]

Using data on slum characteristics collected by Morales and Rojas (1986) and from the MINVU's slum censuses, we find the same patterns established by previous researchers. We report means by intervention in columns (1) and (2) of Table 1, and column (3) reports the simple difference between treatments. Panel A shows that dis-

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.

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

placed slums are similar in size (# families), but they are denser as they house fewer families in smaller land areas. They are located in more elevated areas with higher slopes, are closer to rivers or canals, and consequently, have a higher risk of flooding. They are also closer to the central business district (CBD) by almost 1 km. Additionally, in Panel A we classify slums' names as either military related or not as a proxy for support for the dictatorial regime, finding that displaced slums are less likely to have a military-related name.[17]

In Panel B, we report attributes of the census districts where slums were originally located to proxy for neighborhood characteristics. We find that displaced slums are located in areas with higher average schooling, slightly higher property prices, but fewer schools.

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

After 1985, Aldunate et al. (1987) surveyed 592 displaced families, who reported that they thought their homes were better than their previous ones. However, they reported that the quality of their new neighborhoods was worse than the slums, citing fewer job market opportunities and limited access to transportation, education, and health care services. They also perceived their new neighborhoods as more dangerous and lacking public services (see Figure B.2 for a summary).

3 Data

We construct a novel dataset that tracks slum dwellers who become homeowners, their slum of origin and destination neighborhood, and then we match these individual records

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

to birth, marriage and death certificates.[18]

3.1 Archival data: Slums and homeowners

We digitize two slum censuses conducted by MINVU in 1979 and 1980 that contain information on slums' names, their locations, and destination projects. We classify each slum as displaced or non-displaced, and the neighborhood of destination of 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 treatment and 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.[19] These records correspond to the lists of homeowners and their spouses who received a property deed through the Program for Urban Marginality. We focus our data collection on households in Greater Santiago in municipalities with variation in treatment, that is, that we observe both displaced and non-displaced slums in each municipality.[20] We were able to find 19,365 unique households that were recipients of social housing from 14 different municipalities of origin. Based on the numbers reported by Molina (1986), approximately 27,500 families in the program received housing in the Greater Santiago area, thus, the 19,365 families in our sample represent 70.4% of the total number of recipients and correspond to 34,505 adults in 98 different slums.

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, and we match them to their slum of origin using the slum census of 1979. We lose households with a missing NID due to typing errors or outdated NID numbers that we could not validate using contemporaneous data.[21] Of the 19,365 records

[18] See Online Appendix in Rojas-Ampuero and Carrera (2025) for a detailed description of the data creation process and variables.

[19] 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.

[20] This means we exclude all rural municipalities, because most of them only received displaced families.

[21] To validate a NID we use data from Chilean electoral records in 2016, and children's birth certifi-

we found in the archives, 16,268 are households (31,869 adults) where at least one partner has a valid NID. This implies that our sample represents 59% of the total number of housing recipients.

In Appendix table C.1 we report characteristics of the individuals in the full sample of archival records (columns (1)–(4)), and the sample with non-missing NIDs (columns (5)–(8)). Using individuals' full names, we can identify gender, head of household (recipient of the property title), Mapuche last name (indigenous), and the number of partners per record. The table shows a larger proportion of women and Mapuche last names among the displaced families compared to the non-displaced (though small differences), and a slightly larger proportion of couples as opposed to only one person per record. Finally, the attrition rate due to missing NIDs is larger among non-displaced individuals. The sample with non-missing NIDs is balanced in gender, but includes more couples among the non-displaced. Interestingly, we do not lose entire slums from the sample. The selection that we observe is due to missing NIDs being more common among men, older people, or single individuals. Because all these variables correlate with mortality, in the robustness checks section, we will correct for this attrition and will show that our baseline results on mortality do not change.

3.2 Death and marriage certificates

We worked with Genealog Chile and web scraped birth, marriage, and death certificates for the dwellers in our sample with non-missing NIDs.[22] We used marriage certificates to find spouses that were not in the archival data, and to study marital status after the intervention. We also matched homeowners' archival data with their children using both NID and full names to compute the number of children per household.[23] The death certificates include date of death, municipality of death, and cause of death.

3.3 Earnings, employment, and pension data

In addition to death certificates, we match individuals to administrative data using NID numbers to three different data sources that contain labor market outcomes and

cates.

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

[23]The sample of 16,268 records with non-missing NIDs described in the previous paragraph was built using marriage certificates and children's birth certificates.

pensions.

Social Household Registry (RSH). The RSH (Registro Social de Hogares) is an information system managed by the Ministry of Social Development. The RSH is used to provide information on a family's needs and use of social and governmental benefits for income, housing, and education. Approximately 75% of all Chilean households voluntarily register to be in it. We have access to biannual data from June 2007 to December 2023 and observe self-reported income, employment status, informal employment, self-reported pensions, family composition and dwelling characteristics. A caveat of this dataset is that it only starts in 2007, thus, matching will include attrition directly related to treatment if displaced and non-displaced individuals die at different rates before the beginning of the sample.

Unemployment Insurance Records (AFC). The AFC is a monthly employer-employee dataset managed by the Ministry of Labor, that records wages and employment of all private sector workers in Chile from 2002 to 2023. In the AFC, we observe monthly wages and employers. We use this dataset to study formal employment, as being matched to the data is a proxy for formal employment, but not for unemployment necessarily, as 25% of the Chilean population works informally.

Pensions. We have access to pension data managed by the Superintendence of Pensions. We have access to monthly records from 2014 to 2023 that report who is a pensioner, an individual's total social security contributions, self-funded pension, and complementary or subsidized pension for those with low or null social security contributions before retirement. When we estimate the displacement effect on pensions, we will discuss what each of these components stands for.

3.4 Municipality and district attributes

We measure location attributes, such as longevity, education, and employment, by municipality and by census district, which come from the 1982 Census of Population and Vital Statistics. We combine these measures with historical records from the Ministry of Health in 1985 or earlier on hospitals and family health care centers per municipality.

4 Empirical Strategy

4.1 Identifying a displacement effect

To estimate the impact of the forced displacement on adults mortality, we exploit the fact that treatment was determined at the slum level and not based on individual family demographics. The empirical strategy involves comparing displaced individuals with non-displaced individuals who come from slums with the same probability of being relocated. The process of selecting slums into displaced and non-displaced groups did not depend on households' characteristics but rather on the feasibility of renewal on-site as a function of slums' location attributes.

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

We estimate a linear model to study the impact of the displacement on mortality, using the following specification (Deryugina and Molitor, 2020):

$$Died_{it} = \alpha + \beta Displaced_{s\{i\}} + \psi_o + p(X_s) + \psi_o \times p(X_s) + \phi_t + X_i'\theta + \varepsilon_{it}, \qquad (1)$$

where we define the time dimension of the data to be a year panel since the year of treatment, where $Died_{it}$ is a dummy that equals zero if individual i survived through year t, and equals one if the individual died in year t. If the individual dies in year t then $Died_{it}$ is missing in all years after t; $s(i)$ indexes the slum of origin for individual i and the variable $Displaced_{s\{i\}}$ equals 1 if an individual lived in a displaced slum or 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; $p(X_s)$ is the propensity score of the probability of slum s being cleared and relocated as a function of vector X_s that incluyes slums' characteristics; ϕ_t are calendar year fixed effects from first year of intervention through 2019. For precision, in (1) we add baseline controls for individual and family characteristics at the time of intervention, X_i, that include cohort fixed effects, a dummy for head of household, dummy for married, marital status unknown, indigenous last name, number of children at baseline, a slum's average formal employment before treatment, and year of intervention fixed effects (1979 to 1985) that control for aggregate temporal differences across the six years this housing program was in effect. We cluster standard errors at the level of slum of origin; however, later in the text we show robustness to other clustering methods.[24]

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

Equation (1) implies that we match on the propensity score, which requires first estimating the propensity score function (Abadie and Imbens, 2016). We choose the control function approach where we control directly for $p(X_s)$ and its interactions with ψ_o, instead of nearest neighbor or propensity score re-weighting because it offers greater flexibility and is more effective in cases where the overlap of the common support is imperfect (Busso et al., 2014).[25]

4.2 Propensity score estimation

To estimate the probability of relocation, we use data from Morales and Rojas (1986), who compiled the most complete sample of slums and their characteristics in urban areas. In these data, we observe 233 slums with full information on their characteristics (columns (1)-(4) in Table 1). To avoid overfitting, we estimate the probability of slum relocation using a LASSO model with a logit function, where we include all the variables in the table, but exclude the price index from the model because it might reflect expectations of future land prices due to slum clearance. LASSO selects as determinants of

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

[25]In the following sections, we will show robustness of our results to different versions of the propensity score method.

relocation density, military name, elevation, and census district population's schooling. Figure C.1 panel (a) shows the estimated densities of the propensity score by treatment. As expected, displaced slums have higher propensity scores compared to non-displaced slums. The common support is between 0.2 and 0.65. In column (4) of Table 1, we report the difference in slums' characteristics after conditioning on $\hat{p}(X_s)$ for slums in the common support. The results show balancedness and the exclusion of 12 slums from the full sample that have high estimates of $p(X_s)$.

Columns (5) and (6) in Table 1 show the characteristics of the slums in our archival sample, and column (7) shows the simple difference between treatments. In the archival sample, the differences between treatments are smaller than in the full sample, suggesting these slums are more similar to each other. Our data shows we are more likely to find displaced slums, larger (more families), that are located closer to the city center, and with a lower risk of flooding.

Because the slums in our sample are not a random sample of the universe of slums in the program, we use the estimates from the LASSO regression in the full sample to predict the probability of slum relocation in our archival sample of 98 slums. This approach increases statistical power and reduces selection on observables. Figure C.1 (b) plots the estimated propensity score densities in the archival sample of slums. Because the slums in the archives are more similar to each other, the propensity score densities are also more similar between treatments. In our archival sample, we do not have displaced slums with high probabilities of treatment (above 70%), thus when imposing common support, we only exclude 4 out of the 98 slums, which correspond to slums with high risk of flooding.

To account for the non-randomness of our archival sample, we re-weight the slums in our sample by the inverse probability of finding a slum in the archives.[26] Panel (c) in C.1 plots the re-weighted propensity score densities, showing that the weighted sample is more similar to the full sample, as it gives a larger weight to displaced slums with a high probability of displacement. We will use these weights as a robustness check for our baseline results.

Finally, we implement the propensity score method in four steps. First, we estimate the propensity score $\hat{p}(X_s)$ at the slum level using a LASSO logit function as described before. Second, we impose common support. Based on the propensity score densities

[26]See Appendix for a full description of the estimation of the weights.

by treatment in Figure C.1, we keep slums where $0.2 < \hat{p}(X_s) < 0.65$: from the 98 slums in our archival sample, 94 are in the common support. Third, we run equation (1) on the outcomes of interest where $p(X_s)$ is included as a continuous variable $\hat{p}(X_s)$ and interacted with municipality-of-origin fixed effects. This ensures that we compare displaced and non-displaced individuals within the same municipality with similar values of the probability of relocation.[27]

4.3 Evaluation of the identification strategy

The validity of our research design depends on whether the decision to displace a slum was uncorrelated with the characteristics of families conditional on the probability their slum was cleared. Under the assumption that conditional on the policy function, $p(X_s)$, the covariance between $Displaced_{s\{i\}}$ and ε_{it} is 0, the coefficient β estimates the causal effect of the displacement on adults' mortality. To provide evidence in favor of our empirical strategy, we compare the demographics of the displaced and non-displaced adults the year of the intervention (baseline).

Table 2 panel A shows summary statistics of baseline demographics of adults in the sample with common support. Columns (1) and (2) report means for the non-displaced and displaced groups, respectively. In our sample we keep individuals with a non-missing NID, between the ages of 18 and 80 years old at baseline. The numbers in Table 2 panel A indicate that individuals in the sample are 35 years old, 54% of them are women, and 34% live in a household where the woman is the head of the household (received the property right). They have 2.2 children on average, and 16% of them have no children, 47% of them were not born in Greater Santiago, and the average formal employment in their slum of origin is 40%. Finally, we report child mortality under the age of 1 and 5, in the five years before treatment, and find that 1.8% of the adults in the sample had a child who died under the age of 1.

Column (3) reports the difference between treatment groups conditional on the propensity score and municipality of origin fixed effects ($\hat{p}(X_s) + \psi_o + \hat{p}(X_s) \times \psi_o$). The table shows that after adjusting for the probability of relocation, displaced and non-displaced individuals have similar demographics at baseline, with no statistical dif-

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

ferences between both groups for 10 out of 12 observables. There are two variables that show a statistically significant difference from 0, though the sizes of the coefficients are small. The table shows that displaced individuals are 3.5 percentage points less likely to be married, and are 1.1 percentage points more likely to have an indigenous lastname (Mapuche). Columns (6) and (9) repeat the previous exercise separately for women and men. Most of the differences come from the women sample, who among the displaced are less likely to be married at baseline, but all other differences are balanced. It is important to mention that this difference in marital status between treatments is due to the loss of men without NIDs in the non-displaced group (see Table C.1 in Appendix), when we correct for the missing NIDs, we find a difference of 0.5 percentage points between groups, with a p-value of 0.15. Overall, the numbers show that displaced and non-displaced individuals look similar in their demographics, conditional on the probability of relocation.

In panel B of Table 2 we report the proportion of individuals who have died until 2023, and their age at death. In our estimation sample 40% of the individuals have died, with no differences between treatments; however, the displaced die younger in 3 years. These numbers are similar between men and women, but women on average live longer lives.

Finally, in panel C of Table 2, we report matching rates to administrative data, and find that 73% of the adults in the sample matches at least once with the RSH, 28% with the AFC, and 73% with the pensions records. The attrition rate between treatments is similar for the RSH, but in the AFC and the pensions data we find opposite differential attrition signs. Displaced individuals are 8 percentage points more likely to be found in the formal employment data (AFC), and 5 percentage points less likely to have a pension record.[28] Notice that these attrition rates are outcomes themselves. Poorer people and women are more likely to be found in the RSH because the system is used to provide social benefits to families, while men and higher-income individuals are more likely to be found in the AFC because it corresponds to the formal private sector.[29] These matching rates are likely a function of treatment, because if displacement affects mortality, then the final attrition to administrative data is both a function of mortality and type of employment.

[28]Our matching rates are not 100% because we are not conditioning on being alive. Once we condition on being alive in 2014, our matching rates are 95%.

[29]The National Bureau of Statistics reports that in Chile women are more likely than men to work in informal jobs.

We will discuss these differences in more detail when we estimate displacement effects on earnings and employment, and will adjust our estimates accordingly.

5 Main Results

5.1 Displacement effect on new location attributes

We begin our analysis by showing that displaced families are relocated to areas of lower quality compared to the non-displaced families. In Figure 2, we plot the densities of the characteristics of places in the relocation areas for both displaced and non-displaced households. Panel (a) plots the densities of average longevity in municipalities of destination in 1985 for each treatment. The figures show that displaced households are relocated to municipalities with lower average longevity in 1.9 years compared to municipalities of origin for the non-displaced. Panel (b) plots densities of the distance to the CBD for both displaced and non-displaced after the treatment, showing that displaced households are relocated into neighborhoods that are 2.9 kms farther from the CBD compared to non-displaced. This reduction in neighborhood quality is also found in other neighborhood attributes. Appendix Table A.1 shows that displaced households, compared to non-displaced, end up in places where unemployment levels are higher, there are fewer primary care centers per municipality, and the property prices in surrounding areas are lower.

5.2 Displacement effect on annual mortality

We continue our analysis by estimating the average displacement effect on mortality in Table 3. The dependent variable is annual mortality in year t, conditional on having survived until year $t-1$, expressed as percentage points. For better exposition, we multiply the displacement coefficient by 100, so it is interpreted in percentage points. The results in column (1) control for municipality fixed effects (ψ_o) and calendar year fixed effects. The coefficient of displacement indicates that displaced adults die 0.1 percentage points more per year in a 40-year period, which represents 10.3% more relative to the non-displaced mean. In column (2), we include baseline controls, and the effect increases to 0.19 percentage points, suggesting the importance of controlling for demographic characteristics as determinants of mortality. Column (3) adds slums characteristics at baseline, which increases the displacement effect to 0.27, and column (4) replaces the slums characteristics for $\hat{p}(X_s)$, finding very similar results, suggesting that the predicted propensity score is a good summary of slums characteristics. Finally, in column (5) we run regression (1) where we add a full set of interactions between the propensity score $\hat{p}(X_s)$ and municipality of origin fixed effects (ψ_o). This result in column (5) indicates that displaced individuals die 0.23 percentage points more per year, which represents a 21.6% higher risk of mortality relative to non-displaced adults per year. This is our preferred specification, as it fully saturates the model with the propensity score interactions.

We compute separate regressions by sex and cohort. Table 4 shows estimates of the displacement effect equivalent to column (5) of Table 3, by sex and cohort. The results in the first two columns show that the displacement effect is larger for men than for women, 0.31 versus 0.17 percentage points, with corresponding relative effects of 27.3% versus 16.9%, respectively. The next two columns estimate the displacement effect by cohort. Individuals older than 40 experience a larger displacement effect on annual mortality compared to younger adults (40 or less), but where the percent effect is larger for younger individuals (30.4% versus 17.4%).

Previous estimates correspond to the average annual displacement effect in a 40-year period, thus, we turn to study the effects of displacement on mortality by years since intervention to understand when the displacement effect is larger. Panel (a) of Figure 3 presents the displacement effect estimates for mortality in year t after the intervention. These are the annual marginal effects on mortality. The figure shows positive effects of the displacement on mortality immediately after the treatment (five years later). The marginal annual displacement effect is relatively stable for 15 years, then, it decreases on year 20, but returns to an increasing trajectory after year 25 until year 40. This means that the cumulative effect on mortality is increasing. To show this, we compute the cumulative displacement effect t years after intervention. Let ΔM_t be the cumulative change in mortality t years after treatment (Deryugina and Molitor, 2020),

$$\Delta M_t = \prod_{\tau=1}^{t}(1 - m_\tau + \beta_\tau) - \prod_{\tau=1}^{t}(1 - m_\tau), \tag{2}$$

where m_τ is the fraction of individuals who died in year τ in the full sample, and β_τ is the marginal displacement effect on mortality in year τ, from equation (1).

Panel (b) of Figure 3 presents the displacement effect on cumulative mortality. After 40 years, the cumulative displacement effect on mortality is 5 percentage points, which represents a 7% lower survival rate of displaced adults compared to non-displaced adults.

5.3 Displacement effect on expected longevity

We repeat the exercise in 3, but instead we estimate the effects by age at measurement, and the result is in Figure 4 panel (a). The figure shows that the displacement effect on mortality increases with age, and it becomes positive and statistically different from zero by the age of 50. We use these estimates to compute the survival rates by age for both displaced and non-displaced individuals in our sample. We plot the cumulative survival rates separately for each treatment group. The results are in Figure panel (b). The survival rate for the displaced group is below of the survival function of the non-displaced for all ages after 35. We use these functions to compute the displacement effect on expected longevity as the area between the two curves, and the result is -2.425 years. This result implies that the mortality rates in our sample predict a decrease of 2.43 years in the age at death for the displaced individuals relative to the non-displaced group. This estimate es large, and is comparable to the positive effects of displacement on mortality for WWII refugees in Germany (Bauer et al., 2019), or the negative effects of displacement on mortality in New Orleans after Hurricane Katrina (Deryugina and Molitor, 2020).

We repeat this exercise by sex and cohort, and report the corresponding decrease in longevity for each group in the lower panel of Table 4. The displacement effect on expected age at death is larger for men than for women in more than 1 year (2.7 vs 2.1), and it is larger for older cohorts; however, the effect for young adults (below the age of 40 at baseline) is -1.68 years, which is also a large effect compared to the previous literature.

5.4 Displacement effect by causes of death

In this section, we estimate the effects of displacement on mortality by cause of death. We clean the causes of death in death certificates and validate them using administrative data from Chile's Ministry of Health. By doing so, we classify causes of death into five categories: Cancer, cardiovascular diseases (hypertensive heart disease, hypertensive kidney disease, cerebrovascular diseases, atherosclerosis, other diseases of the circulatory system), internal causes of death (diabetes, influenza, pneumonia, chronic lower respiratory disease, chronic liver disease and cirrhosis), external causes of death (accidents, suicides, homicides, other accidents, other external causes), and other causes not classified in the previous four categories, including missing causes of death.[30]

We estimate cumulative displacement effects on mortality (Figure 3 panel (b)) by cause of death and sex. The results are shown in Figure 5. Displacement increases mortality in all four categories ("other causes" not plotted in the figure), but its magnitude varies by sex. Most of the increase in mortality due to displacement is found in internal causes of death, with almost no differences by sex, followed by cardiovascular diseases, with a slightly larger effect for women in the medium term, but with the same results by the end of the 40-year period. The displacement effect on cancer shows larger effects for men, but effects converge after 40 years between the sexes. The largest difference between men and women is for the external causes of death, where displacement affects men almost entirely. After 40 years, the cumulative effect on mortality due to external causes is 2 percentage points larger for displaced men compared to non-displaced, which explains around 25% of the increased risk of dying among men after 40 years.

In Appendix Table A.2 we report the main causes of death for the Chilean population by sex in 2001 and 2018. The main cause of death is cancer, followed by cardiovascular disease, internal causes of death, and external causes of death. Based on these numbers, we confirm that displacement increases mortality by cause of death in all four categories; however, the increased risk of dying due to displacement is exacerbated in internal and external causes of death in our sample. These results may be associated with an income effect (Lleras-Muney et al., 2024). The fact that we find increases in mortality due to external causes among men, might be related to environmental and/or work conditions. We will return to these patterns when we discuss the mechanisms.

5.5 Displacement effect on retirement pensions

In an ideal setting, we would like to estimate displacement effects on employment and earnings immediately after the treatment took place. This is not possible in our setting because we only observe income data starting in 2007; however, we have access to pension

[30]These categories are not mutually exclusive, because some of the death certificates could include several causes of death without a strict order.

data from 2014-2013. Pension data provides a good measure of formal social security contributions, and hence income due to formal employment. In the Chilean system, we can decompose the total pension received by a worker into three parts: (1) self-funded pension coming directly from a worker's social security contributions; (2) a subsidized pension, corresponding to a lump-sum subsidy for individuals with very low or zero social security contributions; and (3) a complementary pension, that corresponds to a subsidy that varies with the amount in (1) and complements (2), this is, for individuals with positive social security contributions that are too low for a basic living standard, individuals receive a complementary pension that is decreasing in (1).

Table 5 presents displacement effects for all three components of pensions among the individuals we match with the pension data. The results show that displaced individuals have self-funded pensions that are 18% lower compared to those non-displaced. This is an indication of longer informality or unemployment spells among displaced individuals across their lifetime cycles. We find no differential effects between displaced and non-displaced in the subsidized pension, but the complementary pension is 20% larger for the displaced. Taken together, the pensions of displaced individuals are CLP$3,369 lower, which is -3.3% compared to non-displaced. Our results have important implications: due to displacement, individuals are less likely to contribute to social security contributions, which has negative impacts on their retirement pensions, but the Chilean pension system counteracts the negative effect and leaves displaced adults at more similar levels of those of the non-displaced individuals.

Finally, Table 5 also shows that displaced individuals are 3 percentage points more likely to claim a disability pension before the age of retirement (column (4)). The displacement estimate is small but large in economic terms because it represents an 88.2% higher likelihood compared to non-displaced adults. Because they are more likely to claim a disability, their disability pensions are also larger (column (5)).

5.6 Attrition

In this section, we discuss how the different stages of attrition in our sample may influence our results on mortality and retirement pensions. Our final sample of individuals with non-missing NIDs for whom we estimate a displacement effect suffers from two levels of attrition. The first is attrition from not finding all slums in the program in the archival records, and the second is from losing observations of individuals without a valid NID.

5.6.1 Attrition in Archival Records

To account for the missing slums, we reweight our archival sample of slums by the inverse probability of finding a slum in the archival records stratified by treatment status, such that the propensity score densities by treatment resemble the densities of the full sample of slums. Figure C.1 panel (c) shows how the reweighted densities put a higher weight on slums with high probabilities of treatment, which are the slums that we are less likely to find in the archives.

We report the reweighted effects of displacement on annual mortality in panel (a) of Table C.2. Columns (1) to (5) are equivalent to Table 3. Results show very similar estimates to those in our baseline estimation. Column (5) indicates that displaced individuals die 0.15 percentage points more per year, which is 20% more. This result is very similar to that of column (5) in 3. Similarly, columns (6) and (7) report reweighted estimates by women and men separately, and as expected, the effects are larger for men than for women.

5.6.2 Attrition due to missing NIDs

Similarly to the previous exercise, to account for the missing NIDs in our archival sample, we reweight each individual observation by the inverse probability of having a missing NID, where this probability is estimate using logit model as function of gender, the number of partners in the archival data, female head of household, and mapuche last-name. Recall from the data section, that those with missing NID are more likely to be non-displaced single men. If single men have higher mortality than the rest of the demographic groups, then, the displacement effect we estimate in our baseline sample is an upper bound. Thus, accounting for this type of attrition is desirable.[31]

The results are reported in panel (b) of Table C.2. The results show more muted displacement effects on mortality, but still large in economic terms and highly significant. Displaced individuals have higher annual mortality in 0.18 percentage points, which is 22.6% more compared to non-displaced. The effects by gender are larger for men than for women, and the corresponding percent effects are 27% and 20%, correspondingly. All these percent effects are very similar to those in our baseline results.

[31] Another way of accounting for this type of attrition is to perform Lee bounds. We prefer to reweight our baseline sample because our outcome variable is binary, so we do not have to choose the trimming portion based on a random sample.

5.7 *Robustness checks*

In this section we show that our baseline displacement effect on annual mortality is robust to other propensity score methods, and to different parametric models to estimate the risk of dying.

In Table A.4 we show robustness of our baseline result to variations in the propensity score method. First, we find very similar estimates when we run our baseline results using inverse propensity score re-weighting. Second, we restrict our common support to include slums in the 5–95, and 10-90 percentiles of the common support of the propensity score. Finally, we drop from our sample three municipalities with low propensity score overlap between treatments. All these exercises show very similar displacement effects on annual mortality.

In Figure A.1, we run our baseline results using a parametric approach. Instead of running a linear probability model we estimate Kaplan-Meier survival functions by treatment since years of intervention. The figure shows displaced individuals have a higher risk of dying (or a lower risk of surviving) compared to non-displaced individuals since the initial year of intervention. We reject equality of functions. Figure A.2 presents a similar figure by age, showing that displaced individuals die younger than non-displaced, and we also reject equality of survival functions between groups. Estimates are comparable to those presented in previous sections estimated using a linear model.

6 Mechanisms

We investigate the mechanisms behind our baseline results on mortality. Based on families' impressions after relocation and the lower-quality attributes of destination neighborhoods, we study which changes in neighborhood attributes explain changes in mortality. We then examine how displacement affects individuals' locations in the long-run.

6.1 *Changes in location attributes*

We explore the relationship between the displacement effect on mortality and changes in location attributes. Figure 6 plots the predicted increase in annual mortality in the sample of displaced individuals computed as the difference in annual mortality when

$Displaced = 1$ and when $Displaced = 0$ in equation (1), against the change in average longevity between municipalities of destination and origin in 1985. The correlation shows that the increase in annual mortality is negatively correlated with increases in average longevity, implying that individuals relocated to worse areas in terms if life expectancy, have a higher risk of annual mortality.

More formally, we estimate the relationship between changes in location attributes and annual mortality. To do so, we follow a similar strategy to Chetty and Hendren (2018) by replacing the displacement dummy for the change in average longevity between destination and origin municipalities in the following equation:

$$Died_{it} = \alpha + \gamma \Delta Longevity_{do} + \delta Longevity_o + \psi_o + p(X_s) + \psi_o \times p(X_s) + \phi_t + X_i' \theta + \varepsilon_{it}, \tag{3}$$

where all variables are defined as in Equation (1), $\Delta Longevity_{do}$ stands for the change in average longevity between municipality of destination d and municipality of origin o. The estimate of interest is γ, which estimates the relationship between the change in one year of longevity between destination and origin municipalities and the annual mortality rate. We control for $Longevity_o$, the average longevity in the municipality of origin in 1985, to account for differential mortality rates in municipalities of origin. Importantly, this measure is not collinear with ψ_o, because it is measured at a smaller geographic level than municipalities of origin in 1980.[32] Notice that identification comes from the displaced sample, for whom there is variation in $\Delta Longevity_{do}$, and it is 0 for non-displaced individuals. This strategy relies on the assumption that new locations were not chosen by families in the program. In Appendix Table A.5, we provide evidence that families' characteristics do not predict the attributes of destination locations, nor the changes.

The results of estimating equation (3) are in Table 6 column (1). The estimates show that moving to a municipality where the population has one year higher longevity compared to the origin, reduced the risk of annual mortality in 0.013 percentage points, which is a reduction of 1.3% relative to the sample mean. Put in different words, the

[32] ψ_o corresponds to 14 different municipalities with municipality borders measured in 1980. After 1982, a new set of municipality borders were drawn, which divided Greater Santiago in 30 different municipalities. We use 1982 municipality borders to measure average longevity in 1985.

average $\Delta Longevity_{do}$ in our sample is -2.21 years, which implies an increase in annual mortality of 0.029 percentage points. This estimate is statistically significant at the 5% level.

Notice that the changes in longevity between destination and origin municipalities do not account for the total displacement effect, and this is likely because displacement involved several other changes in location attributes. That is why in the next following columns of Table 6 we include other location changes induced by displacement. We observe that an increase in the distance to CBD in 1 kilometer increases the risk of dying in 0.015 percentage points per year (column (2)). This variable proxies for access to employment. Column (3) shows that individuals that relocate with their whole slum network reduce their risk of dying in 0.2 percentage points per year. This imply that group relocations that reduce distortions in networks have a protective effect against mortality. Column (4) reports that a positive change in the number of primary care health centers per municipality reduce mortality, but the estimate is not statistically different from zero. Finally, column (5) combines all changes and their effects on annual mortality. Importantly, the coefficient on $\Delta Longevity_{do}$ is robust to the inclusion of other location changes, which sheds light on the role of place effects to explain the variation in mortality risk in our sample, in addition to access to employment, networks, and public services.

6.1.1 Results by sex

The first two columns of Table A.6 estimate the regression in column (5) of Table 6 differentially by sex. The results are interesting, and show that men and women's mortality respond differentially to different changes in location attributes. Mortality of both sexes is lower in areas with higher longevity, but with a larger effect for women than men. Both sexes benefit from moving with their whole slum network; however, longer distances to the CBD impact men's mortality positively, with almost null effects for women, while access to more primary care centers impacts women's mortality negatively with null effects on men.

These results point to different place effects by sex. In the 1980s, men were more likely to be the sole breadwinners, even more among low-income households, which is the case of the population we study. In our sample, men have a baseline formal employment level of 50%, while women have only a 10% level of formal employment.Estimates from pension data before 1980 that we matched to our sample of adults with a valid NID. Thus, the fact that men's risk of mortality responds more to lower access to jobs in destination areas speaks to an employment channel explaining the increases in mortality among displaced individuals. On the contrary, women's mortality is more sensitive to access to health centers, which is consistent with the fact that women, compared to men, are more likely to use these services,[33] and thus, lower access to health services impacts displaced women negatively.

6.1.2 Results by causes of death

In addition to the results by sex, we explore whether mortality by causes of death respond differently to changes in location attributes. The results are reported in columns (3) to (6) in Table A.6. The results are noisy but the estimates point to a mechanism where the lack of access to jobs, measured as the distance to the CBD, increases the risk of dying due to internal and external causes of death, especially the latter, which was also more prominent among men (recall section 5.4). While we cannot study more granular external causes of deaths, the fact that these are more likely among men who have lower access to employment could be due to several incidents, such as accidents going to work, a higher incidence of suicides, or accidents on the job.

6.2 Long-term locations

To better understand the place effects found before, we study where these individuals currently live. In Table 7 we report displacement effects on the likelihood of living in the assigned municipality (column (1)) and the municipality of origin (column (2)). The results show that displaced individuals are 1.6 percentage points more likely than non-displaced individuals to live in their municipality of assignment in the period 2007-2023, however, this estimate is small and not statistically different from zero. Column (2) shows that displaced individuals are 68% less likely to live in their municipality of residence before the treatment took place, meaning they are not likely to return. Columns (3) to (5) measure similar outcomes, but at the neighborhood level (smaller than a municipality), but the data is only available starting in 2016, which is why

[33]A report conducted by Chile's Superintendency of Health in 2021, shows that women are more likely to uses demand and use health care services. See report here https://www.superdesalud.gob.cl/app/uploads/2022/10/articles-21784$_recurso_1.pdf$.

we observe fewer individuals for these outcomes. The results show patterns similar to those in the first two columns. Displaced individuals are equally likely to live in their neighborhood of assignment, compared to non-displaced, but if they move, they move close, as the distance to the neighborhood of assignment is 308 meters less for the displaced from a baseline of 4 km. Finally, column (5) reports that the neighborhoods where the displaced households currently reside are 2% more likely to be considered poor, but the estimate is not statistically significant.

The previous results show that due to relocation (and probably homeownership), displaced individuals are less mobile compared to non-displaced individuals. To understand this better, in Table A.7, we explore whether displaced households sell their homes. These outcomes are only available for the sample of households that received a home in the northwest, northeast, and central areas of Greater Santiago, which represents about a third of the total number of households in our baseline sample.[34] With these caveats in mind, the results show that displaced individuals are 15% less likely to sell their houses, though the estimate for the displacement effect is very small and not significant, the percent effect is large because the baseline is very low, only 5% of housing units in the non-displaced group has ever been sold by 2019. Displacement is associated with a higher likelihood of giving the house for inheritance, and while the estimated coefficient is small and not statistically significant, it represents a 6% higher probability relative to the comparison group. After inspecting the data, most of these houses are inherited by the children in these households (not shown in the table). Conditional on selling, though the sample is very small (N=224), displaced households sell at lower prices and 1.7 years earlier. Notice that on average, the homes in the sample were sold around 2008, which is consistent with the fact that families were not allowed to sell their homes until they had finished paying for them after 12 or 25 years. Thus, selling was only possible in the long run. This evidence is consistent with displacement reducing mobility; however, we cannot rule out that these homes were rented informally, which might explain why only a third of individuals in our sample remain in their neighborhoods of assignment, but close to them, probably within the boundaries of the same municipality (columns (1), (3), and (4) in Table 7).

[34] We partnered with Santiago's Real Estate Registrar to track families' addresses in our archival data. Their jurisdiction does not include the Southern part of the city.

7 Long-term effects on labor market outcomes

In this final section, we investigate how displacement shapes individuals' long-term labor market outcomes. In the previous section 5.5 we documented that displacement had an impact on retirement pensions, finding a negative displacement effect on self-funded pensions that was compensated by a governmental subsidy. These results suggested a negative effect on formal employment. To investigate this more formally, we use the RSH and AFC administrative data, where we observe individuals employment before and after retirement. The youngest we observe individuals of our sample in the RSH/AFC data is 50 years old. We perform the analysis by splitting the sample between individuals below the age of 65 and older than 65 at the time their income is measured. We do this to investigate how displacement affects labor market outcomes before and after retirement.

In Table 8 panel A, we report displacement effects on monthly employment and earnings before the mandatory age of retirement.[35] The result in column (1) shows displaced individuals have 2.9 percentage points higher employment, which represents a 5.3% higher employment compared to non-displaced individuals at the same age. Column (2) shows no displacement effect on the likelihood of working with a contract, thus the positive employment effect is driven by more informal jobs. Column (3) reports the average displacement effect on self-reported labor earnings, finding a negative effect of CLP$13.5 lower earnings, which represent a reduction of 10% compared to non-displaced adults. Rojas-Ampuero and Carrera (2025), find a very similar negative result on the adult labor earnings of displaced children, but they find null effects on employment. Next, column (4) reports effects on the portion of self-reported earnings worked under a contract. The displacement effect is negative but smaller in absolute value compared to column (3), indicating that the displacement effect is also negative on the informal portion of earnings. Finally, column (5) shows the displacement effect con "corrected" income. The corrected income is a measure of total household income computed by the Ministry of Social Development, which includes information from different administrative sources, the RSH, and family demographics. It is used to target social benefits, but unfortunately, it is only available starting in 2016. The displacement effect on this variable is more negative both in levels and in percent effect, indicating that displaced

[35]In Chile the age at retirement is 65 for men and 60 for women. According to the Superintendency of Pensions, in practice, men retire on average at the age of 67 and women at the age of 63.

adults are poorer than non-displaced.

Columns (6) and (7) report similar estimates but using data from the AFC, which reports formal and private employment in Chile. Column (6) shows a positive employment effect, consistent with column (1), but a positive displacement effect on formal wages in column (7), that is 10% larger compared to non-displaced individuals. Why do we find different displacement effects on wages? The reason is selection. In Table 2 we showed that our matching rates to the RSH were large (73%), and were not different between treatments. On the contrary, attrition to the AFC is much larger, as only 27% of our baseline sample matches with the AFC, and displaced individuals are 8.3 percentage points more likely to be found in the formal employment data. Because of the formal nature of the AFC, the individuals that we find in this system are expected to be positively selected on their wages, which might explain the positive displacement effect. In Table A.8, we control for selection, and find a more muted displacement effect on wages, but still a negative displacement effect on self-reported labor earnings. This result is consistent with the findings of Bauer et al. (2019), who find that individuals in the upper part of the income distribution were able to counteract the positive displacement effect on mortality due to their higher earnings.

Next, we study the same labor market outcomes, but after the age of 65 when individuals retire in Table 8 panel B. Our results show that displaced adults are 3.8 percentage points more likely to work (13.2% more) compared to non-displaced individuals. Because they work more, they have slightly higher earnings (column (2)). Notice that the average employment rate of the non-displaced group is low and equal to 28.4%, and the average monthly wages are very low and equal to CLP$10,262, which is less than 2% of the minimum wage in Chile in 2024. Overall, their total income is lower by 10% (column (5)). Results in columns (6) and (7) report null effects on formal employment and wages after retirement, with very low average outcomes for the non-displaced. These results are expected, because firms are not mandated to report unemployment insurance when workers retire, and that is why we do not observe many formal records after the age of 65. Taken together, these results are consistent with our results on self-funded pensions: displaced elderly adults keep working after retirement as a form of compensation for their lower income after the age of 65.

8 Conclusions

This paper studies the long-term effects on mortality of a housing policy that affected approximately 5% of the population of Greater Santiago during the Pinochet dictatorship. As part of the program, families that lived in a large number of slums were relocated to the periphery of the city. Because the relocation decision was made at the slum level and families did not choose their final locations, we are able to compare the mortality and earnings of displaced and non-displaced adults who lived in slums that had the similar *ex-ante* probabilities of being relocated.

Our results show a statistically significant displacement effect on mortality for both women and men. Displaced adults have a 21.6% higher probability of dying per year compared to non-displaced adults, with the consequences of experiencing a 7% lower survival rate forty years after the intervention ended. These large mortality effects are associated with different causes of death by sex. The analysis of causes of death shows that displaced individuals experience an increased risk of dying from all types of causes of death, but in particular, displaced men have an increased risk of dying due to external causes, which include accidents and other violent causes of death.

Our analysis of mechanisms suggest that the displacement effects on mortality are associated with segregation and isolation. Higher mortality risk is associated with worse environments in destination locations. Decreases in longevity in destination municipalities correlate positively with own-individual mortality, longer distances from origin, and the disruption of slum networks increase the risk of dying among displaced individuals. And finally, mortality by causes of death respond differently by changes in location attributes, in particular, the increased risk of mortality among the displaced due to internal and external causes of death correlate positively with longer distances to the CBD, especially among men. This last result suggests that access to employment is important in explaining the increased risk of mortality for displaced men.

In the longer run, individuals who survive to the age of retirement, we find that displacement reduces the likelihood of working with a contract, and has a negative impact on labor earnings. We observe that displaced adults have lower self-funded pensions compared to non-displaced adults. This may encourage the elderly to continue working after the age of 65. However, we find positive selection effects among individuals who work in formal jobs, as they have higher wages before retirement.

All these results together point to the direction of displaced individuals having worse access to labor markets and lower quality jobs, which may be related to worse health outcomes.

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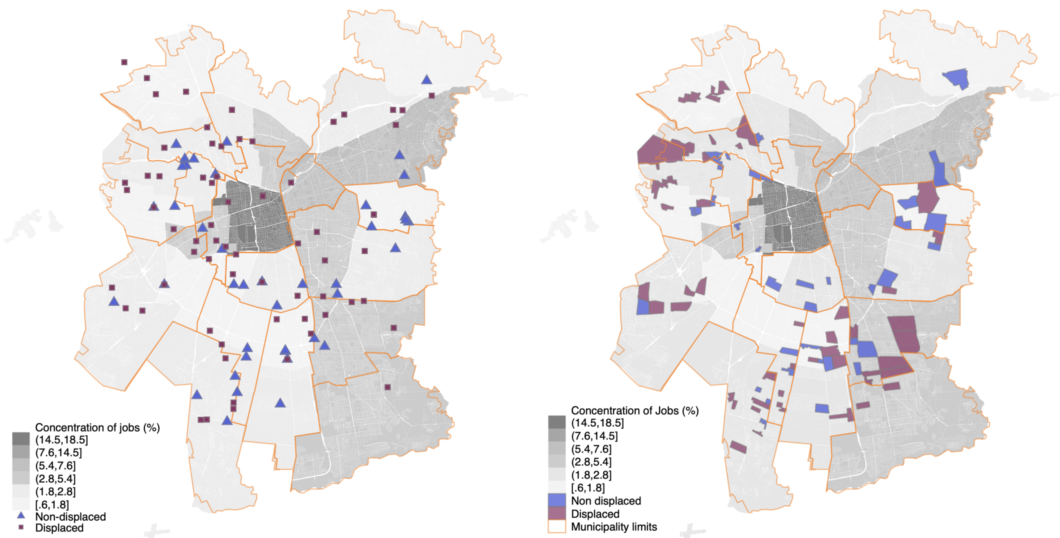
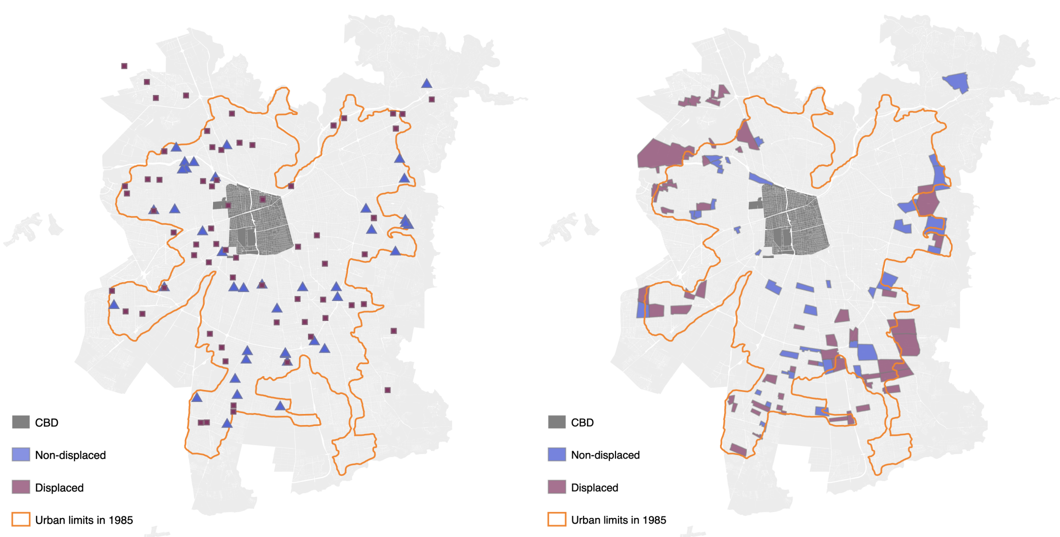
Figures and Tables

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



(a) Slum location before 1979

(b) Public housing projects after 1985



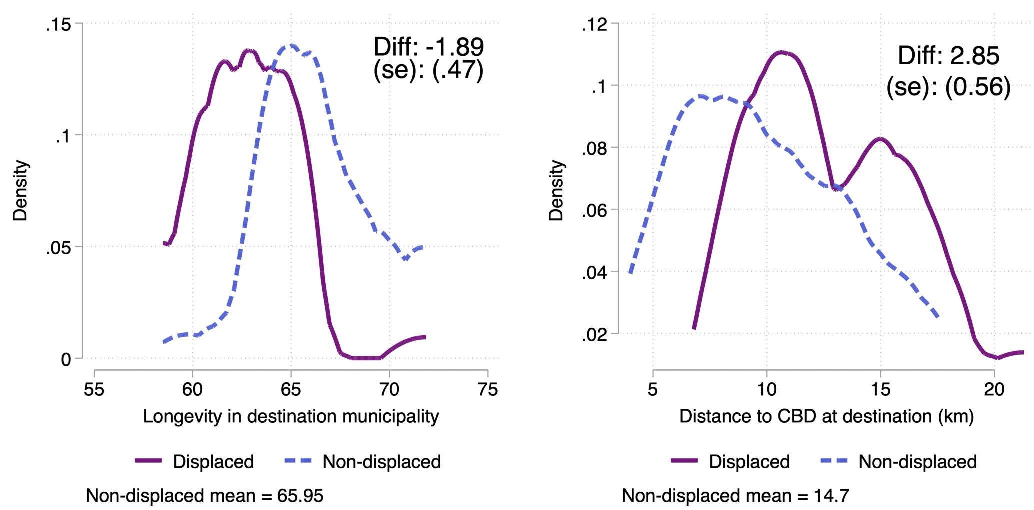
(c) Slum location before 1979

(d) Public housing projects after 1985

Notes: Orange lines represent municipalities borders in 1980 in panels (a) and (b), and urban limits of Greater Santiago in panels (c) and (d). 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 and relocated into 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), Morales and Rojas (1986), and the population censuses of 1982 and 1992.

Figure 2: Density of neighborhood attributes after relocation

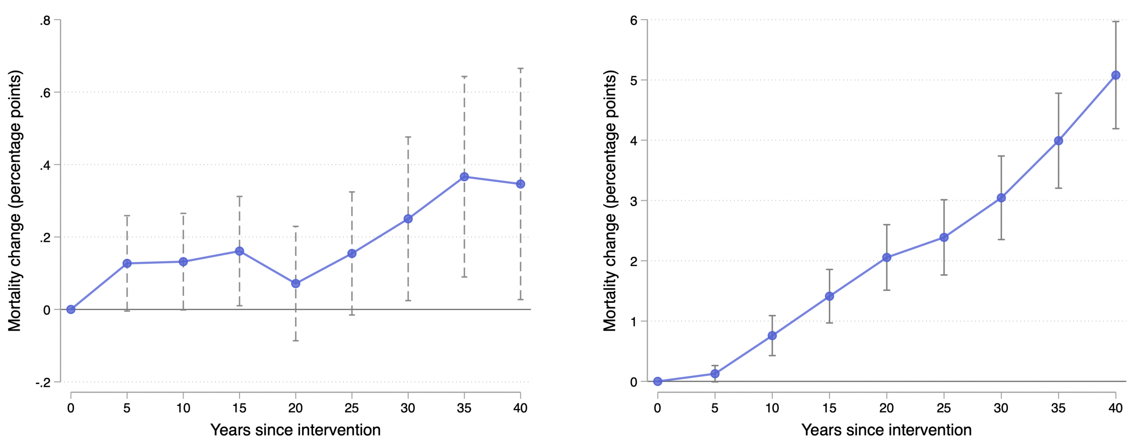
(a) Longevity **(b)** Distance to the CBD



Notes: The figure shows densities by treatment for the average neighborhood attributes for each pair of slum of origin and project of destination in the archival sample (N = 112 unique pairs of slum-project of destination). Each subfigure's footnotes indicate the mean difference between treatments for all households in the sample, conditional on the propensity score (N = 16,198). We compute the average for all households in the sample with common support.

Figure 3: Displacement effect on adults' mortality by years since treatment

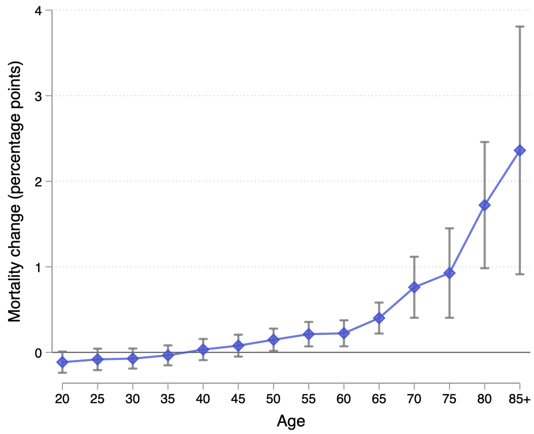
(a) Annual effect **(b)** Cumulative effect



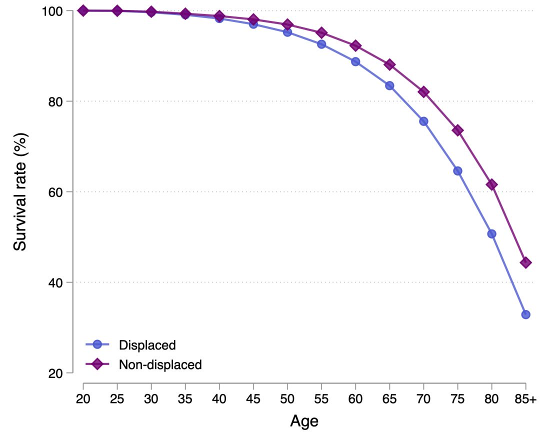
Notes: Estimates in percentage points. The figure in panel (a) plots the coefficients β_τ and their 95% confidence intervals from regression $Died_{it} = \sum_{\tau=0}^{40} \beta_\tau 1(t=\tau) \cdot Displaced_{s\{i\}} + X_i'\theta + \psi_o + \hat{p}(X_s) + \psi_o \times \hat{p}(X_s) + \gamma_t + \varepsilon_{it}$. Panel (b) plots estimates of the displacement effect on annual and cumulative mortality. See equation (2) in text.

Figure 4: Displacement effect on mortality by age at measurement

(a) Effect by age



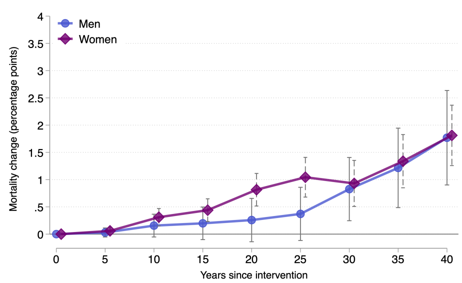
(b) Survival by treatment



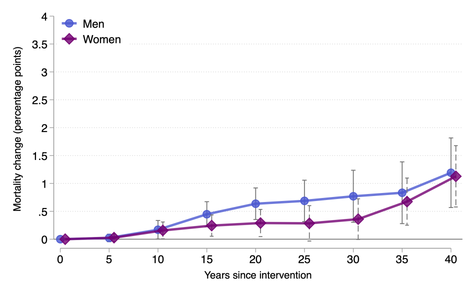
Notes: Estimates in percentage points. The figure in panel (a) plots the coefficients β_τ and their 95% confidence intervals from regression $Died_{it} = \sum_{\tau=20}^{85+} \beta_\tau 1(age = \tau) \cdot Displaced_{s\{i\}} + X_i'\theta + \psi_o + \hat{p}(X_s) + \psi_o \times \hat{p}(X_s) + \gamma_t + \varepsilon_{it}$. Panel (b) plots estimates of the displacement effect on annual and cumulative mortality by age, similar to equation (2) in the text.

Figure 5: Displacement effect on cumulative mortality by causes of death and gender

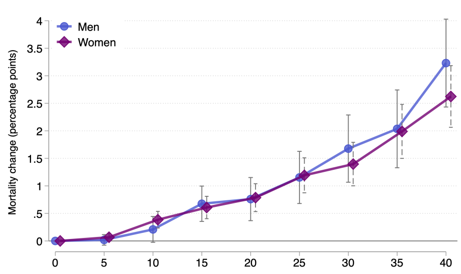
(a) Cardiovascular



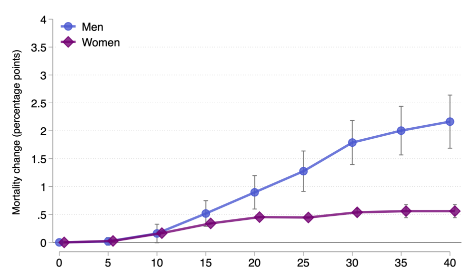
(b) Cancer



(c) Internal

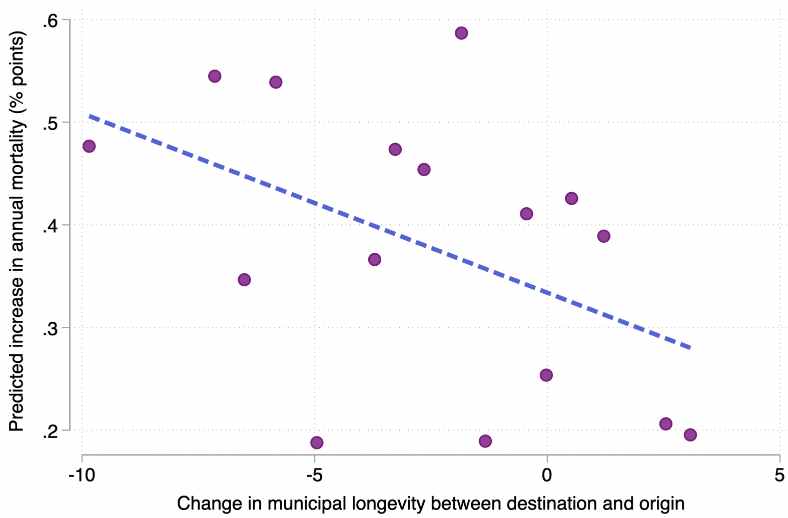


(d) External



Notes: The figure plots the cumulative displacement estimates and their 95% confidence intervals equivalent to panel (b) in Figure 3 stratified by sex and cause of death.

Figure 6: Relationship between predicted change in mortality and change in municipal longevity



Notes:The y-axis plots the predicted displacement effect on annual mortality in the group of displaced adults, and the x-axis the change in municipal longevity in 1985 between municipality of destination and origin. The change in longevity is broken down into 20 equal-size bins, and the data is collapsed into these bins. To compute the predicted displacement effect we estimate equation (1) separately for displaced and non-displaced, and take the difference between treatments for each observation i.

Table 1: Slum characteristics before intervention

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

Notes: The table shows summary statistics for displaced (cleared and relocated) slums and non-displaced (redeveloped) in Morales and Rojas (1986)'s sample with non-missing attributes or locations. Slum locations and characteristics are constructed from Benavides et al. (1982), Morales and Rojas (1986), MINVU (1979), newspapers, and the Population Census of 1982. Elevation, slope, and flooding risk data are obtained from Geoportal. Prices, unemployment, number of schools, and population's schooling are measured at the census district level where a slum was located. Column (3) reports the simple difference in each attribute between displaced and non-displaced slums, column (4) shows the difference between groups controlling for the propensity score at the slum level, $\hat{p}(X_s)$, in the sample with common support (see text for explanation of how the propensity score is estimated). Columns (5) to (8) repeat the exercise for the slums in the archival sample. Robust standard errors are in parentheses. 10%*, 5%**, 1%***.

Table 2: Comparing displaced and non-displaced adults at baseline (year of intervention)

	All adults			Women			Men		
	Displaced mean	Non-displaced mean	Difference	Displaced mean	Non-displaced mean	Difference	Displaced mean	Non-displaced mean	Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Demographics									
Female	0.542	0.542	-0.001						
			(0.003)						
Age	34.941	34.956	0.337	34.611	34.657	0.270	35.330	35.309	0.414
			(0.563)			(0.567)			(0.572)
Female HH	0.348	0.340	0.007	0.419	0.396	0.024	0.263	0.274	-0.013
			(0.027)			(0.030)			(0.025)
Married	0.760	0.794	-0.035***	0.715	0.757	-0.043***	0.813	0.838	-0.026***
			(0.010)			(0.012)			(0.010)
Married w/cert	0.840	0.852	-0.011	0.832	0.851	-0.019**	0.849	0.854	-0.001
			(0.008)			(0.008)			(0.009)
Mapuche lastname	0.057	0.046	0.011***	0.057	0.041	0.014***	0.056	0.051	0.007
			(0.004)			(0.005)			(0.005)
Born out Stgo	0.462	0.481	-0.017	0.467	0.497	-0.027	0.455	0.463	-0.005
			(0.019)			(0.020)			(0.019)
# children	2.244	2.194	0.046	2.233	2.178	0.045	2.257	2.213	0.047
			(0.064)			(0.059)			(0.073)
No children	0.157	0.164	0.000	0.165	0.172	0.005	0.148	0.155	-0.005
			(0.010)			(0.011)			(0.012)
Years of educ[a]	6.267	6.604	-0.328*	5.939	6.253	-0.283	6.703	7.070	-0.393*
			(0.190)			(0.185)			(0.202)
Formal employment (1975-1980)									
by slum [b]	0.380	0.415	-0.029	0.380	0.414	-0.028	0.381	0.416	-0.029
			(0.021)			(0.021)			(0.021)
Child mortality (last 5 years)[c]									
# Children died $<$ 1y	0.016	0.020	-0.004	0.015	0.018	-0.002	0.017	0.021	-0.003
			(0.003)			(0.004)			(0.004)
# Children died $<$ 5y	0.020	0.024	-0.004	0.020	0.023	-0.003	0.021	0.026	-0.006*
			(0.003)			(0.003)			(0.003)
B. Mortality after treatment									
Died	0.400	0.401	-0.006	0.338	0.346	-0.011	0.474	0.465	0.000
			(0.022)			(0.020)			(0.027)
Age at death	68.398	71.894	-2.995***	70.358	73.843	-3.189***	66.745	70.180	-2.776***
			(0.367)			(0.442)			(0.435)
C. Matching rates to administrative data									
In RSH	0.724	0.741	-0.008	0.779	0.791	-0.005	0.660	0.682	-0.011
			(0.020)			(0.019)			(0.025)
In AFC	0.332	0.240	0.083***	0.226	0.144	0.071***	0.457	0.353	0.097***
			(0.016)			(0.015)			(0.020)
In Pensions	0.718	0.775	-0.048***	0.768	0.817	-0.045***	0.659	0.725	-0.052**
			(0.017)			(0.013)			(0.023)
Adults	18,996	9,569	28,565	10,287	5,182	15,469	8,709	4,387	13,096
Slums	55	40	94	55	40	94	55	40	94
Municipalities		14			14			14	

Table contains information for individuals in common support with non-missing NIDs. Column (1) reports means for displaced adults at baseline, and column (2) for non-displaced adults. Column (3) reports the difference between groups, adjusted for the estimated probability of slum clearance within municipalities ($\hat{p}(X_s) + \psi_o + \hat{p}(X_s) \times \psi_o$). Columns (4)–(6) repeat the exercise for women, and columns (7)–(9) repeat the exercise for men. Standard errors are clustered by slum of origin in parentheses. 10%*, 5%**, 1%***. [a]Years of schooling is observed in the sample of individuals found in the RSH, conditional on an individual being alive after 2007. [b]Formal employment is measured at the slum level using historical data from the Superintendence of Pensions. [c]Child mortality measures whether the individual had a child born alive but died before the age of 1 or the age of 5, in the five years before treatment.

Table 3: Displacement effect on annual mortality of adults (percentage points)

	1[Died after intervention]				
	(1)	(2)	(3)	(4)	(5)
Displaced	0.100*	0.189***	0.266***	0.260***	0.231***
	(0.056)	(0.043)	(0.058)	(0.052)	(0.049)
Adj. R^2	0.000	0.017	0.018	0.018	0.018
Non-displaced mean	0.973	0.973	0.973	1.068	1.068
Percent effect (%)	10.3	19.4	27.3	24.3	21.6
Longevity difference					-2.425
Observations	1,073,057	1,073,057	1,073,057	1,073,057	1,073,057
Individuals	28,558	28,558	28,558	28,558	28,558
Municipality of origin FE	✓	✓	✓	✓	✓
Baseline controls		✓	✓	✓	✓
Slum controls			✓		
$\hat{p}(X_s)$				✓	✓
$\psi_o \times \hat{p}(X_s)$					✓

Notes: Outcome is annual mortality in t conditional on surviving in $t-1$. Coefficients multiplied by 100 to represent percentage points. All regressions include calendar year fixed effects, and year of treatment. Standard errors are clustered by slum of origin in parentheses (94 clusters). Baseline controls include the following: female, woman is head of household, married, cohort fixed effects, number of children per household, Mapuche last name dummy, formal employment at the slum level. Slum characteristics include families per hectare, military name, closeness to rivers/canals, slope, risk of flooding, average schooling and unemployment by census district, number of schools per census district, and distance to the CBD. The row labeled as "Percent effect" stands for percentage variation with respect to the non-displaced mean. The non-displaced mean in columns (4) and (5) is computed conditional on propensity score $\hat{p}(X_s)$. 10%*, 5%**, 1%***.

Table 4: Annual mortality of adults by sex and cohort (percentage points)

	Women	Men	Age at baseline	
			≤ 40	> 40
	(1)	(2)	(3)	(4)
Displaced	0.169***	0.317***	0.217***	0.364**
	(0.048)	(0.073)	(0.043)	(0.148)
Adj. R^2	0.017	0.018	0.009	0.030
Non-displaced mean	0.998	1.163	0.714	2.092
Percent effect (%)	16.9	27.3	30.4	17.4
Longevity difference	-2.118	-2.725	-1.675	-2.068
Observations	597,964	475,093	881,639	191,418
Individuals	15,464	13,094	22,151	6,407
Municipality of origin FE	✓	✓	✓	✓
Baseline controls	✓	✓	✓	✓
$\hat{p}(X_s)$	✓	✓	✓	✓
$\psi_o \times \hat{p}(X_s)$	✓	✓	✓	✓

Notes: Regressions equivalent to column (5) in Table 3. Outcome is annual mortality in t conditional on surviving in $t-1$. Coefficients multiplied by 100 to represent percentage points. All regressions include calendar-year and year-of-treatment fixed effects. Standard errors are clustered by slum of origin in parentheses (94 clusters). Baseline controls include the following: female, woman is head of household, married, cohort fixed effects, number of children per household, Mapuche last name dummy, formal employment at the slum level. The row labeled as "Percent effect" stands for percentage variation with respect to the non-displaced mean. The non-displaced mean is computed conditional on propensity score $\hat{p}(X_s)$. 10%*, 5%**, 1%***.

Table 5: Displacement effect on retirement and disability pensions

	Retirement pension CLP$1,000/month			Disability pension	
	Self-funded	Subsidized	Complementary	1[Pensioner]	Amount CLP$1,000
	(1)	(2)	(3)	(4)	(5)
Panel A. Main sample					
Displaced	-8.604***	1.138	4.398***	0.026***	1.999***
	(1.895)	(0.827)	(0.797)	(0.006)	(0.474)
Adj. R^2	0.039	0.071	0.019	0.018	0.018
Non-displaced mean	42.712	17.560	28.721	0.034	2.787
Percent effect (%)	–20.1	6.48	15.31	76.47	71.72
Panel B. Sample corrected for selection					
Displaced	-8.313***	1.061	4.039**	0.027***	2.328***
	(1.793)	(0.861)	(0.798)	(0.006)	(0.539)
Adj. R^2	0.042	0.065	0.020	0.017	0.017
Non-displaced mean	41.422	19.796	28.883	0.036	3.081
Percent effect (%)	–20.06	5.36	13.98	75.0	75.56
Observations	20,306	20,306	20,306	11,063	11,063

Notes: Regressions equivalent to column (5) in Table 3. Outcomes reported in the upper row. Sample contains one observation per individual computed by collapsing each outcome after controlling for age and semester-year dummies. Data available from 2014 to 2013. Standard errors are clustered by slum of origin in parentheses (94 clusters). Baseline controls include the following: female, woman is head of household, married, cohort fixed effects, number of children per household, Mapuche last name dummy, formal employment at the slum level. The row labeled as "Percent effect" stands for percentage variation with respect to the non-displaced mean. The non-displaced mean is computed conditional on propensity score $\hat{p}(X_s)$. 10%*, 5%**, 1%***.

Table 6: Changes in location attributes and annual mortality

	1[Died after treatment]				
	(1)	(2)	(3)	(4)	(5)
Δ Longevity$_{do}$	-0.013**	-0.012**	-0.015**	-0.015**	-0.018***
	(0.007)	(0.005)	(0.007)	(0.007)	(0.006)
Δ distance to CBD$_{do}$		0.015***			0.012**
		(0.004)			(0.005)
% Slum network			-0.199***		-0.181**
			(0.062)		(0.068)
Δ PCHC$_{do}$				-0.015	-0.028
				(0.022)	(0.019)
Longevity$_o$	-0.012	-0.033**	-0.007	-0.013	-0.026
	(0.015)	(0.016)	(0.014)	(0.015)	(0.016)
Dep. var. mean	1.037	1.037	1.037	1.037	1.037
Adj. R^2	0.018	0.018	0.018	0.018	0.018
Observations	1,073,057	1,073,057	1,073,057	1,073,057	1,073,057

Notes: The table reports coefficients γ in equation (3) in the text. Outcome is annual mortality in t conditional on surviving in $t-1$. Coefficients multiplied by 100 to represent percentage points. All regressions include calendar-year and year-of-treatment fixed effects. Standard errors are clustered by slum of origin in parentheses (94 clusters). Baseline controls include the following: female, woman is head of household, married, cohort fixed effects, number of children per household, Mapuche last name dummy, formal employment at the slum level. The row labeled as "Percent effect" stands for percentage variation with respect to the non-displaced mean. Dependent variable mean is computed conditional on propensity score $\hat{p}(X_s)$. 10%*, 5%**, 1%***.

Table 7: Displacement effect on adults' long-term locations

	Probability of living in ...			Distance	% poor
	assigned municipality	municipality of origin	assigned neighborhood	from assigned neighborhood	in current neighborhood
	(1)	(2)	(3)	(4)	(5)
Panel A.					
Displaced	-0.013	-0.291***	-0.003	-0.308	0.012
	(0.074)	(0.060)	(0.056)	(0.711)	(0.010)
Non-displaced mean	0.587	0.569	0.339	4.047	0.595
Percent effect	**-2.21**	**-51.14**	**-0.9**	**-7.6**	**2.0**
Observations	23,302	23,302	17,952	15,429	17,952

Notes: Regressions equivalent to column (5) in Table 3. Outcomes reported in the upper row. Sample contains one observation per individual computed by collapsing each outcome after controlling for age and semester-year dummies. Outcomes in columns (1) and (2) measured between 2007 and 2023, and outcomes in columns (3)-(5) measured between 2016 and 2023. Standard errors are clustered by slum of origin in parentheses (94 clusters). Baseline controls include the following: female, woman is head of household, married, cohort fixed effects, number of children per household, Mapuche last name dummy, formal employment at the slum level. The row labeled as "Percent effect" stands for percentage variation with respect to the non-displaced mean. The non-displaced mean is computed conditional on propensity score $\hat{p}(X_s)$. 10%*, 5%**, 1%***.

Table 8: Displacement effect on long-term labor market outcomes

	Outcomes in RSH					Outcomes in AFC	
	Employed	Contract	Labor earnings	Formal earnings	RSH Corrected Income	Employed	Wage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Before retirement (age $\leq$ 65)							
Displaced	2.968**	0.515	-11.907***	-6.960	-19.986***	2.401***	17.482*
	(1.324)	(1.440)	(4.478)	(5.962)	(5.586)	(0.708)	(9.168)
Non-displaced mean	54.841	21.967	134.691	66.518	149.795	18.606	173.929
Percent effect (%)	5.4	2.3	–8.8	–10.4	–13.3	12.9	10.1
Observations	12,978	12,978	12,978	12,978	7,258	21,042	21,042
Panel B. After retirement (age > 65)							
Displaced	3.005***	1.482**	0.887	1.877	-12.768***	-0.103	-1.118
	(0.910)	(0.744)	(3.337)	(2.545)	(2.938)	(0.269)	(1.894)
Non-displaced mean	28.436	10.262	59.390	29.724	129.791	3.305	20.628
Percent effect (%)	10.5	14.4	1.7	6.3	–9.8	–3.1	–5.4
Observations	22,222	22,222	22,222	22,222	19,053	23,873	23,873

Notes: Regressions equivalent to column (5) in Table 3. Outcomes reported in the upper row. Sample contains one observation per individual computed by collapsing each outcome after controlling for age and semester-year dummies. Outcomes in the RSH measured between 2007 and 2023, and outcomes in the AFC measured between 2002 and 2023. Standard errors are clustered by slum of origin in parentheses (94 clusters). Baseline controls include the following: female, woman is head of household, married, cohort fixed effects, number of children per household, Mapuche last name dummy, formal employment at the slum level. The row labeled as "Percent effect" stands for percentage variation with respect to the non-displaced mean. The non-displaced mean is computed conditional on propensity score $\hat{p}(X_s)$. 10%*, 5%**, 1%***.

Appendix and Supplementary Material

A Additional Figures and Tables

Figure A.1: Kaplan-Meier survival functions by year after treatment

(a) Women **(b)** Men

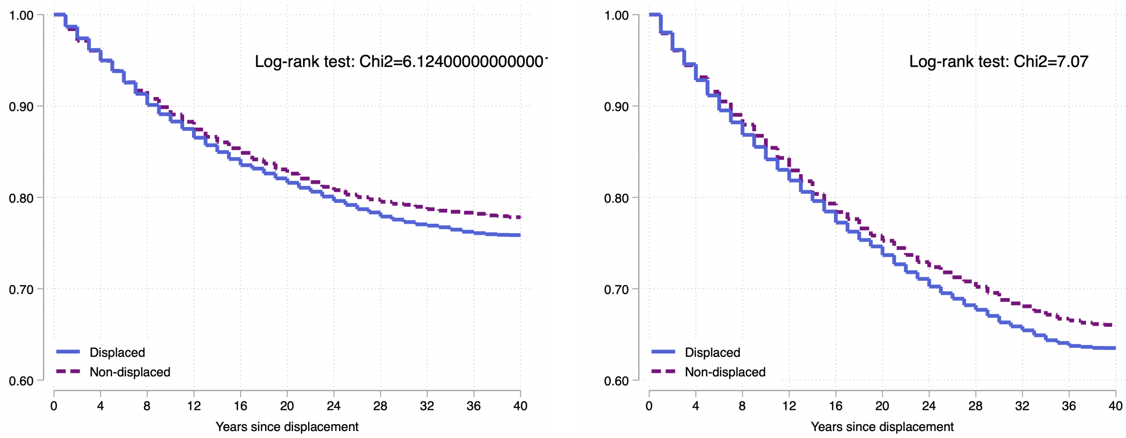


Figure A.2: Kaplan-Meier survival functions by age at death

(a) Women **(b)** Men

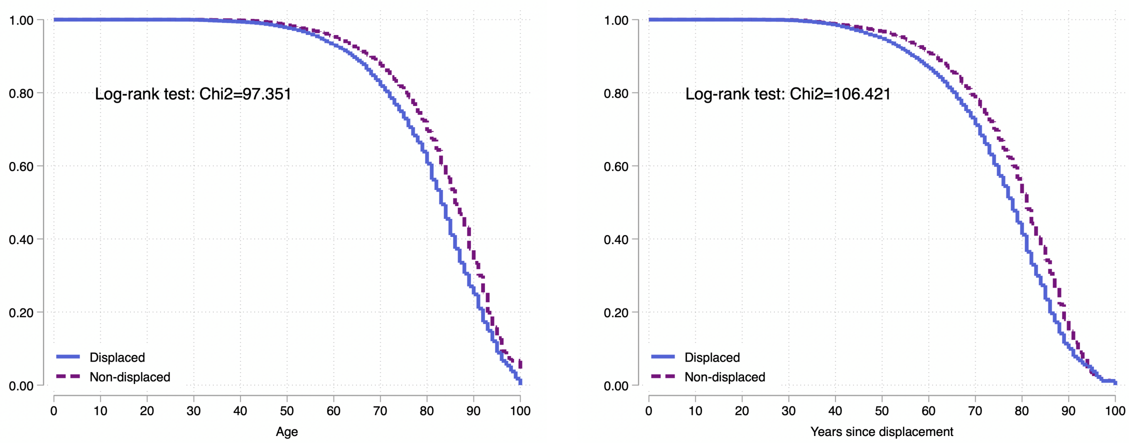
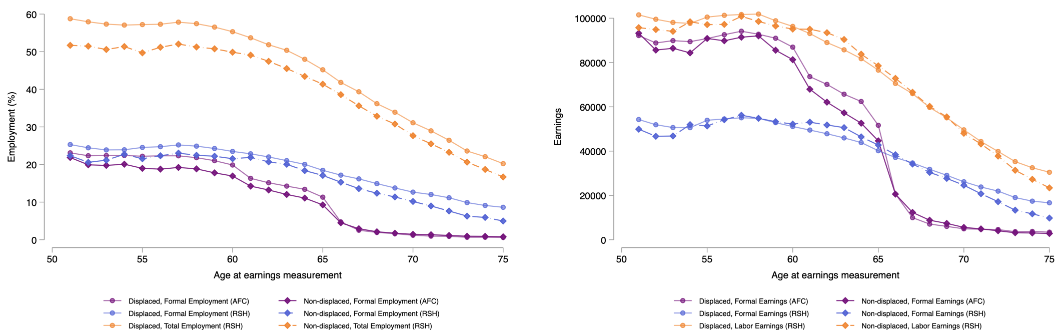


Figure A.3: Labor market outcomes trajectories by age at measurement

(a) Employment **(b)** Earnings



Notes: Sample includes individuals that are matched to the RSH and AFC data between 2007 and 2023. Panels plot the predicted trajectories for displaced and non-displaced individuals between ages 50 and 84 from the regression $y_{it} = \sum_{\tau=50}^{84} \beta_\tau Displaced * 1[Age = \tau] + \sum_{\tau=50}^{84} \delta_\tau 1[Age] + \psi_o + \hat{p}(X_s) + \hat{p}(X_s) \times \psi_o + X'_{it}\gamma + u_{it}$. Baseline controls include the following: woman is head of household, married, age fixed effects, number of children, Mapuche last name, formal employment, and year-of-intervention fixed effects.

Table A.1: Location characteristics after treatment

Location Attributes by Census District	Non-displ. mean (1)	Displaced mean at destin. (2)	Difference (within munic.) (3)	
Longevity (municipality)[a]	65.74	62.46	-2.10	(0.40)***
Schooling HH	7.38	6.51	-1.04	(0.18)***
Unemployed HH	0.19	0.23	0.05	(0.01)***
Primary Care Centers/1000 HH	0.01	0.01	0.01	(0.01)
Hospitals/1000 HH	0.03	0.01	-0.01	(0.02)
Distance to CBD (km)	10.06	13.04	2.81	(0.61)***
Surrounding property prices[b]	14.80	14.56	-0.30	(0.15)**
Home value	256.5	222.03	-35.8	(10.85)***
Individuals			28,565	
Slums			94	

Notes: (a) Measured at the municipality level. (b) Measured in a buffer of 2km around neighborhood of destination. Within difference corresponds to the coefficient of *displaced* in equation (1) conditional on the interaction between municipality of origin fixed effects and the estimated slum propensity score. Clustered standard errors by slum in parenthesis. 10%*, 5%**, 1%***.

Table A.2: Main causes of death Chilean population (DEIS)

Cause of death	2001		2018	
	Males (%)	Females (%)	Males (%)	Females (%)
Cancer	22.46	25.32	25.90	25.70
Lung cancer	3.24	1.89	3.73	2.78
Stomach cancer	4.56	2.78	3.91	2.05
Breast cancer	0.01	2.89	0.01	3.14
Colorectal cancer	1.00	1.46	2.07	2.29
Pancreatic cancer	0.77	1.11	1.30	1.61
Prostatic cancer	3.10	0.00	4.00	0.00
Gynecologic cancer	0.0	3.65	0.00	3.10
Bile duct cancer	1.26	3.84	1.26	2.66
Cardiovascular disease	15.03	15.91	14.18	13.07
Diabetes	3.27	4.50	2.94	3.30
High blood pressure	2.77	4.64	4.73	7.45
External cause	8.81	2.12	6.92	2.45
Alcohol related	4.50	0.78	2.25	0.41
Tobacco related	4.09	2.80	5.20	4.34

Table A.3: Causes in Death Certificates (DC) and Admin. Data (DEIS)

Cause of death	Women		Men	
	DEIS (%)	DC (%)	DEIS (%)	DC (%)
Cancer	31.40	25.70	28.05	24.40
Lung cancer	3.52	4.37	5.57	6.50
Stomach cancer	3.66	2.98	5.18	4.31
Breast cancer	2.84	2.33	0.02	0.02
Colorectal cancer	2.30	1.10	1.71	0.94
Pancreatic cancer	1.67	1.63	1.26	1.27
Prostatic cancer	0.0	0.02	3.12	2.75
Gynecologic cancer	3.97	2.90	0.00	0.0
Bile duct cancer	4.43	3.82	1.47	1.13
Cardiovascular disease	15.12	15.74	15.64	16.62
Diabetes	5.84	0.82 (4.20)	4.90	0.53 (3.18)
High blood pressure	5.62	0.36 (11.69)	4.63	0.30 (9.73)
External cause	2.38	1.49	4.93	5.25
Alcohol related	2.87	1.51	5.69	4.84
Tobacco related	6.67	5.71	8.37	8.88
Undetermined	0.84	1.26	0.86	1.87
Not classified	-	0.3	-	0.4

Table A.4: Variations to the propensity score method

	Outcome: 1[Died after intervention]				
	Baseline	Inv. weight	$p_5 < p < p_{95}$	$p_{10} < p < p_{90}$	Restricted munic.
	(1)	(2)	(3)	(4)	(5)
Displaced	0.231***	0.278***	0.215***	0.168***	0.217***
	(0.049)	(0.058)	(0.050)	(0.056)	(0.050)
Adj. R^2	0.018	0.018	0.018	0.018	0.018
Non-displaced mean	1.068	1.045	1.043	1.045	1.080
Percent effect	21.6	26.6	20.6	16.07	20.1
Observations	1,073,057	1,073,057	1,002,654	859,714	918,865
Individuals	28,558	28,558	26,755	24,005	24,518
# Slums	94	94	87	76	79

Notes: Regression in Column (1) equivalent to column (5) in Table 3. Column (2) estimates the regression using propensity score reweighting. Columns (3) and (4) restrict the common support to the 90 and 80% of the sample. Finally, column (5) drops from the sample 3 municipalities with low common support of the propensity score within municipality. The non-displaced mean is computed conditional on propensity score $\hat{p}(X_s)$. Standard errors are clustered by slum of origin in parentheses (94 clusters). 10%*, 5%**, 1%***.

Table A.5: Assignment location attributes and displaced families' characteristics at baseline

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

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

Table A.6: Changes in location attributes and annual mortality

	By sex		By cause of death			
	Women	Men	Cardiovascular	Cancer	Internal	External
	(1)	(2)	(3)	(4)	(5)	(6)
Δ longevity$_{do}$	-0.013***	-0.009	-0.002	-0.002	-0.000	-0.003
	(0.005)	(0.008)	(0.003)	(0.002)	(0.001)	(0.002)
Δ distance to CBD$_{do}$	0.004	0.022***	0.003	0.002	0.003***	0.007***
	(0.004)	(0.008)	(0.002)	(0.001)	(0.001)	(0.002)
% Slum network	-0.154***	-0.243**	-0.113***	-0.001	-0.008	-0.063*
	(0.057)	(0.098)	(0.041)	(0.028)	(0.013)	(0.036)
Δ PCHC$_{do}$	-0.042**	-0.004	-0.004	-0.001	-0.006	0.004
	(0.018)	(0.028)	(0.010)	(0.008)	(0.005)	(0.011)
Longevity$_o$	-0.022**	-0.035*	-0.009	-0.009**	-0.003	-0.013***
	(0.010)	(0.018)	(0.006)	(0.004)	(0.003)	(0.005)
Adj. R^2	0.015	0.016	0.006	0.003	0.001	0.006
Observations	554,195	444,884	1,073,057	1,073,057	1,073,057	1,073,057

Notes: The table reports regressions equivalent to column (5) of Table 6 stratified by sex in column (1) and (2), and by cause of death in columns (3)-(6). Standard errors are clustered by slum of origin in parentheses (94 clusters). 10%*, 5%**, 1%***.

Table A.7: Displacement effect on the probability of selling home by 2019

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

Notes: Due to our small sample, we compute inverse propensity score estimates in the archival sample of families who received a home in a municipality located in the northern and central areas of Greater Santiago. The data include 45 slums of origin, 9 municipalities of origin, and 15 municipalities of destination. Baseline controls include the following: female-headed household, number of children in family, married head of household, head of household's age, Mapuche head of household, average slums' formal employment, head of household's year-of-birth fixed effects, and year-of-intervention fixed effects. Clustered standard errors by slum of origin are in parentheses. 10%*, 5%**, 1%***.

Table A.8: Displacement effect on long-term labor market outcomes accounting for selection into sample

	Outcomes in RSH					Outcomes in AFC	
	Employed	Contract	Labor earnings	Formal earnings	RSH Corrected Income	Employed	Wage
	(1)	(2)	(3)	(4)		(5)	(6)
Panel A. Before retirement (age ≤ 65)							
Displaced	3.259***	0.563	-8.564**	-5.249	-19.335***	1.889***	12.906
	(1.473)	(1.376)	(4.233)	(5.160)	(5.443)	(0.648)	(8.028)
Non-displaced mean	52.656	20.980	126.440	63.816	151.171	17.209	153.582
Percent effect (%)	6.2	2.7	–6.7	–8.2	-12.8	10.9	8.4
Observations	12,978	12,978	12,978	12,978	7,258	21,042	21,042
Panel B. After retirement (age > 65)							
Displaced	3.081***	1.256*	0.885	1.791	-12.351***	-0.189	-1.822
	(0.907)	(0.673)	(3.113)	(2.239)	(2.861)	(0.255)	(1.636)
Non-displaced mean	26.144	8.770	51.707	24.880	127.467	2.933	17.671
Percent effect (%)	11.7	14.3	1.7	7.2	–9.6	–6.4	–10.1
Observations	22,222	22,222	22,222	22,222	19,053	23,873	23,873

Notes: 10%*, 5%**, 1%***.

B Eviction Policies

Table B.1: Characteristics of each version of the program

Intervention	**Location**	Property right	Type of dwelling	Public services	Cost for family
Non-displaced (1/3) (urban renewal)	**Same**	Yes	Starting kit (*) or apartment	Yes	25% paid in 15 years
Displaced (2/3) (evicted)	**New (periphery)**	Yes	Apartment or house	Yes	25% paid in 15 years

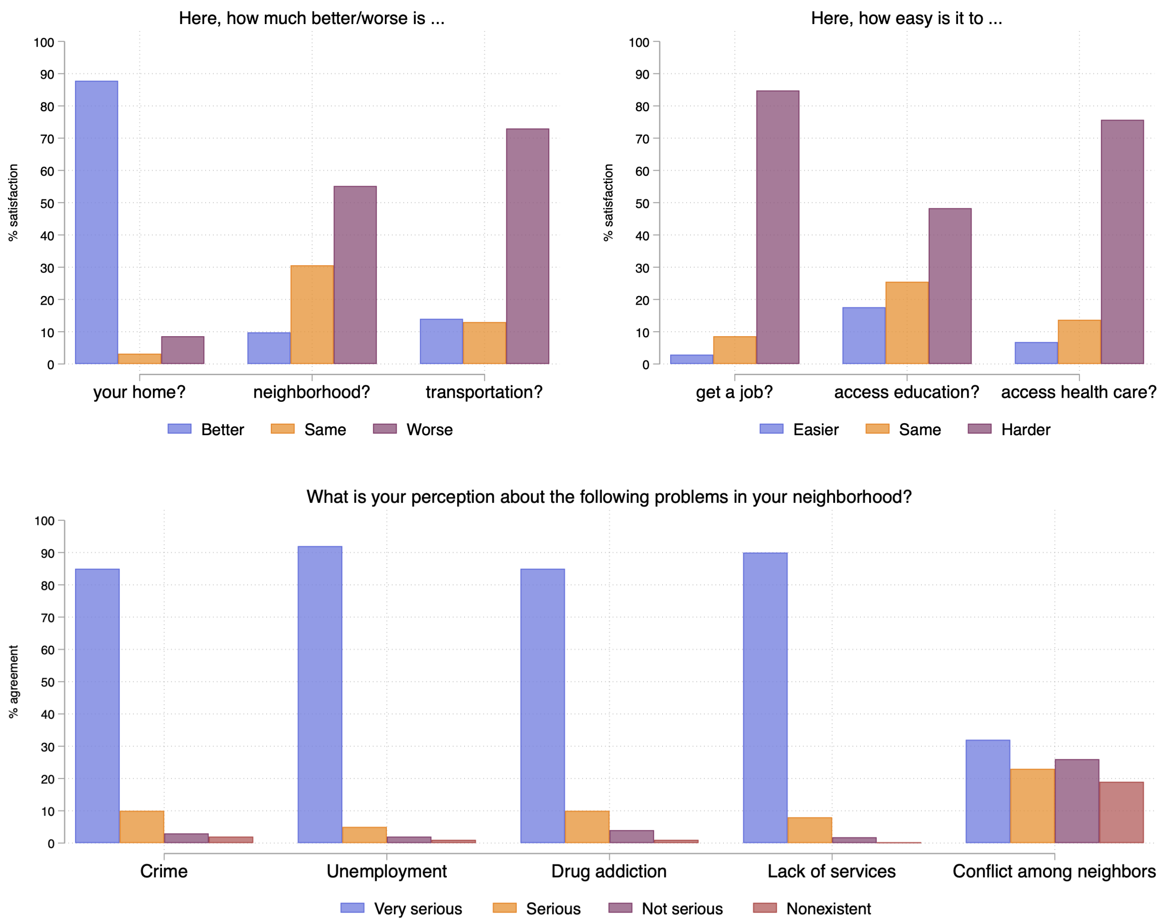
(*) A starting kit includes a living room, a bathroom, and a kitchen.

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

Notes: Examples of neighborhoods from Hidalgo (2019).

B.1 Evaluation of evictions program in 1987

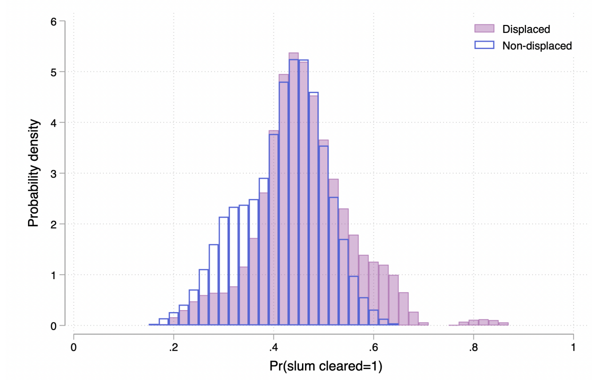
Figure B.2: Summary of evaluation of the Program for Urban Marginality (Aldunate et al., 1987)



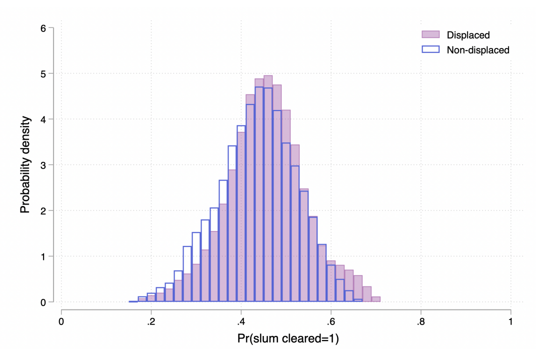
Notes: Summary of results found by Aldunate et al. (1987). The authors interviewed 592 displaced slum dwellers that were relocated into four new neighborhoods.

C Attrition

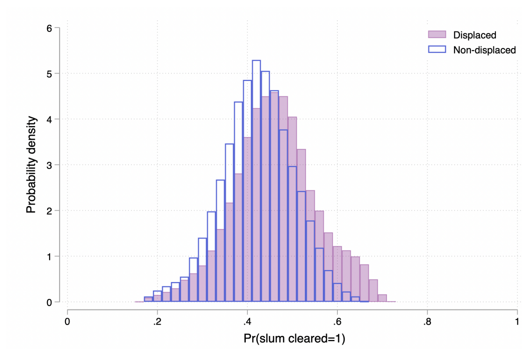
Figure C.1: Distribution of the probability of slum clearance versus redevelopment



(a) Full sample of urban slums



(b) Urban slums in Archives



(c) Urban slums in Archives (weighted)

Notes: Panel (a) plots the fitted values of a LASSO logit regression of the probability of slum relocation on slums' attributes in Table (1) for the full sample of slums by treatment. The LASSO estimation selects as determinants of relocation slum density, military name, elevation, and average population schooling. Panel (b) plots the propensity score estimates in the sample of slums found in the archives. Finally, panel (c) reweights the densities in panel (b) by the inverse probability of finding a slum in the archives. The probability of finding a slum in the archives is computed using a logit model on slums' characteristics and stratified by treatment.

Table C.1: Comparing displaced and non-displaced individuals in Archival records

	All adults in Archives				Adults with NID			
	Displaced mean	Non-displaced mean	Difference	Difference weighted	Displaced mean	Non-displaced mean	Difference	Difference weighted
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female	0.527	0.516	0.010***	0.009***	0.531	0.529	0.003	0.003
			(0.003)	(0.003)			(0.002)	(0.002)
Female household head	0.370	0.355	0.012	0.006	0.360	0.343	0.014	0.007
			(0.022)	(0.024)			(0.023)	(0.025)
Mapuche lastname	0.056	0.044	0.016***	0.014***	0.057	0.045	0.016***	0.015***
			(0.004)	(0.003)			(0.004)	(0.003)
Two partners in sample	0.889	0.851	0.028*	0.025	0.906	0.932	-0.027***	-0.025***
			(0.017)	(0.017)			(0.005)	(0.005)
Missing NID	0.053	0.123	-0.061***	-0.056***	0.000	0.000	–	–
			(0.014)	(0.014)			(–)	(–)
Observations	23,180	11,325	34,505	34,505	21,941	9,928	31,869	31,869
Households	12,874	6,491	19,365	19,365	10,888	5,380	16,268	16,268
Slums	58	40	98	98	58	40	98	98
Municipalities	14	14	14	14	14	14	14	14

Notes: Sample includes all individual found in archival data in urban municipalities with variation in treatment. Column (1) reports means for displaced adults at baseline and column (2) for non-displaced adults. Column (3) reports the simple difference between groups, and column (4) reports the difference between treatment weighted by the inverse probability of finding a slum in the archival data. Columns (5)–(8) repeat the previous exercise in the sample of individuals with non-missing NIDs. Variable "Female household head" is computed as the person who receives the property deed. Standard errors are clustered by slum of origin in parentheses. 10%*, 5%**, 1%***.

Table C.2: Displacement effect on annual mortality of adults after correcting for attrition

	1[Died after intervention]				
	All	All	All	Women	Men
	(1)	(2)	(3)	(4)	(5)
Panel A. Weighted by attrition from slum					
Displaced	0.197***	0.199***	0.146***	0.069**	0.241***
	(0.051)	(0.052)	(0.044)	(0.033)	(0.074)
Non-displaced mean	0.760	0.733	0.733	0.650	0.844
Percent effect (%)					
Panel B. Weighted by attrition from missing NIDs					
Displaced	0.178***	0.178***	0.171***	0.138***	0.278***
	(0.039)	(0.039)	(0.034)	(0.035)	(0.077)
Non-displaced mean	0.644	0.757	0.757	0.680	1.032
Percent effect (%)	27.6	23.5	22.6	20.3	27.0
Observations	1,073,057	1,073,057	1,073,057	597,964	475,093
Individuals	28,558	28,558	28,558	15,464	13,094
Municipality of origin FE	✓	✓	✓	✓	✓
Baseline controls	✓	✓	✓	✓	✓
$\hat{p}(X_s)$		✓	✓	✓	✓
$\psi_o \times \hat{p}(X_s)$			✓	✓	✓

Notes: Results equivalent to Tables 3 and 4. Panel A. reweights the main sample by the inverse probability of finding a slum in the archives. Panel B. reweights the main sample by the inverse probability of having a non-missing NID. Standard errors are clustered by slum of origin in parentheses (94 clusters). 10%*, 5%**, 1%***.