Displacement and Infrastructure Provision: Evidence from the Interstate Highway System *

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Abstract

This paper studies the long-run effects of displacement caused by the largest public works project in U.S. history, the Interstate Highway System. I develop a method to identify individuals living in destroyed homes in the 1950 census and link them to administrative mortality records. I document that highway construction disproportionately affected vulnerable communities. Comparing affected individuals to their unaffected neighbors, I find that displacement reduces longevity and causes relocation to worse neighborhoods. These effects spill over to residents living next to the highway. Relocation assistance payments, adopted by states over time, fully offset the mortality effects for displaced individuals.

JEL Codes: N92, R23, R42, J15, I14.
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1. Introduction

Infrastructure provision is a key driver of economic growth and regional development, yet its construction often requires the displacement of large numbers of local residents.[1] The World Bank estimates that infrastructure construction displaced about 20 million people per year between 2011 and 2020 worldwide (Cernea and Maldonado, 2018). These costs fall disproportionately on communities with the least economic and political power to resist it (Penz et al., 2011). While there is abundant evidence on the benefits of infrastructure for cities and regions, we know remarkably little about what happens to the individuals displaced by these projects and to the communities they leave behind, particularly in the long run.

This paper studies the long-run consequences of infrastructure provision for displaced individuals and for residents of the communities where these projects are built. To do so, I examine the construction of the Interstate Highway System, the largest public infrastructure project in U.S. history, which is estimated to have displaced more than one million people between 1956 and 1970 (Schwartz, 1975). Interstate construction proceeded rapidly and without any relocation assistance, leaving those whose homes were destroyed to bear the full costs of displacement without government support. Despite its scale, the identities and locations of displaced households were never systematically documented, making it hard to empirically study the effects of displacement. A large literature outside economics has documented how highway construction reshaped neighborhoods and communities (Rothstein, 2017; Lewis, 2013), but estimating the costs borne by displaced individuals poses a distinct challenge: unlike the benefits of highway access, which accrue to places, the costs fall on people who are often no longer in the same location. Identifying these costs requires tracking individuals over extended periods.

I address this challenge by developing a new method to identify individuals displaced by Interstate construction using the 1950 full-count census. By geocoding the universe of urban residents in metropolitan areas, I classify displaced individuals as those living in homes subsequently destroyed by highway construction. This approach provides one of the first systematic estimates of displacement at scale: approximately 456,000 individuals resided in homes that would be destroyed by Interstate construction between 1956 and 1970. To study long-run outcomes, I link individuals in the 1950 census to administrative death records from 1988 to 2007, which allows me to observe both pre-construction residence and post-construction longevity and location at death. The dataset allows me to conduct the first long-run study of those directly displaced by Interstate construction, as well as the effects on those residing in nearby communities.

[1] The Interstate Highway System increased trade and growth in the U.S., raising welfare (Duranton et al., 2014; Allen and Arkolakis, 2014). Similar patterns arise for port development (Ducruet et al., 2024; Brooks et al., 2021) and for airport hubs (Redding et al., 2011).

Before turning to the long-run effects, I show that highway construction disproportionately affected vulnerable individuals. Examining how pre-construction characteristics vary with distance to newly constructed highway segments, I document that residents within two kilometers were more likely to be Black, lived in lower-value housing, earned lower wages, had lower educational attainment, and were less likely to own their home. These patterns do not stem from the sorting of vulnerable populations into neighborhoods that Interstates were meant to connect, such as downtown areas (Boustan, 2010). Using the placement of federal plans, which were also routed along primary roads and city centers, I find no systematic differences in pre-construction characteristics along planned but unbuilt routes. Instead, deviations from the federal plans resulted in construction disproportionately affecting vulnerable communities. These findings are consistent with historical accounts documenting that officials deliberately displaced minority communities through highway construction, often without providing any relocation assistance (Rose and Mohl, 2012).

To identify the long-run effects, the empirical strategy exploits within-neighborhood variation in proximity to the highway. While federal guidelines prioritized building along primary roads, there is ample historical evidence that state highway departments deviated from these guidelines to target entire communities (Rose and Mohl, 2012; Lewis, 2013). Given that state builders focused on entire neighborhoods, the exact distance from each household in the neighborhood to the constructed highway is unrelated to individuals' outcomes. However, construction may affect residents beyond those directly displaced, as highways increase noise, congestion, and pollution, which may have long-run effects on nearby residents (Brinkman and Lin, 2024; Currie and Walker, 2011). Examining how long-run outcomes vary with distance to newly constructed segments, I document highly localized spillovers. Relative to residents living beyond two kilometers, those within 100 meters had lower life expectancy and were more likely to leave the neighborhood. Beyond 100 meters, all long-run outcomes are indistinguishable from those of unaffected residents. I therefore identify the long-run effects of highway construction by comparing displaced individuals and those living within 100 meters to unaffected neighbors living between 100 and 200 meters from the constructed highway. After accounting for the factors that state builders considered in placement, namely the race and socioeconomic composition of the neighborhood, displaced and adjacent individuals are similar to their unaffected neighbors across a range of pre-construction characteristics. When linked to the 1940 census, these groups are also on parallel trends prior to construction.

My findings indicate that highway construction reduces the longevity of displaced individuals and those living adjacent to the Interstate. Both treatment groups die approximately 0.08 years earlier than their unaffected neighbors, equivalent to a reduction of roughly one month in life expectancy. These effects are concentrated among those who did not survive past age 80. The magnitude is comparable to the mortality effects of other traumatic life events, such as divorce, and is approximately a third of the longevity differences between the homeless and housed populations (Sbarra et al., 2011; Meyer et al., 2023). Although both groups experience similar mortality reductions, the underlying channels differ. A Gelbach (2016) decomposition indicates that relocation farther from the origin community and disruption of social networks account for approximately one quarter of the mortality effect among displaced individuals, but explain little of the effect among adjacent residents.

Interstate construction not only reduces longevity but also shapes where affected individuals live. Displaced individuals are 2.6 percentage points less likely to reside in their 1950 neighborhood at the time of death, but 1.3 percentage points more likely to remain in the same metropolitan area. They also live in lower-quality neighborhoods, as measured by a composite index of sociodemographic characteristics. Adjacent individuals show smaller but qualitatively similar patterns of mobility, with no evident effect on neighborhood quality.

The findings are robust to two alternative empirical strategies that address potential concerns with the neighbor comparison design. A first concern is that the control group, given its proximity to constructed highways, may itself have been affected by construction. Although I find spillovers only within 100 meters, those in the control group may be affected in ways this analysis does not fully capture. For example, if demolition of the local housing stock and the influx of displaced residents raised housing costs, individuals in the control group may themselves have been affected. I address this by comparing affected individuals to a matched sample living more than 2,000 meters from any built highway, matched on individual, household, and neighborhood characteristics. The second concern is that, because highways were often routed through preexisting major roads, residents along those roads may have differed systematically from nearby neighbors even before construction. I address this using individuals along planned but never-built highway routes as an alternative control group. Since these planned routes share the same selection characteristics as those ultimately constructed, following major roads and passing closer to city centers, any systematic differences along these dimensions cannot account for the results. Across all outcomes, the three strategies yield consistent results, lending credibility to the causal estimates.

Finally, I examine whether relocation payments could mitigate the negative consequences of displacement. Beginning in the early 1960s, states gradually adopted relocation assistance programs that provided payments to displaced tenants. These payments were small, capped at $200, with an average of $110 in 1967 ($740 in 2010), and were intended to cover the moving expenses associated with relocating to a new home (U.S. Department of Transportation, 1967). I build a novel database of the year each state enacted legislation providing relocation assistance to those displaced by construction. By comparing individuals within the same state who were displaced with and without relocation payment laws, I show that relocation payments fully offset the mortality effects of displacement. I do not find evidence that relocation assistance affected those adjacent to newly constructed highways, consistent with the fact that they were not eligible for payments. Crucially, the effect of relocation assistance is larger for displaced individuals than for those adjacent to the highway, consistent with payments operating through direct compensation of displacement costs rather than through broader neighborhood-level channels. The results suggest that the introduction of payments had a large return, as a cost-benefit calculation indicates that each dollar spent on relocation payments yielded $13.2 in benefits from mortality alone.

This paper contributes to the literature on the long-run effects of displacement on individuals by studying displacement resulting from policy decisions. While the literature has documented positive effects of displacement on earnings and health when it encourages individuals to move to higher-opportunity locations (Nakamura et al., 2021; Deryugina et al., 2018; Deryugina and Molitor, 2020; Sarvimäki et al., 2022), I find that displacement generated by highway construction had large negative effects on the longevity of those displaced.[2] An important objective of ongoing research in this literature is to understand the sources of these heterogeneous effects and, in particular, how to mitigate them and protect affected populations. My results suggest that the negative effects of displacement are not driven by changes in local economic conditions, since displaced individuals overwhelmingly remain in the same city, but rather by the disruption of established living arrangements in the absence of relocation assistance. Understanding the costs of displacement by eminent domain is important as the practice remains highly contested, particularly since *Kelo v. City of New London* (U.S. Supreme Court, 2005), which upheld its use for private projects deemed to benefit the public. By separating the effect of forced relocation from relocation assistance, I provide evidence on both the consequences of displacement and the role of policy in mitigating them.

My work also contributes to a growing literature on the rising costs of infrastructure provision in the United States (Liscow, 2025; Mehrotra et al., 2024). This paper is among the first to provide quantitative evidence on the non-pecuniary costs of displacement from infrastructure construction. A back-of-the-envelope calculation suggests that the mortality cost alone amounts to approximately $10,000 per displaced individual (in 2010 dollars), which would have increased the total cost of early interstate construction by more than $4.5 billion. This estimate is likely a lower bound, as it captures neither the costs borne by nearby residents who were not displaced nor the broader effects of community disruption. By documenting these costs and showing that relocation assistance can sub-

[2]These papers study unanticipated events such as natural disasters or wars, which involve large changes to local economies with substantial general equilibrium effects, and often feature monetary assistance or other support for affected individuals. By contrast, I study a policy-induced displacement that affected a small share of the population in each city and initially offered no relocation assistance. Rojas-Ampuero (2025) also finds negative effects of displacement, showing that slum-dwellers relocated to the city's outskirts die younger than those resettled within the same community.

stantially mitigate them, this paper informs the design of compensation policies, which Brooks and Liscow (2023) identify as a significant driver of rising construction costs, and the cost-benefit analysis of large infrastructure projects more broadly.

Furthermore, this paper contributes to the extensive literature on the consequences of Interstate construction (Baum-Snow, 2007; Duranton et al., 2014; Herzog, 2021; Brinkman and Lin, 2024; Bagagli, 2023; Valenzuela-Casasempere, 2024). While this literature focuses on the places that received highways, I study the long-run effects on the individuals affected by their construction. I find that displacement has lasting negative consequences both for displaced individuals and for residents of the communities where these projects were built. These distributional costs complement the aggregate welfare gains documented in previous work (Weiwu, 2025). The negative effects documented for displaced individuals, who were forced to relocate from locations they had chosen, are consistent with a decrease in their overall utility, as predicted by spatial equilibrium models of residential sorting (Rosen, 1974; Roback, 1982).

Finally, this paper contributes to the literature on the racial legacy of US government policies. Recent work has documented how federal programs affected racial groups differently, either by narrowing income gaps (Derenoncourt and Montialoux, 2020; Aizer et al., 2020) or by reinforcing existing inequalities through biased implementation (Althoff and Szerman, 2025). This study is among the first to provide systematic quantitative evidence on the role of race in the placement of US highways. Earlier work by Carter (2023) found that Interstates in Detroit were routed through low-value properties. In comparison, my study uses micro-level data rather than census tract data to analyze racial patterns in highway placement across all metropolitan areas, rather than a single city.

2. Historical Context

This section provides historical context on the Interstate Highway System, the factors that shaped routing decisions, and the consequences for displaced households.

2.1 The Interstate Highway System

The Interstate Highway System was established by the Federal-Aid Highway Act of 1956. The bill authorized more than 70,000 kilometers of highways, funded 90% by the Federal Government, while leaving routing decisions to state and local officials. The legislation included no provisions for the relocation of individuals displaced by construction beyond condemnation payments, largely because policymakers feared these provisions would be too costly (Lewis, 2013).[3] To facilitate congressional approval, the Federal Government released a set of proposed urban routes for 100 cities in the "Yellow Book."[4] These routes reflected federal engineers' efforts to create a connected national network minimizing the cost of construction and were usually routed through primary roads. As the main purpose was to minimize cost, there routes were not influenced by local political considerations beyond geography and land values (Weiwu, 2025). The Yellow Book routes, however, were only advisory. State highway agencies had the full authority over the final placement of urban interstates, which allowed state and local officials to use highway placement to push for their own political priorities (Rose and Mohl, 2012, p. 97).[5]

State officials frequently envisioned highways as instruments for clearing areas labeled "blighted," often targeting predominantly Black neighborhoods (Guerra, 2025). In Miami, for example, Interstate 95 was routed directly through Overtown, the center of the city's Black economic and cultural life. Planners rejected an alternative route along an abandoned railway right-of-way that would have minimized displacement (Rothstein, 2017).[6] Contemporaneous assessments predicted that interstate construction would displace more than one million people, disproportionately affecting Black households (Rose and Mohl, 2012, p. 96). Both federal and local agencies provided little assistance in securing new housing. As a result, displaced families were often forced to relocate to the urban periphery or to emerging "second ghettos" (Archer, 2020).

Opinions diverged over the consequences of displacement. Supporters argued that relocation would move families out of deteriorating neighborhoods and into better housing, claiming that highway construction "launched otherwise contained Black folk into the suburbs" (Connolly, 2014, p. 282–283). Critics countered that relocation rarely produced such benefits. Highway construction occurred amid peak levels of racial segregation (Cutler et al., 1999), sharply limiting the housing options available to displaced Black households. At the same time, interstate construction destroyed a large stock of affordable housing, worsening the constraints facing low-income families (Archer, 2020). Combined with the absence of financial and advisory support, displaced households frequently relocated to areas with lower-quality housing, greater distance from employment centers, and limited access to economic opportunities.

[3]In 1956, Arthur Burns, chairman of the Council of Economic Advisors, warned that compensating displaced residents would be prohibitively expensive, as the highway program was expected to evict nearly one hundred thousand people per year (Archer, 2020).

[4]The report's official title is "General Location of National System of Interstate Highways, Including All Additional Routes at Urban Areas." Appendix Figure A.5 illustrates the maps for Atlanta, Detroit, Miami, and New Orleans.

[5]Figure A.6 compares planned and built highway networks for Atlanta, Detroit, Miami, and New Orleans. While constructed routes broadly matched the intended origins and destinations, there were notable deviations in their precise alignments relative to the initial plans.

[6]Similar patterns emerged in cities with relatively small Black populations. In St. Paul, Minnesota, Interstate 94 cut through the city's Black neighborhood, displacing one-seventh of its African American residents. As one critic observed, "very few Black individuals lived in Minnesota, but the road builders found them" (Rose and Mohl, 2012, p. 108).

Growing recognition of hardships caused by displacement prompted local opposition to highway construction which led Congress to introduce relocation assistance requirements, though implementation proved gradual and uneven. The Federal Government introduced the first relocation-assistance requirements in 1962, seven years after construction began.[7] The Federal-Aid Highway Act of 1962 required states to offer relocation payments and advisory services as a condition for receiving federal highway funding. However, the legislation did not take effect until July 1965, when relocation assistance became available with a maximum payment of $200 for displaced families. Even then, adoption varied substantially across states: by mid-1965, only fifteen states had established programs, as shown in Appendix Table F.1. States faced significant implementation barriers, including constitutional prohibitions on certain types of payments and limited administrative capacity for managing relocation services (Highway Research Board, 1967). As of January 1, 1967, thirty-three states and the District of Columbia had authorized the payment of moving expenses for displaced households. These obstacles generated staggered adoption of relocation assistance programs between 1965 and 1971, when the Uniform Relocation Assistance and Real Property Acquisition Act mandated standardized procedures across all federal projects.[8]

Evidence from the Highway Research Board (1967) evaluation shows that the early program had limited reach. Of the nearly fifty thousand people displaced between April 1965 and October 1966, only 37% sought advisory assistance, and only 15% secured housing through this aid. Average relocation payments were roughly $100, an amount insufficient to cover the costs of moving (Highway Research Board, 1967). In addition, states differed substantially in when they began implementing relocation payments and in the generosity of the support offered.[9] For example, New York and West Virginia authorized relocation payments shortly after the 1962 Act, while states such as Texas and Indiana delayed implementation until the end of the decade, creating substantial cross-state differences in the support available to displaced households. By the time relocation programs became operational, most urban interstate segments had already been constructed, and the majority of displacement had already taken place.

Taken together, the qualitative evidence suggests that early highway construction imposed substantial and uneven costs on displaced households, with limited institutional support to mitigate these effects.

[7] Throughout early construction, homes were acquired by eminent domain. State highway departments negotiated purchases or condemned properties to secure right-of-way, providing "just compensation" at fair market value for real estate. No federal requirement existed to offset the practical costs of moving or to help families secure replacement housing.

[8] Congress expanded relocation assistance in 1968 before the comprehensive 1971 reform.

[9] As of January 1, 1967, thirty-three states and the District of Columbia had authorized the payment of moving expenses for displaced households. Among these, twenty-one states aligned their payments with the reimbursement provisions of the Federal-Aid Highway Act of 1962, while thirteen adopted either more generous or less generous payment levels (Highway Research Board, 1967). Appendix Table F.1 reports the dates at which states introduced relocation payments.

3. Data

The major empirical challenge is identifying individuals displaced by Interstate construction and observing their long-run outcomes. To do so, I geocode address information in the 1950 census for all residents in the 168 metropolitan areas and overlay the highway network to determine which individuals were living in homes destroyed by highway construction each year. I then link these individuals to administrative mortality records from 1988 to 2007.

3.1 Measuring highway-induced displacement

No systematic records of households displaced by highway construction exist. Therefore, I develop a novel approach to identify displaced individuals using historical address information from the 1950 U.S. census. Throughout this paper, I define displacement as those individuals whose homes were destroyed by highway construction, as they were living under what became the highway right-of-way.

The 1950 census recorded the address of each household. Following Logan and Zhang (2018), I standardize and geocode these addresses to obtain latitude and longitude coordinates for each home in the 168 Standard Metropolitan Areas (SMAs).[10] Within this set, I successfully geocode 58.53% of total households. Panel (a) of Appendix Figure A.7 presents an example of the geocoded data, showing the location of homes in a neighborhood in Cleveland, Ohio, in 1950. Appendix Section A provides additional details on the sample and the geocoding process, as well as the performance by state.

Identifying displaced households requires the location and opening date of each highway segment. This information comes from the Federal Highway Administration's PR-511 database linked to the built highway network from OpenStreetMap (Baum-Snow, 2007; OpenStreetMap, 2017). Because I observe individuals' locations in 1950 but highway construction began in 1956, I focus on segments opened between 1956 and 1970, the years of most intense construction, as shown in Appendix Figure A.8.[11] However, some families may have moved before construction began, which would introduce classical measurement error and attenuate the estimated effects of displacement. Appendix Section A.4 presents a bounding exercise showing that the *true* effects are bounded between the estimates and three times their magnitude.

To estimate the right-of-way acquisition for highway construction, I assume a two-lane highway had a width of 150 feet (45.7 meters), the minimum width used in the 1950s

[10] The geocoding is done using ArcGIS's Street Map Premium 2024 locator. Street Map Premium performs geocoding on the local computer, thereby avoiding data transmission to the cloud.

[11] Appendix Figure A.9 shows that the results are robust to including segments opened after 1970 or only those opened before 1960.

(Weingroff, 2017).[12] For segments with more than two lanes, I add 12 feet (3.66 meters) per additional lane, equal to the average lane width (Federal Highway Administration, 2007).[13] Using these width assumptions, I construct a buffer around each highway segment representing the right-of-way area and calculate each household's distance to the nearest segment. Displaced households are those whose 1950 residence fell within this right-of-way area. I apply the same procedure to planned highway routes from the 1955 Federal engineering maps, also known as the Yellow Book, which covered 100 metropolitan areas (Bureau of Public Roads, US, 1955). Thus, for each household I observe whether it was displaced by highway construction, the distance to the nearest highway segment, and whether it lay within the planned route. Panel (b) of Appendix Figure A.7 shows the highway network in Cleveland, Ohio. Solid red lines represent highway segments, and the light red area corresponds to the right-of-way buffer.

Identifying displaced individuals depends on accurate coordinates. Because highway construction destroyed street segments, and street names or numbering may have changed over time, using a modern geocoder to locate historical addresses can misclassify individuals' locations. I address this concern in three ways. First, I restrict the sample to addresses with high-quality matches, i.e., geocoding score above 85, which reduces classification error.[14] Second, I construct a historical geocoder for nine cities using 1940 street grids that predate highway construction, providing benchmark coordinates from pre-highway street grids.[15] The two geocoders yield highly correlated locations, with more than 65% of matches falling within 40 meters (Appendix Figure A.2) and with no directional bias in discrepancies between the two geocoders (Appendix Figure A.3). At the level of treatment classification, Appendix Figure A.4 shows that the two geocoders agree closely across distance bands, with more than 70 percent of observations classified in the same 100-meter bin. Third, in Appendix Figure A.9, I re-estimate the main results using the historical geocoder, yielding quantitatively similar estimates.

Together, these checks suggest that high geocoding quality and that the few differences can be attributed to classical measurement error that causes attenuation bias. More details on these analyses can be found in Section A.3.

[12]The modal and median number of lanes in the sample is two.

[13]If actual right-of-way widths exceeded my assumptions, some displaced individuals would be misclassified as adjacent, and the estimated spillover effects on adjacent individuals could partly reflect this misclassification. However, the estimated spillovers are robust to dropping individuals living within 25 meters of the highway (Appendix Figure A.9).

[14]ArcGIS's Street Map Premium provides a geocoding score from 0 to 100 that indicates the quality of the match. A score above 85 indicates a high-quality match, such as an exact match to a street segment or parcel. Appendix Figure A.9 shows that the results are robust to increasing the quality threshold.

[15]The historical street grids were created by the *Urban Transition Historical GIS Project* and are available at https://s4.ad.brown.edu/Projects/UTP2/citymaps.htm. Accessed February 10, 2026.

3.2 Linkage to administrative mortality records

To observe individuals after highway construction, I link the 1950 census to the Numerical Identification (NUMIDENT) database. These administrative mortality records contain detailed information on age at death, reliance on Social Security, and nine-digit ZIP codes of last residence. The database covers nearly all deaths among American citizens aged 65 and older between 1988 and 2007, though coverage is incomplete before 1988 (Breen and Goldstein, 2022). Accordingly, the analysis focuses on individuals 65 or older who died between 1988 and 2007.[16]

One advantage of NUMIDENT records is the inclusion of deceased parents' names, which allows the linkage of women who changed their last name after marriage. Additionally, nine-digit ZIP codes provide granular location indicators that refer to "a segment or one side of a street" (U.S. Postal Service, 2024). These ZIP codes allow me to observe the socioeconomic characteristics of neighborhoods at death by linking to the National Historical Geographic Information System (NHGIS) for the year 2000, near the midpoint of the mortality window.

To maximize the linkage between the 1950 census and the NUMIDENT records, I combine two linkage methods. The first, the IPUMS Multigenerational Longitudinal Panel version 2.0 (Ruggles et al., 2025), uses machine learning to create links across databases. The second uses the "ABE-EI" algorithm developed by the Census Linking Project (CLP) (Abramitzky et al., 2022, 2024), which matches individuals based on time-invariant characteristics and uses extra information to resolve ties. For individuals who cannot be linked directly from the 1950 census to the NUMIDENT, I use Ruggles et al.'s (2025) census-to-census crosswalk to link them to the 1940 census and then match the resulting 1940 records to the NUMIDENT dataset. When both methods link the same Social Security Number to different individuals in the 1950 census, I use the direct 1950-NUMIDENT link.

I combine the linked data as follows: of the 1950-NUMIDENT links identified by either MLP or CLP, 1.1% were identified in both MLP and CLP crosswalks, 81.6% were in MLP only, and 17.3% were in CLP only. For the cases where the MLP and CLP links differed, I used the MLP link, although the results are robust to using CLP links instead. Appendix Figure A.9 shows that the results do not change when using only MLP links or CLP links, but the sample size decreases, increasing standard errors, and some results are not significant at the usual levels.

I do not find evidence of systematic linkage rates by displacement status. Appendix Table A.2 shows the probability of linkage to NUMIDENT by treatment status. The results show that displaced individuals are slightly less likely to be linked, while adjacent individuals are slightly more likely to be linked than the rest of the sample. However, the differences are small in magnitude, and barely statistically significant after controlling for

[16] Appendix Figure A.9 shows that the results do not depend on these restrictions.

metropolitan area fixed effects and demographic characteristics.

One concern when working with linked samples is that the linked population may not be representative of the entire population. Following Bailey et al. (2020), I show that the results are robust to reweighting the observations by the inverse of the probability of being geocoded and linked (Appendix Figure A.9). Another concern is that the NUMIDENT records undercount the total number of deaths for younger cohorts. I use the mortality-adjusted weights developed by Breen et al. (2023), which account for these differences in coverage, to show that these differences do not explain my results (Appendix Figure A.9).

4. Who was affected by highway construction?

A large historical and sociological literature argues that Interstate construction disproportionately affected low-income and Black communities (Rose and Mohl, 2012; Lewis, 2013; Rothstein, 2017; Archer, 2020). Yet no systematic records of displaced individuals exist, leaving both their number and socioeconomic characteristics largely unknown.[17] The dataset I build allows me to address both questions directly. I estimate that Interstate construction displaced 456,405 individuals between 1956 and 1970, or an average of 30,427 per year. These figures represent a lower bound, as the sample covers only urban residents in metropolitan areas and geocoding of the full household sample is incomplete. The estimates align closely with official figures reported in 1967, when the federal government estimated that highway construction relocated an average of 33,070 individuals per year between April 1965 and October 1966 (U.S. Department of Transportation, 1967, pg. 2). The correspondece between my estimates and the official figures validates the accuracy of the geocoding procedure and, by extension, to the methodology used to identify those displaced. I use these records to describe the socioeconomic profile of displaced individuals and their communities, something the historical literature has been unable to do systematically.

To characterize how pre-construction socioeconomic characteristics vary with distance from the highway, I estimate regressions of the form:

$$y_i = \beta_0 \text{ Displaced}_i + \sum_{k \in \mathcal{K}} \beta_k \text{ Bin}_i^k + \mathbf{X}_i' \, \Gamma + \varepsilon_i \tag{1}$$

where y_i is an individual outcome measured in the 1950 census, Displaced_i indicates individuals whose homes were demolished by highway construction, and Bin_i^k are indicators for 100-meter distance bins measured from the future highway. For instance, Bin_i^{500} equals one if the individual lived between 400 and 500 meters from a future Interstate in 1950.

[17] Federal and state agencies did not collect data on who was displaced primarily because relocation payments were not required, and because accurate counts would have imposed legal liability on planners and authorities (Schwartz, 1975, pg. 235).

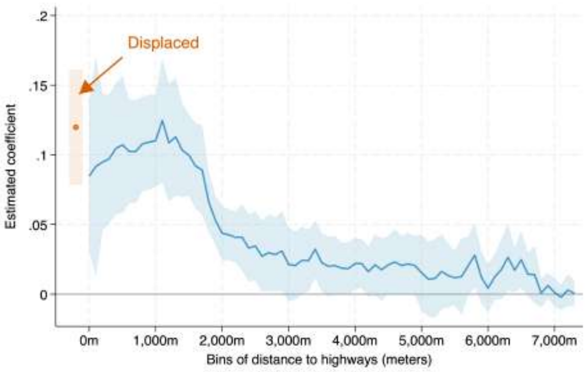
This approach treats individuals symmetrically regardless of which side of the highway they lived on, and avoids the misclassification that can arise from using census tracts, which vary substantially in shape and area. I set the cap at 7,500 meters (4.7 miles) and use individuals living between 7,400 and 7,500 meters as the comparison group. $\mathbf{X}_i$ includes metropolitan area fixed effects and the individual's log distance to the city center, since highways were built to connect downtown areas and Black residents disproportionately lived near city centers during this period (Boustan, 2010). Standard errors are clustered at the metropolitan area level.

Figure 1 shows that Interstates were disproportionately routed through Black communities and areas with lower income and education levels. In each panel, the blue line plots the distance coefficients β_k with 95% confidence intervals shown as shaded bands, and the red marker plots the coefficient for displaced individuals. Panel (a) shows that residents living within 2,000 meters (1.25 miles) of a future Interstate were 10 percentage points more likely to be Black than the comparison group, a gap that remains positive and statistically significant up to 4,000 meters. Panel (b), however, shows no significant gradient for foreign-born residents, suggesting that the racial gradient is not driven by a general pattern of highways passing through minority communities. When looking at the educational attainment, Appendix Figure B.1 shows that education among adults was 15 percentage points lower near Interstates, regardless of the level of education.

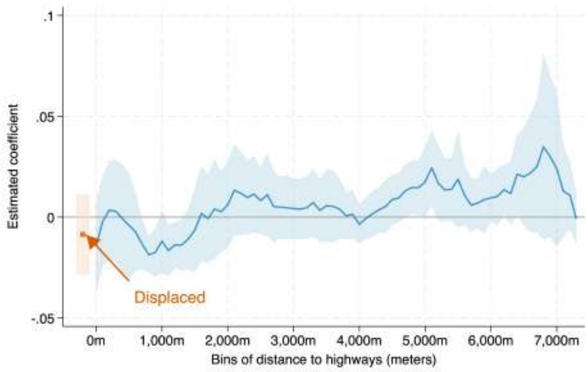
The results suggest that Interstates were built through communities with lower income. Figure 1 Panel (c) shows no meaningful differences in employment rates, but residents near built Interstate highways earned approximately 0.05 log-points less per year than the comparison group (Panel (d)). In Appendix Figure B.2, I show that the income gap is not driven by differences in hours worked, as Panel (a) shows no meaningful differences in weekly hours worked. Most of the income gap appears to be driven by differences in the type of jobs held, as the results in Panel (b) show that residents near built Interstates were employed in occupations with a lower occupational score.

I further explore the determinants of highway routing by examining homeownership rates and property values, since planners sought to minimize right-of-way acquisition costs. While the 1950 census did not collect housing information, I use housing characteristics from the 1940 census. In particular, I use the linkages provided by Ruggles et al. (2025) and keep only individuals who were living in the same house in 1950 as in 1940.[18] Figure 2 presents the results on homeownership, home value, and rents using the restricted sample. Panels (a) and (b) show that Interstates were built in communities with a lower cost of land, which is in line with the minimization of right-of-way acquisition costs. The results indicate that properties located within 2,000 meters of Interstates were 0.25 log points cheaper than the comparison group, regardless of whether I use home val-

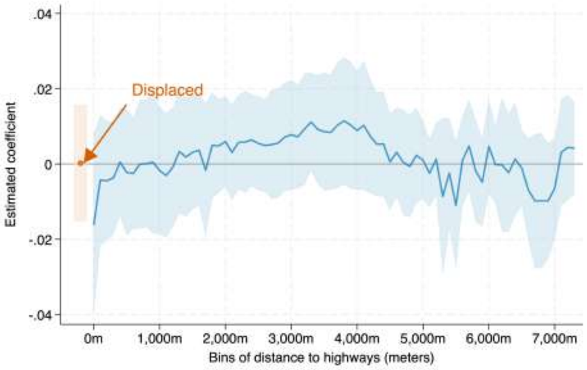
[18]Individuals are considered to live in the same house across censuses if their residences are within 30 meters of each other. The results do not change when using more restrictive or lenient thresholds.



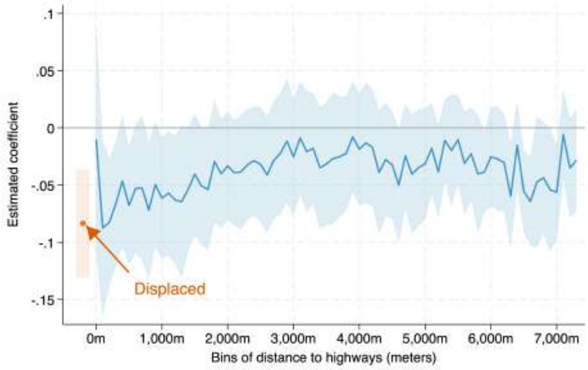
(a) Black population



(b) Foreign-born population



(c) Employment



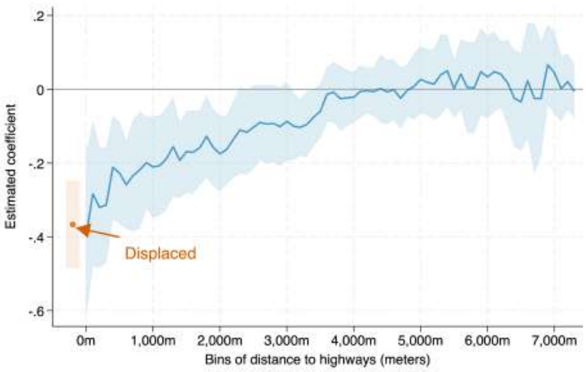
(d) Log income wage

Figure 1: Pre-construction characteristics by distance to future Interstates.

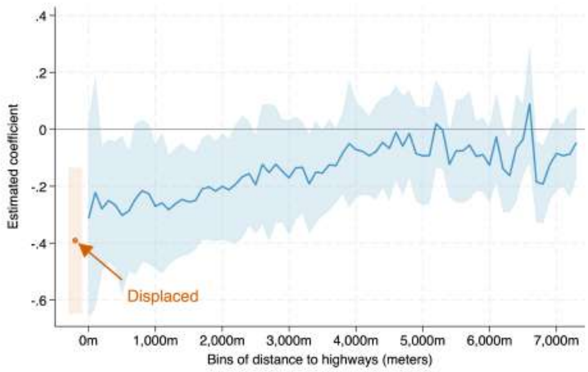
Notes: The figures present the estimated coefficients and confidence intervals of equation 1. The blue line plots the distance coefficients β_k with 95% confidence intervals shown as shaded bands, and the red marker plots the coefficient for displaced individuals. The sample corresponds to individuals living in metropolitan areas and were living closer than 7,500 meters to a future Interstate in 1950. The comparison group corresponds to those living between 7,400 and 7,500 meters from the future Interstate. The dependent variables are reported in the name of each panel. All regressions include metropolitan area fixed effects and control for the individual's log distance to the city center. Standard errors are clustered at the metropolitan area level.

ues or rents as the measure of land price. Panel (c) shows that, although not systematically different from zero, highways were built through neighborhoods with homeownership rates 5 percentage points lower, suggesting that they were built in communities that had accumulated lower wealth, as homeownership is the largest component of Americans' wealth portfolio (Lyons et al., 2025). There are two considerations to keep in mind with this interpretation. First, the census reports property values and rents but does not record housing characteristics such as unit size, meaning I cannot construct a quality-adjusted price measure. Second, home values in affected areas may have diverged from the rest of the city between 1940 and 1950 if housing markets anticipated the construction of highways.

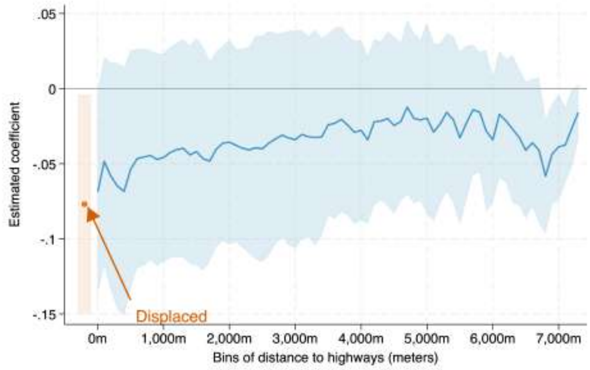
The sorting of Black families into low-rent downtown areas (Boustan, 2010), combined with the stated policy goal of connecting city centers to the broader highway network,



(a) Log home value in 1940 (2010 dollars)



(b) Log rent in 1940 (2010 dollars)



(c) Homeownership rate in 1940

Figure 2: Pre-construction demographic characteristics by distance to future Interstates.

Notes: The figures present the estimated coefficients and confidence intervals of equation 1. The blue line plots the distance coefficients β_k with 95% confidence intervals shown as shaded bands, and the red marker plots the coefficient for displaced individuals. The sample corresponds to individuals living in metropolitan areas and were living closer than 7,500 meters to a future Interstate in 1950 who were living in the same house in the 1940 census. Census-to-census linkages are provided by Ruggles et al. (2025). The comparison group corresponds to those living between 7,400 and 7,500 meters from the future Interstate. The dependent variables are reported in the name of each panel. All regressions include metropolitan area fixed effects and control for the individual's log distance to the city center. Standard errors are clustered at the metropolitan area level.

suggests that any counterfactual Interstate network might also have been routed closer to Black communities. To examine this possibility, I analyze the characteristics of communities along the Yellow Book planned routes in Appendix Figure B.3. Like the built network, these plans likely followed major roads and sought to minimize construction costs. I find no evidence that planned highways were disproportionately routed through Black neighborhoods. Within 2,000 meters of a planned highway, the estimated coefficients are positive but statistically insignificant and less than one-third the magnitude of those for the built network. Planned routes were also not disproportionately routed through immigrant neighborhoods or areas where workers earned lower wages. For the subsample matched to 1940, I find that communities near planned highways had home values roughly 20 log-points lower, though this estimate is not statistically distinguishable from zero.

The previous analysis cannot disentangle the role of each of these factors in highway placement, as they are likely correlated. To address this and incorporate geographic controls such as terrain slope, I study which census tract characteristics in 1950 predict future highway construction. Appendix Table B.1, columns (1) to (5) show that highways were more likely to be built through neighborhoods with a larger Black population share, lower land values, greater proximity to the city center, and a planned Yellow Book route. The relationship between Black population share and highway construction remains positive after accounting for land values, distance to the city center, and local geography. A one-percentage-point increase in the share of the city's Black population living in the tract increases the probability of a highway being built through it by 0.8 percentage points, comparable in magnitude to a $9,600 reduction in average median land value (in 2010 dollars).

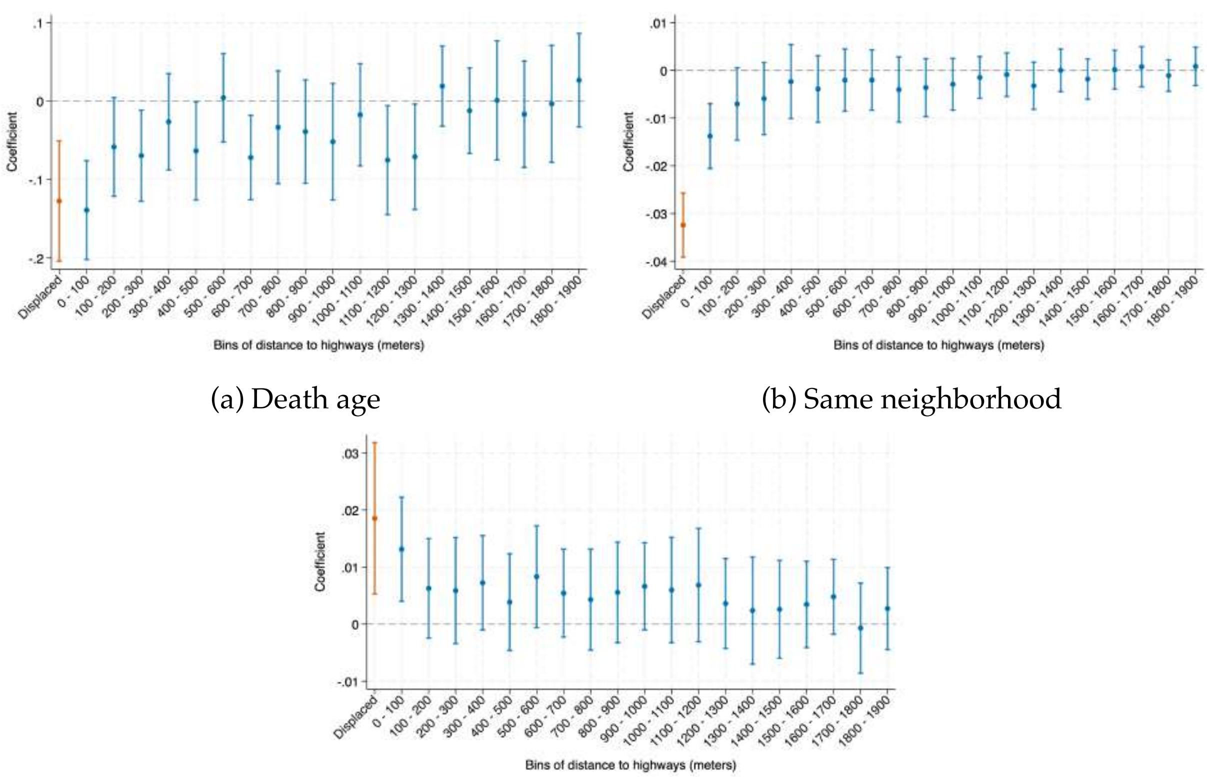
These results are not fully explained by the minimization of right-of-way acquisition costs. Column (6) of Appendix Table B.1 uses as the dependent variable an indicator for whether a highway was planned in the neighborhood according to the Yellow Book, federal maps that identified potential Interstate routes based on cost minimization, but did not have final authority over where highways were built. In contrast to the construction results, the racial composition of neighborhoods does not predict whether a highway was planned in the Yellow Book. This divergence suggests that state planners systematically deviated from the federal plan in ways that disproportionately routed highways through neighborhoods with higher Black population shares, a pattern not explained by proximity to the city center or land values alone. While the analysis does not establish a causal relationship, it indicates that racial composition influenced routing decisions beyond what federal cost-minimization criteria would predict. More details on this analysis are available in Appendix Section B.2.

Taken together, these two analyses indicate that Interstate placement disproportionately affected low-income and Black communities, through both displacement and the bisection of their neighborhoods. Since planned routes show no such pattern, the evidence suggest that deviation from the federal plans played a role in the disproportionate impact on these communities.

5. Long-run Effects on Affected Communities

The previous section showed that highways were built through low-income and Black communities. Whether highway construction harmed or benefited residents of these communities, however, is an empirical question. On the one hand, highways impose direct costs: they expose nearby residents to elevated noise and pollution (Currie and Walker, 2011) and they erode neighborhood social capital by creating physical barriers between neighbors (Brinkman and Lin, 2024), with potentially long-lasting consequences. On the other hand, policymakers at the time viewed highway construction as a tool for urban revitalization, and the economic activity generated by highway projects could have improved local living conditions (Schwartz, 1975).

To answer this question, I estimate equation 1 using outcomes measured at the time of death. I focus on three outcomes: life expectancy, the probability of residing in the same neighborhood, and the probability of residing in the same city at death. Because the effects of interest are on residents of affected neighborhoods rather than entire cities, I restrict the sample to individuals living within 2,000 meters of Interstate segments built before 1970, with those living 1,900 to 2,000 meters from the highway serving as the comparison group. Motivated by the fact that highways were disproportionately routed through low-income and Black communities, all regressions control for the household head's occupational score as well as race, gender, birth year, and metropolitan area fixed effects. Standard errors are clustered at the 1950 metropolitan area of residence.



(a) Death age

(b) Same neighborhood

(c) Same city

Figure 3: Long-run effects of highway construction on affected communities.

Notes: The figures present the estimated coefficients and confidence intervals of equation 1. The blue line plots the distance coefficients β_k with 95% confidence intervals shown as shaded bands, and the red marker plots the coefficient for displaced individuals. The sample corresponds to individuals living in metropolitan areas and were living closer than 2,000 meters to a future Interstate in 1950, linked to administrative mortality records from 1988 to 2007. The comparison group corresponds to those living between 1,900 and 2,000 meters from the future Interstate. The dependent variables are reported in the name of each panel. All regressions include the household head' occupational score, as well as race, gender, birth year, and metropolitan area fixed effects. Standard errors are clustered at the metropolitan area level.

Figure 3 shows that the long-run effects of Interstate construction on communities are highly localized. The figure presents estimated effects on life expectancy and residential mobility for individuals in different distance bins from the highway, with the coefficient for displaced individuals shown in red. Panel (a) reports effects on life expectancy. Displaced individuals die about 0.12 years younger than neighbors living 1,900 to 2,000 meters from the highway. The effects spill over to residents within 100 meters of the highway, who also die at younger ages, but there is no significant effect beyond 100 meters. These findings complement evidence of negative effects of highway externalities on newborns living within 2,000 meters of a highway (Currie and Walker, 2011). For adults, spillovers are considerably more spatially concentrated.

Panels (b) and (c) examine spatial mobility, using indicators for whether individuals remained in the same neighborhood and the same city between 1950 and death, respectively. Panel (b) shows that displaced individuals are 3 percentage points less likely to reside in the same neighborhood at death. Residents within 100 meters of the highway, though not directly displaced, are also more likely to leave the neighborhood. These estimates are economically meaningful, as they represent 60 and 30 percent of the sample mean for displaced and adjacent individuals, respectively. The reverse pattern emerges for the probability of residing in the same city: both displaced and adjacent individuals are *more* likely to remain in the same city at death. Together, these results suggest that affected individuals relocate within the same city rather than leaving it entirely. As with mortality, spillovers fade quickly, with no systematic evidence of mobility effects beyond 100 meters.

Overall, the results that the construction of the Interstate network had localized negative effects on individuals living near highways, which aligns with previous findings by Moretti and Wheeler (2025) and Kashner and Ross (2025) who also find that highway noice and traffic externalities are highly local.

6. Empirical Strategy

The results of Section 4, along with the historical evidence, indicate that while the communities where highways were placed differed from the rest of the city, location decisions within those neighborhoods were driven by the availability of large roads that could be converted into highways. Although these communities differed from the rest of the city, individuals in these communities were similar to one another, and displacement status varied according to idiosyncratic factors once you condition on the decision to live in the neighborhood. Accordingly, the empirical strategy compares individuals affected by highway construction with those in the same neighborhood who are not affected, conditional on 1950 characteristics.

In Section 5, I show that highway construction had spillovers into communities beyond those displaced. Those living within 100 meters of the highway die at a younger age and are less likely to be in the same community at the time of death. These findings motivate including individuals within 100 meters as a second treatment group in all analyses. I will refer to this group as those adjacent to highways.

The results also suggest that spillovers are highly localized, with no significant effect beyond 100 meters. Therefore, I use individuals living 100-200 meters from the newly constructed highway as the comparison group. These individuals chose to live in the affected communities and, for idiosyncratic reasons, were located farther from the highway.[19]

I compare individuals whose homes were destroyed by highway construction and those living adjacent to the newly constructed highway with their unaffected neighbors, controlling for 1950 characteristics. The main specification controls for race, gender, birth year, metropolitan area of residence, and the household head's occupational score. Ideally, it would be desirable to control for measures of earnings and wealth, such as income, homeownership and property values. However, these variables are not systematically available in the 1950 census. Income is only available for one out of every six individuals, and homeownership and property values were not recorded in the 1950 census. I proxy these characteristics with the household head's occupational score, which is available for all individuals and is positively correlated with income and wealth. Appendix Figure C.8 shows that the household head's occupational score is positively correlated with income, homeownership, and property values in the 1940 census.

The sample includes those individuals displaced by highway construction and those living within 200 meters of these projects in the 1950 census, linked to administrative mortality records from 1988 to 2007. Appendix Figure C.9 presents a visual representation of the empirical strategy. Each green dot corresponds to a household in Cleveland, Ohio, in the 1950 census. The red band corresponds to the highway built, the blue band to those adjacent, and the golden band to those living between 100 and 200 meters.

I estimate the impacts for those affected by highway construction by estimating the following specification:

$$y_i = \beta_1 \text{ Displaced}_i + \beta_2 \text{ Adjacent}_i + \mathbf{X}_i^{'} \Gamma + \varepsilon_i. \tag{2}$$

In Equation 2, y_i is an individual long-term outcome. The treatment status, Displaced$_i$ and Adjacent$_i$, is determined by the home they resided in during the 1950 census, not by where they were living at the time of construction. Measurement error arising from out-migration before construction would bias the estimates toward zero, making the estimates a lower bound of the *true* effect. Appendix Section A.4 presents a bounding exercise

[19] Appendix Figure D.2 shows that the results are robust to using individuals living 500-600 meters and 1,000-1,100 meters away as the control group.

that suggests the *true* effect should be bounded between the estimates and 3.2 times the estimate.

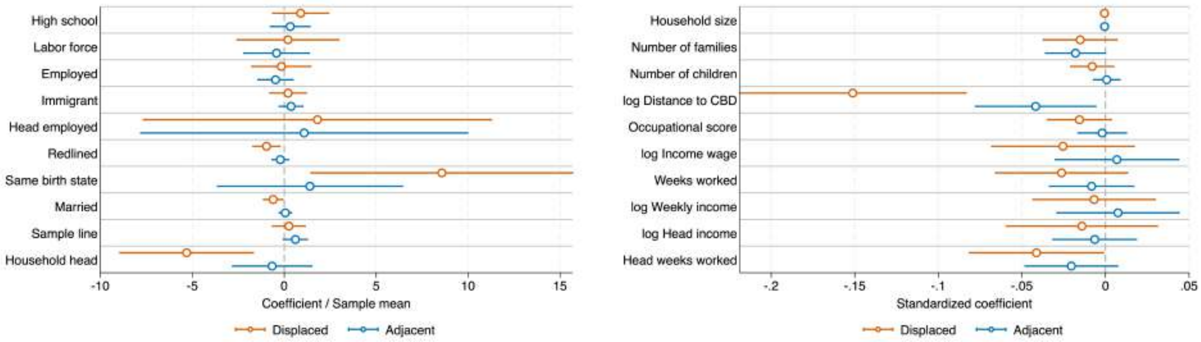
The vector $\mathbf{X}_i$ includes controls for race, gender, birth year, the metropolitan area of residence, as well as the household's socioeconomic status proxied by the household head's occupational score. Standard errors are always clustered at the metropolitan area level.[20] This regression estimator identifies the causal effects of highway construction on individuals displaced by construction or living adjacent to the highway, under a conditional-independence assumption and the stable unit treatment value assumption (SUTVA). Specifically, among individuals living in the same neighborhoods, displacement decisions were driven by idiosyncratic factors independent of potential future outcomes. Conditioning on observables accounts for the otherwise idiosyncratic assignment process arising from routing decisions through low-income and Black communities. Conditional independence is required for analyzing long-run individual outcomes, since they are observed only in the post-period. Equation 2 is a cross-sectional regression with outcomes defined for a given period. As specified, identification requires a balance of potential outcomes across treatment status, conditional on the specified observable characteristics.

The results from the previous section provide evidence that SUTVA likely holds, as spillovers are highly local and the control group is unaffected by highway construction across all studied outcomes. Nonetheless, general equilibrium spillovers from increased market access due to highway construction are possible. That would be the case in a counterfactual world in which a different Interstate network had been built, leading to different changes in market access across these cities. It is difficult to envision a case in which these counterfactual networks would have affected the control group differently from the treatment groups. My conjecture is that most counterfactual Interstate networks would have connected the metropolitan areas regardless, as they were large hubs of manufacturing and jobs, so any differences in market access within the metropolitan areas used in the analysis would have been negligible.

To assess the plausibility of the conditional independence assumption, I conduct balance tests by estimating equation 2 with pre-highway construction characteristics as outcomes. Continuous characteristics are standardized to have mean zero and standard deviation of one, and indicators are reported as percentage changes ($\hat{\beta}$ divided by the sample mean). Appendix Figure 4 plots differences between treatment and control individuals, adjusting for birth year, race, gender, metropolitan area of residence, and household head occupational score. Both displaced individuals and those adjacent to newly constructed highways are similar to their unaffected neighbors in pre-treatment characteristics. I find differences in household head employment, the probability of being an immigrant, education, and proximity to the city center. However, the magnitude of these differences is economically small. Figure D.2 shows that the results are robust to controlling for these

[20] Figure D.2 shows that the results are robust to clustering at the state level.

differences.



(a) Indicator variables

(b) Continuous variables

Figure 4: Pre-construction balance.

Notes: The figures display estimates from regressions of pre-construction characteristics on the two treatment indicators in equation 2, along with 95% confidence intervals. Each row corresponds to a different regression, and the sample includes individuals living within 200 meters of the newly constructed highway in the 1950 census. All regressions include household head's occupational score, as well as gender, race, birth year, and metropolitan area fixed effects. Standard errors are clustered at the metropolitan area level. Coefficients and confidence intervals in Panel (a) are normalized by the sample mean of the outcome, so they can be interpreted as percentage changes. Coefficients in Panel (b) are standardized to have mean zero and standard deviation of one, so they can be interpreted as standard deviation changes.

I also test for pre-trends by linking the 1950 and 1940 census using the crosswalk created by Ruggles et al. (2025). Similar to the balance analysis, I estimate Equation 2 using cross-decade changes in economic and demographic outcomes. I restrict the sample to those older than 10 years in 1950 for demographic variables, and those between 24 and 65 years old for labor variables. Appendix Figure C.1 shows that both displaced individuals and those adjacent are balanced in pre-trends. I find that displaced individuals are less likely to live in the same neighborhood and to be employed in both years. Consistent with the identification assumptions, I find no systematic evidence of differential economic trends between the treatments and the control group.

6.1 Alternative Empirical Strategies

I present two complementary strategies that address potential threats to the main empirical design. The first uses propensity score matching to construct control groups from individuals living farther from highways, addressing concerns about spillover to the control group. The second exploits variation in planned versus actual highway routes from the Yellow Book to address concerns about differences between treated and control groups.

Matching: One concern would be whether unobserved spillovers may still affect the control group. For example, highway construction reduced the housing stock in affected neighborhoods, particularly for low-income households, which may have raised neighborhood rents and affected the control group. To address this concern, I developed an alternative empirical strategy that compares individuals displaced by, and those living adjacent to, newly constructed highways with a pool of potential controls residing in the same city and more than 2,000 meters from any constructed highway, using propensity score matching. The threshold of 2,000 meters is based on health literature indicating that spillovers from highway pollution on newborns completely fade above this threshold (Currie and Walker, 2011). The match is done based on individual, household, and neighborhood characteristics.[21] The matching algorithm matches 56% of the treatment group. The strategy still relies on the conditional independence assumption to identify the treatment effects. Appendix Figures C.3 and C.4 show that CIA is likely to hold, as there are almost no differences between treatment and control groups.

Yellow Book: Another concern is that, even conditional on the neighborhood, displaced individuals differ from those living more than 100 meters away in ways not captured by the balance test. For example, highways were typically built along primary roads, which could later be upgraded to Interstates. There may be an unobserved difference between individuals who live on primary roads and their neighbors a few blocks away. I use the highway routes planned in the Yellow Book as a third empirical strategy to address this concern. These maps were created for 100 cities and were the results of the minimization of construction costs and were usually aligned with primary roads at the time. Thus, this strategy uses individuals who would have been displaced if these routes were built as a control group. Appendix Figures C.5 and C.6 show that samples are balanced and that these individuals were not trending in a different way than those displaced and adjacent.

A concern with this strategy is that living in primary roads may have, by itself, an effect on the outcomes of interest. I can test this possibility by running a placebo test using as a treatment group individuals living near these planned highways. I then repeat the exercise of comparing affected individuals to those living between 100 and 200 meters away.[22] The results show no significant effect of living close to a planned highway on individuals' long-term outcomes. Figure C.7 shows that the coefficients that are very close to zero and not statistically significant at the usual levels. More details about this strategy are provided in Appendix Section C.3.

7. Long-run consequences of Interstate construction

This section studies the long-run effects of Interstate construction on individuals whose homes were destroyed and on those living within 100 meters of the newly built highway.

[21] See Appendix Section C.2 for more details about the matching algorithm.

[22] Planned highways consists of a unidirectional segment of the highway. I assume that planned highways had a right-of-way width of 150 feet (45.72 meters).

7.1 Effects on longevity

How did Interstate construction affect the well-being of displaced residents and their neighbors? To answer this question, I examine the effect of Interstate construction on longevity. Longevity is often regarded as one of the largest components of wellbeing (Jones and Klenow, 2016). Given the lack of protection provided for those displaced, one would expect adverse effects. However, the findings of Deryugina and Molitor (2020) raise the possibility that, faced with the forced move, those whose homes were destroyed relocate to cities with better access to health care, which may increase life expectancy. Thus, the effect of Interstate construction on longevity for those with seized homes and those living adjacent to the highway is an empirical question.

To examine this question, I estimate equation 2 on a set of longevity outcomes. The results are presented in Figure 5. The top section of Panel (a) presents the results of using age at death as the dependent variable. The estimates indicate that both displaced and adjacent individuals die younger than their unaffected neighbors. On average, displaced individuals die approximately 0.08 years earlier than their unaffected neighbors, or about 1 month ($0.08 \times 12 \approx 0.96$). To benchmark this magnitude, Chetty et al. (2016b) find a clear positive relationship between longevity and income percentile in the US. My estimated effect is equivalent to the difference in life expectancy between households at the 48th and 50th income percentiles, or approximately $2,726 in annual income.

To study the mechanism behind the decrease in longevity, I examine the probability of surviving past the ages of 75 and 80. The top sections of Panels (b) and (c) present results from the main research design, which compares treated individuals to their unaffected neighbors. These results indicate that the overall decrease in life expectancy operates primarily through lower survival rates past age 80 among displaced and adjacent individuals. The magnitudes of the estimated effects are also sizable. Relative to a baseline survival probability to age 80 of 30.42 percent in the sample, I estimate an average decline of 2.16% in the probability of living to age 80 among those displaced.

The results do not appear to be driven by unobserved spillovers to the control group or by differences in unobserved characteristics that influence sorting. The middle panel of Figure 5 presents results for the propensity score matching design, which matches treated individuals to residents in the same city who live more than 2,000 meters from any highway, based on observable individual, household, and neighborhood characteristics. Because this control group lives farther from any highway, they are less likely to be affected by unobserved spillovers from Interstate construction. A remaining concern is that primary roads were often converted into highways, raising the possibility that families living on these roads differ from their neighbors in ways unobserved to the econometrician. The routes planned in the Yellow Book, like the actual network built, were also more likely to correspond to primary roads. The lower section of Figure 5 uses as a compari-

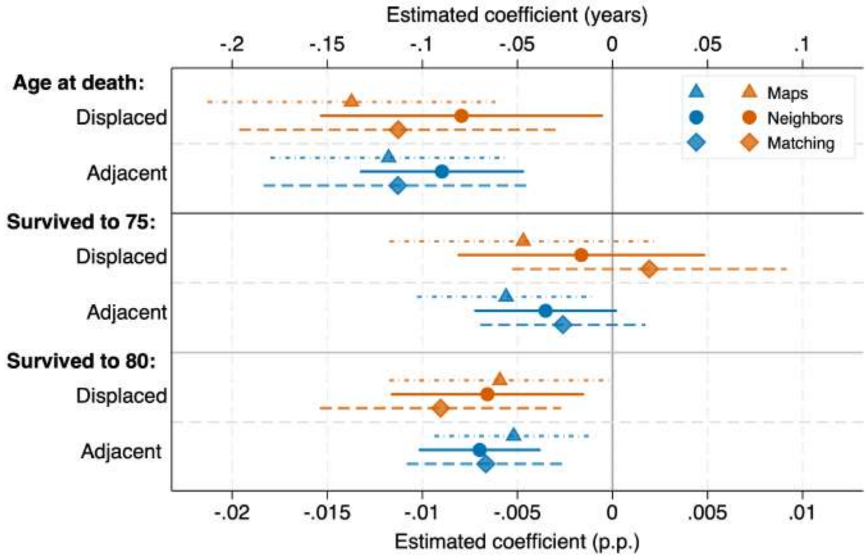


Figure 5: Effects of Interstate construction on longevity.

Notes: The figures display estimates from regressions of longevity characteristics on the two treatment indicators in equation 2, along with 95% confidence intervals. Each row corresponds to a different regression, using the three research designs described in Section 6 and standard errors clustered at the metropolitan area level. The strategy denoted as "Neighbors" compares treated individuals to their unaffected neighbors living between 100 and 200 meters from the highway; strategy "Matching" compares treated individuals to matched individuals living in the same city, and strategy "Maps" compares treated individuals to those who would have been displaced by the Yellow Book routes. The top section uses age at death as the dependent variable, while the middle and bottom sections use the probability of surviving past the ages of 75 and 80 as the dependent variable, respectively. The estimates are plotted against two different horizontal axes to improve readability. All regressions include household head's occupational score, as well as gender, race, birth year, and metropolitan area fixed effects. Appendix Table D.1 reports the numerical values of the estimates and standard errors.

son group individuals who would have been displaced by the Yellow Book routes. Both complementary specifications yield results consistent with the main empirical strategy. Displaced individuals and those living within 100 meters of newly constructed highways die younger and are less likely to reach age 80, suggesting that highway construction had a detrimental effect on the wellbeing of these populations.

One interpretation of these results is that highway construction shifted the entire survival distribution for displaced and adjacent individuals. To examine this possibility, I estimate a Cox Proportional Hazard model, which models the hazard rate of death semi-parametrically. More details about this method can be found in Appendix Section D.3. Appendix Table D.4 reports the hazard rate estimates. I find that both displaced and adjacent individuals face a 1.1 to 2 percent higher hazard rate of death.[23] One advantage of estimating hazard rates is that it enables direct comparison with estimates for other traumatic life experiences. The estimated effect hazard rate for displacement is similar in

[23]Transforming the increased hazard rate to average expected years of life lost yields an estimate of 0.9 months, close to the OLS estimate of 1 month. More details about this transformation can be found in Appendix Section D.3.

magnitude to the effects the literature has found for divorce (Sbarra et al., 2011), equal to one-third of the mortality differences between homeless and housed populations (Meyer et al., 2023), and equal to 3 percent of the mortality effect for parents who lose child (Song et al., 2019).

All three strategies produce similar estimates. In the subsequent analysis, I focus on the preferred neighbor-comparison design. Appendix Section D presents the results using the other two strategies.

The results in this section indicate that highway construction reduced life expectancy both for individuals whose homes were destroyed and for those living within 100 meters of newly constructed highways. These findings contrast with Deryugina and Molitor (2020), who find that individuals displaced by Hurricane Katrina lived longer, a result they attribute to relocation to cities with better health care access. This contrast motivates the next section, which examines the effects of highway construction on spatial mobility.

7.2 Effect on spatial mobility

Section 4 shows that highways were built in lower-income neighborhoods with lower property values. The literature has found that when moving is costly, forced relocation may help solve the spatial mismatch (Nakamura et al., 2021). In modern settings, researchers have found that displaced individuals move to cities with better healthcare (Deryugina and Molitor, 2020) and labor markets (Deryugina et al., 2018). In addition, construction occurred during the last years of the Great Migration (Boustan, 2010). There is evidence that Black individuals who left the South die younger (Black et al., 2015), and the cities they moved to saw a decrease in intergenerational mobility (Derenoncourt, 2022). Thus, a natural question is whether highway construction helped individuals overcome spatial mismatch and relocate. To do so, I use the nine-digit ZIP code of residence at death to infer spatial mobility.

Results for the preferred specification are shown in Figure 6.[24] The top row shows that both displaced and adjacent individuals are, respectively, 2.58 percentage points and 0.67 percentage points less likely to be living at death in the same neighborhood as in the 1950 census. These effects are also sizable, accounting for 64.6% and 16.7% of the sample mean.

Although both treatment groups leave their neighborhoods, the results show that they remain in the same city. The middle row looks at the probability of staying in the same city. Displaced individuals are 1.27 percentage points more likely to stay in the city, whereas those adjacent are 0.67 percentage points more likely.

One potential explanation is that highways opened up the suburbs to individuals otherwise contained to the city center (Connolly, 2014). There is strong evidence that the

[24] Appendix Table D.2 shows the results for the three strategies.

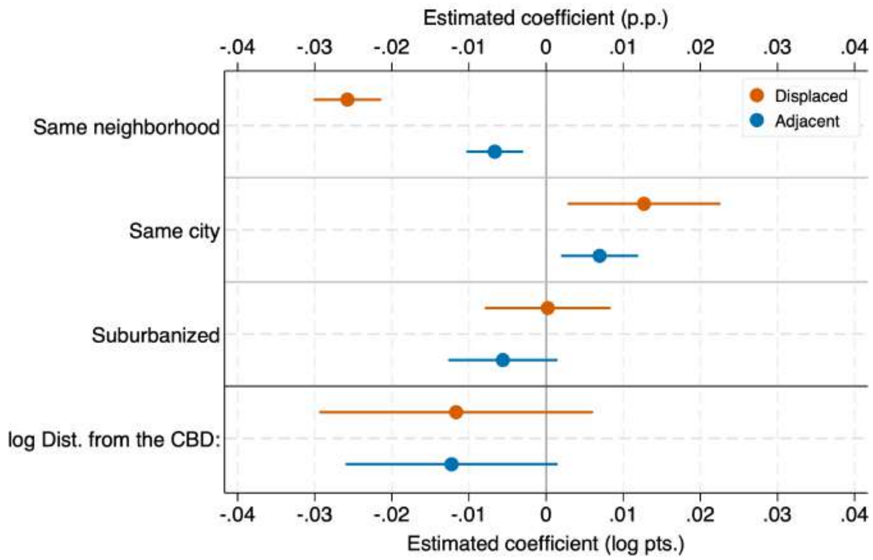


Figure 6: Effects of Interstate construction on spatial mobility.

Notes: The figures display estimates from regressions of spatial mobility characteristics on the two treatment indicators in equation 2, along with 95% confidence intervals. Each row corresponds to a different regression, and the sample includes individuals living within 200 meters of the newly constructed highway in the 1950 census. The dependent variables are reported in the y-axis, and the estimates are plotted against two different horizontal axes to improve readability. All regressions include household head's occupational score, as well as gender, race, birth year, and metropolitan area fixed effects. Standard errors are clustered at the metropolitan area level. Appendix Table D.2 reports the numerical values of the estimates and standard errors.

Interstate system caused suburbanization in the US (Baum-Snow, 2007). However, Panel (c) shows no effect on suburbanization. For both treatment groups, I do not find evidence that they were more likely to be living in suburban neighborhoods at death. In Panel (d), I find that both groups are closer to the city center, although these results are not statistically significant at the usual levels (p-value = 0.19 for those displaced, p-value = 0.08 for those adjacent).

There are several reasons why, in the context of highway construction, displaced individuals do not move outside the city. The literature has focused on natural disasters, which cause greater destruction in affected cities with large general equilibrium effects. For example, employers could relocate from the affected city if they anticipate that environmental hazards will recur. Additionally, most individuals displaced by highway construction did not receive any relocation payment to cover moving costs or secure new housing, which may prevent them from relocating farther. Finally, unlike in the other settings, displacement in this context arises from a policymaker's decision rather than an environmental catastrophe. I am open to the possibility that the consequences differ when the source is not an environmental catastrophe, given the vast anecdotal evidence that highway-induced displacement has a substantial psychological toll on those affected (Caro, 1974; Archer, 2020; Rose and Mohl, 2012).

Thus far, I have shown that those affected by highway construction moved out of their neighborhoods and relocated within the same city. This reorganization of urban dynamics raises the question of where these individuals relocated, given ample evidence that the neighborhood of residence affects long-run outcomes (Chetty et al., 2016a).

7.3 Neighborhood characteristics at death

The previous results show that individuals affected by highway construction leave their neighborhoods and relocate within the same city. In this section, I use the last recorded residence to infer neighborhood proxies for wealth, income, and education. A vast literature in economics shows that relocating individuals from struggling neighborhoods has a positive effect on their long-run earnings (Chetty et al., 2016a; Chyn, 2018). However, given anecdotal accounts of displaced individuals moving to economically disadvantaged areas and the increased mortality found in Section 7.1, one would expect displaced individuals to end up in neighborhoods with lower socioeconomic indicators.

Figure 7 presents suggestive evidence that displaced individuals end up in neighborhoods with lower economic characteristics. Each outcome corresponds to the year 2000, the median year in the mortality sample, and has been normalized to have a mean of zero and a standard deviation of one. I find that those displaced lived in neighborhoods with unemployment rates 0.017 standard deviations higher and college graduate shares 0.029 standard deviations lower (p-value = 0.065). I find no significant effect on neighborhood income, home values, rents, or homeownership rates. In contrast, I do not find any effect for those adjacent in any variable studied.

How do these results translate into the *quality* of the neighborhood where individuals live? To answer this question, I construct a quality index using the first principal component analysis (PCA) of neighborhood characteristics for both Black and white individuals. The PCA incorporates all the economic characteristics of the neighborhood analyzed earlier and condenses them into a one-dimensional measure. I then normalize the index to have mean zero and a standard deviation of one. More details on the construction of the quality index appear in Appendix Section D.4. The results show that displaced individuals lived in lower-quality neighborhoods at the time of death. The bottom row of Figure 7 shows that displaced individuals relocated to neighborhoods with quality 0.022 standard deviations lower than the control group. In contrast, I find no effect on neighborhood quality among those adjacent to newly constructed highways.

Ideally, one would like to characterize the entire residency path of those displaced, from displacement to death. While publicly available data do not enable me to test this, I proxy for it by examining how the neighborhood of residence at death changed between 1960 and 2000. To compare these neighborhoods over time, I estimate equation 2 separately for each census year, using the normalized quality index as the dependent variable.

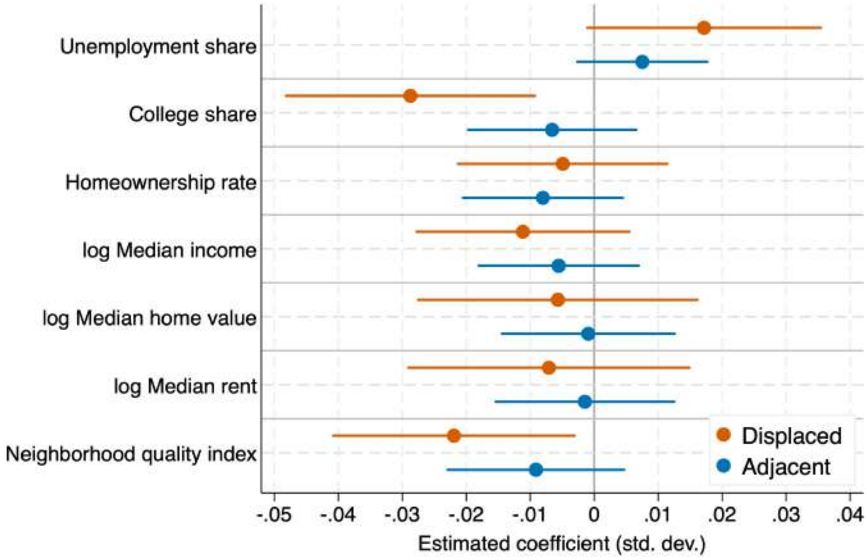


Figure 7: Effects of Interstate construction on neighborhood characteristics.

Notes: The figures display estimates from regressions of neighborhood characteristics on the two treatment indicators in equation 2, along with 95% confidence intervals. Each row corresponds to a different regression, and the sample includes individuals living within 200 meters of the newly constructed highway in the 1950 census. The dependent variables are reported in the y-axis, and they are normalized to have a mean of zero and a standard deviation of one. All regressions include household head's occupational score, as well as gender, race, birth year, and metropolitan area fixed effects. Standard errors are clustered at the metropolitan area level. Appendix Table D.3 reports the numerical values of the estimates and standard errors.

The results shown in Appendix Figure D.1 indicate that the neighborhoods where those displaced relocated were not different from the areas the control group relocated to in the years after construction, but their quality declined in the nineties and the two-thousands.

7.4 Robustness

Appendix Figure D.2 presents additional robustness tests of the estimates. The results remain stable when controlling for every covariate that was unbalanced in the balance test. I observe similar effect patterns when using as control groups those living between 500 and 600 meters, between 1,000 and 1,100 meters, and between 2,000 and 2,100 meters, as well as everyone living in the city, which alleviates concerns about spatial spillovers affecting the control group. When estimating effects across multiple treatments simultaneously, regression coefficients can be contaminated by treatment effects from other treatments through non-convex weighting, potentially biasing the estimates (Goldsmith-Pinkham et al., 2024). The results remain unchanged when applying their proposed correction for this contamination bias. Moreover, these results are similar when the exercise is repeated using the 1940 census, which alleviates concerns about selective outmigration between the census and highway construction.[25] Likewise, the results are robust to clustering the standard errors at the state level.

7.5 Heterogeneities

I study how highway construction differentially affected individuals based on their pre-construction characteristics.

Effects by age at displacement. I examine whether results vary by age at displacement. It is well-documented that the causal effects of neighborhoods and public housing on individuals vary with age and exposure to these places (Chetty and Hendren, 2018; Beauregard, 2025), and there is evidence that the effect of displacement also varies with age (Nakamura et al., 2021). In my setting, the negative consequences may mask substantial heterogeneity across ages.

Appendix Figure D.4 presents results from a modified version of equation 2 that allows treatment effects to vary with age at construction. Panel (a) shows that, on average, displacement had its largest effects on those aged 41-50. Although I do not find any negative effects among those displaced before age 20, the results for those living near a highway show that growing up near a highway reduces life expectancy. In Panels (b) and (c), I show that these patterns are not due to different out-migration from the neighborhoods or the city. Finally, I find that only those displaced in late adulthood, older than 45 years, were living in worse neighborhoods at the time of death.

Effects by Race, gender, public housing availability, and family networks. The top panel of Appendix Figure D.5 tests whether the results vary by race. Section 4 shows that highways were built in communities with larger Black populations than the rest of the city. Highway construction coincides with the Civil Rights Era, a period when several social movements fought to end legalized racial discrimination and segregation (Ang and Chinoy, 2025). I find that mortality effects for both displaced individuals and those adjacent to highways are driven mostly by white individuals. Displacement moved individuals out of their neighborhoods regardless of race. In contrast, only white individuals adjacent to a highway were able to move out of these neighborhoods, with no effects found for Black individuals. Finally, I find that only white individuals end up in *worse* neighborhoods at the time of death.

The remaining panels in Appendix Figure D.5 examine how the estimated effects vary by gender, the availability of social housing in the city, and the individual's family networks. First, I do not find evidence of different effects across gender. When examining the availability of social housing, I find that the negative mortality effects are concentrated in cities with greater availability, in line with the literature that finds that removing children

[25] Given the longer period between the observed residence and treatment, the effects are smaller in magnitude.

from public housing projects has positive long-term consequences (Chyn, 2018). Finally, I find that individuals with a strong family network drive the mortality results. The estimates also show that those displaced with a large family network are 0.3 percentage points more likely to stay in the city, which is in line with accounts that displaced individuals ended up living with relatives, leading to overcrowding (Archer, 2020).

Effects by neighborhood quality. How do the results vary by the quality of the neighborhood of residence? In Appendix Figure D.6, I present the results using three different proxies for neighborhood quality. The first two correspond to the neighborhood's income and educational composition. In particular, I examine how the effect varies when individuals live in neighborhoods with above-city-median average income and college share. Both proxies suggest that the mortality results are driven by residents of neighborhoods of below-median quality, with no differences in mobility and neighborhood quality.

The third proxy of neighborhood quality uses the Home-Owner's Loan Corporation (HOLC) redlining maps, which created a classification of neighborhoods based on their perceived risk for mortgage lending (Rothstein, 2017). I compare individuals living in redlined neighborhoods, which received the lowest grade in the HOLC maps, to those living in non-redlined neighborhoods. Exclusionary institutions, including restrictive housing markets, further constrained displaced individuals, particularly Black families, limiting their ability to move to better neighborhoods or secure improved employment opportunities (Weiwu, 2025).[26] I find comparable effects for those living in redlined and non-redlined areas, except for neighborhood quality. Those displaced from redlined areas end up in *worse* neighborhoods. These findings indicate that, despite minor differences, highway construction caused comparable disruption to individuals' livelihoods, regardless of the quality of the neighborhood of residence before construction.

8. Mechanisms driving the mortality results

This section examines the channels through which displacement may affect long-term mortality. I first test the mechanisms proposed in the urban affairs literature, then decompose their relative contributions to the mortality results.

8.1 Attributes of destination locations and social networks

The urban affairs literature identifies several channels linking displacement to long-term outcomes. Displaced households tend to relocate to lower-quality neighborhoods, often in economically distressed areas farther from their original homes and at the urban pe-

[26] Additionally, discrimination in labor markets based on the neighborhood of residence may have further constrained displaced individuals from securing better jobs (Angeli et al., 2026).

riphery. Highway construction also disrupted the social fabric of affected communities, severing residents' social networks. Whether these patterns generalize, however, remains an open empirical question.

Appendix Table E.1 presents estimates from equation 2 using five proxies for the channels identified in the literature. Column 1 shows that displaced individuals lived in neighborhoods at death with 0.1 percentage points higher unemployment rates, a proxy for economic distress. This estimate can be compared to average unemployment rate in the sample, which was 5.7%. By contrast, those who lived adjacent to the highway do not exhibit higher unemployment in their destination tracts. Column 2 presents the same pattern for neighborhood quality index.[27] Together, these findings align with qualitative accounts documenting that displaced families often had no alternative but to relocate to struggling communities (Avila, 2014).

I next examine whether Interstate construction disrupted social networks. This analysis is motivated by well-documented evidence of the highway program's impact on the social fabric of affected communities.[28] Column 3 reports estimates using an index of social network stability as the dependent variable, constructed as the share of individual i's neighbors who lived in the same census tract in 1950 and in the same census tract as i in 2000, normalized to range from 0 to 100.[29] Displacement significantly reduced network stability, with an average decline of 0.067 percentage points, or 5.02% of the sample mean. In contrast, I find no evidence that highway construction affected the networks of adjacent residents. This difference reflects the nature of relocation. Interstate construction forced inframarginal individuals to move, individuals who would not have relocated otherwise. Those living adjacent to the highway, however, chose to leave, possibly due to reduced neighborhood amenities. Because endogenous moves are at least partially explained by social networks (Green, 2025), adjacent movers likely relocated to the same places as their networks, mitigating disruption.

Finally, I examine the spatial relocation patterns of affected individuals. In Section 7.2, I showed that those affected by construction are more likely to leave their neighborhood but remain within the same city. Here, I analyze their post-construction locations relative to their pre-construction homes. Column 4 reports estimates using the log distance between pre- and post-construction residences, while column 5 uses the log difference in distance to the city center.

The distance between homes captures whether displaced individuals relocated to the

[27] For this exercise, I exclude employment-related variables when constructing the PCA to avoid mechanical correlation between the results in columns 1 and 2.

[28] In an interview with an East Tremont resident, Caro (1974) provides a compelling example: *"The manner [the construction of the Cross-Bronx Expressway occurred] had a major impact on how the community reacted to change thereafter. It left them with feelings of being isolated, left alone, that no one cared, no one listened to them."*

[29] Note that these two census tracts do not need to be the same; the measure is calculated for each origin-destination census tract pair.

urban fringe, as documented in case studies (Archer, 2020). Relocation farther from one's original home, however, may not imply worse outcomes: Zimmer (1964) found that businesses forced to relocate due to highway construction experienced greater improvements in site conditions when they moved farther away. The estimates indicate that displaced individuals lived farther from their pre-construction homes and marginally closer to the city center after construction. Adjacent residents, by contrast, did not move significantly farther from their 1950 homes and were only slightly more likely to move toward the city center.[30] These relocation patterns may reflect institutional discrimination (Sood et al., 2024), racial segregation (Cutler and Glaeser, 1997), housing stock depletion, or insufficient relocation funds. I remain agnostic about the underlying causes and focus on equilibrium outcomes.

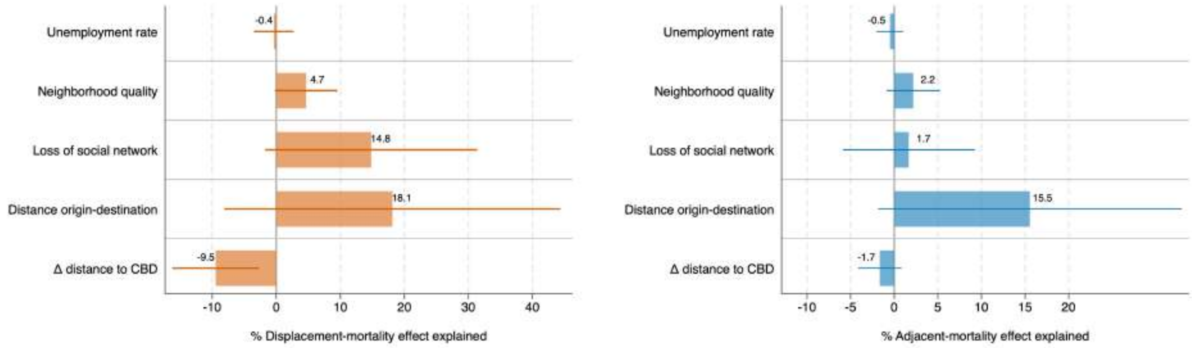
8.2 Decomposing the Mortality Effects

The preceding analysis shows that highway construction affects outcomes along each channel identified in the urban affairs literature. These channels may, in turn, mediate the mortality effects documented in Section 7.1. I quantify their contributions using the decomposition method of Gelbach (2016), which is well-suited to settings with correlated variables.[31] Standard approaches that sequentially add controls conflate the contributions of correlated variables. For example, the change in the displacement coefficient when controlling for unemployment would also reflect differences in neighborhood quality, as these two variables are likely correlated. Appendix Section E.2 provides further details of this methodology.

Panel A of Figure 8 presents the decomposition for individuals displaced by highway construction. Four patterns emerge. First, distance between pre- and post-construction homes accounts for the largest share of the mortality effect (18.1%), though this estimate is not statistically significant at the usual levels. Second, the disruption of social networks explains 14.9% of the effect, statistically significant at the 10% level. Third, the quality of the at death accounts for 4.7% of the effect, statistically significant at the 5% level, which is consistent with the literature on causal neighborhood effects on long-term outcomes (Chyn, 2018). Finally, neighborhood economic conditions and changes in distance to the city center account for negative contributions, which means that the effect of mortality is attenuated by these factors. Together, these channels account for 27.7% of the displacement–mortality relationship.

[30] Panels (c) and (d) of Figure 6 present the results for the log distance to the city center at death and an indicator for suburban residence. The estimates show weak evidence that affected individuals lived closer to the city center (p-values of 0.19 and 0.08 for displaced and adjacent, respectively) and no evidence that they were more likely to live in the suburbs.

[31] Gelbach's (2016) decomposition has been applied to study the nutrition–income relationship (Allcott et al., 2019), the gender pay gap in the gig economy (Cook et al., 2020), and partisan support for taxation (Stantcheva, 2021).



(a) Displaced individuals

(b) Adjacent individuals

Figure 8: Gelbach decomposition of mortality effects.

Notes: The figures use the method described in Gelbach (2016) to decompose the mortality effects of displacement and adjacency into the contributions of the channels identified in Section 8.1. More details of the methodology are provided in Appendix Section E.2.

Panel B presents the decomposition for individuals living adjacent to newly constructed highways. Distance between homes again explains the largest share (15.5%). The remaining channels make smaller contributions, with none of them statistically significant. Overall, the channels account for only 17.2% of the mortality effect, less than half the share explained for displaced individuals. This pattern suggests that while proximity to highways negatively affects mortality, the underlying mechanisms differ in part from those operating through displacement.

9. Relocation Assistance

Most families displaced by highway construction received no compensation for the seizure of their homes.[32] Beginning in the early 1960s, states gradually adopted relocation assistance programs that provided payments to displaced tenants, but adoption was uneven: by 1963, only 11 states offered such assistance (Highway Research Board, 1963, p. 43). Whether these payments were able to mitigate the negative consequences of displacement is an empirical question. Even modest cash transfers can help vulnerable populations recover from disruption (Dwyer et al., 2023), but the average relocation payment was only $110 ($740 in 2010 USD), potentially too small to alter long-run trajectories.

To study the role of relocation payments, I exploit differences in adoption timing across states to compare outcomes for individuals displaced before versus after their state enacted assistance. By using enactment timing rather than actual receipt of payments, I

[32]Right-of-way payments were made to homeowners, but most residents of seized homes were tenants (U.S. Department of Transportation, 1967). Payment amounts varied across states: in 1959, Indiana's average was $12,900 ($95,000 in 2010 USD), while California's ranged from $9,789 to $14,430 ($72,000 to $106,000 in 2010 USD) (McCoy, 1958; Indiana, 1960).

avoid concerns about selection into receipt.[33] I construct a new dataset on the timing of state relocation assistance legislation. Appendix Figure F.3 shows that half of all states enacted assistance between 1965 and 1969. Details on the construction of this dataset are provided in Appendix Section F.

I estimate the following equation:

$$\begin{aligned} y_i = \gamma_1 \text{ Displaced}_i + \gamma_2 \text{ Displaced}_i \quad &\times \quad R_i + \delta_1 \text{ Adjacent}_i \\ &+ \quad \delta_2 \text{ Adjacent}_i \times R_i + \rho\, R_i + \mathbf{X}_i' \, \Gamma + \varepsilon_i \end{aligned} \tag{3}$$

where corresponds to the outcome of interest for individual i, Displaced_i is an indicator for whether individual i was displaced by highway construction, and Adjacent_i is an indicator for whether individual i lived within 100 meters of a newly constructed highway. I restrict the sample to individuals living in states that enacted relocation assistance between 1956 and 1969 and with displacement occuring both before and after enactment. The vector $\mathbf{X}_i$ includes the same controls as in equation 2, and standard errors are clustered at the state level. The variable R_i indicates whether construction of the highway nearest to individual i's home began after the state enacted relocation assistance. For displaced individuals, $R_i = 1$ indicates eligibility for payments, while for adjacent individuals, R_i acts as a placebo. The coefficient γ_2 captures the effect of relocation assistance on displaced individuals, while δ_2 captures any differential effects for adjacent individuals.

Relocation payments were an effective policy tool for mitigating the mortality consequences of displacement. Figure 9 presents the results of estimating equation 3 using age at death as the dependent variable. The two bars to the left correspond to the effects for those displaced, and the two bars to the right correspond to adjacent individuals. Among the estimates for those displaced, the left bar corresponds to the effect of displacement for individuals who were ineligible for relocation payments (i.e., γ_1) and the right bar corresponds to those who were eligible (i.e., $\gamma_1 + \gamma_2$). The same applies for adjacent individuals on the right side of the figure. Relative to those displaced without assistance, individuals displaced after their state enacted payments experienced an increase in life expectancy of 0.216 years (p-value = 0.005). This increase offsets nearly all of the mortality decline associated with displacement. As expected, the introduction of relocation payments does not affect estimated outcomes for adjacent individuals, who were ineligible to receive such payments. I also find that the effects of relocation assistance for displaced individuals is statistically larger than the effects for adjacent individuals ($\hat{\gamma}_2 > \hat{\delta}_2$, p-value=0.02), suggesting that the benefits of relocation assistance are not driven by general improvements in health care or other factors that may have coincided with the introduction of relocation

[33]Many eligible households likely never received payments, as most states and cities lacked the administrative infrastructure to facilitate disbursement. Incomplete take-up is common in U.S. social insurance programs: Moving to Opportunity vouchers had a take-up rate of 43 percent (Chetty et al., 2016a), and roughly half of eligible workers apply for Unemployment Insurance benefits (Moore and McQuillan, 2025).

payments.

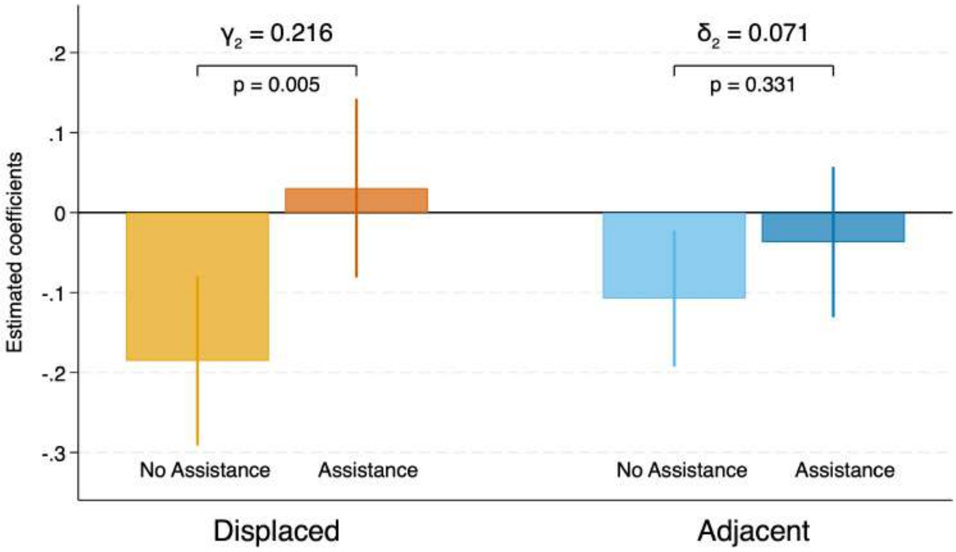


Figure 9: Effects of relocation assistance on longevity.

Notes: The figure compares the effect of Interstate highway construction on longevity for displaced individuals and those residing within 100 meters of the highway, with and without relocation assistance laws in place. The comparison group consists of unaffected neighbors living between 100 and 200 meters from the highway. All regressions control for the household head's occupational score and include fixed effects for gender, race, birth year, and metropolitan area of residence. Standard errors are clustered at the metropolitan area level. The coefficient γ_2 captures the differential effect of displacement for those with access to relocation assistance, and δ_2 captures the corresponding differential for adjacent individuals.

The results suggest that payments of a few hundred dollars offset nearly all mortality consequences of displacement. To quantify the return on investment, I estimate the monetary cost of displacement to be $9,825 in 2010 USD for each individual.[34] The average relocation payment to those displaced between 1965 and 1966 was $110, or $740 in 2010 dollars (U.S. Department of Transportation, 1967). Therefore, each dollar spent on relocation payments yielded $13.20 in benefits from expected mortality alone. These estimates suggest that relocation assistance generated substantial net gains, consistent with the net gains documented for other programs that help individuals find new living arrangements (Hendren and Sprung-Keyser, 2020; Chetty et al., 2016a).

The mortality benefits of relocation assistance do not appear to operate through improved residential outcomes. Appendix Figure F.1 presents effects of relocation payments on spatial mobility and neighborhood quality. I find no evidence that payments helped displaced individuals remain in their neighborhood or city, or that they moved to better areas. In line with the lack of payment elegibility, I find no effects for adjacent individuals on any of these outcomes.

[34]This equals the value of a statistical life year: VSL ($9.59 million in 2010 USD) divided by average life expectancy (77.5 years), multiplied by the mortality effect (0.079 years) (U.S. Department of Transportation, 2023; Kochanek et al., 2024).

A potential concern is that the introduction of relocation payments altered where or what type of projects were built (Cordes and Weisbrod, 1979). I find limited evidence of such selection. Appendix Figure F.2, Panel A, compares those displaced with and without relocation payment availability. Panel B repeats the exercise for adjacent individuals. Those displaced while eligible for payments were less likely to reside in redlined areas and had slightly better labor market outcomes, but the differences are modest.

Overall, these results suggest that the introduction of relocation assistance was an effective policy tool for mitigating the mortality consequences of displacement.

10. Conclusions

This paper studies the long-term consequences of the construction of the Interstate Highway System for those whose homes were destroyed and for those living in affected communities. Given that no systematic records of who was displaced exist, I develop a novel method to identify displaced households in historical censuses and link them to administrative mortality records. Displacement was widespread: more than 33,000 individuals were displaced annually between 1956 and 1970, mostly without relocation assistance. Construction disproportionately affected vulnerable populations, as the residents of these communities were more likely to be Black or to earn lower wages. I also find evidence of spillover effects on those living within 100 meters of the highway, though these effects dissipate rapidly and are not detectable beyond that distance.

The main analysis compares individuals displaced by construction or living within 100 meters of the highway to their neighbors living between 100 and 200 meters away. Displaced individuals die earlier, are more likely to leave their neighborhood, and relocate to worse neighborhoods, though they tend to remain in the same city. A back-of-the-envelope calculation suggests that the mortality cost of displacement is approximately $9,800 per individual (in 2010 dollars). There is also evidence of spillover effects on those living close to the highway, who die younger and relocate out of their neighborhood to other parts of the city. The results are robust across alternative specifications and identification strategies. Decomposing the mortality effect, I find that relocation farther from the original location and the destruction of social networks are the most important mediators. Finally, the introduction of relocation assistance was an effective tool in mitigating these effects: relocation payments fully offset the mortality consequences of displacement.

This paper has limitations that future research may address. Due to data constraints, the analysis focuses on the long-run consequences of highway construction for individuals. Recent advancements in linking restricted-access census data to tax records could enable a more comprehensive analysis of the short- and medium-run effects of forced relocation, particularly on labor market outcomes. Future work should also consider the general equilibrium effects of displacing large numbers of individuals on the communities that receive them.

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Appendix and Supplementary Material

A. Data Appendix

A.1 Cleaning historical addresses

I geocode the restricted access full-count censuses from 1940 to 1950 and focus on the 285 counties that contained one of the 168 Standard Metropolitan Areas in 1950. (Ruggles et al., 2024).[35] Rural addresses usually have missing street names and enumeration, introducing inaccurate geocoding. For this reason, I focus only on urban areas in these counties. The sample includes 74.28% of the urban households in the U.S. in 1950. To clean historical addresses, I follow the procedures recommended by Logan and Zhang (2018). For many observations, the address information is either incomplete or include additional information that needs to be cleaned. In particular, I start by parsing raw addresses and numerations.

To parse the addresses, I assume that enumerators stayed on the same side of the street when moving from building to building and then followed the algorithm described below:

1. *Dwellings IDs:* I work with the serial ID provided by IPUMS, as at the moment of writing there is a coding issue with IPUMS' *dwelling ID* variable in which every household is incorrectly coded in a unique dwelling (Ager et al., 2023). Street name and numeration are constant across serial IDs. I also keep the state, county, city, and enumeration district.

2. *Extract the street name:* Sometimes it contains additional information. For example, it may include a word like "Cont." or "Rear". In these cases, I consider the street name to be the same as the previous record. In some cases, the street name contains the house number, for example, in large apartment complexes, hotels, or hospitals. In those cases, I store the house number by parsing the street name in the search for street numbers in addition to other keywords such as apartment, hospital, and hotel.

3. *Carrying forward a street name:* Some addresses have a valid house number but no street names. To fill in the missing information, I carry forward the street name from the previous record under two conditions. First, the two records should be on the same enumeration page. Second, the adjacent records should not have a skip in the house number larger than 6, taking into consideration if the numeration is odd or even.

[35] Standard Metropolitan Areas are the equivalent to Metropolitan Statistical Areas in the 1950 census.

4. *Cleaning house numbers:* There is considerable variation in the way house numbers are recorded. To standardize the records, I set to "missing" any record that includes the following fields:

   (a) A continuation of the previous house number: 'cont', 'con't', 'contd', 'cont'd', etc.

   (b) A location relative to the previous house number: 'rear', 'basement', '1/2', 'back', 'front'. Unless the house number is within the text (5147rear), these records are set to missing.

   (c) A house number that indicates a floor, indicated by 'floor' or 'fl' in the text.

   (d) Apartment numbers indicated by 'apt' in the text.

   (e) A combination of numbers and letters that do not follow the standard format of a house number, like '[0-9][ ][a-zA-Z]' or '[0-9][-][a-zA-Z]'. For example: 7-B. These cases are most likely the rooms in a hotel.

5. *Dealing with missing house numbers:* I interpolated missing house numbers in a similar way as I did with missing street names. The only difference is that I take into consideration the side of the street. Some interpolations result in suspicious numbers and are cataloged as missing. For example, the interpolation may result in house numbers far outside the logical range of house numbers. To identify these anomalies, I compare the interpolated house number to the range of house numbers on the same side of the street in the same ED. If the interpolated house number is outside the range or has a skip larger than 6, I set it to missing.

Once the historical addresses are standardized, I proceed to clean them by using the historical addresses for each decade available in *StevenMorse.org*. This website includes addresses for the 1910, 1920, 1930, 1940, and 1950 censuses in each enumeration district. This allows me to overcome possible errors in the OCR process of the historical addresses or during the standardization of the data. To do so, I perform a probabilistic match between the standardized street name and the street name in SM's records for each ED. I do a Jaro-Winkler distance match between the standardized street name and the street name in SM's records, with a similarity tolerance greater than 0.8. I use the street name in SM's records for those records with a match. If there is no match, due to the similarity being too low or no match, I use the street in the census records.

Once I cleaned and standardized, I proceeded to geocode the census addresses. Given that the confidentiality agreement signed with Ruggles et al. (2024) does not allow using cloud geocoding, I rely on ArcGIS Street Map Premium. The software does the geocoding within your computer, circumventing the use of external geocoders. I dropped all the observations that had suspicious geocoding. In particular, I set the geocoding to missing those records that are geocoded to different cities, counties, or states, even if the match addresses belong to the same metropolitan areas.[36] I also drop the geocoding for those records with a *match score* lower than 85 and those without a unique match, as in Hynsjö and Perdoni (2022).[37] In addition, I also drop those matches that do not allow me to identify an address, such as those that only match the street. The cleaning and geocoding procedure leaves me with 12,309,022 geocoded households in the 1950 census, which accounts for 58.53% of urban households in the counties of study. Appendix Table A.1 shows a breakdown of the number of geocoded dwellings by state.

A.2 Identifying displacement

The definition of displaced individuals is based on the location of their residence in 1950 relative to the highway network. In specific, I classify an individual as displaced if their residence is within the right-of-way area of a highway segment, which I define as a buffer around the highway segment with a width equal to the right-of-way acquisition for that segment. For every individual, I estimate the distance to the nearest highway and flag the record as displaced if the distance to the buffer is zero. As discussed before, modern geocoding engines can interpolate the record's location from the range of addresses nearby. Thus, if highway construction destroys a segment of the street, the geocoder will still match the address to the correct street segment, and hence, those individuals would be included in the sample as displaced, even if their exact street segment is destroyed. In addition, I classify an individual as living next to a highway if the dwelling is within 100 meters of the highway buffer.

A.3 Benchmarking geocoding accuracy

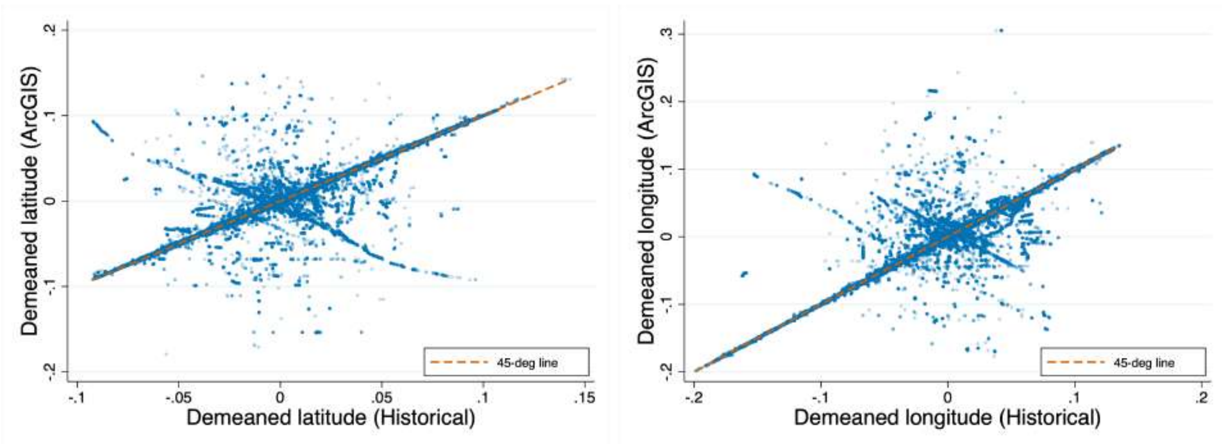
Geocoding historical addresses with modern geocoders could be problematic because street names and numerations may change over time. However, I do not think these concerns invalidate the results. First, I only study those individuals living in dwellings, which the geocoder was able to match, and the match passed all the filters mentioned. If an address is not matched, either because the street is destroyed or the street has changed its name, the record will not be considered in the analysis. This leads to fewer observations to work with, but an accurate geocoding. Second, modern geocoders are equipped to handle missing numeration. This is particularly helpful when the reason behind the missing numeration is highway construction. In this case, the geocoder will match the address to the street segment that is closest to the original address. The geocoder flags

[36] For example, the address 24 SW 3rd Ave in Miami was geocoded to 24 SW 3rd Ave in the city of Boca Ratón. These types of matches are not in the final sample.

[37] The match score of a candidate address ranges from 0 to 100. A score of 100 corresponds to a perfect match. The score is penalized according to the number of changes the geocoder needs to do to match the address.

this type of match as *StreetAddress* and penalizes the match score accordingly. Thus, these observations' geometry will come from an interpolation at the block level, minimizing the location error. In other words, these observations will be located in the correct block, and their exact location within this block will come from a linear interpolation based on their enumeration. As a robustness and to minimize measurement errors, I re-estimate the main specification using only observations with a high match score. These observations are the ones that, in addition to passing all the aforementioned filters, have a match score over 98. Figure A.9 shows that the main results remain unchanged when I use this more restrictive sample.

Finally, using historical street grids from 1940 I construct a locator for nine cities. These cities are: Albany, NY; Buffalo, NY; Cincinnati, OH; Cleveland, OH; Dayton, OH; Louisville, KY; Spokane, WA; Springfield, MA; and St. Louis, MO. The historical street grids were created by the *Urban Transition Historical GIS Project* and are available at `https://s4.ad.brown.edu/Projects/UTP2/citymaps.htm`. [38] These grids predate highway construction and thus provide the "ground truth" against which to benchmark modern geocoding accuracy.



(a) Latitude

(b) Longitude

Figure A.1: Correlation between modern and historical geocoding results.

Notes: Each point corresponds to a household in the 1950 census. The y-axis corresponds to the coordinates obtained with the modern geocoder, and the x-axis corresponds to the coordinates obtained with the historical geocoder. The coordinates are demeaned by city average coordinates to make the figure more readable. The cities included in the figure are: Albany, NY; Buffalo, NY; Cincinnati, OH; Cleveland, OH; Dayton, OH; Louisville, KY; Spokane, WA; Springfield, MA; and St. Louis, MO.

The two geocoders yield highly correlated locations, as shown in Figure A.1. More than 65% of the locations fall within 40 meters, which is considered within the same parcel. Figure A.2 shows the CDF of the distance between the two coordinates. When the coordinates do not match, there is no directional bias in the difference between the matched locations, as shown in Figure A.3, suggesting random measurement error. Also, in Ap-

[38] Accessed February 10, 2026.

pendix Figure A.9, I use the historical geocoder results to re-estimate the paper's main findings, which, if anything, become stronger.

Finally, Appendix Figure A.4 presents column-conditional confusion matrices comparing treatment classifications produced by the modern and historical geocoders. Each cell reports the share of individuals assigned to a given historical-geocoder category (column) who fall into each modern-geocoder category (row). Panel (a) presents results for the full sample. The two geocoders agree closely for distance bands, with diagonal shares typically exceeding 70 percent. Agreement is weaker near the displacement boundary: among individuals classified as displaced by the historical geocoder, 38 percent are classified into the adjacent 0–100 meter band by the modern geocoder. This discordance reflects the difficulty of distinguishing displacement from near-adjacency when geocoded locations fall close to the right-of-way edge, as modern geocoders assign addresses on street segments razed by highway construction to the closest intersection of existing roads. The conservative right-of-way width assumptions discussed in the previous section may further contribute, leading some displaced individuals to be classified as adjacent. Given the focus on displacement, I ease on the side of caution by classifying these individuals as adjacent, which may attenuate the estimated displacement gap and inflate the spillover effects on adjacent individuals, rather than the opposite. Panel (b) excludes individuals geocoded within 25 meters of band boundaries, after which the diagonal share for displaced individuals rises to 64.7 percent and off-diagonal mass declines throughout. Figure A.9 shows that the main estimates remain virtually unchanged when dropping these observations, indicating that classification uncertainty at the margin does not drive the results.

As a conclusion, modern geocoders *may* miss some addresses or misclassify some, but the ones they match are accurate and reliable.

A.4 Outmigration from the 1950 residence

The displacement gap captures differences between individuals who lived in a house that was destroyed by a highway and those who, due to idiosyncratic reasons, lived in a house that was not destroyed. Because highway construction started in 1956, individuals could have moved out their houses before the highway construction started, biasing the estimates. Therefore, some individuals may wrongly be classified as displaced. This type of misclassification is a for of classical measurement error, which leads to attenuation bias in the estimated displacement gap. In this section, I will bound the true displacement gap by exploiting the mobility of individuals between 1950 and 1956.

Without loss of generality, I will only focus on the displacement treatment. The effect taking into consideration individuals living close by is analogous. The bounding exercise

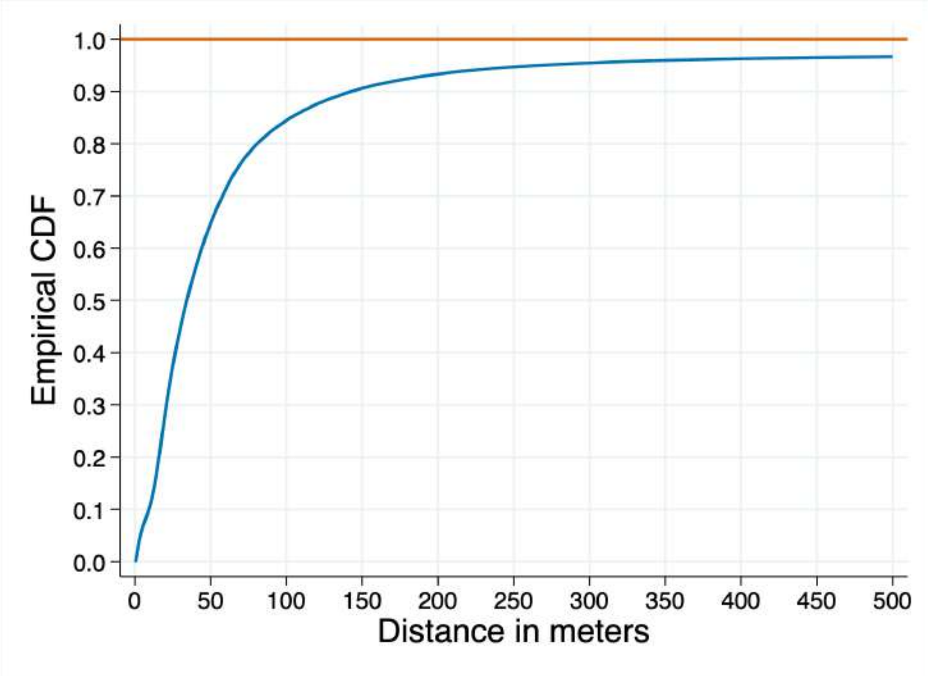


Figure A.2: CDF of distance between the coordinates obtained with both geocoders.

Notes: CDF of the distance between the coordinates obtained with the modern geocoder and the historical geocoder. Distance is measured in meters and presented in the x-axis. The y-axis corresponds to the share of observations with a distance smaller than the value in the x-axis. The cities included in the figure are: Albany, NY; Buffalo, NY; Cincinnati, OH; Cleveland, OH; Dayton, OH; Louisville, KY; Spokane, WA; Springfield, MA; and St. Louis, MO.

abstracts from the controls included in the main specification.[39] The estimated displacement gap is given by:

$$y_i = \alpha + \beta \cdot D_i^{50} + \varepsilon_i$$

where D_i^{50} is an indicator that equals one if the individual lived in a house in 1950 that was later destroyed by highway constuction. However, displacement is determined by the location of the individual when construction occured, which I will call D_i^{56}. Thus, the *true* effect of displacement is given by the following equation:

$$y_i = a + b \cdot D_i^{56} + e_i$$

where b corresponds to the *true* parameter.

The estimated displacement gap in Section 7 can be written as:

$$\hat{\beta} \xrightarrow{p} \mathbb{E}[y_i|D_i^{50} = 1] - \mathbb{E}[y_i|D_i^{50} = 0] = b \cdot \left(\mathbb{E}[D_i^{56}|D_i^{50} = 1] - \mathbb{E}[D_i^{56}|D_i^{50} = 0]\right) \tag{A.1}$$

[39]Using controls would result in a slightly more complicated expression for the bias. After partialling out controls using FWL, the treatment indicator would no longer be binary. However, the intuition of the bounding exercise would remain the same.

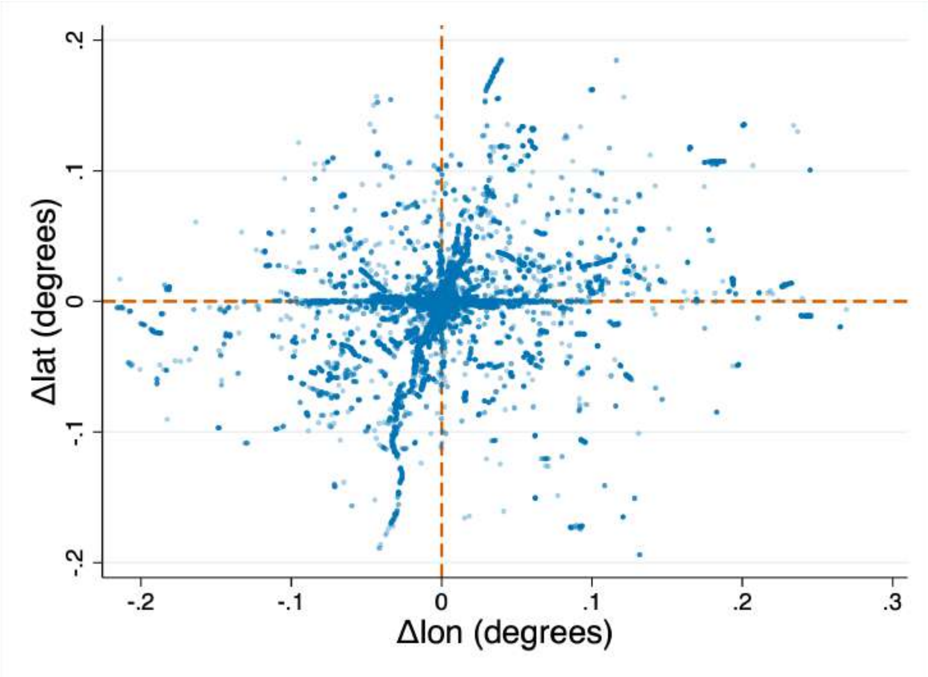


Figure A.3: Directional bias between modern and historical geocoding results.

Notes: Each point corresponds to a household in the 1950 census. The y-axis corresponds to the difference in latitude between the modern and historical geocoding results. The x-axis corresponds to the difference in longitude between the modern and historical geocoding results. The cities included in the figure are: Albany, NY; Buffalo, NY; Cincinnati, OH; Cleveland, OH; Dayton, OH; Louisville, KY; Spokane, WA; Springfield, MA; and St. Louis, MO.

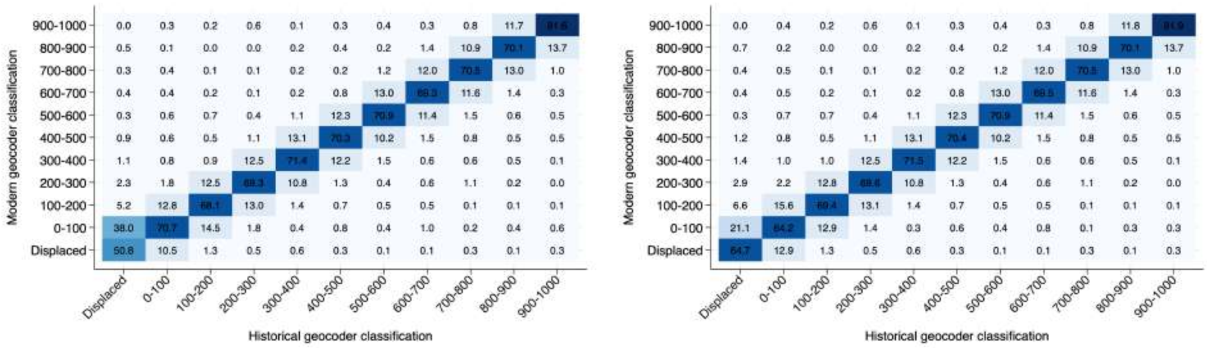
We can exploit this expression to bound the true displacement effect, b. If there was no mobility and all individuals stayed in their houses from 1950 to 1956, we would have that $\hat{\beta} \xrightarrow{p} b$.[40] If the probability of living in a place that was destroyed by a highway in 1956 is larger if you were living in a house that would be destroyed in 1950 than if you were not, then we have that $\hat{\beta} \xrightarrow{p} b \cdot \left(\mathbb{P}(D_i^{56} = 1 | D_i^{50} = 1) - \mathbb{P}(D_i^{56} = 1 | D_i^{50} = 0)\right)$.[41] Thus, we can bound the true effect of displacement as:

$$b \in [\hat{\beta}, \hat{\beta} / \left(\mathbb{P}(D_i^{56} = 1 | D_i^{50} = 1) - \mathbb{P}(D_i^{56} = 1 | D_i^{50} = 0)\right)]$$

I can empirically assess this bias by analyzing the likelihood for a person to stay (or move) into a house that would be destroyed by a highway by study census-to-census migration. For that end, I geocode the 1940 census and use the linkage provided by Ruggles et al. (2025) to match individuals in the 1950 census to the 1940 census. I then calculate the share of individuals who lived in 1940 and 1950 in a house that would be destroyed by a highway in 1956, and those who moved between 1940 and 1950 into property that will

[40]Under no mobility, we have that $\mathbb{P}(D_i^{56} = 1 | D_i^{50} = 1) = 1$ and $\mathbb{P}(D_i^{56} = 1 | D_i^{50} = 0) = 0$.

[41]It seems reasonable to assume that the likelihood of staying in a house that would be destroyed by a highway is larger if you were already living in a house that would be destroyed by a highway. This assumptions ensures that the true effect of displacement is larger than the estimated effect, and that the upper bound is finite.



(a) All households

(b) Dropping those within 25m of the highway

Figure A.4: Heatmap of correlation between modern and historical geocoding results.

Notes: Panel (a) shows the full sample; Panel (b) excludes individuals whose modern-geocoder distance to the highway is between 0 and 25 meters. Each cell reports the column-conditional percentage of individuals classified into a given modern-geocoder distance bin (y-axis) among those assigned to a particular historical-geocoder distance bin (x-axis). Distance bins are measured in meters from the highway network built before 1970. The modern geocoder uses ArcGIS address matching to current-day coordinates; the historical geocoder uses parcel-level geocoding to period-appropriate coordinates. The sample includes individuals from Albany, NY; Buffalo, NY; Cincinnati, OH; Cleveland, OH; Dayton, OH; Louisville, KY; Spokane, WA; Springfield, MA; and St. Louis, MO., restricted to those within 1,000 meters of a highway. "Displaced" denotes individuals whose residence falls within the highway right-of-way. Panel (b) removes near-boundary cases where small geocoding discrepancies are most likely to alter treatment classification.

be destroyed.[42] This is a noisy measure of *remaining in the same property* because trends in the neighborhood could be different between 1940 and 1950 than they could be between 1950 and 1956. However, this exercise is still informative to assess the potential bias in the estimated displacement gap. I find that,

$$\begin{aligned}
\hat{\mathbb{P}}(D_i^{50} = 1 | D_i^{40} = 1) &= 0.333 \\
\hat{\mathbb{P}}(D_i^{50} = 1 | D_i^{40} = 0) &= 0.006 \\
\hat{\mathbb{P}}(A_i^{50} = 1 | A_i^{40} = 1) &= 0.401 \\
\hat{\mathbb{P}}(A_i^{50} = 1 | A_i^{40} = 0) &= 0.016
\end{aligned}$$

suggesting that the *true* gap for displaced individuals is between $\hat{\beta}$ and $\sim 3.05 \cdot \hat{\beta}$. Doing the analogous exercise for adjacent individuals yields that $b^A \in [\hat{\beta}^A, 2.6 \cdot \hat{\beta}^A]$

A.5 Highways data

Highway information comes from Open Street Maps (OSM). I download the actual network of highways and their exits from OpenStreetMap (2017) and then link it to the PR-511 database from Baum-Snow (2007) to get the opening date of each highway segment financed by the Highways Act. Since I'm interested in the displacement effect of highway

[42] I assume that if the residence of an individual in the 1950 census was within 30 meters of the residence in the 1940 census, then the individual stayed in the same property.

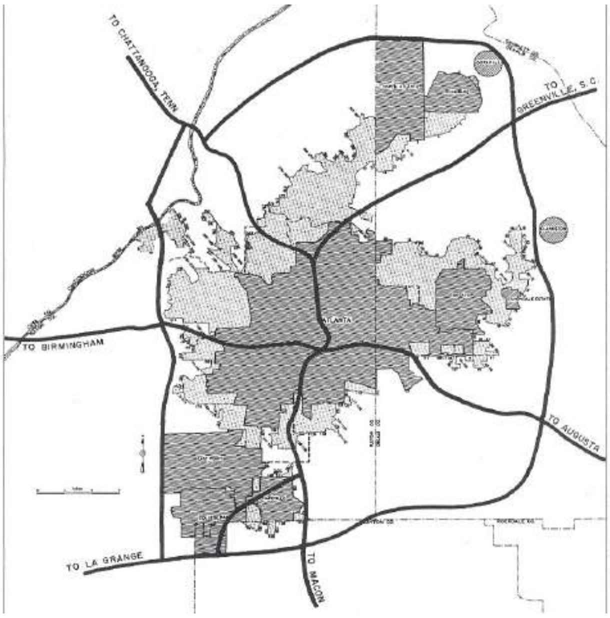
construction, I include exits as part of the highway database. Since I observe the residence in 1950, I only consider the highway segments that were opened between 1956 and 1970. The network in OSM is recorded as *Polylines* with negligible width, whereas in reality, highways are polygons.

To estimate the right-of-way acquisition for highway construction, I assume that a two-lane highway had a width of 150 feet (45.7 meters), the minimum width used in the 1950s (Weingroff, 2017).[43] For segments with more than two lanes, I add 12 feet (3.66 meters) per additional lane, equal to the average lane width (Federal Highway Administration, 2007).[44] Using these width assumptions, I construct a buffer around each highway segment representing the right-of-way area. For all households, I calculate the distance to the nearest highway segment. I define displaced households as those whose 1950 residence fell within this buffer. I apply the same methodology to planned highway routes from Federal engineering maps created in 1955 (the Yellow Book) (Bureau of Public Roads, US, 1955), which covered 82 metropolitan areas. Thus, for each household, I can determine whether it was displaced by highway construction, the distance to the nearest highway segment, and whether it was within the planned route of the highway system. Panel (b) of Appendix Figure A.7 shows the highway network in Cleveland, Ohio, in 1950. The solid red lines represent highway segments, and the light red area corresponds to the buffer around each segment.

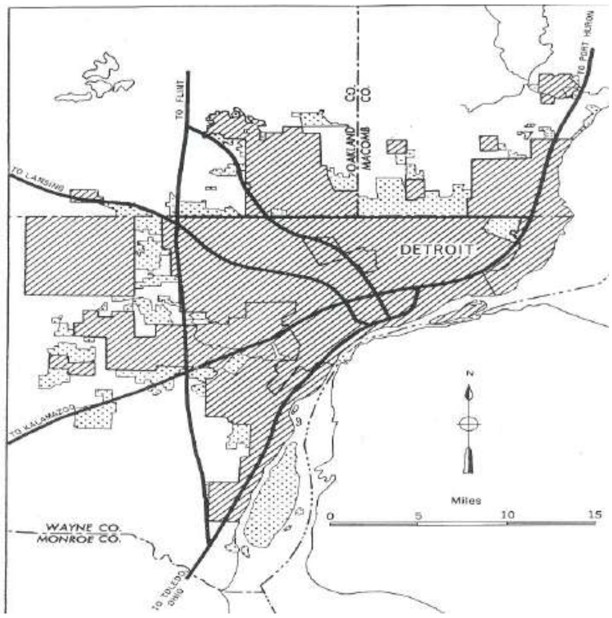
[43]The modal and median number of lanes in the sample is two.

[44]If actual right-of-way widths exceeded my assumptions, some displaced individuals would be misclassified as adjacent. Therefore, the spillover effects on adjacent individuals could be driven by misclassification of displaced individuals. However, the results are robust to dropping individuals living within 25 meters of the highway, which would be the case if the right-of-way width was 82 feet (25 meters) larger than my assumptions (Appendix Figure A.9).

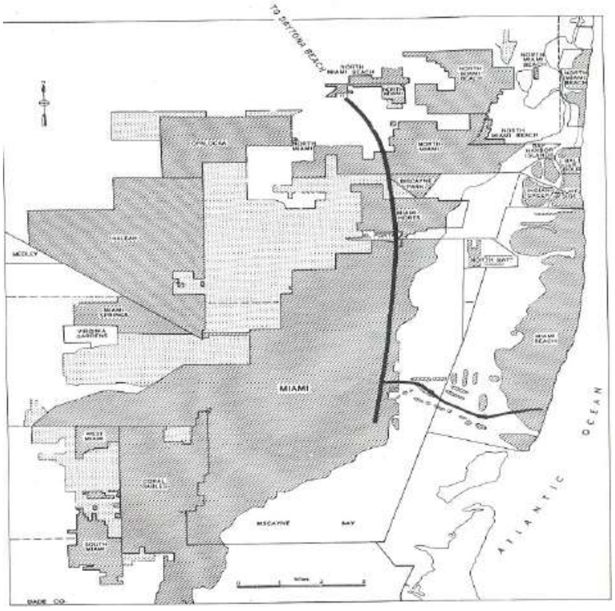
A.6 Additional figures



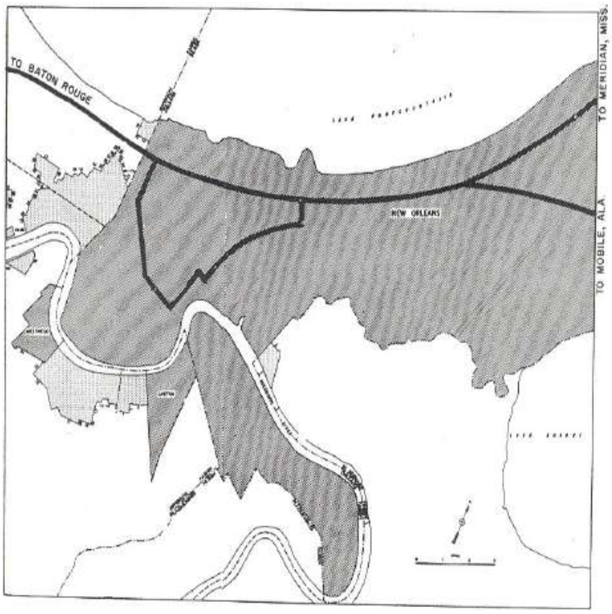
(a) Atlanta



(b) Detroit



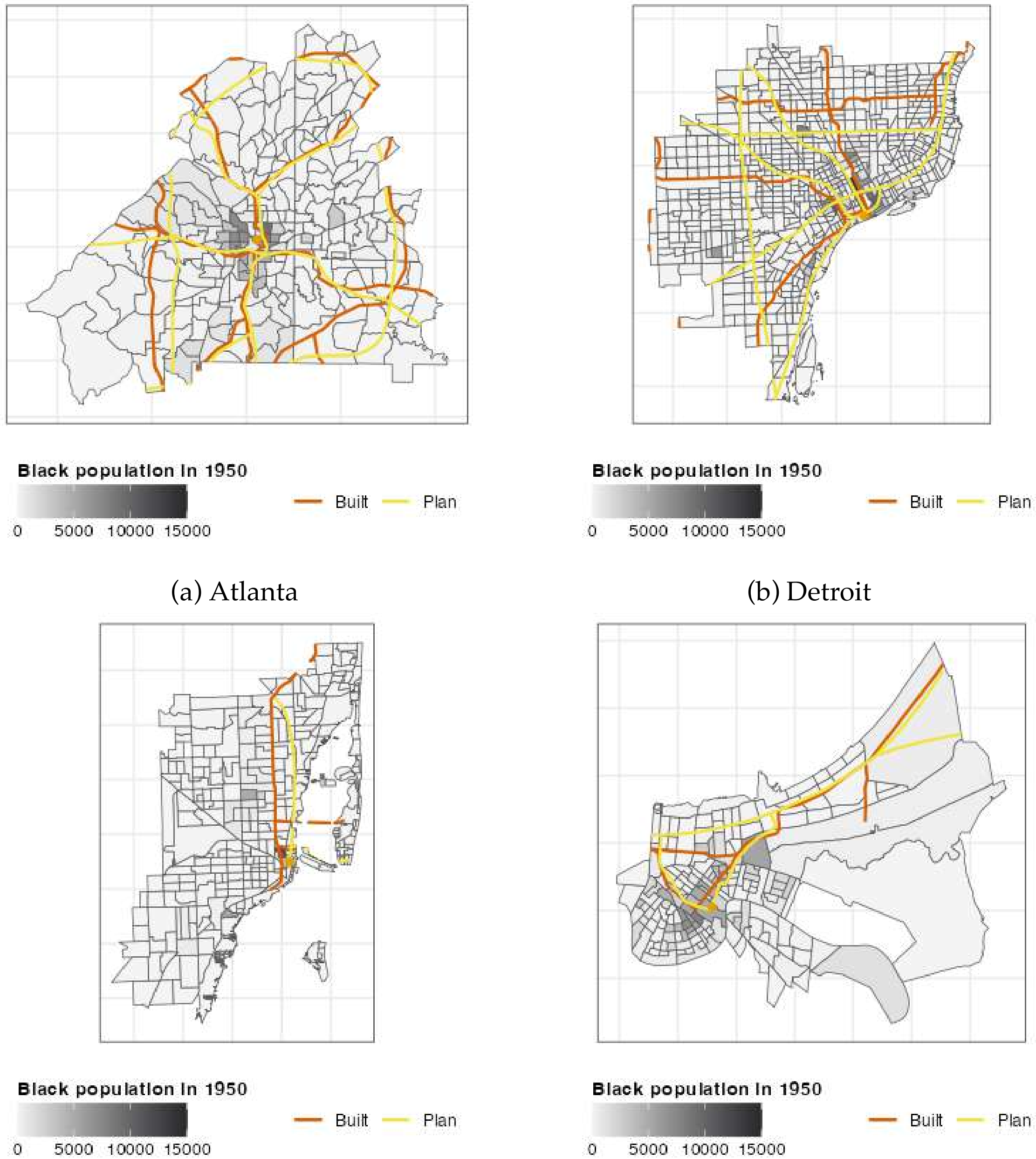
(c) Miami



(d) New Orleans

Figure A.5: Yellow Book Maps

Notes: The figure includes the maps in the Yellow Book for the cities of Atlanta, Detroit, Miami, and New Orleans.



(a) Atlanta

(b) Detroit

(c) Miami

(d) New Orleans

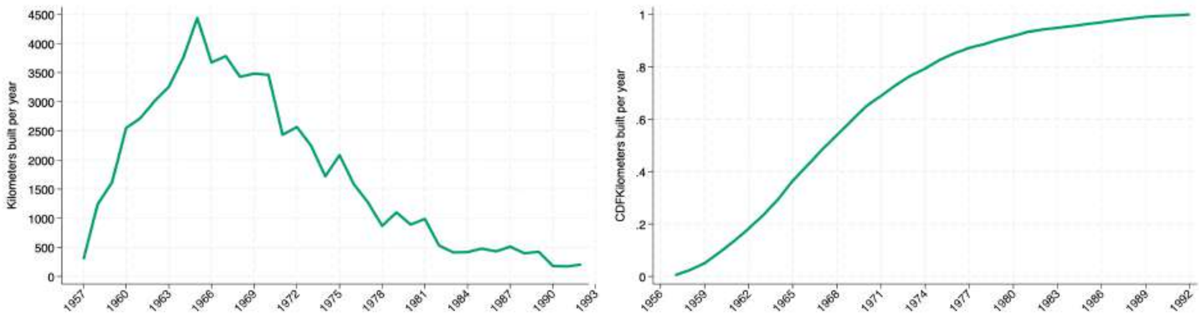
Figure A.6: Racial Distribution, Highways, and Planned Routes

Notes: The figure includes maps for Atlanta, Detroit, Miami, and New Orleans. Each observation is a census tract, and its filling corresponds to the number of Black residents in the tract in 1950. Depicted in red is the highway network that was built. The network planned in the Yellow Book is presented in yellow. Finally, the city center is plotted in orange.

(a) Geocoded households (b) Highway network

Figure A.7: Example of geocoded households in the 1950 census.

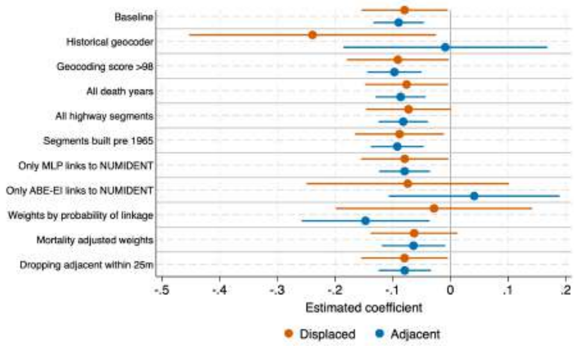
Notes: Each green point corresponds to a household in the 1950 census. Panel (a) shows the geocoded households in Cleveland, Oh, and panel (b) shows the highway network later built.



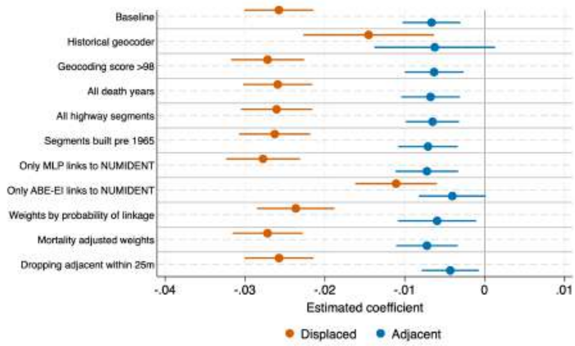
(a) Time-series (b) Cumulative share

Figure A.8: Kilometers of Interstate highways built per year.

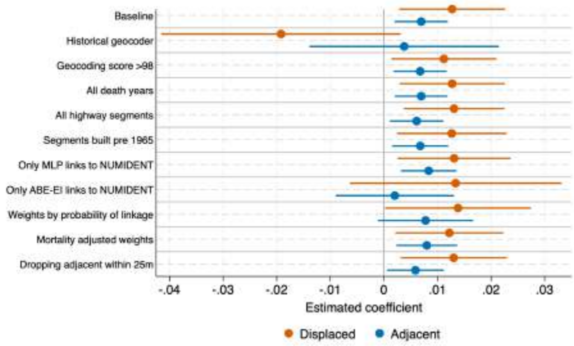
Notes: The data in this figure comes from Brooks and Liscow (2023). Panel (a) shows the kilometers of Interstate highways built per year, and panel (b) shows the share of total kilometers built up until each year.



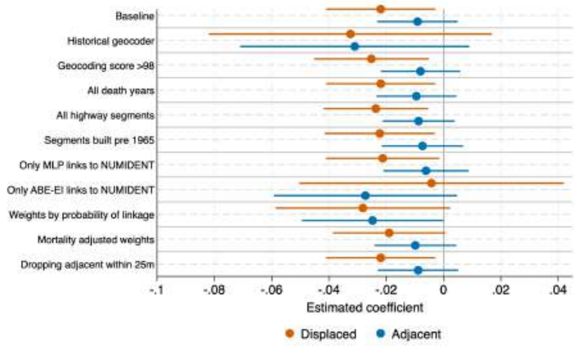
(a) Death age



(b) Same neighborhood



(c) Same city



(d) Neighborhood quality

Figure A.9: Robustness of the results to different geocoding methods.

Notes: The figures display estimates from regressions of the main outcomes on the two treatment indicators in equation 2, along with 95% confidence intervals. Each row corresponds to a different regression, and the sample includes individuals living within 200 meters of the newly constructed highway in the 1950 census. All regressions include household head's occupational score, as well as gender, race, birth year, and metropolitan area fixed effects. Standard errors are clustered at the metropolitan area level. The top panel corresponds to the baseline results. *Historical geocoders* reports results obtained using a geocoder constructed from historical street grids. *Geocoding score > 98* restricts the sample to addresses with a geocoding match score above 98. *All death years* uses all NUMIDENT records, including those for individuals who died before 1988. *All highway segments* includes all segments in the Interstate Highway System, while *Built pre-1965* restricts to segments completed before 1965. *MLP only* restricts the sample to individuals linked to the NUMIDENT database using Ruggles et al. (2025) linkages; *ABE-EI only* applies the same restriction using Abramitzky et al. (2024) algorithm. *Weights by linkage probability* weights each observation by its estimated probability of being linked to the NUMIDENT database. *Mortality-adjusted weights* applies the Breen and Goldstein (2022) weights to account for differential coverage of the Social Security Number sample. The bottom row drops all adjacent individuals residing within 25 meters of the highway right-of-way.

A.7 Additional tables

Table A.1: Geocoded households by state

State	Households	Geocoded	Share
Alabama	219,546	118,025	53.76%
Arizona	52,000	34,155	65.68%
Arkansas	48,697	30,294	62.21%
California	2,260,200	1,419,042	62.78%
Colorado	175,096	98,000	55.97%
Connecticut	318,914	194,638	61.03%
Delaware	38,251	22,571	59.01%
District of Columbia	261,726	170,954	65.32%
Florida	300,689	215,877	71.79%
Georgia	239,220	13,275	5.55%
Illinois	1,754,758	1,044,632	59.53%
Indiana	427,419	294,591	68.92%
Iowa	179,858	130,924	72.79%
Kansas	125,027	79,805	63.83%
Kentucky	194,775	124,547	63.94%
Louisiana	263,824	183,648	69.61%
Maine	37,961	22,628	59.61%
Maryland	311,664	207,771	66.67%
Massachusetts	1,001,544	537,184	53.64%
Michigan	994,475	624,279	62.77%
Minnesota	371,015	215,124	57.98%
Mississippi	29,770	16,057	53.94%
Missouri	582,270	352,632	60.56%
Nebraska	114,856	79,519	69.23%
New Hampshire	37,098	18,176	48.99%
New Jersey	1,008,146	596,849	59.20%
New Mexico	36,024	18,811	52.22%
New York	3,451,076	1,788,624	51.83%
North Carolina	166,316	70,066	42.13%
Ohio	1,327,072	739,360	55.71%
Oklahoma	154,549	92,802	60.05%
Oregon	147,216	112,643	76.52%
Pennsylvania	1,722,267	1,044,917	60.67%
Rhode Island	183,745	120,681	65.68%
South Carolina	95,112	37,228	39.14%
South Dakota	17,086	8,054	47.14%
Tennessee	272,132	148,219	54.47%
Texas	960,195	587,464	61.18%
Utah	85,641	59,098	69.01%
Virginia	290,152	152,936	52.71%
Washington	318,449	224,738	70.57%
West Virginia	105,695	57,976	54.85%
Wisconsin	347,823	200,208	57.56%
Total	21,029,348	12,309,022	58.53%

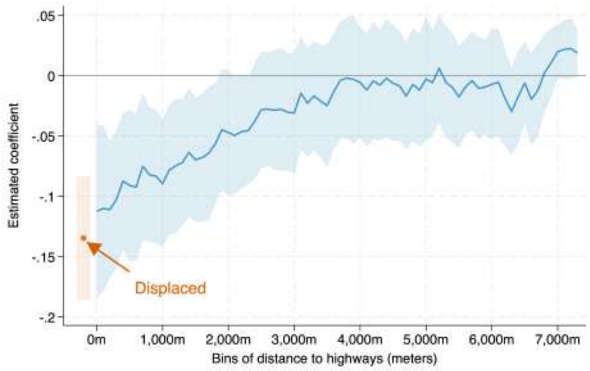
Table A.2: Probability of linkage to NUMIDENT by treatment status

	(1)	(2)	(3)	(4)	(5)	(6)
Displaced	-0.00174^{b}	-0.00124^{c}	-0.00067	-0.00082	-0.00088	-0.00096^{c}
	(0.00080)	(0.00070)	(0.00064)	(0.00057)	(0.00056)	(0.00056)
Adjacent	-0.00019	0.00003	0.00019	0.00010	0.00016	0.00006
	(0.00047)	(0.00039)	(0.00038)	(0.00035)	(0.00036)	(0.00036)
Observations	2,874,666	2,874,666	2,874,666	2,874,661	2,874,661	2,861,346
Adjusted R-squared	0.000	0.001	0.001	0.044	0.063	0.063
Mean dependent variable	0.089	0.089	0.089	0.089	0.089	0.089
Std. dev. dependent variable	0.285	0.285	0.285	0.285	0.285	0.285
MSA FE	No	Yes	Yes	Yes	Yes	Yes
Race FE	No	No	Yes	Yes	Yes	Yes
Birth year FE	No	No	No	Yes	Yes	Yes
Gender FE	No	No	No	No	Yes	Yes
HH's occ. score	No	No	No	No	No	Yes

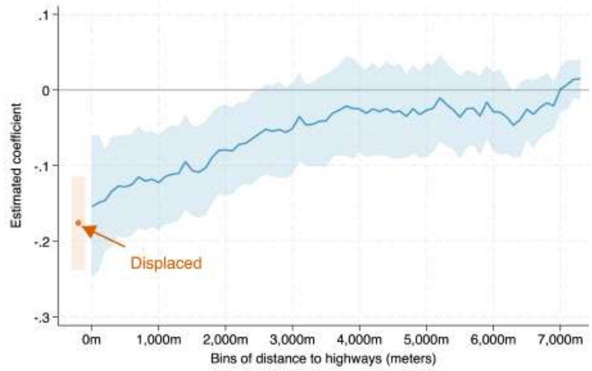
Note: OLS estimates are reported. Coefficients are reported with standard errors, clustered at the MSA level, in parentheses. The unit of observation is an individual in the 1950 census. The dependent variables is an indicator of whether the individual was linked to a NUMIDENT in the 1950 census. The sample includes individuals living within 200 meters of the newly constructed highway in the 1950 census. ^{a} indicates the coefficient is significant at the 1%, ^{b} at the 5%, and ^{c} at the 10% level.

B. Placement Appendix

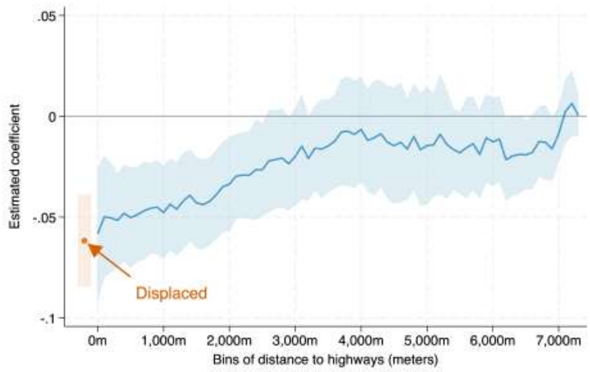
B.1 Additional results using 1950 microdata



(a) Middle school completion



(b) High school completion



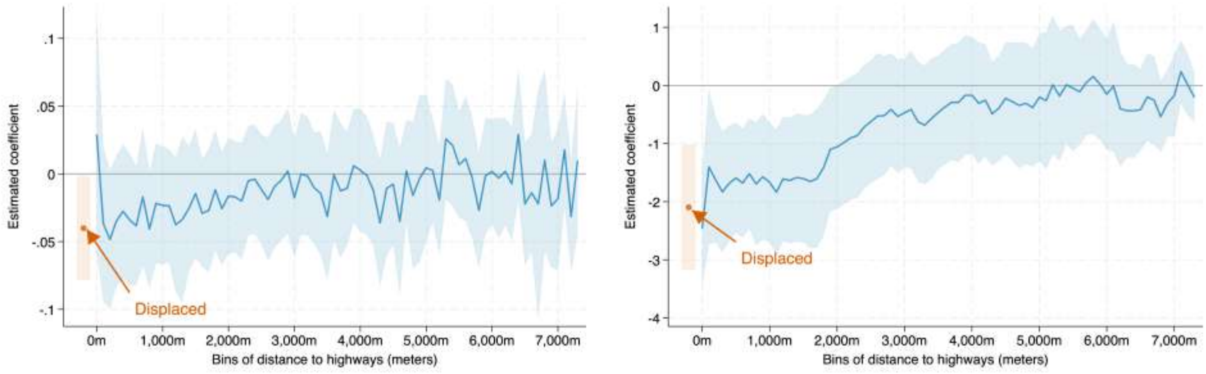
(c) College completion

Figure B.1: Pre-construction educational characteristics by distance to future Interstates.

Notes: The figures present the estimated coefficients and confidence intervals of equation 1. The blue line plots the distance coefficients β_k with 95% confidence intervals shown as shaded bands, and the red marker plots the coefficient for displaced individuals. The sample corresponds to individuals living in metropolitan areas and were living closer than 7,500 meters to a future Interstate in 1950. The comparison group corresponds to those living between 7,400 and 7,500 meters from the future Interstate. The dependent variables are reported in the name of each panel. All regressions include metropolitan area fixed effects and control for the individual's log distance to the city center. Standard errors are clustered at the metropolitan area level.

B.2 Analysis using census tracts

In this section, I present cross-sectional evidence on the relationship between neighborhood's socioeconomic composition and geographic characteristics with future highway construction. To do this, I exploit variations among census tracts in 1950, the last recorded census before the 1956 Federal-Aid Highway Act that initiated highway construction. I transform the 1950 census tract data into the 2010 census tract definition using area-



(a) log Weekly income (b) Occupational score

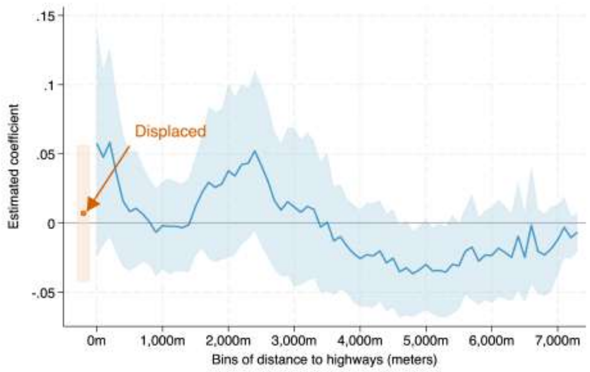
Figure B.2: Pre-construction labor characteristics by distance to future Interstates.

Notes: The figures present the estimated coefficients and confidence intervals of equation 1. The blue line plots the distance coefficients β_k with 95% confidence intervals shown as shaded bands, and the red marker plots the coefficient for displaced individuals. The sample corresponds to individuals living in metropolitan areas and were living closer than 7,500 meters to a future Interstate in 1950. The comparison group corresponds to those living between 7,400 and 7,500 meters from the future Interstate. The dependent variables are reported in the name of each panel. All regressions include metropolitan area fixed effects and control for the individual's log distance to the city center. Standard errors are clustered at the metropolitan area level.

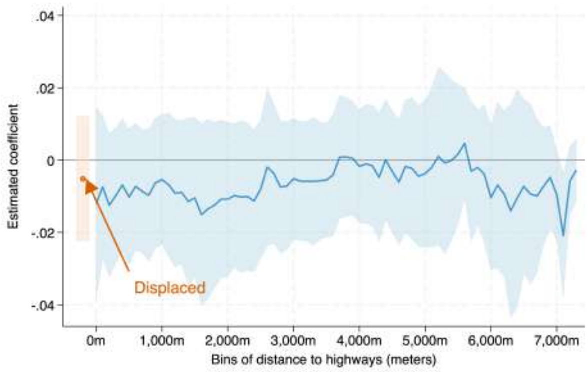
weighted interpolation, following Lee and Lin's (2018) crosswalk. The estimating equation follows:

$$y_n = \lambda_{c(n)} + \gamma \log(\text{DistCBD})_n + \beta \text{Black Share}_n + \Gamma_1' \mathbf{S}_n + \Gamma_2' \mathbf{P}_n + \Gamma_3' \mathbf{G}_n + \varepsilon_n \qquad \text{(B.1)}$$

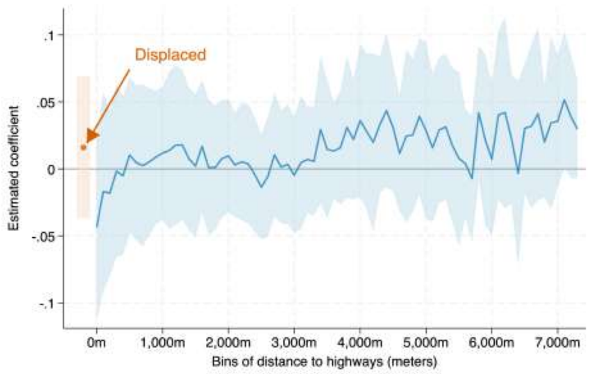
The sample consists of census tracts from 62 metropolitan areas that have spatial information available for 1950. In this equation, n indexes census tracts, and $c(n)$ indexes the metropolitan area in which the census tract is located. The dependent variable, y_n, takes a value of one if a highway was built through the census tract and zero otherwise. The variable $\log(\text{DistCBD})_n$ is the log of the distance to the central business district, which is included to account for the fact that highways were built to connect city centers and Black households sorted themselves into city centers (Boustan, 2010). The vector $\mathbf{S}_n$ contains socioeconomic characteristics of the tract, such as the share of the city's Black population residing in the tract, the median income, and the share of the adult population with a high school degree. The vector $\mathbf{P}_n$ contains contols for the log median rent and the log median home value. These controls are included to account for the price of land in the neighborhood. The vector $\mathbf{G}_n$ contains geographic and state controls, including log average slope in degrees, the log area, and the distance to the nearest river, railroad network, number of cars per 10,000 inhabitants, and the governor's political party. $\lambda_{c(n)}$ corresponds to the city fixed effects. The regression results are weighted by the total population of tract n in 1950, and the standard errors are clustered at the city level.



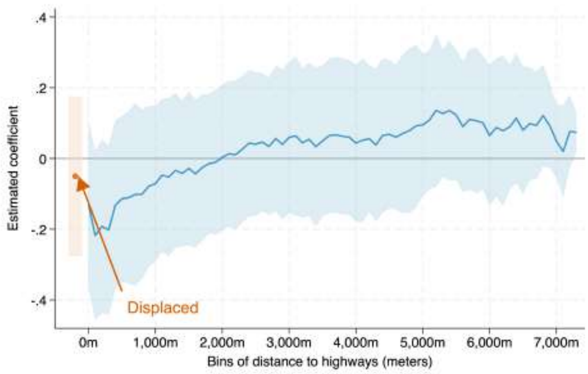
(a) Black share



(b) Immigrant share



(c) Log income wage



(d) Log home value in 1940

Figure B.3: Pre-construction characteristics by distance to planned highways.

Notes: The figures plot estimated coefficients and confidence intervals from equation 1 for distance to planned highways. The blue line plots the distance coefficients β_k with 95% confidence intervals shown as shaded bands; the red marker indicates the coefficient for individuals that would have been displaced. The sample includes individuals residing in metropolitan areas with planned highways who lived within 7,500 meters of the planned highway in 1950, with those living between 7,400 and 7,500 meters serving as the comparison group. The dependent variable in each panel is indicated by the panel title. Panel (d) restricts the sample to individuals linked to the 1940 census who lived in the same house in 1950 and 1940. All regressions include metropolitan area fixed effects and control for log distance to the city center. Standard errors are clustered at the metropolitan area level.

The variable Black Share$_n$ corresponds to the share of the city's Black population residing in tract n in 1950:

$$\text{Black Share}_n = \frac{\text{Black population}_n}{\text{Black population}_{c(n)}}$$

This definition differs from the conventional measure used in the urban economics literature (Weiwu, 2025), which captures the share of a tract's *total* population that is Black. The definition used here is motivated by qualitative accounts suggesting that highway builders targeted neighborhoods "where Black individuals live" (Rose and Mohl, 2012), which implies that the concentration of Black residents within a city, not their share of any given tract's population, was what lead routing decisions. To illustrate, consider a city with two neighborhoods: one with 1,000 Black residents out of a total population of

2,000, and a second with 100 Black residents out of a total population of 100. The conventional measure assigns a Black share of 50% to the first neighborhood and 100% to the second, suggesting the second is more racially salient. Yet the first neighborhood contains roughly 90% of the city's Black population, the quantity that better reflects the targeting motive described above. Appendix Figure B.4 shows the demeaned distribution of this variable across census tracts in the sample. It shows that most census tracts had a small share of the city's Black population, with long tails on both sides of the distribution, showing the prevalent racial segregation at the time (Cutler et al., 1999).

Estimates of equation B.1 are reported in Appendix Table B.1. I find that, in equilibrium, highways were built through neighborhoods with lower price of land, larger Black share, and closer to the central business district. Columns (1) to (5) present the results for the network of highways built. In all specifications, Black households reside closer to future highway developments, which can partially be due to political agendas of builders which lead to an unequal placement of highways (Trounstine, 2018). In addition, highways were built in neighborhoods with an initially lower price of land, as it can be seen in the negative and significant coefficient of median home value and rent. These results are partially explained by the fact that highways were built to connect city centers, which were experiencing a decline in its economic conditions and an increase in its Black population (Boustan, 2010). However, the results are robust to the inclusion of distance to the central business district. Thus, Black individuals were, on equilibrium, more likely to be displaced by highways than their White counterparts.[45]

One possible explanation for these results is that state planners followed the Federal government's dictates. In column (6) of Table B.1, I re-estimate equation B.1 using the Yellow Book maps as the dependent variable. In particular, I use an indicator that takes the value of one if a highway was planned in the neighborhood and zero otherwise. I find that the estimate for racial composition is not different from zero after including proximity to the city center and the price of land. These results suggest that the racial composition of neighborhoods played a role in the decision of state planners to deviate from the Federal plan.

These results indicate that highways were disproportionately routed through neighborhoods with higher Black population shares, even after accounting for proximity to the city center and land values. While the analysis does not establish a causal relationship between the racial composition of neighborhoods and the decision to build highways through them, it suggests that racial composition systematically influenced routing decisions, beyond what federal planning alone would predict.

[45]These results contradict part of the findings of Carter (2023) and Weiwu (2025). They find that the median home value was the most significant predictor of the highway location and that the share of Black individuals did not have a substantial effect. These papers differ in scope and in the way we model neighborhoods' Black share. While they use the share of the tract's population that is Black, I use the share of the city's Black population residing in the tract.

B.3 Results using census tracts

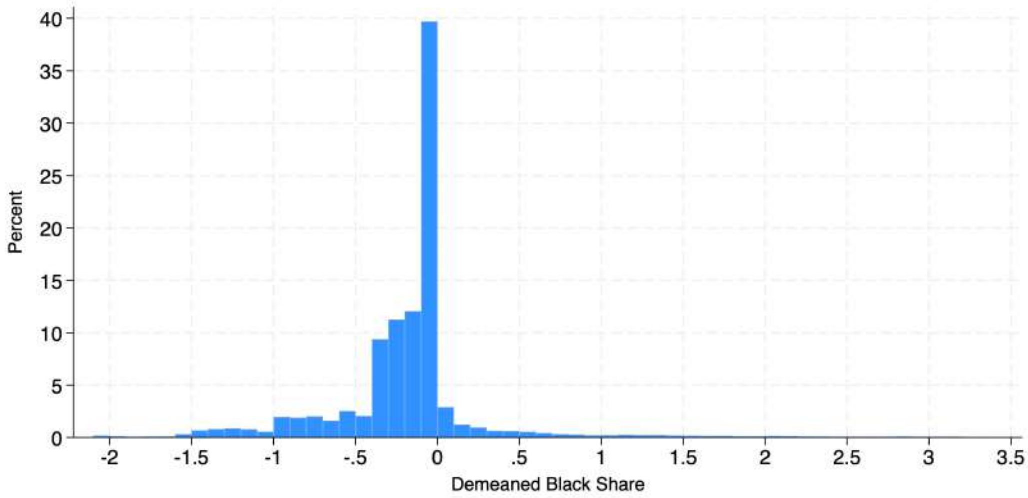


Figure B.4: Demeaned distribution of Black share.

Notes: The figure presents the distribution of the Black share variable, which is defined as the share of the city's Black population residing in the tract in 1950. The variable is demeaned by the city average, so the mean of the distribution is zero. The sample consists of census tracts from 62 metropolitan areas that have spatial information available for 1950, using the 2010 census tract boundaries.

Table B.1: Census Tract Determinants of Highway Construction

	Dep Var: Indicator if the tract is crossed by					
	Built highway					Plan
	(1)	(2)	(3)	(4)	(5)	(6)
Black share	1.236^a	1.083^a	1.276^a	0.855^a	0.837^a	0.060
	(0.276)	(0.278)	(0.281)	(0.254)	(0.226)	(0.218)
(log) Median income	-0.009	-0.004	0.002	0.002	0.001	0.002
	(0.006)	(0.006)	(0.009)	(0.008)	(0.006)	(0.006)
High school share	-0.002^b	-0.000	-0.001	0.001	0.001^c	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
(log) Median home value		-0.058^b	-0.078^a	-0.074^a	-0.067^a	-0.022
		(0.028)	(0.025)	(0.024)	(0.023)	(0.018)
(log) Median rent		-0.079^a	-0.043^b	-0.029^c	-0.028^c	-0.003
		(0.022)	(0.016)	(0.016)	(0.015)	(0.019)
(log) Distance to city center				-0.108^a	-0.078^a	-0.095^a
				(0.015)	(0.012)	(0.014)
Highway planned					0.318^a	
					(0.037)	
Mean dependent var.	0.220	0.223	0.223	0.223	0.223	0.186
Geo. controls	No	No	Yes	Yes	Yes	Yes
Obs.	18,246	17,206	17,197	17,195	17,195	17,195
R^2 (Adj.)	0.049	0.055	0.098	0.117	0.199	0.088

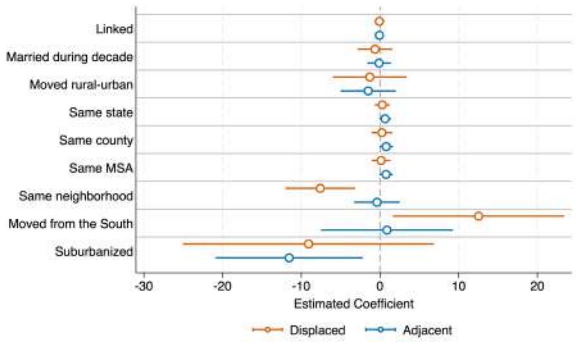
Note: OLS estimates are reported. Coefficients are reported with standard errors, clustered at the MSA level, in parentheses. The unit of observation is census tract in 1950, using the 2010 census tract boundaries. In columns (1) to (5), the dependent variable is an indicator for whether a highway was built through the tract. In column (6), the dependent variable is an indicator for whether a highway was planned through the tract according to the Yellow Book maps. The sample consists of census tracts from 62 metropolitan areas that have spatial information available for 1950. The regression results are weighted by the total population of tract in 1950. All specifications include MSA fixed effects and controls for the (log) area and slope of the tract, distance to the nearest river, an indicator if the governor of the state was part of the Republican party, the state (log) number of car registrations per 10k inhabitants, and the distance to the 1921 railroad network. ^{a} indicates the coefficient is significant at the 1%, ^{b} at the 5%, and ^{c} at the 10% level.

C. Empirical Strategy Appendix

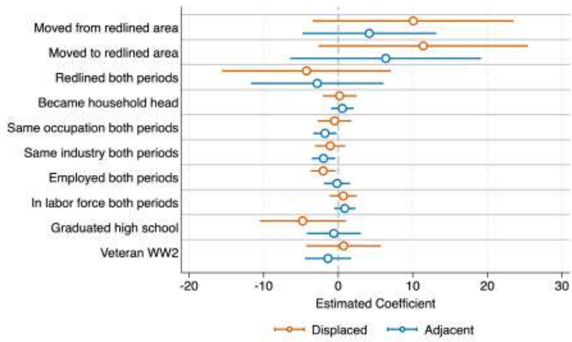
This section includes a further discussion on the alternative empirical strategies used to estimate the effect of displacement on mortality and also supporting tables and figures.

C.1 Pre-trends for the main empirical strategy

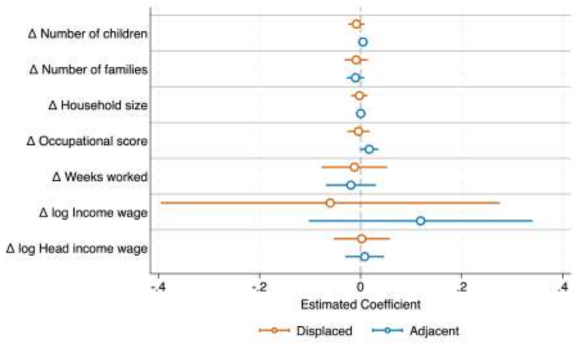
The main empirical strategy compares individuals displaced and those living within 100 meters of newly constructed highways to those living between 100 and 200 meters of the highway construction. To test if these individuals were trending in a similar fashion before highway construction, I use the sample of individuals linked to the 1940 Census and run equation 2 changes between 1940 and 1950. The linkage to the 1940 Census is done using Ruggles et al. (2025) links.



(a) Balance in discrete variables 1



(b) Balance in discrete variables 2



(c) Balance in continuous variables

Figure C.1: Pre-construction trends for the unaffected neighbors strategy.

Notes: The figures display estimates from regressions of pre-construction trends on the two treatment indicators in equation 2, along with 95% confidence intervals. Each row corresponds to a different regression, and the sample includes individuals living within 200 meters of the newly constructed highway in the 1950 census. All regressions include household head's occupational score, as well as gender, race, birth year, and metropolitan area fixed effects. Standard errors are clustered at the metropolitan area level. Coefficients and confidence intervals in Panel (a) and (b) are normalized by the sample mean of the outcome, so they can be interpreted as percentage changes. Coefficients in Panel (c) are standardized to have mean zero and standard deviation of one, so they can be interpreted as standard deviation changes.

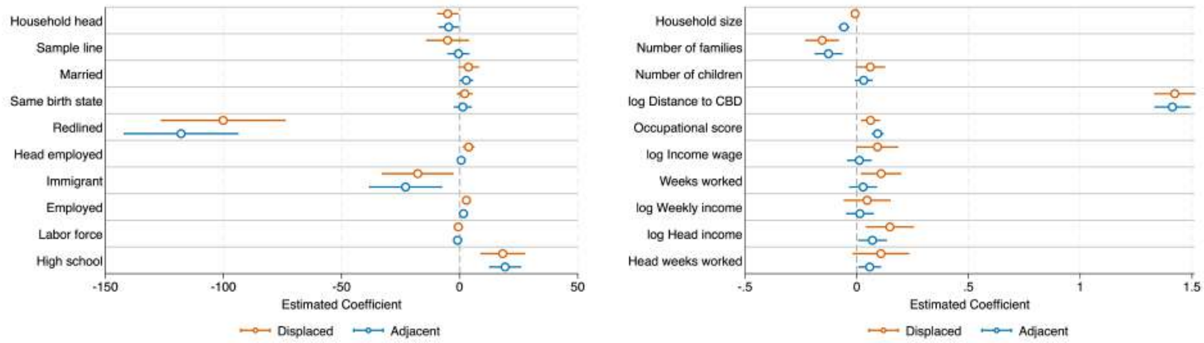
C.2 Matching

The main empirical strategy compares individuals displaced and those living within 100 meters of newly constructed highways to those living between 100 and 200 meters of the highway construction. Although the results in Section 5 show that any spillover effects of highway construction are limited to individuals living within 100 meters of the highway, the estimates may still be biased if individuals living within 200 meters of the highway construction differ in unobservable characteristics that are correlated with the outcomes. For example, highway construction reduced the housing stock in affected neighborhoods, particularly for low-income households, which may have raised neighborhood rents and affected the control group. Restricting the control group to individuals living further away from the highway construction may mitigate this concern, but it may also introduce bias if the control group is not comparable to the displaced individuals. To address this concern, I use propensity score matching to select a control group living in the same city and with similar observable characteristics, but living more than 2,000 meters from any highway.

I construct a matched sample as an alternative strategy to estimate the causal effects of highway displacement on long-term mortality. Matching addresses selection bias arising from non-random highway placement and systematic differences between individuals living near and far from construction sites. I define two treatment groups based on residential proximity to Interstate Highways constructed before 1970: *displaced* individuals ($D_i = 1$), whose 1950 residence fell on a future highway right-of-way (distance ≤ 0 meters), and *adjacent* individuals ($A_i = 1$), residing 0–100 meters from a future highway segment. The control pool consists of individuals residing more than 2,000 meters from any highway segment.

Because I use both displaced and adjacent individuals as separate treatment groups, I implement sequential matching to ensure that each control unit is matched to at most one treated observation. I first match displaced individuals to the full control pool, then match adjacent individuals to the remaining controls, so that no control unit appears in both matched samples. Within each MSA, I estimate propensity scores via probit regression using 27 pre-treatment covariates capturing demographics (race, sex, age polynomials, immigration status), socioeconomic status (employment, occupational score polynomials, education, log household income), household composition (household size, number of families), geography (log distance to CBD, redlining grade), and six interaction terms to absorb non-linear selection patterns.

Overall, the matching process is able to match 63.1% percent of the displaced individuals. Matched individuals are different from unmatched individuals, as shown in Appendix Figure C.2. They are more likely to be white, have higher property values, and have higher levels of education. Thus, the results should be interpreted as the effect of displacement on the matched sample, not the entire displaced population.



(a) Indicator variables

(b) Standardized variables

Figure C.2: Balance of Pre-Treatment Characteristics: Matched vs. Unmatched Treated Individuals.

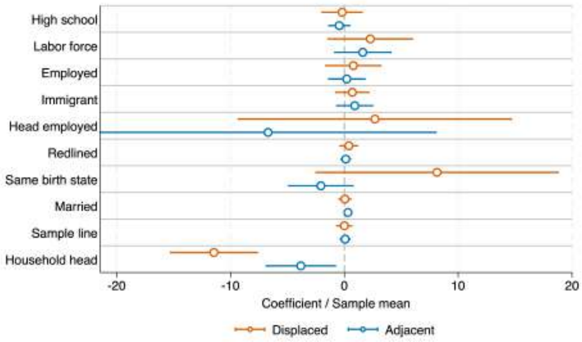
Notes: The figure present the coefficient and 95% confidence intervals for being being matched in the matching strategy compared to unmatched treated individuals, for a set of pre-treatment characteristics. Each coefficient corresponds to a different regression and is reported with standard errors clustered at the metropolitan area level. The coefficients and confidence intervals in panel (a) are normalized by the sample mean of the dependent variable, while the dependent variables in panel (b) are standardized to have mean zero and standard deviation one. All regressions control for the household head's occupational score and include fixed effects for gender, race, birth year, and metropolitan area of residence.

The identifying assumption of the matching strategy is that, conditional on the full set of individual- and neighborhood-level covariates used to construct matched pairs within each MSA, proximity to a highway is independent of potential outcomes. By matching on characteristics that capture both the official criteria for route selection and the factors governing residential sorting across neighborhoods, the design addresses selection at both the infrastructure and household level. Appendix Figures C.3 and C.4 show that the sample is balanced and lacking pre-trends, lending credibility to the strategy.

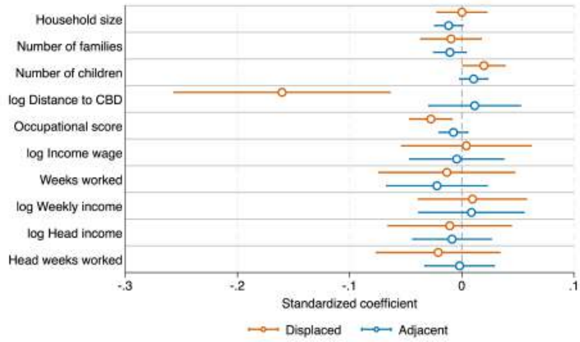
C.3 Planned highways as potential control group

To minimize construction costs, primary roads were often converted into highways (Rose and Mohl, 2012). A natural concern is that individuals who used to live in primary roads may differ from their neighbors in unobservable characteristics that are correlated with the outcomes of interest. The routes planned in the Yellow Book, like the actual network built, were also more likely to correspond to primary roads. Thus, individuals that would have been displaced by the Yellow Book routes are similar to displaced individuals in that they were living near a planned highway, but were not displaced. These individuals are similar to displaced individuals in that they were living near a planned highway, but were not displaced.

The identifying assumption of this strategy is that, conditional on the characteristics used by states and local officials to select highway routes, treatment is independent of po-



(a) Indicator variables



(b) Continuous variables

Figure C.3: Pre-construction balance for the matching strategy.

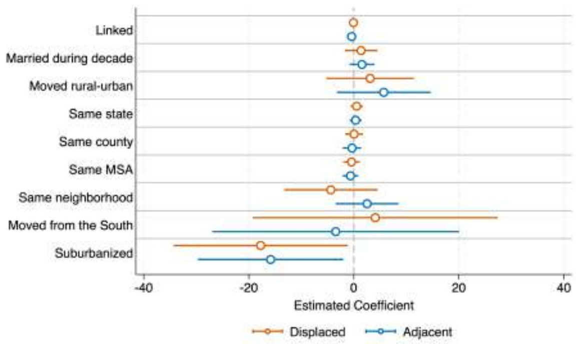
Notes: The figures display estimates from regressions of pre-construction characteristics on the two treatment indicators in equation 2, along with 95% confidence intervals. Each row corresponds to a different regression, and the sample includes matched individuals in the 1950 census. All regressions include household head's occupational score, as well as gender, race, birth year, and metropolitan area fixed effects. Standard errors are clustered at the metropolitan area level. Coefficients and confidence intervals in Panel (a) are normalized by the sample mean of the outcome, so they can be interpreted as percentage changes. Coefficients in Panel (b) are standardized to have mean zero and standard deviation of one, so they can be interpreted as standard deviation changes.

tential outcomes between treated individuals and those who would have been displaced by the Yellow Book routes. Appendix Figure C.5 presents the balance test for this strategy, which shows that treated individuals and those who would have been displaced by the Yellow Book routes are similar in observable characteristics. Finally, Appendix Figure C.6 use the sample of individuals linked to the 1940 Census to show that treated individuals and those who would have been displaced by the Yellow Book routes were trending in a similar fashion before highway construction.

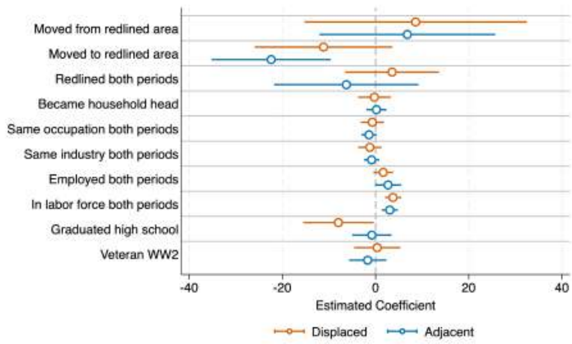
A concern with this strategy is that living in primary roads may have, by itself, an effect on the outcomes of interest. I can test this possibility by running a placebo test using as a treatment group individuals living near these planned highways. I then repeat the exercise of comparing affected individuals to those living between 100 and 200 meters away.[46] The results show no significant effect of living close to a planned highway on individuals' long-term outcomes. Appendix Figure C.7 shows that the coefficients that are very close to zero and not statistically significant at the usual levels. I interpret the lack of significant effects as evidence against the hypothesis that living near a primary road had an effect on the outcomes of interest, and thus as evidence in favor of the validity of this strategy.

C.4 Additional figures

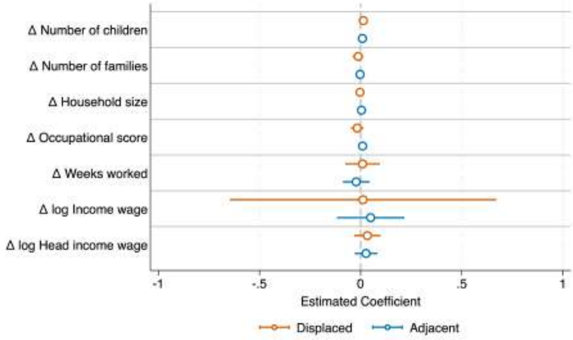
[46]Planned highways consists of a unidirectional segment of the highway. I assume that planned highways had a right-of-way width of 150 feet (45.72 meters).



(a) Balance in discrete variables 1



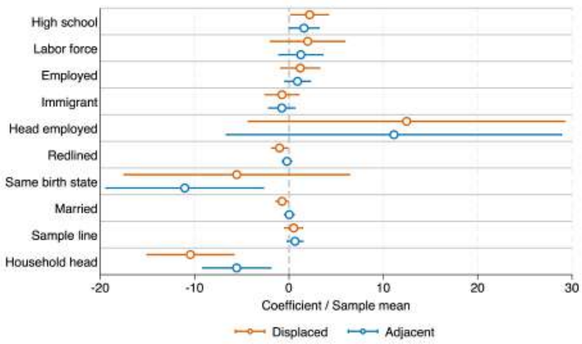
(b) Balance in discrete variables 2



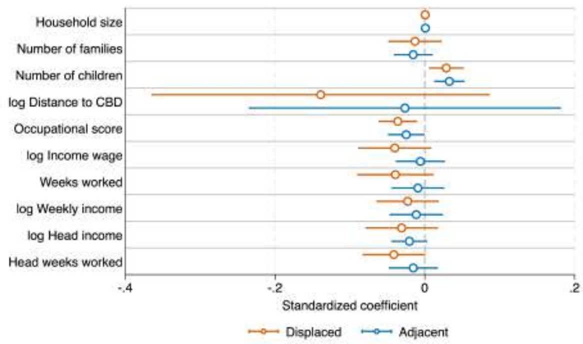
(c) Balance in continuous variables

Figure C.4: Pre-construction trends for the matching strategy.

Notes: The figures display estimates from regressions of pre-construction trends on the two treatment indicators in equation 2, along with 95% confidence intervals. Each row corresponds to a different regression, and the sample includes matched individuals in the 1950 census. All regressions include household head's occupational score, as well as gender, race, birth year, and metropolitan area fixed effects. Standard errors are clustered at the metropolitan area level. Coefficients and confidence intervals in Panel (a) and (b) are normalized by the sample mean of the outcome, so they can be interpreted as percentage changes. Coefficients in Panel (c) are standardized to have mean zero and standard deviation of one, so they can be interpreted as standard deviation changes.



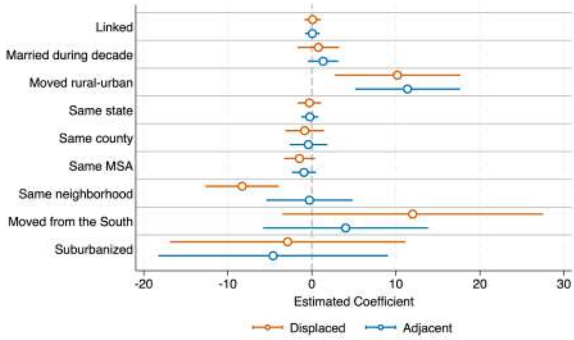
(a) Indicator variables



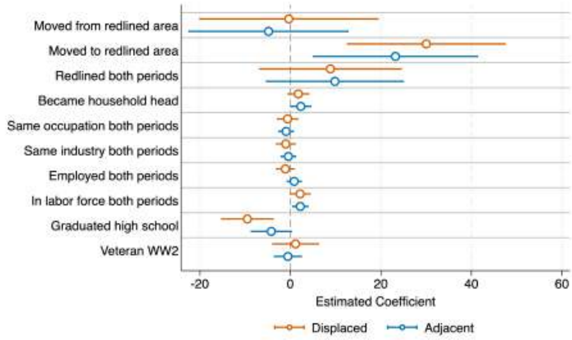
(b) Continuous variables

Figure C.5: Pre-construction balance for the strategy using the Yellow Book Maps.

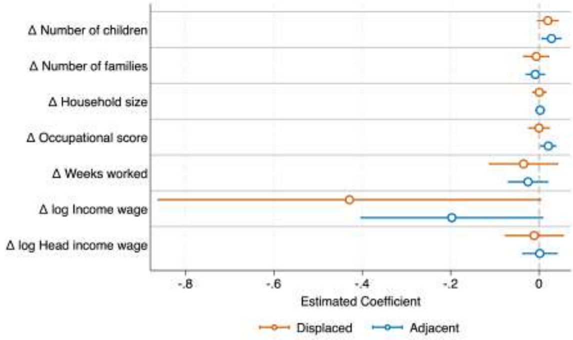
Notes: The figures display estimates from regressions of pre-construction characteristics on the two treatment indicators in equation 2, along with 95% confidence intervals. Each row corresponds to a different regression, and the sample includes individuals that would have been affected by the planned highways in the 1950 census. All regressions include household head's occupational score, as well as gender, race, birth year, and metropolitan area fixed effects. Standard errors are clustered at the metropolitan area level. Coefficients and confidence intervals in Panel (a) are normalized by the sample mean of the outcome, so they can be interpreted as percentage changes. Coefficients in Panel (b) are standardized to have mean zero and standard deviation of one, so they can be interpreted as standard deviation changes.



(a) Balance in discrete variables 1



(b) Balance in discrete variables 2



(c) Balance in continuous variables

Figure C.6: Pre-construction trends for the strategy using the Yellow Book Maps.

Notes: The figures display estimates from regressions of pre-construction trends on the two treatment indicators in equation 2, along with 95% confidence intervals. Each row corresponds to a different regression, and the sample includes individuals that would have been affected by the planned highways in the 1950 census. All regressions include household head's occupational score, as well as gender, race, birth year, and metropolitan area fixed effects. Standard errors are clustered at the metropolitan area level. Coefficients and confidence intervals in Panel (a) and (b) are normalized by the sample mean of the outcome, so they can be interpreted as percentage changes. Coefficients in Panel (c) are standardized to have mean zero and standard deviation of one, so they can be interpreted as standard deviation changes.

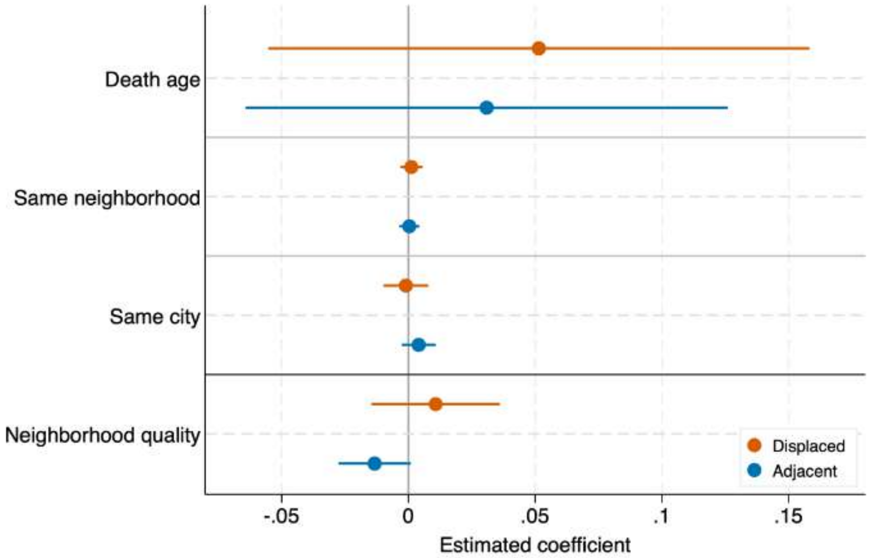
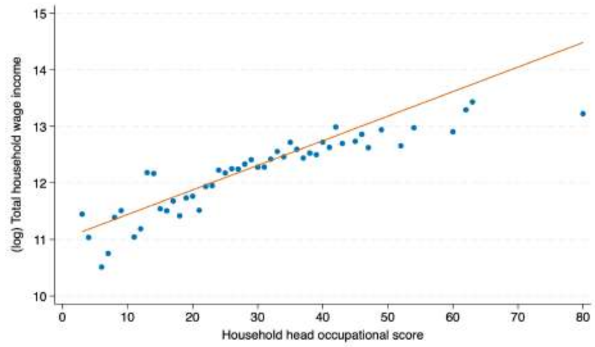
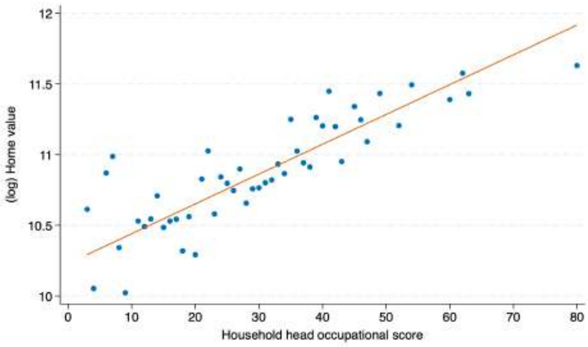


Figure C.7: Placebo test using the planned highway strategy.

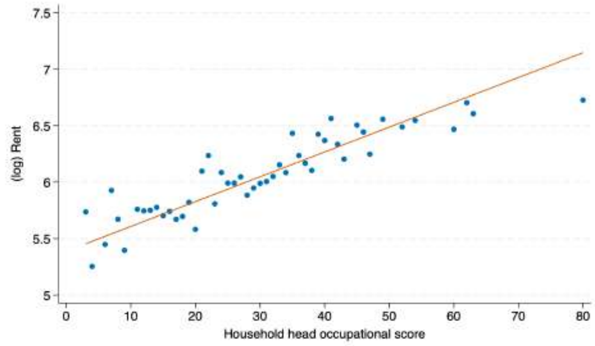
Notes: The figures display estimates from regressions of long-run characteristics on the two treatment indicators in equation 2, along with 95% confidence intervals. Each row corresponds to a different regression, and the sample includes individuals living within 200 meters of the planned highways in the 1950 census. All regressions include household head's occupational score, as well as gender, race, birth year, and metropolitan area fixed effects. Standard errors are clustered at the metropolitan area level. The units for the coefficients and confidence intervals in the first row are years, while for the two rows below are percentage points. Finally, the units for the coefficients and confidence intervals in the bottom row correspond to standard deviations.



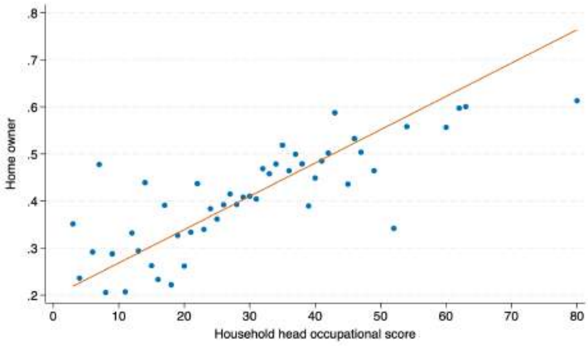
(a) Total household income (log)



(b) Home value (log)



(c) Monthly rent (log)



(d) Home ownership

Figure C.8: Correlation between household head's occupational score and wealth proxies.

Notes: Each figure presents a binscatter plot of the relationship between the household head's occupational score and the indicated outcome. The sample consists of household heads in the 1940 Census with non-missing MSA of residence. Monetary outcomes are reported in 2010 dollars. Home ownership is an indicator variable equal to one if the household head owns the home. The fitted line is estimated via OLS.

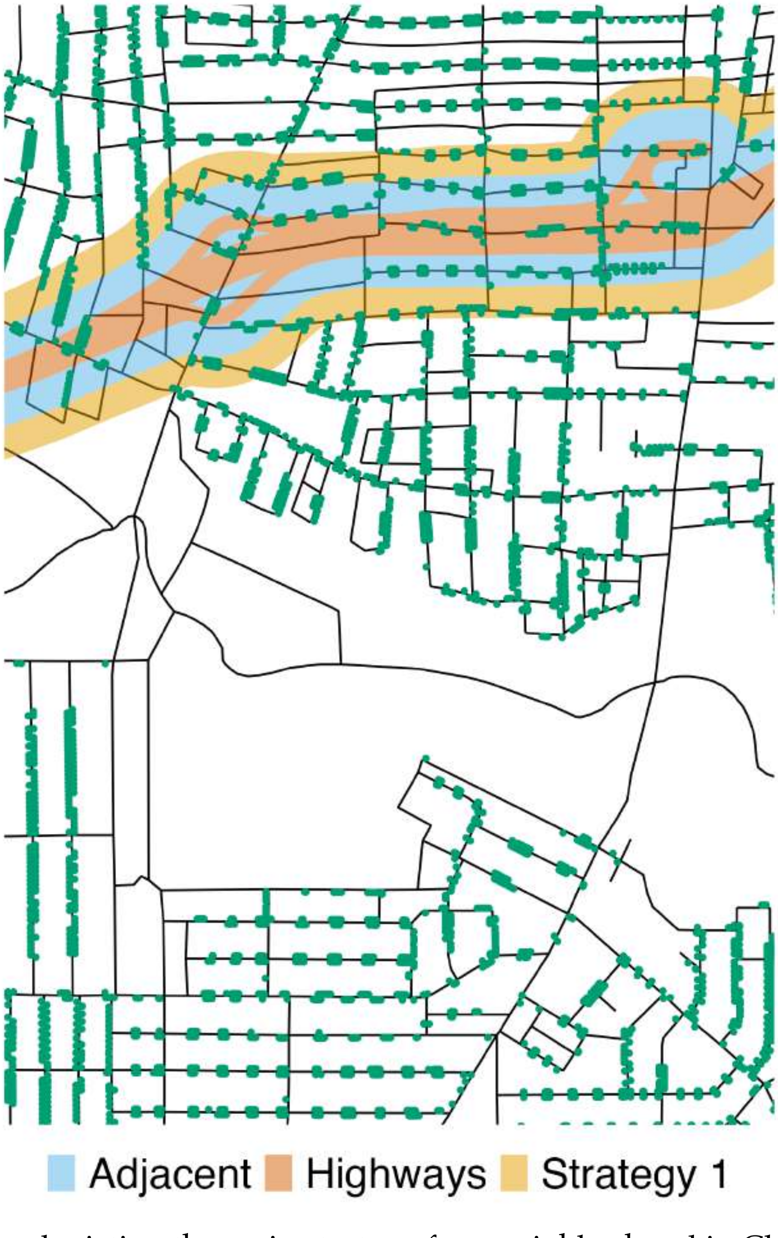


Figure C.9: Map depicting the main strategy for a neighborhood in Cleveland, Ohio.

Notes: The map is based on the geocoded 1950 census and the highway construction data described in Section 3. Each green point corresponds to a household in the 1950 census. Displaced individuals are those living in the red band, adjacent individuals are those living in the blue band, and the control group consists of those living in the golden band.

Figure C.10: Map depicting the matching strategy for a neighborhood in Cleveland, Ohio.

Notes: The map is based on the geocoded 1950 census and the highway construction data described in Section 3. Each green point corresponds to a household in the 1950 census. Displaced individuals are those living in the red band and adjacent individuals are those living in the blue band. The pool of potential controls consists of those living in the pink area.

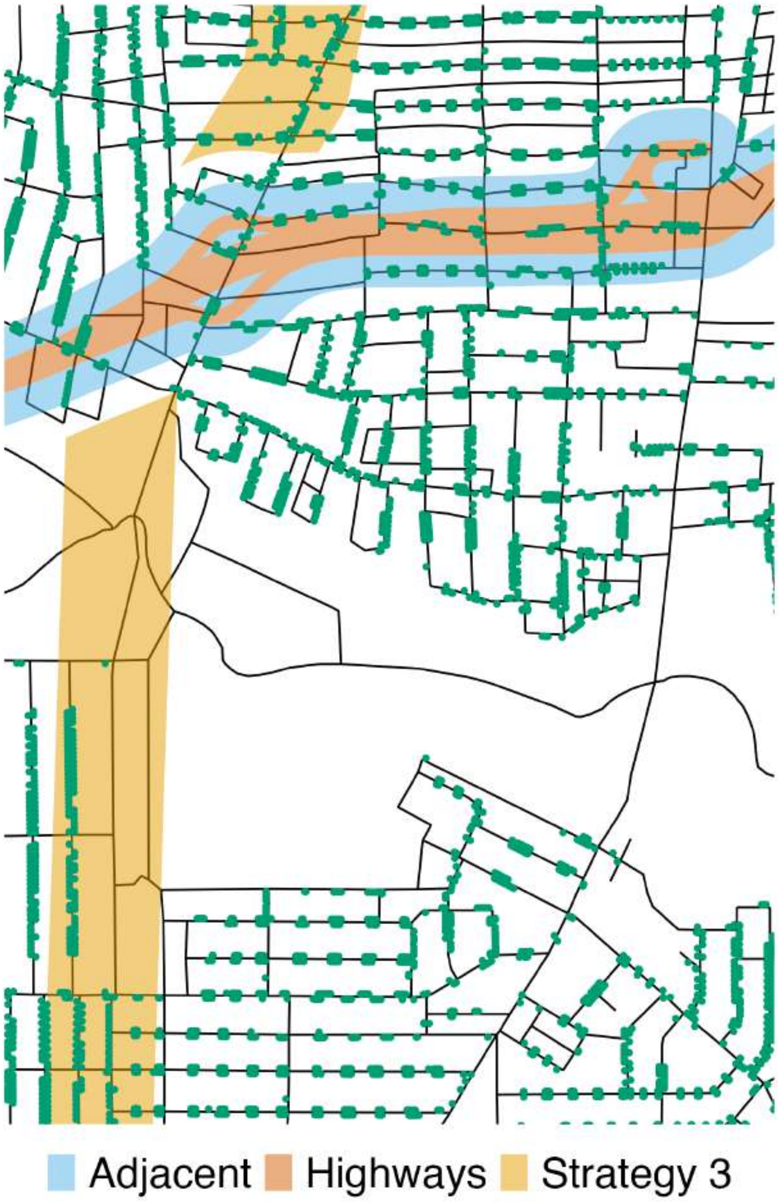


Figure C.11: Map depicting the planned strategy for a neighborhood in Cleveland, Ohio.

Notes: The map is based on the geocoded 1950 census and the highway construction data described in Section 3. Each green point corresponds to a household in the 1950 census. Displaced individuals are those living in the red band, adjacent individuals are those living in the blue band, and the control group consists of those living in the golden band.

D. Displacement Appendix

In this appendix, I present additional analyses related to the displacement effects of highway construction.

D.1 Additional tables

Table D.1: Effects of Interstate construction on longevity

	Death Age (1)	Survive 75 (2)	Survive 80 (3)
Panel A. Neighbors sample			
Displaced	-0.079[b]	-0.002	-0.007[b]
	(0.037)	(0.003)	(0.003)
Adjacent	-0.090[a]	-0.004[c]	-0.007[a]
	(0.021)	(0.002)	(0.002)
Observations	151,257	151,257	151,257
Adj. R^2	0.495	0.348	0.329
Mean of dep. var.	75.889	0.541	0.304
Panel B. Yellow Book sample			
Displaced	-0.137[a]	-0.005	-0.006[b]
	(0.038)	(0.004)	(0.003)
Adjacent	-0.118[a]	-0.006[b]	-0.005[b]
	(0.031)	(0.002)	(0.002)
Observations	147,513	147,513	147,513
Adj. R^2	0.495	0.349	0.330
Mean of dep. var.	75.995	0.547	0.309
Panel C. Matching sample			
Displaced	-0.113[a]	0.002	-0.009[a]
	(0.042)	(0.004)	(0.003)
Adjacent	-0.113[a]	-0.003	-0.007[a]
	(0.036)	(0.002)	(0.002)
Observations	99,244	99,244	99,244
Adj. R^2	0.496	0.349	0.328
Mean of dep. var.	76.127	0.554	0.318

Note: OLS estimates are reported. Coefficients are reported with standard errors, clustered at the MSA level, in parentheses. The unit of observation is an individual in the sample of linked census-mortality records. The dependent variables are reported at the top of each column. Panel A reports the results comparing treated individuals to their neighbors living between 100 and 200 meters from future highway construction, Panel B compares them to those who would have been displaced or living adjacent to the Yellow Book routes, and Panel C to matched individuals. More details on each of these specifications can be found in the main text and in the Appendix Section C. All specifications include MSA fixed effects and controls for head-of-household occupational score, race, birth year, and gender. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

Table D.2: Effects of Interstate construction on mobility

	Same Neighborhood (1)	Same City (2)	Suburban 2000 (3)	log Dist to CBD (4)
Panel A. Neighbors sample				
Displaced	-0.026[a]	0.013[b]	0.000	-0.012
	(0.002)	(0.005)	(0.004)	(0.009)
Adjacent	-0.007[a]	0.007[a]	-0.006	-0.012[c]
	(0.002)	(0.002)	(0.004)	(0.007)
Observations	119,927	119,927	118,595	118,595
Adj. R^2	0.025	0.106	0.123	0.155
Mean of dep. var.	0.040	0.628	0.721	9.521
Panel B. Yellow Book sample				
Displaced	-0.025[a]	0.013	0.001	-0.001
	(0.004)	(0.009)	(0.006)	(0.011)
Adjacent	-0.006[c]	0.007	-0.005	0.001
	(0.004)	(0.008)	(0.006)	(0.014)
Observations	117,304	117,304	116,038	116,038
Adj. R^2	0.022	0.096	0.112	0.141
Mean of dep. var.	0.039	0.635	0.745	9.572
Panel C. Matching sample				
Displaced	-0.030[a]	0.026[a]	0.002	-0.008
	(0.004)	(0.006)	(0.006)	(0.012)
Adjacent	-0.008[a]	0.015[b]	0.000	0.007
	(0.003)	(0.007)	(0.005)	(0.008)
Observations	78,795	78,795	77,884	77,884
Adj. R^2	0.028	0.079	0.099	0.127
Mean of dep. var.	0.045	0.618	0.796	9.709

Note: OLS estimates are reported. Coefficients are reported with standard errors, clustered at the MSA level, in parentheses. The unit of observation is an individual in the sample of linked census-mortality records. The dependent variables are reported at the top of each column. Panel A reports the results comparing treated individuals to their neighbors living between 100 and 200 meters from future highway construction, Panel B compares them to those who would have been displaced or living adjacent to the Yellow Book routes, and Panel C to matched individuals. More details on each of these specifications can be found in the main text and in the Appendix Section C. All specifications include MSA fixed effects and controls for head-of-household occupational score, race, birth year, and gender. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

Table D.3: Effects of Interstate construction on neighborhood characteristics

	Unemploy. Share (1)	College Share (2)	Homeowner. Share (3)	log Median Income (4)	log Median Home Value (5)	log Median Rent (6)	Neighborhood Quality (7)
Panel A. Neighbors sample							
Displaced	0.017^{c}	-0.029^{a}	-0.005	-0.011	-0.006	-0.007	-0.022^{b}
	(0.009)	(0.010)	(0.008)	(0.008)	(0.011)	(0.011)	(0.010)
Adjacent	0.008	-0.007	-0.008	-0.006	-0.001	-0.001	-0.009
	(0.005)	(0.007)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
Observations	119,569	119,668	119,795	119,570	117,880	118,963	119,554
Adj. R^2	0.207	0.116	0.123	0.190	0.296	0.217	0.130
Mean of dep. var.	-0.004	0.004	0.002	0.008	0.007	0.009	0.007
Panel B. Yellow Book sample							
Displaced	0.028^{b}	-0.080^{a}	0.005	-0.029^{a}	-0.054^{a}	-0.037^{a}	-0.066^{a}
	(0.013)	(0.014)	(0.011)	(0.011)	(0.015)	(0.013)	(0.013)
Adjacent	0.022^{a}	-0.063^{a}	0.002	-0.027^{b}	-0.055^{a}	-0.034^{a}	-0.058^{a}
	(0.008)	(0.014)	(0.009)	(0.012)	(0.010)	(0.010)	(0.012)
Observations	116,891	116,998	117,122	116,891	114,956	116,241	116,878
Adj. R^2	0.205	0.112	0.119	0.178	0.297	0.211	0.122
Mean of dep. var.	-0.002	0.003	0.001	0.007	0.006	0.008	0.006
Panel C. Matching sample							
Displaced	0.036^{b}	-0.110^{a}	-0.032	-0.054^{a}	-0.078^{a}	-0.067^{a}	-0.102^{a}
	(0.016)	(0.026)	(0.022)	(0.015)	(0.019)	(0.014)	(0.022)
Adjacent	0.029^{a}	-0.078^{a}	-0.040^{c}	-0.051^{a}	-0.066^{a}	-0.049^{a}	-0.084^{a}
	(0.011)	(0.018)	(0.023)	(0.011)	(0.011)	(0.010)	(0.013)
Observations	78,567	78,637	78,708	78,576	77,508	78,221	78,561
Adj. R^2	0.150	0.088	0.085	0.120	0.258	0.196	0.110
Mean of dep. var.	0.000	-0.000	-0.000	-0.000	0.000	0.000	-0.000

Note: OLS estimates are reported. Coefficients are reported with standard errors, clustered at the MSA level, in parentheses. The unit of observation is an individual in the sample of linked census-mortality records. The dependent variables are reported at the top of each column. Panel A reports the results comparing treated individuals to their neighbors living between 100 and 200 meters from future highway construction, Panel B compares them to those who would have been displaced or living adjacent to the Yellow Book routes, and Panel C to matched individuals. More details on each of these specifications can be found in the main text and in the Appendix Section C. All specifications include MSA fixed effects and controls for head-of-household occupational score, race, birth year, and gender. ^{a} indicates the coefficient is significant at the 1%, ^{b} at the 5%, and ^{c} at the 10% level.

D.2 Additional figures

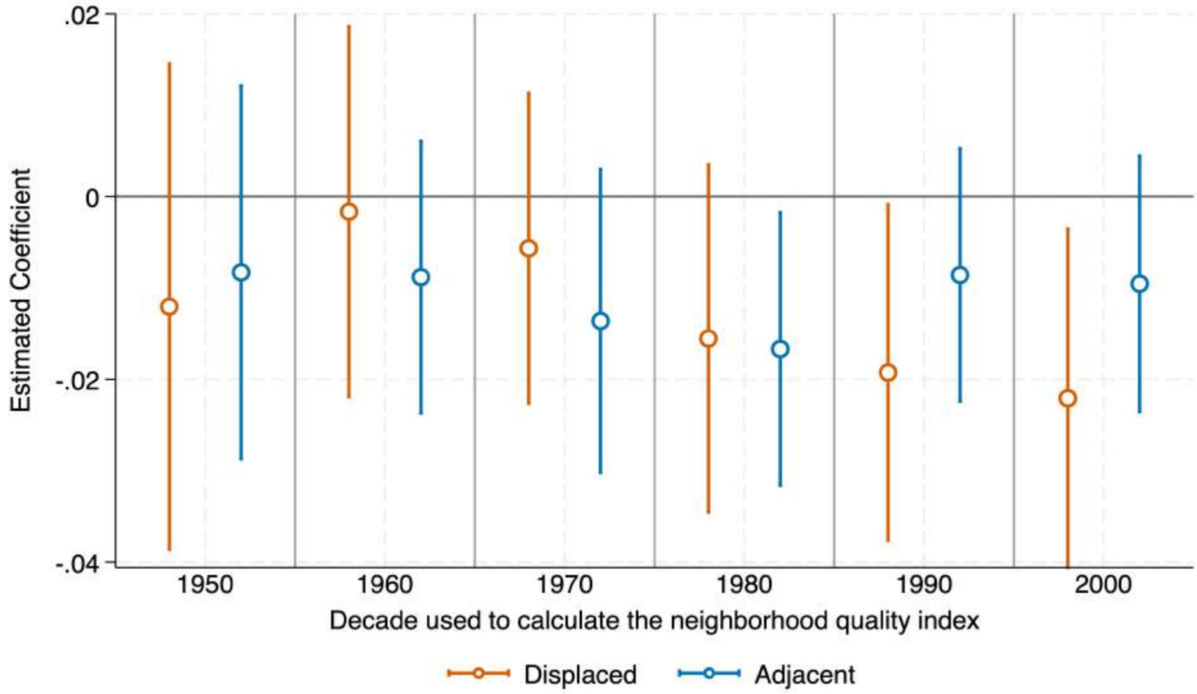
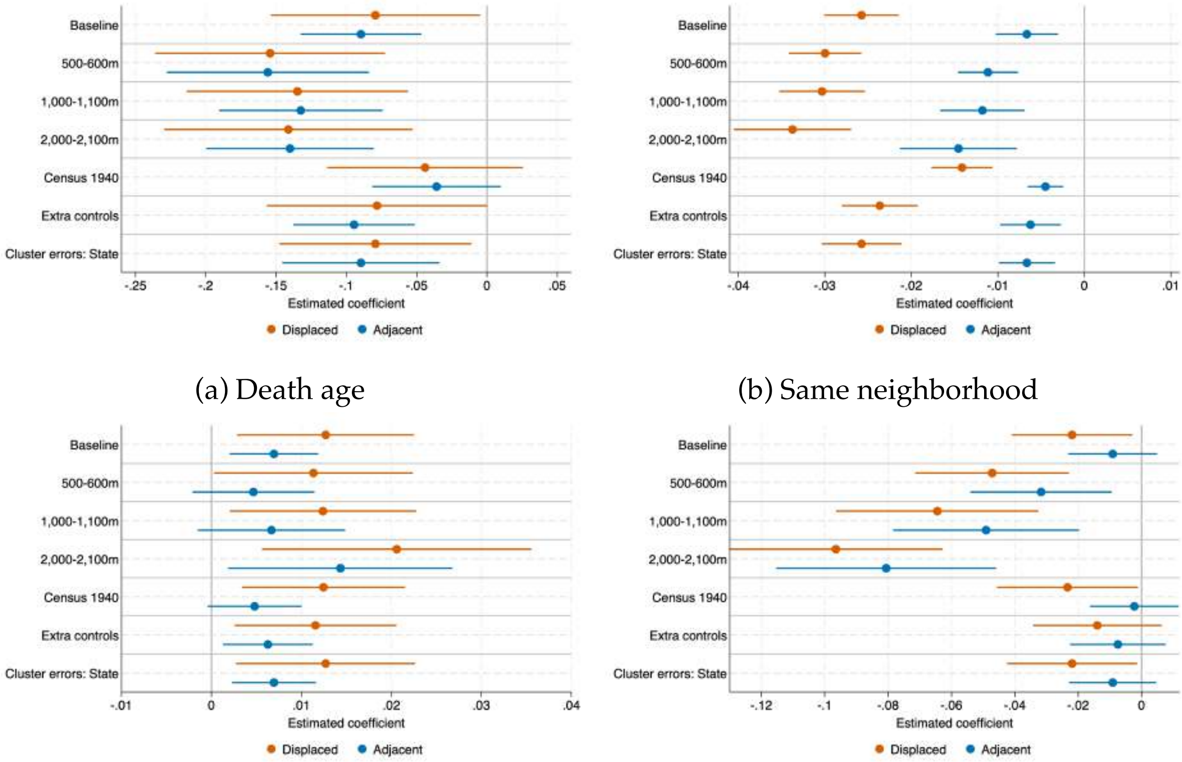


Figure D.1: Evolution of neighborhood quality over time.

Notes: The figures display estimates from regressions of neighborhood quality on the two treatment indicators in equation 2, along with 95% confidence intervals. Each column corresponds to a different regression. The sample corresponds to individuals living within 200 meters of the newly constructed highway in the 1950 census, unless otherwise noted. The dependent variable corresponds to the neighborhood quality index described in Appendix Section D.4, calculated for the decade denoted in the x-axis and normalized to have mean zero and standard deviation one. The index is inputed to each individual based on the neighborhood in which they lived at the time of death. All regressions include household head's occupational score, as well as gender, race, birth year, and metropolitan area fixed effects. Standard errors are clustered at the metropolitan area level.



(a) Death age

(b) Same neighborhood

(c) Same city

(d) Neighborhood quality

Figure D.2: Robustness of the results.

Notes: The figures display estimates from regressions of the main outcomes on the two treatment indicators in equation 2, along with 95% confidence intervals. Each row corresponds to a different regression. The sample corresponds to individuals living within 200 meters of the newly constructed highway in the 1950 census, unless otherwise noted. All regressions include household head's occupational score, as well as gender, race, birth year, and metropolitan area fixed effects. Standard errors are clustered at the metropolitan area level. The top panel corresponds to the baseline results. *500-600m, 1,000-1,100m, and 2,000-2,100* reports results obtained using individuals living in those respective distance ranges from the highway as the comparison group. *Census 1940* repeats the analysis using the 1940 census instead of the 1950 census to identify the treatment and control groups. *Extra controls* includes in the regression the following controls: employment dummies, middle school and high school completion dummies, log distance to the city center in 1950, number of families in the household, household head employment dummy, household head high school degree dummy, and the number of weeks worked in the last year by the household head. *Cluster errors: State* clusters standard errors at the state level, instead of the metropolitan area level.

D.3 Duration Analysis

Given the nature of the mortality record, I can estimate a duration model to study the effect of displacement on mortality. In this context, I will study if individuals who were displaced by highway construction are more likely to die earlier than their peers who were not displaced. This semi-parametric approach leaves the baseline hazard unrestricted, handles left-truncation through delayed entry at age-specific risk sets, and absorbs high-dimensional fixed effects through stratification without incurring incidental parameters bias.

The hazard function for individual i in stratum s at age t takes the form:

$$h_i(t) = h_{0s}(t) \cdot \exp(\beta_1 \cdot \text{displaced}_i + \beta_2 \cdot \text{adjacent}_i + \beta' \mathbf{X}_i) \tag{D.1}$$

where $h_{0s}(t)$ is a stratum-specific baseline hazard (one per MSA $\times$ birth-year cell) and $\mathbf{X}_i$ includes head-of-household occupational score, race, and sex. We stratify on MSA and birth year rather than including them as covariates, allowing the baseline hazard to vary flexibly across geographic and cohort dimensions. Standard errors are clustered at the MSA level.

We model age rather than calendar time as the time scale and implement left-truncation to address delayed entry into the observation window. Individuals enter the risk set at the age they attain on January 1, 1988, when BUNMD mortality records begin: an individual born in 1920, for example, enters at age 68. The partial likelihood conditions on survival to the start of observation and appropriately adjusts risk sets for late entry, so that delayed entry does not bias hazard ratio estimates.

An advantage of proportional hazard models is that the identification of β_1 and β_2 does not require the specification of the functional form of $\lambda_0(t, \alpha)$ (Cameron and Trivedi, 2005, ch. 17.8). This functional form permits an easy to interpret the coefficients β_1 and β_2. Suppose that two individuals are identical in all aspects except for the displacement indicator. Then, the hazard ratio between the two individuals is:

$$\frac{\lambda(t|\text{Disp} = 1, \mathbf{X}_i)}{\lambda(t|\text{Disp} = 0, \mathbf{X}_i)} = exp(\beta_1) \tag{D.2}$$

Thus, the new hazard is $exp(\beta_1)$ times the hazard of the individual who was not displaced. In other words, the change in the hazard for displaced individuals is $exp(\beta_1) - 1$ times the original hazard. The results are reported in Appendix Table D.4. I report the values of the hazard rates minus one (ie $exp(\beta_1) - 1$), so that the estimates can be interpreted as percentage changes in the hazard of death at a given age, relative to the control group. The estimates suggest that individuals displaced and those living close to future highway construction have between 1.1 and 2.0% higher risk of early death over the study period than their peers.

Table D.4: Mortality hazard rates for displaced and adjacent individuals

Variable	Neighbors	Matching	Yellow Book
Displaced	0.011	0.021^b	0.020^a
	0.008	0.010	0.007
Adjacent	0.012^a	0.022^a	0.018^a
	0.004	0.006	0.006
N	136,625	90,371	133,430
Pseudo R2	0.00061	0.00052	0.00053

Note: The table reports hazard rates from a Cox Proportional Hazard model, where the dependent variable is the hazard of death at a given age. The sample includes individuals in the linked census-mortality records, and the model is stratified by MSA and birth year. The key independent variables are indicators for being displaced by highway construction and for living adjacent to future highway construction in the 1950 census. ^{a} indicates the coefficient is significant at the 1%, ^{b} at the 5%, and ^{c} at the 10% level.

Transforming the increased hazard rate to average expected years of life lost.

To translate the Cox proportional hazard estimates into a measure comparable to the OLS results, I compute the restricted mean survival time (RMST) implied by the model. I use the results from estimating equation D.1 to predict the cumulative hazard function from which the survival function can be derived. I then predict survival curves separately for each of the four demographic cells defined by race (Black/White) and sex (male/female), evaluating the head-of-household occupational income score at its sample mean within each cell. Within each cell, I construct predicted curves for each of the three mutually exclusive treatment groups (displaced, adjacent, and control) and numerically integrate the survival function over the observed age range using the trapezoidal rule to obtain the RMST. The difference in RMST between each treatment group and the control group provides a cell-specific estimate of months of life lost to highway construction. The results are reported in Appendix Table D.5.

Table D.5: Mortality hazard rates for displaced and adjacent individuals

Variable	Non-Black Male	Non-Black Female	Black Male	Black Female
Displaced	-0.9541	-0.9344	-0.9609	-0.9450
Adjacent	-0.7584	-0.7427	-0.7638	-0.7511

Note: The table reports the associated reduction in expected months of life implied by the Cox Proportional Hazard model estimates in Appendix Table D.4, for the neighbor strategy. The sample includes individuals in the linked census-mortality records, and the model is stratified by MSA and birth year.

D.4 Principal Component Analysis

To measure the quality of the neighborhood at the time of death, I use a principal component analysis (PCA) of the neighborhood characteristics. I create the index separately for Black and White individuals. Each variable is normalized to have a mean of zero and a standard deviation of one. I include the share of adults with a high school degree, the share of adults with a college degree, the employment rate, the median income, the median house value, and the share of homeowners. I then normalize the first principal component to have a mean of zero and a standard deviation of one.

I find that the first principal component is positively related to the six variables used for both Black and White individuals. Appendix Table D.6 shows the factor loadings for the first two principal components. The first principal component also has explanatory power for the neighborhood characteristics (e.g., the eigenvalues are 3.01 and 2.37 for White and Black individuals). The estimated weights for all characteristics have the same sign and are very similar in magnitude for both groups.

Table D.6: Factor Loadings for the first two principal components by race

	White		Black	
	(1)	(2)	(3)	(4)
	1st PC	2nd PC	1st PC	2nd PC
Eigenvalue	3.01	1.17	2.37	1.05
Household median income	0.48	0.02	0.51	-0.13
Median home value	0.41	-0.26	0.09	0.68
Employment share	0.41	-0.34	0.42	-0.06
Homeownership share	0.24	0.51	0.28	-0.01
College share	0.19	0.69	0.28	-0.64
High school share	0.44	-0.22	0.47	0.19

Note: Factor loadings for the first two principal component analysis.

Appendix Figure D.3 shows the eigenvalues of the first six principal components by race. It shows that the first principal component explains most of the variation in the neighborhood characteristics, for both racial groups.

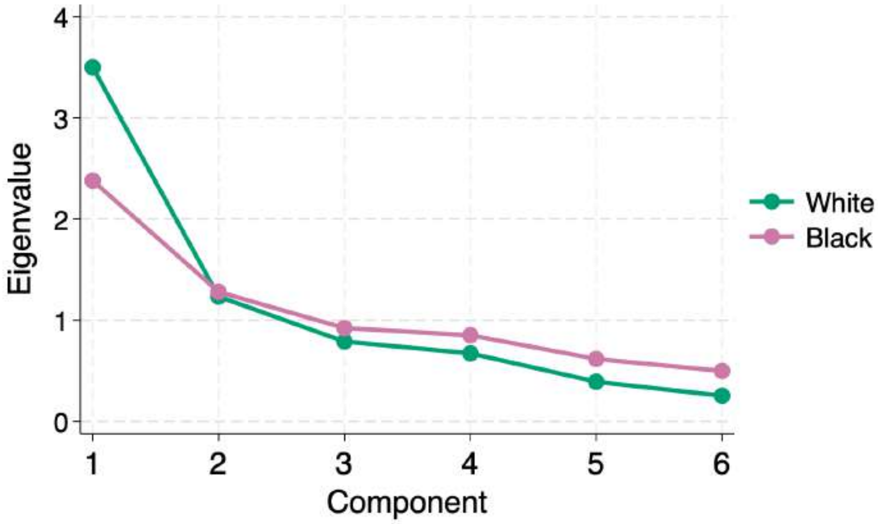


Figure D.3: PCA Eigenvalues for the first six principal components by race

Note: Eigenvalues of the first six principal components.

D.5 Heterogeneities

In this section, I describe the data used in the heterogeneity analysis of Section 7.5.

D.5.1 Empirical strategy for heterogeneity analysis

I estimate a modified version of the main specification in equation 2 to study heterogeneities in displacement effects.

$$\begin{aligned} y_i = \quad & \beta_1 \text{ Displaced}_i + \beta_2 \text{ Displaced}_i \times H_i \\ & + \beta_3 \text{ Adjacent}_i + \beta_4 \text{ Adjacent}_i \times H_i + \rho H_i + \mathbf{X}_i^{'} \Gamma + \varepsilon_i \end{aligned} \tag{D.3}$$

where H_i is the heterogeneity variable of interest. In all the analysis, H_i is a factor variable, so I will be studying the effects for different groups of individuals. The coefficient β_1 captures the effect of displacement for the reference group, while β_2 captures the additional effect of displacement for the group defined by H_i. β_3 and β_4 capture the analogous effects for individuals living adjacent to future highway construction. The control variables $\mathbf{X}_i$ include the same set of covariates as in the main specification, and I also include MSA fixed effects to control for time-invariant differences across MSAs. Standard errors are clustered at the MSA level to account for spatial correlation in the error term.

In the next subsections, I describe the construction of the heterogeneity variables used in the analysis.

D.5.2 Public housing availability in the MSA

I construct a measure of the pre-existing availability of public housing in the metropolitan area to examine whether displacement effects differ by this dimension. Using the HUD Characteristics of the Development Database, which was digitize by Shester (2013), I construct a measure of the pre-existing availability of public housing in the metropolitan area by calculating the ratio of public housing units to urban households in each MSA. The number of households is drawn from the 1950 Census household microdata, and public housing projects are identified as those with construction contracts signed or full occupancy by 1965. I discretize this measure by classifying MSAs as above or below the sample median of the public housing share. The results can be found in Panel (c) of Appendix Figure D.5.

D.5.3 Kin presence in the city

I study how family networks affect displacement effects. To proxy family network strength, I construct a surname concentration index using the 1950 census. This measure captures whether an individual's surname is unusually concentrated in their MSA relative to national patterns. I begin by using the `NYSIIS` algorithm to harmonize surnames using their phonetic equivalents. For example, treating Roberts and Robert as the same name (Abramitzky et al., 2017). For individual i with surname s in MSA c, I construct a family network measure using a location quotient:

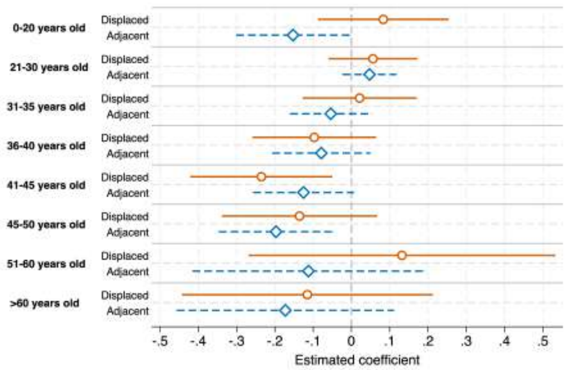
$$\text{FamilyNetwork}_{isc} = \frac{N_{sc}/N_c}{N_s/N}, \tag{D.4}$$

where N_{sc} is the number of people with surname s in MSA c, N_c is the total population in MSA c, N_s is the national count of surname s, and N is the national population. This index measures the concentration of a surname in a location relative to its national prevalence. A value of 1 indicates that the surname appears in the MSA at exactly the rate expected under random geographic sorting. A value greater than 1 suggests family clustering: for example, a value of 3 means the surname is three times more concentrated in that MSA than it is nationally. This specification addresses the concern that common surnames, like "Smith", would mechanically imply larger networks. Even if many individuals named Smith reside in a given MSA, the index equals 1 if their concentration is proportional to the national Smith distribution. I discretize this measure by using last names in the top 20 percent of the distribution of this location quotient as having strong family networks. Similar surname-based indices have been used in the literature to proxy for social capital and family networks (Posch et al., 2026). Finally, I construct a dummy variable equal to 1 if the individual's surname is in the top 20 percent of the city's distribution of this location quotient, and 0 otherwise. The results can be found in Panel (d) of Appendix Figure D.5.

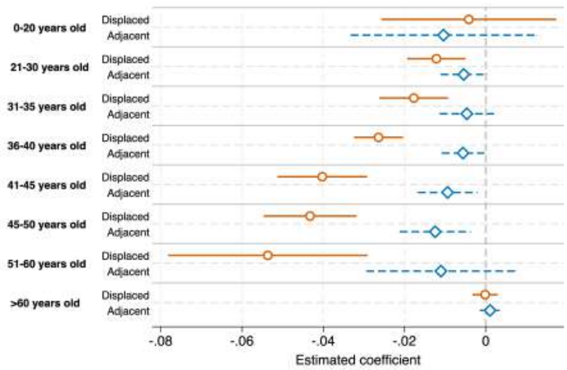
D.5.4 Neighborhood quality

I analyze whether the effects of displacement differ by the quality of the neighborhood in which individuals lived before displacement. To measure neighborhood quality, I use three proxies. The first two measures are based on the sociodemographic composition of the neighborhood. For each MSA, I create dummies if the neighborhood is above the median in median income and in the share of college graduates. The third measure is based on the redlining status of the neighborhood in which the household lived. The redlining status is based on the Home Owners' Loan Corporation (HOLC) maps, which graded neighborhoods from A to D based on their perceived credit risk. I create a dummy variable equal to 1 if the neighborhood was graded as D, and 0 otherwise. These maps have been digitized and georeferenced by Nelson et al. (2023) and are available at the census tract level. The results can be found in Appendix Figure D.6.

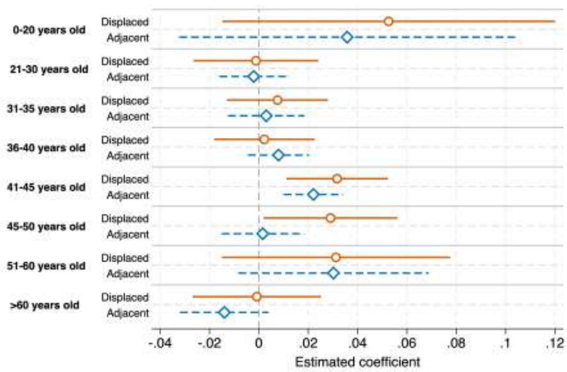
D.6 Figures for the heterogeneity analysis



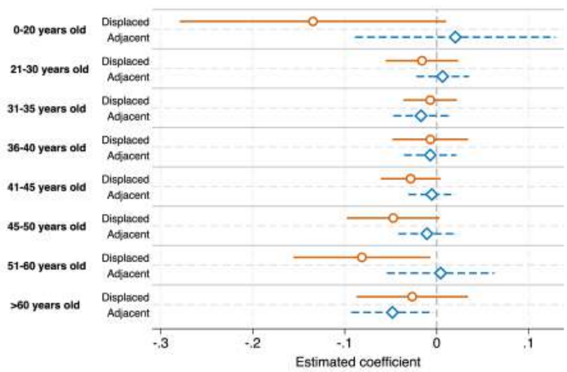
(a) Death age



(b) Same neighborhood



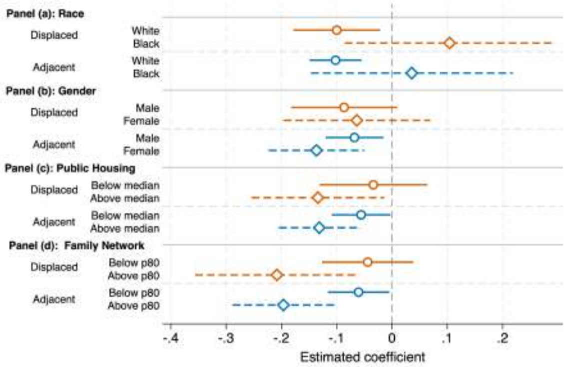
(c) Same city



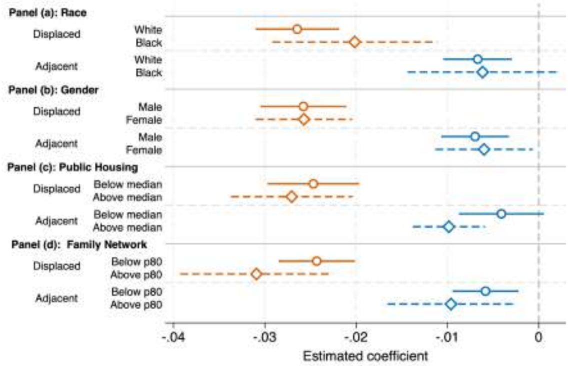
(d) Neighborhood quality

Figure D.4: Heterogeneity analysis by age at construction.

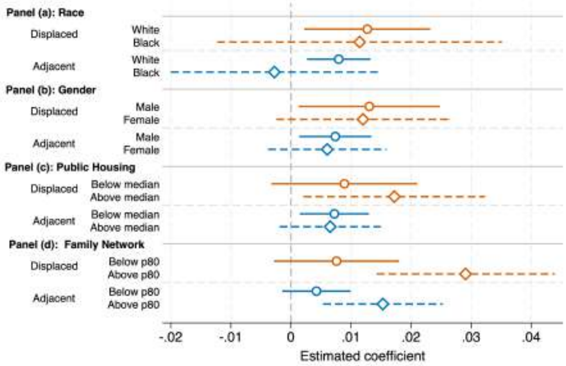
Notes: The figures display estimates from regressions of long-term characteristics on the two treatment indicators in equation D.3, along with 95% confidence intervals. Each row presents the results for individuals at different age groups when the highway was built. The sample includes individuals living within 200 meters of the newly constructed highway in the 1950 census. All regressions include household head's occupational score, as well as gender, race, birth year, and metropolitan area fixed effects. Standard errors are clustered at the metropolitan area level.



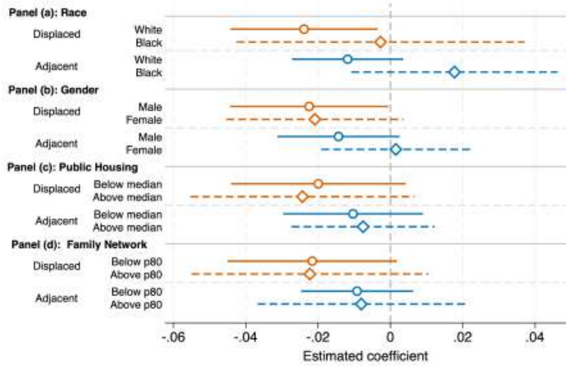
(a) Death age



(b) Same neighborhood



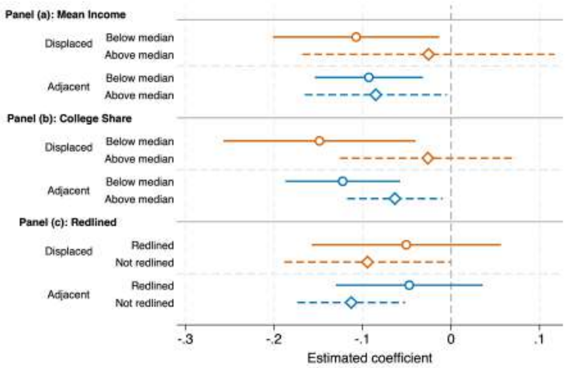
(c) Same city



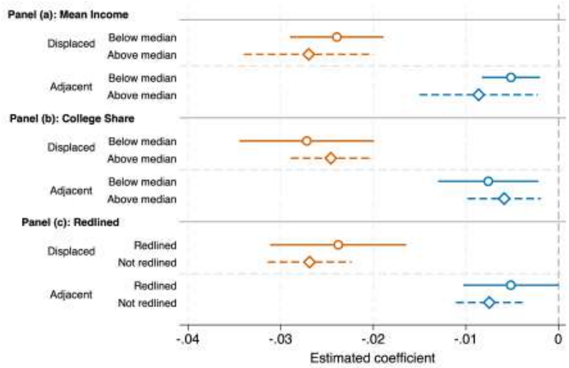
(d) Neighborhood quality

Figure D.5: Heterogeneity analysis by race, gender, public housing, and family network.

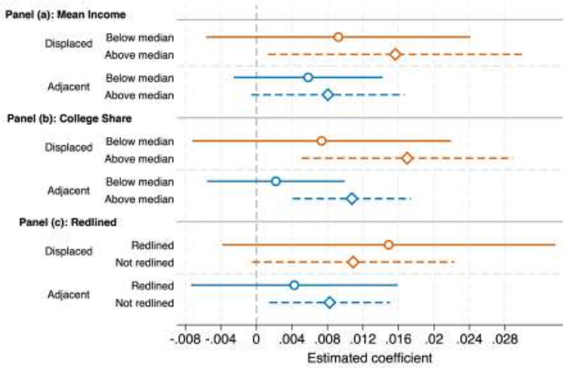
Notes: The figures display estimates from regressions of long-term characteristics on the two treatment indicators in equation D.3, along with 95% confidence intervals. Each row corresponds to a different regression, and the sample includes individuals living within 200 meters of the newly constructed highway in the 1950 census. All regressions include household head's occupational score, as well as gender, race, birth year, and metropolitan area fixed effects. Standard errors are clustered at the metropolitan area level.



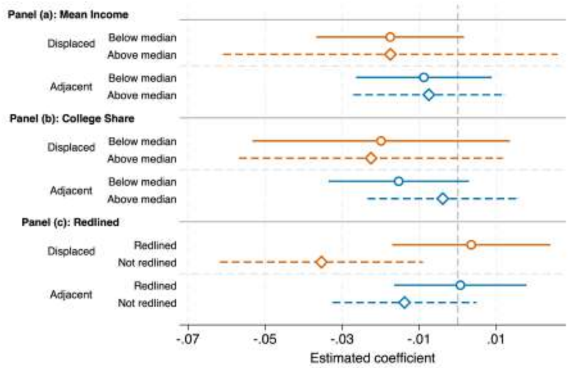
(a) Death age



(b) Same neighborhood



(c) Same city



(d) Neighborhood quality

Figure D.6: Heterogeneity analysis by neighborhood of residence in 1950.

Notes: The figures display estimates from regressions of long-term characteristics on the two treatment indicators in equation D.3, along with 95% confidence intervals. Each row corresponds to a different regression, and the sample includes individuals living within 200 meters of the newly constructed highway in the 1950 census. All regressions include household head's occupational score, as well as gender, race, birth year, and metropolitan area fixed effects. Standard errors are clustered at the metropolitan area level.

E. Mechanisms

This appendix provides additional details on the mechanisms driving the mortality results.

E.1 Mechanisms proposed in the literature

This section presents additional results on the channels through which displacement may affect long-term mortality, focusing on the mechanisms proposed in the urban affairs literature. In particular, I estimate equation 2 using five proxies for the channels identified in the literature: neighborhood economic distress, neighborhood quality, social network stability, distance from pre-construction home, and distance from city center. Appendix Table E.1 presents the results, which are discussed in detail in Section 8.1 of the main text.

Table E.1: Mechanisms driving mortality outcomes

	Unemploy. Share (1)	Neighborhood Quality (2)	Social Networks (3)	log Distance Between Homes (4)	Δ log distance to CBD (5)
Displaced	0.094^{c}	-0.020^{b}	-0.067^{c}	0.074^{a}	-0.012
	(0.051)	(0.009)	(0.036)	(0.023)	(0.009)
Adjacent	0.041	-0.008	-0.013	-0.003	-0.012^{c}
	(0.028)	(0.007)	(0.020)	(0.016)	(0.007)
Observations	119,569	119,648	117,814	118,595	118,595
Adj. R^2	0.207	0.191	0.066	0.118	0.155
Mean of dep. var.	5.713	0.001	1.333	10.589	9.521

Note: OLS estimates are reported. Coefficients of estimating equation 2 are reported with standard errors, clustered at the MSA level, in parentheses. The sample includes those individuals displaced by highway construction and those living within 200 meters of these projects in the 1950 census, linked to administrative mortality records from 1988 to 2007. The dependent variables are reported at the top of each column. All specifications include MSA fixed effects and controls for head-of-household occupational score, race, birth year, and gender. ^{a} indicates the coefficient is significant at the 1%, ^{b} at the 5%, and ^{c} at the 10% level.

E.2 Gelbach Decomposition

I employ the decomposition method developed by Gelbach (2016) to quantify the contribution of each mediating variable to the displacement effect. The approach treats each potential mechanism as an omitted variable and measures the bias that results from its exclusion.

Without loss of generality, we will focus on a single treatment variable (displacement) and a set of mechanism variables $\mathbf{X}_i$. Consider the full regression model:

$$y_i = \beta D_i + \mathbf{X}_i'\gamma + \varepsilon_i \tag{E.1}$$

where y_i is age at death, D_i is the displacement indicator, and $\mathbf{X}_i$ contains the mechanism variables. Omitting $\mathbf{X}_i$ from the regression yields the baseline estimate $\hat{\beta}^{base}$. By the omitted variable formula, this baseline estimate converges to $\beta + \Gamma'\gamma$, where Γ is the coefficient vector from projecting $\mathbf{X}_i$ onto D_i. The contribution of variable k to the baseline effect is thus $\hat{\delta}_k = \hat{\Gamma}_k \hat{\gamma}_k$, decomposing the total effect into interpretable components.

A key advantage of the Gelbach decomposition is that the contributions $\hat{\delta}_k$ are invariant to the order in which controls are added. This contrasts with sequential approaches where the change in the treatment coefficient depends on which controls have already been included. However, the decomposition captures correlations rather than causal mediation effects, and results depend on which covariates are included. Variables that do not directly affect the outcome but are correlated with included mechanisms may spuriously explain portions of the treatment effect.

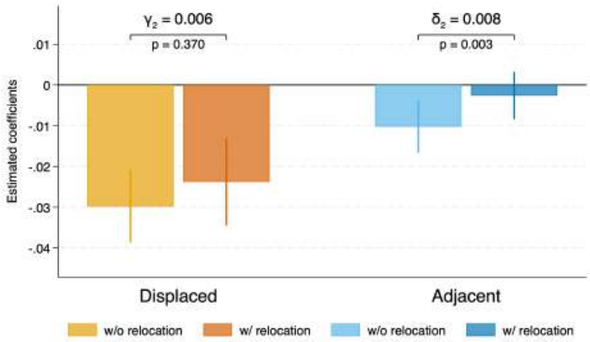
F. Relocation Assistance

F.1 Building the list of states providing relocation assistance

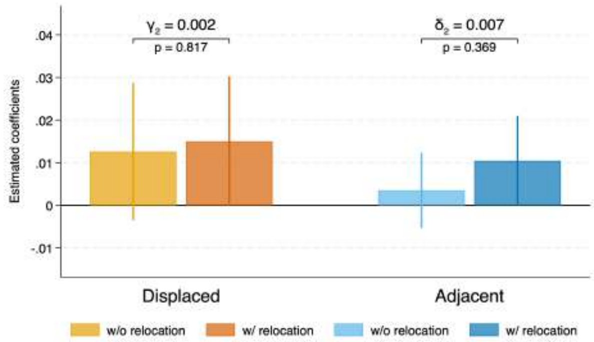
To study whether these payments improved long-run outcomes, I construct a novel dataset documenting when each state began providing relocation payments to displaced individuals. To the best of my knowledge, this is the first systematic compilation of state-level policy adoption timing. I begin by reviewing federal and state legislative records to identify when each state enacted laws authorizing such payments. I then cross-reference these records with four official sources documenting the state of relocation assistance programs as of January 1963, January 1965, and January 1967, as well as reported start years from the 1968 Federal-Aid Highway Act (Highway Research Board, 1963; US Advisory Commission on Intergovernmental Relations, 1965; U.S. Department of Transportation, 1967; United States. Department of Transportation, 1970). Together, these sources provide comprehensive coverage of state-level policy adoption, allowing me to construct a complete timeline of when each state began providing relocation assistance to displaced individuals. Appendix Table F.1 summarizes the data effort.

One limitation is that legislative enactment does not guarantee implementation. South Dakota passed authorizing legislation in 1963 but declined to operate the program (US Advisory Commission on Intergovernmental Relations, 1965, pg. 70). However, this is the only documented case of non-implementation, and South Dakota is excluded from my sample because it had no metropolitan areas in 1950.

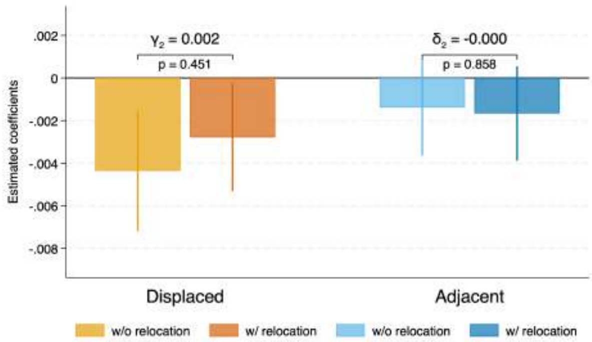
Appendix Figure F.3 shows the number of states enacting relocation assistance programs by year. We can see that most states adopted these programs between 1963 and 1966, although a quarter of states had not done so by 1968. As seen in Appendix Figure F.4, most late are in the South and non-coastal Western states. Finally, Appendix Table F.2 shows states with earlier adoption dates had larger GDP per capita and population in 1960, as well as lower number of Black population.



(a) Same neighborhood



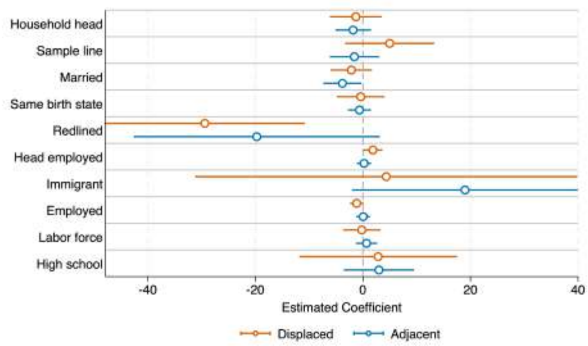
(b) Same city



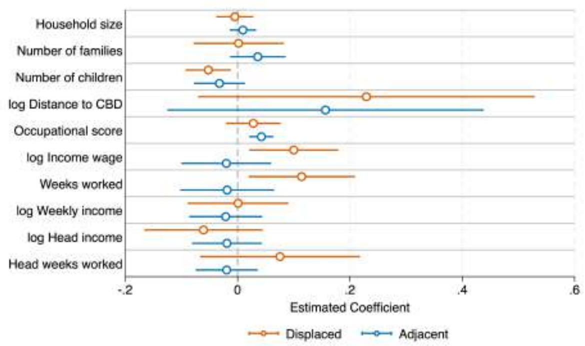
(c) Neighborhood quality index

Figure F.1: Effects of relocation assistance on other outcomes.

Notes: The figure compares the effect of Interstate highway construction for displaced individuals and those residing within 100 meters of the highway, with and without relocation assistance laws in place. The dependent variable in Panel (a) is an indicator for whether the individual lived in the same neighborhood (census tract) at the time of the census and at the time of death. The dependent variable in Panel (b) is an indicator for whether the individual lived in the same city (metropolitan area) at the time of the census and at the time of death. The dependent variable in Panel (c) is an index of neighborhood quality, constructed as the first principal component of the The comparison group consists of unaffected neighbors living between 100 and 200 meters from the highway. All regressions control for the household head's occupational score and include fixed effects for gender, race, birth year, and metropolitan area of residence. Standard errors are clustered at the metropolitan area level. The coefficient γ_2 captures the differential effect of displacement for those with access to relocation assistance, and δ_2 captures the corresponding differential for adjacent individuals.



(a) Indicator variables



(b) Standardized variables

Figure F.2: Balance tests for relocation assistance.

Notes: The figure present the coefficient and confidence intervals for being treated while eligible for relocation assistance, compared to being treated while ineligible, for a set of pre-treatment characteristics. Each coefficient corresponds to a different regression and is reported with standard errors clustered at the metropolitan area level. The coefficients and confidence intervals in panel (a) are normalized by the sample mean of the dependent variable, while the dependent variables in panel (b) are standardized to have mean zero and standard deviation one. All regressions control for the household head's occupational score and include fixed effects for gender, race, birth year, and metropolitan area of residence.

F.2 Additional tables and figures

Table F.2: Correlations between state characteristics and relocation assistance enactment.

	log GDP per capita (1963)	log Total Population (1960)	log White Population (1960)	log Non-White Population (1960)
Year of enactment	-2.258^{c}	-0.308	-0.430	0.240
	(1.312)	(0.381)	(0.376)	(0.199)
Observations	48	48	48	48
R-squared	0.036	0.012	0.024	0.025

Note: OLS estimates are reported. Each coefficient corresponds to a different regression and is reported with robust standard errors in parentheses. The unit of observation is a state. The dependent is the year of enactment of relocation assistance. Each independent variable is denoted in the column header. ^{a} indicates the coefficient is significant at the 1%, ^{b} at the 5%, and ^{c} at the 10% level.

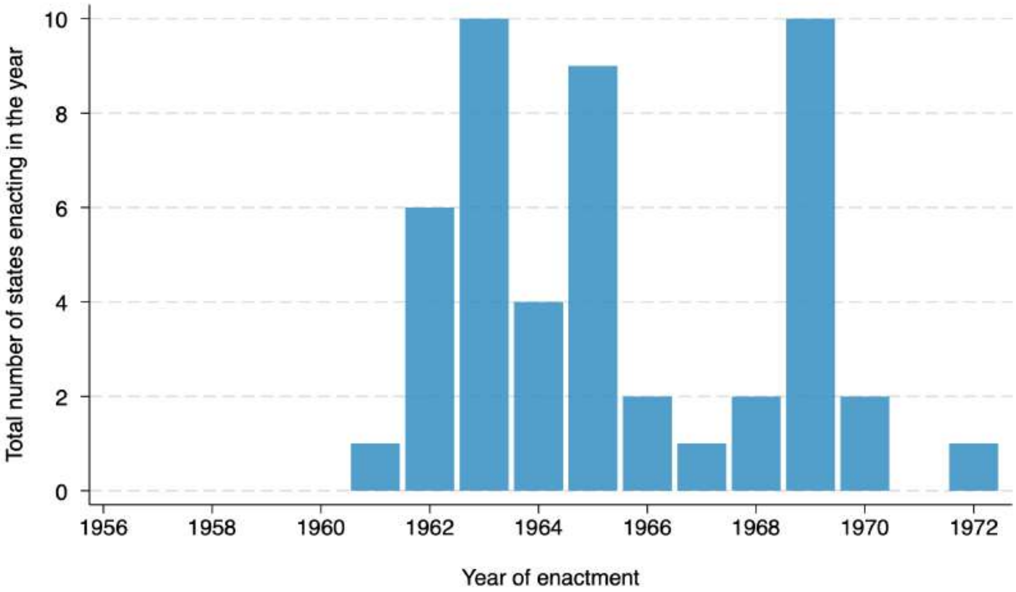


Figure F.3: Number of states enacting relocation assistance by year.

Notes: Each bar corresponds to the number of states enacting relocation assistance in a given year, as shown in Appendix Table F.1. Tennessee is not included in the figure because it enacted relocation assistance in 1951, before the start of my sample period.

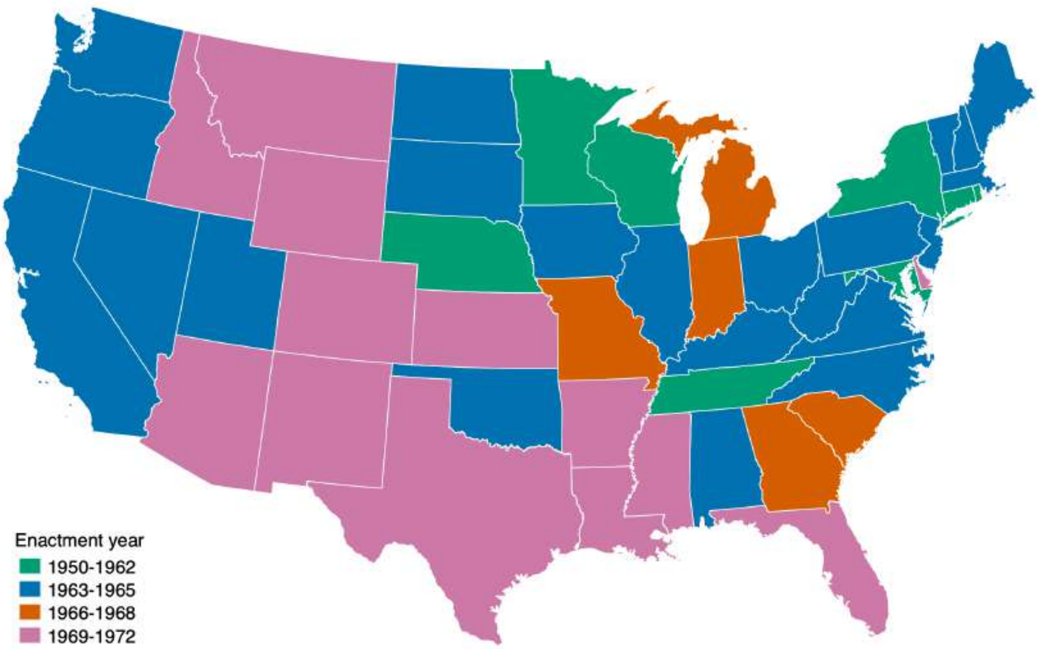


Figure F.4: Spatial distribution of years of enactment of relocation assistance.

Notes: Map showing the years of enactment of relocation assistance for each state, as shown in Appendix Table F.1.

Table F.1: Years of enactment of relocation assistance laws by state.

State	Year	Source
Alabama	1965	Acts of Alabama 1965, Number 228
Arizona	1969	United States. Department of Transportation (1970)
Arkansas	1969	Arkansas Acts 1969, Number 62
California	1965	The California Relocation Assistance Act of 1965 (Chapter 1650)
Colorado	1969	Colorado Session Laws 1970, Chapter 95
Connecticut	1962	U.S. Department of Transportation (1967)
Delaware	1970	Delaware Laws 1970, Chapter 634, codified in 29 Delaware Code § 8004
District Of Columbia	1964	Public Law 88-629, 78 Statute 1015
Florida	1969	United States. Department of Transportation (1970)
Georgia	1966	Georgia Laws 1966, p. 60, also known as House Bill 105
Idaho	1969	Idaho Session Laws 1969, Chapter 295
Illinois	1965	Illinois Laws 1965, p. 3377, House Bill 2210
Indiana	1967	Indiana Acts 1967, Chapter 273, codified as IC 8-13-18.5
Iowa	1965	Iowa Code Ann. § 316.6, as established by 61 G.A. ch. 319 (1965)
Kansas	1969	United States. Department of Transportation (1970)
Kentucky	1964	Kentucky Acts 1964, Chapter 15
Louisiana	1972	Louisiana Act 121 of 1972, codified as Louisiana Revised Statute 19:201
Maine	1965	U.S. Department of Transportation (1967)
Maryland	1962	Maryland Laws 1963, Chapter 36, Article 33A, §12
Massachusetts	1963	Massachusetts Acts of 1963, Chapter 718
Michigan	1966	U.S. Department of Transportation (1967)
Minnesota	1962	Minnesota Statutes § 117.50 et seq.
Mississippi	1970	Mississippi Acts 1971, Chapter 520
Missouri	1968	United States. Department of Transportation (1970)
Montana	1969	United States. Department of Transportation (1970)
Nebraska	1963	Nebraska Revised Statute § 39-1329
Nevada	1963	Nevada Statutes 1963, Chapter 408
New Hampshire	1965	New Hampshire Laws 1965, Ch. 433, codified as NH RSA 233-A
New Jersey	1963	New Jersey P.L. 1963, c. 79, codified as N.J.S.A. 27:7-44.3
New Mexico	1969	United States. Department of Transportation (1970)
New York	1962	Gov. Rockefeller's NY Relocation Department (1962)
North Carolina	1965	U.S. Department of Transportation (1967)
North Dakota	1963	North Dakota Century Code 24-01-41.1
Ohio	1963	Ohio Administrative Code 5501:2-5-03
Oklahoma	1963	Highway Research Board (1963)
Oregon	1963	Oregon Revised Statute § 366.515, modified by Ore. Laws 1963, ch. 472
Pennsylvania	1964	Pennsylvania Eminent Domain Code of 1964
Rhode Island	1962	Highway Research Board (1963), R.I. General Laws § 37-6.1-1 et seq.
South Carolina	1968	South Carolina Act N. 1233, codified as S.C. Code § 57-5-1610 et seq.
South Dakota	1963	US Advisory Commission on Intergovernmental Relations (1965)
Tennessee	1951	Public Acts of Tennessee 1951, Chapter 176
Texas	1969	Texas Acts 1969, Chapter 363, codified as Article 6674n-4
Utah	1963	Highway Research Board (1963)
Vermont	1963	Vermont Acts 1963, Number 110, codified at 19 V.S.A. § 2001 et seq.
Virginia	1964	Virginia Acts 1964, Chapter 46
Washington	1965	U.S. Department of Transportation (1967)
West Virginia	1963	West Virginia Acts 1963, Chapter 60
Wisconsin	1961	Highway Research Board (1963)
Wyoming	1969	United States. Department of Transportation (1970)

Note: Years of enactment of relocation assistance laws for each state, as documented in the sources listed in the third column.

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