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

Pablo Valenzuela-Casasempere†

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Abstract

I study the long-run effects of displacement and neighborhood division by examining individuals affected by the construction of the Interstate Highway System. To do so, I track individuals over time by linking the 1940 census to administrative mortality records from 1995 to 2005. I find that displaced individuals die three months younger, are more likely to leave their neighborhoods, and reside in areas with lower socioeconomic characteristics at the time of death. I also find highly localized spillovers: individuals living within 100 meters of a highway are more likely to leave their neighborhoods and relocate to lower socioeconomic areas, yet they do not experience increased mortality. The neighborhoods where individuals relocate after displacement explain 30% of the displacement-mortality effect. Accounting for the mortality effects of displacement would have increased the cost of building the highway system by 10%. Together, these results enhance our understanding of the costs displacement imposes on individuals and their communities and provide new insights for the design of future infrastructure projects.

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†Vancouver School of Economics, The University of British Columbia. pv93@student.ubc.ca

1. INTRODUCTION

Infrastructure provision is a key driver of economic growth and development, yet its provision is intrinsically linked to the forced relocation of residents.[1] For instance, the construction of the Three Gorges Dam in China, although impactful in its energy provision, removed more than 1.3 million people from their homes.[2] This, and many other infrastructure projects, have lead to the displacement and disruption of predominantly low-income and minority communities. Despite the prevalence of such displacements, research has primarily focused on estimating the benefits of infrastructure provision. Thus, we have little understanding of the costs of infrastructure construction, especially in the long-run. Understanding the long-run consequences of infrastructure provision is crucial for designing future projects and policies aimed at compensating for the negative consequences caused by these developments. The construction of the U.S. Interstate Highway System (IHS) offers a unique setting to study these dynamics.

The construction of the IHS was unique in both its scale and due to the absence of relocation assistance for affected individuals. The project built over 4,500 kilometers of highways within metropolitan areas between 1956 and 1972. Although no official records exist regarding the number of individuals displaced by its construction, estimates suggest that over one million people were displaced to make way for the highways, with millions more affected by living in impacted communities. Historical reports suggest that the displacement, combined with the lack of relocation assistance, had long-term negative effects on both individuals and communities.[3] Policymakers at the time, however, argued that the relocation of individuals would benefit communities by moving people out of blighted neighborhoods into better housing areas.[4] Despite the magnitude and significance of highway construction in the U.S., there continues to be limited understanding of how these projects affect displaced individuals and the residents of impacted communities.

This paper sheds new light on the consequences of infrastructure construction by providing the first causal estimates of the long-run impacts on individuals displaced by or living next to highways. Specifically, I focus on the early years of the IHS construction, between 1956 and 1960, when no relocation assistance was provided to those affected. While a broad range of research has explored how highway construction negatively impacts individuals and communities (Caro, 1974; Rose and Mohl, 2012), empirically iden-

[1] Highways increased trade and growth in U.S. cities (Duranton et al., 2014; Herzog, 2021). Ducruet et al. (2024) and Brooks et al. (2021) show similar results for port development, while Redding et al. (2011) highlight the positive impact of Frankfurt's airport hub on economic growth.

[2] See The Editors of Encyclopaedia Britannica (2024).

[3] Archer (2020) argues that displaced households often relocated to economically depressed areas or emerging "second ghettos."

[4] Grutzner (1957) reports that over one-fifth of the residential houses affected by urban renewal and highway projects in Inwood, New Jersey, required major repairs or were unfit for habitation.

tifying these effects is challenging. Unlike the benefits of highway construction, the costs are borne by displaced individuals who are often no longer in the same location. Identifying these costs is particularly difficult as it requires tracking individuals, not places, over extended periods of time.

I overcome this challenge by developing a novel method to measure who was displaced by highway construction by geocoding address-level information from the full-count 1940 census for all urban residents in the U.S. Once geocoded, I link each individual's home location to the highway network constructed by the Federal Highway Administration, supplemented with the PR-511 dataset, which contains information on the opening dates of each highway segment (Baum-Snow, 2007). Individuals whose residences were destroyed by highway construction are classified as displaced. To measure individuals' long-run outcomes, I link the geocoded individuals to administrative mortality records from 1995 to 2005 (Goldstein et al., 2023).[5] These records include details such as the date, age, and last residence of individuals, allowing me to track their movements and outcomes over time. Together, these datasets enable me to identify individuals displaced by highway construction and observe the long-term consequences of displacement. A limitation of this analysis is the time lapse between observing individuals in 1940 and the onset of highway construction in 1956, which means some individuals may have moved beforehand. Thus, the estimates presented correspond to a lower bound, as they represent the *intention-to-treat*.

My analysis begins with a descriptive account of the neighborhoods affected by highway construction. I examine cross-sectional variation across neighborhoods in 1950, the last census before the 1956 Federal Highway Act, to study which characteristics predict the location of highway construction. I find that housing prices, proximity to the city center, and the neighborhood's racial composition are significant predictors of highway placement. To discern whether these findings reflect a federal plan to route highways through marginalized communities or are driven by local political agendas, I analyze the Federal Engineering Maps created by the Bureau of Public Roads in 1955. These maps, which contain proposed highway routes in urban centers, were less likely to have been influenced by local political agendas (Weiwu, 2023). I digitize these maps for 100 metropolitan areas. When analyzing the factors that predict highway placement in these maps, I find that land prices and proximity to the city center are strong predictors. However, I do not find evidence that the racial composition of neighborhoods influenced the routing in these federal plans, hinting that local agendas may have influenced the location of highways.

Having examined neighborhoods in which highways were constructed, I then turn to analyze who lived in these neighborhoods before construction. No existing records docu-

[5] These records include the names of the individual's parents, which allows me to link women, something standard linked datasets can not do (Abramitzky et al., 2017).

ment the number or the identity of individuals displaced by highway construction before 1965, as governmental agencies did not keep track of affected individuals. To the best of my knowledge, this paper provides the first estimates of the magnitude of displacement during the early years of highway construction. I find that between 1956 and 1960 an estimated 33,000 to 40,000 individuals were displaced each year. This displacement rate is likely to have continued during the 1960s (Highway Research Board, 1967). Displaced individuals and those living near highways were more likely to be Black or immigrants, have lower employment rates, and live in cheaper housing than the rest of the city. These results are consistent with the historical evidence that highway construction occurred disproportionately in marginalized communities (Lewis, 2013). Together, these results suggest that highway construction disproportionately affected racial and socioeconomic minorities.

These imbalances in observed characteristics are important. To address these differences, the empirical strategy compares displaced individuals to their neighbors who were unaffected by the construction. However, highway construction may impact residents beyond those directly displaced, posing the challenge of determining an arbitrary distance cutoff where the effects of highway proximity no longer apply. I tackle this issue using a data-driven approach. I select the cutoff based on observed changes in long-run outcomes as the distance from the highway increases. Individuals are grouped into 100-meter bins based on their distance from the highway, with displaced individuals placed in a separate category. I then analyze how distance from the highway affects long-run life expectancy, the likelihood of leaving the neighborhood, and wealth accumulation at the time of death. The results suggest that spatial spillovers are highly localized, with significant effects observed only for displaced individuals and those living within 100 meters of the highway. No effects are found beyond 100 meters in any of the studied outcomes, indicating that spatial spillovers diminish rapidly. These findings align with prior research on highly localized spatial spillovers (Rossi-Hansberg et al., 2010; Hornbeck and Keniston, 2017; Moretti and Wheeler, 2024).

Building on the evidence of highly localized spillovers, the primary empirical strategy compares individuals displaced by construction to those living within 100 to 200 meters of the highway. Since individuals within 100 meters are also affected, they are included as a second treatment group in the analysis. For identification, this approach leverages the fact that, within the same neighborhood, some individuals were displaced while others were not, and the effects of highway construction diminish rapidly with distance. All groups are balanced in terms of pre-construction observable characteristics, and the results are robust to directly controlling for any imbalance. In all specifications, I control for race, city, gender, homeownership, and birth year fixed effects.

The results show that highway construction significantly affects both displaced individuals and those living nearby. Displaced individuals die approximately three months earlier than the control group and are significantly less likely to live to the age of 70, conditional on reaching 65. In contrast, the effect on mortality for individuals living within 100 meters of the highway is smaller and only marginally significant. Additionally, nearly all displaced individuals had moved to different neighborhoods by the time of their deaths, and those living within 100 meters were also more likely to relocate. However, despite these moves, both displaced and nearby individuals generally remained within the same city at the time of their deaths.

Despite the previous evidence supporting the primary strategy, the results could still be affected by spillovers due to proximity to highways or by endogenous routing. I develop two alternative empirical strategies to address these concerns. First, I perform caliper matching on observable characteristics using a pool of potential controls located more than 2,000 meters from any highway. This specification tackles the concerns that pre-existing differences or proximity effects influence the results. Second, I utilize digitized Federal planned engineering maps and select individuals who would have been displaced or lived within 100 meters of a planned highway as the control group. Since these maps were less likely to be influenced by political factors, this strategy addresses concerns about endogenous highway placement. I find quantitatively similar results across all specifications, suggesting that highway construction displaced individuals from their neighborhoods and reduced their life expectancy.

Building on the findings that highway-induced displacement reduces life expectancy, I conduct a duration analysis to examine how displacement affects the hazard rate of dying at a given age. I use a Cox proportional hazard model on the sample of individuals living within 200 meters of a highway to assess the role of displacement and highway proximity. The results indicate that displaced individuals face a 7.5% higher risk of earlier death compared to non-displaced individuals, with no discernible effect observed for those living adjacent to highway construction. These estimates are both large and economically significant. When benchmarking against other traumatic life experiences, I find that the effect of displacement on longevity is roughly a quarter of the effect experienced by parents after the death of a child, a third of the impact of a divorce, and an eighth of the mortality differences associated with homelessness (Song et al., 2019; Sbarra et al., 2011; Meyer et al., 2023). Thus, the analysis suggests that displacement significantly increases the risk of early death.

Next, I also explore whether and how displacement affects economic outcomes. Displacement can affect long-term economic wealth accumulation by eroding social networks, destroying valuable jobs, and forcing individuals to relocate to marginalized neighborhoods. While post-1950 individual wealth data is not publicly available, I use neighborhood level information from individuals' last known addresses as a proxy for their economic outcomes. The findings show that highway construction reduces wealth accumulation, with displaced individuals and those near highways residing in neighborhoods with lower educational attainment and employment at time of death.

The historical context suggests that the long-run effects of highway construction could have affected some groups differently. The period I study, from 1956 to 1960, is characterized by a series of social and policy changes that may have interacted with the effects of highway construction. Additionally, the use of eminent domain without providing relocation aid for displaced individuals suggests that the effects of highway construction may have been different for renters and homeowners. I explore how factors such as homeownership, race, exclusionary institutions, and age at the time of displacement influence the long-run outcomes of affected individuals. The findings reveal that the adverse effects of highway construction are more pronounced for Black individuals and renters, while there is no significant difference for those living in redlined areas. When examining the heterogeneous effects of age on displacement, I find that displaced individuals are more likely to move out of their neighborhoods regardless of age. Nevertheless, the adverse effects on mortality are concentrated among those displaced at both ends of the age distribution.

I perform a series of robustness checks. First, I use the Federal Engineering Maps as a placebo test. The analysis shows that highway construction often occurred in lower socioeconomic neighborhoods, raising concerns that pre-existing conditions could influence the estimated long-run effects. However, when using these maps, which were also routed through neighborhoods with lower socioeconomic characteristics, I find no significant impact. This suggests that neighborhood characteristics do not drive the previous results. Second, to control for spillovers from highway proximity, I re-estimate the results using individuals living between 1,000 and 1,100 meters, 2,000 and 2,100 meters, and more than 200 meters from highways as alternative control groups. Third, I repeat the analysis using the preferred specification, but I control for a larger set of individual characteristics that may affect the long-run outcomes. Fourth, I address potential contamination bias in the estimates by applying the approach of Goldsmith-Pinkham et al. (2024). Finally, I conduct a bounding exercise to assess the potential impact of household movement between 1940 and highway construction. Across all these robustness checks, the results remain quantitatively unchanged. With this multi-faceted empirical strategy, the effects on displaced and nearby individuals can be interpreted as causal.

With the negative effect of displacement on longevity established, I now examine the mechanisms driving these results. To do so, I perform an exact decomposition to determine how destination neighborhoods, loss of social capital, changes in economic opportunities, and access to healthcare contribute to explaining why displaced individuals die at a younger age. The analysis builds on the procedure developed by Gelbach (2016), which decomposes the contribution of each factor to the displacement-mortality effect. The estimates indicate that destination neighborhoods account for approximately thirty percent of the mortality results, with moderate contributions from the other factors. Overall, these factors collectively explain one-third of the displacement-mortality effect, leaving the rest of the effect unexplained.

This project provides two important policy implications. First, it offers valuable insights for guiding future infrastructure developments. Currently, infrastructure projects face rising costs due to extensive permitting processes and higher land prices (Brooks and Liscow, 2023), alongside urgent repair needs driven by aging structures and increasing climate change risks (American Society of Civil Engineers, 2021). The dual challenge is building cost-effective infrastructure while minimizing displacement. This paper's findings show that displacement has long-lasting adverse effects. Internalizing these costs would have increased the building cost of the IHS by 10% during the first years of highway construction. Second, it provides new insights for ongoing policies on neighborhood resilience. Current programs, like the *2022 Reconnecting Communities and Neighborhoods* project, focus on areas affected by highways rather than individuals.[6] Displaced individuals bear a disproportionate burden and are more likely to leave their neighborhoods. Based on my results, targeting individuals or their descendants, rather than just neighborhoods, would more effectively mitigate the negative consequences of highway construction.

The findings in this paper contribute to two strands of the literature. First, it deepens our understanding of the long-run consequences of displacement. To the best of my knowledge, this paper is the first to study the direct consequences of displacement caused by public good provisions on individuals.[7] Existing literature has focused on the long-run effects of displacement caused by environmental disasters, wars, or accidental events such as great fires and hurricanes, situations in which aid is usually provided (Becker et al., 2020; Deryugina and Molitor, 2020; Nakamura et al., 2021; Schwank, 2023). In contrast, I examine displacement caused by highway construction, a human-made event that offered no compensation to the affected individuals. Second, this paper further contributes to our understanding of the economic determinants and consequences of the IHS, which transforms neighborhoods in lasting ways (Weiwu, 2023; Valenzuela-Casasempere, 2024; Bagagli, 2023; Mahajan, 2023).[8] I develop methods to study the impact of IHS construction on individuals rather than neighborhoods and apply them to the study of highway-induced displacement. Tracing individuals after displacement provides insights into the mechanisms underlying its long-run effects. For instance, I show that the neighborhoods to which displaced individuals relocated played a key role in mediating the negative consequences of displacement.

[6] The *Reconnecting Communities and Neighborhoods* project, part of the 2022 Inflation Reduction Act, aims to "remove, retrofit, or mitigate an existing eligible dividing facility to reconnect communities."

[7] Rojas-Ampuero and Carrera (2023) study slum clearance programs in Chile, which also a forced individuals to relocate. However, in their context, individuals displaced received a new property, thus conflating the pure effect of displacement.

[8] Carter (2023) and Williams (2024) also study highway placement and its consequences for neighborhoods in the cities of Detroit and New Orleans, respectively.

2. HISTORICAL CONTEXT

This section provides historical context on the IHS construction, the historical evidence on the factors influencing routing decisions, and the short-run consequences of highway-induced displacement.

2.1 The Interstate Highway System

The Interstate Highway System (IHS) was established by the Federal-Aid Highway Act of 1956. This legislation aimed to enhance the nation's transportation infrastructure by constructing a National System of Interstate and Defense Highways. The bill proposed constructing over 41,000 miles of highways and allocated 25 billion dollars over twelve years to the project. The Federal Government would cover 90% of the construction costs, while state and local officials determined the routing of the interstates (Rose and Mohl, 2012). The proposed, and later approved, bill did not include provisions for the relocation of individuals or families displaced by highway construction, aside from eminent domain. To fund the construction without increasing government debt, the legislation established a highway trust fund, financed through an increase in federal gasoline and diesel taxes (Lewis, 2013, pp. 112-118). The enactment of this bill ensured the development of a modern, interconnected, and transcontinental highway network, addressing the country's need for a comprehensive transportation system (Murphy, 2009).

A key factor in the bill's approval was the allocation of over 2,175 miles of interstate highways routed through metropolitan areas. The Federal Government proposed these routes and detailed them in a report informally known as the "Yellow Book," which was distributed to every senator before the vote (Lewis, 2013, pp. 119-120).[9] The Yellow Book contained maps outlining the Federal Government's plans for a network of urban highways in 100 metropolitan areas (Rose and Mohl, 2012). Appendix Figure A.1 presents these maps for Atlanta, Detroit, Miami, and New Orleans. These maps resulted from federal engineers designing a highway network that connected to interstate segments while minimizing construction costs, and were not influenced by local factors beside land prices. By illustrating how the interstates would benefit their districts, the maps helped representatives secure the necessary votes for the bill's passage. However, these proposed routes were not binding. As Rose and Mohl noted, "Congress and President Eisenhower reaffirmed the long-standing principle that the locus of authority in highway programming rested unambiguously in the hands of state highway officials" (Rose and Mohl, 2012, p. 161). This flexibility granted state and local officials the power to determine the location of urban routes, which they often used to advance their own agendas

[9] The official title of the report is "General Location of National System of Interstate Highways, Including All Additional Routes at Urban Areas".

(Rose and Mohl, 2012, p. 97). Figure A.2 shows the planned and built highway networks for Atlanta, Detroit, Miami, and New Orleans, indicating that while highway segments generally aligned with the intended origins and destinations, there were variations in the specific locations compared to the initial plans.

Despite the intended benefits of a network of urban highways, the construction of the IHS led to widespread displacement of people. City officials often envisioned these new highways as means of clearing "blighted" urban areas, frequently at the expense of residents in predominantly Black neighborhoods. As Alfred Johnson, executive director of the American Association of State Highway Officials, recalled, "Some city officials expressed the view in the mid-1950s that the urban interstates would give them an opportunity to get rid of the local "n*****towns"" (Rothstein, 2017, p. 128). Planning experts in the mid-1960s forecasted that the construction of the interstate network would result in the displacement of more than one million people, primarily African Americans (Rose and Mohl, 2012, p. 96). Moreover, federal and local agencies provided little to no assistance to displaced households in finding new living arrangements. Consequently, highway construction forced these households to relocate to the fringes of cities or emerging "second ghettos" (Rothstein, 2017; Archer, 2020).

One of the most well-documented cases of highway construction used for Black removal occurred in Miami, Florida. State planners routed Interstate 95 directly through the heart of Overtown, a community that was the center of economic and cultural life for the city's Black population. State officials overlooked an alternative route that would have used an abandoned railway right-of-way, resulting in minimal population displacement (Rothstein, 2017). Consequently, Interstate 95's construction displaced approximately ten thousand Black individuals from their homes and communities (Archer, 2020). A similar situation unfolded in Detroit, Michigan, where the predominantly Black neighborhood of Black Bottom was eradicated by the construction of Interstate 75 (Avila, 2014, pp. 89-90). Figure A.3 shows how highways bisected the neighborhood, destroying several hundred homes. In St. Paul, Minnesota, Interstate 94 cut through the city's Black community, displacing one-seventh of St. Paul's African American population. As one critic noted, "very few Black individuals lived in Minnesota, but the road builders found them" (Rose and Mohl, 2012, p. 108).

The 1956 Federal-Aid Highway Act included no obligations at the federal or state level to assist residents whose homes were demolished or whose homes would be in proximity to new roads. Arthur Burns, chairman of the Council of Economic Advisors in the Eisenhower administration, warned policymakers that compensating people for losing their homes would be too costly, as the highway program was predicted to evict nearly one hundred thousand people a year (Archer, 2020). As a result, no provisions were made to compensate displaced residents or families experiencing air and noise pollution from proximity to a highway (Schwartz, 1975). Thus, during the first decade of highway construction, displaced individuals entered the housing market on their own, bearing the full costs of relocation (Davis, 1965).

Supporters of highway projects at the time justified the displacement of residents as an opportunity for urban renewal. Some policymakers argued that relocating residents would ultimately lead to better housing conditions for displaced families. For example, a survey of housing in Inwood, New Jersey, found that over one-fifth of the 650 housing units required significant repairs or were deemed uninhabitable (Grutzner, 1957). However, this vision of improved living conditions failed to materialize in practice, as many residents were left with few alternatives after being displaced.

The first bill to allocate funds for relocation assistance was the Highway Act of 1962.[10] The Act, which came into effect in July 1965, provided a maximum of $200 for residential moves and $3,000 for business relocations. The legislation mandated that transportation projects receiving federal aid must provide support to secure relocation housing for those displaced by road construction (Archer, 2020). However, by the time relocation assistance was integrated into highway construction, much of the damage had already been inflicted. Most urban interstates had been built before housing support became part of highway construction policy.

Albeit late, the assistance provided by the 1962 Act fell short of the needs of displaced individuals. It was not until 1965, almost a decade after the 1956 Federal Highway Act, that the Federal Government required advanced housing relocation for families and businesses displaced by highway construction. By 1967, only 33 states had authorized the payment of relocation costs (US DoT, 1970). As Rose and Mohl (2012) note: *"during most of the expressway-building era, little was done to link the Interstate highway program with public or private housing construction or even with relocation assistance for displaced families, businesses, or community institutions such as churches and schools."* In that line, the Highway Research Board (1967) notes that a small fraction of relocated families found a new home with agency aid. Among the nearly fifty thousand individuals displaced between April 1965 and October 1966, only 37% sought advisory assistance, and a mere 15% were relocated due to this assistance. Also, the average payment made to displaced households was $110, which was insufficient to cover the costs of moving (Highway Research Board, 1967). In addition, the communities affected by highway construction were not given any support to rebuild. The first Federal plan to allocate funds for households and communities affected by their proximity to highways came only under the Biden presidency, more than sixty years after the 1956 Federal Highway (117th Congress, 2022).[11] In sum,

[10] The bill was later modified in 1968 to include more comprehensive relocation assistance. In 1971, the Uniform Relocation Assistance and Real Property Acquisition Act was passed, which required the creation of standard relocation procedures for those displaced by federal eminent domain.

[11] The *Reconnecting Communities and Neighborhoods* project, included in the Inflation Reduction Act of 2022, aims to reconnect neighborhoods divided by highways. It is part of the 2022 Inflation Reduction Act and allocates funds to "remove, retrofit, or mitigate an existing eligible dividing facility to reconnect communities."

up until recent years, assistance for relocation has been scarce, and even when it was available, it did not give enough support to affected households.

In a highly segregated era for the U.S., highway construction forced households to relocate. Forced relocation has long-term adverse effects on affected individuals' socio-economic welfare and psychological well-being. At the time the IHS was built, housing alternatives for the displaced were largely limited to other racially segregated and economically disadvantaged communities. These options included "emerging second ghettos" or transitioning neighborhoods where working-class Whites predominated (Archer, 2020). Figure A.4 exemplifies these reports. It shows the residence location at the time of death for individuals living in Detroit and New Orleans before and after highway construction. The figure presents stark differences in neighborhood choice among the racial lines. Even fifty years after highway construction began, Black individuals are clustered close to the city center, whereas White individuals are spread across the city. In addition, highway construction impacted individuals' psychological well-being. By looking at families displaced by urban renewal projects in Boston's West End, Fried (2017) finds that most of the interviewees expressed deep grief. The feelings of loss manifest long after the individuals relocate to new living arrangements. These feelings stem from fragmented routines and the loss of personal and social factors (Fried, 2017). Another example of psychological distress caused by forced relocation is the case of urban expressways in New York. Caro (1974) noticed that displaced residents of the Cross-Bronx Expressway were left with a sense of loss and disorientation. Consequently, the forced relocation of households has long-term adverse effects on affected individuals' socio-economic welfare and psychological well-being.

2.2 Short-run consequences of highway-induced displacement

Beginning in 1973, the U.S. Census Bureau started collecting data on the housing characteristics of households nationwide. This survey not only collected information on housing conditions but also asked residents about their previous residence and the reasons for moving. In the 1973 and 1974 rounds, the survey included an option for households to indicate if they had moved in the past twelve months due to being *Displaced by Urban Renewal, Highway Construction, or Other Public Activity*. With this data, I can test the historical evidence that highway construction led to adverse housing outcomes for affected individuals.[12] The sample I use corresponds to all individuals who moved in the past twelve months on both surveys, and the equation I estimate is follows:

[12] While I cannot distinguish between types of displacement, urban renewal and highway construction were closely intertwined. Although both policies led to family displacement, urban renewal projects often included some form of relocation assistance, while highway construction did not (Rothstein, 2017). Therefore, the consequences presented here may represent a lower bound of the effects of highway-induced displacement, as the survey does not distinguish between displacement types.

$$y_{i,s} = \beta \text{Displaced}_{i,s} + \delta \text{PrevOwnership}_{i,s} + \mathbf{X}_i' \Gamma + \lambda_s + \epsilon_{i,s} \tag{1}$$

where i denotes an individual who moved in the past twelve months, and s denotes the survey year. $y_{i,s}$ is the housing characteristic of the actual residence of individual i in survey s. $\text{Displaced}_{i,s}$ is an indicator that equals one if the individual was displaced by highway construction in the previous twelve months. The control $\text{PrevOwnership}_{i,s}$ is an indicator that equals one if the individual owned their previous residence and accounts for differences in wealth/economic status between individuals.[13] The vector $\mathbf{X}_i$ includes an indicator if the household head is Black, and a quadratic term in age. λ_s are survey fixed effects. Robust standard errors are presented.

The survey suggests that displaced individuals are more likely to live in public housing. Table 1 presents the estimates for the housing characteristics of displaced individuals. Individuals displaced by highway construction are 9.7 percentage points more likely to live in public housing after displacement. They also reside in smaller houses, with approximately 0.23 fewer rooms. There is mixed evidence regarding the quality of the new housing for displaced individuals. The survey indicates that displaced individuals are 10.8 percentage points more likely to be connected to a sewage system, which aligns with the notion that highway construction displaced individuals into better housing. However, they also lived in houses with lower home prices and rents.

The survey also gathered information on satisfaction with the new neighborhood. As shown in Table B.1, displaced families reported satisfaction levels comparable to the rest of the sample. I find no statistical evidence that displaced individuals are less satisfied with their new neighborhood. Together, the findings using the 1973 and 1974 American Housing Survey aligns with the historical evidence that argues that highway-induced displacement led to adverse housing outcomes for affected individuals. Also, it suggest that individuals move to public housing and lived in cheaper houses, which by its own could have detrimental long-run effects (Chyn, 2018). In the next section, I present the data used and describe the novel strategy to identify individuals displaced by highway construction.

3. DATA

A major empirical challenge to overcome in this paper is to identify individuals affected by highway construction and then observe these individuals after highway construction. I exploit the address information in the 1940 U.S. Census and develop new methods to

[13] The results are equivalent if I use the previous value of the housing characteristic as a control, but using them decreases considerably the sample size. Results are available upon request.

geocode the housing information in the historical census. Then, I use data on the highway network linked to opening dates to identify individuals living in areas affected by highway construction each decade. Finally, I link the 1940 census to administrative mortality records from 1995 to 2005 to observe the long-term impacts of highway construction on urban residents.

3.1 Measuring highway-induced displacement

I use the address information in the 1940 U.S. Census to identify individuals living in areas affected by highway construction.[14]

Method. I identify displaced families as those living in dwellings that were destroyed by highway construction. The census collected information on the exact address of each household as shown in Appendix Figure C.1. I clean the addresses following the practices recommended by Logan and Zhang (2018) and geocoded them to obtain the latitude and longitude of each dwelling. Panel (a) of Figure 1 presents the location of dwellings in a neighborhood in Cleveland, Ohio, in 1940. Because this project's scope is on urban displacement, I only focus on urban residents of counties that are part of the 168 Standard Metropolitan Areas (SMA) in 1950. The sample covers 82.16% of the urban residents in the U.S. in 1940, for which I managed to geocode 79.11% of the dwellings or 80.85% of the urban residents. Appendix Section C contains more information about the sample and geocoding process.

To identify which households were displaced by highway construction, I need information on the exact location of each highway segment with its respective opening date. I obtained this information by linking the Federal Highway Administration PR-511 database used by Baum-Snow (2007) to the actual highway network (OpenStreetMap, 2017). I create a buffer around each highway segment equal to the total count of lanes multiplied by 3.6 meters, the average lane width in the U.S. (Federal Highway Administration, 2007). Panel (b) of Figure 1 shows the location of the highway network in Cleveland, Ohio, in 1940. The solid red lines represent the highway segments, and the light red area surrounding them corresponds to the buffer around each segment. The figure also shows which dwellings were affected by the construction of the highway. Displaced individuals correspond to those who were living in dwellings that were location within the buffer of a highway segment. I complement this information with the Federal engineering maps created by the Bureau of Public Roads in 1955, informally known as the Yellow Book (Bureau of Public Roads, US, 1955). This collection included planned urban segments of the IHS for 100 metropolitan areas, which I manually geocoded.[15]

[14] The full-count 1950 U.S. census has recently been released and I plan to use it in future versions of this paper.

[15] Appendix Figure A.1 shows these maps for the cities of Atlanta, Detroit, Miami, and New Orleans.

The classification strategy identifies individuals affected by highway construction. In principle, highway construction destroyed street segments and street names or numerations may have changed over time, which pose a challenge for geocoding. Hence, I inevitably misclassify some individuals as affected by highway construction. However, the classification strategy is conservative, as it only includes individuals which the procedure was able to geocode to an unique latitude and longitude. Also, modern geocoders are equipped with algorithms that can handle these challenges so that they are able to interpolate the location of addresses in street segments without numeration. To minimize the number of misclassified individuals, I only include individuals living in dwellings that were geocoded to the exact latitude and longitude. Although these decisions reduce the sample size, they ensure that the results are not influenced by the geocoding process.

3.2 Following individuals after highway construction

In addition to where individuals lived in 1940, I needed information on individuals after highway construction. I link the 1940 census to administrative mortality records from 1995 to 2005 (Goldstein et al., 2023). These records cover almost the entire universe of deaths among American citizens during this period. The records were linked using a linking algorithm proposed by Abramitzky et al. (2017), which uses time invariant characteristics to match individuals. In particular, a person is linked from the 1940 census to the mortality records if their name, year of birth, and state of birth match and if the match is *unique* conditional on race. I use a method that allows for misspellings by matching names based on their phonetic sound (NYSIIS). An advantage of the mortality records is that it includes the names of the deceased's parents, which allows me to link women who changed their last name after marriage.

Mortality records contain detailed information on age of death, reliance on social security, and the nine-digit ZIP codes of the last residence. Nine-digit ZIP codes are highly ganular indicators of location, which refer to a "segment or one side of a street" (United States Postal Service, 2024). The ZIP code information allows me to obtain rich information on the socioeconomic characteristics of a person's neighborhood. Neighborhood level information is retrieved from the National Historical Geographic Information System (NHGIS) for the year 2000, which lies in the middle of the mortality records period.

3.3 Sample

For the main analysis, I focus on individuals affected by highway construction that occurred between 1956 and 1960 for two primary reasons. First, this period marks the initial years of the IHS, increasing the likelihood that individuals remained in the same location. Second, limiting the sample to this timeframe excludes individuals who may have received relocation assistance from the Federal government, allowing me to isolate the pure effects of displacement on individuals. These restrictions lead me to identify 124,665 individuals who were living in 1940 in a dwelling that was destroyed by highway construction. The sample is distributed across 75 SMAs with 13.34% of them being Black individuals.

Turning the attention to the 1940 census linked to the mortality records, I impose an extra restriction to the linked sample. I restrict the sample to individuals who died between 1995 and 2005 to improve coverage in mortality records during this period and to reduce errors in identifying the last place of residence.[16] The restriction results in 2,141 displaced individuals linked to the administrative mortality records, 7.29% of them being Black individuals.

Potential Sample Bias. In constructing the main sample, I rely on geocoding individuals in 1940. One may be concerned that the geocoding procedure introduces mechanical differences in the sample between families affected by highway construction and those that were not. For example, the geocoding process may be less accurate for individuals living in areas with lower-quality housing, which could lead to only include the most affluent individuals in the sample.

To examine the quantitative importance of this concern, in Appendix Table C.2 I compare the characteristics of individuals geocoded and not geocoded and presents their average outcomes by group. The evidence for this concern is mixed. On the one hand, individuals that were not geocoded are more likely to be Black, are less likely to be home owners, and perform occupations with a lower occupational score. On the other hand, they are more likely to have a high school and college degree, their home values are higher, and they are more likely to employed. Overall, while there are some differences between the two groups, the evidence does not point to a consistent or significant bias affecting the results.

Another concern is that the linking procedure between the 1940 census and the mortality records may introduce mechanical differences between individuals affected by highway construction and those that were not. A plausible concern is that life expectancy and socioeconomic status depends on the ability to link individuals across datasets. For example, highways were built in areas with a higher concentration of Black individuals, who have lower linking rates and life expectancy. Goldstein et al. (2023) find that Black individuals are underrepresented in the linked sample, but the linked individuals are representative of the U.S. population for each racial group. I examine the quantitative differences between the linked sample and the rest of the geocoded sample in Appendix Table C.3. Individuals matched to mortality records are younger due to the double truncated mortality years (1995 to 2005). Consequently, individuals are less likely to be mar-

[16] Some ZIP codes changed over time, potentially leading to misclassification of the last residence location. I use TIGER Line data from 1995 onwards to match ZIP codes to the corresponding neighborhoods.

ried, have a college degree, or be employed. The matched sample also underrepresents Black individuals and immigrants, but is similar to the non-matched sample for the rest of the variables. To err on the side of caution, I use mortality adjusted weights that account for differences in mortality between the linked sample and the US population throughout this paper.

4. Interstate Highway System Construction

This section documents that highway routing was not random, but rather systematically placed through neighborhoods with a larger Black share and lower socioeconomic status. Exploiting neighborhood-level data from the 1950 census, I show that highways were built through neighborhoods with a larger Black share, lower home values, and rents, and closer to the city center. I then use the geocoded data to provide the first estimate of the number of individuals displaced by highway construction during its early years, followed by a characterization of these individuals.

4.1 In which type of neighborhoods were highways built?

Which factors dictate highway construction and, consequently, who gets displaced? 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. The sample size is limited to 62 cities with spatial information in the 1950 census. The estimating equation follows:

$$y_n = \lambda_{c(n)} + \gamma \text{DistCBD}_n + \Gamma_1' \mathbf{S}_n + \Gamma_2' \mathbf{P}_n + \Gamma_3' \mathbf{G}_n + \epsilon_n \tag{2}$$

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 DistCBD_n is 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. The equation city fixed effect, $\lambda_{c(n)}$. The regression results are weighted by the total population of tract n in 1950, and the standard errors are clustered at the city level.

Estimates of equation 2 are reported in Table 2. Columns (1) to (5) present the results for the network of highways built. 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. The results are similar when using distance to future highway developments as the dependent variable, as seen in Appendix Table D.2. 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.[17]

One possible explanation for these results is that state planners followed the Federal government's dictates. In column (6) of Table 2, I re-estimate equation 2 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 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.

This section's results are robust to a battery of robustness checks. In particular, as shown in the Appendix Section D results are robust to the use of alternative definitions of neighborhoods, incorporating nonlinear controls for proximity to the city center, the

[17] These results contradict part of the findings of Carter (2023) and Weiwu (2023). 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. This 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. Although these two variables are highly correlated, they differ in spirit. Qualitative accounts indicate that highways were constructed "where Black individuals live" (Rose and Mohl, 2012). I argue that the definition used in this paper better reflects this motive. To illustrate, consider a city consisting of two neighborhoods: one with 1,000 Black residents and a total population of 2,000, and a second with a Black and total population of 100. If a highway is constructed through the first neighborhood, then the authors' definition of the Black share will not be statistically significant. However, the highway was built through the neighborhood that housed roughly 90% of the city's Black population.

omission of weights, and to different cluster definitions as well as Conley standard errors to account for spatial correlation within a 10-kilometer radius. Also, the results are widespread across different cities, as shown in the leave-one-city-out estimations.

4.2 Affected individuals

To the best of my knowledge, this paper is the first to provide an estimate of the number of individuals displaced by highway construction during its early years. Before the enactment of the 1962 Federal-Aid Highway Act, highway builders did not systematically collect data on the number of individuals displaced by highway construction. The primary reasons for this lack of data collection were the absence of relocation payments or assistance for displaced individuals and the potential liability that accurate counts might impose on planners and authorities (Schwartz, 1975, pg. 235). As a result, the number of individuals displaced by highway construction, particularly during the first years of construction, is not documented. This paper is the first study to fill this gap by providing an estimate of the number of individuals displaced by highway construction during the first years of highway construction. In addition to estimating displacement, I also characterize both the displaced individuals and those residing in neighborhoods bisected by the highways.

Table 3 presents an estimate of the number of individuals displaced by or living in close proximity to future highways, and their characteristics. Panel A presents the estimated number and the corresponding percentage of the population affected by highway construction. I estimate that the total number of individuals displaced by highways constructed between 1950 and 1960 is 131,486. The Federal Highway Act of 1956 was in place only for four years during this decade. So, this estimate implies that, on average, 32,850 individuals were displaced by highway construction in urban areas each year. The estimate would rise to 40,060 displaced individuals if the ungeocoded fraction had the same proportion of displaced individuals as the geocoded sample. This estimate is a lower bound, as I only manage to geocode 80% of the individuals in the 1940 census.[18] These estimates are in line with the official numbers reported in 1967, which indirectly supports the quality of the geocoding process. The Federal Government started collecting data on the number of individuals displaced by highways in 1965. Their official estimate is that an average of 33,070 individuals were relocated by highway construction each year during the period of April 1965 and October 1966 (Highway Research Board, 1967, p. 2).

I now turn my attention to the characteristics of individuals living in places where

[18] Another reason why this estimate is a lower bound is that I can only observe highway openings. If the construction started before 1960 but the opening occurred afterward, I would only observe those displaced households in the following decade. Appendix Table D.8 ameliorates these concerns by providing estimates for all segments opened. The results are consistent with the estimates for highways opened between 1950 and 1960.

highways were built. Panel B of Table 3 reports the variable averages for individuals in each bin of proximity to highways, whereas Panel C reports household-level averages. The results align with the evidence presented in the previous section: the Black share is larger closer to future developments, with lower high school graduation rates. Individuals living close to future developments are more likely to be part of the labor force but are less likely to be employed. Conditional on employment, individuals living closer to future highways were performing occupations with lower occupational income, proxied by the 1940 census' occupational score. Households are less likely to be homeowners and inhabited in places with lower home values and rents. These results are consistent with the anecdotal evidence that highways were built through neighborhoods with a larger Black share and lower socioeconomic status.

5. Consequences of the Interstate Highway System Construction

Thus far, I have shown that highway placement disproportionately affected low-income and minority individuals. This section assesses the long-run effects of the IHS's construction on the individuals and communities impacted. I begin by showing that highway construction had localized effects on individuals living near highways, with no discernible impact on those living 100 meters or farther away. To identify the effects of highway construction, I compare individuals displaced by or living close to a highway with those residing in the same neighborhood in 1940. As alternative specification, I use historical maps and matching on observable characteristics. My findings indicate that highway construction displaced individuals from their neighborhoods, increased mortality rates, and reduced long-run wealth accumulation.

5.1 Long-run Consequences of Highway Construction for Residents in Affected Neighborhoods

How can highway construction affect individuals living near highways? First, highways are a source of noise and pollution that impact the health of individuals living nearby (Currie and Walker, 2011). Additionally, highway construction erodes the social capital of neighborhoods, which can have long-lasting effects on residents (Rose and Mohl, 2012). However, policymakers at the time viewed highway construction as a means to revitalize urban areas and improve the living conditions of those communities (Schwartz, 1975). Economic opportunities resulting from highway projects could also offer some potential benefits. In this section, I test these hypotheses by estimating the effects of highway construction on individuals living at varying distances from highways.

I estimate the effects of highway construction on long-run socioeconomic outcomes for residents of the affected neighborhoods. I only use highway segments that were opened between 1950 and 1960 in the analysis. To account for potential heterogeneous effects based on proximity to highways, I group individuals into 100-meter distance bins. Displaced individuals are included as a separate bin in the analysis. I only include individuals living within 2,100 meters of the highway in the analysis to avoid the potential bias of including individuals living far from highways. The estimating equation is given by:

$$y_i = \beta_0 \text{Disp}_i + \sum_{k=1}^{21} \beta_k \text{Bin}_{ik} + \lambda_{c(i)} + \Gamma' \mathbf{X}_i + \epsilon_i \tag{3}$$

where y_i is the outcome of interest for individual i measured at time of death. The sample corresponds to individuals living in 1940 within 2,100 meters of highway segments opened between 1950 and 1960, linked to administrative mortality records between 1994 and 2005. Disp_i is an indicator that equals one if individual i was living in 1940 in a highway location, Bin_{ik} is an indicator that equals one if individual i was living in 1940 in a 100 meter distance bin k to the closest highway, $\lambda_{c(i)}$ are city fixed effects, and ϵ_i is the error term. The vector X_i includes race, birth year, gender at birth, and homeownership status in 1940 fixed effects. I cluster standard errors at the city level.

I find that the long-run effects of the construction of the Interstate Highway System is highly localized. Figure 2 shows the estimated effects of highway construction on long-run outcomes for individuals living in different distance bins to the highway. Panel (a) shows the estimated effects on the probability of dying in a different neighborhood than the one individuals were living in 1940. Compared to individuals living in the same city but two kilometers away from highway construction, displaced families are 1.5 percentage points more likely to die in a different neighborhood. I also find that highway construction has localized externalities on individuals living in the neighborhood. Although not directly affected by construction, families living within 100 meters of the highway are also more likely to out-migrate from the neighborhood. The effect, however, fades away quickly, as I find that no coefficient is statistically different from zero for bins located further than 100 meters.

I find that highway construction impacted life expectancy as individuals displaced by construction die at a younger age. I do not find any evidence that construction affected life expectancy for individuals living in the neighborhood. The coefficient for individuals living This finding goes against to previous studies which found that living near highways has negative health effects (Currie and Walker, 2011).

I also find that highway construction had long-term effects on the wealth accumulation of individuals living near highways. Using the information conveyed by the ZIP code of residence at time of death, I find that individuals displaced by construction live in neighborhoods with lower college share and home values at time of their death (Panels (c) and (d)). I find that individuals displaced by construction live in neighborhoods with 2.9 percentage points lower college share and 0.04 log points lower home values. I also find evidence of negative spillovers on individuals living in the neighborhood, as families living within 100 meters of highways also die in *worse* neighborhoods. Consistent with the results on out-migration, I find that the effects of highway construction on neighborhood characteristics fade away quickly, as I find no statistically significant effects for bins located further than 100 meters.

Overall, I find that the construction of the IHS had localized effects on individuals living near highways, which aligns with the previous findings looking at spatial spillovers of urban developments.[19] These results suggest that communities affected by highway construction were not only displaced but also experienced negative spillovers. However, the scope of these effects is limited, which goes against anecdotal accounts of highway construction razing entire neighborhoods (Rose and Mohl, 2012). In the next section, I turn to the effects of highway construction on individuals displaced by construction. Motivated by these results, I focus on individuals living within 200 meters of highway construction.

5.2 Consequences of highway construction for displaced individuals

In this section, I look at the effects of highway construction on individuals displaced by construction. Having documented the highly localized effects of highway construction on individuals living near highways, I will use a "near versus far" strategy to estimate the effects of highway construction on individuals displaced by construction, and on those living right next to a highway. In particular, I will compare individuals displaced by construction and those living within 100 meters, to those living between 100 and 200 meters of highways.[20] For the rest of the paper, I will refer to individuals living within 100 meters of highways as "adjacent" individuals, and those living between 100 and 200 meters as "control."

The empirical strategy relies on the assumption that outcomes for treated and control individuals would have evolved similarly in the absence of highway construction. Although there is no direct test for this assumption, I can indirectly test it by looking at the balance of the sample. For this approach to yield credible results, individuals living

[19] There is a large literature finding that spatial spillovers are highly local. Weiwu (2023) and Moretti and Wheeler (2024) find that spillovers arising from highways are highly localized. There is also evidence that the effects of housing renovations externalities are highly localized (Hornbeck and Keniston, 2017; Rossi-Hansberg et al., 2010).

[20] The strategy has also been used to study spatial spillovers of housing (Diamond and McQuade, 2019; Asquith et al., 2021), bankruptcy (Shoag and Veuger, 2018), startups success (Campusano Garate, 2022), among others.

between 100 and 200 meters of highways should not differ in observable characteristics during the pre-period. I assess sample balance in Table 4, where I examine characteristics from the 1940 census for the sample linked to administrative mortality records. Columns (1), (2), and (3) display the average values for control, displaced, and adjacent individuals. In column (4), I conduct a mean difference test between control and displaced individuals, while column (5) presents the coefficient and corresponding p-value from a regression between the control and displaced groups, controlling for city, birth year, race, gender at birth, and homeownership fixed effects. Columns (6) and (7) provide the same statistics for control and adjacent individuals. These fixed effects are incorporated into the estimating equation to account for characteristics that could influence the long-run outcomes of interest.

I find small differences in the socioeconomic characteristics of treated and control individuals. Both displaced and adjacent individuals lived in houses with lower rent and value, and they were less likely to have completed high school. However, these differences are not large in magnitude. Displaced individuals are also younger, more likely to be Black, and less likely to be homeowners. To account for these differences, I control for these characteristics in the main specification. When examining labor market characteristics and a myriad of other individual characteristic, I find no significant differences. In summary, the samples are highly balanced, and these differences are unlikely to drive the results.[21] In addition, I will use a larger set of controls to account for these differences in the main specification.[22]

The "near-versus-far" design compares the socioeconomic outcomes at time of death of individuals who were displaced by highway construction to those living in close proximity to them in 1940. The estimating equation follows:

$$y_i = \lambda_{c(i,1940)} + \beta_1 \text{Displaced}_i + \beta_2 \text{Adjacent}_i + \mathbf{X'}_i \Gamma + \epsilon_i \tag{4}$$

The sample corresponds to individuals living in 1940 within 200 meters of highway segments opened between 1950 and 1960, linked to administrative mortality records between 1995 and 2005. y_i denotes the dependent variables for individual i at their time of death, such as age at death, survival until age 70, migration, or neighborhood socioeconomic characteristics. When the outcome is the age of death and survival until age 70, I use the weights developed by Goldstein et al. (2023) to account for the differences in inclusion probabilities by period, age, and demographic characteristics.[23] $\lambda_{c(i,1940)}$ corre-

[21] Appendix Table E.6 repeats the exercise using the sample of all displaced individuals in the 1940 census. The results are similar to those presented in this section.

[22] In particular, I will include controls for home value, rent, high school completion, employment, occupational score, and indicators for staying in the same house in the last five years and marriage.

[23] The post-stratification weights are constructed using population totals from the Multiple Cause-of-

sponds to 1940 city of residence fixed effects. $Displaced_i$ is an indicator that equals one if individual i lived in 1940 in a dwelling destroyed by highway construction. Conversely, $Adjacent_i$ is an indicator that equals one if individual i lived in 1940 within 100 meters from the highway. $\mathbf{X'}_i$ denotes a vector of individual-level characteristics that includes an indicator if the reported race of the individuals is Black, a gender indicator, indicator if the individual's household owned the property they lived in 1940, and birth year indicators. Standard errors are clustered at the city in 1940 level.

Table 5 Panel A presents the results of using the "near-versus-far" approach to estimate equation 4. Column 1 reports the estimates where the dependent variable is an indicator that equals one if the individual was living in the same neighborhood in 1940 and at the time of death. I find that both displaced and adjacent individuals are less likely to remain in their neighborhood. Displaced individuals are 1.4 percentage points less likely to stay in the same neighborhood. This can be compared to the overall probability of staying in the same neighborhood, which is 0.021, and suggest that displaced individuals are 67% (0.014/0.021) less likely to stay in the same neighborhood. Construction also affected adjacent individuals, who are 0.4 percentage points less likely to stay in the same neighborhood.

Column 2 reports estimates that provide evidence that displaced individuals are more likely to stay in the same city. The dependent variable is an indicator that equals one if the individual was living in the same city in 1940 and at the time of death. I find that the estimated coefficients are positive in magnitude, but only statistically significant for adjacent individuals. The magnitudes of the coefficients are smaller than the ones for the neighborhood outcome once compared to the sample mean. These estimates, along with the results suggesting that highways were placed in low socioeconomic status neighborhoods, align with previous literature, which finds positive selection in out-of-city migration (Black et al., 2015).

Columns 3 and 4 of Table 5 Panel A show that highway construction had profound effects on individuals' health and wellbeing. In Column 3 the dependent variable is the age at death, and I find that displaced individuals die at a younger age. The estimated coefficient suggests that displaced individuals die 0.228 years earlier. This effect is equivalent to a decrease from the 75th percentile to the 70th percentile in the income distribution (Chetty et al., 2016b).[24] Adjacent individuals also see their life expectancy decrease, but the effect is barely statistically significant. Column 4 shows that highway-induced dis-

Death (MCOD) mortality data. The purpose of the weights is to adjust for slightly worse coverage of younger ages of death within birth cohorts. Individuals born outside the 48 contiguous states and those without a birthplace are excluded from the weighted sample. More details on the construction of the weights can be found on Breen et al. (2023). Appendix Table E.5 shows that the results do not hinge on the use of weights.

[24] When looking at a slum clearance and urban renewal project in Chile, Rojas-Ampuero and Carrera (2024) also find that individuals sent-away from their neighborhoods have lower life expectancy. Duque et al. (2024) also find that free housing programs in Colombia have positive effects on life expectancy.

placement is associated with a significant and sizeable decline in life expectancy.

Controlling for differences in pre characteristics. Table 4 shows that the fixed effects used in the previous section purged most of the differences in the pre-period characteristics of the sample. However, some differences already exists in characteristics such high school completion and home prices, which could influence the long-run evolution of the studied outcomes. In Table 5 Panel B, I estimate equation 4 including log home value, log rent, high school completion, employment, log occupational score, and indicators for staying in the same house in the last five years and marriage as additional controls. By explicitly controlling for observable differences, the estimating equation thus compares invididuals with similar characteristics in 1940 affected by highway construction to those who were not. Relative to Panel A, the results are essentially unchanged. The estimates suggest that individuals displaced by highway construction are more likely to die in a different neighborhood, to stay in the same city, have a lower life expectancy, and are less likely to survive until the age of seventy.

Matching on observable characteristics. Even conditional on observable characteristics, the estimates could be biased if individuals living near highways are also affected by construction, e.g., noise and pollution emanating from the roads. One strategy is to match individuals displaced by and living within 100 meters of highways to individuals living further away. In particular, I restrict the pool of potential controls to individuals living further than two kilometers from the highway construction. I performed exact matching on individual characteristics such as race, gender, city, employment status, high school education, and homeownership status. Additionally, I allowed a caliper of two years for birth year and one standard deviation in household income. Given recent findings on the importance of neighborhood characteristics for long-term outcomes (Chetty et al., 2014), I also included neighborhood-level characteristics in the matching procedure. Specifically, I performed exact matching on the redlining status of the neighborhood in 1940, and a caliper of one standard deviation on the neighborhood's Black population share, employment rate, high school graduation rate, homeownership rate, average household income, home value, rent, and educational mobility.[25,26] The matching procedure is successful in balancing the observable characteristics between the displaced and control groups, lending credibility to the estimates. Appendix Table E.11 compares the average characteristics of the displaced and control groups after matching. More information about the matching procedure and results can be found in Appendix Section E.4 .

[25] The calipers are neighborhood Black share (0.178), employment share (0.052), educational mobility (0.133), high school share (0.196), homeownership rate (0.211), income (397.77), home value (6315.811), rent (241.873). The caliper for household income is 1934.749.

[26] The educational mobility is constructed following Card et al. (2022). In particular, I estimate the fraction of 14 to 18-year-old boys and 14 to 16-year-old girls in each neighborhood with nine or more years of schooling from households where the most educated parent has between 5 and 8 years of schooling. I used the enumeration districts in 1940 as the neighborhood definition because it allowed me to estimate educational mobility without relying on geocoding. Derenoncourt (2022) also uses this definition of educational mobility.

Table 5 Panel C presents the estimates of equation 4 that uses individuals matched on observable characteristics as the control group. I find that, on the whole, the matching estimates concur with those estimated using the near-versus-far strategy, as magnitudes are similar for all the outcomes. Although coefficients are slightly larger, the estimates do not differ in their qualitative findings. Individuals displaced by highway construction are more likely to die in a different neighborhood, to stay in the same city, have a lower life expectancy, and are less likely to survive until the age of thirty.

Planned maps as control group. An additional strategy to address the potential bias of individuals living near highways is to use transportation plans designed by the Federal government as a control group. I digitize plans created by Federal engineers for 100 metro areas in the 1955 *General Location of National System of Interstate Highways* report. I use as control group individuals who would have been displaced by or living within 100 meters of a highway if the Federal plans had been implemented.[27] However, there are two drawbacks to this approach. First, the Federal government only provided maps for 100 cities, where only 42 built highways between 1950 and 1960. Thus, it reduces the sample of cities used in the analysis.[28] Second, the fact that some segments were built and other were not may indicate the presence of ommitted variables that can affect the interpretation of these estimates. Mowitz and Wright (1962) presents anecdotal evidence in favor of this interpretation. When discussing the construction of their highway network, councilman Del Smith, of Detroit City Council, raised the question as to the city was constrained by the routes indicated in the maps. State officials responded that the map were not a legal contract, but rather a guideline. As a consequence, the estimates presented in this section should be interpreted with caution.

Table 5 Panel D presents the estimates of equation 4 using individuals living near planned highways as control group.[29] The estimates closely mirror the findings using the near-versus-far strategy. Column 1 shows that the estimated effects on the probability of living in the same neighborhood at time of death is negative and statistically significant for both displaced and adjacent individuals. Both groups have a positive coefficient on the probability of dying in the same city, but the coefficient is only statistically significant for adjacent individuals. I also find that highway construction caused a decrease in life expectancy and probability of surviving until the age of 70 for individuals displaced by construction, although the magnitude is slightly smaller than previous estimates.

In sum, these four strategies paint a clear picture of the long-term effects of highway

[27] These maps have been used as instrument for neighborhood proximity to interstate highways (Weiwu, 2023; Bagagli, 2023; Brinkman and Lin, 2022). Given the granular geography I use in this paper, these maps are not suitable as instruments.

[28] In contrast, it increased the number of observations because I consider all individuals living in proximity to these maps, which does not restrict the sample to segments opened between 1950 and 1960.

[29] Appendix Table E.12 presents the balance test for the sample of individuals living near planned highways. In general, control individuals are more likely to be homeowners, live in more expensive properties, and have higher educational attainment.

construction on individuals displaced by construction. Individuals displaced by highway construction are more likely to die in a different neighborhood, to stay in the same city, have a lower life expectancy, and are less likely to survive until the age of thirty. As the results are consistent across different strategies, I interpret them as causal and a consequence of the harms caused by highway construction.

5.2.1 Long-run effects of displacement on wealth accumulation

Highway construction may impact the long-term wealth accumulation of individuals in several ways. Individuals displaced by construction may have been forced to move to economically struggling neighborhoods, which could have limited their ability to accumulate wealth (Archer, 2020). Also, by destroying valuable jobs, highway construction may have limited the ability of displaced individuals to secure well-paying jobs (Taylor, 1974). Moreover, by eroding social capital, affected individuals may have lost valuable connections that could impact their long-term economic outcomes. In this section, I explore the long-term effects of highway construction on wealth accumulation.

I extend the results to study the effects of highway construction on long-term wealth accumulation by exploiting the residential information in the linked sample. Administrative mortality records include the nine-digit ZIP code of a person's residence at the time of death, from which I derive neighborhood-level information on employment, wealth, and educational distribution. Using neighborhood-level data from mortality records linked to the 1940 census, I find that, in 2000, individuals displaced by highway construction live in neighborhoods with substantially lower educational attainment than their neighbors in 1940. Table 6 presents the results for the 4 specifications of the model. Because these estimates ignore within-neighborhood differences, they should be considered an underestimate of the actual displacement effect on wealth accumulation.

I find that highway construction has a negative effect on wealth accumulation for affected individuals. Displaced individuals were between 0.96 and 1.54 percentage points less likely to hold a high school degree and 1.54 and 2.14 percentage points less likely to hold a college degree, depending on the specification used. Those living within 100 meters of highways were also affected, although the effect is smaller in magnitude. I also find that displaced individuals lived in placed with higher unemployment rates. The estimated coefficients correspond to a roughly 1% increase in the mean unemployment rate of the neighborhood. When looking at the homeownership share, log income, and log home value I find that displacement is associated with a negative effect. However, the estimates are are only statistically significant when using the matching appoach. Similar results are found when looking at adjacent individuals, the magnitude of the estimate being smaller than for displaced individuals. Overall, the results suggest that displacement due to highway construction had long-lasting negative economic consequences.

The different specifications shown in this section suggest that highway construction removed individuals from their neighborhoods, increased mortality, and decreased long-term wealth accumulation. In the following analysis, I will only focus on individuals living within 200 meters of highways.

5.3 Robustness checks

In this section, I present several robustness checks to assess the validity of the results presented in the previous sections.

5.3.1 Possible SUTVA violation

A possible threat to the results is the potential spillovers of highway construction onto individuals in the control group. Highways alter the characteristics of the neighborhoods in which they are built. They spur consumption and create opportunities (Wang, 2024), potentially affecting individuals living nearby in ways not captured by the analysis in Section 5.1. To address this concern, I use as a control group individuals living further away from the highways. Specifically, I employ three different control groups: individuals living between 1,000 and 1,100 meters, between 2,000 and 2,100 meters, and the rest of the city. It is important to note the trade-off between maintaining the balance of the sample and accounting for possible spillovers. Appendix Tables E.2, E.3, and E.4 show that the results remain robust across these different control groups.

5.3.2 Placebo using planned highways

One possible explanation is that highways were planned and subsequently built in neighborhoods that could impact long-run outcomes. For instance, highways may have been routed through neighborhoods already in decline or through areas with particular economic characteristics, such as low housing quality. If so, the results could be driven by the type of neighborhoods where highways are built and not by highway construction itself. I test this hypothesis by estimating the effect of living close to a planned highway. As mentioned in Section 2, these segments were designed with the goal of minimizing the cost of connecting each city center to the interstate network. If the estimated effects stem from where highways were built, I would expect that the Federal engineering plans would also have an independent effect on individuals' long-term outcomes.

I find that the estimated effects of being displaced by highway construction are not driven by the location of highways. By creating placebo "displaced" and "adjacent" individuals who would have been displaced or lived within 100 meters of a planned highway, I find no significant effect on individuals' long-term outcomes.[30] Appendix Tables

[30] Planned highways consist of a unidirectional segment of the highway. I assume that planned high-

E.13 and E.14 present the placebo test for the main outcomes. The coefficients for treatment groups, displaced and adjacent, are close to zero and not statistically significant. I interpret the absence of these patterns as evidence against the hypothesis that the location of highway plans drives the results. An in-depth discussion of the placebo test can be found in Appendix Section E.6.

5.3.3 Outmigration of the dwelling before construction

One concern with is that individuals living near highways could have moved out of their dwellings before construction began. In this section I tackle this concern in two different ways. First, I create bounds for the *true* parameter as a function of the estimated coefficient. Second, I perform several analysis to assess the robustness of the results to the outmigration of individuals before construction began.

Equation 4 estimates an *intention-to-treat* effect because it proxies individuals' locations when construction occurred with their location in 1940. The *true* (b) coefficient for displaced individuals is therefore bounded by:

$$b \in \left[\hat{\beta}, \frac{\hat{\beta}}{(\mathbb{P}(D_i^{56} = 1|D_i^{40} = 1) - \mathbb{P}(D_i^{56} = 1|D_i^{40} = 0))}\right]$$

where $\hat{\beta}$ is the estimated effect of highway construction on individuals displaced by construction, $\mathbb{P}(D_i^{56} = 1|D_i^{40} = 1)$ is the probability of being displaced by construction given that the individual lived in the highway location in 1940, and $\mathbb{P}(D_i^{56} = 1|D_i^{40} = 0)$ is the probability of being displaced by construction given that the individual did not live in the highway location in 1940. I estimate the probabilities by geocoding and linking the 1930 and 1940 census to the highway construction data. I find that the *true* coefficient is bounded between $[\hat{\beta}, 4.2\hat{\beta}]$. For example, the *true* effect on mortality is bounded between $[-0.228, -0.957]$. The complete derivation can be found in Appendix Section E.8.

I also perform an array of robustness checks to assess the sensitivity of the results to the outmigration of individuals before construction began. In particular, I re-estimate equation 4 with three different samples that, I argue, have a higher likelihood to remain in the same property. More details can be found in Appendix Section E.8.1. First, I estimate the probability of staying in the same house by age between census waves using linked censuses. I then restrict the sample to individuals who were "more likely to stay in their property" based on their age. The results are presented in Appendix Table E.16 and paint a similar picture to the main results. Second, in Appendix Table E.17 I use individuals who were 47 years or older in 1940, as they are less likely to move. Again, the results remain unchanged. Finally, I use the newly develped linkage between the 1940

ways had traffic in both directions and had four lanes in each direction, with each lane measuring 3.65 meters in width, and a 5 meters berm.

and 1950 census done by Ruggles et al. (2020). I then keep those linked individuals who were residing in the same state and county as in 1940.[31] This exercise leaves the results virtually unaffected as shown in Appendix Table E.18.

5.3.4 Potential contamination bias in the estimation

So far, I have estimated the causal effects of highway construction on individuals displaced by or living in close proximity to highways. Recent studies have highlighted that settings involving multiple treatments and flexible controls, like the one I estimate, may suffer from contamination bias (Goldsmith-Pinkham et al., 2024). The intuition behind this issue is that in contexts with mutually exclusive treatment indicators, conditioning on covariates is insufficient to render other treatments ignorable, "contaminating" the results. I address this concern employing Goldsmith-Pinkham et al.'s 2024 approach. Appendix Table E.15 presents the estimates for the own-treatment effect of highway construction for each treatment and empirical specification. These effects correspond to the weighted average of conditional average treatment effects for each treatment, as detailed in Goldsmith-Pinkham et al. (2024). I find that while contamination bias is present, it does not substantially alter the conclusions drawn from my estimates. For example, in Panel A, the estimated effect on mortality for displaced individuals changes only slightly, from -0.228 to -0.234. The results remain robust across different specifications and samples used in the analysis.

5.4 Displacement and the Probability of Survival at Any Age

I re-examine the mortality results and estimate the effect of highway construction on the likelihood of survival at any given age. For this analysis, I use the same sample of individuals living within 200 meters of a highway, and estimate a Cox Proportional Hazard Model of the effect of highway construction on the hazard rate of dying. The results are reported in Appendix Table E.8 and they correspond to the hazard rate.[32] The estimates suggest that displaced individuals have a 7.5% higher risk of early death over the study period than their peers. I find no significant effect for individuals living close to future highway construction. Appendix Section E.3 provides more details about the duration analysis and the results.

The hazard rate estimates for displacement are significant and align with those found for other traumatic life events. The results of the duration analysis suggest that the effect of displacement on mortality is equivalent to 23.5% of the impact of losing a child, 32.6% of the impact of a divorce, and 13.4% of the impact of homelessness (Song et al., 2019;

[31] This is the most granular geographic information available in the 1950 census so far.

[32] A hazard rate of 1.1 indicates a 10 percent higher probability of dying at that age compared to the reference group.

Sbarra et al., 2011; Meyer et al., 2023). These findings underscore the significant toll that highway-induced displacement had on the affected individuals.

5.5 Heterogenous Effects

I study how the effects of highway construction on mortality vary by groups. To ensure comparability across coefficients, I standardize the estimates by the mean value of the outcome variable for each respective group. Consequently, the resulting estimates can be interpreted as percentage changes from the group mean.

5.5.1 Heterogeneous effects by pre-construction characteristics

Highway construction offered no relocation aid to displaced renters, though homeowners were compensated at market value for their properties. Out of the displaced individuals, only 20% were homeowners. Moreover, highway construction disproportionately impacted Black individuals during a time of peak racial segregation (Cutler et al., 1999). 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.[33] I show how homeownership, race, and institutional barriers interacted with highway construction to shape the long-term outcomes of displaced individuals. I draw upon redlining maps from the Home Owners' Loan Corporation (HOLC), which evaluated the credit risk of neighborhoods, to explore the importance of institutionalized barriers.[34]

Figures 3, 4, and 5 present the results of these exercises. In all figures, Panel (a) displays the results for the probability of living in the same neighborhood at the time of death, Panel (b) shows the results for the death age, and Panel (c) focuses on the college share of the neighborhood of the individual's last residence. The coefficient for displaced individuals is represented in orange, while the coefficient for adjacent individuals is shown in blue. The estimated difference between displaced and non-displaced individuals is the center line in each boxplot. The top and bottom of each box represent effects that are one standard error above and below the point estimate. The whiskers represent the 95% confidence interval.

There are three main findings from this analysis. First, Black individuals, whether displaced or adjacent, are not more likely to outmigrate from their neighborhoods. This

[33] Additionally, discrimination in labor markets based on the neighborhood of residence may have further constrained displaced individuals from securing better jobs (Angeli et al., 2024).

[34] The HOLC maps assigned grades to residential neighborhoods that reflected their "mortgage security" that would then be reflected in their associated color. Those neighborhoods receiving the lowest grade, "D", were colored red and were considered the riskiest for mortgage lenders (Nelson et al., 2023). There is a large literature studying the consequences of these maps, particularly for racial minorities (Aaronson et al., 2021; Fishback et al., 2021; Hynsjö and Perdoni, 2022).

pattern suggests that limited mobility options may have constrained the ability of Black individuals to move to better neighborhoods displaced. Second, point estimates for death age are always negative and significative, except for homeowners which hints that financial compensation may play a role in ameliorating the negative long-run consequences. Finally, the results suggest that displaced renters and white individuals are more likely to die in neighborhoods with lower educational attainment.

Other subgroups. I explore the role of different characteristics in shaping the effects of highway construction. First, I examine whether gender plays a role in influencing these effects. Next, I investigate whether human capital mitigates the negative impact of highway construction by comparing individuals from households with varying education levels.[35] I also analyze the importance of regional differences, particularly how institutionalized oppression in the American South may affect the outcomes of highway construction.[36] Finally, I explore whether family ties can alleviate the consequences of displacement, using an index based on the commonality of last names in 1940 as a proxy for family ties (Schwank, 2023; Ghosh et al., 2024).[37]

Figure 6 highlights five key findings from the subgroup analysis. First, displacement significantly reduces the likelihood of staying in the same neighborhood across all groups, with the effect being strongest in Southern states, likely due to the overlap with the Great Migration (Wilkerson, 2020). Second, the mortality effect is consistent across subgroups, except for those displaced in the South, where migration to the North may have mitigated the impact. Third, human capital influence wealth accumulation, as there is no effect for individuals from households with high school education on neighborhood outcomes. Fourth, individuals with strong family ties are just as likely to leave the neighborhood and tend to die younger in less educated neighborhoods. Lastly, there are no significant heterogeneous effects for individuals living near highways.

5.5.2 Heterogeneous effects by birth cohort

The consequences of highway construction likely varied across cohorts born in different periods. Research has consistently shown that the benefits of moving-to-opportunity are larger for children than for adults (Chetty et al., 2016a; Chyn, 2018). Moreover, individuals in later stages of their life cycle, who tend to exhibit lower mobility, may have been disproportionately affected by the loss of social networks and the disintegration of their communities. In this section, I test these hypotheses by estimating the effects of highway construction on individuals born in different periods.

[35] I use high school completion as a proxy for human capital. In 1960, 53% of white adults and 32% of Black adults had completed high school (Weiwu, 2023).

[36] I use Derenoncourt's 2022 definition of the American South, which includes the states of Alabama, Arkansas, Florida, Georgia, Kentucky, Louisiana, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia.

[37] Appendix Section C.6 provides more details about the construction of the index.

Figure 7 presents the results for different birth cohorts. Given that the sample consists of individuals in the 1940 census linked to administrative mortality records from 1995 to 2005, birth cohorts are divided into four broad groups: individuals born before 1910, between 1910 and 1920, between 1920 and 1930, and after 1930. The analysis reveals that displaced individuals are more likely to leave their neighborhoods, particularly those displaced at older ages, while no significant effect is found for individuals living adjacent to highways. Displacement also negatively impacts life expectancy, with the largest reductions observed among those displaced at an older age, and younger individuals experiencing a smaller, yet still significant, decrease. Additionally, displaced individuals tend to die in neighborhoods with lower educational attainment, especially those born after 1930, who die in areas with a 7.5% lower college share. Overall, the findings highlight that forced relocation due to highway construction disproportionately affects individuals at both ends of the age spectrum, with varied impacts on mobility, mortality, and neighborhood quality.

6. Underlying Mechanisms

In this section, I explore the underlying mechanisms that explain the long-term effects of highway construction on mortality. I decompose the mortality-displacement effect into a set of indirect effects, operating through observed variables such as the neighborhood where individuals moved, their access to healthcare and jobs, and their social networks. I find that the destination neighborhood explains thirty percent of the long-term effects of highway construction on mortality, with the other channels having moderate, and negative, influence on the estimates.

6.1 Decomposition of the displacement-mortality relationship

Relocation after highway construction disrupted social networks, affected economic opportunities, and changed healthcare access. To measure the extent to which each factor contributes to the overall effect of displacement on mortality, I follow the approach developed by Gelbach (2016).[38] Intuitively, this approach treats each potential channel as an "omitted variable" in the relationship between displacement and mortality, estimating the bias that would result from excluding each factor. This method allows me to disentangle the impact of each factor on the mortality estimates, independent of the order in which they were sequentially added in the previous analysis.[39]

[38] Gelbach's 2016 decomposition has been applied in various fields, including the study of the nutrition-income relationship (Allcott et al., 2019), the GDP-speed relationship (Akbar et al., 2023), the gender pay gap in the gig economy (Cook et al., 2020), and partisan support for taxation in the U.S. (Stantcheva, 2021).

[39] See Appendix Section E.9 for more details on the decomposition.

To proxy for these channels, I include neighborhood of residence in 2000 fixed effects, log distance between residences, proximity to hospitals, and changes in unemployment rates and distance to city centers.[40,41] Table 7 presents the results of incorporating the variables into equation 4. The results indicate that individuals who moved further from their original home, moved further from the city center in 2000, and were located further from a hospital tend to have somewhat lower mortality rates. Conversely, individuals who moved to neighborhoods with higher unemployment rates exhibit higher mortality rates. Despite these adjustments, the effect of displacement remains stable across all specifications, except when accounting for the characteristics of the destination neighborhood. When all variables are included, these channels account for 26% of the overall effect of displacement on mortality. However, a potential concern is that these variables are correlated with each other, which makes identifying the individuals contribution to the overall effect of displacement challenging.

Figure 8 presents the Gelbach decomposition of the displacement-mortality relationship. Each bar represents the share of the displacement-mortality estimate explained by a given factor.[42] Three key patterns emerge from this decomposition. First, individuals displaced by highway construction die younger primarily because they relocated to *worse* neighborhoods. The contribution of the destination neighborhood accounts for 29% of the overall effect of displacement on mortality. In other words, the type of neighborhood where individuals moved after highway construction is the most significant factor in explaining the long-term effects of displacement. This finding aligns with the literature on the causal effects of neighborhoods on long-term outcomes (Chyn, 2018). Second, the contributions of the other factors are moderate and negative. For instance, the analysis suggests that, holding all other variables constant, the displacement-mortality effect would be approximately 0.36% larger if unemployment rates were the same for displaced and control individuals. Finally, the unexplained effect of displacement on mortality remains substantial, accounting for roughly 70% of the overall effect.

Additionally, in Appendix Table E.21 I explore three possible mechanisms that could explain the displacement-mortality estimates. First, the loss of social capital may have worsened outcomes for displaced individuals, with studies noting feelings of loss and loneliness. Using Facebook's Social Connectedness Index, I find that displaced individuals die in neighborhoods with higher social capital, suggesting they could rebuild connections over time. Second, I test whether displacement increases the risk of eviction,

[40] Individuals in the sample lived in more than 9,000 census tracts at time of death, many of which include only one individual. I address this concern by using census tracts with at least three individuals in the sample. Individuals not meeting this criterion are grouped by county-state, resulting in approximately 4,300 neighborhoods.

[41] Individuals relocated to places with worse health outcomes, as shown in Appendix Table E.20. Hospital location corresponds to ESRI's "Hospitals Registered with Medicare" shapefile.

[42] Appendix Table E.19 provides the estimated $\hat{\delta}$ for each of the factors.

which can raise mortality rates.[43] Although the risk is higher for displaced individuals, the result is not statistically significant. Third, I examine racial segregation as a factor contributing to poor outcomes, finding higher segregation levels in neighborhoods where displaced individuals died. These results suggest that only racial segregation may partly explain the observed effects.

7. Policy Implications of the Results

In this section I discuss how the previous results relate to public policy discussions surrounding highway construction in the U.S. In particular, I discuss their implications to the rising infrastructure costs in the U.S. and to the *Reconnecting Communities and Neighborhoods* project aimed at reconnecting neighborhoods divided by highways.

7.1 Rising Infrastructure Costs

There is growing consensus that construction costs in the U.S. have been rising over the past decades. Brooks and Liscow (2023) document that the real cost of constructing a highway kilometer more than tripled between the 1960s and the 1980s, driven by rising housing prices and increased "citizen voice" in government decision-making.[44] Appendix Figure A.5 shows the time-series of the real cost of constructing a highway kilometer in the U.S. from 1957 to 1993. The figure shows a steady increase in the cost of highway construction starting at the end of the 1960s and continuing through the 1980s. There has also been speculation that the declining productivity in U.S. infrastructure construction, highlighted by high-profile and over-budget projects, may be contributing to these rising costs (Long, 2017).[45] However, these estimates are misleading because they fail to account for the highway-induced displacement costs imposed on individuals.

To understand the evolution and magnitude of infrastructrure costs, I account for the costs imposed on displaced individuals. Early highway construction projects offer a unique setting for that end, as they displaced thousands of individuals without providing any compensation. Notably, the rise in per kilometer construction costs, as shown in Appendix Figure A.5, coincides with the passage of the Uniform Relocation Assistance Act. In this analysis, I use the reduced-form estimate from Table 5, Panel A, column 3.

[43] Gromis et al. (2022) collect and aggregate eviction data, which is accessible through the Eviction Lab's website. I use eviction filings and threats per 100,000 inhabitants for each neighborhood from the year 2000 to 2005 as measures for the risk of eviction.

[44] Many policymakers have partially attributed these rising costs to the extensive bureaucracy that new projects now face. In an effort to counter this tendency, the Biden administration has relaxed federal environmental reviews to facilitate semiconductor manufacturing in the U.S. (Ngo, 2024).

[45] At the same time, user costs for highways decreased by half between 1994 and 2008 (Mehrotra et al., 2024). Since the focus of this paper is to estimate highway construction costs, I do not explore the benefits of highway construction in depth.

These estimates capture the impact of displacement relative to the counterfactual of individuals continuing to live in their original neighborhood without being displaced. The results suggest that highway-induced displacement reduced life expectancy by an average of 0.228 years. It is important to note that this is likely an underestimation of the total cost, as the analysis focuses only on residents and does not consider the potential costs imposed on businesses.

To give a monetary value to a year of life, I use the estimated value of life elaborated by the U.S. Department of Transportation, which was $13.2 million per person in 2023 (U.S. Department of Transportation, 2023a). To transform this value into a yearly cost, I divide it by the life expectancy in 2023, which was 77.5 (Kochanek et al., 2024).[46] Based on these assumptions, the monetary cost of highway-induced displacement is $38,833 per individual. Thus, in the 1960s, including the cost of displacement would increase the cost of building a highway by $895,788 per kilometer, roughly a 20% increase in the construction cost.[47]

Overall, this accounting suggests that internalizing the cost borne by displaced individuals increases highway construction costs by 20% by kilometer built. The large monetary cost of highway-induced displacement increases the potential for large returns on investment for policies that aim to reduce these costs by compensating affected individuals. The estimated cost per person, $38,833, is larger than the maximum compensation given today to individuals displaced by highway construction, which is $9,570 per household (U.S. Department of Transportation, 2023b). Given that the neighborhood to which displaced individuals relocate explains thirty percent of the displacement-mortality effect, policies like "Moving-to-Opportunity" has the potential to reduce the costs of highway construction by mitigating the negative effects of displacement on health. Yet, it is important to recognize that these cost-benefit calculations ignore any potential spillovers to residents where displaced households move.[48]

7.2 The *Reconnecting Communities and Neighborhoods* Project

The Inflation Reduction Act of 2021 allocated over $3 billion to support neighborhoods and communities harmed by infrastructure provision, through a program known as the Reconnecting Communities and Neighborhoods project. Through this initiative, neighborhood residents can apply for federal grants to convert aging infrastructure into com-

[46] Using the life expectancy of 1940, 62.5, would yield higher estimates. I use the life expectancy of 2023 to be consistent with the value of a statistical life.

[47] Note that these costs do not include moving costs for displaced families. I find that 35,686 families were displaced between 1956 and 1960. Once the government started compensating for these costs in 1965, the average compensation was roughly $1,000 per family (in 2023 dollars), which would add an additional $35.7 million to the cost of building highways.

[48] For example, Weiwu (2023) and Valenzuela-Casasempere (2024) find sizable spillovers from highway construction on neighborhoods in close proximity to affected communities.

munity amenities. Place-based policies like this have the potential to repurpose infrastructure and highways for the benefit of current and future residents, helping to mitigate the long-term effects of living near a highway.

Nevertheless, policies targeting neighborhoods may not benefit individuals who relocated. The results in Table 5, Panel A, column 1, indicate that nearly all individuals displaced by construction, and roughly twenty percent of those living near future highways, outmigrated from these neighborhoods. Therefore, a different policy approach would compensate displaced individuals and their descendants. This approach would focus on those who bore the costs of of relocating due to highway construction, rather than those who were affected from living in neighborhoods impacted by it. However, this policy may not always be feasible due to the lack of documentation on displaced individuals. As previously mentioned, the federal government did not track displaced individuals until 1965, making it difficult to identify those affected by earlier highway projects. Additionally, determining appropriate compensation amounts for affected individuals could present another significant challenge.

Overall, two distinct approaches can be taken to address the harms caused by highway construction. Both approaches have their pros and cons: neighborhood-based policies can create community-wide improvements, while individual compensation addresses the specific wrongs suffered by those displaced. This project remains agnostic about which policy is the most effective. Whichever approach is taken, it is important to recognize the costs imposed on individuals to avoid perpetuating the inequalities that highway construction initially created.

8. Conclusions

The construction of the Interstate Highway System in the United States was one of the most significant infrastructure projects of the twentieth century. In its early years, more than 33,000 individuals were displaced annually, receiving no assistance from authorities. The impact of highway construction was not random. I find that it disproportionately affected racial and socioeconomic minorities. Understanding the consequences of highway construction on these individuals and communities is crucial for informing the design of future infrastructure projects.

This paper provides the first evidence on the long-run causal impacts of highway construction on residents of affected neighborhoods. To achieve this, I develop a novel method for identifying individuals who lived in houses destroyed by highway construction using historical census data. I then link these individuals to administrative mortality records from 1995 to 2005. This approach enables me to observe affected individuals both before and after highway construction, offering a unique opportunity to estimate the long-term effects of displacement.

I find that the effect of highway construction on individuals diminishes quickly with distance, with no discernible impact on various outcomes beyond 100 meters. When examining the effects of displacement, I find that individuals displaced by highway construction are more likely to outmigrate from their neighborhoods, live in areas with lower socioeconomic status, and die at a younger age. There is also evidence of spillover effects on individuals living close to the highway, who are similarly more likely to outmigrate and move to neighborhoods with lower socioeconomic status. The results remain robust across alternative specifications and identification strategies. Decomposing the effect of displacement on mortality, I find that relocation to *worse* neighborhoods accounts for 30% of the estimated impact. Other potential mechanisms discussed in the urban affairs literature, such as the loss of social capital, job opportunities, and healthcare access, play a minimal role in explaining the estimated effect.

My findings are crucial for the design of future infrastructure projects and policies aimed at compensating for the injustices caused by highway construction. First, the results highlight the significant costs imposed on individuals displaced by highway construction. A back-of-the-envelope calculation suggests that the cost of displacement is approximately $38,833 per individual. When these costs are internalized, the overall cost of building a highway increases by 20% per kilometer. This implies that policies designed to reduce the social costs of highway construction by compensating displaced individuals could yield substantial returns on investment. Second, the results indicate that policies focused on mitigating the long-term effects of living near highways by targeting neighborhoods may not adequately benefit displaced individuals. I find that nearly all displaced individuals, as well as 20% of families living near highways, had outmigrated from their neighborhoods by the time of their death. Overlooking these individuals in the design of compensation policies may exacerbate the long-term consequences of highway construction.

This paper has limitations that future research may be able to address. First, due to data constraints, the analysis focuses primarily 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 highway construction, particularly in understanding how forced relocation influences labor market outcomes. Second, future research should consider the general equilibrium effects of relocating large numbers of individuals on the communities that receive them.

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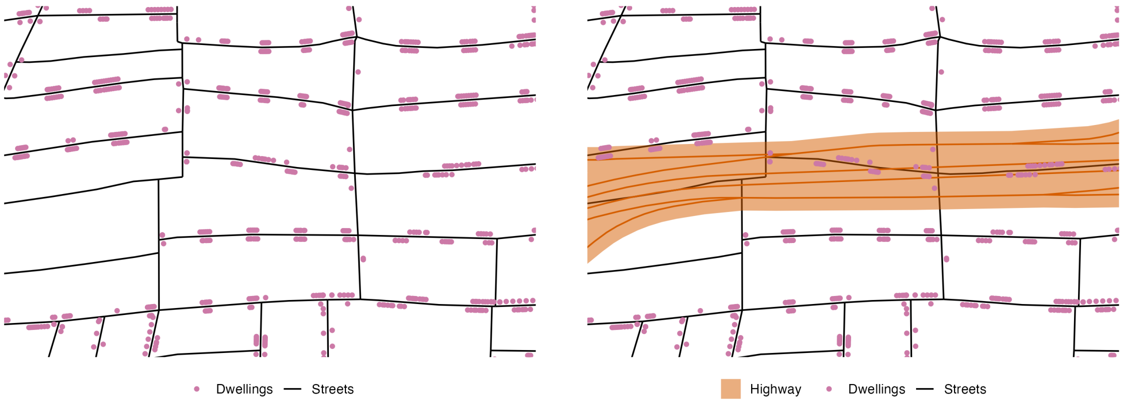
FIGURES

FIGURE 1: Geocoded Dwellings and Highways

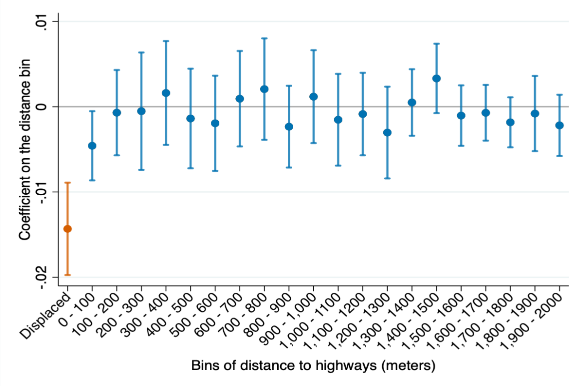


(A) Dwelling locations

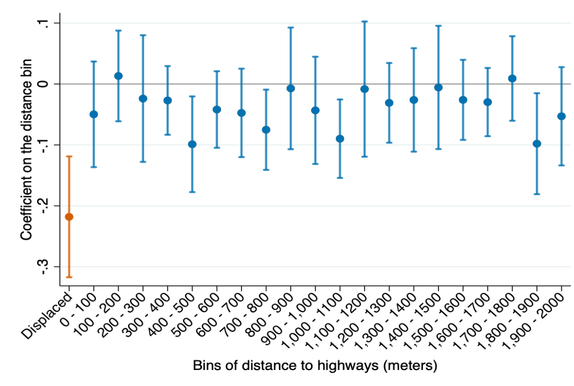
(B) Highway construction

Note: Panel (a) presents an example of geocoded dwellings in 1940 following the approach described in Section 3. Panel (b) presents the same dwellings and the highway construction in the sample.

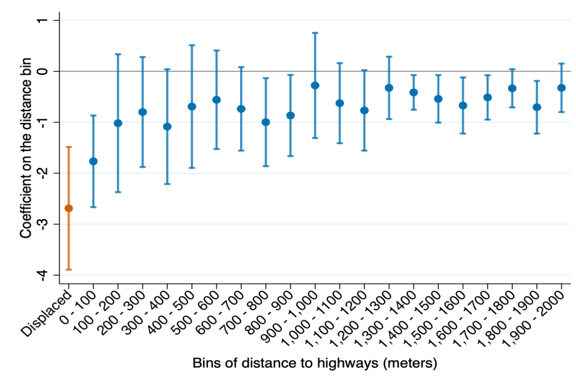
FIGURE 2: Effects for Affected Neighborhoods



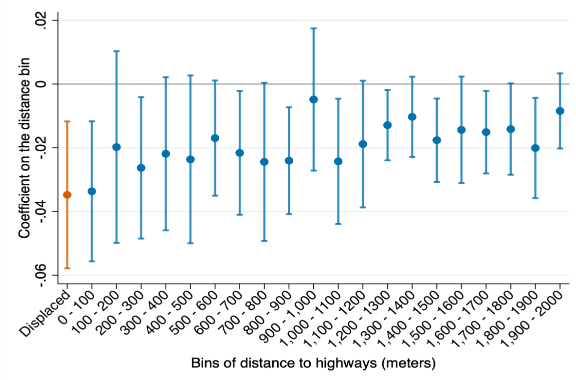
(A) Same neighborhood



(B) Death age



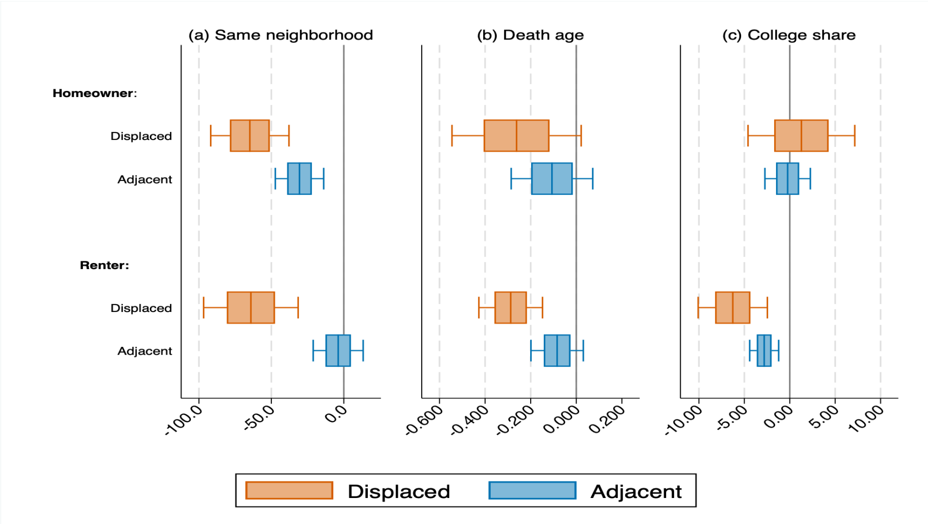
(C) College share



(D) Log Median home value

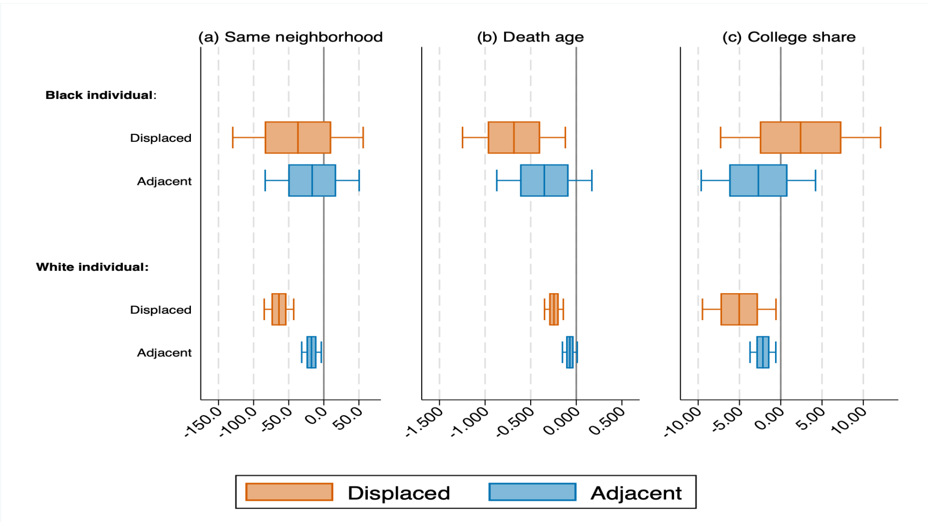
Note: These figures present the estimates of equation 3. The sample corresponds to individuals living in 1940 within 2,100 meters of highway segments opened between 1950 and 1960, linked to administrative mortality records between 1994 and 2005. Each panel presents the coefficients for a different dependent variable. Panel (a) uses an indicator of living in the same neighborhood as dependent variable. Panel (b) uses the death age as dependent variable. Panel (c) uses the college share of the neighborhood of residence at time of death as dependent variable. Panel (d) uses the log median home value of the neighborhood of residence at time of death as dependent variable. Each coefficient corresponds to a different bin of distance to the closest highway segment. The base coefficient corresponds to the bin of distance 2,000 and 2,100 meters. All panels control for race, gender, homeownership, city, and birth year fixed effects. Standard errors are clustered at the city level.

FIGURE 3: Heterogeneity by Homeownership



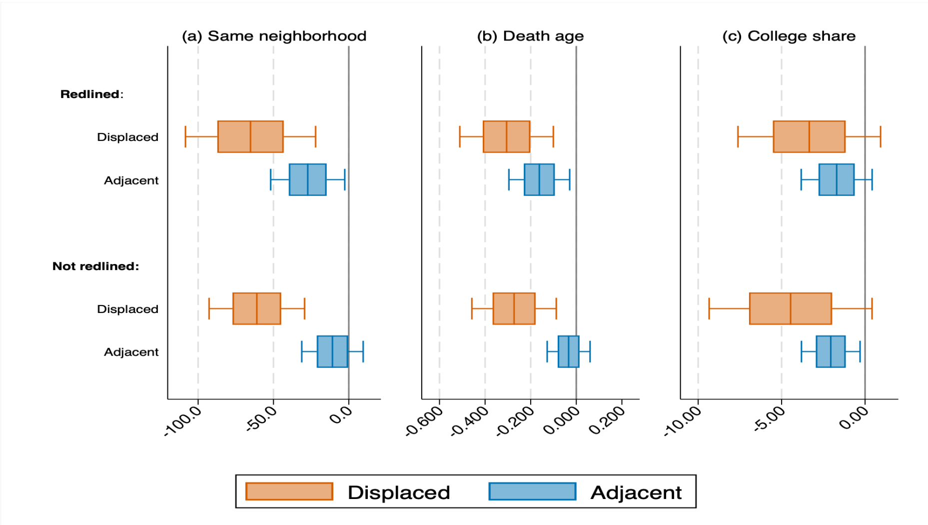
Note: Heterogeneity by homeownership status in 1940. The sample only uses highway segments opened between 1950 and 1960 are included in the sample. Each panel presents the coefficients of a different dependent variable. Coefficient for displaced individuals is presented in orange, and for adjacent individuals in blue. Only coefficients for adjacent individuals are presented. Panel (a) uses an indicator of living in the same neighborhood as dependent variable. Panel (b) uses the death age as dependent variable. Panel (c) uses the college share as dependent variable. Each row is a different regression with the heterogeneity corresponding to the variable in the y-axis. All panels control for race, gender, homeownership, city, and birth year fixed effects. Standard errors are clustered at the city level.

FIGURE 4: Heterogeneity by Race



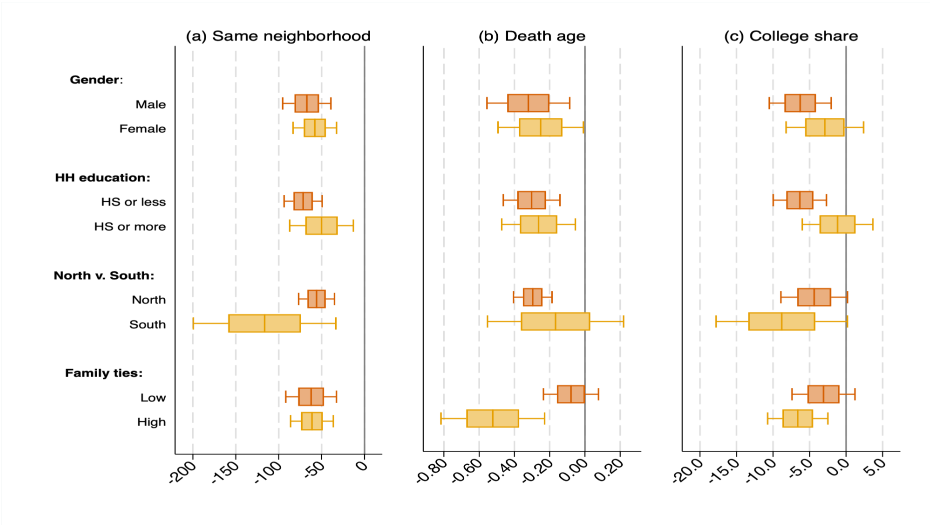
Note: Heterogeneity by race. The sample only uses highway segments opened between 1950 and 1960 are included in the sample. Each panel presents the coefficients of a different dependent variable. Coefficient for displaced individuals is presented in orange, and for adjacent individuals in blue. Only coefficients for adjacent individuals are presented. Panel (a) uses an indicator of living in the same neighborhood as dependent variable. Panel (b) uses the death age as dependent variable. Panel (c) uses the college share as dependent variable. Each row is a different regression with the heterogeneity corresponding to the variable in the y-axis. All panels control for race, gender, homeownership, city, and birth year fixed effects. Standard errors are clustered at the city level.

FIGURE 5: Heterogeneity by Exclusionary Institutions

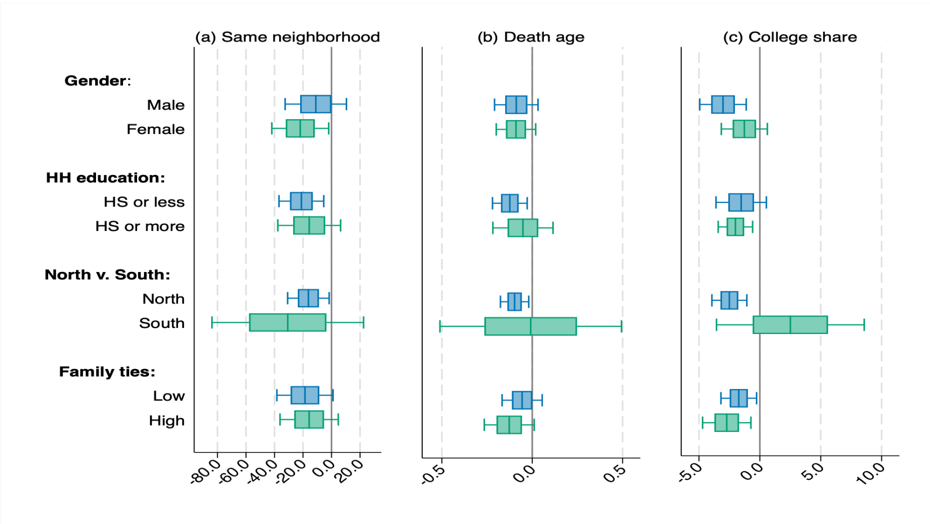


Note: Heterogeneity by households living in redlined neighborhoods in 1940. The sample only uses highway segments opened between 1950 and 1960 are included in the sample. Each panel presents the coefficients of a different dependent variable. Coefficient for displaced individuals is presented in orange, and for adjacent individuals in blue. Only coefficients for adjacent individuals are presented. Panel (a) uses an indicator of living in the same neighborhood as dependent variable. Panel (b) uses the death age as dependent variable. Panel (c) uses the college share as dependent variable. Each row is a different regression with the heterogeneity corresponding to the variable in the y-axis. All panels control for race, gender, homeownership, city, and birth year fixed effects. Standard errors are clustered at the city level.

FIGURE 6: Heterogeneity by Subgroups



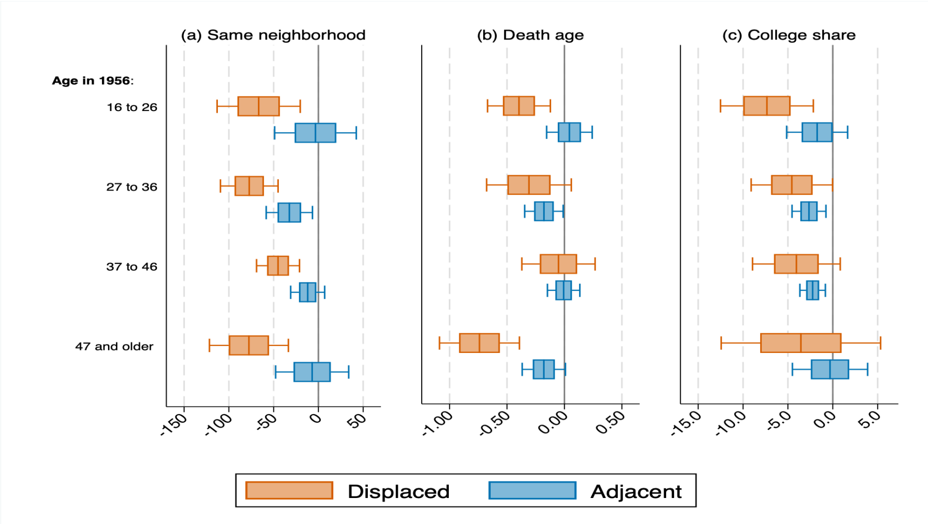
(A) Displaced individuals



(B) Adjacent individuals

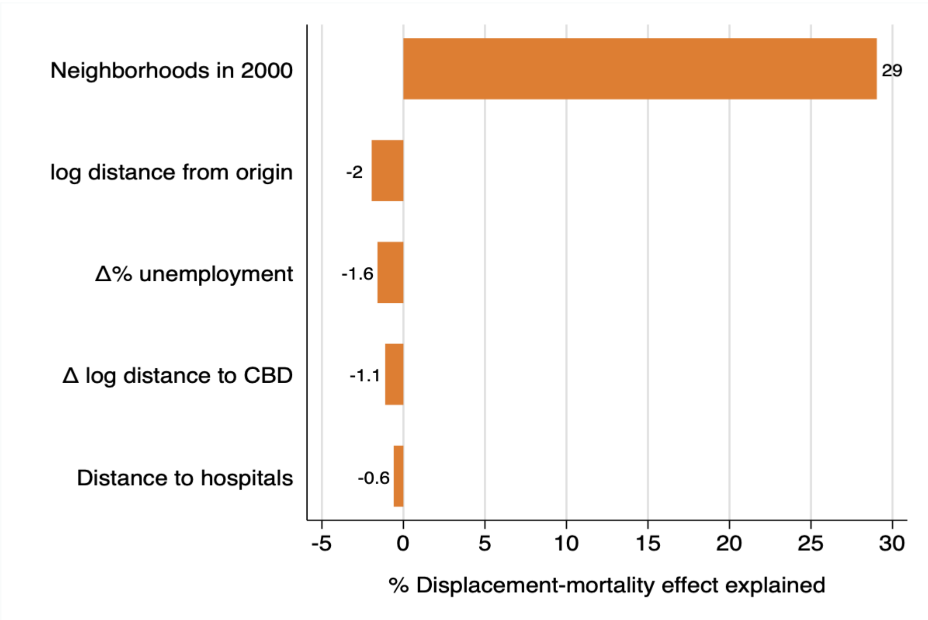
Note: Heterogeneity by different individual characteristics. The sample only uses highway segments opened between 1950 and 1960 are included in the sample. Each panel presents the coefficients of a different dependent variable. Coefficient for displaced individuals is presented in orange, and for adjacent individuals in blue. Only coefficients for adjacent individuals are presented. Panel (a) uses an indicator of living in the same neighborhood as dependent variable. Panel (b) uses the death age as dependent variable. Panel (c) uses the college share as dependent variable. Each row is a different regression with the heterogeneity corresponding to the variable in the y-axis. All panels control for race, gender, homeownership, city, and birth year fixed effects. Standard errors are clustered at the city level.

FIGURE 7: Effects Across the Life-Cycle



Note: Highway segments opened between 1950 and 1960 are included in the sample. Each panel presents the coefficients of a different dependent variable. Panel (a) uses an indicator of living in the same neighborhood as dependent variable. Panel (b) uses the death age as dependent variable. Panel (c) uses the college share as dependent variable. Each row presents the estimates for the corresponding cohort in the y-axis. All panels control for race, gender, homeownership, city, and birth year fixed effects. Standard errors are clustered at the city level.

FIGURE 8: Gelbach Decomposition



Note: The figure uses the effect on mortality estimated in Panel A of Table 5. This estimate corresponds to equation 4 using individuals living between 100 and 200 meters as control. The figure use the method described in Gelbach (2016) to plot the share of the displacement-mortality effect that can be explained by each factor considered: destination neighborhood, distance between residences, difference in unemployment rates, difference in distance to CBD, and distance to hospital.

TABLES

TABLE 1: Short-run Consequences of Displacement

	Public housing	N. of rooms	Sewer connection	Home value	Monthly rent
	(1)	(2)	(3)	(4)	(5)
Displaced	0.097[b]	-0.229	0.108[a]	-8.436[a]	-0.018
	(0.046)	(0.163)	(0.032)	(2.659)	(0.011)
Previous ownership	-0.001	0.837[a]	-0.132[a]	7.178[a]	0.005[c]
	(0.007)	(0.036)	(0.009)	(0.547)	(0.003)
Observations	8,652	14,798	14,798	4,204	4,326
R^2	0.0265	0.0546	0.0392	0.1382	0.0991
Mean dep. var.	0.051	4.805	0.722	35.175	0.120

Note: OLS estimates are reported. Each column corresponds to a different regression. The sample consists of individuals who moved residences in the last twelve months in the 1973 and 1974 American Housing Survey. Displaced corresponds to individuals that moved because their house was destroyed by highway construction. The dependent variable is denoted on top of each column. Home value and monthly rent are expressed in thousands of 1974 dollars. Previous ownership is an indicator that equals one if the household owned the previous residence. All specifications include Race fixed effects and a quadratic in age. Robust standard errors are reported in parentheses. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

TABLE 2: Determinants of Highway Placement

	Dependent Variable: Indicator if the tract is crossed by					
	Built highway					Plan
	(1)	(2)	(3)	(4)	(5)	(6)
Black share	1.179[a]	0.996[a]	1.009[a]	0.942[a]	0.902[a]	0.137
	(0.314)	(0.306)	(0.302)	(0.299)	(0.271)	(0.239)
(log) Median income	-0.028[b]	-0.016[c]	-0.009	-0.014	-0.012	-0.007
	(0.012)	(0.009)	(0.016)	(0.013)	(0.010)	(0.011)
High school share	-0.003[b]	-0.001	-0.001	-0.000	0.000	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
(log) Median rent		-0.079[a]	-0.064[a]	-0.061[a]	-0.060[a]	-0.005
		(0.025)	(0.022)	(0.021)	(0.018)	(0.035)
(log) Median home value		-0.094[a]	-0.120[a]	-0.112[a]	-0.085[a]	-0.093[a]
		(0.030)	(0.023)	(0.023)	(0.022)	(0.021)
Distance to city center				-0.008[a]	-0.006[a]	-0.008[a]
				(0.001)	(0.001)	(0.002)
Highway planned					0.290[a]	
					(0.042)	
Mean dependent var.	0.214	0.217	0.218	0.218	0.218	0.218
Geo. Controls	No	No	Yes	Yes	Yes	Yes
Obs.	18,242	17,210	16,944	16,944	16,944	16,944
R^2 (Adj.)	0.086	0.094	0.155	0.169	0.239	0.115

Note: Each column corresponds to a different regression. The unit of observation is a census tract. The sample consists of census tracts in the 62 metropolitan areas with spatial information in 1950. The dependent variable is an indicator if a highway was built or planned through the tract. The vector of controls includes 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. All columns include city fixed effects. Coefficients are reported with standard errors clustered at the city level. [a] indicates the coef. is significant at the 1%, [b] at the 5%, and [c] at the 10% level. Regressions are weighted by the census tract's population.

TABLE 3: Characteristics of Displaced Individuals

	Displaced	Distance to the closest highway					Rest of the city
		0 and 100m	100 and 200m	200 and 300m	300 and 400m	400 and 500m	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Number of displaced individuals:							
Total population	131,486	882,590	808,629	844,234	855,238	857,239	40.434m
Population share	0.29%	1.95%	1.79%	1.87%	1.89%	1.90%	89.53%
Panel B: Individual level characteristics:							
Black	0.132	0.098	0.097	0.103	0.095	0.093	0.076
	(0.338)	(0.297)	(0.296)	(0.303)	(0.293)	(0.290)	(0.265)
Immigrant	0.166	0.173	0.175	0.178	0.178	0.178	0.146
	(0.372)	(0.378)	(0.380)	(0.382)	(0.382)	(0.383)	(0.353)
First gen. US born	0.161	0.165	0.165	0.167	0.167	0.165	0.134
	(0.367)	(0.371)	(0.371)	(0.373)	(0.373)	(0.371)	(0.340)
High school share	0.213	0.231	0.246	0.247	0.253	0.264	0.311
	(0.409)	(0.421)	(0.430)	(0.431)	(0.435)	(0.441)	(0.463)
Labor force	0.646	0.642	0.643	0.641	0.640	0.641	0.637
	(0.478)	(0.479)	(0.479)	(0.480)	(0.480)	(0.480)	(0.481)
Employed	0.890	0.894	0.901	0.901	0.903	0.903	0.920
	(0.312)	(0.308)	(0.298)	(0.299)	(0.295)	(0.297)	(0.271)
Occupational score	24.383	25.034	25.344	25.479	25.556	25.659	26.318
	(9.464)	(9.512)	(9.779)	(9.834)	(9.873)	(9.919)	(10.301)
Panel C: Household level characteristics:							
Homeowner	0.218	0.260	0.257	0.258	0.269	0.270	0.353
	(0.413)	(0.439)	(0.437)	(0.437)	(0.443)	(0.444)	(0.478)
Household size	3.439	3.474	3.490	3.500	3.525	3.494	3.493
	(2.027)	(2.043)	(2.034)	(2.019)	(2.033)	(2.027)	(1.952)
Log home value	10.562	10.619	10.660	10.691	10.708	10.733	10.852
	(0.985)	(0.960)	(0.979)	(0.955)	(0.961)	(0.931)	(0.906)
Log rent	5.728	5.811	5.887	5.907	5.936	5.959	6.001
	(0.715)	(0.780)	(0.795)	(0.804)	(0.807)	(0.807)	(0.832)

Note: Each observation corresponds to an individual in the 1940 census. Displacement and the proximity to highways are calculated for all segments opened between 1950 and 1960. Mean values are reported for each variable, with the corresponding standard deviation in parentheses. High school share and labor force are calculated for the sample of individuals aged 25 to 55. Employed is calculated for individuals aged 25 to 55 in the labor force. Occupational score is calculated for individuals aged 25 to 55 and employed. Home value and rent are conditional on ownership.

TABLE 4: Balance Test

	Displaced	Control	Mean Test [Displaced = Control]	
Fixed Effects			No	Yes
	(1)	(2)	(3)	(4)
Age	18.219	18.578	-0.359	
	(7.490)	(7.574)	[0.039]	
Female	0.520	0.528	-0.009	
	(0.500)	(0.499)	[0.459]	
Race identified as Black	0.073	0.047	0.026	
	(0.260)	(0.211)	[0.000]	
Home owner	0.191	0.240	-0.049	
	(0.394)	(0.427)	[0.000]	
N. of subfamilies in the household	0.107	0.102	0.006	0.005
	(0.343)	(0.332)	[0.450]	[0.529]
N. of own children in the household	0.238	0.242	-0.004	0.015
	(0.709)	(0.722)	[0.807]	[0.267]
Married (indicator)	0.209	0.225	-0.016	-0.006
	(0.407)	(0.418)	[0.093]	[0.413]
(log) Monthly contract rent	5.843	5.990	-0.147	-0.110
	(0.627)	(0.711)	[0.000]	[0.000]
(log) House value	10.641	10.733	-0.092	-0.072
	(0.920)	(0.926)	[0.044]	[0.097]
College graduate	0.024	0.026	-0.002	0.001
	(0.153)	(0.158)	[0.620]	[0.864]
High school graduate	0.237	0.268	-0.032	-0.029
	(0.425)	(0.443)	[0.002]	[0.001]
Middle school graduate	0.800	0.803	-0.003	0.005
	(0.400)	(0.398)	[0.731]	[0.320]
Employed	0.793	0.814	-0.022	-0.019
	(0.406)	(0.389)	[0.130]	[0.184]
Labor force participation	0.537	0.538	-0.001	-0.000
	(0.499)	(0.499)	[0.913]	[0.980]
Occupational score	22.856	23.516	-0.660	-0.203
	(7.439)	(7.935)	[0.028]	[0.454]
Born outside the U.S.	0.041	0.044	-0.003	0.002
	(0.197)	(0.204)	[0.523]	[0.690]
Parents born outside the U.S.	0.384	0.379	0.006	0.031
	(0.487)	(0.485)	[0.622]	[0.002]
Living in their birth state	0.761	0.791	-0.030	-0.017
	(0.426)	(0.406)	[0.002]	[0.057]
Same house as 5 years ago	0.343	0.371	-0.028	0.005
	(0.475)	(0.483)	[0.014]	[0.624]
Same community as 5 years ago	0.878	0.877	0.002	0.010
	(0.327)	(0.329)	[0.841]	[0.163]
Within county mig. in the last 5 years	0.563	0.536	0.027	0.003
	(0.496)	(0.499)	[0.023]	[0.765]
Within state mig. in the last 5 years	0.039	0.038	0.001	-0.001
	(0.194)	(0.192)	[0.870]	[0.757]
Between state mig. in the last 5 years	0.051	0.051	0.000	-0.007
	(0.221)	(0.220)	[0.980]	[0.137]

Note: The sample corresponds to individuals in the 1940 full count census matched to administrative mortality records who were displaced or lived between 100 and 200 meters of a highway. Only segments opened between 1950 and 1960 are included in the sample. Column (1) reports the mean and standard deviation for displaced individuals. Column (2) reports the mean and standard deviation for individuals living between 100 and 200 meters of future highways. Columns (3) and (4) Wald-test of equality of averages for both samples, with p-values reported in square brackets. Column (3) does not include any fixed effect. Column (4) include city, birth year, homeownership, race, and gender fixed effects.

TABLE 5: Long-term Effects of Highway Construction

	Same Neigh. (1)	Same City (2)	Death Age (3)	Survival to age 70 (4)
Panel A: *near-versus-far* approach				
Displaced	-0.014[a]	0.014	-0.228[a]	-0.010[a]
	(0.002)	(0.014)	(0.044)	(0.003)
Adjacent	-0.004[b]	0.016[a]	-0.061[c]	-0.001
	(0.002)	(0.004)	(0.036)	(0.001)
Mean dep. var.	0.021	0.607	79.076	0.894
R-squared (adj)	0.023	0.019	0.863	0.525
Observations	35,652	35,652	33,712	33,712
Panel B: *near-versus-far* approach + controls				
Displaced	-0.015[a]	0.011	-0.231[a]	-0.011[a]
	(0.003)	(0.015)	(0.045)	(0.003)
Adjacent	-0.005[a]	0.013[a]	-0.057	-0.001
	(0.002)	(0.004)	(0.039)	(0.001)
Mean dep. var.	0.022	0.610	79.399	0.915
R-squared (adj)	0.026	0.028	0.855	0.428
Observations	33,236	33,236	32,281	32,281
Panel C: matching on observables				
Displaced	-0.018[a]	0.032[c]	-0.263[a]	-0.004
	(0.002)	(0.017)	(0.049)	(0.003)
Adjacent	-0.008[a]	0.032[a]	0.010	0.004[c]
	(0.003)	(0.005)	(0.034)	(0.002)
Mean dep. var.	0.024	0.609	78.795	0.894
R-squared (adj)	0.019	0.012	0.856	0.511
Observations	20,935	20,935	19,861	19,861
Panel D: Federal engineering maps as control group				
Displaced	-0.015[a]	0.019	-0.141[a]	-0.007[a]
	(0.003)	(0.020)	(0.042)	(0.002)
Adjacent	-0.005[b]	0.021[a]	0.023	0.001
	(0.002)	(0.007)	(0.022)	(0.001)
Mean dep. var.	0.023	0.611	79.140	0.895
R-squared (adj)	0.022	0.018	0.864	0.523
Observations	43,875	43,875	41,601	41,601

Note: OLS estimates are reported. Only segments opened between 1950 and 1960 are included in the sample. An observation is an individual in the 1940 full count census matched to administrative mortality records. The sample consists of individuals who died after 1995. Displaced corresponds to individuals living in 1940 in houses destroyed by highway construction. Adjacent corresponds to individuals living in 1940 within 100 meters of future development. The control group corresponds to individuals living between 100 and 200 meters from a future highway. Each column corresponds to a different regression. The dependent variable in column (1) is an indicator that equals one if the individual lives at time of death in the same neighborhood they lived in 1940. In column (2), the dependent variable is an indicator that equals one if the individual lives in the same city they lived in 1940. Column (3) uses the age at death as the dependent variable. Finally, the dependent variable in Column (4) is an indicator if the individual survived until the age of 70. All regressions control for race, gender at birth, homeownership, city, and birth year fixed effects. Coefficients are reported with standard errors clustered at the city where the individual lived in 1940 level. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level .

TABLE 6: Neighborhood Characteristics at Time of Death

	High School Share (1)	College Share (2)	Employment Share (3)	Homeowner. Share (4)	Log Median Income (5)	Log Median Home Value (6)
Panel A: *near-versus-far* approach						
Displaced	-1.008[a]	-1.607[b]	-0.005[a]	-0.226	-0.018	-0.014
	(0.328)	(0.729)	(0.002)	(0.479)	(0.013)	(0.016)
Adjacent	-0.303[c]	-0.749[a]	0.000	-0.089	-0.010[b]	-0.013[b]
	(0.152)	(0.280)	(0.001)	(0.232)	(0.004)	(0.006)
Mean dep. var.	85.056	34.170	0.581	68.007	10.976	12.114
R-squared (adj)	0.145	0.098	0.058	0.073	0.118	0.235
Observations	35,562	35,562	35,650	35,618	35,519	35,036
Panel B: *near-versus-far* approach + controls						
Displaced	-0.962[a]	-1.535[b]	-0.005[a]	-0.094	-0.014	-0.012
	(0.318)	(0.676)	(0.002)	(0.421)	(0.011)	(0.015)
Adjacent	-0.235[c]	-0.726[a]	0.000	0.083	-0.009[b]	-0.013[b]
	(0.125)	(0.226)	(0.001)	(0.214)	(0.004)	(0.006)
Mean dep. var.	85.100	34.311	0.581	68.030	10.979	12.119
R-squared (adj)	0.155	0.114	0.058	0.075	0.121	0.239
Observations	33,151	33,151	33,234	33,205	33,111	32,663
Panel C: matching on observables						
Displaced	-1.535[a]	-2.144[a]	-0.002	-1.506[a]	-0.019[a]	-0.011
	(0.204)	(0.437)	(0.003)	(0.470)	(0.006)	(0.013)
Adjacent	-0.620[a]	-0.577	0.004[c]	-1.049[a]	-0.003	-0.001
	(0.190)	(0.353)	(0.002)	(0.256)	(0.007)	(0.015)
Mean dep. var.	86.024	34.884	0.580	70.039	11.022	12.199
R-squared (adj)	0.090	0.067	0.040	0.037	0.044	0.155
Observations	20,891	20,891	20,934	20,915	20,879	20,615
Panel D: Federal engineering maps as control group						
Displaced	-1.149[a]	-1.999[b]	-0.006[a]	-0.025	-0.015	-0.015
	(0.376)	(0.774)	(0.002)	(0.325)	(0.010)	(0.019)
Adjacent	-0.444[a]	-1.066[a]	0.001	0.025	-0.007	-0.016[c]
	(0.138)	(0.244)	(0.001)	(0.222)	(0.004)	(0.008)
Mean dep. var.	85.446	34.761	0.581	68.326	10.987	12.127
R-squared (adj)	0.125	0.093	0.052	0.071	0.098	0.232
Observations	43,762	43,762	43,868	43,835	43,716	43,028

Note: OLS estimates are reported. Only segments opened between 1950 and 1960 are included in the sample. An observation is an individual in the 1940 full count census matched to administrative mortality records. The sample consists of individuals who died after 1995. Displaced corresponds to individuals living in 1940 in houses destroyed by highway construction. Adjacent corresponds to individuals living in 1940 within 100 meters of future development. The control group is individuals living between 100 and 200 meters from a future highway. Each column corresponds to a different regression. The dependent variables correspond to the neighborhood-level characteristics of the residence at time of death. The dependent variable in column (1) corresponds to the share of adults living in the neighborhood who have completed high school. In column (2), the dependent variable is the college share. Column (3) uses the employment share as dependent variable. Columns (4) and (5) use the log median income and log median house value, respectively. Finally, column (6) uses the log average rent for a two bedroom apartment as dependent variable. All regressions control for race, gender at birth, homeownership, city, and birth year fixed effects. Coefficients are reported with standard errors clustered at the city where the individual lived in 1940 level. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

TABLE 7: Coefficient Decomposition

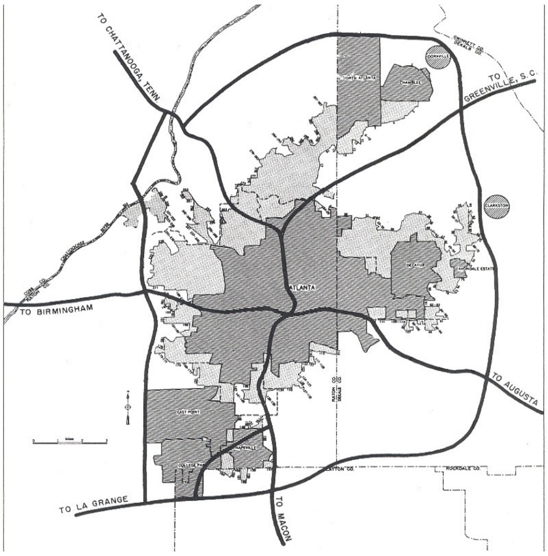
	Outcome: Death age						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Displaced	-0.228[a]	-0.157[b]	-0.237[a]	-0.226[a]	-0.234[a]	-0.231[a]	-0.169[b]
	(0.044)	(0.073)	(0.045)	(0.043)	(0.048)	(0.044)	(0.074)
log Distance from origin			0.063[a]				0.034[b]
			(0.008)				(0.014)
Δ % Unemployment				-0.157[a]			-0.141[b]
				(0.035)			(0.056)
Δ Distance to CBD					0.112[a]		0.054[b]
					(0.017)		(0.026)
log Distance to hospital						0.120[a]	0.042
						(0.017)	(0.029)
Neigh. destination FE	N	Y	N	N	N	N	Y
Mean death age	79.076	79.065	79.080	79.074	79.080	79.076	79.076
Adj. R^2	0.863	0.868	0.863	0.864	0.863	0.864	0.869
Observations	33,712	33,155	33,198	33,664	33,198	33,712	32,807

Note: OLS estimates are reported. An observation is an individual in the 1940 full count census matched to administrative mortality records living within 200 meters of a highway opened between 1950 and 1960. The sample consists of individuals who died after 1995. Displaced corresponds to individuals living in 1940 in houses destroyed by highway construction. Adjacent corresponds to individuals living in 1940 within 100 meters of future development. The control group is individuals living between 100 and 200 meters from a future highway. Each column corresponds to a different regression. For all columns the dependent variable is age at death. Column (1) presents the baseline estimates shown in Panel A Column (3) of Table 5. Column (2) includes destination neighborhood fixed effects. Column (3) controls for the log distance between residence in 1940 and 2000. Column (4) controls for the percentage difference in unemployment rate for the neighborhood living in 1940 and 2000. Column (5) controls for the difference in distance to the CBD between the 1940 and 2000 residences. Column (6) controls for the log distance to the nearest hospital in 2000. Finally, Column (7) includes all controls. All regressions control for race, gender at birth, homeownership, city, and birth year fixed effects. Coefficients are reported with standard errors clustered at the city where the individual lived in 1940 level. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

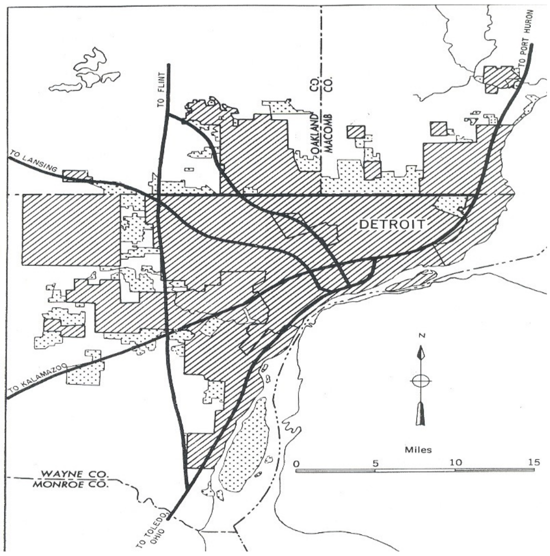
Appendix and Supplementary Material

A. APPENDIX FIGURES

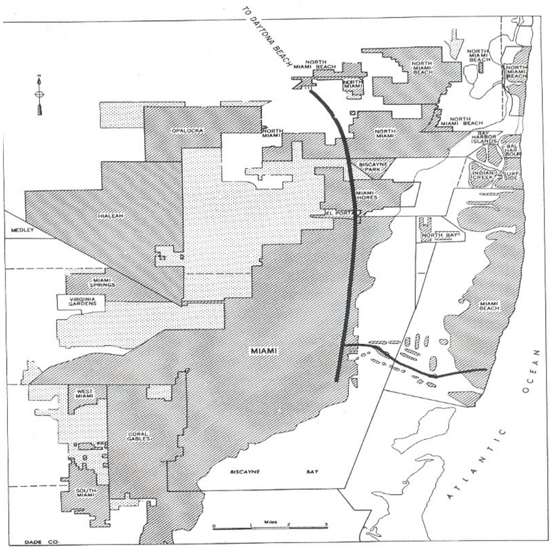
FIGURE A.1: Yellow Book Maps



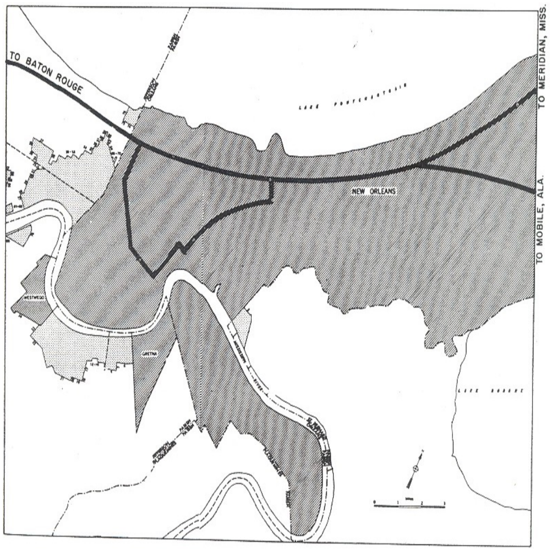
(A) Atlanta



(B) Detroit



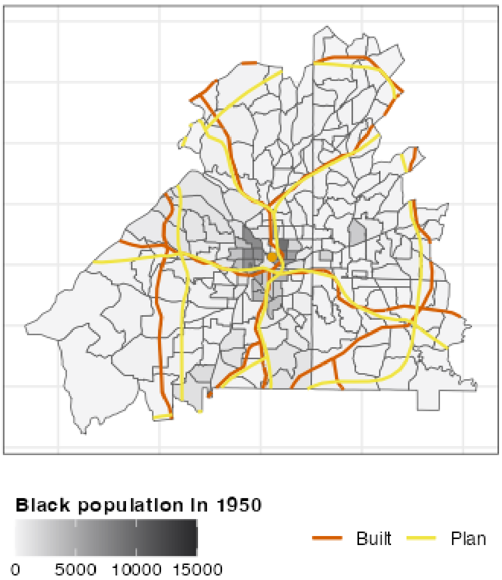
(C) Miami



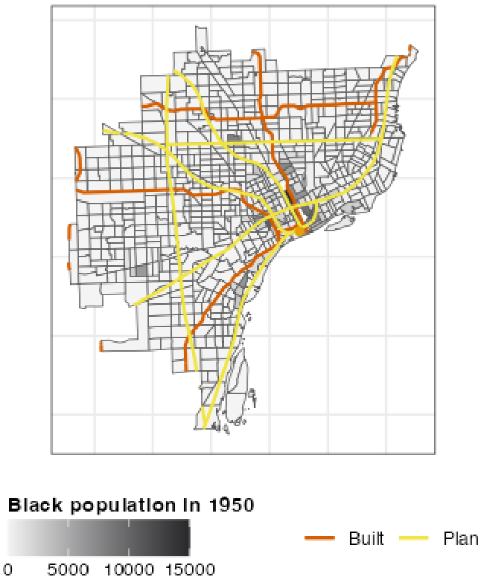
(D) New Orleans

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

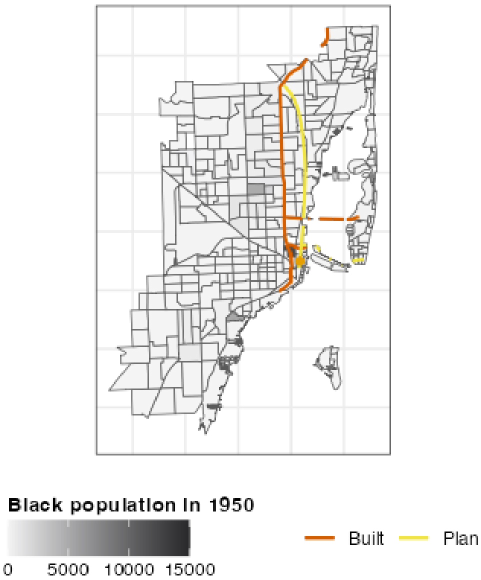
FIGURE A.2: Racial Distribution, Highways, and Planned Routes



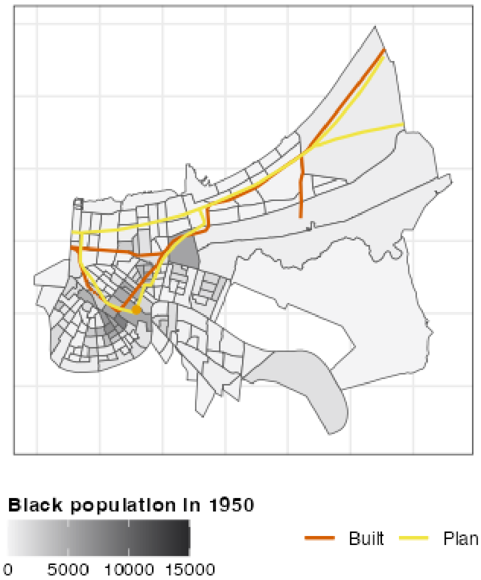
(A) Atlanta



(B) Detroit



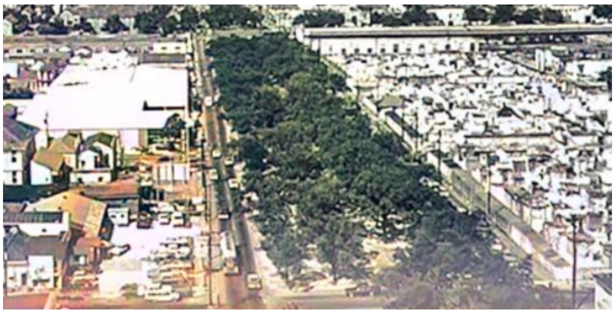
(C) Miami



(D) New Orleans

Note: 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. 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.

FIGURE A.3: Disruptive Effects of Highway Construction



(A) Claiborne before Interstate 10

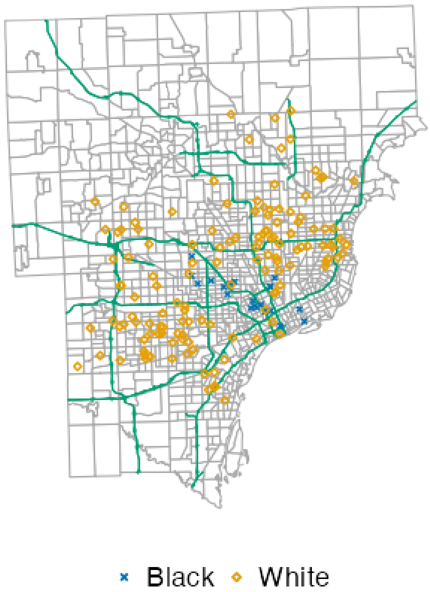
(B) Claiborne after Interstate 10

(C) Black Bottom before Interstate 75

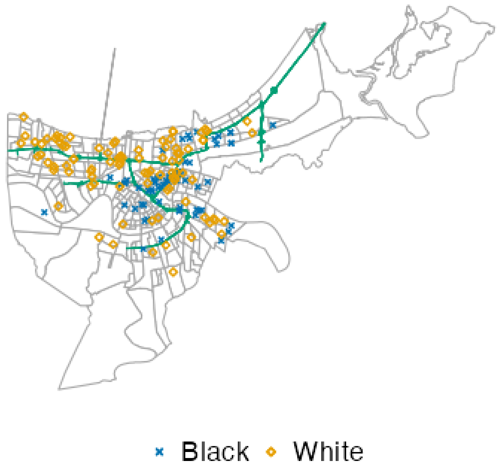
(D) Black Bottom after Interstate 75

Note: The figure presents a visual representation of two neighborhoods, Claiborne in New Orleans and Black Bottom in Detroit, before and after highway construction.

FIGURE A.4: Location of displaced residents at their time of death



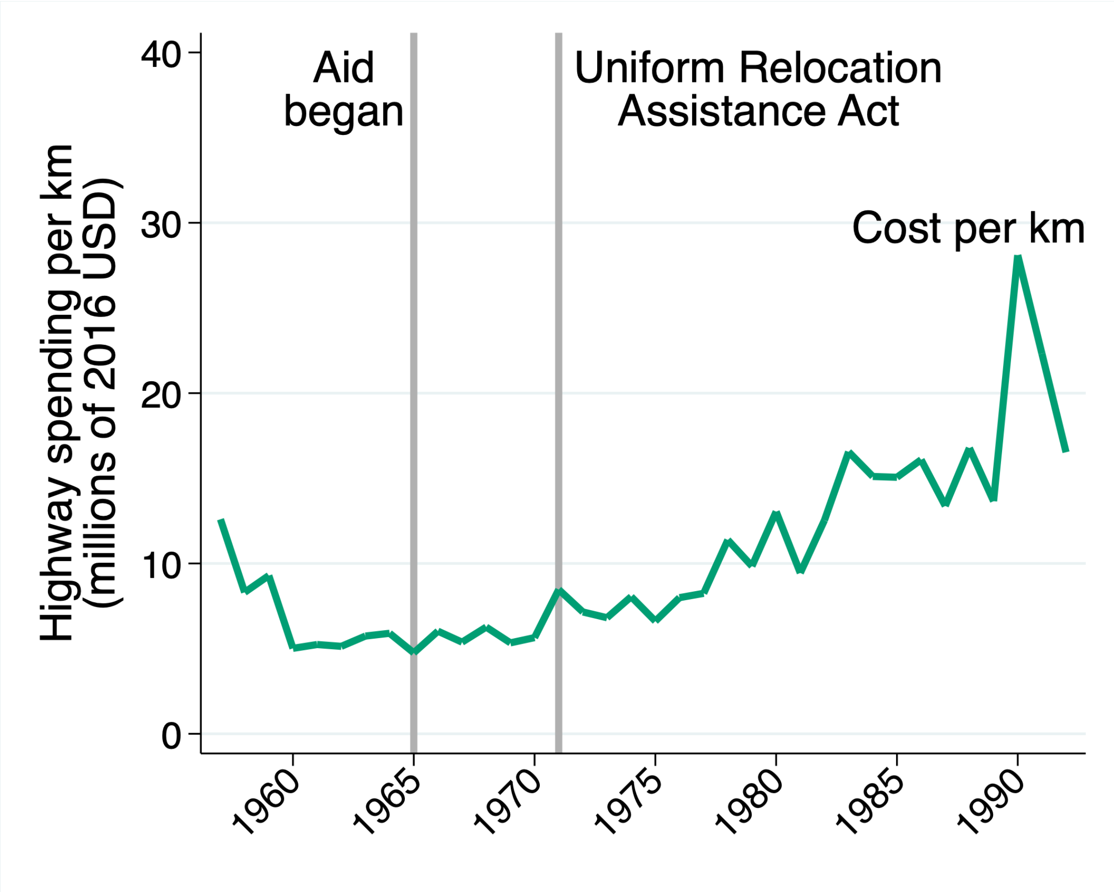
(A) Detroit



(B) New Orleans

Note: The observational unit corresponds to the location at the time of death of individuals displaced by the construction of the IHS in Detroit and New Orleans. The sample consists of individuals residing in Detroit and New Orleans before and after construction, split by the recorded race of the individual. Panel (a) presents the location at time of death of individuals living in Detroit who were displaced by highway construction. presents the location at the time of death of individuals displaced by highway construction. Pabel (b) presents the same analysis for New Orleans.

FIGURE A.5: Real cost per kilometer of highway construction



Note: data taken from Brooks and Liscow (2023).

B. APPENDIX TABLES

TABLE B.1: Short-run Consequences of Displacement Neighborhood Satisfaction

	Positive opinion	Crime	Bad public trans.	Bad schools	Street noise	Heavy traffic
	(1)	(2)	(3)	(4)	(5)	(6)
Displaced	0.004	0.007	0.009	0.058[c]	0.007	0.015
	(0.046)	(0.028)	(0.046)	(0.033)	(0.044)	(0.043)
Previous ownership	0.074[a]	-0.030[a]	-0.064[a]	0.031[a]	-0.032[a]	-0.040[a]
	(0.007)	(0.004)	(0.009)	(0.007)	(0.008)	(0.008)
Observations	14,746	14,798	14,635	14,462	14,798	14,798
R^2	0.0397	0.0264	0.0356	0.4770	0.0125	0.0127
Mean dep. var.	0.795	0.071	0.414	0.452	0.271	0.208

Note: OLS estimates are reported. Each column corresponds to a different regression. The sample consists of individuals who moved residences in the last twelve months in the 1973 and 1974 American Housing Survey. Displaced corresponds to individuals that moved because their house was destroyed by highway construction. The dependent variable is denoted on top of each column. All dependent variables are indicator variables. Previous ownership is an indicator that equals one if the household owned the previous residence. All specifications include Race fixed effects and a quadratic in age. Robust standard errors are reported in parentheses. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10%.

C. DATA APPENDIX

C.1 Cleaning historical addresses

I geocode the restricted access full-count censuses from 1930 to 1940 and focus on the 285 counties that contained one of the 168 Standard Metropolitan Areas in 1950. (Ruggles et al., 2024).[49, 50] 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 82.16% of the urban population in the US in 1940.[51] To clean historical addresses, I follow the procedures recommended by Logan and Zhang (2018). Figure C.1 exemplifies the address information in the 1940 census. 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 dwelling ID provided by IPUMS. Street name and numeration are constant across dwelling 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.

[49] I am waiting for the release of the restricted access full-count 1950 census, which was the last census before the 1956 Federal Highway Act.

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

[51] New England used a different approach to define urban areas. This sometimes leads to the same county housing two urban areas. To simplify the analysis, I assign the county to the urban area with the largest population. Thus, the total number of SMAs in my sample is 161.

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, and 1940 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 of smaller than 0.2. 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.

As a robustness for the address cleaning procedure, I take advantage of the extraordinary job done by Logan et al. (2023), who cleaned and gave a consistent format to street addresses and numeration for 181 cities. Their definition of city differs from mine. They use cities with at least 30 thousand inhabitants in at least one decade between 1910 and 1940, whereas I use the 1950 Standard Metropolitan Areas. I use this data as an additional robustness check for the address-cleaning procedure.

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 Streetmap 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.[52] I also drop the geocoding for those records with a *match score* equal to or higher than 85 and with a unique address match, as in Hynsjö and Perdoni (2022).[53] 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 45,770,203 geocoded individuals in the 1940 census, which accounts for 80.85% of urban dwellers in the counties I study. Table C.1 shows a breakdown of the number of geocoded dwellings by SMA.

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 this type of match as *StreetAddress*. As a robustness and to minimize measurement errors, I flag those observations with a perfect match score. These observations are the ones that, in addition to passing all the aforementioned filters, have a match score of 100. 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 conclusion, modern geocoders *may* miss some addresses, but the ones they match are accurate and reliable.

To study if there is any selection into geocoding, I compare the characteristics of the geocoded individuals with the non-geocoded individuals. Table C.2 presents the results for the 1940 census. The analysis shows some small differences between the geocoded and non-geocoded groups. Black individuals are less likely to be geocoded,

[52] 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.

[53] 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.

and geocoded individuals are more likely to be homeowners. The average home value is larger for the non-geocoded group, whereas rents are higher in the geocoded group. Overall, the differences are small and do not seem to be systematic.

C.2 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 construction, I include exits as part of the highway database. The network in OSM is recorded as *Polylines* with negligible width, whereas in reality, highways are polygons. I exploit the information in OSM when possible to convert the polylines into polygons. In particular, I use the number of lanes multiplied by 3.65 meters (12 feet), the average width of an Interstate lane, plus 5 meters to account for the berm (Federal Highway Administration, 2007). When the number of lanes is unavailable, I input the number of lanes to four, which is the median number of lanes a highway has in the sample. The buffer choice was made based on the median number of lanes a highway has in the sample, four, and the minimum lane width recommended by Federal Highway Administration (2007). This buffer is the one used in the subsequent analysis.

C.3 Identifying displacement

I identify displaced individuals as those living in dwellings within the highway buffer. 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, I can identify displaced households. I classify an individual as living next to a highway if the dwelling is within 100 meters of the highway buffer.

C.4 Linkage to administrative mortality records

To study the long-term consequences of highway displacement, I use the linkage between the 1940 census and the administrative mortality records from Goldstein et al. (2023). The linkage is done using the algorithm developed by Abramitzky et al. (2017). Table C.3 shows a balance test between those records matched to mortality records and those not. Individuals matched to mortality records are younger due to the double truncated mortality years (1995 to 2005). Consequently, individuals are less likely to be married, have a college degree, or be employed. The matched sample also underrepresents Black individuals and immigrants. The matched sample is similar to the non-matched sample for the rest of the variables. The results suggest that the sample differs from the population in some aspects, but the differences are small and do not present a systematic bias that can confound the results.

C.5 Other data

I use Facebook data to measure social connection inside and outside the neighborhood of residence. In particular, I use zipcode level connectness measures from Jahani et al. (2023). The variables I use are:

1. *Mean degree*: average number of nodes residing in the zipcode.
2. *Weighted mean clustering coefficient*: average clustering coefficient of the nodes residing in the zipcode. It measures how connected groups of three Facebook friends are in the zipcode.
3. *Weighted long-ties*: zipcode weighted fraction of each individual's ties that are long ties (i.e. lack any mutual network neighbors).

C.6 Kin presence in the city

Social networks such as the extended family can be important determinant how affected individuals respond to displacement. This may be particularly important becuase there was no relocation assistance for those displaced by the highway construction during the period of study.

To measure the presence of kin in the city, I use the last names of the individuals in the 1940 census and construct an index that measure the kin presence in the city. Intuitevely, the index measures "how common" is the last name in the city relative to the country.[54] The index is similar in spirit to the measure developed by Schwank (2023). However, I don't incorporate physical proximity in the index. In particular, I use the following formula to construct the index:

$$LN_c(s) = \frac{q_{c(i)}(s)}{q(s)}$$

where $LN_c(s)$ measures the commonality of last name s in city c. The numerator $q_{c(i)}(s)$ is the quantile on the last name in city's c last name distribution. The denominator $q(s)$

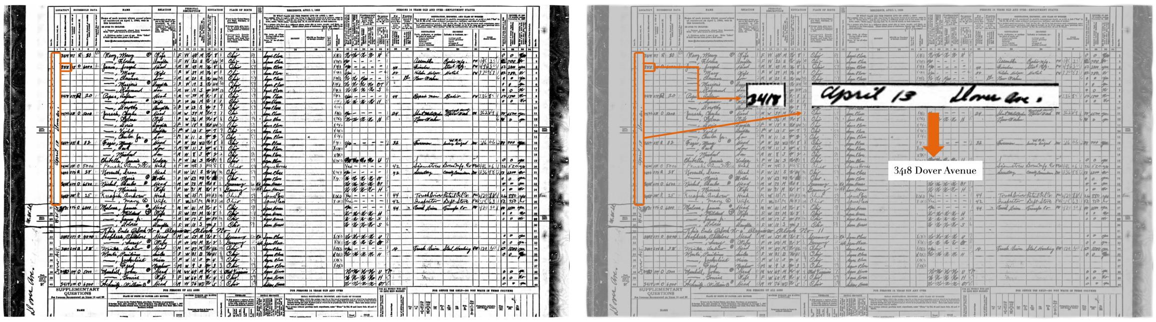
[54] The index is based on last names, thus it only captures patrilineal kinship ties as women usually takes their husband's last name.

is the quantile on the last name in the U.S.'s last name distribution, and it is included to account for relatively common last names such as Smith or Williams. The index is constructed for each city in the sample and matched to individuals based on their last name.

To account for misspelled last names, I create their phonetic representation using `NYSIIS` package in Stata. This takes into account potential differences induced by enumerators or the OCR process. For example, *Valenzuela* and *Valensuela* have the same phonetic representation. Finally, to create a discrete index, I transform the index into a binary variable that takes the value of one if the index is above the 90th percentile of the city distribution of the index in the sample.

C.7 Additional figures

FIGURE C.1: Example of addresses in the 1940 census



(A) 1940 census (B) Address Information

Note: Panel (a) and (b) highlight the address information in the 1940 census.

C.8 Additional tables

TABLE C.1: Number of Geocoded Dwellings by SMA

Metropolitan Area Name	State	Code	N. of Dwellings	Geocoded	Share (%)
Akron	OH	80	83,654	70,753	84.58%
Albany-Schenectady-Troy	NY	160	109,436	94,583	86.43%
Albuquerque	NM	200	11,100	7,821	70.46%
Allentown-Bethlehem	NJ-PA	240	77,579	59,077	76.15%
Altoona	PA	280	27,778	24,228	87.22%
Amarillo	TX	320	16,245	13,441	82.74%
Asheville	NC	480	15,331	12,017	78.38%
Atlanta	GA	520	102,697	74,634	72.67%
Atlantic City	NJ	560	31,161	23,515	75.46%
Augusta	GA-SC	600	22,409	15,384	68.65%
Austin	TX	640	29,208	19,666	67.33%
Baltimore	MD	720	258,356	198,428	76.80%
Baton Rouge	LA	760	9,896	6,441	65.09%
Bay City	MI	800	13,098	10,085	77.00%
Beaumont-Port Arthur	TX	840	30,852	25,758	83.49%
Binghamton	NY	960	35,387	28,891	81.64%
Birmingham	AL	1000	89,889	74,081	82.41%
Boston	MA	1120	510,083	291,597	57.17%
Brockton	MA	1200	18,827	17,286	91.81%
Buffalo	NY	1280	223,401	193,466	86.60%
Canton	OH	1320	46,950	41,752	88.93%
Cedar Rapids	IA	1360	21,089	18,606	88.23%
Charleston	SC	1440	21,765	17,694	81.30%
Charleston	WV	1480	27,065	16,680	61.63%
Charlotte	NC	1520	28,128	19,995	71.09%
Chattanooga	GA-TN	1560	38,199	29,474	77.16%
Chicago	IL-IN	1600	1,346,533	1,141,562	84.78%
Cincinnati	KY-OH	1640	216,362	153,677	71.03%
Cleveland	OH	1680	357,514	265,703	74.32%
Columbia	SC	1760	21,999	12,819	58.27%
Columbus	AL-GA	1800	19,106	15,073	78.89%
Columbus	OH	1840	106,097	86,854	81.86%
Corpus Christi	TX	1880	18,188	11,920	65.54%
Dallas	TX	1920	98,091	78,808	80.34%
Davenport-Rock Island-Moline	IA-IL	1960	52,089	42,591	81.77%
Dayton	OH	2000	72,350	59,161	81.77%
Decatur	IL	2040	17,879	14,150	79.14%
Denver	CO	2080	116,679	71,191	61.01%
Des Moines	IA	2120	51,787	45,715	88.28%
Detroit	MI	2160	592,206	454,455	76.74%
Duluth-Superior	MN-WI	2240	55,852	43,060	77.10%
Durham	NC	2280	16,574	11,775	71.05%
El Paso	TX	2320	26,861	21,541	80.19%
Erie	PA	2360	37,826	31,662	83.70%

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Table C.1 – *Continued from previous page*

Metropolitan Area Name	State	Code	N. of Dwellings	Geocoded	Share (%)
Evansville	IN	2440	28,670	25,358	88.45%
Flint	MI	2640	43,354	33,979	78.38%
Fort Wayne	IN	2760	36,765	30,690	83.48%
Fort Worth	TX	2800	56,813	50,361	88.64%
Fresno	CA	2840	23,612	19,610	83.05%
Gadsen	AL	2880	10,974	7,879	71.80%
Galveston	TX	2920	21,577	16,623	77.04%
Grand Rapids	MI	3000	51,643	43,188	83.63%
Green Bay	WI	3080	15,077	12,434	82.47%
Greensboro-High Point	NC	3120	25,593	16,954	66.24%
Greenville	SC	3160	10,855	8,371	77.12%
Hamilton-Middleton	OH	3200	24,398	21,563	88.38%
Harrisburg	PA	3240	46,334	34,392	74.23%
Houston	TX	3360	126,074	100,742	79.91%
Huntington-Ashland	KY-OH-WV	3400	37,055	29,457	79.50%
Indianapolis	IN	3480	122,486	100,279	81.87%
Jackson	MI	3520	14,975	11,601	77.47%
Jackson	MS	3560	18,386	13,833	75.24%
Jacksonville	FL	3600	50,322	40,327	80.14%
Johnstown	PA	3680	33,753	22,920	67.91%
Kalamazoo	MI	3720	18,841	12,407	65.85%
Kansas City	KS-MO	3760	181,166	159,427	88.00%
Kenosha	WI	3800	13,349	12,440	93.19%
Knoxville	TN	3840	34,425	19,649	57.08%
Lancaster	PA	4000	27,550	22,882	83.06%
Lansing	MI	4040	26,891	19,369	72.03%
Laredo	TX	4080	8,969	7,703	85.88%
Lexington	KY	4280	17,260	11,603	67.22%
Lima	OH	4320	14,495	12,228	84.36%
Lincoln	NE	4360	26,508	22,995	86.75%
Little Rock-North Little Rock	AR	4400	35,620	27,072	76.00%
Lorain-Klyria	OH	4440	22,277	19,492	87.50%
Los Angeles	CA	4480	859,418	672,334	78.23%
Louisville	IN-KY	4520	107,216	90,186	84.12%
Lubbock	TX	4600	10,831	8,856	81.77%
Macon	GA	4680	17,295	8,014	46.34%
Madison	WI	4720	22,142	18,196	82.18%
Manchester	NH	4760	22,710	19,673	86.63%
Memphis	TN	4920	90,103	76,715	85.14%
Miami	FL	5000	67,235	54,624	81.24%
Milwaukee	WI	5080	208,231	157,514	75.64%
Minneapolis-St. Paul	MN	5120	267,304	225,713	84.44%
Mobile	AL	5160	23,956	18,277	76.29%
Montgomery	AL	5240	22,749	10,489	46.11%
Muncie	IN	5280	15,867	12,920	81.43%

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Table C.1 – *Continued from previous page*

Metropolitan Area Name	State	Code	N. of Dwellings	Geocoded	Share (%)
Nashville	TN	5360	49,121	39,490	80.39%
New Britain-Bristol	CT	5440	74,925	63,188	84.33%
New Orleans	LA	5560	148,288	122,205	82.41%
New York- Northeastern New Jersey	NJ-NY	5600	3,210,862	2,581,327	80.39%
Norfolk-Portsmouth	VA	5720	64,668	37,196	57.52%
Ogden	UT	5840	12,832	11,456	89.28%
Oklahoma City	OK	5880	67,797	53,530	78.96%
Omaha	IA-NE	5920	80,834	71,472	88.42%
Orlando	FL	5960	13,936	10,765	77.25%
Peoria	IL	6120	44,068	22,235	50.46%
Philadelphia	NJ-PA	6160	767,116	593,790	77.41%
Phoenix	AZ	6200	25,144	17,931	71.31%
Pittsburgh	PA	6280	412,566	269,883	65.42%
Pittsfield	MA	6320	13,746	11,676	84.94%
Portland	ME	6400	30,529	25,212	82.58%
Portland	OR-WA	6440	125,876	107,588	85.47%
Providence	MA-RI	6480	208,857	187,181	89.62%
Pueblo	CO	6560	19,254	13,490	70.06%
Racine	WI	6600	20,385	17,143	84.10%
Raleigh	NC	6640	14,359	8,182	56.98%
Reading	PA	6680	41,414	30,500	73.65%
Richmond	VA	6760	56,901	44,642	78.46%
Roanoke	VA	6800	21,360	12,502	58.53%
Rochester	NY	6840	110,896	93,085	83.94%
Rockford	IL	6880	26,719	21,995	82.32%
Sacramento	CA	6920	37,488	31,134	83.05%
Saginaw	MI	6960	23,322	18,259	78.29%
St. Joseph	MO	7000	26,962	20,979	77.81%
St. Louis	IL-MO	7040	363,030	289,852	79.84%
Salt Lake	UT	7160	50,546	40,863	80.84%
San Angelo	TX	7200	7,879	6,801	86.32%
San Antonio	TX	7240	76,845	61,520	80.06%
San Bernardino	CA	7280	30,547	25,011	81.88%
San Diego	CA	7320	90,377	67,076	74.22%
San Francisco-Oakland	CA	7360	467,825	401,332	85.79%
San Jose	CA	7400	35,838	30,555	85.26%
Savannah	GA	7520	27,806	20,568	73.97%
Scranton	PA	7560	68,280	47,516	69.59%
Seatle	WA	7600	143,821	128,787	89.55%
Shreveport	LA	7680	28,655	23,602	82.37%
Sioux city	IA	7720	25,234	19,971	79.14%
Sioux falls	SD	7760	12,513	11,118	88.85%
South Bend	IN	7800	37,525	31,818	84.79%
Spokane	WA	7840	44,019	38,681	87.87%
Springfield	IL	7880	23,260	19,258	82.79%

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Table C.1 – *Continued from previous page*

Metropolitan Area Name	**State**	**Code**	**N. of Dwellings**	**Geocoded**	**Share (%)**
Springfield	MO	7920	19,520	16,650	85.30%
Springfield	OH	7960	20,865	18,901	90.59%
Springfield-Holyoke	MA	8000	84,401	68,560	81.23%
Stamford-Norwalk	CT	8040	66,071	55,425	83.89%
Stockton	CA	8120	26,214	17,655	67.35%
Syracuse	NY	8160	66,930	59,433	88.80%
Tacoma	WA	8200	40,904	35,000	85.57%
Tampa-St. Petersburg	FL	8280	61,391	48,641	79.23%
Terre Haute	IN	8320	21,738	18,702	86.03%
Toledo	OH	8400	89,533	76,475	85.42%
Topeka	KS	8440	21,602	18,052	83.57%
Trenton	NJ	8480	39,097	32,489	83.10%
Tulsa	OK	8560	47,464	39,052	82.28%
Utica-Rome	NY	8680	56,283	42,215	75.00%
Waco	TX	8800	17,893	15,363	85.86%
Washington D.C.	DC-MD-VA	8840	224,652	171,150	76.18%
Waterbury	CT	8880	72,557	63,379	87.35%
Waterloo	IA	8920	18,455	15,202	82.37%
Wheeling-Steubenville	OH-WV	9000	56,634	43,667	77.10%
Wichita	KS	9040	37,485	32,966	87.94%
Wichita Falls	TX	9080	16,130	13,888	86.10%
Wilkes-Bare-Hazleton	PA	9120	80,591	52,535	65.19%
Wilmington	DE-NJ	9160	39,268	33,131	84.37%
Winston-Salem	NC	9220	22,167	15,295	69.00%
Worcester	MA	9240	55,988	48,472	86.58%
York	PA	9280	24,087	19,545	81.14%
Youngstown	OH-PA	9320	86,165	74,368	86.31%
			16,783,010	13,276,985	79.11%

TABLE C.2: Sample selection: Geocoded

	Geocoded	Non-Geocoded	Mean Diffs.
	(1)	(2)	(3)
Black (indicator)	0.0784	0.0952	0.0168
	(0.2688)	(0.2935)	[0.000]
Female (indicator)	0.5126	0.5021	-0.0105
	(0.4998)	(0.5000)	[0.000]
Age	32.5461	32.3638	-0.1823
	(19.4842)	(19.3879)	[0.000]
Married (indicator)	0.4435	0.4147	-0.0287
	(0.4968)	(0.4927)	[0.000]
Home owner (indicator)	0.3085	0.3013	-0.0072
	(0.4619)	(0.4588)	[0.000]
Home value (000)	70.1813	74.1869	4.0056
	(57.4663)	(64.2508)	[0.000]
Rent (000)	0.9553	0.9124	-0.0429
	(4.2648)	(4.1162)	[0.000]
College degree (indicator)	0.0389	0.0420	0.0031
	(0.1933)	(0.2006)	[0.000]
High school degree (indicator)	0.2358	0.2380	0.0022
	(0.4245)	(0.4258)	[0.000]
Middle school degree (indicator)	0.7631	0.7474	-0.0157
	(0.4252)	(0.4345)	[0.000]
Immigrant (indicator)	0.1491	0.1399	-0.0092
	(0.3562)	(0.3469)	[0.000]
First gen. immigrant (indicator)	0.1372	0.1233	-0.0139
	(0.3441)	(0.3288)	[0.000]
Same house last 5 years	0.3856	0.3724	-0.0132
	(0.4867)	(0.4834)	[0.000]
Within county migration last 5 years	0.5129	0.4989	-0.0140
	(0.4998)	(0.5000)	[0.000]
Within state migration last 5 years	0.0429	0.0575	0.0146
	(0.2027)	(0.2328)	[0.000]
Between state migration last 5 years	0.0536	0.0661	0.0125
	(0.2252)	(0.2485)	[0.000]
Employed (indicator)	0.8881	0.8894	0.0013
	(0.3152)	(0.3136)	[0.000]
Labor force participation (indicator)	0.5551	0.5529	-0.0022
	(0.4969)	(0.4972)	[0.000]
Occupational score	25.2746	24.8657	-0.4089
	(9.9995)	(10.3155)	[0.000]
Observations	45,161,046	10,699,634	

Note: Each observation is an individual in the 1940 census living in the urban centers in the sample of counties. Column (1) reports the mean and standard error for geocoded individuals. Column (2) reports the mean and standard error for non-geocoded individuals. Column (3) reports the mean difference and the two-sided p-value of the difference

TABLE C.3: Sample selection: Matched to administrative mortality records

	Matched	Non-Matched	Mean Diffs.
	(1)	(2)	(3)
Black (indicator)	0.0519	0.0799	0.0279
	(0.2219)	(0.2711)	[0.000]
Female (indicator)	0.5303	0.5116	-0.0187
	(0.4991)	(0.4999)	[0.000]
Age	19.3770	33.2718	13.8948
	(9.2257)	(19.6413)	[0.000]
Married (indicator)	0.2634	0.4534	0.1899
	(0.4405)	(0.4978)	[0.000]
Home owner (indicator)	0.3187	0.3079	-0.0108
	(0.4660)	(0.4616)	[0.000]
Home value (000)	68.6802	70.2633	1.5831
	(55.3883)	(57.5766)	[0.000]
Rent (000)	0.9053	0.9581	0.0529
	(4.1120)	(4.2732)	[0.000]
College degree (indicator)	0.0337	0.0392	0.0055
	(0.1804)	(0.1940)	[0.000]
High school degree (indicator)	0.2989	0.2323	-0.0666
	(0.4578)	(0.4223)	[0.000]
Middle school degree (indicator)	0.7973	0.7612	-0.0361
	(0.4020)	(0.4264)	[0.000]
Immigrant (indicator)	0.0400	0.1551	0.1151
	(0.1959)	(0.3620)	[0.000]
First gen. immigrant (indicator)	0.2394	0.1316	-0.1078
	(0.4267)	(0.3380)	[0.000]
Same house last 5 years	0.3712	0.3864	0.0151
	(0.4831)	(0.4869)	[0.000]
Within county migration last 5 years	0.5143	0.5128	-0.0015
	(0.4998)	(0.4998)	[0.000]
Within state migration last 5 years	0.0501	0.0425	-0.0075
	(0.2181)	(0.2018)	[0.000]
Between state migration last 5 years	0.0613	0.0532	-0.0081
	(0.2398)	(0.2244)	[0.000]
Employed (indicator)	0.8446	0.8901	0.0455
	(0.3623)	(0.3127)	[0.000]
Labor force participation (indicator)	0.5132	0.5573	0.0440
	(0.4998)	(0.4967)	[0.000]
Occupational score	23.4902	25.3535	1.8632
	(8.2732)	(10.0617)	[0.000]
Observations	2,358,731	42,802,315	

Note: Each observation is a geocoded individual in the 1940 census living in the urban centers in the sample of counties. Column (1) reports the mean and standard error for matched individuals. Column (2) reports the mean and standard error for non-matched individuals. Column (3) reports the mean difference and the two-sided p-value of the difference

D. Placement Appendix

D.1 Robustness of the Placement Results

In this section I present evidence that the results are robust to alternative specifications. A potential concern is the use of the 2010 census tract definition. Evidence suggests that state and city officials had detailed micro-data about neighborhood racial composition in 1950 (Caro, 1974, p.968), which is more disaggregated than any available census tract definition. In the previous analysis, I used the 2010 definition of census tracts because its geographic unit is smaller than the 1950 definition. However, it relies on area-weighted interpolation to convert the 1950 census tracts into the 2010 definition. To check the extent to which the results rely on this interpolation, I re-estimate equation 2 using the 1950 definition. In Appendix Tables D.3 and D.4, I present the results for both the discrete and continuous dependent variables. The results, however, remain virtually unchanged. Therefore, for the rest of the paper, I will use the 2010 census tract definition, the standard in the urban economics literature (Couture et al., 2023).

A second concern is that using a linear measure of proximity to the city center may not fully account for the city's socioeconomic distribution, particularly since Black households often resided in city centers targeted by highways (Boustan, 2010). This is especially relevant given findings from Brinkman and Lin (2022), which show that highways in central city areas frequently deviated from the original plan. To address this, I re-estimate equation 2 using the log distance to the city center, and as shown in Appendix Table D.5, the results remain robust.

Another concern is the use of population weights, as tracts closer to the city center had larger populations in the 1950s and may have been more likely to receive a highway. To address this, I test the sensitivity of the estimates by excluding weights. As shown in Appendix Table D.6, the data does not support this concern, and the estimated effect is even larger without weights. I also test whether the results are driven by a few cities by re-estimating 2 while leaving one city out each time. The results for the Black share, log median home value, log median rent, and distance to the city center remain similar in magnitude and statistical significance, as shown in Appendix Figure D.1. Finally, I check the robustness of the results by using different methods of calculating standard errors, clustering by city, state, 1950 census tracts, and allowing for spatial correlation within 10 kilometers.[55] As shown in Appendix Table D.7, the magnitude of the standard errors and statistical significance is consistent across methods.

[55] To calculate spatial standard errors, I use Colella et al.'s (2019) implementation of Conley (1999).

D.2 Additional Tables

TABLE D.1: List of MSAs Used in the Analysis

Metropolitan Area Name	State	Code	# tracts in 1950	Yellow Book
Akron	OH	80	95	No
Atlanta	GA	520	228	Yes
Austin	TX	640	71	No
Baltimore	MD	720	476	Yes
Birmingham	AL	1000	70	Yes
Boston	MA	1120	596	Yes
Bridgeport	CT	1160	70	No
Brockton	MA	1200	57	No
Buffalo	NY	1280	188	Yes
Chattanooga	TN-GA	1560	50	Yes
Chicago	IL-IN	1600	1547	Yes
Cincinnati	OH-KY	1640	233	Yes
Cleveland	OH	1680	473	Yes
Columbus	OH	1840	284	Yes
Dallas	TX	1920	205	Yes
Dayton	OH	2000	126	No
Denver	CO	2080	126	Yes
Detroit	MI	2160	748	Yes
Duluth-Superior	MN-WI	2240	36	No
Durham	NC	2280	60	No
Flint	MI	2640	113	Yes
Fort Worth	TX	2800	131	Yes
Greensboro-High Point	NC	3120	119	No
Hartford	CT	3280	108	Yes
Houston	TX	3360	785	Yes
Indianapolis	IN	3480	186	Yes
Kalamazoo	MI	3720	46	No
Kansas City	MO-KS	3760	136	Yes
Los Angeles	CA	4480	2348	Yes
Louisville	KY-IN	4520	85	Yes
Memphis	TN	4920	93	Yes
Miami	FL	5000	286	Yes
Milwaukee	WI	5080	297	Yes
Minneapolis-St. Paul	MN	5120	329	Yes
Nashville	TN	5360	86	Yes
New Haven	CT	5480	41	No
New Orleans	LA	5560	183	Yes
New York-Northeastern NJ	NY-NJ	5600	2491	Yes
Norfolk-Portsmouth	VA	5720	85	Yes
Oklahoma City	OK	5880	144	Yes
Omaha	NE-IA	5920	73	Yes
Philadelphia	PA-NJ	6160	1300	Yes

Continues on next page

Table D.1 – *Continued from previous page*

Metropolitan Area Name	**State**	**Code**	**# tracts in 1950**	**Yellow Book**
Pittsburgh	PA	6280	420	Yes
Portland	OR-WA	6440	117	Yes
Providence	RI	6480	53	Yes
Richmond	VA	6760	71	Yes
Rochester	NY	6840	106	Yes
Sacramento	CA	6920	318	No
St. Louis	MO-IL	7040	348	Yes
San Diego	CA	7320	406	No
San Francisco-Oakland	CA	7360	421	Yes
San Jose	CA	7400	47	No
Seattle	WA	7600	283	Yes
Spokane	WA	7840	50	No
Springfield-Holyoke	MA-CT	8000	86	Yes
Syracuse	NY	8160	140	Yes
Tacoma	WA	8200	149	No
Toledo	OH-MI	8400	77	Yes
Trenton	NJ	8480	35	No
Utica-Rome	NY	8680	34	Yes
Washington	DC-MD-VA	8840	266	Yes
Wichita	KS	9040	56	Yes
Total			18,687	

TABLE D.2: Determinants of Highway Placement: Continuous Dependent Variable

	Dependent Variable: Distance to the closest					
	Built highway					Plan
	(1)	(2)	(3)	(4)	(5)	(6)
Black share	-4.268[a]	-3.596[a]	-3.678[a]	-2.656[a]	-1.744[b]	-1.537
	(0.956)	(1.026)	(0.985)	(0.957)	(0.705)	(1.113)
(log) Median income	-0.184	-0.225	-0.174	-0.107	0.072[c]	-0.330
	(0.262)	(0.254)	(0.194)	(0.121)	(0.037)	(0.227)
High school share	0.032[a]	0.023[c]	0.013	-0.004	-0.005	0.003
	(0.011)	(0.012)	(0.011)	(0.007)	(0.003)	(0.012)
(log) Median rent		-0.097	-0.058	-0.100	0.200	-0.648[b]
		(0.337)	(0.230)	(0.152)	(0.155)	(0.295)
(log) Median home value		0.712[b]	0.544[a]	0.429[b]	0.248[b]	0.457[c]
		(0.294)	(0.166)	(0.174)	(0.117)	(0.241)
Distance to city center				0.128[a]	0.011	0.217[a]
				(0.026)	(0.008)	(0.056)
Distance to the planned route					0.536[a]	
					(0.038)	
Mean dependent var.	3.207	3.100	3.104	3.104	3.031	3.031
Geo. Controls	No	No	Yes	Yes	Yes	Yes
Obs.	18,242	17,210	16,944	16,944	15,416	15,416
R^2 (Adj.)	0.115	0.120	0.227	0.319	0.596	0.389

Note: Each column corresponds to a different regression. The unit of observation is a census tract. The dependent variable is the distance to the closest highway/planned segment. The vector of controls includes the (log) area and slope of the tract, distance to the nearest river, an indicator if the governor of the state was a Republican, the state (log) number of car registrations per 10k inhabitants, and the distance to the 1921 railroad network. All columns include city fixed effects. Coefficients are reported with standard errors clustered at the city level. [a] indicates the coef. is significant at the 1%, [b] at the 5%, and [c] at the 10% level. Regressions are weighted by the census tract's population.

TABLE D.3: 1950 Census Tract Definition

	Dependent Variable: Indicator if the tract is crossed by					
	Built highway					Plan
	(1)	(2)	(3)	(4)	(5)	(6)
Black share	0.306	0.253	0.268	0.275	0.374[c]	-0.313
	(0.274)	(0.271)	(0.252)	(0.254)	(0.220)	(0.203)
(log) Median income	-0.009	0.055[c]	-0.067[c]	-0.059	-0.055[c]	-0.011
	(0.030)	(0.029)	(0.038)	(0.037)	(0.029)	(0.035)
High school share	-0.114	0.056	-0.053	-0.020	0.034	-0.172
	(0.094)	(0.104)	(0.096)	(0.086)	(0.073)	(0.134)
(log) Median rent		-0.120[a]	-0.084[a]	-0.082[a]	-0.073[a]	-0.028
		(0.034)	(0.025)	(0.023)	(0.023)	(0.029)
(log) Median home value		-0.065	-0.073[b]	-0.077[a]	-0.062[b]	-0.049[b]
		(0.041)	(0.028)	(0.029)	(0.027)	(0.019)
Distance to city center				-0.007[a]	-0.005[a]	-0.006[a]
				(0.001)	(0.001)	(0.002)
Highway planned					0.317[a]	
					(0.054)	
Mean dependent var.	0.233	0.237	0.238	0.238	0.238	0.238
Geo. Controls	No	No	Yes	Yes	Yes	Yes
Obs.	9,968	9,566	9,439	9,439	9,439	9,439
R^2 (Adj.)	0.063	0.070	0.162	0.170	0.251	0.125

Note: Each column corresponds to a different regression. The unit of observation is a census tract, using the 1950 census tract definition. The dependent variable is an indicator if a highway was built or planned through the tract. The vector of controls includes the (log) area of the tract, distance to the nearest river, an indicator if the governor of the state was a Republican, the state (log) number of car registrations per 10k inhabitants, and the distance to the 1921 railroad network. All columns include city fixed effects. Coefficients are reported with standard errors clustered at the city level. [a] indicates the coef. is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

TABLE D.4: 1950 Census Tract Definition and Continuous Dependent Variable

	Dependent Variable: Distance to the closest					
	Built highway					Plan
	(1)	(2)	(3)	(4)	(5)	(6)
Black share	-1.238	-0.937	-1.850[c]	-1.960[c]	-2.265[b]	-1.005
	(0.930)	(0.948)	(1.015)	(0.985)	(1.022)	(1.192)
(log) Median income	0.802[a]	0.675[b]	-0.058	-0.199	0.194	-0.823[c]
	(0.263)	(0.334)	(0.358)	(0.342)	(0.189)	(0.445)
High school share	1.597	1.310	0.452	-0.084	-0.616	0.966
	(0.996)	(1.039)	(0.902)	(0.795)	(0.380)	(1.279)
(log) Median rent		-0.120	-0.015	-0.058	0.185	-0.504
		(0.325)	(0.223)	(0.156)	(0.181)	(0.395)
(log) Median home value		0.498[c]	0.395[b]	0.463[a]	0.173	0.670[a]
		(0.268)	(0.182)	(0.168)	(0.110)	(0.235)
Distance to city center				0.123[a]	0.021[c]	0.191[a]
				(0.024)	(0.012)	(0.044)
Distance to the planned route					0.532[a]	
					(0.044)	
Mean dependent var.	2.398	2.434	2.439	2.439	2.424	2.424
Geo. Controls	No	No	Yes	Yes	Yes	Yes
Obs.	9,968	9,566	9,439	9,439	8,430	8,430
R^2 (Adj.)	0.119	0.121	0.205	0.270	0.577	0.286

Note: Each column corresponds to a different regression. The unit of observation is a census tract, using the 1950 census tract definition. The dependent variable is the distance to the closest highway/planned segment. The vector of controls includes the (log) area of the tract, distance to the nearest river, an indicator if the governor of the state was a Republican, the state (log) number of car registrations per 10k inhabitants, and the distance to the 1921 railroad network. All columns include city fixed effects. Coefficients are reported with standard errors clustered at the city level. [a] indicates the coef. is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

TABLE D.5: Determinants of Highway Placement: Log Distance to CBD

	Dependent Variable: Indicator if the tract is crossed by					
	Built highway					Plan
	(1)	(2)	(3)	(4)	(5)	(6)
Black share	1.179[a]	0.996[a]	1.009[a]	0.720[b]	0.735[a]	-0.052
	(0.314)	(0.306)	(0.302)	(0.295)	(0.262)	(0.245)
(log) Median income	-0.028[b]	-0.016[c]	-0.009	-0.002	-0.003	0.003
	(0.012)	(0.009)	(0.016)	(0.013)	(0.010)	(0.011)
High school share	-0.003[b]	-0.001	-0.001	0.001	0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
(log) Median rent		-0.079[a]	-0.064[a]	-0.048[b]	-0.050[a]	0.006
		(0.025)	(0.022)	(0.020)	(0.017)	(0.035)
(log) Median home value		-0.094[a]	-0.120[a]	-0.101[a]	-0.077[a]	-0.084[a]
		(0.030)	(0.023)	(0.023)	(0.022)	(0.021)
(log) Distance to city center				-0.110[a]	-0.083[a]	-0.096[a]
				(0.017)	(0.014)	(0.015)
Highway planned					0.284[a]	
					(0.041)	
Mean dependent var.	0.214	0.217	0.218	0.218	0.218	0.218
Geo. Controls	No	No	Yes	Yes	Yes	Yes
Obs.	18,242	17,210	16,944	16,944	16,944	16,944
R^2 (Adj.)	0.086	0.094	0.155	0.177	0.243	0.119

Note: Each column corresponds to a different regression. The unit of observation is a census tract. The dependent variable is an indicator if a highway was built or planned through the tract. The vector of controls includes the (log) area and slope of the tract, distance to the nearest river, an indicator if the governor of the state was a Republican, the state (log) number of car registrations per 10k inhabitants, and the distance to the 1921 railroad network. All columns include city fixed effects. Coefficients are reported with standard errors clustered at the city level. [a] indicates the coef. is significant at the 1%, [b] at the 5%, and [c] at the 10% level. Regressions are weighted by the census tract's population.

TABLE D.6: Determinants of Highway Placement: Unweighted

	Dependent Variable: Indicator if the tract is crossed by					
	Built highway					Plan
	(1)	(2)	(3)	(4)	(5)	(6)
Black share	1.298[a]	1.143[a]	1.390[a]	1.269[a]	1.118[a]	0.472[c]
	(0.299)	(0.304)	(0.311)	(0.295)	(0.263)	(0.237)
(log) Median income	-0.007	-0.004	0.002	-0.002	-0.002	-0.001
	(0.006)	(0.006)	(0.008)	(0.006)	(0.005)	(0.004)
High school share	-0.001[c]	-0.000	-0.000	0.001	0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
(log) Median rent		-0.072[a]	-0.039[b]	-0.041[b]	-0.038[b]	-0.009
		(0.023)	(0.018)	(0.016)	(0.015)	(0.016)
(log) Median home value		-0.050[c]	-0.063[b]	-0.072[a]	-0.064[a]	-0.027
		(0.028)	(0.027)	(0.024)	(0.021)	(0.018)
Distance to city center				-0.008[a]	-0.005[a]	-0.007[a]
				(0.001)	(0.001)	(0.001)
Highway planned					0.321[a]	
					(0.040)	
Mean dependent var.	0.214	0.217	0.218	0.218	0.218	0.218
Geo. Controls	No	No	Yes	Yes	Yes	Yes
Obs.	18,242	17,210	16,944	16,944	16,944	16,944
R^2 (Adj.)	0.048	0.052	0.093	0.111	0.194	0.087

Note: Each column corresponds to a different regression. The unit of observation is a census tract. The dependent variable is the distance to the closest highway/planned segment. The vector of controls includes the (log) area and slope of the tract, distance to the nearest river, an indicator if the governor of the state was a Republican, the state (log) number of car registrations per 10k inhabitants, and the distance to the 1921 railroad network. All columns include city fixed effects. Coefficients are reported with standard errors clustered at the city level. [a] indicates the coef. is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

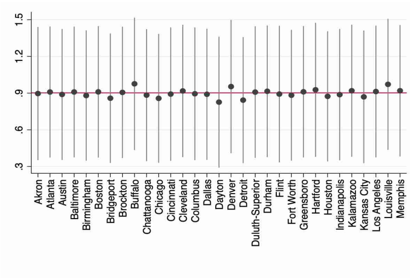
TABLE D.7: Determinants of Highway Placement: Different standard errors

	Dep. var.: Indicator if the tract is crossed by					
	Built Highway					Plan
	(1)	(2)	(3)	(4)	(5)	(6)
Black share	1.179	0.996	1.009	0.942	0.902	0.137
	(0.314)[a]	(0.306)[a]	(0.302)[a]	(0.299)[a]	(0.271)[a]	(0.239)
	[0.239][a]	[0.239][a]	[0.232][a]	[0.227][a]	[0.212][a]	[0.188]
	{0.297}[a]	{0.295}[a]	{0.291}[a]	{0.287}[a]	{0.261}[a]	{0.229}
(log) Median income	-0.028	-0.016	-0.009	-0.014	-0.012	-0.007
	(0.012)[b]	(0.009)[c]	(0.016)	(0.013)	(0.010)	(0.011)
	[0.005][a]	[0.006][a]	[0.006]	[0.005][b]	[0.005][b]	[0.005]
	{0.007}[a]	{0.007}[b]	{0.008}	{0.008}[c]	{0.006}[c]	{0.007}
High school share	-0.003	-0.001	-0.001	-0.000	0.000	-0.002
	(0.001)[b]	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
	[0.001][a]	[0.001][b]	[0.001][b]	[0.001]	[0.001]	[0.001][a]
	{0.001}[a]	{0.001}	{0.001}	{0.001}	{0.001}	{0.001}[c]
(log) Median rent		-0.079	-0.064	-0.061	-0.060	-0.005
		(0.025)[a]	(0.022)[a]	(0.021)[a]	(0.018)[a]	(0.035)
		[0.021][a]	[0.020][a]	[0.019][a]	[0.018][a]	[0.020]
		{0.029}[a]	{0.026}[b]	{0.026}[b]	{0.022}[a]	{0.033}
(log) Median home value		-0.094	-0.120	-0.112	-0.085	-0.093
		(0.030)[a]	(0.023)[a]	(0.023)[a]	(0.022)[a]	(0.021)[a]
		[0.020][a]	[0.019][a]	[0.019][a]	[0.018][a]	[0.020][a]
		{0.029}[a]	{0.025}[a]	{0.026}[a]	{0.024}[a]	{0.028}[a]
Distance to city center				-0.008	-0.006	-0.008
				(0.001)[a]	(0.001)[a]	(0.002)[a]
				[0.001][a]	[0.001][a]	[0.001][a]
				{0.001}[a]	{0.001}[a]	{0.001}[a]
Highway planned					0.290	
					(0.042)[a]	
					[0.013][a]	
					{0.032}[a]	
Geo. Controls	No	No	Yes	Yes	Yes	Yes
Obs.	18,242	17,210	16,944	16,944	16,944	16,944
R^2 (adj.)	0.089	0.098	0.158	0.173	0.242	0.118

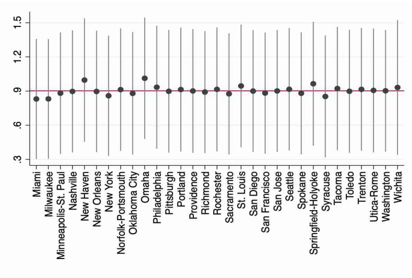
Note: Each column corresponds to a different regression. The unit of observation is a census tract. The dependent variable is an indicator if a highway was built or planned through the tract. The vector of controls includes the (log) area and slope of the tract, distance to the nearest river, an indicator if the governor of the state was a Republican, the state (log) number of car registrations per 10k inhabitants, and the distance to the 1921 railroad network. All columns include city fixed effects. Regressions are weighted by the census tract's population. Standard errors clustered at the city level in parentheses. Standard errors clustered at the census tract in 1950 level in squate brackets. Standard errors allowing for spatial correlation within 10 kilometers of a census tract's centroid in curly brackets. [a] indicates the coef. is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

D.3 Additional Figures

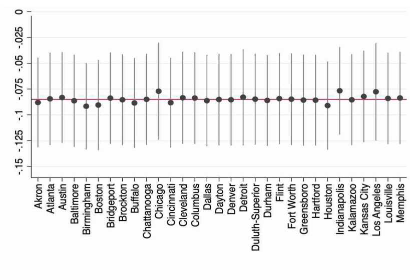
FIGURE D.1: Leave-one-out Analysis



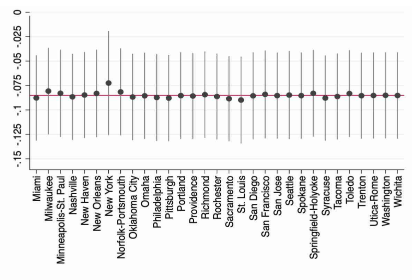
(A) Black share (1/2)



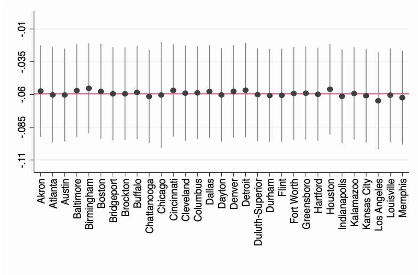
(B) Black share (2/2)



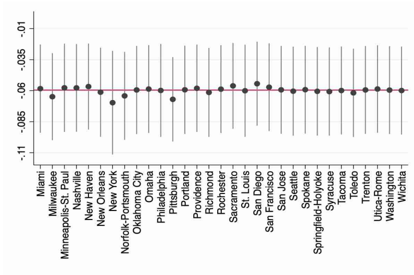
(C) Log median home value (1/2)



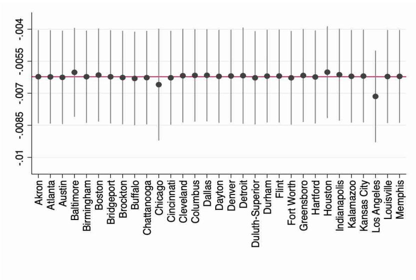
(D) Log median home value (2/2)



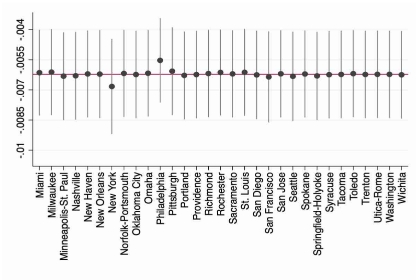
(E) Log median rent (1/2)



(F) Log median rent (2/2)



(G) Distance to city center (1/2)



(H) Distance to city center (2/2)

Note: Each figure present the results of estimating Equation 2 while leaving one city out of the sample each time. The first two panels show the estimated coefficients for the Black share of the city's population residing in the tract. The second two panels show the estimated coefficients for the log median home value. The third two panels show the estimated coefficients for the log median rent. The last two panels show the estimated coefficients for the distance to the city center. All regressions include city fixed effects, Black share, high school share, log median income, log median rent, log median home value, log area, log slope, log distance to the nearest river, log number of cars per 10k inhabitants, and an indicator if the governor was a Republican. Standard errors are clustered at the city level and observations are weighted by the tract's total population.

TABLE D.8: All Segments

	Displaced	Distance to the closest highway					Rest of the city
		0 and 100m	100 and 200m	200 and 300m	300 and 400m	400 and 500m	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Number of displaced individuals:							
Total population	356,314	2,318,919	2,150,826	2,202,002	2,197,029	2,119,511	33.816m
Population share	0.79%	5.13%	4.76%	4.88%	4.86%	4.69%	74.88%
Panel B: Individual level characteristics:							
Black	0.129	0.112	0.103	0.101	0.094	0.090	0.071
	(0.335)	(0.315)	(0.304)	(0.301)	(0.292)	(0.286)	(0.257)
Immigrant	0.155	0.159	0.163	0.166	0.167	0.165	0.144
	(0.362)	(0.366)	(0.370)	(0.372)	(0.373)	(0.371)	(0.351)
First gen. US born	0.137	0.145	0.146	0.146	0.147	0.144	0.135
	(0.344)	(0.352)	(0.353)	(0.353)	(0.354)	(0.351)	(0.341)
High school share	0.236	0.247	0.265	0.271	0.275	0.282	0.317
	(0.425)	(0.431)	(0.441)	(0.444)	(0.446)	(0.450)	(0.465)
Labor force	0.660	0.648	0.649	0.647	0.646	0.648	0.635
	(0.474)	(0.478)	(0.477)	(0.478)	(0.478)	(0.478)	(0.482)
Employed	0.894	0.901	0.906	0.907	0.907	0.909	0.923
	(0.308)	(0.299)	(0.292)	(0.290)	(0.290)	(0.288)	(0.267)
Occupational score	24.463	24.965	25.336	25.502	25.577	25.637	26.507
	(9.926)	(9.781)	(10.056)	(10.167)	(10.086)	(10.054)	(10.303)
Panel C: Household level characteristics:							
Homeowner	0.217	0.271	0.268	0.268	0.277	0.285	0.368
	(0.412)	(0.444)	(0.443)	(0.443)	(0.447)	(0.451)	(0.482)
Household size	3.362	3.458	3.444	3.452	3.451	3.451	3.508
	(2.110)	(2.071)	(2.088)	(2.051)	(2.059)	(2.031)	(1.925)
Log home value	10.583	10.626	10.684	10.720	10.743	10.774	10.872
	(0.985)	(0.943)	(0.957)	(0.949)	(0.945)	(0.929)	(0.899)
Log rent	5.755	5.828	5.900	5.940	5.959	5.963	6.020
	(0.778)	(0.822)	(0.831)	(0.836)	(0.838)	(0.831)	(0.827)

Note: Each observation corresponds to an individual in the 1940 census. Displacement and the proximity to highways are calculated for all segments of the IHS. Mean values are reported for each variable, with the corresponding standard deviation in parentheses. High school share and labor force are calculated for the sample of individuals aged 25 to 55. Employed is calculated for individuals aged 25 to 55 in the labor force. Occupational score is calculated for individuals aged 25 to 55 and employed. Home value and rent are conditional on ownership status.

E. Displacement Appendix

This section presents additional results for the main analysis of the paper. It includes a further discussion on the alternative empirical strategies used to estimate the effect of displacement on mortality and also supporting tables and figures.

E.1 Balance test of the samples

In this section, I present the balance test for all the individuals geocoded in 1940, rather than only those matched to the mortality records.

Table E.1: Balance Test — Full Sample

	Displaced	Control	Mean Test [Displaced = Control]	
Fixed Effects			No	Yes
	(1)	(2)	(3)	(4)
Age	31.669	31.884	-0.215	
	(19.464)	(19.286)	[0.005]	
Female	0.496	0.504	-0.008	
	(0.500)	(0.500)	[0.000]	
Race identified as Black	0.099	0.073	0.026	
	(0.299)	(0.261)	[0.000]	
Home owner	0.191	0.223	-0.032	
	(0.393)	(0.416)	[0.000]	
N. of subfamilies in the household	0.116	0.114	0.002	-0.001
	(0.361)	(0.355)	[0.256]	[0.610]
N. of own children in the household	0.734	0.731	0.003	0.039
	(1.355)	(1.307)	[0.529]	[0.000]
Married (indicator)	0.412	0.435	-0.024	-0.012
	(0.492)	(0.496)	[0.000]	[0.000]
(log) Monthly contract rent	5.836	5.992	-0.156	-0.107
	(0.679)	(0.746)	[0.000]	[0.000]
(log) House value	10.665	10.755	-0.090	-0.062
	(0.973)	(0.967)	[0.000]	[0.000]
College graduate	0.021	0.028	-0.006	-0.005
	(0.144)	(0.164)	[0.000]	[0.000]
High school graduate	0.163	0.192	-0.029	-0.028
	(0.369)	(0.394)	[0.000]	[0.000]
Middle school graduate	0.710	0.732	-0.023	-0.018
	(0.454)	(0.443)	[0.000]	[0.000]
Employed	0.849	0.864	-0.016	-0.014
	(0.358)	(0.342)	[0.000]	[0.000]
Labor force participation	0.557	0.562	-0.005	-0.008
	(0.497)	(0.496)	[0.038]	[0.000]
Occupational score	24.126	24.821	-0.695	-0.415
	(8.921)	(9.358)	[0.000]	[0.000]
Born outside the U.S.	0.192	0.199	-0.007	0.008
	(0.394)	(0.399)	[0.000]	[0.000]
Parents born outside the U.S.	0.213	0.214	-0.001	0.015
	(0.409)	(0.410)	[0.648]	[0.000]
Living in their birth state	0.580	0.594	-0.014	-0.005
	(0.494)	(0.491)	[0.000]	[0.001]
Same house as 5 years ago	0.353	0.374	-0.021	0.004
	(0.478)	(0.484)	[0.000]	[0.017]
Same community as 5 years ago	0.896	0.885	0.011	0.017
	(0.305)	(0.319)	[0.000]	[0.000]
Within county mig. in the last 5 years	0.566	0.540	0.026	0.006
	(0.496)	(0.498)	[0.000]	[0.001]
Within state mig. in the last 5 years	0.034	0.035	-0.000	-0.001
	(0.182)	(0.183)	[0.845]	[0.060]
Between state mig. in the last 5 years	0.043	0.044	-0.002	-0.008
	(0.202)	(0.206)	[0.029]	[0.000]

Note: Individuals in the 1940 full count census. Only segments opened between 1950 and 1960 are included in the sample. Column (1) reports the mean and standard deviation for displaced individuals. Column (2) reports the mean and standard deviation for individuals living between 100 and 200 meters of future highways. Columns (3) and (4) Wald-test of equality of averages for both samples, with p-values reported in square brackets. Column (3) does not include any fixed effect. Column (4) includes city, birth year, homeownership, race, and gender fixed effects.

E.2 Sensitivity of the Results

This section presents the results of the main analysis using different control groups.

TABLE E.2: Long-term Effects of Highway Construction — Control Group 1km

	Same Neigh. (1)	Same City (2)	Death Age (3)	Survival to age 70 (4)
Displaced	-0.014[a]	0.021	-0.148[a]	-0.008[a]
	(0.002)	(0.015)	(0.040)	(0.002)
Adjacent	-0.004[b]	0.021[a]	0.034	0.001
	(0.002)	(0.007)	(0.033)	(0.002)
Mean dep. var.	0.023	0.608	79.074	0.893
R-squared (adj)	0.023	0.018	0.864	0.526
Observations	37,960	37,960	35,919	35,919

Note: OLS estimates are reported. Only segments opened between 1950 and 1960 are included in the sample. An observation is an individual in the 1940 full count census matched to administrative mortality records. The sample consists of individuals who died after 1995. Displaced corresponds to individuals living in 1940 in houses destroyed by highway construction. Adjacent corresponds to individuals living in 1940 within 100 meters of future development. The control group is individuals living between 1,000 and 1,100 meters from a future highway. Each column corresponds to a different regression. The dependent variable in column (1) is an indicator that equals one if the individual lives at the time of death in the same neighborhood they lived in 1940. In column (2), the dependent variable is an indicator that equals one if the individual lives in the same city they lived in 1940. Column (3) uses the age at death as the dependent variable. Finally, the dependent variable in Column (4) is an indicator if the individual survived until the age of 70. All regressions control for race, gender at birth, homeownership, city, and birth year fixed effects. Coefficients are reported with standard errors clustered at the city where the individual lived in the 1940 level. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

TABLE E.3: Long-term Effects of Highway Construction — Control Group 2km

	Same Neigh. (1)	Same City (2)	Death Age (3)	Survival to age 70 (4)
Displaced	-0.016[a]	0.031[c]	-0.212[a]	-0.009[a]
	(0.003)	(0.018)	(0.045)	(0.003)
Adjacent	-0.006[a]	0.031[b]	-0.043	0.000
	(0.002)	(0.015)	(0.044)	(0.001)
Mean dep. var.	0.024	0.607	79.115	0.895
R-squared (adj)	0.025	0.019	0.864	0.528
Observations	33,847	33,847	31,990	31,990

Note: OLS estimates are reported. Only segments opened between 1950 and 1960 are included in the sample. An observation is an individual in the 1940 full count census matched to administrative mortality records. The sample consists of individuals who died after 1995. Displaced corresponds to individuals living in 1940 in houses destroyed by highway construction. Adjacent corresponds to individuals living in 1940 within 100 meters of future development. The control group is individuals living between 2,000 and 2,100 meters from a future highway. Each column corresponds to a different regression. The dependent variable in column (1) is an indicator that equals one if the individual lives at the time of death in the same neighborhood they lived in 1940. In column (2), the dependent variable is an indicator that equals one if the individual lives in the same city they lived in 1940. Column (3) uses the age at death as the dependent variable. Finally, the dependent variable in Column (4) is an indicator if the individual survived until the age of 70. All regressions control for race, gender at birth, homeownership, city, and birth year fixed effects. Coefficients are reported with standard errors clustered at the city where the individual lived in the 1940 level. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

TABLE E.4: Long-term Effects of Highway Construction — Control Group Rest of the City

	Same Neigh. (1)	Same City (2)	Death Age (3)	Survival to age 70 (4)
Displaced	-0.017[a]	0.040[b]	-0.189[a]	-0.009[a]
	(0.002)	(0.018)	(0.044)	(0.002)
Adjacent	-0.006[a]	0.041[a]	-0.025	-0.000
	(0.001)	(0.007)	(0.026)	(0.001)
Mean dep. var.	0.037	0.582	79.266	0.898
R-squared (adj)	0.027	0.024	0.865	0.524
Observations	874,641	874,641	828,993	828,993

Note: OLS estimates are reported. Only segments opened between 1950 and 1960 are included in the sample. An observation is an individual in the 1940 full count census matched to administrative mortality records. The sample consists of individuals who died after 1995. Displaced corresponds to individuals living in 1940 in houses destroyed by highway construction. Adjacent corresponds to individuals living in 1940 within 100 meters of future development. The control group is all individuals living in the city further than 200 meters of any highway segment opened. Each column corresponds to a different regression. The dependent variable in column (1) is an indicator that equals one if the individual lives at the time of death in the same neighborhood they lived in 1940. In column (2), the dependent variable is an indicator that equals one if the individual lives in the same city they lived in 1940. Column (3) uses the age at death as the dependent variable. Finally, the dependent variable in Column (4) is an indicator if the individual survived until the age of 70. All regressions control for race, gender at birth, homeownership, city, and birth year fixed effects. Coefficients are reported with standard errors clustered at the city where the individual lived in the 1940 level. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

TABLE E.5: Long-term Effects of Highway Construction — Unweighted

	Same Neigh. (1)	Same City (2)	Death Age (3)	Survival to age 70 (4)
Panel A: *near-versus-far* approach				
Displaced	-0.014[a]	0.014	-0.125[b]	-0.010[a]
	(0.002)	(0.014)	(0.051)	(0.003)
Adjacent	-0.004[b]	0.016[a]	-0.035	-0.001
	(0.002)	(0.004)	(0.038)	(0.002)
Mean dep. var.	0.021	0.607	78.257	0.848
R-squared (adj)	0.023	0.019	0.862	0.640
Observations	35,652	35,652	35,652	35,652
Panel B: *near-versus-far* approach + controls				
Displaced	-0.015[a]	0.011	-0.139[a]	-0.012[a]
	(0.003)	(0.015)	(0.050)	(0.003)
Adjacent	-0.005[a]	0.013[a]	-0.043	-0.001
	(0.002)	(0.004)	(0.040)	(0.002)
Mean dep. var.	0.022	0.610	79.029	0.892
R-squared (adj)	0.026	0.028	0.841	0.513
Observations	33,236	33,236	33,236	33,236
Panel C: matching on observables				
Displaced	-0.018[a]	0.032[c]	-0.152[a]	-0.006
	(0.002)	(0.017)	(0.052)	(0.004)
Adjacent	-0.008[a]	0.032[a]	0.020	0.004[c]
	(0.003)	(0.005)	(0.038)	(0.003)
Mean dep. var.	0.024	0.609	77.992	0.849
R-squared (adj)	0.019	0.012	0.852	0.626
Observations	20,935	20,935	20,935	20,935
Panel D: Federal engineering maps as control group				
Displaced	-0.015[a]	0.019	-0.072	-0.008[b]
	(0.003)	(0.020)	(0.048)	(0.003)
Adjacent	-0.005[b]	0.021[a]	0.019	0.002
	(0.002)	(0.007)	(0.023)	(0.002)
Mean dep. var.	0.023	0.611	78.369	0.851
R-squared (adj)	0.022	0.018	0.862	0.636
Observations	43,875	43,875	43,875	43,875

Note: OLS estimates are reported. Only segments opened between 1950 and 1960 are included in the sample. An observation is an individual in the 1940 full count census matched to administrative mortality records. The sample consists of individuals who died after 1995. Displaced corresponds to individuals living in 1940 in houses destroyed by highway construction. Adjacent corresponds to individuals living in 1940 within 100 meters of future development. The control group is individuals living between 100 and 200 meters from a future highway. Mortality variables are not weighted. Each column corresponds to a different regression. The dependent variable in column (1) is an indicator that equals one if the individual lives at the time of death in the same neighborhood they lived in 1940. In column (2), the dependent variable is an indicator that equals one if the individual lives in the same city they lived in 1940. Column (3) uses the age at death as the dependent variable. Finally, the dependent variable in Column (4) is an indicator if the individual survived until the age of 70. All regressions control for race, gender at birth, homeownership, city, and birth year fixed effects. Coefficients are reported with standard errors clustered at the city where the individual lived in the 1940 level. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

TABLE E.6: Long-term Effects of Highway Construction — All Highway Segments

	Same Neigh. (1)	Same City (2)	Death Age (3)	Survival to age 70 (4)
Displaced	-0.014[a]	0.013	-0.049	-0.004[a]
	(0.002)	(0.013)	(0.043)	(0.002)
Adjacent	-0.005[a]	0.011[a]	-0.059[a]	-0.003[a]
	(0.001)	(0.004)	(0.022)	(0.001)
Mean dep. var.	0.028	0.600	79.094	0.892
R-squared (adj)	0.026	0.027	0.864	0.533
Observations	85,877	85,877	81,153	81,153

Note: OLS estimates are reported. All highway segments are included in the sample. An observation is an individual in the 1940 full count census matched to administrative mortality records. Displaced corresponds to individuals living in 1940 in houses destroyed by highway construction. Adjacent corresponds to individuals living in 1940 within 100 meters of future development. The control group is individuals living between 100 and 200 meters from a future highway. Each column corresponds to a different regression. The dependent variable in column (1) is an indicator that equals one if the individual lives at the time of death in the same neighborhood they lived in 1940. In column (2), the dependent variable is an indicator that equals one if the individual lives in the same city they lived in 1940. Column (3) uses the age at death as the dependent variable. Finally, the dependent variable in Column (4) is an indicator if the individual survived until the age of 70. All regressions control for race, gender at birth, homeownership, city, and birth year fixed effects. Coefficients are reported with standard errors clustered at the city where the individual lived in the 1940 level. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

TABLE E.7: Long-term Effects of Highway Construction – All Death Years

	Same Neigh. (1)	Same City (2)	Death Age (3)	Survival to age 70 (4)
Displaced	-0.016^{a}	0.010	-0.127^{c}	-0.010^{a}
	(0.002)	(0.015)	(0.069)	(0.004)
Adjacent	-0.005^{a}	0.014^{a}	-0.029	0.000
	(0.002)	(0.004)	(0.035)	(0.002)
Mean dep. var.	0.023	0.616	77.537	0.825
R-squared (adj)	0.029	0.018	0.782	0.546
Observations	44,005	44,005	44,005	44,005

Note: OLS estimates are reported. Only segments opened between 1950 and 1960 are included in the sample. An observation is an individual in the 1940 full count census matched to administrative mortality records. The sample consists of individuals who died between 1988 and 2005. Displaced corresponds to individuals living in 1940 in houses destroyed by highway construction. Adjacent corresponds to individuals living in 1940 within 100 meters of future development. The control group is individuals living between 100 and 200 meters from a future highway. Each column corresponds to a different regression. The dependent variable in column (1) is an indicator that equals one if the individual lives at the time of death in the same neighborhood they lived in 1940. In column (2), the dependent variable is an indicator that equals one if the individual lives in the same city they lived in 1940. Column (3) uses the age at death as the dependent variable. Finally, the dependent variable in Column (4) is an indicator if the individual survived until the age of 70. All regressions control for race, gender at birth, homeownership, city, and birth year fixed effects. Coefficients are reported with standard errors clustered at the city where the individual lived in the 1940 level. ^{a} indicates the coefficient is significant at the 1%, ^{b} at the 5%, and ^{c} at the 10% level.

E.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. To keep a comparable sample, I only include individuals who were living in 1940 within 200 meters of a future highway and estimate a Cox Proportional Hazard Model of the effect of highway construction on the hazard rate of dying. Similar as the analysis in the main text, I will only focus on highway segments that opened between 1950 and 1960.

$$\lambda(t|\mathbf{x}_i) = \lambda_0(t,\alpha)exp\left(\beta_1\text{Disp}_i + \beta_2\text{Adj}_i + \Gamma'\mathbf{X}_i\right) \tag{5}$$

where $\lambda(t|\mathbf{x})$ is the time elapsed to an individuals' death, $\lambda_0(t, \alpha)$ is the baseline hazard, α are the parameters of the baseline hazard, β_1 and β_2 are the coefficients for displaced and adjacent individuals, respectively, and γ are the coefficients of the covariates $\mathbf{X}$. The vector of controls $\mathbf{X}$ includes race, gender at birth, homeownership, and city fixed effects. I control for a fourth-degree polynomial in age, although the Cox model partially accounts for age.[56] 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:

$$\frac{\lambda(t|\text{Disp} = 1, \mathbf{X}_i)}{\lambda(t|\text{Disp} = 0, \mathbf{X}_i)} = exp(\beta_1) \tag{6}$$

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 $1 - exp(\beta_1)$ times the original hazard. [57]

The results are reported in Appendix Table E.8. I report hazard rates, where a hazard rate of 1.1 indicates a 10 percent higher probability of dying at that age compared to the reference group. The preferred specification is column (4), which controls for race, gender at birth, homeownership status, a fourth-degree polynomial term on age, and city-fixed effects. The estimates suggest that displaced individuals have a 7.5% higher risk of early death over the study period than their peers. I find no significant effect for individuals living close to future highway construction.

The hazard rate estimates for displacement are significant and align with those found for other traumatic life events. For instance, Song et al. (2019) report that parents in the US who have lost a child face a 32% higher risk of early death compared to peers who have not experienced such a loss. Similarly, Sbarra et al. (2011) find that individuals who have gone through a divorce have a mortality hazard rate 1.23 times higher than that of the general population. In another study, Meyer et al. (2023) shows that the homeless population in the US is 56% more likely to die at any age than individuals living under the poverty line.[58] In comparison, my preferred estimates suggest that the effect of displacement on mortality is equivalent to 23.5% of the impact of losing a child, 32.6% of the impact of a divorce, and 13.4% of the impact of homelessness.

[56] Due to computational constraints, I control for a fourth-degree polynomial in age instead of birth year fixed effect.

[57] This interpretation comes from the noncalculus results that $log\{\beta\} \simeq 1 + \beta$.

[58] The estimate increases to 4.6 when comparing the homeless population to the entire US population, rather than just those under the poverty line.

TABLE E.8: Cox Proportional Hazard Model

	(1)	(2)	(3)	(4)
Displaced	1.078^{a}	1.076^{a}	1.077^{a}	1.075^{a}
	(0.015)	(0.012)	(0.015)	(0.014)
Adjacent	1.017^{c}	1.019^{c}	1.015	1.015
	(0.009)	(0.010)	(0.013)	(0.014)
Black		1.134^{a}	1.076^{a}	1.070^{c}
		(0.029)	(0.030)	(0.042)
Female		0.838^{a}	0.871^{a}	0.870^{a}
		(0.007)	(0.008)	(0.007)
Home owner		0.994	0.990	0.983
		(0.013)	(0.016)	(0.017)
Observations	33,965	33,965	33,965	33,965
Fourth-degree Age polynomial	N	N	Y	Y
City Fixed Effect	N	N	N	Y

Note: The table reports hazard rates from a Cox Proportional Hazard Model. Only segments opened between 1950 and 1960 are included in the sample. An observation is an individual in the 1940 full count census matched to administrative mortality records. The sample consists of individuals who died between 1995 and 2005. Displaced corresponds to individuals living in 1940 in houses destroyed by highway construction. Adjacent corresponds to individuals living in 1940 within 100 meters of future development. The control group is individuals living between 100 and 200 meters from a future highway. Each column corresponds to a different regression. A hazard rate of 1.1 denotes an approximately 10 percent higher probability of dying at time t than the reference group. The outcome of interest is the age at death. Column (1) only includes the displacement and adjacent indicators. Column (2) includes a set of individual controls: gender, race, homeownership status, and a fourth-degree polynomial on age. Finally, column (3) includes city-fixed effects. Coefficients are reported with standard errors clustered at the city where the individual lived in the 1940 level. ^{a} indicates the coefficient is significant at the 1%, ^{b} at the 5%, and ^{c} at the 10% level.

E.4 Matching

Section 5 presents the estimates using a close versus far approach. The sample used is balanced in many individual characteristics, as shown by Table 4. However, there are some minor differences in the balance of certain characteristics, such as property prices and high school shares. These differences may raise questions concerning the validity of the estimates. Comparing displaced individuals to those who were not displaced may lead to biased estimates if the two groups differ in unobservable characteristics that are correlated with the outcome, as shown in Appendix Table E.9. To address these concerns, I employ caliper matching to choose a control group whose distribution of observable covariates is similar to the distribution of covariates among displaced individuals. To avoid possible spillovers from proximity to highway construction, I restrict the pool of potential controls to individuals living further than two kilometers from the highway construction. I performed exact matching on individual characteristics such as race, gender, city, employment status, high school education, and homeownership status. Additionally, I allowed a caliper of two years for birth year and one standard deviation in household income. Given recent findings on the importance of neighborhood characteristics for long-term outcomes (Chetty et al., 2014), I also included neighborhood-level characteristics in the matching procedure. Specifically, I performed exact matching on the redlining status of the neighborhood in 1940, and a caliper of one standard deviation on the neighborhood's Black population share, employment rate, high school graduation rate, homeownership rate, average household income, home value, rent, and educational mobility.[59],[60]

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 Table E.10. 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.

I re-estimate equation 4 using the matched sample. The estimates have causal interpretation if, conditional on the characteristics used by states and local officials to select highway routes, displacement is independent of potential outcomes. The matching procedure is successful in balancing the observable characteristics between the displaced and control groups, lending credibility to the estimates. Appendix Table E.11 compares the average characteristics of the displaced and control groups after matching. For characteristics that were matched exactly, the balance is mechanically achieved. For example, the Black and female shares are identical between the displaced and control groups. Balance is also achieved for characteristics used in the caliper, such as household income, neighborhood Black share, educational mobility, and property values. Some minor differences remain in the balance of other characteristics, such as average rent and income. Although the differences are statistically significant for both characteristics, they are small in magnitude. The matching procedure also achieves balance in characteristics that were not used in the matching process, such as the household education of the parent and migration within the last five years. This suggests that the estimates do not arise from differences in observable characteristics between the displaced and control groups.

Panel C of Table 5 replicates the main results using the matched sample. The estimates

[59] The calipers are neighborhood Black share (0.178), employment share (0.052), educational mobility (0.133), high school share (0.196), homeownership rate (0.211), income (397.77), home value (6315.811), rent (241.873). The caliper for household income is 1934.749.

[60] The educational mobility is constructed following Card et al. (2022). In particular, I estimate the fraction of 14 to 18-year-old boys and 14 to 16-year-old girls in each neighborhood with nine or more years of schooling from households where the most educated parent has between 5 and 8 years of schooling. I used the enumeration districts in 1940 as the neighborhood definition because it allowed me to estimate educational mobility without relying on geocoding. Derenoncourt (2022) also uses this definition of educational mobility.

are similar to the ones presented for the other strategies. Displaced individuals are 1.7 percentage points more likely to migrate out of their neighborhood. They also die 0.235 years earlier than their peers who were not displaced. Finally, displaced individuals are 0.6 percentage less likely to survive until the age of 70. Overall, the results under matching are virtually the same as the ones in 5, lending credibility to the causal interpretation of the results.

TABLE E.9: Descriptive statistics before matching

	Full sample	≥2 km	Close	Test [Close = ≥2 km]	
	(1)	(2)	(3)	(3)	(4)
Black	0.056	0.044	0.102	0.057	0.048
	(0.229)	(0.206)	(0.302)	[0.000]	[0.000]
Age	32.493	32.658	31.810	-0.847	-0.687
	(19.504)	(19.538)	(19.344)	[0.000]	[0.000]
Female	0.512	0.515	0.503	-0.012	-0.013
	(0.500)	(0.500)	(0.500)	[0.000]	[0.000]
Household homeownership	0.403	0.436	0.267	-0.169	-0.168
	(0.491)	(0.496)	(0.442)	[0.000]	[0.000]
Household max educ: HS	0.525	0.548	0.433	-0.115	-0.135
	(0.499)	(0.498)	(0.495)	[0.000]	[0.000]
Household max educ: College	0.122	0.131	0.083	-0.048	-0.058
	(0.327)	(0.338)	(0.276)	[0.000]	[0.000]
Household total income	1805.462	1858.155	1587.274	-270.881	-286.240
	(1,934.749)	(1,884.859)	(2,114.998)	[0.000]	[0.000]
Household employment	0.916	0.922	0.892	-0.030	-0.030
	(0.277)	(0.269)	(0.310)	[0.000]	[0.000]
Household same house last 5 years	0.381	0.394	0.328	-0.066	-0.035
	(0.486)	(0.489)	(0.470)	[0.000]	[0.000]
Household moved state last 5 years	0.045	0.044	0.048	0.004	-0.003
	(0.207)	(0.206)	(0.214)	[0.000]	[0.000]
Household in a redlined neighborhood	0.230	0.177	0.449	0.272	0.282
	(0.421)	(0.382)	(0.497)	[0.000]	[0.000]
Neighborhood avg rent	84.646	91.352	56.900	-34.451	-30.104
	(241.873)	(257.406)	(159.553)	[0.000]	[0.000]
Neighborhood avg home value	4729.661	4868.737	4137.900	-730.836	-993.237
	(6,315.811)	(3,906.612)	(12010.316)	[0.000]	[0.000]
Neighborhood avg occupational score	26.589	26.985	24.951	-2.034	-2.074
	(3.148)	(3.011)	(3.174)	[0.000]	[0.000]
Neighborhood income	1183.304	1231.733	982.864	-248.869	-269.116
	(397.768)	(392.831)	(352.833)	[0.000]	[0.000]
Neighborhood homeownership share	0.394	0.426	0.261	-0.164	-0.163
	(0.211)	(0.203)	(0.190)	[0.000]	[0.000]
Neighborhood HS share	0.305	0.323	0.230	-0.093	-0.116
	(0.196)	(0.198)	(0.170)	[0.000]	[0.000]
Neighborhood avg educational mobility	0.259	0.257	0.265	0.009	0.024
	(0.133)	(0.133)	(0.132)	[0.000]	[0.000]
Neighborhood avg employment share	0.925	0.932	0.896	-0.035	-0.038
	(0.052)	(0.047)	(0.064)	[0.000]	[0.000]
Neighborhood Black share	0.057	0.045	0.103	0.058	0.048
	(0.178)	(0.154)	(0.249)	[0.000]	[0.000]

Column (1), (2), and (3) present the mean values of the variables. Standard errors in parentheses. Column (1) corresponds to the full sample. Column (2) corresponds to households further than 2 km from a future highway, while column (3) corresponds to households within 300 meters of a future highway. Column (4) presents the differences between the means of the two groups, with p-values in brackets. Column (5) presents the differences controlling for city fixed effects, with p-values in brackets.

TABLE E.10: Balance between matched and unmatched displaced individuals

	Displaced	Matched	Unmatched	Test [M = U]	
	(1)	(2)	(3)	(3)	(4)
Black	0.085	0.013	0.160	-0.147	-0.152
	(0.279)	(0.113)	(0.367)	[0.000]	[0.000]
Age	18.738	18.266	19.227	-0.961	-1.355
	(8.841)	(8.327)	(9.320)	[0.000]	[0.000]
Female	0.511	0.501	0.521	-0.020	-0.022
	(0.500)	(0.500)	(0.500)	[0.114]	[0.139]
Employment	0.325	0.319	0.331	-0.011	0.003
	(0.468)	(0.466)	(0.471)	[0.336]	[0.835]
Labor force participation	0.397	0.380	0.415	-0.035	-0.037
	(0.489)	(0.485)	(0.493)	[0.004]	[0.011]
Household homeownership	0.229	0.254	0.203	0.051	0.076
	(0.420)	(0.435)	(0.402)	[0.000]	[0.000]
Household max educ: HS	0.457	0.473	0.439	0.034	0.065
	(0.498)	(0.499)	(0.496)	[0.006]	[0.000]
Household max educ: College	0.068	0.058	0.079	-0.022	-0.016
	(0.252)	(0.233)	(0.270)	[0.001]	[0.036]
Household total income	1482.295	1596.557	1363.934	232.623	192.006
	(1,350.090)	(1,161.660)	(1,511.966)	[0.000]	[0.000]
Household employment	0.909	0.952	0.864	0.088	0.135
	(0.287)	(0.213)	(0.343)	[0.000]	[0.000]
Household same house last 5 years	0.296	0.338	0.253	0.085	0.026
	(0.457)	(0.473)	(0.435)	[0.000]	[0.057]
Household moved state last 5 years	0.058	0.049	0.067	-0.019	-0.011
	(0.234)	(0.215)	(0.251)	[0.001]	[0.134]
Household in a redlined neighborhood	0.515	0.444	0.589	-0.145	-0.232
	(0.500)	(0.497)	(0.492)	[0.000]	[0.000]
Neighborhood average rent	50.694	46.797	54.749	-7.952	-33.413
	(137.930)	(48.569)	(190.639)	[0.020]	[0.000]
Neighborhood average home value	3675.705	3898.496	3435.714	462.783	22.401
	(2,698.028)	(2,240.584)	(3,099.067)	[0.000]	[0.753]
Neighborhood avg occupational score	24.543	25.651	23.394	2.257	2.031
	(2.796)	(2.127)	(2.939)	[0.000]	[0.000]
Neighborhood income	920.125	1039.258	796.719	242.539	199.621
	(311.191)	(291.399)	(281.575)	[0.000]	[0.000]
Neighborhood homeownership rate	0.247	0.291	0.202	0.089	0.132
	(0.174)	(0.177)	(0.158)	[0.000]	[0.000]
Neighborhood HS share	0.212	0.217	0.207	0.010	0.048
	(0.165)	(0.164)	(0.166)	[0.018]	[0.000]
Neighborhood educational mobility	0.263	0.297	0.228	0.069	0.020
	(0.127)	(0.120)	(0.123)	[0.000]	[0.000]
Neighborhood employment share	0.890	0.901	0.878	0.023	0.044
	(0.062)	(0.049)	(0.072)	[0.000]	[0.000]
Neighborhood Black share	0.099	0.025	0.175	-0.150	-0.143
	(0.232)	(0.098)	(0.297)	[0.000]	[0.000]

The table presents a balance test for the matched and unmatched displaced individuals after caliper matching. Columns (1), (2), and (3) present the mean values of the variables. Standard errors in parentheses. Column (1) corresponds to the full sample. Column (2) corresponds to households further than 2 km from a future highway, while column (3) corresponds to households within 300 meters of a future highway. Column (4) presents the differences between the means of the two groups, with p-values in brackets. Column (5) presents the differences controlling for city fixed effects, with p-values in brackets.

TABLE E.11: Balance test for matched and control group

	Full sample	Displaced	Control	Test [Displaced = Control]	
	(1)	(2)	(3)	(3)	(4)
Black	0.013	0.013	0.013	0.000	0.000
	(0.113)	(0.113)	(0.113)	[1.000]	[1.000]
Age	18.278	18.266	18.290	-0.024	-0.024
	(8.325)	(8.327)	(8.324)	[0.905]	[0.904]
Female	0.501	0.501	0.501	0.000	-0.000
	(0.500)	(0.500)	(0.500)	[1.000]	[1.000]
Employment	0.319	0.319	0.319	0.000	-0.000
	(0.466)	(0.466)	(0.466)	[1.000]	[1.000]
Labor force participation	0.378	0.380	0.377	0.003	0.003
	(0.485)	(0.485)	(0.485)	[0.781]	[0.780]
Household homeownership	0.254	0.254	0.254	0.000	0.000
	(0.435)	(0.435)	(0.435)	[1.000]	[1.000]
Household max educ: HS	0.477	0.473	0.480	-0.007	-0.007
	(0.499)	(0.499)	(0.500)	[0.589]	[0.573]
Household max educ: College	0.061	0.058	0.065	-0.008	-0.008
	(0.240)	(0.233)	(0.247)	[0.184]	[0.178]
Household total income	1600.412	1596.557	1604.267	-7.709	-7.709
	(1,147.061)	(1,161.660)	(1,132.434)	[0.784]	[0.782]
Household employment	0.952	0.952	0.952	0.000	0.000
	(0.213)	(0.213)	(0.213)	[1.000]	[1.000]
Household same house last 5 years	0.333	0.338	0.329	0.009	0.009
	(0.471)	(0.473)	(0.470)	[0.450]	[0.447]
Household moved state last 5 years	0.048	0.049	0.047	0.002	0.002
	(0.214)	(0.215)	(0.212)	[0.730]	[0.721]
Household in a redlined neighborhood	0.444	0.444	0.444	0.000	0.000
	(0.497)	(0.497)	(0.497)	[1.000]	[1.000]
Neighborhood average rent	48.276	46.797	49.755	-2.958	-2.958
	(47.643)	(48.569)	(46.659)	[0.011]	[0.010]
Neighborhood average home value	3936.283	3898.496	3974.069	-75.573	-75.573
	(2,185.794)	(2,240.584)	(2,129.262)	[0.159]	[0.109]
Neighborhood avg occupational score	25.919	25.651	26.186	-0.535	-0.535
	(2.197)	(2.127)	(2.233)	[0.000]	[0.000]
Neighborhood income	1059.737	1039.258	1080.217	-40.959	-40.959
	(287.028)	(291.399)	(281.145)	[0.000]	[0.000]
Neighborhood homeownership rate	0.315	0.291	0.339	-0.048	-0.048
	(0.180)	(0.177)	(0.181)	[0.000]	[0.000]
Neighborhood HS share	0.227	0.217	0.238	-0.021	-0.021
	(0.164)	(0.164)	(0.165)	[0.000]	[0.000]
Neighborhood educational mobility	0.297	0.297	0.297	0.001	0.001
	(0.121)	(0.120)	(0.122)	[0.789]	[0.754]
Neighborhood employment share	0.906	0.901	0.910	-0.009	-0.009
	(0.047)	(0.049)	(0.045)	[0.000]	[0.000]
Neighborhood Black share	0.024	0.025	0.023	0.002	0.002
	(0.098)	(0.098)	(0.098)	[0.359]	[0.307]

Balance test between matched and control group after caliper matching. Columns (1), (2), and (3) present the mean values of the variables. Standard errors in parentheses. Column (1) corresponds to the full sample. Column (2) corresponds to households displaced by highway construction, while column (3) corresponds to matched control group. Column (4) presents the differences between the means of the two groups, with p-values in brackets. Column (5) presents the differences controlling for city fixed effects, with p-values in brackets.

E.5 Planned highways as potential control group

A natural control group for displaced individuals are individuals living near highways that were planned but never built. These individuals are similar to displaced individuals in that they were living near a planned highway, but were not displaced. However, the fact that some segments were built and other were not may indicate the presence of ommitted variables that can affect the interpretation of these estimates. Mowitz and Wright (1962) presents anecdotal evidence in favor of this interpretation. When discussing the construction of their highway network, councilman Del Smith, of Detroit City Council, raised the question as to wheter the city was constrained by the routes indicated in the map. State officials responded that the map were not a legal contract, but rather a guideline. In addition, the results of Table 2 show that highways the racial composition of a tract predicts the likelihood of a highway being built, but not the planned routes. As a consequence, the estimates presented in this section should be interpreted with caution. I estimate equation 4 using individuals living near planned highways as a control group. The sample consists of individuals living in cities with a planned highway and with highway segments opened between 1950 and 1960.

Table 5 Panel (D) presents the results of estimating the effect of displacement on long-term outcomes using individuals living near planned highways as a control group. The results mirror the ones using the other specifications. Individuals displaced by highway construction are more likely to leave their neighborhood, die earlier, and less likely to survive until the age of 70. Table 6 Panel (D) also finds that affected individuals die in neighborhoods with worse socio-economic characteristics. The estimates are slightly larger that the preferred specification, which is consistent with the historical accounts that mentioned that the local opposition to highway construction was more successful when the communities' connections to local political and economic elites were stronger (Rose and Mohl, 2012).

Table E.12: Balance Test — Federal Engineering Maps Sample

	Displaced	Control	Mean Test [Displaced = Control]	
Fixed Effects			No	Yes
	(1)	(2)	(3)	(4)
Age	18.219	18.578	-0.359	
	(7.490)	(7.574)	[0.039]	
Female	0.520	0.528	-0.009	
	(0.500)	(0.499)	[0.459]	
Race identified as Black	0.073	0.047	0.026	
	(0.260)	(0.211)	[0.000]	
Home owner	0.191	0.240	-0.049	
	(0.394)	(0.427)	[0.000]	
N. of subfamilies in the household	0.107	0.102	0.006	0.005
	(0.343)	(0.332)	[0.450]	[0.529]
N. of own children in the household	0.238	0.242	-0.004	0.015
	(0.709)	(0.722)	[0.807]	[0.267]
Married (indicator)	0.209	0.225	-0.016	-0.006
	(0.407)	(0.418)	[0.093]	[0.413]
(log) Monthly contract rent	5.843	5.990	-0.147	-0.110
	(0.627)	(0.711)	[0.000]	[0.000]
(log) House value	10.641	10.733	-0.092	-0.072
	(0.920)	(0.926)	[0.044]	[0.097]
College graduate	0.024	0.026	-0.002	0.001
	(0.153)	(0.158)	[0.620]	[0.864]
High school graduate	0.237	0.268	-0.032	-0.029
	(0.425)	(0.443)	[0.002]	[0.001]
Middle school graduate	0.800	0.803	-0.003	0.005
	(0.400)	(0.398)	[0.731]	[0.320]
Employed	0.793	0.814	-0.022	-0.019
	(0.406)	(0.389)	[0.130]	[0.184]
Labor force participation	0.537	0.538	-0.001	-0.000
	(0.499)	(0.499)	[0.913]	[0.980]
Occupational score	22.856	23.516	-0.660	-0.203
	(7.439)	(7.935)	[0.028]	[0.454]
Born outside the U.S.	0.041	0.044	-0.003	0.002
	(0.197)	(0.204)	[0.523]	[0.690]
Parents born outside the U.S.	0.384	0.379	0.006	0.031
	(0.487)	(0.485)	[0.622]	[0.002]
Living in their birth state	0.761	0.791	-0.030	-0.017
	(0.426)	(0.406)	[0.002]	[0.057]
Same house as 5 years ago	0.343	0.371	-0.028	0.005
	(0.475)	(0.483)	[0.014]	[0.624]
Same community as 5 years ago	0.878	0.877	0.002	0.010
	(0.327)	(0.329)	[0.841]	[0.163]
Within county mig. in the last 5 years	0.563	0.536	0.027	0.003
	(0.496)	(0.499)	[0.023]	[0.765]
Within state mig. in the last 5 years	0.039	0.038	0.001	-0.001
	(0.194)	(0.192)	[0.870]	[0.757]
Between state mig. in the last 5 years	0.051	0.051	0.000	-0.007
	(0.221)	(0.220)	[0.980]	[0.137]

Note: Individuals in the 1940 full count census matched to administrative mortality records are included in the sample. Only segments opened between 1950 and 1960 are included in the sample. Column (1) reports the mean and standard deviation for displaced individuals. Column (2) reports the mean and standard deviation for individuals living between 100 and 200 meters of future highways. Columns (3) and (4) Wald-test of equality of averages for both samples, with p-values reported in square brackets. Column (3) does not include any fixed effect. Column (4) include city, birth year, homeownership, race, and gender fixed effects.

E.6 Placebo Exercise

I test the hypothesis that the results are driven by the location of the highway plans, rather than a direct effect of displacement. To do this, I use the Federal Engineering maps as placebo highways and create a sample of individuals living near these planned highways. I then repeat the exercise of comparing affected individuals to those living between 100 and 200 meters away.[61] The results show no significant effect of living close to a planned highway on individuals' long-term outcomes. Tables E.13 and E.14 show 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 the location of the highway plans drives the results.

TABLE E.13: Placebo: Long-term Effects of Federal Engineering Plans

	Same Neigh. (1)	Same City (2)	Death Age (3)	Survival to age 70 (4)
Displaced	0.002	-0.007	0.040	0.001
	(0.002)	(0.007)	(0.041)	(0.004)
Adjacent	0.000	0.000	-0.018	-0.001
	(0.001)	(0.004)	(0.022)	(0.001)
Mean dep. var.	0.028	0.613	79.177	0.897
R-squared (adj)	0.024	0.025	0.864	0.516
Observations	57,515	57,515	54,436	54,436

Note: OLS estimates are reported. Only highways planned in the Yellow Books are included in the sample. An observation is an individual in the 1940 full count census matched to administrative mortality records. The sample consists of individuals who died between 1995 and 2005. Displaced corresponds to individuals living in 1940 in houses potentially destroyed by planned highways. Adjacent corresponds to individuals living in 1940 within 100 meters from a planned highway. The control group is individuals living between 100 and 200 meters from a planned highway. Each column corresponds to a different regression. The dependent variable in column (1) is an indicator that equals one if the individual lives at the time of death in the same neighborhood they lived in 1940. In column (2), the dependent variable is an indicator that equals one if the individual lives in the same city they lived in 1940. Column (3) uses the age at death as the dependent variable. Finally, the dependent variable in Column (4) is an indicator if the individual survived until the age of 70. All regressions control for race, gender at birth, homeownership, city, and birth year fixed effects. Coefficients are reported with standard errors clustered at the city where the individual lived in 1940 level. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

[61] Planned highways consists of a unidirectional segment of the highway. I assume that planned highways had traffic in both directions and assume that they have four lanes each way, each lane with a width of 3.6 meters.

TABLE E.14: Placebo: Neighborhood Characteristics at Time of Death for Federal Engineering Maps

	High School Share (1)	College Share (2)	Employment Share (3)	Homeownership Share (4)	Log Median Income (5)	Log Median Home Value (6)
Displaced	0.136	0.178	0.001	0.168	0.002	0.001
	(0.132)	(0.251)	(0.001)	(0.176)	(0.004)	(0.007)
Adjacent	0.204[b]	0.239	0.002[b]	0.073	0.005[c]	0.004
	(0.099)	(0.145)	(0.001)	(0.131)	(0.003)	(0.004)
Mean dep. var.	85.046	34.505	0.582	67.347	10.963	12.088
R-squared (adj)	0.139	0.101	0.058	0.079	0.110	0.251
Observations	76,803	76,803	76,990	76,935	76,703	75,393

Note: OLS estimates are reported. Only highways planned in the Yellow Books are included in the sample. An observation is an individual in the 1940 full count census matched to administrative mortality records. The sample consists of individuals who died between 1995 and 2005. Displaced corresponds to individuals living in 1940 in houses potentially destroyed by planned highways. Adjacent corresponds to individuals living in 1940 within 100 meters from a planned highway. The control group is individuals living between 100 and 200 meters from a planned highway. Each column corresponds to a different regression. The dependent variables correspond to the neighborhood-level characteristics of the residence at time of death. The dependent variable in column (1) corresponds to the share of adults living in the neighborhood who have completed high school. In column (2), the dependent variable is the college share. Column (3) uses the employment share as the dependent variable. Columns (4) and (5) use the log median income and log median house value, respectively. Finally, column (6) uses the log average rent for a two-bedroom apartment as dependent variable. All regressions control for race, gender at birth, homeownership, city, and birth year fixed effects. Coefficients are reported with standard errors clustered at the city where the individual lived in 1940 level. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

E.7 Contamination bias on the estimates

In this section I test if there is contamination bias in the estimates. Contamination bias arises when two mutually exclusive treatments are regressed including controls. I follow Goldsmith-Pinkham et al. (2024) and present the results for what they called "own" effect.

TABLE E.15: Contamination Bias

	Same Neigh. (1)	Same City (2)	Death Age (3)	Survival to age 70 (4)
Panel A: *near-versus-far* approach				
Displaced	-0.014[a]	0.014	-0.234[a]	-0.010[a]
	(0.002)	(0.014)	(0.044)	(0.003)
Adjacent	-0.004[b]	0.016[a]	-0.070[c]	-0.002
	(0.002)	(0.004)	(0.036)	(0.001)
Observations	35,654	35,654	33,713	33,713
Panel B: *near-versus-far* approach + controls				
Displaced	-0.015[a]	0.009	-0.256[a]	-0.011[a]
	(0.003)	(0.015)	(0.041)	(0.003)
Adjacent	-0.005[a]	0.013[a]	-0.071[b]	-0.001
	(0.002)	(0.004)	(0.036)	(0.001)
Observations	33,238	33,238	32,282	32,282
Panel C: matching on observables				
Displaced	-0.018[a]	0.035[b]	-0.249[a]	-0.003
	(0.002)	(0.017)	(0.066)	(0.004)
Adjacent	-0.007[b]	0.033[a]	0.061	0.006[a]
	(0.003)	(0.006)	(0.045)	(0.002)
Observations	19,399	19,399	18,365	18,365
Panel D: Federal engineering maps as control group				
Displaced	-0.014[a]	0.023	-0.162[a]	-0.007[a]
	(0.002)	(0.020)	(0.052)	(0.003)
Adjacent	-0.005[b]	0.022[a]	0.009	0.001
	(0.002)	(0.007)	(0.024)	(0.001)
Observations	43,878	43,878	41,601	41,601

Note: OLS estimates are reported. Only segments opened between 1950 and 1960 are included in the sample. An observation is an individual in the 1940 full count census matched to administrative mortality records. The sample consists of individuals who died after 1995. Displaced corresponds to individuals living in 1940 in houses destroyed by highway construction. Adjacent corresponds to individuals living in 1940 within 100 meters of future development. The control group is individuals living between 100 and 200 meters from a future highway. I perform Goldsmith-Pinkham et al.'s 2024 correction for contamination bias. Each column corresponds to a different regression. The dependent variable in column (1) is an indicator that equals one if the individual lives at time of death in the same neighborhood they lived in 1940. In column (2), the dependent variable is an indicator that equals one if the individual lives in the same city they lived in 1940. Column (3) uses the age at death as the dependent variable. Finally, the dependent variable in Column (4) is an indicator if the individual survived until the age of 70. All regressions control for race, gender at birth, homeownership, city, and birth year fixed effects. Coefficients are reported with standard errors clustered at the city where the individual lived in 1940 level. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

E.8 Adjusting estimates from moving out of the 1940 house

The displacement gap captures differences between individuals who lived in a house that was destroyed by a highway and those who 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 1940 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. Also, by FWL we can only focus on the displacement effect, purging out the influence of the controls (home-ownership, race, gender, city, and birth year). The estimated displacement gap is given by:

$$y_i = \alpha + \beta \cdot D_i^{40} + \epsilon_i$$

where D_i^{40} is an indicator that equals one if the individual lived in a house in 1940 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 desired parameter.

The estimated displacement gap in Section 5.2 can be written as:

$$\hat{\beta} \xrightarrow{p} \mathbb{E}[y_i|D_i^{40} = 1] - \mathbb{E}[y_i|D_i^{40} = 0] = b \cdot \Big(\mathbb{E}[D_i^{56}|D_i^{40} = 1] - \mathbb{E}[D_i^{56}|D_i^{40} = 0]\Big) \tag{7}$$

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 1940 to 1956, we would have that $\hat{\beta} \xrightarrow{p} b$.[62] I assume that 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 1940 than if you were not, we lead to $\hat{\beta} \xrightarrow{p} b \cdot \left(\mathbb{P}(D_i^{56} = 1|D_i^{40} = 1) - \mathbb{P}(D_i^{56} = 1|D_i^{40} = 0)\right)$.[63]

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

[63] 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.

Thus, we can bound the true effect of displacement as:

$$b \in [\hat{\beta}, \hat{\beta}/\left(\mathbb{P}(D_i^{56} = 1|D_i^{40} = 1) - \mathbb{P}(D_i^{56} = 1|D_i^{40} = 0)\right)]$$

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 1930 census and use the linkage provided by Abramitzky et al. (2022) and Ruggles et al. (2020) to match individuals in the 1940 census to the 1930 census. I then calculate the share of individuals who lived in 1930 and 1940 in a house that would be destroyed by a highway in 1956, and those who moved between 1930 and 1940 into property that will be destroyed. I acknowledge that this is a noisy measure of *remaining in the same property* because trends in the neighborhood could be different between 1930 and 1940 than they coould be between 1940 and 1956. However, this exercise is still informative to assess the potential bias in the estimated displacement gap. I find that,

$$\begin{aligned}
\hat{\mathbb{P}}(D_i^{40} = 1|D_i^{30} = 1) &= 0.242 \\
\hat{\mathbb{P}}(D_i^{40} = 1|D_i^{30} = 0) &= 0.008 \\
\hat{\mathbb{P}}(A_i^{40} = 1|A_i^{30} = 1) &= 0.336 \\
\hat{\mathbb{P}}(A_i^{40} = 1|A_i^{30} = 0) &= 0.067
\end{aligned}$$

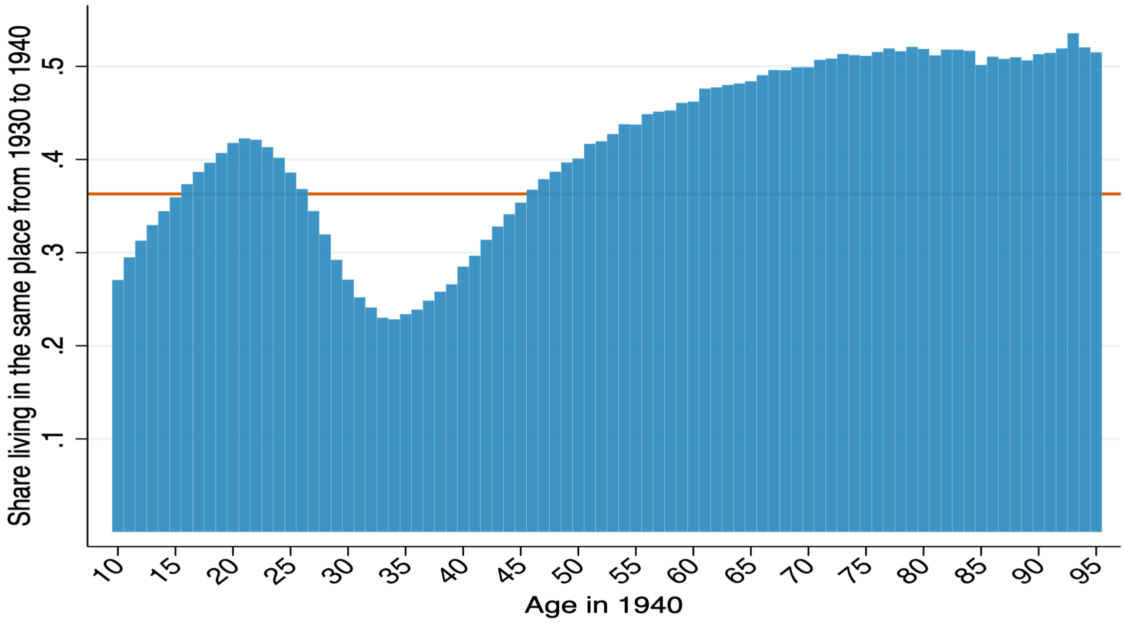
suggesting that the *true* gap for displaced individuals is between $\hat{\beta}$ and $\sim 4.27 \cdot \hat{\beta}$. Doing the analogous exercise for adjacent individuals yields that $b^A \in [\hat{\beta}^A, 3.72 \cdot \hat{\beta}^A]$

E.8.1 Additional robustness checks for mobility between 1940 and 1956

I further assess the robustness of the results to the potential mobility of individuals between 1940 and 1956. To that end, I perform three robustness checks. First, I repeat the analysis using individuals who were more likely to stay in the same house based on their age. To do this, I geocode the 1930 census and use the linked 1930-1940 census to calculate the share of individuals for each age who lived in the same house in both 1930 and 1940. The distribution of the share of individuals who remained in the same house is shown in Figure E.1. I choose 40% as the probability cutoff to subset the sample to individuals who were *more likely* to stay in the same house. The results are presented in Table E.16. This exercise yields similar estimates to the main analysis.

Second, I use linked records between 1940 and 1950 - the last census before the passage of the Federal Aid Highway Act of 1956. These links are provided by Ruggles et al. (2020). I then select individuals linked between censuses who did not change their state or county of residence between 1940 and 1950. Since the restricted full-count census is not available, I cannot restrict the sample to a more granular geography. Thus, this exercise is suggestive, as individuals could still have moved within their respective counties. The results are presented in Table E.18 and align with the main results. In the final robustness, I subset the sample to individuals in the 1940 full-count census who were born before 1910, as they were more likely to stay in the same house. The results are presented in Table E.17 and are consistent with the main analysis. Together, these checks suggest that the estimated displacement gap is robust to the potential mobility of individuals between 1940 and 1956.

FIGURE E.1: Empirical distribution of staying in the same house by age



Note: The sample corresponds to the linked 1930 and 1940 geocoded census data. Each bar corresponds to the share of individuals of the corresponding age group who stayed in the same house. The red line corresponds to the mean probability of staying in the same house.

TABLE E.16: Long-term Effects of Highway Construction on Likely Stayers

	Same Neigh. (1)	Same City (2)	Death Age (3)	Survival to age 70 (4)
Panel A: *near-versus-far* approach				
Displaced	-0.019[a]	0.014	-0.311[b]	0.010
	(0.004)	(0.029)	(0.151)	(0.011)
Adjacent	-0.006	-0.004	-0.140[c]	-0.005
	(0.005)	(0.012)	(0.077)	(0.008)
Mean dep. var.	0.020	0.593	75.781	0.523
R-squared (adj)	0.040	0.010	0.969	0.621
Observations	4,935	4,935	3,775	3,775
Panel B: *near-versus-far* approach + controls				
Displaced	-0.019[a]	0.014	-0.322	0.005
	(0.006)	(0.029)	(0.203)	(0.015)
Adjacent	-0.010	-0.010	-0.166[c]	-0.004
	(0.006)	(0.012)	(0.086)	(0.008)
Mean dep. var.	0.022	0.599	77.551	0.605
R-squared (adj)	0.048	0.018	0.964	0.548
Observations	3,814	3,814	3,102	3,102
Panel C: matching on observables				
Displaced	-0.021[a]	-0.017	-0.432[b]	0.015
	(0.004)	(0.044)	(0.200)	(0.017)
Adjacent	-0.002	0.008	0.190	0.001
	(0.003)	(0.017)	(0.133)	(0.008)
Mean dep. var.	0.020	0.587	74.452	0.500
R-squared (adj)	0.022	0.014	0.969	0.603
Observations	2,729	2,729	2,061	2,061
Panel D: Federal engineering maps as control group				
Displaced	-0.018[a]	0.015	-0.172	0.022[c]
	(0.004)	(0.024)	(0.143)	(0.012)
Adjacent	-0.009	-0.001	0.016	0.002
	(0.006)	(0.015)	(0.074)	(0.008)
Mean dep. var.	0.023	0.604	76.243	0.529
R-squared (adj)	0.026	0.014	0.969	0.635
Observations	6,002	6,002	4,628	4,628

Note: OLS estimates are reported. Only segments opened between 1950 and 1960 are included in the sample. An observation is an individual in the 1940 full-count who's estimated probability of staying in their property is greater than 40%. The sample consists of individuals who died after 1995 and were born before 1910. Displaced corresponds to individuals living in 1940 in houses destroyed by highway construction. Adjacent corresponds to individuals living in 1940 within 100 meters of future development. The control group is individuals living between 100 and 200 meters from a future highway. Each column corresponds to a different regression. The dependent variable in column (1) is an indicator that equals one if the individual lives at the time of death in the same neighborhood they lived in 1940. In column (2), the dependent variable is an indicator that equals one if the individual lives in the same city they lived in 1940. Column (3) uses the age at death as the dependent variable. All regressions control for race, gender at birth, homeownership, city, and birth year fixed effects. Coefficients are reported with standard errors clustered at the city where the individual lived in 1940 level. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

TABLE E.17: Long-term Effects of Highway Construction on Cohorts Born Before 1910

	Same Neigh. (1)	Same City (2)	Death Age (3)
Displaced	-0.025[a]	-0.110[a]	-0.645[a]
	(0.008)	(0.035)	(0.165)
Adjacent	-0.004	0.022	-0.170[c]
	(0.007)	(0.018)	(0.092)
Mean dep. var.	0.033	0.592	92.063
R-squared (adj)	0.044	0.007	0.433
Observations	2,615	2,615	2,516

Note: OLS estimates are reported. Only segments opened between 1950 and 1960 are included in the sample. An observation is an individual in the 1940 full count census matched to administrative mortality records. The sample consists of individuals who died after 1995 and were born before 1910. Displaced corresponds to individuals living in 1940 in houses destroyed by highway construction. Adjacent corresponds to individuals living in 1940 within 100 meters of future development. The control group is individuals living between 100 and 200 meters from a future highway. Each column corresponds to a different regression. The dependent variable in column (1) is an indicator that equals one if the individual lives at the time of death in the same neighborhood they lived in 1940. In column (2), the dependent variable is an indicator that equals one if the individual lives in the same city they lived in 1940. Column (3) uses the age at death as the dependent variable. All regressions control for race, gender at birth, homeownership, city, and birth year fixed effects. Coefficients are reported with standard errors clustered at the city where the individual lived in 1940 level. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

TABLE E.18: Long-term Effects of Highway Construction on Matched to 1950 Census

	Same Neigh. (1)	Same City (2)	Death Age (3)	Survival to age 70 (4)
Panel A: *near-versus-far* approach				
Displaced	-0.019[a]	0.004	-0.138	-0.010[c]
	(0.004)	(0.010)	(0.135)	(0.006)
Adjacent	-0.005	0.010	-0.113[b]	-0.001
	(0.003)	(0.006)	(0.054)	(0.002)
Mean dep. var.	0.032	0.704	78.788	0.857
R-squared (adj)	0.040	0.015	0.887	0.547
Observations	15,023	15,023	13,842	13,842
Panel B: *near-versus-far* approach + controls				
Displaced	-0.020[a]	-0.006	-0.140	-0.011[c]
	(0.004)	(0.011)	(0.142)	(0.006)
Adjacent	-0.007[b]	0.009	-0.116[b]	-0.000
	(0.003)	(0.007)	(0.055)	(0.002)
Mean dep. var.	0.032	0.709	79.268	0.888
R-squared (adj)	0.041	0.018	0.878	0.444
Observations	13,662	13,662	13,082	13,082
Panel C: matching on observables				
Displaced	-0.025[a]	0.033[a]	-0.018	0.000
	(0.004)	(0.011)	(0.128)	(0.005)
Adjacent	-0.009[b]	0.043[a]	0.041	0.006[b]
	(0.004)	(0.006)	(0.093)	(0.002)
Mean dep. var.	0.034	0.690	78.554	0.861
R-squared (adj)	0.030	0.014	0.882	0.528
Observations	9,179	9,179	8,513	8,513
Panel D: Federal engineering maps as control group				
Displaced	-0.022[a]	0.011	0.081	-0.004
	(0.004)	(0.012)	(0.110)	(0.005)
Adjacent	-0.008[b]	0.021[a]	0.086[c]	0.004[c]
	(0.004)	(0.007)	(0.048)	(0.002)
Mean dep. var.	0.036	0.706	78.783	0.856
R-squared (adj)	0.035	0.018	0.890	0.545
Observations	18,058	18,058	16,680	16,680

Note: OLS estimates are reported. Only segments opened between 1950 and 1960 are included in the sample. An observation is an individual in the 1940 full-count census matched to the 1950 census and administrative mortality records who lived in the same state and county in 1940 and 1950. The sample consists of individuals who died after 1995 and were born before 1910. Displaced corresponds to individuals living in 1940 in houses destroyed by highway construction. Adjacent corresponds to individuals living in 1940 within 100 meters of future development. The control group is individuals living between 100 and 200 meters from a future highway. Each column corresponds to a different regression. The dependent variable in column (1) is an indicator that equals one if the individual lives at the time of death in the same neighborhood they lived in 1940. In column (2), the dependent variable is an indicator that equals one if the individual lives in the same city they lived in 1940. Column (3) uses the age at death as the dependent variable. All regressions control for race, gender at birth, homeownership, city, and birth year fixed effects. Coefficients are reported with standard errors clustered at the city where the individual lived in 1940 level. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

E.9 Underlying Mechanisms

Gelbach (2016) decomposition is defined as:

$$y_i = \beta D_i + \mathbf{X}i^1 \gamma 1 + \epsilon_i$$

where y_i is the age at death, D_i is the displacement indicator, and $\mathbf{X}i^1$ represents the full set of control variables (neighborhood fixed effects, log distance to the 1940 home, percentage change in unemployment, change in distance to the central business district, and distance to the nearest hospital). Omitting the variables in $\mathbf{X}i^1$ from the base model yields the baseline estimate $\hat{\beta}^{base}$. The coefficient $\hat{\beta}^{base}$ converges in probability to $\beta + \mathbf{X}i^1\gamma 1 = \beta + \Gamma\gamma_1 = \beta + \delta$, where Γ is the matrix of coefficients obtained by projecting the columns of $\mathbf{X}i^1$ onto Di: $\mathbf{X}i^1 = \Gamma Di + v$. The Gelbach decomposition provides a closed-form expression for the bias that arises from omitting the variables in $\mathbf{X}_i^1$, represented by δ.

TABLE E.19: Gelbach's Deltas

	Neigh. 2000 (1)	Dist. origin (2)	Δ Unempl. (3)	Δ Dist. CBD (4)	Dist. hosp. (5)	Total (6)
Displace	-0.066[c]	0.004	0.004	0.003	0.001	-0.054
	(0.037)	(0.003)	(0.003)	(0.003)	(0.001)	
% Contribution to changes	29.04	-1.95	-1.59	-1.12	-0.60	

Note: These estimates correspond to Gelbach's 2016 deltas, which are the conditional contribution of the variable of interest to the displacement-mortality effect. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

TABLE E.20: Healthcare Outcomes at Time of Death

	Disability Share (1)	Poor Mental Health Share (2)	Poor Physical Health Share (3)	Poor Health Share (4)
Panel A: *near-versus-far* approach				
Displaced	0.402^{b}	0.109	0.156^{b}	0.324^{b}
	(0.162)	(0.109)	(0.059)	(0.140)
Adjacent	0.115^{c}	0.096^{c}	0.065^{a}	0.140^{b}
	(0.061)	(0.050)	(0.022)	(0.057)
Mean dep. var.	28.097	15.118	11.103	16.069
R-squared (adj)	0.157	0.196	0.170	0.198
Observations	31,941	31,941	31,941	31,941
Panel B: *near-versus-far* approach + controls				
Displaced	0.443^{a}	0.104	0.176^{a}	0.355^{a}
	(0.150)	(0.093)	(0.051)	(0.121)
Adjacent	0.127^{c}	0.088^{b}	0.071^{a}	0.135^{b}
	(0.069)	(0.044)	(0.023)	(0.053)
Mean dep. var.	28.037	15.077	11.073	16.009
R-squared (adj)	0.160	0.201	0.172	0.200
Observations	29,767	29,767	29,767	29,767
Panel C: matching on observables				
Displaced	0.377^{b}	0.157	0.181^{b}	0.403^{a}
	(0.174)	(0.112)	(0.082)	(0.145)
Adjacent	-0.035	0.082	0.040	0.117
	(0.150)	(0.102)	(0.078)	(0.147)
Mean dep. var.	27.417	14.678	10.828	15.349
R-squared (adj)	0.093	0.119	0.105	0.091
Observations	18,495	18,495	18,495	18,495
Panel D: Federal engineering maps as control group				
Displaced	0.367^{b}	0.085	0.136^{b}	0.312^{b}
	(0.163)	(0.084)	(0.066)	(0.142)
Adjacent	0.098	0.089^{c}	0.062	0.145
	(0.093)	(0.050)	(0.045)	(0.103)
Mean dep. var.	27.868	15.048	10.996	15.787
R-squared (adj)	0.139	0.170	0.149	0.160
Observations	38,944	38,944	38,944	38,944

Note: OLS estimates are reported. Only segments opened between 1950 and 1960 are included in the sample. An observation is an individual in the 1940 full count census matched to administrative mortality records. The sample consists of individuals who died after 1995. Displaced corresponds to individuals living in 1940 in houses destroyed by highway construction. Adjacent corresponds to individuals living in 1940 within 100 meters of future development. The control group is individuals living between 100 and 200 meters from a future highway. Each column corresponds to a different regression. The dependent variables correspond to the neighborhood-level characteristics of the residence at time of death. The dependent variable in column (1) corresponds to the share of adults living in the neighborhood who are obese. In column (2), the dependent variable is the share of adults on a tract who had suffered a stroke. Column (3) uses the share of adults with a physical disability. Columns (4) and (5) use the share of adults with poor mental and physical health, respectively. Finally, column (6) uses the share of adults with poor health status as dependent variable. All regressions control for race, gender at birth, homeownership, city, and birth year fixed effects. Coefficients are reported with standard errors clustered at the city where the individual lived in 1940 level. ^{a} indicates the coefficient is significant at the 1%, ^{b} at the 5%, and ^{c} at the 10% level.

TABLE E.21: Mechanisms of the Mortality Effects

	Interactions within Zipcode (1)	Long tie interactions (2)	Clustering coefficient (3)	Eviction Filings rate (4)	Eviction Threatened rate (5)	Dissimilarity Index (6)
Panel A: *near-versus-far* approach						
Displaced	0.340[a]	-0.373[b]	0.045[b]	0.397	0.314	0.008[b]
	(0.123)	(0.159)	(0.022)	(0.306)	(0.241)	(0.004)
Adjacent	0.057	-0.066	0.012	0.042	0.039	0.000
	(0.076)	(0.062)	(0.008)	(0.082)	(0.068)	(0.002)
Mean dep. var.	30.838	48.124	7.021	4.004	3.591	0.549
R-squared (adj)	0.254	0.213	0.052	0.043	0.046	0.054
Observations	35,611	35,611	35,611	10,395	10,395	35,636
Panel B: *near-versus-far* approach + controls						
Displaced	0.356[a]	-0.383[b]	0.042[b]	0.440	0.359	0.008[b]
	(0.126)	(0.161)	(0.020)	(0.312)	(0.254)	(0.004)
Adjacent	0.096	-0.116[c]	0.010	0.043	0.043	0.001
	(0.079)	(0.067)	(0.007)	(0.080)	(0.066)	(0.002)
Mean dep. var.	30.784	48.188	7.017	3.992	3.578	0.549
R-squared (adj)	0.255	0.216	0.062	0.042	0.045	0.053
Observations	33,199	33,199	33,199	9,651	9,651	33,222
Panel C: matching on observables						
Displaced	-0.114	-0.184	0.047[a]	0.656	0.516	0.007
	(0.270)	(0.236)	(0.014)	(0.475)	(0.386)	(0.006)
Adjacent	-0.236	0.157	0.014	0.040	0.087	0.000
	(0.159)	(0.171)	(0.018)	(0.090)	(0.061)	(0.004)
Mean dep. var.	30.135	48.559	7.027	4.027	3.590	0.559
R-squared (adj)	0.185	0.176	0.034	0.029	0.028	0.042
Observations	20,880	20,880	20,880	6,377	6,377	20,923
Panel D: Federal engineering maps as control group						
Displaced	0.244	-0.420[c]	0.060[b]	0.549[c]	0.471[c]	0.009
	(0.236)	(0.219)	(0.025)	(0.297)	(0.238)	(0.006)
Adjacent	-0.005	-0.110	0.019	0.129[b]	0.137[a]	0.003
	(0.264)	(0.136)	(0.016)	(0.054)	(0.044)	(0.004)
Mean dep. var.	30.945	48.193	6.995	3.885	3.464	0.550
R-squared (adj)	0.258	0.216	0.041	0.045	0.048	0.047
Observations	43,822	43,822	43,822	13,309	13,309	43,853

Note: OLS estimates are reported. Only segments opened between 1950 and 1960 are included in the sample. An observation is an individual in the 1940 full count census matched to administrative mortality records. Displaced corresponds to individuals living in 1940 in houses destroyed by highway construction. Adjacent corresponds to individuals living in 1940 within 100 meters of future development. The control group is individuals living between 100 and 200 meters from a future highway. Each column corresponds to a different regression. Columns (1) to (3) use the Facebook-connected measures as dependent variables. Columns (4) and (5) present the rate of eviction filings and threats from Gromis et al. (2022). Finally, column (6) corresponds to the racial dissimilarity index of the tract. All regressions control for race, gender at birth, homeownership, city, and birth year fixed effects. Coefficients are reported with standard errors clustered at the city where the individual lived in the 1940 level. [a] indicates the coefficient is significant at the 1%, [b] at the 5%, and [c] at the 10% level.

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