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Abstract

Freeway revolts were widespread protests across the U.S. following early urban Interstate construction in the mid-1950s. We present theory and evidence from panel data on neighborhoods and travel behavior to show that diminished quality of life from freeway disamenities inspired the revolts, affected the allocation of freeways within cities, and changed city structure. First, actual freeway construction diverged from initial plans in the wake of the growing freeway revolts and subsequent policy responses, especially in central neighborhoods. Second, freeways caused slower growth in population, income, and land values in central areas, but faster growth in outlying areas. These patterns suggest that in central areas, freeway disamenity effects exceeded small access benefits. Third, in a quantitative general equilibrium spatial model, the aggregate benefits from burying or capping freeways are large and concentrated downtown. This result suggests that targeted mitigation policies could improve welfare and helps explain why opposition to freeways is often observed in central neighborhoods. Disamenities from freeways, versus their commuting benefits, likely played a significant role in the decentralization of U.S. cities.

Keywords: central cities, amenities, commuting costs, suburbanization, highways
JEL classification: N72, N92, O18, Q51, R14, R23, R41, R42

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1 Introduction

The Federal-Aid Highway Act of 1956 authorized and financed the Interstate Highway System, with the ambitious goal of completing 41,000 miles of freeways by 1969. Early freeway building was fast: planners faced few constraints and little opposition as they moved to build the Interstates in 1956. The prevailing view among engineers, policy makers, and the public was that freeways would ease congestion and revitalize downtowns. Lewis Mumford, later an important critic of urban freeways, initially "viewed the automobile as a beneficient liberator of urban dwellers from the cramped confines of the industrial city" (DiMento and Ellis, 2013, p. 38).

But mass construction soon led to skepticism, then outright protests, which spread to at least 50 U.S. cities. These *freeway revolts* often set central-city residents (concerned about local quality of life) against regional planners (who viewed freeways as key to regional growth). (Famously, neighborhood advocates including Jane Jacobs fought the construction of central-city freeways such as the Lower Manhattan Expressway.) Mass construction sharpened the side effects of freeways in the public imagination—e.g., land taking, negative externalities from pollution and noise, and barriers between neighborhoods. In response, policy gradually ceded more control to local neighborhood concerns. In San Francisco, an early center of the freeway revolts,[1] the Board of Supervisors halted further freeway construction in January 1959, leaving the Embarcadero Freeway—and most of the planned freeway network—permanently unfinished. Across the U.S., aided by federal highway legislation in 1962 and 1966 and other policy changes in the 1960s, protesters often significantly altered, or stopped outright, proposed freeway routes.

What factors motivated the freeway revolts? How did the revolts and subsequent policy responses shape the allocation of freeways in U.S. cities? And how, and why, did freeways affect the shape of U.S. cities? In this paper, we shed light on the causes and the consequences of the freeway revolts. A central theme is that—aside from reducing commuting costs—freeways produce local disamenities that significantly reduce neighborhood quality of life. These disamenities disproportionately affected central city neighborhoods, with important implications for both the eventual allocation of freeways within cities and the spatial structure of U.S. cities today.

First, we analyze the consequences of the freeway revolts on the allocation of freeways to U.S. cities and neighborhoods. The revolts were a surprise to engineers and planners as they began building the Interstates in the middle 1950s. As the revolts spread, federal and state policy evolved to better accommodate protesters' concerns. For example, the Federal-Aid Highway Act of 1958 first required state highway officials to hold at least one public hearing and consider economic effects in advance of construction. Subsequent legislation in 1962, 1966, 1968, and oversight by the new Department of Transportation beginning in 1967 added additional constraints on state highway departments. Thus, freeway segments that were completed early, in the late 1950s, tended to follow planned routes, while freeway segments that were delayed into the 1960s were more likely to be altered in routing or canceled entirely in the face of opposition. Compared with 1955 planned

[1]In 1955, residents in the path of the Western Freeway in San Francisco organized to fight its proposed route (DiMento and Ellis, 2013, p. 137).

routes, the realized freeway network of the late 1960s was more likely to be aligned near rivers and historical rail roads. These patterns are consistent with the increasing challenges faced by freeway builders in acquiring rights of way. The divergence of the built freeway network from initial plans was especially pronounced in central cities, highlighting the intensity of protests in downtown neighborhoods and their success in diverting planned freeways. Finally, better-educated and more-white neighborhoods were increasingly more successful at avoiding planned freeway construction over the 1960s. These groups may have been better able to take advantage of new freeway-fighting policies, a channel emphasized by Glaeser and Ponzetto (2018).

Second, we present theory and evidence highlighting the disamenity effects of freeways on city structure. Using panel data on U.S. cities and neighborhoods between 1950 and 2010, we show that downtown neighborhoods closer to newly-opened freeways declined more in population and income compared with neighborhoods farther away. But in the suburbs, proximity to a freeway has no such effect. Intuitively, in downtown neighborhoods, the disamenity value of a new freeway dominates its access benefits. But in outlying neighborhoods, access benefits are greater. These findings can be easily explained by disamenity effects but are more difficult to reconcile with standard city structure models that focus exclusively on freeways' effects on reducing commuting costs.

We use planned-route and historical-route instrumental variables (following the typology of Redding and Turner, 2015) to identify the causal effect of freeways on neighborhoods. The IV results suggest a strongly negative causal effect of freeways on population in central cities. We also show evidence from historical travel survey data from Chicago and Detroit of null employment effects of freeways in central neighborhoods. Thus, increases in firm demand for central land near freeways seem unlikely to be driving population and income declines near central freeways. In Chicago, appraised land values also grew more slowly near central freeways, again consistent with freeway disamenities and not with freeway-related productivity gains.

We also show evidence of barrier effects—that is, increases in the cost of travel *across* a freeway—from newly-rediscovered travel diary microdata from Detroit in 1953 and 1994. Travel flows decline, and travel times increase, for trips up to 3 miles that cross new freeways. These estimates take into account changes in the desirability of neighborhoods as origins or destinations caused by freeway construction and fixed characteristics of neighborhood pairs using high-dimensional fixed effects in a "structural gravity" model (Head and Mayer, 2014).

Third, we develop a quantitative spatial general equilibrium model of city structure to measure and quantify the effects of freeway disamenities. The model builds on quantitative spatial models that consider the joint location decisions of employment and population in a city with costly commuting following Ahlfeldt et al. (2015). The model takes into account several features that are less well-handled by reduced-form techniques, including spillovers between neighborhoods, endogenous job location, and general equilibrium effects. By using observed travel times with the structure of the model, we also take into account the variation among neighborhoods in treatment intensity caused by the geometry of radial freeway networks that concentrate freeways downtown. We calibrate our model to match cross-sectional variation within the Chicago metropolitan area in the year

2000 in neighborhood population, jobs, and travel times. Using residual neighborhood amenities recovered from the model, we estimate neighborhood amenities are 17.5 percent lower next to a freeway, and this disamenity attenuates by 95 percent at three miles' distance. Intuitively, this disamenity is identified by freeway-adjacent neighborhoods that have superior access (low travel times to employment centers) but low populations. This result is robust to alternative calibrations, control variables, and instrumental variables estimates.

We use the quantitative model to consider a counterfactual experiment in which freeway disamenities are mitigated. This policy is analogous to real-world policies like Boston's "Big Dig" that attempt to mitigate the negative effects of freeways by burying or capping them. In our baseline calibration, the aggregate benefits are large and concentrated near downtown. The concentration of mitigation benefits downtown (or the concentration of disamenities downtown) follow from two factors: one, downtown freeways affect more people, due to higher population densities, and two, there are more freeways downtown, due to the radial structure common to most U.S. metropolitan areas. These results are important for three reasons. One, disamenity mitigation policies that target central neighborhoods could provide net benefits to cities. Two, our results help explain why the freeway revolts (and political opposition to freeways in general) were concentrated in central city neighborhoods. Three, freeway disamenities, as opposed to commuting benefits, likely played a significant role in the decentralization of U.S. cities.

Finally, we quantify the relative importance of land use exclusion and barrier effects in neighborhood amenities. The aggregate benefits from removing barrier effects alone are large relative to the baseline estimated effect of mitigating all disamenities from freeways. This result suggests that barrier effects are an important source of freeway disamenities.

1.1 Related work

Our paper makes contributions to several literatures. First, a large literature estimates the effects of freeways on economic geography (Chandra and Thompson, 2000, Michaels, 2008, Allen and Arkolakis, 2014). For example, Duranton and Turner (2012) estimate the impact of Interstate highways on the distribution of employment across cities, and Baum-Snow (2007) estimates the effects on freeways on the movement of population from central cities to the suburbs. Traditionally, economists have understood these freeway effects through the channel of reduced costs of transporting goods and people (see the review by Redding and Turner, 2015). Our paper contributes to this literature by emphasizing that freeways also affect the spatial organization of economic activity by changing relative amenity values.[2] Further, we provide evidence at a finer spatial scale (census tracts or neighborhoods) compared with previous work.

Second, a large literature examines the decentralization of U.S. cities. Previous papers have

[2]Two contemporaneous working papers are on related themes. Ahlfeldt et al. (2016) contrast accessibility versus noise effects of a rail line in Berlin. Our analysis considers additional disamenity effects and identifies barrier effects as an important source of disamenity. Carter (2018) analyzes the allocation of Interstate highways and their effects in Detroit. Our analysis pools evidence from neighborhoods across 64 large U.S. cities. We also analyze welfare and outcomes under counterfactual experiments using a quantitative spatial model.

highlighted freeways' effects through reducing commuting costs (LeRoy and Sonstelie, 1983; Baum-Snow, 2007; Kopecky and Suen, 2010). As Duranton and Puga (2015) note, while the relative decline of central cities in response to lower transportation costs is consistent with the monocentric city model, it is more difficult to rationalize the large *absolute* declines in central city population. Margo (1992) and Kopecky and Suen (2010) have appealed to increases in household incomes to fill this gap. White flight in response to African-American migration to northern cities (Boustan, 2010) and the 1960s riots (Collins and Margo, 2007) also contributed to declines in central city populations. Our contribution is to identify the disamenity effects of freeways, apart from their effects in reducing commuting costs, as an important contributor to the decentralization of cities. In our analysis, freeways have disproportionately negative effects in central cities because (i) these areas see relatively less improvement in access and (ii) these areas receive more freeways due to the radial design common to most U.S. city freeway networks.

Third, there is a large body of work on negative externalities of freeways. For example, Anderson (2019) identifies increased mortality from particulate pollution among elderly residents near freeways using wind patterns. Other recent papers evaluating negative externalities from freeways include Hoek et al. (2002), Gauderman et al. (2007), Currie and Walker (2011), Rosenbloom et al. (2012), and Parry, Walls, and Harrington (2007). Much of this literature considers the effects of freeways on housing or land prices. Our paper adds to these results by considering their implications for the spatial structure of cities, i.e., quantities. In addition, another contribution is that we provide evidence that freeways create barriers between neighborhoods. This evidence is from newly-rediscovered travel diary microdata from Detroit in 1953 (and a follow-up survey from 1994) that was famously used in Kain's (1968) study of spatial mismatch.

Fourth, a recent literature has developed and applied quantitative models to study urban spatial structure and the role of infrastructure investment. Prominent examples include Ahlfeldt et al. (2015) and Allen and Arkolakis (2014).[3] Our contribution is to use a spatial quantitative model to study the negative amenity effects of transportation infrastructure. In addition, we use neighborhood amenities recovered from the structure of the model to estimate the magnitude and importance of highway disamenities. This method is related to approaches following Roback (1982) that use local wages and prices to study productivity and quality of life factors across and within cities (e.g., Albouy, 2016, and Albouy and Lue, 2015). Our contributions are to focus on freeways as amenity factors and estimate quality of life at a detailed (census tract) spatial scale.

Finally, there is a small literature on the political economy of infrastructure investment (Knight, 2002; Altshuler and Luberoff, 2003; Glaeser and Ponzetto, 2018). We add to this literature by providing evidence on the types of neighborhoods that received urban freeways in the 1950s and 1960s, and by showing changes over time in these patterns.

[3] For surveys of the literature see Redding and Rossi-Hansberg (2017) and Holmes and Sieg (2015). Several articles have studied the effects of infrastructure investment on trade or commuting (Donaldson, 2018, Monte et al., 2018, and Severen, 2019).

2 The effects of freeway disamenities

What are the effects of freeways disamenities? To fix ideas, consider a monocentric city following von Thunen (1826), Mills (1967), and others.[4] Workers choose where to live and commute to a city center, an exogenous point in space.[5] Commuting is costly, so workers trade higher land prices for shorter commutes. In equilibrium, prices adjust so that utility is equalized at every location, and both population density and land prices decline with distance to the center. Figure 1a illustrates this equilibrium pattern of declining density with distance to the city center (the star). Central areas feature high densities (in red) while peripheral areas feature low densities (in blue).

When a freeway is constructed that connects the city center to suburbs, the first well-known effect is that access to the city center improves via faster commutes. These access benefits vary. Locations near the center do not benefit significantly, since the new freeway has little effect on (already-low) commuting costs. Locations far from the center benefit more, especially if they are near the new freeway. Thus, access benefits cause faster population growth in locations that are farther from the city center and closer to the freeway. Figure 1b shows a freeway aligned along the horizontal axis leads to population growth and decentralization, with population spreading out along the newly-constructed freeway. Population *changes* are shown in Figure 1c. Locations in outlying areas near freeways see the largest increases in population (in blue); population in central areas is little changed (in white).[6]

A second effect of the new freeway is that quality of life declines in neighborhoods because of freeway disamenities. These disamenity effects may stem from several sources, including the loss of developable land, pollution or noise externalities, or barrier effects, i.e., reductions in access between neighborhoods severed by freeways. They may arise in all locations, independent of distance to the city center. Thus, the net effect of both the access and disamenity channels will vary by location.

For central neighborhoods, disamenity effects will dominate given that access benefits are minimal, and population will decline in neighborhoods near the freeway. For locations far from the center, population growth may be larger near the freeways. Figure 1d shows population changes when the freeway improves access and creates disamenities.[7] As in the no-disamenities case, outlying locations near freeways see the largest increases in population. In contrast to the no-disamenities case, central locations see large declines in population (now in orange), especially near freeways.

This discussion offers several predictions. The decline in commuting costs leads to population gains in outlying neighborhoods, especially in those closest to new freeways. Freeway disamenities

[4]The purpose here is to provide a simple model to provide intuition and structure our reduced-form analysis. A richer model of city structure is presented in Section 9.

[5]This analysis may also apply to other regional destinations, not just work commutes. See Section 8.

[6]Our analysis here assumes an open city, where equilibrium utility is fixed at an outside reservation level and total population adjusts. However, a key testable prediction is unchanged in the closed-city case: that freeway disamenities cause faster relative population growth in outlying neighborhoods near freeways compared with central neighborhoods near freeways.

[7]The net effects are ambiguous in outlying areas. If the access benefits dominate the disamenity effects, then population growth will be larger near the freeway in outlying areas. Unambiguously, population growth near freeways will be relatively larger in outlying areas compared with central areas. In the case shown, access benefits dominate freeway disamenities at the periphery.

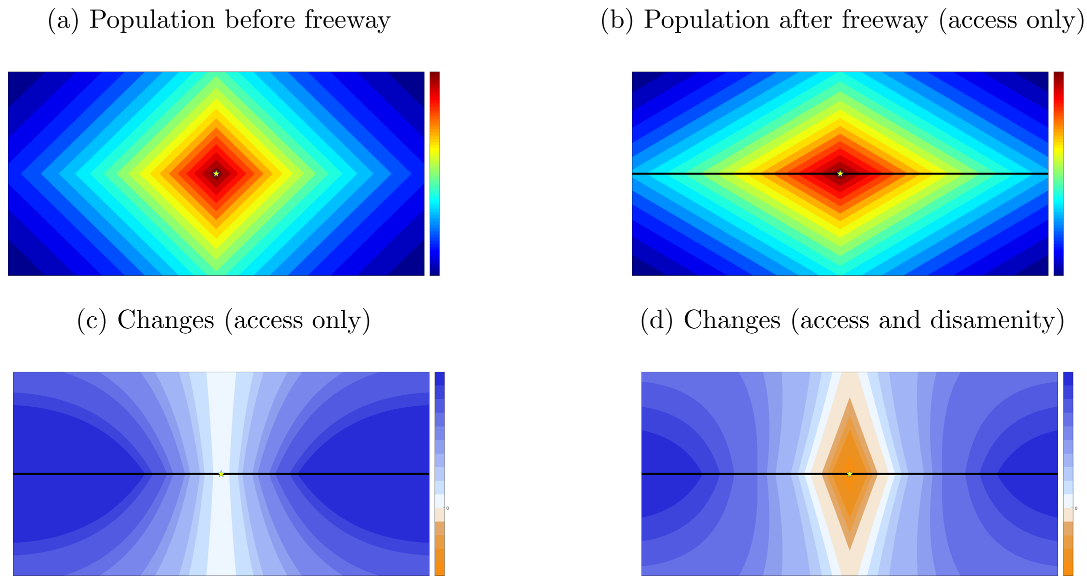


Figure 1: Population effects of a freeway in a monocentric city: Access versus disamenity

lead to population declines in central neighborhoods, especially in those closest to new freeways. Similar predictions can be made about changes in land prices and the sorting of income groups.[8] In general, a common prediction is that *if* freeway disamenities are important, then their effects will show up most in central neighborhoods, *especially* near freeways. We evaluate these predictions, as well as alternative mechanisms, in the following sections.

3 Data

Our analysis uses data from multiple sources. One, we use a consistent-boundary census tract panel for 64 U.S. metropolitan areas between 1950 and 2010.[9] Census tables provide information about population housing for each tract in each census year. For each tract, we compute distance to the city's center, a point in space defined using the 1982 Census of Retail Trade (Fee and Hartley, 2013). We also spatially match tracts to natural features such as coastlines, lakes, rivers, and slope,

[8]The sorting effects can be ambiguous and will depend on the sources of heterogeneity among income groups. In particular, the predictions depend on the relative importance of amenities among income groups. (Several papers have shown that preferences for amenities increase with skill or income, including Lee and Lin (2018), Lee (2010), Handbury (2013), Brinkman (2016), and Diamond (2016)). They also depend on whether or not commuting costs scale with income and the importance of fixed costs as studied by LeRoy and Sonstelie (1983). However, if there is a disamenity effect from being located close to the freeway, then it will be more important for sorting in central neighborhoods. In suburban neighborhoods, the sorting patterns after freeway construction will depend more on the reduced commuting costs. The fact that there are potentially multiple sources of heterogeneity makes overall patterns ambiguous.

[9]Since tract boundaries occasionally change over time, these data are normalized to 2010 boundaries using area weights, or, in later years, block population weights. Our analysis is limited to the 64 metropolitan areas with tract-level measures in 1950. These 64 metropolitan areas contained about one-third of the total U.S. population in 2010. See Lee and Lin (2018) for details about the construction of this database.

(Lee and Lin, 2018) and other factors such as historical rail routes (Atack, 2015).

Two, each tract is matched to the nearest present-day freeway from the National Highway Planning Network (NHPN) (U.S. Federal Highway Administration 2014), a database of line features representing highways in the United States. From the NHPN we select all limited access roads, which include Interstate highways as well as U.S., state, and local highways that offer full access control (i.e., prohibiting at-grade crossings).

Three, we use information on the opening dates for each Interstate highway segment, up until 1993, from the PR-511 database.[10] The PR-511 database was an administrative database compiled by the Federal Highway Administration (FHWA) for the purposes of collecting statistics about the then-rapidly expanding Interstate network. Thus, these data allow us to construct a time-varying measure of tract proximity to the expanding Interstate highway network.

Four, we digitized several maps of planned freeway routes. Of special interest is the *General Location of National System of Interstate Highways Including All Additional Routes at Urban Areas Designated in September 1955*, popularly known as the "Yellow Book" (U.S. Department of Commerce, 1955). At the beginning of the Interstate era in 1955, the Bureau of Public Roads (now the FHWA), in cooperation with State highway departments, designated the routes of urban Interstates in a series of city maps contained in the Yellow Book. Unlike the earlier 1947 plan, which described only routes *between* cities, the Yellow Book described the general routing of highways *within* each of 100 metropolitan areas.[11] Fifty metropolitan areas have both 1950 tract data and a Yellow Book map.

Other data are described in later sections or in the Appendix. We digitized the 1947 Interstate plan and historical routes of exploration. (These data are described and used in Section 5 and Appendix A.) We use summary and micro data from historical travel surveys conducted in 1950s Chicago and Detroit and modern travel surveys to estimate the effects of freeways on job growth and the barrier effects of freeways (Sections 6 and 7 and Appendixes A and C). We use data on appraised land values for 330 by 330 foot grid cells in the Chicago metropolitan area in 1949 and 1990 to estimate the effect of freeways on land values (Ahlfeldt and McMillen, 2014) (Section 8 and Appendix D). We use data on tract-level employment, population, land area, and tract-to-tract travel times from the 2000 Census Transportation Planning Package to calibrate our structural model (Section 10 and Appendix F).

4 Evidence from building the Interstates

The freeway revolts were most successful in affecting the allocation of Interstates in central neighborhoods, especially by the late 1960s. This evidence suggests that freeway disamenities were most

[10]These data were generously shared by Nate Baum-Snow. We did some further cleaning of these data to ensure accuracy in spatial matching of the PR-511 data to highway segments within metropolitan areas.

[11]In 1947, the Bureau of Public Roads had mapped about 90 percent of National System of Interstate Highways authorized (but incompletely funded) by Congress in the Federal-Aid Highway Act of 1944. The 1947 map showed rural routes that terminated outside metropolitan areas.

salient in central neighborhoods. Unfortunately, there are little systematic data on the precise timing and location of opposition to freeway building.[12] Instead, we combine historical narrative with the timing and location of departures from the initial 1955 Yellow Book plans in the routes of completed Interstates. Our evidence suggests that the revolts were most successful in diverting or obstructing planned freeways in central neighborhoods, especially by the late 1960s, after policy changes empowered freeway opponents. Finally, we find evidence that freeways were increasingly allocated to historically black and less-educated neighborhoods.

4.1 The unanticipated freeway revolts and policy responses

By the late 1960s, freeway revolts were widespread. A short-lived survey conducted by the U.S. Department of Transportation (DOT) between October 1967 and June 1968 recorded 123 separate freeway revolts (Mohl, 2002). Lowell K. Bridwell, an early federal administrator who was sympathetic to revolts, noted highway planners faced social and environmental "problems of a serious nature in at least 25 cities" in March 1968 (Mohl, 2008, p. 202). Other sources identify over 200 controversial freeway projects across 50 cities (Wikipedia, 2019).

Despite their eventual extent, in the mid-1950s the freeway revolts were largely unanticipated by planners, builders, and even later critics of the Interstate program. Planners had an immature understanding of the negative side effects of cars and limited-access roads in mature cities. For example, a 1924 plan for Detroit showed superhighways with a "'parkway' ambience [...] reinforced by groups of pedestrians ambling along only a few feet from the freeway, as though it were a Parisian boulevard" (DiMento and Ellis, 2013, p. 19). Engineers at state highway departments and the BPR, who dominated freeway planning in the 1940s and 1950s, had faced little opposition in their experience building the rural sections of the national highway network under the provisions of the Federal-Aid Highway Act of 1944. Finally, even later critics were at first enthusiastic about urban highways. Central-city mayors and officials believed that highways would revitalize struggling downtowns. While local officials supported the program, few were involved in early freeway building. By the mid-1950s, "[s]tate highway departments [had] consolidated their hold on the urban freeway planning process, eclipsing local planning and public works officials" (p. 100).

A consequence of the unexpected freeway revolts was that planners did not systematically select neighborhoods for initial freeway projects in the late 1950s on expected resistance to urban freeways. "[N]o one anticipated the urban battles ahead so no one thought 'I better build my urban segments right away before anyone starts fighting them.' Officials simply made choices about the priority of each segment for construction based on whatever factors they considered important" (Weingroff, 2016). Indeed, state highway departments, "believ[ing] they had to finish the entire 41,000 miles within the 13-year funding framework" (Weingroff, 2016), raced to complete their segments. Which projects were completed first often depended more on the ability of the state highway department

[12] The literature includes several excellent case studies, including Mohl (2004) on revolts in Miami and Baltimore. However, outside of the short-lived DOT survey in 1967–1968, there appears to have been few contemporaneous efforts to catalog all of the freeway revolts. Further, contemporaneous media coverage often fails to clearly identify the location and timing of opposition and may have also been selected on neighborhood factors or famous participants.

to staff up quickly, its experience in right-of-way acquisition or designing (pre-Interstate) freeways, and the pipeline of previously completed plans (Johnson, 1965).

Highway policy evolved in response to the spreading freeway revolts. At the beginning of the Interstate era, state and federal highway engineers "had complete control over freeway route locations" (Mohl, 2004). Subsequent highway bills eroded this power.[13] For example, the 1958 highway act first required state highway planners to hold public hearings and consider economic effects in advance of construction. The 1962 highway act further required that highway projects be "carried out cooperatively" with local communities. Highway legislation in 1966 and 1968 created new environmental and historic-preservation hurdles for new highway construction. In addition, highways were now subject to the DOT, established in 1966 and opened in 1967. Its first secretary, Alan S. Boyd, was sympathetic "to the public clamor over the damaging impact of interstates in urban neighborhoods" (Mohl 2004, p. 681). "Within a year of taking office at the DOT [in 1967], [Secretary of Transportation] Boyd had seemingly become the most effective national spokesman for the freeway revolt." (Mohl 2004, p. 681). By 1967, "the freeway debates and protests of the late 1960s begin to erode formerly uncritical acceptance of urban freeways," and federal and state policy had swung decisively in favor of the revolts (DiMento and Ellis, 2013, p. 140).

4.2 The changing allocation of freeways in U.S. cities

The unanticipated, growing revolts and evolving policy environment combined to shape the allocation of freeways within U.S. cities. Increasingly, built freeways diverged from initial plans, with later-programmed freeways less likely to be built according to plan.

> The timing, progress, and outcome of the emerging freeway revolt differed from city to city ... [I]n cities where the highway builders moved quickly in the late 1950s to build the urban interstates, the inner beltways and radials, opposition never materialized or was weakly expressed. [...] Where freeway construction was delayed into the 1960s, affected neighborhoods, institutions, and businesses had time to organize against the highwaymen. In some cases, freeway fighters successfully forced the adoption of alternative routes, and they even shut down some specific interstate projects permanently (Mohl, 2004, p. 675)

Figure 2 illustrates this pattern in the Washington metropolitan area. Yellow Book planned routes from 1955 are shown in yellow, and completed freeway routes are colored according to the year first opened to traffic, as recorded in the PR-511 database. Several features are worth noting. One, the realized freeway network is spatially correlated with the 1955 plan. Many completed routes lie close to, or are coincident with, planned routes in the Yellow Book. Two, one completed route, I-66 stretching west from downtown D.C., deviated significantly from the initial plan route. In part, this was due to significant opposition from residents of both Arlington and Falls Church, Virginia; a number of lawsuits delayed construction until the late 1970s. Three, several routes were

[13] See Table B.1 for a stylized timeline of federal policy changes.

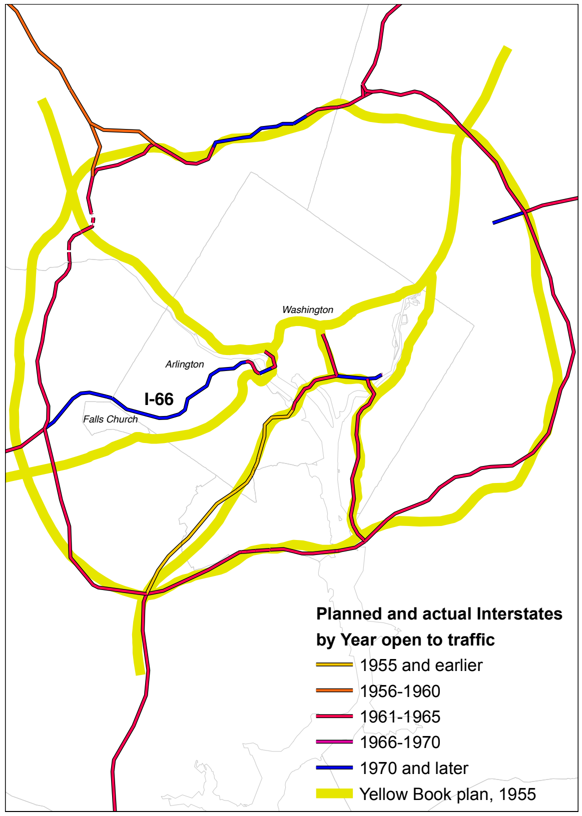


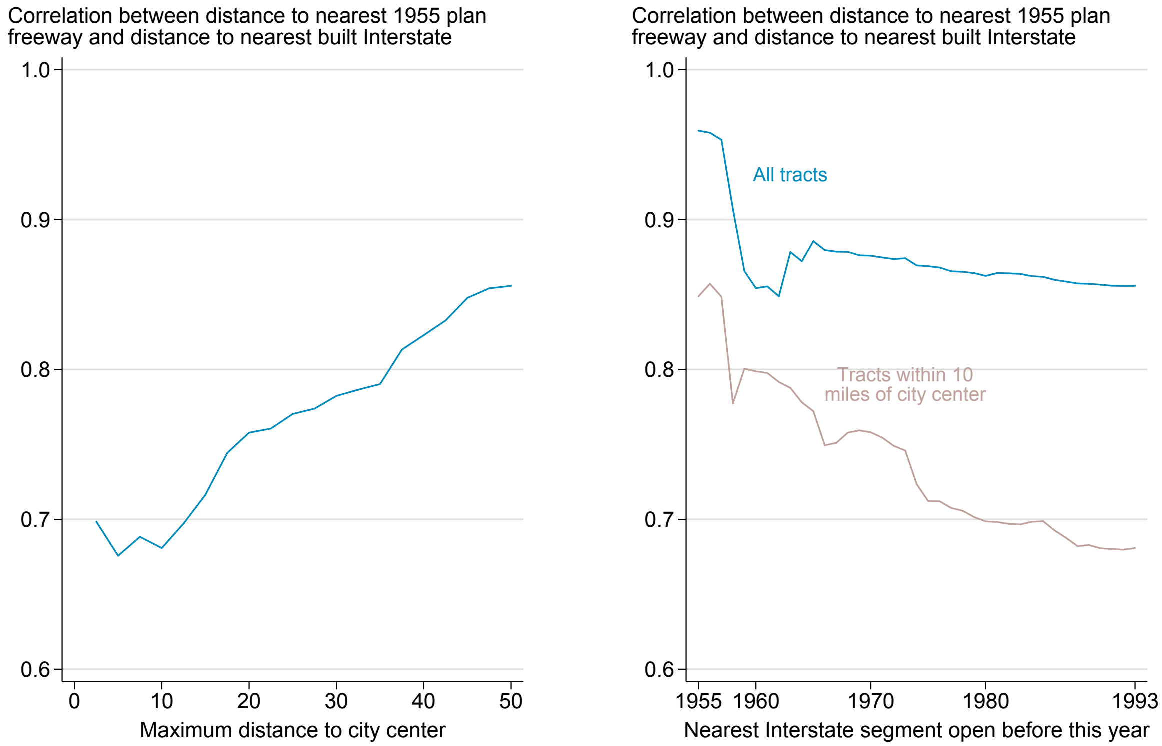
Figure 2: Some highways deviated from initial 1955 plans or were cancelled

This figure shows freeways shown in the 1955 Yellow Book plan and completed limited-access freeways in the Washington, D.C. metropolitan area. Sources: NHPN, FHWA, NHGIS.

canceled altogether in northwest and northeast D.C. There is also historical evidence of significant opposition to new freeways in these areas.

Next, we present three results from our sample of 50 cities. First, today, built freeways least resemble the 1955 Yellow Book plan in central neighborhoods. To show this, we use cross-sectional variation among census tracts in proximity to both completed and planned freeways. Figure 3a shows the within-city, tract-level correlation between distance to the nearest completed freeway and distance to the nearest planned freeway.[14] If the nearest completed freeway is built exactly to plan, this correlation will be maximized at 1. Departures from plan will reduce actual freeway proximity compared with planned freeway proximity for some tracts and increase it for others, leading to correlation coefficients less than 1. We compute correlation coefficients for successively larger groups of census tracts, according to their distance from the city center. Thus, for tracts within 2.5 miles of city centers, the correlation between distances to the nearest planned freeway and the nearest completed freeway is 0.7, indicating positive, but relatively low, spatial correlation between planned and completed freeway networks for the most-central census tracts. This result accords

[14]These correlation coefficients are computed from coefficients of determination from tract-level regressions of distance to the nearest completed freeway on distance to the nearest planned freeway, conditioned on metropolitan area fixed effects.



(a) Completed freeway routes least resemble planned freeway routes in central areas

(b) Over time, the correlation between completed and planned freeway routes declined faster and farther in central areas

Figure 3: Correlation between 1955 Yellow Book plan and built Interstate highways

These figures show correlation coefficients computed from coefficients of determination from tract-level regressions of distance to the nearest completed freeway on distance to the nearest planned freeway, conditioned on metropolitan area fixed effects. In Figure 3a, regressions use tracts within x miles of city centers, as indicated by the horizontal axis. In Figure 3b, regressions use tracts near Interstate segments open by year x, as indicated by the horizontal axis.

with historical evidence that opposition to urban freeways was mostly concentrated in central neighborhoods, as in the Greenwich Village protests against the Lower Manhattan Expressway proposal. Tracts within 10 miles of city centers continue to see relatively low correlations between proximities to planned and built freeway networks of less than 0.7. For tracts farther than 10 miles from city centers, the correlation between planned and built freeways increases, indicating that suburban freeways were likely to be completed according to plan. Campbell and Hubbard (2016) find that in rural areas outside cities, plans were largely implemented as originally specified. Thus, deviations from planned routes seem to be a uniquely central-city phenomenon. The fact that planners appeared to have little difficulty in following plans elsewhere suggests the influence of a uniquely central-city factor—perhaps the freeway revolts.

Second, over time, the correlation between planned and built freeways declined faster and farther in central neighborhoods. In Figure 3b, we conduct a similar exercise as before, except we group tracts according to the year that the nearest built freeway was first open to traffic. Tracts near freeways opened 1955–1957 saw high correlations between proximity to planned and built freeways: over 0.95. However, this correlation fell as new freeways were built along alignments that deviated from planned routes. By 1993, the last year observed in the PR-511 database, the correlation had fallen to 0.86. The decline in spatial correlation between planned and built routes was especially sharp in central neighborhoods, again consistent with opposition concentrated downtown. The correlation coefficient fell from 0.85 in 1955–1957 to 0.68 in 1993. This divergence is also consistent with the timeline of policy changes that ceded more power to neighborhood interests over the 1960s.

Third, we document the changing importance over time of various factors in predicting freeway routes. We construct an annual tract–year panel between 1956 and 1993 and estimate

$$1(f_{g[m]t}) = \alpha_{mt} + Z_g'\beta_t + X_g'\gamma_t + \epsilon_{gt} \tag{1}$$

where $1(f_{gt})$ is an indicator for whether tract g intersects a freeway by year t.[15] A metropolitan area fixed effect α_{mt} ensures that identification comes from variation within metropolitan areas. A vector of persistent factors (Z_g) includes indicators for proximity within one-half kilometer to the nearest coastline, river, lake, park, seaport, and historical rail line, and flexible controls for distance to the city center and for average slope. We also include a vector of initial tract characteristics measured in 1950 (X_g) which includes population density, education, race, income, housing prices and rents, and housing age. These characteristics are standardized to have mean zero and standard deviation 1 within a metropolitan area.

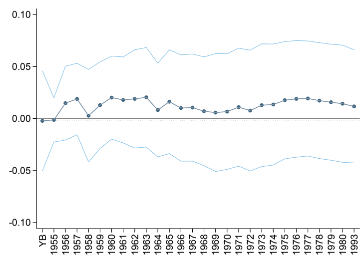
Our goal is to understand the neighborhood factors that predicted selection into the freeway program, and how this predictive relationship evolved over time as the revolts intensified. We estimate equation 1 separately for the planned Yellow Book routes of $t = 1955$ and each year between 1956 and 1993, when the PR-511 database ends. The predictive relationship between initial tract characteristics X_g and Z_g and freeway selection in year t varies over time as the network was built out. By 1993, 26 percent of our sample tracts were "treated" by a freeway.

Figure 4 shows estimates for selected regressors of interest from 28 year-by-year regressions.[16] The vertical axes measure the estimated coefficient of interest ($\hat{\beta}_{it}$). For the linear probability model, the coefficient can be interpreted as the increase (or decrease) in probability associated with a one-unit increase in the regressor indicated by the panel title, conditioned on the other regressors.[17] Thus, the panels show the evolution of the correlation between built freeways and (a) proximity to the coast, (b) proximity to a river, (c) proximity to a historical railroad, (d) 1950 population density, (e) the 1950 black share, (f) the 1950 college share, (g) median household income in 1950, and (h) the median value of owner-occupied housing in single-unit structures in

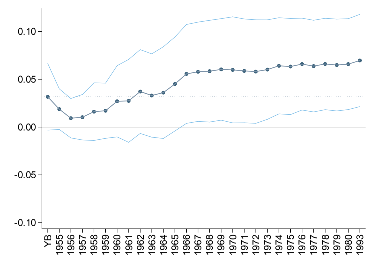
[15] This is a cumulative measure, so that in each year freeway proximity is calculated based on the entire history of freeway openings. This method avoids problems of serial and spatial correlation in the evolution of the highway stock.

[16] Table B.2 displays estimation results for the Yellow Book of 1955 and the completed Interstate network as of 1956, 1960, 1970, and 1980. By 1980 about 95 percent of the eventual mileage had been completed. Table B.3 displays estimates from a corresponding logistic regression, with similar results.

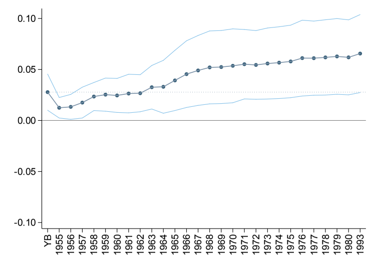
[17] The appendix contains detailed estimation results, including a logit model which produces similar results.



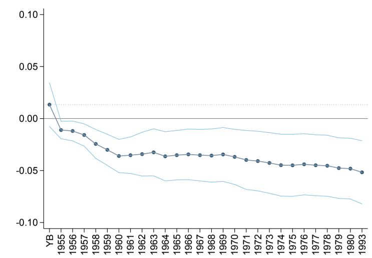
(a) Indicator for proximity to coast



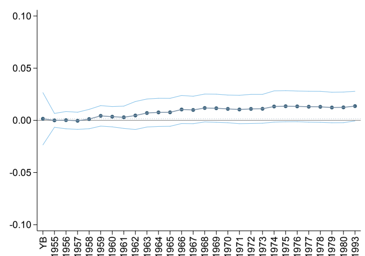
(b) Indicator for proximity to river



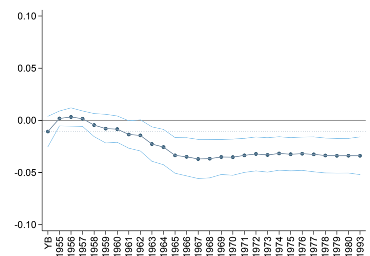
(c) Indicator for proximity to pre-1898 rail



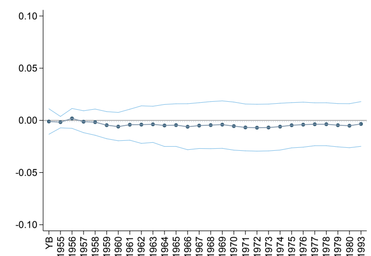
(d) 1950 population density



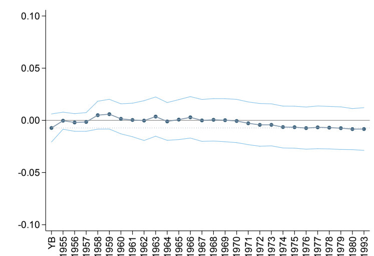
(e) 1950 population share black



(f) 1950 population 25+ share college graduate



(g) 1950 median household income



(h) 1950 median house value

Figure 4: Selection of freeway routes over time by natural, historical, and initial factors

Each panel reports estimates from 28 separate regressions. First point labeled "YB" shows estimated coefficient and 95% confidence interval from fixed-effects regression of the proximity to the nearest Yellow Book plan route on controls for natural and historical factors as shown in Table B.2. Subsequent points show estimated coefficient from fixed-effects regressions of proximity to the nearest freeway open to traffic by that year. All regressions include controls for indicators for distance to the city center, average slope, and proximity to coast, river, lake, park, and seaport, and standardized 1950 population density, black share, college share, median household income, median values for single-unit structures, median rents, and median housing unit age. Standard errors clustered on metropolitan area. Regressions use observations of 14,930 consistent-boundary tracts in 50 metropolitan areas.

1950. (Coefficient estimates for other factors are reported in Table B.2.) The first point of each panel and the dashed horizontal lines show baseline estimates using the Yellow Book ("YB") plan. In general, the 95% confidence intervals (in light blue) are wide. However, the selection dynamics accord with other historical evidence.

Figure 4a shows that in the Yellow Book plan, there was little correlation between freeways and coastlines. However, the completed network of Interstates was increasingly constructed in coastal neighborhoods. By 1993, coastal neighborhoods were 1–2 percentage points more likely to host an Interstate highway. The estimate is imprecise but it accords with other evidence. A virtue of coastlines for freeway construction is that they likely eased land assembly issues. Historically, many shorelines tended to be of public or industrial use, easing land acquisition and rights of way for freeways. In 1957, the American Association of State Highway and Transportation Officials (AASHTO) issued a new codification of standards for interstates in the so-called "Red Book." It offered specific suggestions for the location of urban freeways, including in blighted areas, adjacent to railroads or shore lines of rivers and lakes, and within or along parks or other large parcels owned by cities or institutions. In addition, the Red Book identified corridors of undeveloped land left over from historical development patterns: "The improvement of radial highways in the past stimulated land development along them and often left wedges of relatively unused land between these ribbons of development. These undeveloped land areas may offer locations for radial routes" (AAHSTO, 1957, p. 89). Thus, the Red Book emphasized land assembly and acquisition costs as a guiding principle for freeway route selection.

Figure 4b shows that freeway construction became more likely near rivers through the mid-1960s. Figure 4c shows that built highways increasingly followed historical railroads over time, again suggesting land assembly factors. In 1960, river and historical rail neighborhoods were about 2.5 percentage points more likely to have an Interstate compared with neighborhoods without those factors. By 1970, that premium had increased to about 6 percentage points. These patterns are consistent with the Red Book standards and historical evidence suggesting that urban freeways became increasingly difficult to build over the 1960s in the wake of citizen opposition and the growing freeway revolt.

Next, we turn to evidence on how the initial social characteristics of neighborhoods predicted freeway selection over time. Neighborhood factors in 1950 are standardized, so the coefficient estimates can be interpreted as the change in probability associated with a one-standard-deviation increase in the neighborhood factor in 1950.

Figure 4d shows that densely populated neighborhoods in 1950 were less likely to receive freeways compared with sparsely populated neighborhoods. In other regressions, we also find that among central neighborhoods, selection was even more negative on initial population density. This negative selection on initial population density, especially downtown, is relevant for the discussion of population growth effects in Section 5.

Figure 4e shows that in the Yellow Book, conditioned on natural factors and other 1950 covariates, black neighborhoods were no more likely to be assigned freeways. This continued to be true in the first several years of major Interstate construction. Beginning in the mid-1960s, completed freeways were increasingly located in black neighborhoods (circa 1950), until 1966 or so when the coefficient stabilizes at a level of 0.01. This estimate suggests that a neighborhood with a one-standard deviation increase in the black share in 1950 was 1 percentage point more likely to be assigned a freeway by 1966. Since the distribution of the 1950 black population share is bimodal, a more salient comparison may be that the predicted probability of freeway selection in 1966 was more than 6 percentage points higher for an all-black neighborhood compared with an all-white neighborhood, conditioned on natural factors and education, income, and population density.

Figure 4f shows that neighborhoods with high average educational attainment were less likely to receive freeways in the Yellow Book plan. Though the first freeways were uncorrelated with 1950 educational attainment, selection on initial educational attainment worsened steadily from the late 1950s to the late 1960s. The neighborhood college share is a strong predictor of freeway construction. By 1967, a one-standard deviation increase in the 1950 college share predicted a 3.7 percentage point decline in the probability of freeway selection.

These dynamics with respect to educational attainment confirm the predictions of the model of Glaeser and Ponzetto (2018). Interestingly, results shown in Figures 4g and 4h suggest that, conditioned on race and educational attainment, initial income or house values are not strong predictors of freeway selection, and the final Interstate network of 1993 closely follows the Yellow Book plan in terms of the conditional correlation with initial neighborhood income.[18]

In sum, freeway planning and construction evolved in response to the growing revolts of the late 1950s and 1960s. Completed freeways diverged from initial plans, especially in central neighborhoods, and increasingly favored factors such as coastlines, rivers, and historical rail routes, as well as neighborhoods that were initially more black and less educated. These patterns show that the revolts affected the allocation of freeways within cities, especially near downtowns.

5 Evidence from population growth

Freeways caused population declines in central neighborhoods and increases in outlying neighborhoods. To fix ideas, Figure 5 shows increases (blue) and decreases (orange) over 1950–2010 in census tract population density in the Chicago metropolitan area. The freeway network (red) features radials that converge toward the city center and several beltways. Four features are worth noting. First, outlying areas experienced population growth compared with central neighborhoods. This is consistent with the standard prediction of the monocentric city model, as travel costs declined more in the suburbs. Second, central areas experienced large *absolute* population losses. This may indicate declines in neighborhood amenities. Third, in central areas outside the Loop, population declines appear larger in neighborhoods near freeways. Fourth, in contrast, the pattern is less clear in peripheral neighborhoods, though in some cases neighborhoods near freeways seem

[18] We do not include 1950 housing prices as regressors because the 1950 census tract tables have poor coverage and do not include measures of housing quality or size. See the discussion in Section 8 for details.

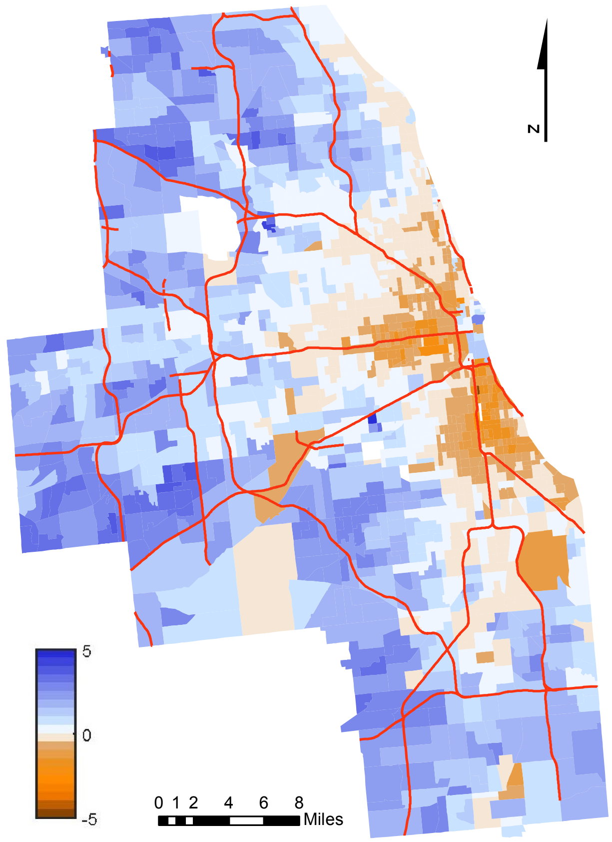


Figure 5: Central neighborhoods declined in population, especially near freeways

This map shows 1950–2010 changes in the natural logarithm of population for consistent-boundary census tracts in the Chicago metropolitan area. The geographic extent is determined by census tract data availability in 1950. Sources: NHPN, NHGIS.

to have experienced larger population increases compared with those farther away.[19]

Across cities, population declined in central neighborhoods near freeways, but increased in outlying neighborhoods near freeways. Figure 6 summarizes these patterns for all census tracts in all 64 metropolitan areas in our sample.[20] We divide the tract sample into four bins by distance to the city center: 0–2.5 miles, 2.5–5 miles, 5–10 miles, and more than 10 miles from the city center.[21] Each line in Figure 6a shows kernel-weighted local polynomial smooths of the 1950–2010 change in the natural logarithm of consistent-boundary tract population.[22] Figure 6b shows that the median sample tract is quite close to a freeway: near city centers, over three-quarters of tracts are within

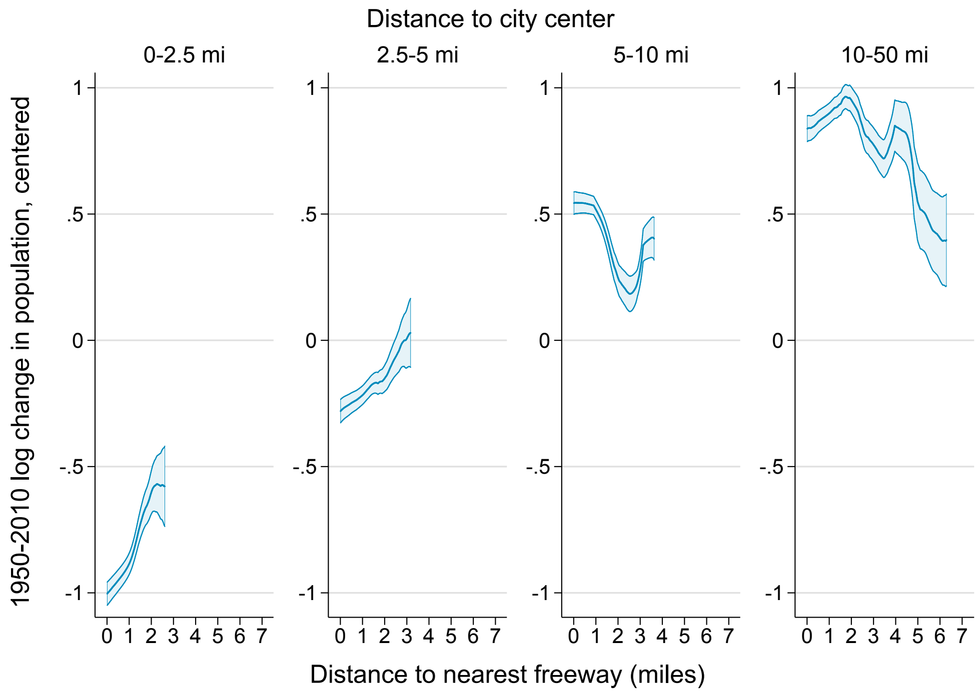
[19]Our analysis excludes exurban areas that were not tracted in 1950. A glance at current development patterns outside of the 1950 footprint of the Chicago metropolitan area suggests that population growth was strongest near freeways.

[20]Metropolitan areas are core-based statistical areas as defined in 2010.

[21]Of the 64 metropolitan areas in our sample, 38 have tracts beyond 10 miles.

[22]To account for variation across cities in overall population growth, tract changes are centered around their metropolitan area means. Each smooth ends at the 99th percentile consistent-boundary tract by distance to the nearest freeway, so e.g., 99 percent of tracts within 2.5 miles of the city center are within 2.8 miles of a freeway.

(a) Change in population by distance to freeway and distance to city center



(b) Cumulative distribution of neighborhood distance to freeway

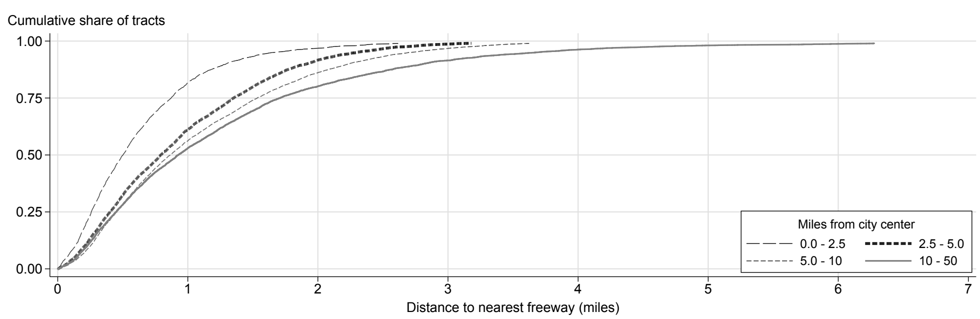


Figure 6: Neighborhoods near freeways declined in central areas and grew in the periphery

The plots in panel (a) show kernel-weighted local polynomial smooths of the 1950–2010 change in the natural logarithm of consistent-boundary tract population for neighborhoods in 64 metropolitan areas. Changes in log population are centered around their metropolitan area means. Each line represents smooths for a separate subsample conditioned on distance to the city center, as indicated by the line labels. Smooths use Epanechnikov kernel with bandwidth 0.5 and local-mean smoothing. Shaded areas indicate 95 percent confidence intervals. Each smooth ends at the 99th percentile consistent-boundary tract by distance to the nearest freeway. Panel (b) shows the empirical cumulative distribution of census tracts by distance to the nearest freeway and distance to the city center.

1 mile of a freeway.

These smooths confirm the patterns observed in Chicago and are consistent with the predicted effects of freeway disamenities. Population declined near city centers and increased in suburban areas following freeway construction. For neighborhoods within 5 miles from city centers, proximity to a freeway is negatively correlated with population growth, consistent with the idea that small access benefits are dominated by freeway disamenities. For neighborhoods farther than 5 miles from city centers, proximity to a freeway appears positively correlated with population growth, pointing to greater net benefits from freeways.

Next, we can more formally analyze the patterns shown in Figure 6 with regression:

$$\Delta n_{g[m]} = \alpha_m + \beta_1 d_F + Z_g' \gamma + \epsilon_g. \tag{2}$$

Here, $\Delta n_{g[m]} \equiv \log n_{g,2010} - \log n_{g,1950}$ is the change in the natural logarithm of population between 1950 and 2010 for neighborhood g in metropolitan area m. d_F is the distance from the neighborhood centroid to the nearest freeway, and Z_g is a vector of controls measuring fixed and persistent neighborhood factors. A metropolitan area fixed effect α_m ensures that identification comes from variation across neighborhoods, within metropolitan areas, in proximity to a completed freeway.

We estimate separately for subsamples conditioned on distance to the city center—0–2.5 miles, 2.5–5 miles, 5–10 miles, and 10-50 miles from the city center. This flexible specification allows us to test whether the effects of freeway construction on neighborhoods vary by proximity to the city center. The key test of the disamenity effect comes from the coefficient on distance to the freeway.[23] A positive estimate means that holding all else equal, neighborhoods farther from the freeway experienced higher population growth. In Section 2's simple framework, β_1 is positive in central neighborhoods only if there is a disamenity from being located near a freeway.

Table 1a shows estimates of equation 2.[24] Each column is a separate regression, using tracts conditioned on distance to the city center identified by the column title. The coefficient estimates have the expected sign and are precisely estimated. The coefficient on miles to freeway can be interpreted as the additional percentage growth in population for each additional mile a tract is located from the highway. For tracts closest to the city center, this effect is positive, meaning that tracts 1 mile from a freeway at the city center grew 24 percent more compared with those located next to the freeway. Additionally, looking across columns, this effect declines with distance to the city center. At 5 miles and more removed from the city center, tracts closest to freeways increased more in population compared with tracts farther from freeways. This is consistent with the idea that the relative importance of access versus amenity varies from the suburbs to the city.

The second row reports the estimated average metropolitan area fixed effect. This estimate can be interpreted as the average change in population for the subsample tracts conditioned on

[23] A disamenity would be also be consistent with the overall decline in population in the center of the city.

[24] Individual tract observations are weighted by the inverse of the number of tracts in the metropolitan area. We weight to obtain the average effect across metropolitan areas, instead of the average effect across tracts. See Appendix D.2 for similar results later without weights.

Table 1: Freeway neighborhoods declined in city centers and grew in the periphery

	Distance to city center: 0–2.5 miles	2.5–5 miles	5–10 miles	10–50 miles
	(a) WLS estimates			
Miles to nearest freeway	0.241^c	0.118^c	-0.156^b	-0.072
	(0.076)	(0.034)	(0.075)	(0.059)
Average metro FE ($\bar{\alpha}_.$)	-0.677^c	0.075^b	1.091^c	1.634^c
	(0.049)	(0.033)	(0.091)	(0.099)
R^2	0.026	0.011	0.019	0.008
Neighborhoods	2,312	3,482	5,561	5,173
Metropolitan areas	64	63	56	38
	(b) ... with controls for natural and historical factors			
Miles to nearest freeway	0.165^c	0.076^b	-0.205^c	-0.062
	(0.059)	(0.031)	(0.071)	(0.042)

This table shows WLS estimates of equation (2). Each panel–column reports a separate regression. Neighborhoods are weighted by the inverse number of neighborhoods in the metropolitan area. All regressions include metropolitan area fixed effects. Estimated standard errors, robust to heteroskedasticity and clustering on metropolitan area, are in parentheses. ^{a}—$p < 0.10$, ^{b}—$p < 0.05$, ^{c}—$p < 0.01$. Regressions reported in panel (b) include controls for neighborhood proximity to nearest park, lake, seaport, river, coastline, and city center in miles, and four categories indicating average neighborhood slope. See Table D.1 for the complete set of estimates.

the distance to the city center noted in the column title and zero distance to the nearest freeway. Thus, freeway tracts within 2.5 miles of city centers declined 68 percent in population, while tracts outside 2.5 miles from city centers increased in population.

Table 1b shows estimates controlling for natural and historical factors: tract distance to the nearest river, lake, coastline, seaport, and city center, and 4 separate dummies for average tract slope.[25] The estimated coefficients on freeway proximity are similar when including these controls.

Of course, highways are not allocated randomly to neighborhoods. There are two potential selection margins. First, highways might be targeted to neighborhoods with greatest growth potential in order to maximize the benefits of public investment. On the other hand, highways might be routed through neighborhoods with less growth potential, perhaps for political economy reasons. Existing evidence on selection, at the municipality or metropolitan area level, is mixed. For example, Duranton and Turner (2012) find evidence that slow-growing or shrinking metros were allocated more highways. Other studies (Baum-Snow et al., 2017, Garcia-Lopez et al., 2015) suggest the opposite. Our analysis departs from earlier studies in that we consider the allocation of freeways to small geographic units—census tracts—compared with municipalities or larger regions.

We follow the literature on causal identification of highway effects including research by Chandra and Thompson (2000), Baum-Snow (2007), Michaels (2008), and Duranton and Turner (2012). We use both planned routes and historical routes as instruments for actual freeway routes, following

[25] A complete set of estimates is reported in Table D.1.

the typology of Redding and Turner (2015). We use neighborhood proximity to routes shown in the 1947 highway plan as an instrument for proximity to an actual limited-access freeway. As argued by Baum-Snow (2007), the objective of the 1947 plan was to improve travel *between* distant cities and national defense.[26] Thus, the plan is unlikely to be correlated with neighborhood growth factors. In fact, the planned routes were drawn at national, not regional or metropolitan, scales, so the routing of planned highways within metropolitan areas is determined by the number and orientation of nearby large metropolitan areas. For example, the north-south orientation of I-35 through Austin, Texas, was predicted by the orientation of Austin compared with Dallas (north) and San Antonio (south), rather than neighborhood-specific factors.

We also experiment with a variant of this instrument that instead connects via shortest-distance routes all city center pairs connected by the 1947 plan without going through an intermediate third city. This variant is correlated with the planned route instrument, except when a "curved" plan route is "straightened out." For example, the actual planned route between Las Vegas and Salt Lake City displays a notable curve; a second instrument shifts this route westward and northward to minimize the distance between the two cities.

We also use neighborhood proximity to historical routes as instruments. Identification relies on the premise that historical transportation routes, such as explorers' paths or rail lines, are unlikely to be correlated with current neighborhood characteristics. These routes are likely low-cost locations either due to topography (first nature) or for land assembly reasons (second nature). Following Duranton and Turner (2012), we use exploration routes in the 16th–19th centuries, digitized from the National Atlas (U.S. Geological Survey 1970), and historical railroads in operation by 1898 by Atack (2015).[27]

We re-digitized the plan and explorer route maps for this project. Previous work by Baum-Snow (2007) and Duranton and Turner (2012) uses cross-metropolitan area variation, so the map-based instruments constructed for those papers contain insufficient spatial detail for our analysis.

Table 2 shows instrumental variables estimates. (For presentation purposes, we have suppressed estimated coefficients for the same control variables as the specification reported in Table 1b.) Panel (a) uses neighborhood distance to the nearest 1947 plan routes and shortest-path routes between 1947 plan cities as instruments for miles to nearest freeway. Panel (b) uses neighborhood distance to the nearest 1898 rail route and pre-1890 exploration route as instruments. Panel (c) uses all four instruments together. The IV estimates reveal qualitatively similar patterns compared with the WLS estimates. The negative freeway effects (positive coefficients) estimated for city centers attenuate with distance to the city center. The IV estimates are larger than those obtained from the OLS exercise, especially for the subsamples of neighborhoods closest to the city center. The inflation of the IV estimates suggests that the causal effect of freeways is larger (more negative)

[26]See Figure D.1.

[27]There are several potential concerns about the validity of these planned and historical route instruments. One, historical trade patterns between neighboring cities may have created industrial corridors along older arterial roads. These may have persistent (dis)amenity value. Two, topography (determining exploration routes) or railroads might have persistent amenity value. Thus, the tests of overidentifying restrictions are of interest.

Table 2: Freeway neighborhoods declined in city centers and grew in the periphery (IV estimates)

	Distance to city center:			
	0–2.5 miles	2.5–5 miles	5–10 miles	10–50 miles
(a) IV estimates using 1947 inter-city plan and shortest-distance route				
Miles to nearest freeway	1.432^{b}	0.252	0.112	-0.017
	(0.683)	(0.228)	(0.341)	(0.266)
Kleibergen-Paap LM test (p)	0.114	0.006	0.077	0.130
Cragg-Donald Wald (F)	11.2	45.8	56.0	74.6
Kleibergen-Paap Wald (F)	2.3	6.9	3.3	2.6
Hansen J test (p)	0.995	0.946	0.893	0.485
(b) IV estimates using 1898 railroad and pre-1890 exploration routes				
Miles to nearest freeway	0.859^{c}	0.706^{c}	0.724	0.286
	(0.273)	(0.220)	(0.574)	(0.259)
Kleibergen-Paap LM test (p)	0.004	0.004	0.018	0.056
Cragg-Donald Wald (F)	124.8	95.7	40.4	120.7
Kleibergen-Paap Wald (F)	17.0	10.1	4.2	4.3
Hansen J test (p)	0.592	0.092	0.749	0.468
(c) IV estimates using all plan and historical route instruments				
Miles to nearest freeway	0.888^{c}	0.562^{c}	0.368	0.177
	(0.273)	(0.184)	(0.335)	(0.198)
Kleibergen-Paap LM test (p)	0.012	0.003	0.013	0.061
Cragg-Donald Wald (F)	64.2	67.7	47.3	88.9
Kleibergen-Paap Wald (F)	10.7	7.7	3.7	3.4
Hansen J test (p)	0.726	0.125	0.813	0.576

Each cell is an estimate from a separate fixed-effects instrumental-variables regression of the logarithm of the 1950–2010 change in consistent-tract population on distance to nearest highway in miles and controls as in Table 1, Panel (b). All regressions include metropolitan area fixed effects. Estimated standard errors, robust to heteroskedasticity and clustering on metropolitan area, are in parentheses. ^{a}—$p < 0.10$, ^{b}—$p < 0.05$, ^{c}—$p < 0.01$.

than what simple growth rates suggest. In other words, highways were generally allocated to neighborhoods that had high growth potential. Historical and statistical evidence (presented previously in section 4) suggests that urban highways, particularly in city centers, were actually built along previously less-developed and less-dense "corridors" left behind by previous radial development patterns. The IV estimates suggest that central-city freeways influenced by planned or historical routes caused especially large neighborhood population losses, compared with the average central neighborhood allocated a freeway. Intuitively, complier routes ended up plowing through dense, long-developed neighborhoods and had very negative effects.

Instrumentation is fairly strong. To test for underidentification, we report p-values for the Kleibergen-Paap (2006) LM test. The null hypothesis that the equation is underidentified is strongly rejected for every specification. To test for weak instruments, we report the Wald statistics of Cragg-Donald (1993) and Kleibergen-Paap (2006), the latter of which is robust to non-i.i.d. errors (in particular, clustering on metropolitan area). These statistics suggest that weak instruments are not a major concern, especially for the two subsamples within 5 miles of city centers. For peripheral neighborhoods beyond 5 miles from the city center, the cluster-robust F-statistic relatively small. The already-large standard errors and confidence intervals that substantially overlap the WLS estimates underline the extent to which weak instruments may pose a challenge to inference about the causal effects of freeways in suburban locations. Finally, we also test the overidentifying restrictions by reporting p-values from a Hansen (1982) test. Overall, we fail to reject the null hypothesis that the full set of instruments is valid.

6 Evidence from job growth

There is little evidence that increases in productivity or firm demand because of freeways are confounding our population growth estimates. In section 5, we inferred freeway disamenities from population declines near central-city freeways. While this conclusion is consistent with the basic monocentric city model, the simple model abstracts from firm location decisions. If firms endogenously choose neighborhoods, then population declines may also reflect increasing bid-rent by firms for land near freeways. (The model presented in section 9 does allow for endogenous firm location.) For example, the growth of large suburban shopping centers near highways ("edge cities") seems to reflect improved productivity rather than decreased amenity (Garreau, 1991). In particular, it would challenge our interpretation of freeway disamenities if population declines near central-city freeways were caused not by declines in amenity value but by increases in firm demand.

A challenge for evaluating the role of firms and productivity growth is obtaining suitable data. In this section, we estimate the effects of freeway proximity on neighborhood job growth. Standard modern measures of employment such as the Economic Census or covered Unemployment Insurance records, which could shed light on firm location decisions, suffer from poor industry and spatial coverage in the early 1950s. Instead, we use data constructed from historical household travel surveys to identify the location of jobs in the 1950s. These household travel surveys record trip characteristics for a reference day or period.[28] They record trip origins and destinations at precise latitudes and longitudes, the purpose of each trip, the mode of travel, and the time spent traveling. By combining information on trip *destinations* with trips with the stated *purpose* of going to work, we are able to measure the location of jobs.[29]

We use data from surveys conducted in the Detroit metropolitan area in 1953 and the Chicago metropolitan area in 1956. These surveys were methodologically advanced—the Detroit study

[28] They are also referred to as "trip diary" or "origin-destination" surveys. Modern versions of these surveys include the National Household Travel Surveys in 2001 and 2009 (previously the National Personal Transportation Surveys of 1969, 1977, 1983, 1990, and 1995) and the Census Transportation Planning Products in 1990 and 2000.

[29] Travel surveys have their origin in the early 20th century, as planning for interregional highways began (Levinson and Zofka, 2006). The Bureau of Public Roads (now the FHWA), in coordination with states, metropolitan planning organizations, and municipal government, developed the modern survey methods still in use following modest funding from the Highway Act of 1944. Schmidt and Campbell (1956) note that at least 45 cities or metropolitan areas conducted household travel surveys between 1946 and 1956. Unfortunately, most of these surveys that predate the Interstate highway construction have apparently been lost.

"put together all the elements of an urban transportation study for the first time" (Weiner 1999, p. 26). The Detroit and Chicago surveys used large stratified samples of about 3 and 4 percent of the metropolitan population, respectively. They are structured similarly compared with modern travel surveys, they record both work and non-work trips, and they provide detailed geographical information. We re-discovered the Detroit trip-level microdata; the last significant use of these microdata appear to have been by Kain (1968) in his pioneering study of segregation and spatial mismatch. Unfortunately, the household- and trip-level microdata from the Chicago survey appear to be lost; a representative of the extant metropolitan planning organization reponsible for the 1956 survey reported that the original records were discarded several years ago during an office relocation. Instead, we digitize summary information on employment by sector and zone, a small geographic unit unique to the travel survey, from Sato (1965). We combine this information with published land-use survey maps conducted at the same time to assign employment by sector and zone to census tracts (State of Illinois et al., 1959). For Detroit, we aggregate jobs to census tracts using the survey's latitude and longitude for trips to work and the sample weights.

Estimates of jobs from these travel surveys tend to match well aggregates reported by other sources (see Appendix A.) For modern estimates of jobs by census tract, we use the Census Transportation Planning Product from 2000 for Chicago and the 1994 Detroit travel survey, whose structure followed very closely the original 1953 survey.

Figure 7 summarizes patterns of long-run population and job growth for census tracts in the Chicago metropolitan area. Each panel represents subsamples conditioned on distance to the city center. Each line shows kernel-weighted local polynomial smooths of the change in the natural logarithm of tract population or employment. Several features are worth noting. One, the relationship between population growth and proximity to freeways and the city center corresponds to the patterns observed in Figure 5 and is similar to the pattern observed across all U.S. cities seen in Figure 6. Population declined in central Chicago, both in absolute terms and compared with the periphery. Further, population declines near freeways are most pronounced at the city center. Two, employment declined in central Chicago up to 5 miles from the city center. Three, among central neighborhoods, those assigned new freeways saw larger employment declines compared with downtown neighborhoods farther from freeways. (Confidence intervals are wide, however.) Four, among neighborhoods more than 10 miles from the city center, those assigned new freeways saw larger employment gains compared with outlying neighborhoods farther from freeways. Interestingly, tracts that lost population also tended to lose jobs. Population and job growth are positively correlated, with correlation coefficients of 0.40 and 0.41 in Chicago and Detroit, respectively. In sum, Figure 7 does not support the hypothesis that increases in firm demand caused by freeways displaced households in central areas.

Table 3 shows regressions of long-run changes in population and employment on freeway proximity for three categories of tracts in Chicago and Detroit by distance to the city center.[30] Panels

[30]There are two differences between these regressions and those reported in Tables 1 and 2. One, we aggregate the downtown tracts within 5 miles into one category because of small sample sizes. Two, we omit controls for average slope since they are not identified in many regressions; Chicago and Detroit have little variation in slope.

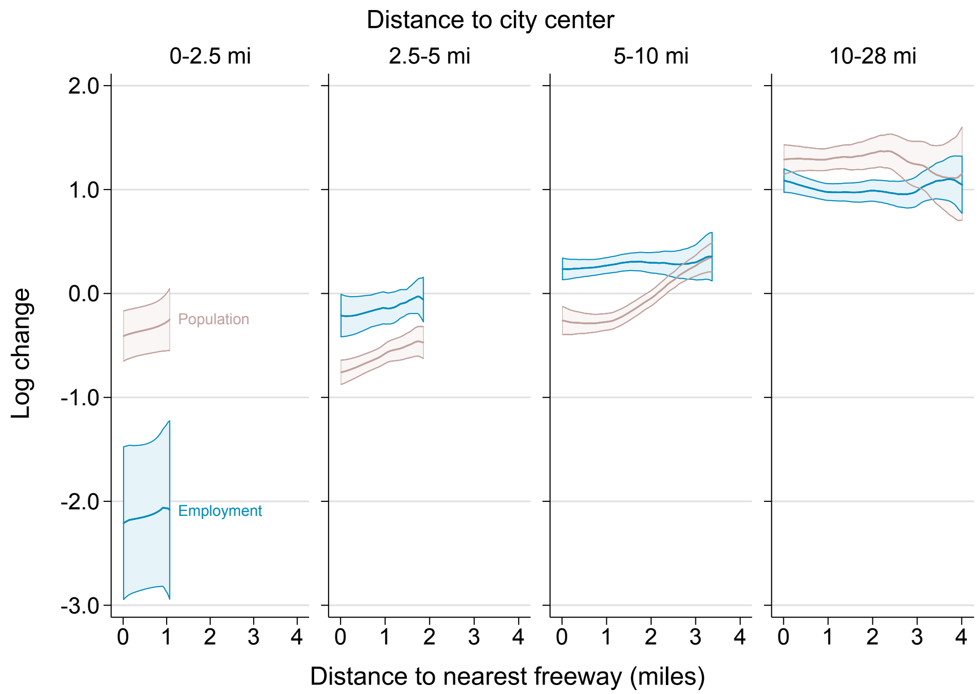


Figure 7: Changes in population and employment in Chicago

Lines show kernel-weighted local polynomial smooths of the 1950–2010 change in the natural logarithm of consistent-boundary tract population or the 1956–2000 change in the natural logarithm of consistent-boundary tract employment for neighborhoods in the Chicago metropolitan area. Smooths use Epanechnikov kernel with bandwidth 0.4 and local-mean smoothing. Shaded areas indicate 95 percent confidence intervals.

(a) and (b) replicate regressions presented in Tables 1b and 2c and show similar results. Freeways are associated with population declines downtown and population increases in peripheral area. The IV results in panel (b) support a causal interpretation, though in Detroit, especially for the downtown sample, instrumentation is weak and confidence intervals are wide. Panels (c) and (d) show regressions of the 1956–2000 (Chicago) and 1953–1994 (Detroit) change in tract employment on miles to the nearest freeway and controls as in Table 1b. In downtown Chicago, jobs increased more farther from freeways, while in suburban Chicago, jobs increased more close to freeways. Both the OLS and IV estimates are consistent with the patterns seen in Figure 7, although they are not precisely estimated. Thus, we cannot reject null effects on jobs. The Detroit results are mixed. The OLS estimates suggest that job growth was faster near downtown freeways compared with suburban freeways, but the IV estimates suggest that freeways caused slower job growth in central neighborhoods. Again, the estimates are imprecise, so we cannot reject null effects.

Overall, the results from Chicago and Detroit suggest that freeways did not cause job growth in central neighborhoods. In Section 8, we show that land prices increased faster away from freeways in downtown neighborhoods. In Section 10, we show that freeway proximity is not associated with increased productivity using recovered structural productivity residuals from our quantitative

	Chicago			Detroit		
	Distance to city center:			*Distance to city center:*		
	0–5 miles	5–10 miles	10–28 miles	0–5 miles	5–10 miles	10–21 miles
(a) Change in population – OLS						
Miles to freeway	0.403^c	0.140^c	-0.114^c	0.095	0.073	-0.049
	(0.092)	(0.034)	(0.040)	(0.151)	(0.046)	(0.057)
Neighborhoods	263	460	648	105	218	207
(b) Change in population – IV						
Miles to freeway	0.220^a	0.332^c	-0.915^c	0.463	0.153	-0.192
	(0.113)	(0.057)	(0.196)	(0.351)	(0.111)	(0.126)
KP LM test (p)	0.000	0.000	0.000	0.031	0.000	0.000
CD Wald (F)	68.3	59.4	9.5	6.3	12.8	13.9
KP Wald (F)	73.7	69.8	8.5	3.8	12.4	11.2
Hansen J test (p)	0.000	0.000	0.000	0.194	0.082	0.000
(c) Change in employment – OLS						
Miles to freeway	0.112	-0.035	-0.080^b	-0.315	-0.228	-0.053
	(0.210)	(0.036)	(0.033)	(0.595)	(0.201)	(0.176)
(d) Change in employment – IV						
Miles to freeway	0.245	-0.179^c	0.175	0.960	-0.031	0.359
	(0.292)	(0.058)	(0.156)	(1.438)	(0.340)	(0.345)
KP LM test (p)	0.000	0.000	0.000	0.139	0.000	0.000
CD Wald (F)	68.3	59.4	9.5	4.7	11.5	6.8
KP Wald (F)	73.7	69.8	8.5	2.2	9.4	5.9
Hansen J test (p)	0.000	0.000	0.007	0.024	0.670	0.000

Table 3: Effect of freeways on population and employment in Chicago and Detroit

Each panel–column reports a separate regression. Estimated standard errors, robust to heteroskedasticity, are in parentheses. ^{a}—$p < 0.10$, ^{b}—$p < 0.05$, ^{c}—$p < 0.01$. Regressions reported in panel include controls for neighborhood proximity to nearest park, lake, seaport, river, coastline, and city center in miles.

model. Taken together, these results suggest that increases in firm demand are unlikely to explain declines in population near downtown freeways.

7 Evidence from travel flows

Using travel survey data, we estimate the *barrier effects* of freeways—that is, reduced accessibility and increased travel costs to destinations on the opposite side of a freeway. Actual barriers, such as the Berlin Wall, can block spatial spillovers (Ahlfeldt et al., 2015; Redding and Sturm, 2008). Less is known about the effects of *pseudo*-barriers such as rail lines or highways. Ananat (2011) uses historical rail lines as an instrument for variation in racial segregation across cities, noting that railroads tend to delineate neighborhoods. Historically, they offered white households a “retreat” from the influx of black households during the Great Migration. Quoting Schelling (1963), Ananat suggests the role of railroads in the coordination of expectations among households, realtors, and others in maintaining racially segregated neighborhoods. Alternatively, by severing the network of streets, railroads also increase the cost of cross-neighborhood interaction. Our contribution is to provide the first evidence of barrier effects from freeways using travel time and flow data.[31]

We analyze trip flows using the Detroit survey from 1953 and the follow-up survey conducted in 1994. Using origin and destination latitudes and longitudes, we construct a panel of travel flows and times between census tract pairs in 1953 and 1994.[32] Then, we estimate a "structural gravity" equation that describes travel flows π_{jk} from origin tract j to destination tract k in period $t \in \{1953, 1994\}$ (Head and Mayer, 2014). This equation follows from the commuting probabilities in Section 9's structural model, except that constant terms are subsumed into fixed effects.

$$\pi_{jkt} = \rho_{jt} \varsigma_{kt} \upsilon_{jk} e^{\mu \tau_{jkt}} \tag{3}$$

Here, origin-year (ρ_{jt}) and destination-year fixed effects (ς_{kt}) capture neighborhood-specific characteristics such as prices, wages, amenity and productivity in each year, origin-destination fixed effects (υ_{jk}) capture pair-specific characteristics that are time invariant, such as pair distance and fixed transportation infrastructure, travel costs are $d_{jk} = e^{\kappa \tau_{jkt}}$, and τ_{jkt} is the cost of traveling from tract j to tract k in year t. The parameter $\mu = -\epsilon\kappa$ is the semi-elasticity of commuting flows with respect to travel costs.

We would like to estimate how the construction of Interstate freeways affected travel volumes π_{jkt} and travel costs τ_{jkt}. First, we assume that travel costs are a function of distance and the freeway network. The effects of distance and other fixed transportation infrastructure are absorbed in origin-destination fixed effects, but the effects of newly-constructed freeways may vary by tract-pair distance. This could be because the marginal cost of detours forced by fewer cross-freeway arterials is higher at shorter distances. At long distances, the benefits from increased travel speeds along freeways likely exceed any local disruptions to the surface street network.

Suppose $\tau_{jkt} = v_1 1(I_{jkt}) 1(D_{jk} < \Delta) + v_2 1(I_{jkt}) 1(D_{jk} \geq \Delta)$, where $1(I_{jkt})$ is an indicator for whether a freeway constructed between 1953 and 1994 crosses the shortest-distance path between tracts j and k, and $1(D_{jk} < \Delta)$ is an indicator for whether the shortest distance path between tracts j and k is within a threshold distance Δ. We use the PR-511 data to identify which freeway segments opened to traffic between 1953 and 1994. We perform separate estimations varying the distance threshold Δ to flexibly account for freeway effects that vary by trip distance.

One could estimate equation 3 by taking logs and assuming an additive i.i.d. error, but this is known to lead to biased estimates (Santos Silva and Tenreyro, 2006). In addition, with 855

[31] A large literature in ecology examines the effects of roads on the movement of wildlife (e.g., Forman and Alexander, 1998).

[32] Summary statistics can be found in Appendix A.6. Consistent with the decline in transportation costs, the average trip (for all purposes) in the Detroit metropolitan area lengthened from 3.7 to 5.1 miles. However, the median trip increased only from 2.6 to 2.7 miles. Trips by automobile increased from 82 percent to 88 percent. Trips to work (one-way) declined from 24 percent to 20 percent.

tracts in 1950, we have over 731,000 tract pairs. Given our relatively small sample size (about 250,000 sample trips in 1953 and 30,000 in 1994), a large share of tract pairs have zero observed flows.[33] Thus, using the logarithm transformation is problematic. Instead, we assume a multiplicative error η_{jkt} with $E[\eta_{jkt}|\alpha_t, \rho_{jt}, \varsigma_{kt}, \upsilon_{jk}, \tau_{jkt}] = 1$ and estimate equation 3 using the Poisson pseudo-maximum likelihood (PPML) estimator. Santos Silva and Tenreyro (2006) show that PPML produces consistent estimates and performs well in the presence of zeros.[34]

The origin-year and destination-year fixed effects absorb changes in the desirability of tracts as origins or destinations that may be caused by the construction of freeways. They also capture year-specific factors that affect all flows. Thus, identification comes from variation *within* origin, *within* destination, and *over time* within origin-destination pair.

(a) Percent change in total trips　　(b) Change in average trip time in minutes

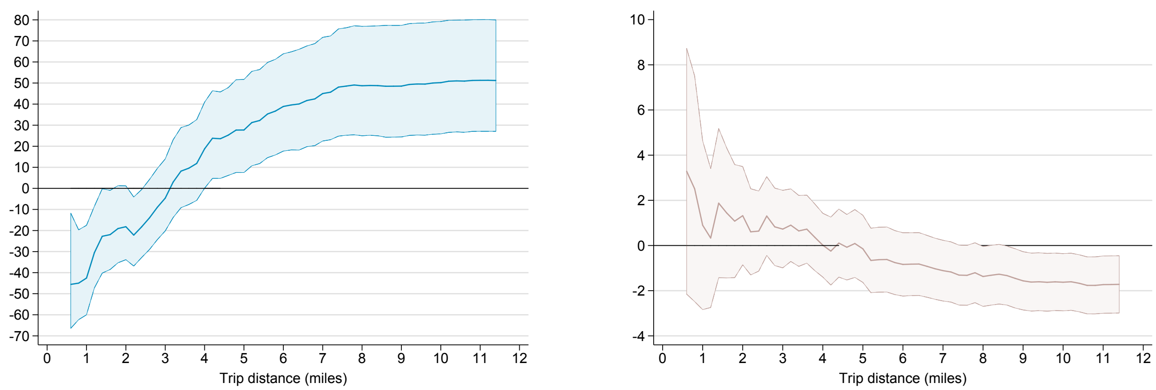


Figure 8: Effect of freeway crossing on volumes and times of trips up to x miles

These panels describe coefficient estimates from regressions of (a) the total volume of trips between a tract pair or (b) the average trip time between a tract pair on an interaction between a freeway crossing indicator and an indicator for trips of less than x miles, where x is indicated by the horizontal axis. We use a tract pair panel of trip flows and times from the Detroit metropolitan area in 1953 and 1994. The estimations include origin–destination, origin–year, and destination–year fixed effects. Panel (a) uses the Poisson pseudo-maximum likelihood estimator and panel (b) uses ordinary least squares. 90% confidence intervals shown.

Figure 8a shows PPML estimates of $e^{\widehat{\mu v_1}}$, the semi-elasticity of travel flows with respect to freeways at distances of less than a threshold distance Δ. Shaded areas show 90 percent confidence intervals using standard errors clustered on origin–year, destination–year, and origin–destination pairs.[35] The estimated parameter combines both the change in travel costs after the tract pair is "treated" with a bisecting freeway (v_1) with the response of trip demand (μ). Each connected point shows a separate estimation, varying the threshold distance Δ. The estimates are exponentiated, so the values can be interpreted as percentage changes. Thus, for trips of 2.5 miles or less, freeway construction is associated with a 20 percent decline in the volume of trips between 1953 and 1994.

[33] Two-thirds of tract pairs less than a mile apart have nonzero observed flows, but just 1.5 percent of pairs more than 10 miles apart have nonzero observed flows. Overall, 6.2 percent of tract pairs have nonzero observed flows.

[34] Head and Mayer (2014) show additional Monte-Carlo evidence showing good performance of the PPML estimator in the presence of "statistical" zeros.

[35] We use the estimator by Correia et al. (2019).

Most trips are 2.5 miles or less and about a quarter of trips are shorter than 1 mile, so these effects may be quantitatively important.[36] In contrast, trips up to 6 miles crossing freeways are associated with increases in travel volumes of about 33 percent. Over larger distances, freeways that bisect tract pairs can be thought of as offering a faster route compared with extant surface streets.

We also estimate the effect of freeways on the average reported travel time in minutes between tract pairs in a linear fixed-effects regression, absorbing origin–year, destination–year, and origin–destination fixed effects. These estimates are shown varying by trip distance in Figure 8b. The point estimates suggest that at distances less than a mile, trip times increase 3 minutes when tract pairs are bisected by freeways. Trips up to 3 miles increase 1–2 minutes when tract pairs are bisected by freeways. When we consider trips up to 5 miles, the point estimate suggests that freeways decrease travel times. For the average trip less than 10 miles, trip times decline nearly 2 minutes.[37] The point estimates are imprecise, but they are consistent with the changes in travel flows shown in Figure 8a.

Freeway routes may have been selected to divide neighborhood pairs where travel flows were expected to fall. If this was the case, then the estimates in Figure 8 cannot be interpreted as causal effects. However, to the extent that route choice was based on time-invariant factors, those will be accounted for in the tract-pair fixed effects υ_{jk}. In Appendix C, we provide additional details and results, including estimates using binned distances. We also estimate barrier effects using cross-sectional data on travel times from Chicago in 2000. Using the Chicago cross-section, we estimate similar barrier effects (up to 1.6 minutes) but over larger distances (up to 8 miles).

8 Other evidence

In Appendix D, we discuss additional evidence that freeway disamenities affected city structure. First, we explore the robustness of our population growth results in Appendix D.2. The results are robust to (i) controlling for 1950 tract characteristics including the black share of the population, average educational attainment, average household income, and average housing values and rents; (ii) excluding New York and Los Angeles, the two largest metropolitan areas; and (iii) ordinary least squares estimation without weights.

We also perform an analysis considering the effects of freeways with respect to access to another type of regional destination. Instead of binning tracts by distance to the city center, we bin tracts by distance to the nearest coastline. Coastlines potentially provide production benefits (i.e., job centers tend to be coastal) and consumption benefits (views, beaches, and moderate temperatures all complement recreational activities). Thus, coastlines tend to be desirable regional destinations. Whether they are destinations for production or consumption reasons, we expect that locations far from the coast benefit more from freeway access, while locations near the coast would experience mostly the freeway disamenity. We find similar effects compared with our city center results:

[36] See Table A.5.

[37] Because most trips are short, in 1953 the average trip less than 10 miles was 4.4 miles taking 22 minutes. The average trip less than 2 miles was 1.2 miles taking 10 minutes.

freeways have large negative effects for neighborhoods close to coastlines, and these negative effects attenuate with distance to the coast.

Using the PR-511 data on freeway completion dates, we also estimate short-run (less than 10 year) effects of freeways on population. These short-run effects are most negative for freeways completed in the 1950s and 1960s. Recall that early freeway routes were somewhat idiosyncratic and likely less selected on neighborhood factors. The strongly negative short-run effects for early freeways are consistent with the strong causal effects estimated with instrumental variables.

We also consider the effects of freeways on the spatial sorting of different income groups. We find that higher incomes sorted away from freeways, and this effect was larger in city centers compared with the suburbs. These results again suggest the importance of freeway disamenities. In Appendix D.3, we discuss identifying the source of these changing sorting patterns in the context of multiple forms of household heterogeneity.

We also estimate the effects of freeways on housing and land prices in Appendix D.4. Data availability is a challenge for these estimates; reliable measures of housing and land prices for small geographic units around 1950 are scarce. In particular, reported housing prices from the 1950 Census of Population and Housing suffer from two defects: (i) the universe of houses for which values are measured is owner-occupied units in single-unit structures, which tend to be scarce in downtown neighborhoods, and (ii) there are no reported measures of housing unit size or quality at the census tract level. That said, we find negative freeway effects on housing prices using these data and a similar concept from the 2006–2010 American Community Survey.

We also perform an analysis using appraised land values for 330 by 330 foot grid cells in the Chicago metropolitan area in 1949 and 1990 (Ahlfeldt and McMillen, 2014 and 2018).[38] Land values grew slower near freeways in central Chicago; in outlying areas, land values grew faster near freeways.

Floberg (2016) documents corroborating evidence on land use in downtown Bridgeport, Connecticut. She digitizes Sanborn fire insurance maps from 1913 and compares land use to a modern map from 2013. All types of private uses declined in central Bridgeport. Instead, land not covered by buildings increased from 69.5% in 1913 to 80.6% in 2013.

9 A quantitative model of freeway disamenities

We describe a spatial equilibrium model of city structure to measure and quantify the effects of freeway disamenities in the context of a realistic urban geography. The model builds on an existing class of quantitative spatial models that consider the joint location decisions of employment and population in a city with costly commuting.[39] We present basic features of the model as well as a few key derivations important for the solution and estimation of the model.

[38]These data were generously shared by Gabriel Ahlfeldt and Dan McMillen.

[39]Our formulation most closely resembles the model developed by Ahlfeldt et al. (2015). Other examples of related models include Allen and Arkolakis (2014); Monte, Redding, and Rossi-Hansberg (2018); and Severen (2019). For surveys of the literature see Redding and Rossi-Hansberg (2017) and Holmes and Sieg (2015).

In subsequent sections, we use the model to recover neighborhood amenities in the Chicago metropolitan area by matching cross-sectional variation in population, employment, and travel times. We use these recovered amenities to estimate freeway disamenities. We use the calibrated model to quantify welfare and decentralization effects when freeway disamenities are mitigated in a counterfactual simulation. Finally, we quantify two potential mechanisms that lead to these disamenities: land use exclusion and barrier effects.

9.1 Geography

A city has J neighborhoods, each with land area L_j that may be split between consumption and production. There are iceberg commuting costs between neighborhoods $d_{jk} \equiv e^{\kappa\tau_{jk}}$, where τ_{jk} is the travel time between neighborhoods j and k, and κ describes the relationship between travel time and costs. To start, we assume the city is closed and thus the total population is fixed at N and expected utility is endogenous. This allows for the comparison of counterfactual experiments in terms of expected utility. It is straightforward to model an open city within a larger economy where workers are free to leave the city. In this case, the population of workers N is endogenously determined by the outside reservation utility, $\overline{U}$. Relative prices and quantities between different neighborhoods *within* the city are independent of this modeling assumption for the functional forms chosen here.

9.2 Workers

Workers are homogeneous and have increasing preferences over consumption c, land l, and neighborhood amenities B_j.[40] Each worker m also has an idiosyncratic preference for a given home-work pair $\{j, k\}$. Utility is

$$U_{jk,m}(c,l) = \nu_{jk,m} B_j \left(\frac{c}{\beta}\right)^{\beta} \left(\frac{l}{1-\beta}\right)^{1-\beta},$$

where β is the consumption share of income. The idiosyncratic component $\nu_{jk,m}$ is drawn from a Frechet distribution with shape parameter ε.[41] Workers earn a wage net of commuting costs $w_k/d_{j,k}$. The workers' budget constraint is then $\frac{w_k}{d_{jk}} = lq_j + c$, where q_j is the price of land at place of residence j. Maximizing utility conditioned on wages and rents yields indirect utility for each commuting pair:

$$V_{jk,m}(w_k, q_j) = \nu_{jk,m} \frac{w_k}{d_{jk}} B_j \ln q_j^{(\beta-1)}.$$

[40] We assume direct consumption of land and thus do not explicitly model the production of housing. This is equivalent to assuming capital is mobile and that the housing production function is Cobb-Douglas. For evidence in support of this assumption see Thorsnes (1997), Epple, Gordon, and Sieg (2010), and Combes, Duranton, and Gobillon (2017).

[41] Formulations of this model often include location-specific mean-shifting terms in the Frechet distribution. These are important when measuring workplace amenities or when wages are used in estimation. Given our focus and identification strategy, we do not explicitly include these terms, and thus they are subsumed by the location specific amenity and productivity terms, B_j and A_k.

Individual workers choose a home and work location that maximizes utility. The probability that a worker will live in j and work in k is given by

$$\pi_{jk} = \frac{\left(d_{jk} q_j^{1-\beta}\right)^{-\varepsilon} \left(B_j w_k\right)^{\varepsilon}}{\sum_{j'=1}^{J} \sum_{k'=1}^{J} \left(d_{j'k'} q_{j'}^{1-\beta}\right)^{-\varepsilon} \left(B_{j'} w_{k'}\right)^{\varepsilon}}, \tag{4}$$

and the probability that a worker will commute to k conditioned on living in j is

$$\pi_{jk|j} = \frac{\left(\frac{w_k}{d_{jk}}\right)^{\varepsilon}}{\sum_{k'=1}^{J} \left(\frac{w_{k'}}{d_{jk'}}\right)^{\varepsilon}}.$$

This implies the commuting market clearing condition

$$N_{Wk} = \sum_{j=1}^{J} \left[\frac{\left(\frac{w_k}{d_{jk}}\right)^{\varepsilon}}{\sum_{k'=1}^{J} \left(\frac{w_{k'}}{d_{jk'}}\right)^{\varepsilon}} N_{Rj} \right] \tag{5}$$

where N_{Wk} is the measure of workers working in k and N_{Rj} is the measure of workers residing in location j. Total residential land consumption in a neighborhood is the sum of land demand by all workers choosing that neighborhood:

$$L_{Rj} = (1-\beta) \frac{N_{Rj}}{q_j} \sum_{k=1}^{J} \pi_{jk|j} \frac{w_k}{d_{jk}}. \tag{6}$$

Freeway disamenities. B_j represents nearly all neighborhood amenities, including natural factors such as beaches or endogenous factors such as schools, shopping, or safety. The notable exception is job accessibility, which is handled explicitly by the commuting structure of the model. We assume

$$B_j = b_j g\left(d_{Fj}\right),$$

where $g\left(d_{Fj}\right)$ describes the disamenity at a given distance to the freeway, d_{Fj}. For now, the disamenity is a simple function of distance to the freeway and does not depend on endogenous variables.[42] The freeway disamenity is

$$g\left(d_{Fj}\right) = 1 - b_F e^{-\eta d_{Fj}}, \tag{7}$$

where b_F is the size the disamenity and η describes the attenuation of the disamenity over space. This form is isomorphic to a cost that decays exponentially with distance to the freeway. Similar forms have been used to study the spatial costs of noise or pollution externalities.[43] Later, we show this functional form is consistent with estimated amenities near freeways.

[42] In the baseline case we do not explicitly model endogenous amenities as in Ahlfeldt et al. (2015). In Section 12 we introduce endogenous amenities to quantify barrier effects which directly affect consumption spillovers.

[43] See Nelson (1982) or Henderson (1977).

9.3 Production

There is a single final good that is costlessly traded and produced under constant returns and perfect competition:

$$Y_k = A_k L_{Wk}^{1-\alpha} N_{Wk}^{\alpha}.$$

A_k is total factor productivity in each location, L_{Wk} is total land used for production in each location, N_{Wk} is total employment in each location, and α is the labor share in production.

We treat the productivity of each location A_k as exogenous. Thus, we abstract from production spillovers. This does not affect the calibration or estimation of freeway disamenities but could affect counterfactuals through general equilibrium effects. However, in our experiments, production spillovers had little effect on the results. Thus, we omit them here for simplicity.

There is no production amenity or disamenity from freeways analogous to the consumption disamenity modeled by equation 7. This is consistent with our results from Section 6 showing null employment effects of freeways (Table 3). Later, we show that structural estimates of neighborhood productivity are uncorrelated with freeway proximity (Figure 10).

Profit maximization yields total commercial land use in each location:

$$L_{W_k} = N_{W_k} \frac{(1-\alpha)}{\alpha} \frac{w_k}{q_j}. \tag{8}$$

9.4 Equilibrium

To define equilibrium, first assume that land area and travel times $\{L_j, d_{jk}\}$, as well as total population N, are exogenous; we directly observe these objects in the data. In addition, values for the model's parameters $\{\alpha, \beta, \varepsilon\}$ and location fundamentals, $\{A_k, B_j\}$, are known. Equilibrium is then defined as a vector of prices $\{q_j, w_j\}$ and a vector of quantities, $\{N_{Hj}, N_{Wk}, L_{Hj}, L_{Wj}\}$ such that: (i) labor markets clear through the commuting market clearing condition described by equation 5, (ii) land markets clear such that land demand from equations 6 and 8 sum to land supply L_j in each location, and (iii) total population equals N.[44]

In practice, the model is solved iteratively. A detailed description of the solution method can be found in Appendix G. In order to extend the model to an open-city framework, total population becomes endogenous and an additional equilibrium condition is that expected utility is equal to the reservation utility:

$$E[u] = \Gamma\left(\frac{\varepsilon - 1}{\varepsilon}\right) \left[\sum_{j'=1}^{J} \sum_{k'=1}^{J} r_{j'} s_{k'} \left(d_{j'k'} q_{j'}^{1-\beta}\right)^{-\varepsilon} \left(B_{j'} w_{k'}\right)^{\varepsilon}\right]^{1/\varepsilon} = \overline{U}, \tag{9}$$

where Γ is the Gamma function.

[44] Ahlfeldt et al. (2015) provide proofs of existence and uniqueness, which extend in a straightforward way to the simplified environment here.

10 Calibration and estimates of freeway disamenities

Next, we calibrate model parameters and estimate freeway disamenities. We use literature estimates to set several global parameters in the model. These parameters, along with tract-level data on population, employment, land area, and commute times, allow us to recover neighborhood amenity and productivity values using the structure of the model. We then estimate freeway disamenities using the recovered amenities.

10.1 Data and calibration

We use data on tract employment, worker population, land area, and tract-to-tract commute times from the 2000 Census Transportation Planning Package for the Chicago metropolitan area.[45] Chicago provides a good setting given that it exhibits relatively centralized employment and radial commuting patterns. Chicago's relatively homogeneous topography (excluding readily observed features such as Lake Michigan) also seems prudent given selection issues outlined in Section 4.

We set values for four global parameters using previous estimates. (Later, we explore the sensitivity of these selections.) We set the consumption share to $\beta = 0.95$[46] and the labor share in production to $\alpha = 0.97$.[47] We set $\kappa = 0.02$, which implies that the wage value of time spent commuting is approximately half the wage rate.[48] Finally, we set $\varepsilon = 4$, which is in the middle of the range of estimates in the literature.[49]

Next, we estimate neighborhood productivity and amenity shifters $\{A_k, B_j\}$. Recall that these shifters contain both endogenous and exogenous components, including freeway disamenities. They are exactly identified using only data on residential population (N_{Rj}), employment (N_{Wk}), land area (L_j), and commuting costs ($d_{jk} = e^{-\kappa\tau_{jk}}$).[50] Through the lens of the model, places with high population density but inferior job access must have superior amenities. Analogously, neighborhoods with high employment density but inferior worker access must have superior productivity.

Rewriting equation 5, we solve for wages paid at each location:

$$w_k = \left(\frac{1}{N_{W_k}} \sum_{j=1}^{J} \frac{\left(\frac{1}{d_{jk}}\right)^{\varepsilon}}{\sum_{k'=1}^{J} \left(\frac{w_{k'}}{d_{jk'}}\right)^{\varepsilon}} N_{Hj} \right)^{-\frac{1}{\varepsilon}}.$$

Next, we use land market clearing and land demand by firms and workers (equations 6 and 8) to solve for land rents in each location:

[45] Commute times are only observed for origin-destination pairs that have non-zero commuting in the data. We use a local adaptive-bandwidth kernel density estimator to impute unobserved values. A description of the imputation method is found in Appendix F. The data also includes tract-to-tract commuting flows, which we do not use at this time.

[46] See Brinkman (2016), Davis and Ortalo-Magné (2011), and Davis and Palumbo (2008).

[47] See Brinkman (2016), Ciccone (2002), and Rappaport (2008).

[48] See Van Ommeren and Fosgerau (2009), and Small (2012).

[49] See Monte, Redding, and Rossi-Hansberg (2018), Ahlfeldt et al. (2015), and Severen (2019).

[50] We choose to use land area, population, and employment, given that they are precisely and easily observed quantities. The model could also be calibrated using land values, house prices, or wages.

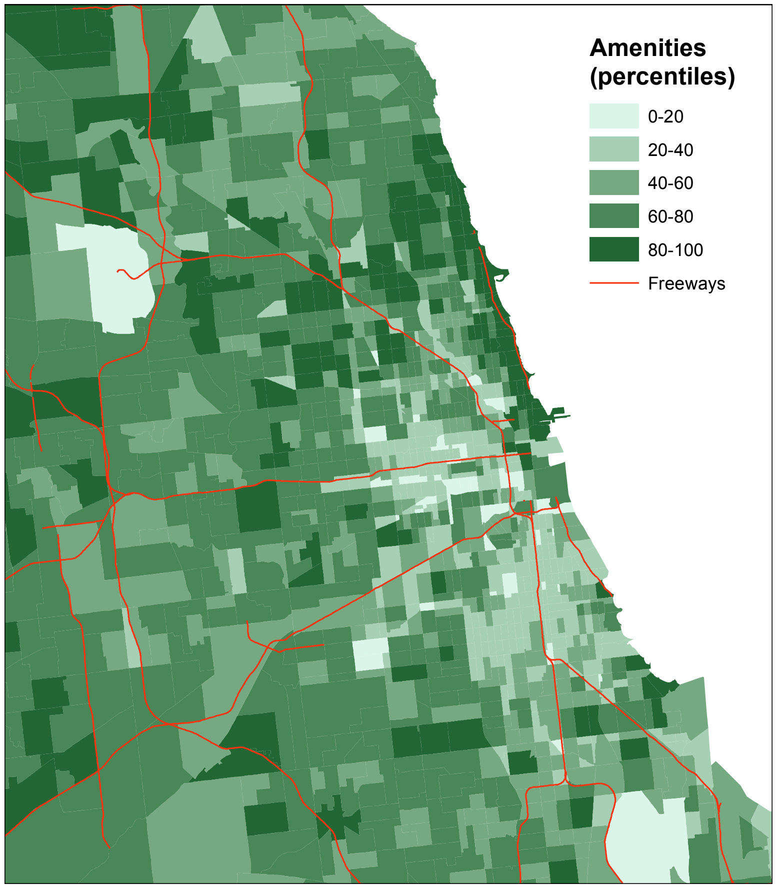


Figure 9: Estimated neighborhood amenities in Chicago

This map shows calibrated amenity values for tracts in the Chicago metropolitan area. Colors show quantiles of neighborhood amenities, with darker shades representing higher amenity neighborhoods.

$$q_j = \tfrac{1}{L_j}\left(N_{W_k}\tfrac{(1-\alpha)}{\alpha}w_k + (1-\beta)\,N_{Hj}\sum_{k=1}^{J}\pi_{jk|j}\tfrac{w_k}{d_{jk}}\right).$$

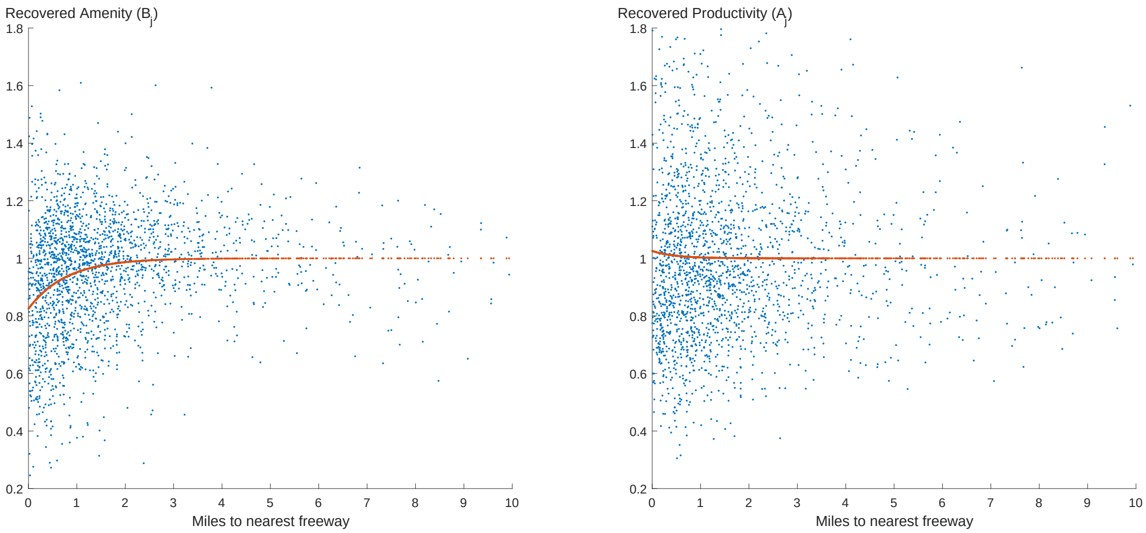
We recover neighborhood amenities using wages and rents and combining equations 4 and 9:

$$B_j = \left(\tfrac{N_{Hj}}{N}\right)^{\frac{1}{\varepsilon}}\left(\tfrac{\overline{U}}{\Gamma\left(\frac{\varepsilon-1}{\varepsilon}\right)}\right)\left(q_j^{1-\beta}\right)\left(\sum_{k=1}^{J}\left(\tfrac{w_k}{d_{jk}}\right)^{\varepsilon}\right)^{-\frac{1}{\varepsilon}}.$$

Finally, profit maximization and zero profits yield neighborhood productivity:

$$A_k = \left(\tfrac{w_k}{\alpha}\right)^{\alpha}\left(\tfrac{q_k}{(1-\alpha)}\right)^{1-\alpha}.$$

Recovered amenity values B_j in the Chicago metropolitan area are shown in Figure 9, with colors representing quantiles. The map shows higher amenity neighborhoods located north of downtown, especially along Lake Michigan, and also throughout the suburbs.



(a) Neighborhood amenity

(b) Neighborhood productivity

Figure 10: Amenities and productivity of neighborhoods near freeways

Panel (a) shows recovered amenity values from the calibration B_j versus distance to the nearest freeway (blue) and a fitted disamenity function (red). Panel (b) shows the recovered productivity of each tract A_j versus distance to the nearest freeway. The values in both plots are normalized by dividing by a scale factor such that the fitted function approaches one asymptotically.

10.2 Freeway disamenity estimates

We estimate the freeway disamenity function (equation 7) using nonlinear least-squares and the calibrated amenity values B_j.[51] The estimator of the vector $\{b_F, \eta\}$ is

$$\{\hat{b_F}, \hat{\eta}\} = argmin_{\{b_F,\eta\}} \sum_{j=1}^{J} (B_j - (1 - b_F e^{-\eta d_{Fj}}))^2.$$

Figure 10a shows recovered amenities for each tract versus distance to the nearest freeway. The fitted freeway disamenity function is in red. (We normalize so that the disamenity function asymptotes to 1.) For our baseline calibration, we estimate $\hat{b}_F = 0.175$ and $\hat{\eta} = 1.28$. Neighborhoods adjacent to freeways have 17.5 percent inferior amenities, and this disamenity attenuates by 95 percent at 2.4 miles away from the freeway.

Note that these estimates complement the reduced form evidence presented earlier. Here, disamenities are identified from neighborhoods that feature superior job access (i.e., low commuting times) but low residential populations. Interestingly, the spatial scale of these estimates is consistent with earlier evidence that (i) population growth effects extend out to 3–4 miles from central freeways (Figure 6) and (ii) barrier effects apply to trips up to 3–4 miles in length.

Figure 10b shows recovered tract productivities A_j. There is little effect of freeway proximity on productivity. We estimate a quantitatively small effect on productivity of 2 percent attenuating

[51]We fit the function in levels, which is a consistent estimator of the parameters. A more natural method might be to fit the function in logs, but this would require truncating the sample to remove zeros.

Table 4: Estimates of disamenity parameters and sensitivity to calibration

κ	β	α	ε	b_F	(s.e.)	η	(s.e.)	c_v	b_F/c_v
0.002	0.950	0.970	4.000	0.175	(0.012)	1.284	(0.131)	0.228	0.769
0.001	0.950	0.970	4.000	0.173	(0.012)	1.357	(0.143)	0.228	0.758
0.004	0.950	0.970	4.000	0.181	(0.011)	1.147	(0.110)	0.229	0.792
0.002	**0.930**	0.970	4.000	0.165	(0.014)	1.748	(0.218)	0.235	0.701
0.002	**0.970**	0.970	4.000	0.192	(0.009)	0.919	(0.077)	0.224	0.858
0.002	0.950	**0.980**	4.000	0.177	(0.012)	1.285	(0.130)	0.228	0.776
0.002	0.950	**0.960**	4.000	0.174	(0.012)	1.284	(0.132)	0.228	0.764
0.002	0.950	0.970	**2.000**	0.299	(0.015)	0.850	(0.074)	0.385	0.778
0.002	0.950	0.970	**6.000**	0.125	(0.011)	1.815	(0.226)	0.175	0.716

This table shows the estimates and standard errors of the freeway disamenity parameters, b_F and η, for various calibrated parameter vectors, shown in columns 1-4. The top row contains baseline estimates.

by 95 percent 1.4 miles from the freeway. However, these estimates are not statistically significant. Taken together with the reduced-form results that showed null employment effects, freeways appear to have little effect on neighborhood productivity.

The estimates of the freeway disamenity parameters b_F and η are mostly robust to calibrated parameters. Table 4 shows baseline estimates in the top row, with subsequent rows showing sensitivity to each of the calibrated parameters in turn. All parameter estimates are significant and positive for all specifications. The value of the Frechet parameter ε plays an important role in the estimates. For larger values of ε, the estimates of the disamenity are considerably smaller. This relationship is mechanical given that for larger values of ε, smaller variation in amenities is needed to rationalize the data.

The last two columns of Table 4 report the variation in neighborhood amenities and the strength of freeway disamenities relative to that variation. The second to last column shows the coefficient of variation c_v (the standard deviation divided by the mean) of neighborhood amenities B_j. For the baseline estimates, a one standard deviation increase is equivalent to a 22.8 percent increase in the amenity value. The sensitivity of the coefficient of variation is similar to the parameter estimates—again, for larger values of ε, smaller variation in amenities is needed to fit the data.

The last column shows the ratio of the disamenity scale parameter, b_F, to the coefficient of variation. For the baseline specification, the freeway disamenity is equivalent to a 0.77-standard deviation decrease in the overall neighborhood amenity distribution. The relative contribution of freeway proximity to amenities is robust to calibration choices.

There might be a selection bias due to the non-random location of freeways. In Appendix G we show results using an instrumental variable strategy following the reduced-form analysis. The IV estimates are slightly larger. We also condition on control variables for natural factors such as lakes and rivers. The estimates remain quantitatively similar.

11 Effects of mitigating freeway disamenities

We simulate a counterfactual policy that mitigates freeway disamenities. We assume that travel costs remain unchanged, but we mitigate freeway disamenities by setting the disamenity parameters to zero. Then, we recompute the equilibrium for the economy.[52] This policy is similar to real-world policies that attempt to mitigate these negative effects by burying or capping freeways, such as Boston's Central Artery/Tunnel Project, known informally as the Big Dig. Total costs of the Big Dig have been estimated at over $15 billion (Flint 2015). Our analysis attempts to understand the benefits of such a project.

Figure 11 shows changes in population density under the counterfactual policy using our baseline parameters. There are large gains in population near the freeways. In addition, the gains appear larger in high-amenity neighborhoods.

We consider three primary outcomes after mitigation: (i) the change in expected utility, (ii) the change in the share of worker population within 5 miles of the city center, and (iii) the change in population within the city of Chicago. In the data, there are 351,465 employed residents living within 5 miles of the CBD, representing 8.7 percent of the total population of the MSA, and 1,156,779 working residents living in the city of Chicago, or 28 percent of total working population. The policy simulation results are shown in Table 5 for various calibrations. The utility values and centralization measures are both calculated as ratios relative to the baseline.

For the baseline calibration (first row), the aggregate utility gains from disamenity mitigation are large: expected utility increases 5 percent. While the magnitude is large, it should be noted that this is a costly policy intervention akin to burying all freeways in the metro area. Estimated welfare gains are sensitive to calibration choices, ranging from a 2.6 percent gain up to 13 percent, with the results being most sensitive to the choice of ε.

There is also a large centralization effect from disamenity mitigation. Population grows 21 percent within five miles of the city center at the expense of population in outlying areas.[53] In the city of Chicago, population grows by 8 percent. The centralization result is robust, with increases in population in the city of Chicago ranging from 7 percent up to 10 percent.

Based on this result, it seems likely that freeway disamenities, versus commuting benefits, played a significant role in the decentralization of U.S. cities. Our results can be compared with Baum-Snow's (2007) estimate that the population of U.S. central cities would have been roughly 25 percent higher had freeways not been constructed.[54] Another benchmark is that the population of the city of Chicago declined by about 25 percent from 1950 to 2010. Our estimates suggest the population of the city of Chicago would increase about 8 percent if freeway disamenities were

[52] For all exercises in this section, we model a closed city where population in the entire city remains constant. This allows us to consider the effect on expected utility. It would be straightforward to perform the same analysis using an open-city framework. Note that relative effects between neighborhoods do not depend on this modeling choice: Rents, population, employment, and wages are the same in both specifications up to a scale factor.

[53] Although these results assume a closed city, recall that relative prices and quantities between neighborhoods within the Chicago metropolitan area are independent of an open or closed city for our chosen functional forms.

[54] Baum-Snow (2007) estimates that central city population would have grown by 8 percent had freeways not been constructed. In reality, central city populations declined by 17 percent in the aggregate over this time period.

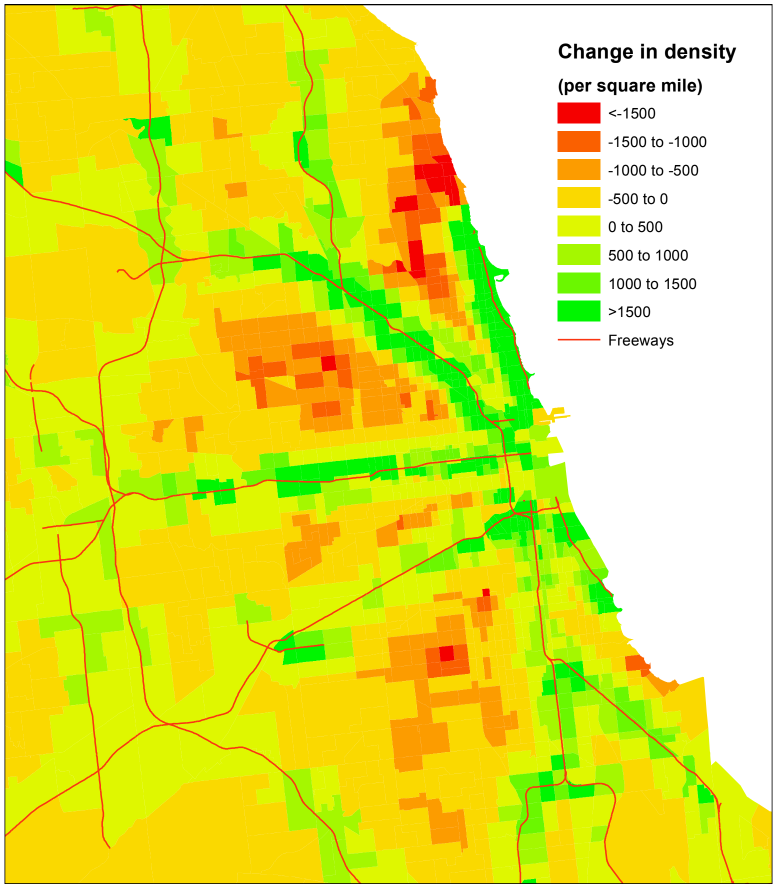


Figure 11: Change in population density after mitigation of freeway disamenities

This figure shows the effect on population density for the counterfactual experiment where all negative effects from freeways are mitigated for the entire metropolitan area. The colors represent changes in population density per square mile. Total population of the city is held constant.

mitigated today. Thus, it seems likely that freeway disamenities were a quantitatively important factor in suburbanization.

Finally, welfare gains from mitigation are concentrated in central neighborhoods. One, we consider a policy where mitigation is only implemented in neighborhoods within a certain distance of the city center. In Figure 12a, we plot the change in expected utility for the entire city as this threshold is moved progressively farther out. The marginal gains in expected utility from mitigation are highest for locations closest to the center, as exhibited by the steeper slope. There is little additional benefit from capping freeways beyond 30 miles from the city center.

Two, Figure 12b shows effects on neighborhood population when neighborhoods mitigate the freeway disamenity unilaterally. We turn off freeway disamenities only for neighborhoods at a given radius and measure the percentage change in population for only those neighborhoods. If mitigation were only applied to neighborhoods within 1 mile of the city center, population in those neighborhoods would increase nearly 60 percent. However, if the mitigation policy were only applied for locations at 10 miles from the city center, population gains would be considerably smaller at

Table 5: Results of simulated mitigation policy

κ	β	α	ε	$\mathbb{E}(U)$	pop. <5mi	pop. city
0.002	0.950	0.970	4.000	1.051	1.206	1.080
0.001	0.950	0.970	4.000	1.048	1.200	1.077
0.004	0.950	0.970	4.000	1.059	1.217	1.086
0.002	**0.930**	0.970	4.000	1.036	1.167	1.062
0.002	**0.970**	0.970	4.000	1.075	1.251	1.103
0.002	0.950	**0.980**	4.000	1.052	1.206	1.080
0.002	0.950	**0.960**	4.000	1.051	1.206	1.080
0.002	0.950	0.970	**2.000**	1.130	1.205	1.085
0.002	0.950	0.970	**6.000**	1.026	1.187	1.069

This table shows the results of counterfactual policies where the negative effects of freeways are removed, and the economy is re-simulated for various parameter calibrations. The first row is the baseline calibration. The first four columns show the parameters used in each simulation. This is followed by the change in expected utility. The last two columns show two measures of population centralization relative to the baseline calibration: the population within 5 miles of the CBD and the population in the City of Chicago. All values represent ratios relative to the initial economy without mitigation. The simulations use a closed-city assumption, such that total population is fixed.

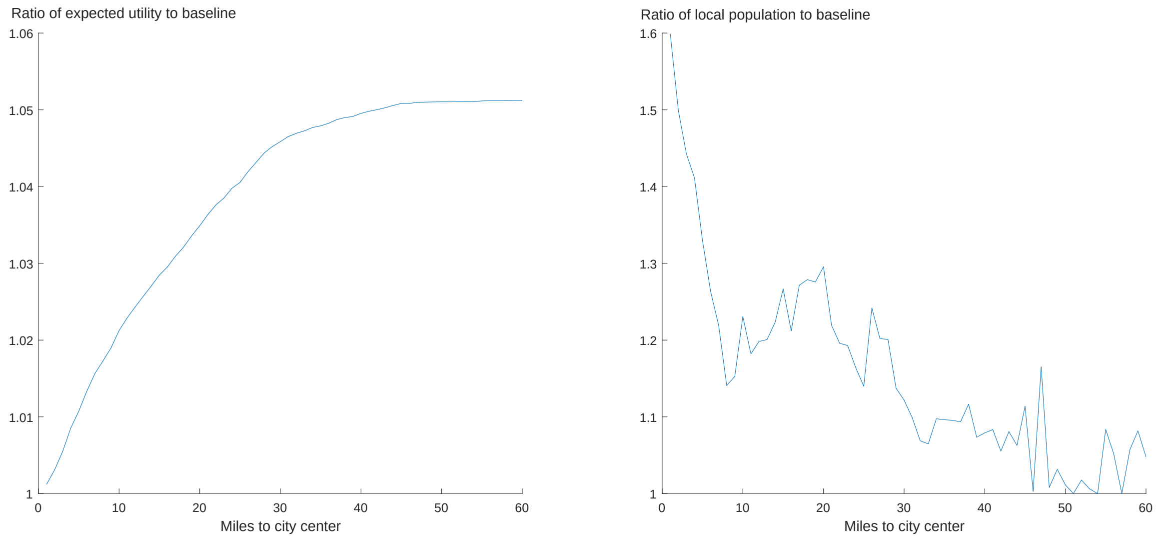
around 20 percent. Generally, the benefits of mitigation decline with distance to the city center.

These results provide insight into why political opposition to freeway projects was concentrated in central neighborhoods and why support for mitigation is often observed in central neighborhoods. Increased benefits due to the concentration of freeways and high population density in central cities could lead to more political will to mitigate the negative effects of freeways compared with suburban locations where a smaller population share may be exposed to freeway disamenities.

11.1 Benefits versus costs

How do the benefits of freeway disamenity mitigation compare with costs? The most well-known project, Boston's Big Dig, included burying 1.5 miles of freeway through the city center. The entire project cost \$15 billion, but the burying of the central freeway was only a fraction of the project that also included the construction of a new 3 mile section of freeway and a tunnel under the Boston Harbor (Flint, 2015).

The costs and benefits obviously depend on individual project details and local factors, so our analysis here is somewhat speculative. It also ignores what may be significant transition costs in terms of construction disruptions and traffic delays—the Big Dig famously took over a decade to complete. However, a number of mitigation projects have been proposed that give insights into the magnitude of these costs. For example, in Denver, a large project has been approved that includes removing an existing 1.8 mile elevated freeway, placing it below ground, and constructing



(a) Mitigation up to x miles from city center

(b) Mitigation at x miles from city center

Figure 12: Effects of mitigation by proximity to center city

The left panel shows the effect on expected utility relative to the baseline for a policy that mitigates all disamenities within a given radius. The right panel shows the effect on local population relative to the baseline for a policy that mitigates the disamenity only at a given location.

a park over a portion of the freeway (Murray, 2017). This is part of a $1.2 billion project that includes a number of additional initiatives. In Atlanta, a proposal to cap a 0.5 mile section of an already below-grade freeway has an estimated cost of $300 million (Green, 2018). A smaller project in Pittsburgh will cover a 0.1 mile section of freeway at a cost of $32 million (Belko 2019). The estimated costs of these projects range from roughly $320 million to $667 million *per mile*.

To estimate an equivalent benefit per mile, we start with the wage equivalent of the utility gains in our counterfactual experiment. Aggregate household income in the Chicago metropolitan area was $290 billion in 2018. In the experiment where freeway disamenities were mitigated for the entire metropolitan area, the utility gain was 5 percent, which corresponds to $14.8 billion per year. This intervention would require mitigating 1,583 freeway miles and therefore would provide a benefit of $9.4 million per mile per year. Using a discount rate of 7 percent[55], this suggests a lifetime benefit of $134 million per mile, somewhat lower than the cost estimates mentioned above.

Given the concentration of mitigation benefits in central neighborhoods, it is useful to calculate the benefits of a more targeted policy. If only freeways within 5 miles of the city center are mitigated, the resulting utility gain is 1 percent, or $3.1 billion per year. However, this intervention only requires mitigating 47 miles of freeway, implying a benefit of $66 million per mile per year or a lifetime benefit of $938 million per freeway mile. Thus, targeted projects that retrofit existing freeways could provide net benefits for cities. In addition, the benefits of *new* freeway construction

[55] This is the discount rate recommended by the Federal Highway Administration, but rates used by state agencies are often lower.

could be greatly improved by considering disamenity effects on surrounding neighborhoods.

12 Decomposing freeway disamenities

We decompose and quantify two potential mechanisms that lead to freeway disamenities. We first consider the role of land use exclusion, given that freeways occupy a significant amount of land, particularly in central cities. Then, we consider barrier effects, where freeways directly reduce access to nearby amenities.

12.1 Land use exclusion

Freeways take up a significant amount of space in cities. This is particularly true in central neighborhoods. Population in freeway neighborhoods could be lower simply because freeways reduce the amount of land available for housing.

To investigate the importance of land use exclusion, we estimate the amount of land used by freeways. Our database does not contain the width of the freeway right-of-way. However, a reasonable estimate can be obtained by using the length of freeways in each census tract along with standard guidelines for interstate freeway widths provided by the American Association of State Highway and Transportation Officials (2005).[56] Using these estimates, we find that freeways cover roughly 0.5 percent of total land area in Chicago metropolitan area. For locations within 5 miles of the city center, freeways account for 2 percent of land use.

To determine the importance of freeway land use for expected utility and decentralization, we return to our quantitative model. First, we re-estimate neighborhood amenities assuming that land used for freeways cannot be used for housing or production. Second, we re-estimate the freeway disamenity parameters shown in the first row of Table 4. We estimate that $\hat{b}_F = 0.172$ and $\hat{\eta} = 1.26$, which are only slightly changed from the baseline estimates. This suggests that land use exclusion is a small part of the freeway disamenities.

We further test the importance of land use exclusion by conducting an experiment where we assume that land used for freeways is reclaimed for residential and production use. In other words, we add the freeway land back to each census tract and recalculate the equilibrium, without changing travel times. In this case we find very small effects on expected utility and decentralization. Expected utility increases less than 0.1 percent compared to the 5 percent estimate shown in Table 5 when we mitigate all disamenities. Likewise, there is little effect on decentralization, with the residential population within 5 miles of the city center increasing only 0.2 percent relative to the 20 percent change in Table 5. These results are not surprising, given that the land share of consumption is only 5 percent. Thus, land use exclusion alone is unlikely to account for the total loss of amenity values near freeways.

[56]For our baseline estimate, we assume that freeways are 6 lanes wide, which corresponds to 114 feet.

12.2 Barrier effects

Removing barrier effects alone increases expected utility by up to 3 percent, or roughly 60 percent of the total gains from mitigating all freeway disamenities. To show this, we first model access to amenities. We use the specification for residential externalities developed by Ahlfeldt et al. (2015). In this case, instead of modeling the freeway disamenity as an exponential decay function, we explicitly model consumption spillovers that depend on proximity and population density of nearby areas. The amenity of a location j is

$$B_j = b_j \left(\sum_{j'=1}^{J} e^{-\rho \tau_{jj'}} \left(\frac{N_{Rj}}{L_j} \right) \right)^{\chi}, \tag{10}$$

where b_j is an amenity shifter, $\tau_{jj'}$ is the travel time between two locations, and $\frac{N_{Rj}}{L_j}$ is population density. The two parameters that determine the strength of the consumption spillovers are χ, a scale parameter, and ρ, which determines the attenuation of spillovers with respect to travel times.

We calibrate $\chi = 0.144$ and $\rho = 0.738$ following Ahlfeldt et al. (2015). To calibrate neighborhood amenities, we first recover the overall neighborhood amenities B_j as we did previously. We then decompose overall amenity into the exogenous component b_j and the endogenous component using equation 10.

Barrier effects reduce amenities by increasing travel times $\tau_{jj'}$, thus reducing access to consumption amenities nearby. We can formally write this time cost as

$$\tau_{jj'} = \tau^*_{jj'} + c_{b,jj'}, \tag{11}$$

where $\tau_{jj'}$ is the observed travel time between locations. This can be decomposed into the travel time without a freeway, $\tau^*_{jj'}$, and the barrier cost after the freeway is built, $c_{b,jj'}$.

We turn to the data to calibrate the barrier cost. In section 7, we estimated that freeways caused travel times to increase by 3 minutes for trips up to a mile and 1–2 minutes for trips up to 3 miles. In Appendix C, using a binned-distance approach, we estimate that freeways caused travel times to increase by 1.5 minutes for trips up to 2 miles. We also perform a similar exercise using cross-sectional data from Chicago. (This regression does not include origin-destination fixed effects, given that we are not using panel data.) We estimate that freeways are associated with increased travel times of up to 1.6 minutes for trips up to 8 miles. For our baseline calibration, we assume that the barrier cost is 2 minutes for trips that cross a freeway and are less than 3 miles.

Next, we use the calibrated model to quantify the magnitude of these barrier effects. We remove the barrier cost $c_{b,jj'}$, recalculate the equilibrium, and estimate the effect on both expected utility and decentralization. In other words, we reduce travel times for all trips that cross a freeway and are less than 3 miles by 2 minutes.

The results of the counterfactual experiment suggest that barrier effects are quantitatively important, potentially accounting for up to 60 percent of total disamenities from freeways. When

Table 6: Outcomes of three different mitigation experiments

	(1) Total mitigation	(2) No barrier effects	(3) Land use reclamation
$\Delta\,\mathbb{E}(U)$	1.051	1.030	1.001
Δ pop. 5 mi from city center	1.206	1.154	1.002
Δ pop. 10 mi from city center	1.077	1.047	1.001
Δ pop., Chicago city	1.080	1.059	1.001
Δ emp. 5 mi from city center	0.998	0.999	1.000
Δ emp. 10 mi from city center	0.998	0.999	1.000
Δ emp. Chicago city	0.998	0.999	1.000
Δ rent 2 mi from freeways	1.045	1.046	1.001
Δ pop. 2 mi from freeways	1.083	1.085	1.001

This table shows the results of three different counterfactual experiments to illustrate the decomposition of freeway disamenities. Column (1) shows the effect of mitigating all disamenities, Column (2) shows the effects of just removing barrier effects, and Column (3) shows the effects of removing the land-use exclusion. All results are reported as ratio of counterfactuals to the baseline calibration. The values reported in each row starting from the top are changes in expected utility, population within 5 miles of the CBD, population within 10 miles of the CBD, population in the city of Chicago, employment within 5 miles of the CBD, employment within 10 miles of the CBD, employment in the city of Chicago, total rent of neighborhoods 2 miles from a freeway, and population of neighborhoods 2 miles from a freeway.

these barrier costs are removed, expected utility rises 3 percent compared to the 5 percent estimate shown in Table 5 when we mitigate all disamenities. In addition, population within 5 miles of the CBD increases 15 percent compared to 20 percent for total mitigation. Thus, barrier effects may have played a large role in the decentralization of population alone. The results are sensitive to calibration of both the amenity spillover parameters χ and ρ as well as the calibration of the barrier cost $c_{b,jj'}$. However, the barrier effects remain quantitatively significant over a reasonable range of parameters. (See the sensitivity analysis in Appendix H.) Thus, mitigation policies that do not address barrier effects are unlikely to significantly improve quality of life.

Table 6 summarizes the results of three different counterfactual experiments to illustrate the relative importance of land use exclusion and barrier effects. Column 1 shows the effect of mitigating all disamenities,[57] column 2 shows the effects of removing barrier effects alone, and column 3 shows the effects of removing land-use exclusion alone. We report the ratio of counterfactual outcomes to the baseline calibration. The top row in the table shows a 5 percent increase in expected utility for total mitigation, 3 percent for barrier effects, and 0.1 percent for land reclamation. The next three rows show measures of population centralization, including the change in the population within the city of Chicago. Again, there are strong decentralization effects largely driven by barrier effects. The next three rows show the effect on employment decentralization. In general, the effects on job

[57] These are the same results shown in Table 5 for the baseline calibration.

location are minimal, with only a slight decline in employment near the center of the city, due to substitution towards residential use. The final two rows show the direct effects on neighborhoods within 2 miles of a freeway in terms of population and rents. Under total mitigation, rents increase by 4.5 percent and population increases by 8.3 percent in neighborhoods near freeways.

13 Conclusions

We analyzed diminished quality of life from freeway disamenities. Our findings are important for understanding suburbanization, for evaluating mitigation policies such as capping or burying freeways, and for understanding the freeway revolts of the 1950s and 1960s.

The collage of evidence suggests that freeway disamenities, versus commuting benefits, likely played a significant role in the decentralization of U.S. cities. One, the freeway revolts themselves are *prima facie* evidence of the importance of freeway disamenities, especially in central neighborhoods. Two, large declines in population and income in central neighborhoods near freeways suggest that freeway disamenities exceeded modest accessibility benefits in central cities. Three, low populations today in freeway-adjacent neighborhoods with superior job access point to significant freeway disamenities. Finally, significant declines in travel volumes and increases in travel times between neighborhoods severed by freeways suggest that barrier effects are an important disamenity factor.

Our estimates also suggest that targeted Big Dig-style policies that cap or bury highways in city centers could provide net benefits. Their effects in mitigating barrier effects seem especially important. Unambiguously, the benefits of new freeway construction could be greatly improved by considering disamenity effects on surrounding neighborhoods.

Our study highlights many of the unintended costs of freeways, but leaves out others. Policy makers did not anticipate many of these effects, and when faced with opposition, they were slow to respond. Further, their responses, in the form of freeway cancellations or re-routings, mostly favored white and educated neighborhoods, increasing divergence. As emphasized by Altshuler and Luberoff (2003), these missteps not only ended the era of infrastructure "mega-projects" but also likely contributed to greater skepticism of government and development in general.

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A Data appendix

A.1 Census tracts and metropolitan areas

We use data on consistent-boundary neighborhoods spanning many U.S. metropolitan areas from 1950 to 2010 from Lee and Lin (2018). We use census tracts as neighborhoods because tracts are relatively small geographic units and data are available at the tract level, or at a more detailed level, over our sample period. The base data are from Decennial Censuses of Population and Housing between 1950 and 2000 and the American Community Survey between 2006 and 2010[58]. These data were previously constructed in Lee and Lin (2018). The online appendix to Lee and Lin (2018) contains additional details about data construction.

Since boundaries change from one decade to the next, these data are normalized historical data to 2010 census tract boundaries. For example, average household income in 1950 for each 2010 tract is computed by weighting the average household incomes reported for overlapping 1950 census tracts, where the weights are determined by overlapping land area.[59]

We assign each neighborhood to one of 64 metropolitan areas, using the Office of Management and Budget's definitions of core-based statistical areas (CBSAs) from December 2009. In the main text we refer to each metropolitan area as a "city." Table A.1 lists our sample metropolitan areas, whether they are in our census tract panel, and whether they are in the "Yellow Book" plan.

For each neighborhood we measure its distance to the principal city's center, a fixed point in space. We use definitions by Fee and Hartley (2013), who identify the latitude and longitude of city centers by taking the spatial centroid of the group of census tracts listed in the 1982 Census of Retail Trade for the central city of the metropolitan area. Metropolitan areas not in the 1982 Census of Retail Trade use the latitude and longitude for central cities using ArcGIS's 10.0 North American Geocoding Service.

The neighborhood data from Lee and Lin (2018) also contain measures of natural amenities. Spatial data on water features—coastlines, lakes, and rivers—is from the National Oceanic and Atmospheric Administration's (2012) Coastal Geospatial Data Project. These data consist of high-resolution maps covering (i) coastlines (including those of the Atlantic, Pacific, Gulf of Mexico, and Great Lakes), (ii) other lakes, and (iii) major rivers. Average slope for each tract is computed using the 90-meter resolution elevation map included in the Esri 8 package and the ArcGIS slope geoprocessing and zonal statistics tools.

Table A.2 displays sample means and standard deviations for variables used in the estimates reported in Table 1.

A.2 Roads

We match each consistent-boundary tract to the nearest present-day freeway from the National Highway Planning Network 14.05 (U.S. Federal Highway Administration, 2014), a database of line features representing highways in the United States. From the NHPN we select only limited access roads, i.e., highway segments that offer "full access control," meaning all access to the highway is via grade-separated interchanges. Interstate highway segments (except for some that pre-date the Interstate designation) are a subset of limited access roads; some limited access roads were financed by non-federal funds only.

[58]The ACS data represent 5-year averages of residents and houses located in each tract. For convenience, we refer to these data as coming from the year 2010.

[59]For census data from 1970 and later, we use the population of overlapping census blocks as weights, instead of overlapping land area.

Table A.1: Metropolitan areas with 1950 census tract data or included in the 1955 Yellow Book

State	Metropolitan area	Both	Tract	YB
AL	Birmingham	✓	✓	✓
	Gadsden			✓
	Montgomery			✓
	Tuscaloosa			✓
AR	Fort Smith			✓
	Little Rock			✓
AZ	Phoenix			✓
	Tuscon			✓
CA	Los Angeles	✓	✓	✓
	Oakland	✓	✓	✓
	Sacramento		✓	
	San Diego		✓	
	San Francisco	✓	✓	✓
	San Jose	✓	✓	✓
CO	Denver	✓	✓	✓
CT	Bridgeport		✓	
	Hartford	✓	✓	✓
	New Haven		✓	
DC	Washington	✓	✓	✓
FL	Miami	✓	✓	✓
	Pensacola			✓
	St. Petersburg			✓
	Tampa			✓
GA	Atlanta	✓	✓	✓
	Macon			✓
IA	Davenport-Moline			✓
	Des Moines			✓
ID	Pocatello			✓
IL	Chicago	✓	✓	✓
IN	Gary	✓	✓	✓
	Indianapolis	✓	✓	✓
	Peoria			✓
KS	Topeka			✓
	Wichita	✓	✓	✓
KY	Louisville	✓	✓	✓
LA	Baton Rouge			✓
	Lake Charles			✓
	Monroe			✓
	New Orleans	✓	✓	✓
	Shreveport			✓
MA	Boston	✓	✓	✓
	Springfield	✓	✓	✓
	Worcester			✓
MD	Baltimore	✓	✓	✓
ME	Bangor			✓
	Biddeford-Saco			✓
	Portland			✓
MI	Battle Creek			✓
	Detroit	✓	✓	✓
	Flint	✓	✓	✓
	Grand Rapids			✓
	Kalamazoo		✓	
	Lansing			✓
	Saginaw			✓
	Warren	✓	✓	✓
MN	Duluth		✓	
	Minneapolis	✓	✓	✓
MO	Kansas City	✓	✓	✓
	St. Joseph			✓
	St. Louis	✓	✓	✓

State	Metropolitan area	Both	Tract	YB
MS	Jackson			✓
MT	Butte			✓
	Great Falls			✓
NC	Durham		✓	
	Greensboro		✓	
NE	Lincoln			✓
	Omaha	✓	✓	✓
NH	Manchester			✓
NJ	Camden	✓	✓	✓
	Trenton	✓	✓	✓
NY	Albany			✓
	Buffalo	✓	✓	✓
	Kingston			✓
	New York	✓	✓	✓
	Rochester	✓	✓	✓
	Schnectady			✓
	Syracuse	✓	✓	✓
	Utica	✓	✓	✓
OH	Akron		✓	
	Cincinnati	✓	✓	✓
	Cleveland	✓	✓	✓
	Columbus	✓	✓	✓
	Dayton		✓	
	Toledo	✓	✓	✓
OK	Oklahoma City	✓	✓	✓
	Tulsa			✓
OR	Eugene			✓
	Portland	✓	✓	✓
	Salem			✓
PA	Allentown-Bethlehem			✓
	Erie			✓
	Harrisburg			✓
	Philadelphia	✓	✓	✓
	Pittsburgh	✓	✓	✓
	Reading			✓
RI	Providence	✓	✓	✓
SC	Columbia			✓
	Greenville			✓
	Spartanburg			✓
SD	Rapid City			✓
	Sioux Falls			✓
TN	Chattanooga	✓	✓	✓
	Knoxville			✓
	Memphis	✓	✓	✓
	Nashville	✓	✓	✓
TX	Austin		✓	
	Dallas	✓	✓	✓
	Fort Worth	✓	✓	✓
	Houston	✓	✓	✓
	San Antonio			✓
VA	Bristol			✓
	Norfolk			✓
	Richmond	✓	✓	✓
	Roanoke			✓
VT	Burlington			✓
WA	Seattle	✓	✓	✓
	Spokane		✓	
	Tacoma		✓	
WI	Milwaukee	✓	✓	✓
WV	Wheeling			✓

Table A.2: Summary statistics for neighborhoods

	Miles from city center:			
	0-2.5	2.5-5	5-10	10-50
Log change population, 1950-2010	-0.49	0.00	0.70	1.67
	(0.82)	(0.94)	(1.27)	(1.52)
Miles to nearest highway	0.64	0.95	1.09	1.30
	(0.53)	(0.70)	(0.83)	(1.30)
Miles to nearest park	0.57	0.43	0.49	0.63
	(1.67)	(0.93)	(0.62)	(0.80)
Miles to nearest lake	16.12	17.33	17.68	17.87
	(13.24)	(13.59)	(12.72)	(12.17)
Miles to nearest port	68.25	65.88	38.07	19.19
	(134.23)	(127.23)	(73.60)	(28.99)
Miles to nearest river	2.69	3.65	4.07	3.46
	(7.25)	(9.68)	(9.07)	(7.82)
Miles to nearest coastline	73.56	71.52	40.20	19.56
	(146.16)	(137.84)	(82.71)	(43.79)
Average slope between 0 and 5 degrees	0.49	0.57	0.66	0.64
	(0.50)	(0.49)	(0.48)	(0.48)
Average slope between 5 and 10 degrees	0.35	0.29	0.24	0.22
	(0.48)	(0.45)	(0.42)	(0.41)
Average slope between 10 and 15 degrees	0.09	0.08	0.06	0.07
	(0.28)	(0.28)	(0.24)	(0.25)
Average slope greater than 15 degrees	0.06	0.06	0.05	0.07
	(0.24)	(0.23)	(0.21)	(0.25)
Number of neighborhoods	2,312	3,482	5,561	5,173
Number of metropolitan areas	64	63	56	38

This table reports sample means and standard deviations for variables used in the estimates reported in Table 1.

A.3 Road opening dates

We use the PR-511 database, an administrative database that contains information about when each Interstate segment first opened to traffic. The PR-511 database has been used in previous studies including Chandra and Thompson (2000), Baum-Snow (2007), Michaels (2008), and Nall (2015). We start with the version digitized by Baum-Snow (2007). Baum-Snow (2007) used line features representing highways that were split into equal length segments of 1 miles each. Then, these segments were matched with the PR-511 database to determine the opening date for each highway route segment. We performed some additional cleaning of these data to achieve better matching of the PR-511 database to route segments at census tract resolution.

A.4 Plan and historical routes

We digitized several maps of planned or historical transportation routes.

One, we digitized the 1947 Interstate plan. The Federal-Aid Highway Act of 1944 had called for the designation of a National System of Interstate Highways, to include up to 40,000 miles. This is the map used in Baum-Snow (2007) as an instrument for completed Interstates. States were asked to submit proposals for their portion of the Interstate highway system. They then negotiated with the Bureau of Public Roads and the Department of Defense over routing and mileage. In 1947, the BPR announced the selection of the first 37,000 miles. Baum-Snow's coding of these planned Interstate routes was precise only to metropolitan-level variation, so was unsuitable for our analysis. Instead, we digitized the 1947 plan map.

Other previous studies using the 1947 Interstate plan as an instrument for completed highways include Chandra and Thompson (2000), Michaels (2008), and Duranton and Turner (2012).

Because the 1947 plan map was drawn at a national scale, there is little detail inside metropolitan areas. In fact, metropolitan areas are represented as open circles. This is a virtue for our instrumental variables analysis, since information about neighborhood factors did not enter into the routing of the 1947 plan map highways. (The 1947 highway plan makes no mention of transportation within cities or future development.) On the other hand, the size of the open circles and the poor resolution of the 1947 plan map mean that in practice it is challenging to precisely assign the routes of plan highways according to the 1947 map. To the extent possible, we use the center of the drawn lines of the 1947 map. When drawn lines terminate at open circles, we extend these lines to principal city centers from Fee and Hartley (2013). We do this to ensure relevant variation in proximity to plan routes—without these extensions, all 1947 plan routes would terminate at the edge of the metropolitan area. In addition, Interstate design principles enshrined later (e.g., AASHO, 1957) codified the radial structure of U.S. city highway networks seen today, where multiple rays converge to locations just outside of central business districts.

Two, we digitized the *General Location of National System of Interstate Highways including All Additional Routes at Urban Areas Designated in September 1955*, popularly known as the "Yellow Book" (U.S. Department of Commerce, 1955) In 1955, the Bureau of Public Roads designated the remaining mileage of Interstates authorized by the 1947 Interstate plan. Unlike the 1947 plan, which described only routes between cities, the Yellow Book described the general routing of highways within each of 100 metropolitan areas. As before, state highway departments submitted proposals to the BPR and then negotiated over routing and mileage for the 1955 Yellow Book routes. In general, they followed a radial-concentric ring pattern codified in *Interregional Highways* (U.S. Congress, 1944), a report that outlined basic highway designs, adapted to topographical and land-use characteristics of each metropolitan area (Ellis, 2001).

Three, we digitized routes of exploration from the 16th to the 19th century from the National

Atlas (U.S. Geological Survey, 1970). These were first used as instruments for actual highways by Duranton and Turner (2012). Again, they used variation across metropolitan areas; we digitized these maps so that the data were suitable for analysis at the scale of census tracts.

Four, we use historical rail routes from Atack (2016). Following Duranton and Turner (2012), we select rail routes in operation by 1898 from the Atack (2016) database.

A.5 Chicago land prices

Ahlfeldt and McMillen (2014) digitized various editions of *Olcott's Blue Books of Chicago*. These volumes provide land value estimates for detailed geographic units in the form of printed maps. Often, different estimates are reported for different sides of the same street, different segments of the same block, and for corner lots. They coded these data for 330×330 foot grid cells. Gabriel Ahlfeldt graciously shared the 1949 and 1990 data with us. These data were also used in Ahlfeldt and McMillen (2014) and McMillen (2015).

A.6 Chicago and Detroit travel surveys

Estimates of jobs from the Chicago and Detroit travel surveys tend to match well aggregates reported by other sources. In 1956 Chicago, we are able to assign to census tracts 1,212 thousand jobs. This compares favorably to other contemporary estimates. The overall 1956 travel survey reported 1,500 thousand aggregate person-trips to work (about 300 thousand jobs were not separately reported by zone). The 1954 Census of Business (now the Economic Census) reported 1,082 thousand jobs in the city of Chicago (a geographic area smaller than our sample area, which is all 1950 tracts in the metropolitan area) and 1,324 thousand jobs in Cook and DuPage counties (larger than our sample area)[60]. Unlike the travel survey, the Census of Business notably lacked coverage of employment in construction, transportation, communications, utilities, finance, and many services. Finally, the 1950 Census of Population reported 2,036 thousand jobs reported by residents of Cook and DuPage counties.

In 1953 Detroit, we are able to assign 983 thousand jobs to census tracts using sampling weights. This compares favorably to 1954 Census of Business estimates of 681 thousand (Wayne County, comparable to our sample area) to 816 thousand (Detroit metropolitan area, larger than our sample area)[61]. The 1950 Census of Population also reported 983 thousand jobs reported by residents of Wayne County.

Table A.5 shows summary statistics for the 1953 and 1994 Detroit surveys. (The last column shows statistics for only households living in the 1950 footprint of the metropolitan area.) Consistent with a decline in transportation costs, the average trip in the Detroit metropolitan area lengthened from 3.7 to 5.1 miles. However, a large share of trips continue to be made at short distances: the median trip increased only from 2.6 to 2.7 miles. (Note that both work and non-work trips are included in these figures.) For households in the 1950 footprint of the city, average trip length increased by 0.1 mile and the median trip decreased by 0.4 mile. The share of trips by automobile increased from 82 percent in 1953 to 88 percent in 1994. Trips to work (one-way) accounted for 24 percent of trips in 1953 and 20 percent of trips in 1994.[62]

[60]The 1956 Chicago travel survey sampled an area consisting of nearly all of Cook County, the eastern half of DuPage County, and very small portions of Will and Lake (IL) counties.

[61]The 1953 Detroit travel survey sampled most of Wayne County and portions of Oakland and Macomb counties.

[62]While the 1953 survey records purpose at both origin and destination, the 1994 survey only records purpose at destination.

Table A.3: Comparison of 1950s employment data for the Chicago metropolitan area

	CATS jobs by zone, 1955-7[a]	CATS person-trips to work, '56	Census of Business, 1954 2-county[d]	Census of Business, 1954 5-county[e]	Census of Business, 1954 City	Census of Population, 1950 2-county
Construction	39.2[c]	.	.	.	.	.
Manufacturing	827.6	713	772.1	843.5	615.7	.
Transp., comm., util.	.	173	.	.	.	.
Wholesale trade	125.0[c]	134	143.5	148.0	131.4	.
Retail trade	131.2[c]	327	280.6	304.5	223.5	.
Private services	.	326	.	.	.	.
... Finance	88.5[c]	.	.	.	.	.
... Selected services[b]	.	.	128.0	134.7	111.8	.
Public administration	.	216	.	.	.	.
Total	1,211.5	1,500	1,324.2	1,430.7	1,082.4	2,036.4

A period (".") indicates employment for the sector indicated by the row title is not reported by the source indicated by the column title. [a]—Average total covered employment over 1955-1957, reported by CATS zone. CATS zones cover nearly all of Cook County; approximately the eastern half of DuPage County, and very small portions of Lake and Will counties. [b]—Selected services covered by the 1954 Census of Business are: Personal services; Business services; Auto repair services; Miscellaneous repair services; Amusement and recreation Services; Hotels and tourism. [c]—Employment by CATS zone for these sectors reported for only 16 central zones (out of 44); other zones censored for low coverage. [d]—Cook and DuPage counties. [e]—Cook, DuPage, Kane, Lake, and Will counties.

Table A.4: Comparison of 1950s employment data for the Detroit metropolitan area

	DMATS, 1953	Census of Business, 1954 Wayne co.	Census of Business, 1954 Detroit metro	C. of Pop., 1950 Wayne co.
Construction	42.8	.	.	.
Manufacturing	527.4	445.5	538.2	.
Transp., comm., util.	61.9	.	.	.
Wholesale trade	27.3	46.3	48.5	.
Retail trade	124.3	138.6	171.0	.
Selected services	.	51.0	58.1	.
... FIRE	33.4	.	.	.
... Personal services	64.0	.	.	.
... Professional services	61.8	.	.	.
Public administration	40.0	.	.	.
Total	982.9	681.4	815.8	983.0

A period (".") indicates employment for the sector indicated by the row title is not reported by the source indicated by the column title. [a]—Selected services covered by the 1954 Census of Business are: Personal services; Business services; Auto repair services; Miscellaneous repair services; Amusement and recreation Services; Hotels and tourism.

Table A.5: Summary statistics, 1953 and 1994 DMATS

	1953	1994	
		Full sample	1950 tracts
Sample			
Households	36,226	6,653	4,265
Persons	75,395	14,036	8,282
Trips	250,453	58,733	30,940
Trip distance, miles			
μ (σ)	3.7 (3.5)	5.1 (13.0)	3.8 (4.3)
p50	2.6	2.7	2.2
(p25, p75)	(1.0, 5.4)	(1.0, 6.5)	(0.8, 5.1)
Origin distance to city center, miles			
	8.7 (4.9)	19.7 (14.1)	12.0 (4.8)
Mode			
Car	0.83	0.88	0.87
Transit	0.16	0.02	0.02
Walk	0.01	0.06	0.08
Purpose			
to work	0.24	0.20	0.19
to shopping	0.08	0.09	0.09

B Building the Interstates

Table B.1 summarizes key federal policy changes that affected the allocation of urban Interstates.

Table B.1: Timeline of federal policy changes

1958	At least one public hearing, economic impact study requirements.
1962	Local cooperation requirements.
1966	Oversight by newly-created Department of Transportation.
	Environmental protection requirements.
	Historical preservation requirements.
1967	First Transportation Secretary Alan Boyd became "most effective national spokesman for the freeway revolt" (Mohl, 2008).
1968	More environmental and historical requirements.
	Relocation assistance & replacement housing requirements.
1970	More environmental requirements.
	More relocation assistance requirements.
1973	De-designation of 190 planned Interstate miles.
	States allowed to exchange federal funds for other transportation projects.

Table B.2: Factors predicting planned freeway and Interstate highway construction

	Yellow Book	*Interstate highway open by:*			
	1955	1956	1960	1970	1993
Population density, 1950	0.013	-0.012^{b}	-0.036^{c}	-0.037^{c}	-0.052^{c}
	(0.010)	(0.005)	(0.008)	(0.013)	(0.015)
Share college graduate, 1950	-0.011	0.003	-0.008	-0.035^{c}	-0.034^{c}
	(0.007)	(0.004)	(0.006)	(0.009)	(0.009)
Share black, 1950	0.002	0.000	0.004	0.011	0.014^{a}
	(0.012)	(0.004)	(0.005)	(0.007)	(0.007)
Median household income, 1950	-0.001	0.002	-0.006	-0.005	-0.003
	(0.006)	(0.005)	(0.007)	(0.011)	(0.011)
Median rent, 1950	0.001	-0.013^{c}	-0.010^{a}	-0.006	-0.005
	(0.005)	(0.005)	(0.006)	(0.008)	(0.008)
Median value, 1950	-0.007	-0.002	0.001	-0.001	-0.008
	(0.007)	(0.004)	(0.007)	(0.010)	(0.010)
Median dwelling age, 1950	-0.004	-0.001	-0.013^{a}	-0.022^{b}	-0.024^{b}
	(0.005)	(0.004)	(0.006)	(0.008)	(0.009)
1(Coast)	-0.002	0.015	0.020	0.007	0.012
	(0.024)	(0.018)	(0.020)	(0.028)	(0.027)
1(Lake)	-0.066^{b}	-0.023	-0.032	-0.144^{c}	-0.157^{c}
	(0.032)	(0.040)	(0.034)	(0.029)	(0.041)
1(River)	0.032^{a}	0.009	0.027	0.060^{b}	0.070^{c}
	(0.017)	(0.010)	(0.019)	(0.028)	(0.024)
1(Park)	0.007	-0.002	0.006	-0.013	-0.007
	(0.008)	(0.005)	(0.008)	(0.009)	(0.010)
1(Historical rail)	0.028^{c}	0.013^{b}	0.025^{c}	0.054^{c}	0.066^{c}
	(0.009)	(0.006)	(0.008)	(0.018)	(0.019)
1(Seaport)	0.113	-0.069^{c}	-0.007	0.084	0.051
	(0.086)	(0.021)	(0.040)	(0.098)	(0.098)
10 categories of distance to city center	x	x	x	x	x
4 categories of average slope	x	x	x	x	x
R^2	0.053	0.047	0.056	0.063	0.082
Neighborhoods	14,930	14,930	14,930	14,930	14,930
Metropolitan areas	50	50	50	50	50
Share 1(freeway)=1		0.046	0.109	0.217	0.262

This table shows OLS estimates of equation (1). Each column reports a separate regression. All regressions include metropolitan area fixed effects. Estimated standard errors, robust to heteroskedasticity and clustering on metropolitan area, are in parentheses. The dependent variable is an indicator that takes a value of 1 if a neighborhood intersects a buffer of 100 meters of a planned freeway or constructed Interstate highway. The last row reports the dependent variable mean. Factors measuring 1950 characteristics are standardized within metropolitan area to have mean zero, standard deviation 1. Indicators for natural and historical factors take a value of 1 if a neighborhood centroid is within 0.5 mile of the factor listed. ^{a}—$p < 0.10$, ^{b}—$p < 0.05$, ^{c}—$p < 0.01$.

Table B.3: Freeway factors: Logistic regression estimates

	Yellow Book	*Interstate highway open by:*			
	1955	1956	1960	1970	1993
Population density, 1950	1.104	0.734^{b}	0.640^{c}	0.793^{b}	0.741^{c}
	(0.076)	(0.115)	(0.072)	(0.073)	(0.068)
Share college graduate, 1950	0.881^{a}	1.083	0.903	0.776^{c}	0.812^{c}
	(0.066)	(0.109)	(0.065)	(0.047)	(0.046)
Share black, 1950	1.002	0.994	1.035	1.062^{a}	1.075^{b}
	(0.080)	(0.104)	(0.054)	(0.038)	(0.038)
Median household income, 1950	0.987	1.045	0.927	0.962	0.987
	(0.059)	(0.128)	(0.079)	(0.082)	(0.068)
Median rent, 1950	1.032	0.703^{c}	0.896	0.964	0.976
	(0.049)	(0.079)	(0.063)	(0.052)	(0.047)
Median value, 1950	0.951	0.959	1.024	1.008	0.955
	(0.050)	(0.098)	(0.081)	(0.069)	(0.058)
Median dwelling age, 1950	0.971	0.962	0.872^{b}	0.867^{c}	0.875^{c}
	(0.041)	(0.104)	(0.054)	(0.044)	(0.045)
1(Coast)	0.979	1.277	1.240	1.061	1.074
	(0.162)	(0.380)	(0.253)	(0.198)	(0.176)
1(Lake)	0.515^{a}	0.631	0.738	0.413^{c}	0.413^{c}
	(0.191)	(0.513)	(0.357)	(0.095)	(0.110)
1(River)	1.315^{b}	1.186	1.267	1.375^{b}	1.404^{c}
	(0.171)	(0.245)	(0.192)	(0.202)	(0.168)
1(Park)	1.069	0.979	1.074	0.927	0.962
	(0.080)	(0.115)	(0.090)	(0.051)	(0.052)
1(Historical rail)	1.229^{c}	1.375^{b}	1.321^{c}	1.392^{c}	1.435^{c}
	(0.085)	(0.171)	(0.104)	(0.139)	(0.131)
1(Seaport)	1.772	1.000	0.907	1.552	1.325
	(0.674)	(.)	(0.297)	(0.729)	(0.630)
10 categories of distance to city center	x	x	x	x	x
4 categories of average slope	x	x	x	x	x

This table shows estimates of a logistic regression in exponentiated form (odds ratios) corresponding to the linear probability model estimates reported in Table B.2. See notes to Table B.2.

C Barrier effects

C.1 Data processing

In the 1953 and 1994 Detroit Metropolitan Area Traffic Study microdata, trip origins and destinations are reported with precise latitude and longitudes. In 1953 there are 17,864 unique origin or destination points. In 1994 there are 22,446 unique origin or destination points. We allocate trips to the 855 census tracts (2010 boundaries) in the 1953 sample area. Then, we intersect tract-to-tract routes with the NHPN. Routes intersecting NHPN freeways are "treated" by a freeway.

Tract-to-tract flows are estimated using sample weights. To estimate average tract-to-tract times, we use trips with mode reported as auto driver, auto passenger, or taxi passenger. We condition on auto travel in order to abstract from changes in mode choice. In practice, nearly all of the mode shifts are from transit to driving or walking (see Table A.5).[63] We trim times in the top 1% as well as times that imply speeds greater than 80 miles per hour. We also drop times where the elapsed time reported in the original database does not match the difference between the reported start and end times. We average the remaining times to estimate tract-to-tract times.

The final sample contains (855 × 855 =) 731,025 tract pairs, although actual regression samples are smaller because (1) many tract pairs do not have observed flows or times and (2) we drop singletons in our PPML estimations (Correia, 2015). Table C.1 shows summary statistics for our tract-pair panel by year. Note that distance and the freeway indicator are defined for all tract pairs in both years of our panel.

Table C.1: Summary statistics for Detroit panel by year

	Observations	μ	σ
	(a) 1953		
Time	66,675	25.1	14.4
Trips	74,142	72.1	146.3
Distance	731,025	13.2	8.6
$1(freeway)$	731,025	0.292	0.455
	(b) 1994		
Time	15,089	23.5	21.2
Trips	17,039	422.8	690.5
Distance	731,025	13.2	8.6
$1(freeway)$	731,025	0.910	0.286

To estimate barrier effects using cross-sectional data from Chicago from 2000, we use data on commute times and flows from the Census Transportation Planning Package (CTPP), which is a database of journey-to-work tabulations derived from the Census 2000 long form. The data are organized into origin-destination tract pairs where origins are residences and destinations are workplaces. For each origin-destination tract pair, CTPP tabulations report average time, in minutes, and total commuting flows.

[63]Detroit's streetcar system was discontinued in 1956.

C.2 Other estimates of barrier effects

We present alternative estimates of barrier effects. First, compared with the regression results presented in section 7, we report estimates of barrier effects by distance bins. Using the same Detroit tract panel as before, we regress average travel time in minutes on interactions between a freeway crossing (1(freeway)) and distance indicators in 2-mile increments. Origin–year and destination–year fixed effects capture neighborhood-specific factors that affect travel times for all trips from or to those tracts. Origin–destination fixed effects capture pair-specific characteristics that are time invariant, such as the main effect of pair distance and fixed transportation infrastructure. Compared with the main results reported in section 7, this is a single regression (versus many regressions) with interactions between a freeway crossing indicator and several distance bins (versus trips of less than and more than a single distance threshold).

Table C.2 displays results from this regression in column (1). Trips of 0–2 miles that are bisected by a freeway are about 1.5 minutes longer compared with trips without freeway crossing. This can be compared with the average travel time of 10 minutes for trips between 0–2 miles in 1953. The estimate is nearly identical to the estimate from the regression shown in Figure 8, panel (b). Although this is not precisely estimated, it is consistent with the sharp drop in actual flows shown in Figure 8, panel (a). In column (2), we perform a similar high-dimensional fixed effects regression of the natural logarithm of trips on the interactions between a freeway crossing and the distance bins. Total flows decline about 23% for trips less than 2 miles bisected by a freeway compared with trips without freeway crossing. This decline is precisely estimated. This is quantitatively similar to the PPML regression estimates shown in Figure 8, panel (a).

For trips longer than 2 miles, travel times decline and flows increase. These time declines and trip increases are precisely estimated. For example, trips between 4–6 miles that are bisected by a freeway see increased travel times of about 2.8 minutes (average trip time of 24 minutes in 1953) compared with trips of similar distance not bisected by a freeway. There are 67% more 4–6 mile trips between origins and destinations that are bisected by a freeway compared with origins and destinations not bisected by a freeway.

We also estimate barrier effects using cross-sectional data from Chicago. Similar to the panel estimation, we include origin and destination fixed effects to account for neighborhood factors that affect all trips from or to these tracts. However, because we are no longer using a panel, we cannot include origin–destination fixed effects. This means we cannot control for unobserved tract-pair factors such as the network of surface streets or other unobserved transportation infrastructure. We do control flexibly for the distance between origin and destination by including indicators for 2–mile distance bins interacted with the origin and destination fixed effects. We also include distance in miles as another control. Thus, identification of barrier effects in this regression comes from variation between trips that originate from the same tract (or end in the same tract) and are the same distance, but are oriented such that some cross a freeway and others do not cross a freeway. Unobserved factors such as the layout of the surface street network, traffic congestion, or the direction of travel that may be correlated with freeway crossings can affect our estimates.

Table C.2 displays results of these cross-sectional regressions in columns (3) and (4). Qualitatively, the estimates are similar to the panel estimates from Detroit in the first two columns. Freeways increase travel times and decrease travel volumes for shorter trips, but decrease travel times and increase travel volumes for longer trips. The estimated barrier effect is largest for trips of 2–4 miles; trips crossing freeways take 1.6 minutes longer, and this is precisely estimated.

In sum, regressions reported here and in section 7 are consistent with barrier effects of up to two minutes for short trips. We weigh the Detroit panel evidence more compared with the Chicago cross-sectional evidence, though qualitatively both display similar patterns.

Table C.2: Barrier effect estimates by distance bin using Detroit panel and Chicago cross-section

	Detroit 1953-1994		Chicago 2000	
	(1)	(2)	(3)	(4)
	Time	Log trips	Time	Log trips
1(freeway) ×				
0–2 miles	1.474	-0.230[c]	0.748[c]	-0.480[c]
	(1.734)	(0.088)	(0.515)	(0.019)
2–4 miles	-0.698	0.379[c]	1.645[c]	-0.122[c]
	(1.327)	(0.071)	(0.315)	(0.012)
4–6 miles	-2.881[a]	0.667[c]	1.204[c]	-0.060[c]
	(1.584)	(0.084)	(0.307)	(0.011)
6–8 miles	-4.043[b]	0.757[c]	0.834[b]	-0.071[c]
	(2.034)	(0.101)	(0.350)	(0.013)
8+ miles	-5.350[a]	0.474[c]	-0.305	-0.025
	(2.919)	(0.157)	(0.427)	(0.016)
Distance			0.666[c]	-0.019[c]
			(0.007)	(0.000)
Constant	17.12[c]	4.88[c]	24.89[c]	2.628[c]
	(0.262)	(0.014)	(0.130)	(0.005)
Observations	11,276	13,774	236,409	237,955
Fixed effects				
Origin–year	1,338	1,406		
Destination–year	1,330	1,396		
Origin–destination	5,638	6,887		
Origin–distance			11,363	11,377
Destination–distance			11,047	11,067

D Other evidence from population, income, and prices

D.1 1947 Interstate plan

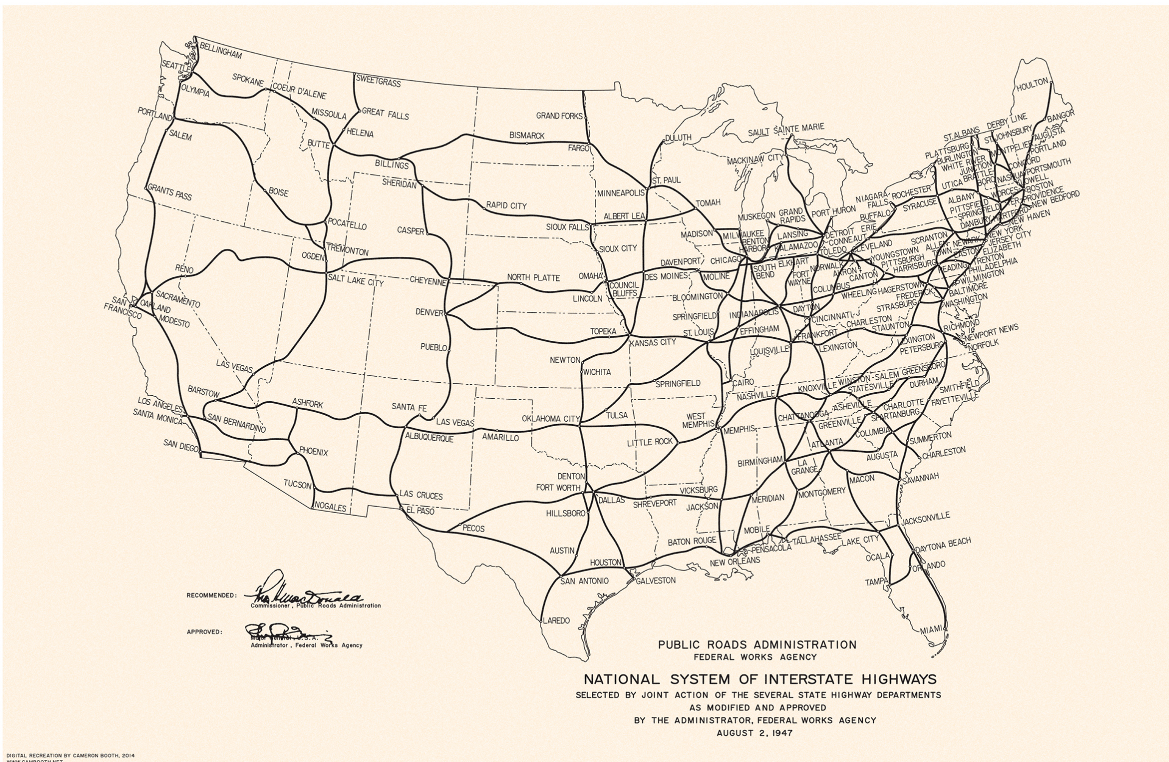

Figure D.1: 1947 highway plan

D.2 Robustness of population results

Table D.1 reports WLS estimates of equation (2), with controls for natural and historical factors. The estimated coefficients on miles to nearest freeway are also reported in Table 1, panel (b).

To illustrate the robustness of our main results, Figure D.2 reports coefficient estimates for other specifications. The baseline IV results reported in Table 2, panel (c) are shown in red on the left side of each panel. (The circle marks the point estimate and the lines indicate the 95 percent confidence interval.) The second line in each panel, and the first blue line, indicate estimates from a specification that also includes 1950 tract characteristics as controls—the black share of the population, the college share of the adult population, average household income, and average housing values and rents. The third line excludes New York and Los Angeles from the sample. The fourth line performs unweighted regressions. Across specifications, the coefficient estimates are precise and stable. They also replicate the important pattern of the main result: Strong negative freeway effects (positive estimates) close to city centers that attenuate with distance to the CBD.

Up to this point, we have only considered the access benefits of highways for commuting to the CBD. However, this same analysis could apply to other regional level destinations. The fifth

Table D.1: WLS estimates with controls for natural and historical factors

	Distance to city center:			
	0–2.5 miles	2.5–5 miles	5–10 miles	10–50 miles
Miles to nearest highway	0.163^c	0.075^b	-0.208^c	-0.042
	(0.059)	(0.031)	(0.072)	(0.038)
Miles to city center	0.306^c	0.294^c	0.225^c	0.032
	(0.043)	(0.039)	(0.037)	(0.022)
Miles to nearest park	0.174	0.148^b	0.078	-0.126
	(0.122)	(0.059)	(0.048)	(0.080)
Miles to nearest lake	-0.020	0.014	0.012	0.015
	(0.023)	(0.012)	(0.013)	(0.012)
Miles to nearest port	0.040	0.032^a	0.058^b	0.003
	(0.040)	(0.017)	(0.025)	(0.027)
Miles to nearest river	0.018	-0.009	0.031	0.022
	(0.042)	(0.031)	(0.032)	(0.031)
Miles to nearest coastline	-0.042	-0.025	-0.046^b	0.011
	(0.044)	(0.017)	(0.023)	(0.020)
Average slope between 0 and 5 degrees	-0.037	-0.166	-0.799^c	2.866^c
	(0.245)	(0.277)	(0.293)	(0.547)
Average slope between 5 and 10 degrees	0.209	-0.037	-0.721^b	2.921^c
	(0.229)	(0.284)	(0.309)	(0.526)
Average slope between 10 and 15 degrees	0.485^b	0.150	-1.096^b	2.721^c
	(0.216)	(0.267)	(0.464)	(0.561)
Average slope greater than 15 degrees	0.560^c	0.192	-0.854^b	2.642^c
	(0.205)	(0.250)	(0.357)	(0.588)
R^2	0.151	0.119	0.124	0.083
Neighborhoods	2,312	3,482	5,561	5,173
Metropolitan areas	64	63	56	38

This table shows WLS estimates of equation (2). The estimated coefficients on miles to nearest freeway are also reported in Table 1, panel (b). Each column reports a separate regression. Neighborhoods are weighted by the inverse number of neighborhoods in the metropolitan area. All regressions include metropolitan area fixed effects. Estimated standard errors, robust to heteroskedasticity and clustering on metropolitan area, are in parentheses. ^{a}—$p < 0.10$, ^{b}—$p < 0.05$, ^{c}—$p < 0.01$.

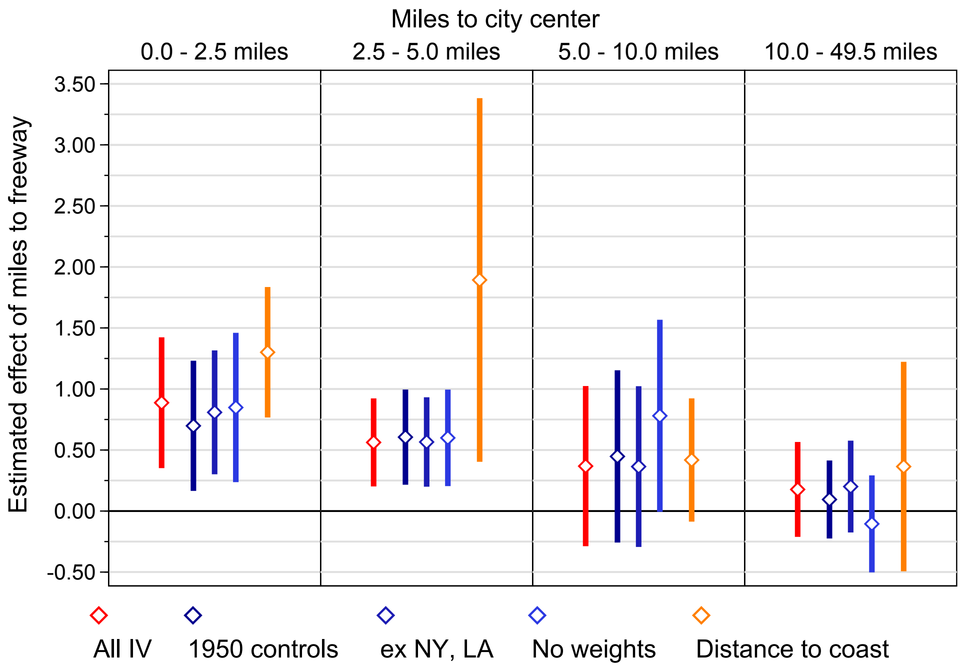

Figure D.2: Robustness of freeway effects on population

Estimates from separate instrumental-variables fixed-effects regressions of the logarithm of the 1950–2010 change in consistent-tract population on distance to nearest highway in miles. All regressions include metropolitan area fixed effects. Lines extending from point estimates show 95 percent confidence intervals, robust to heteroskedasticity and clustering on metropolitan area.

line in each panel of Figure D.2 reports coefficient estimates where the sample of neighborhoods is conditioned on distance to the nearest coastline instead of distance to the city center.[64] Coastlines potentially provide production benefits (i.e., job centers tend to be coastal) and consumption benefits (views, beaches, and moderate temperatures are all complements to recreational activities). Given that coastlines tend to be desirable regional destinations, we expect that locations far from the coast benefit more from freeway access, while locations near the coast would mostly experience only the freeway disamenity. The estimates in this case are very similar to those using distance to the CBD. Freeways have large negative effects for neighborhoods close to coastlines, and these negative effects attenuate with distance to the coast. Overall, this provides additional insight in the cost and benefits of highway construction in urban areas.

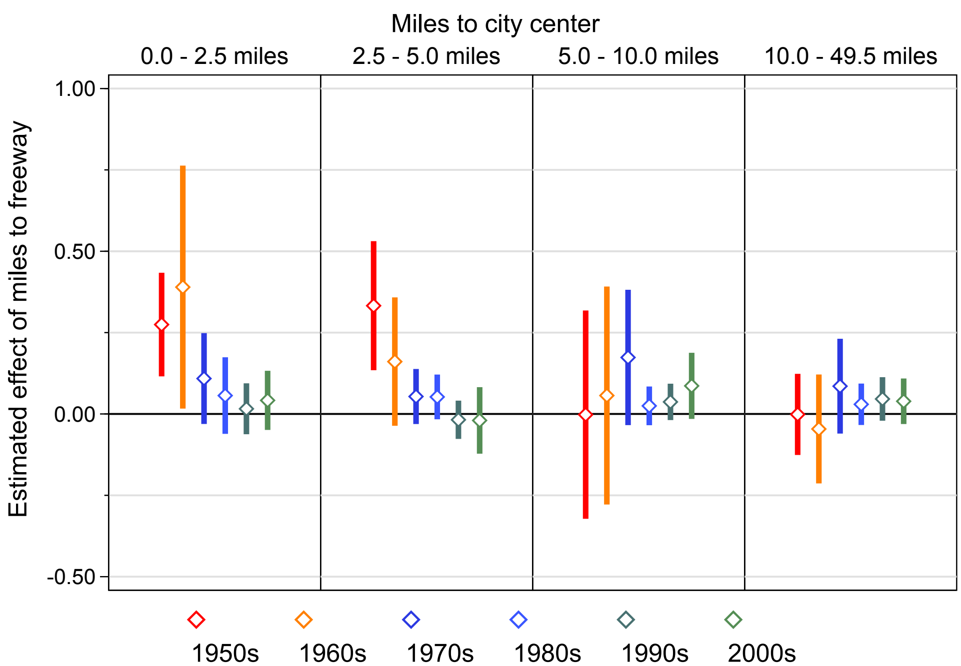

Figure D.3: Freeway effects on population largest in the 1950s and 1960s

Estimates from separate instrumental-variables fixed-effects regressions of the logarithm of the 10-year change in consistent-tract population on distance to nearest highway in miles. All regressions include metropolitan area fixed effects. Lines extending from point estimates show 95 percent confidence intervals, robust to heteroskedasticity and clustering on metropolitan area.

Next, we investigate the change in neighborhood population over time, accounting for the timing of interstate construction. In this exercise we regress change in population in each decade on distance to the CBD and distance to the highway on only highways that were currently completed. We use the same specification and IV strategy as before. Note that these estimates differ in three ways compared with those reported earlier. One, we use the PR-511 database to measure the year each interstate segment was first open to traffic. Two, because the PR-511 database only includes designated Interstate highways, we cannot measure the date when non-Interstate limited-access freeways were first open to traffic. Thus, neighborhood freeway proximity is conditioned on

[64]For this analysis we include Great Lakes in addition to oceans, and we drop metropolitan areas that are not near a coastline.

distance to the nearest *Interstate* highway in these regressions. Three, these are 10-year changes in population, so the magnitudes of the coefficients are expected to be smaller to the extent that adjustment may be slow.

The negative effects of freeway construction in central cities were most pronounced between 1950 and 1970. Figure D.3 shows these estimates. These estimates may provide additional validation of the instrumental variables estimates of the causal effect of freeways on downtown neighborhoods, since the historical and statistical evidence presented in the previous section suggests that early highway construction was less selected on neighborhood factors owing to the surprise of the revolts.

D.3 Sorting

Next, we consider the effects of freeways on the spatial sorting of different types of households. We regress the change in the logarithm of average household income between 1950 and 2010 on neighborhood distance to the nearest freeway. Note that the theoretical predictions for sorting effects are ambiguous and depend on the source(s) of household heterogeneity, as well as the form of the commuting technology.

The results in Figure D.4 illustrate the effect of highway proximity on the relative change in income, separated by distance to the CBD. Neighborhoods farther from highways had larger income growth, and this effect was somewhat larger near the CBD. These results are consistent with several sources of heterogeneity, and thus we cannot definitively attribute these results to specific differences between income groups.

The changes observed would be consistent with lower expenditure shares on housing for higher income groups. As transportation costs decline, higher income groups benefit relatively more from moving to areas farther from the CBD. In addition, particularly near the CBD, high income households would sort away from the freeway due to the disamenity. In suburban areas, the sorting with respect to proximity would be ambiguous, and the estimates are consistent with this explanation.

However, the empirical results would also be consistent with other sources of heterogeneity. If amenity valuation changes by income then this would result in sorting away from freeways everywhere. In addition, differences in relative benefits of increased access could lead to sorting of high income residents away from the CBD. This would happen in the presence of fixed or per mile commuting costs, that are not proportional to income.

While we cannot pin down the structural source of changes in sorting patterns, the results do suggest that freeway construction has a relatively greater effect on the bid rent of high income groups in terms of both increased benefits of access and decreased amenities near freeways. More generally, this result is consistent with the idea that high income workers will outbid low income worker for the "best" neighborhoods in terms of access and amenities, which aligns with the mechanisms and analysis by Lee and Lin (2018).

D.4 Housing and land values

Next, we estimate the effects of freeways on housing and land prices. Land values would seem to be the most direct test of freeway disamenities. However, reliable measures of land value are difficult to obtain for a large universe of small geographic units in the 1950s. While housing prices are available in the Census of Population and Housing, unobserved heterogeneity in housing quality presents another challenge for inference. Unfortunately, the 1950 housing tables for census tracts only report home values for owner-occupied housing units in single-unit structures. Therefore, reported home values represent a selected sample, especially in central neighborhoods where both

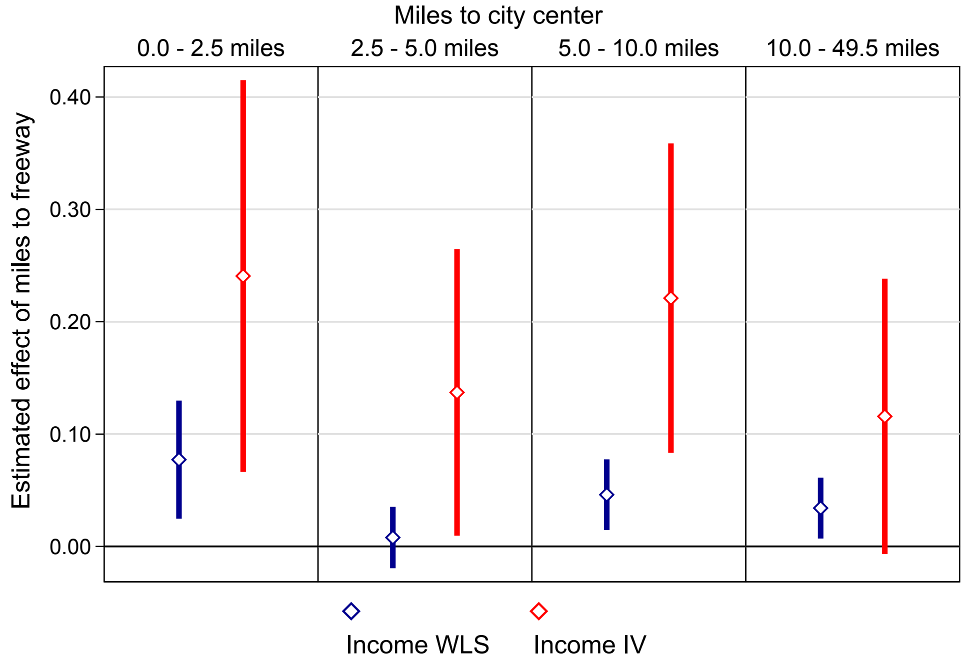

Figure D.4: Incomes increased more farther from freeways

Each point is an estimate from a separate fixed-effects regressions of the logarithm of the 1950–2010 change in consistent-tract average household income on distance to nearest highway in miles. All regressions include metropolitan area fixed effects. Lines extending from point estimates show 95 percent confidence intervals, robust to heteroskedasticity and clustering on metropolitan area.

owner-occupiers and single-unit structures are less common. There are also no measures of housing unit size or quality in the 1950 tract data by which we might adjust reported home values.[65]

Those important caveats aside, we estimate the effect of highways on housing prices for owner-occupied housing units in single-unit structures (having obtained measures of the same concept from the 5-year American Community Survey estimates for 2006–2010.) These estimates are shown in Figure D.5. Conditioned on not being able to measure housing quality, the point estimates suggest that housing prices increased faster away from highways. This is perhaps with disamenities from highways, although the estimates lack the attenuation pattern with proximity to the city center seen for other outcomes.

To provide further evidence in light of the limitations of the census house-price data, we turn to a measure of land values available for Chicago. We obtained appraised land values for 330×330 foot grid cells from *Olcott's Blue Books* in 1949 and 1990 from a database digitized by Ahlfeldt and McMillen (Ahlfeldt and McMillen, 2014 and 2018, and McMillen, 2015). The smoothed data are shown in Figure D.6.[66] Here the patterns are more clear compared with census housing prices. In

[65]The sole exception is a measure of crowdedness, the count of the number of housing units for which the ratio of occupants to rooms exceeds 1. Unfortunately, other census tract tables only report the average number of occupants per housing unit, regardless of size, and units by number of rooms are reported in relatively coarse categories.

[66]Note that this analysis is conducted at the grid cell level (of which there are 86,205), not the tract level. While there are few census tract centroids beyond 1 mile from the nearest freeway, it is nearly 4 miles from a freeway to the eastern Loop.

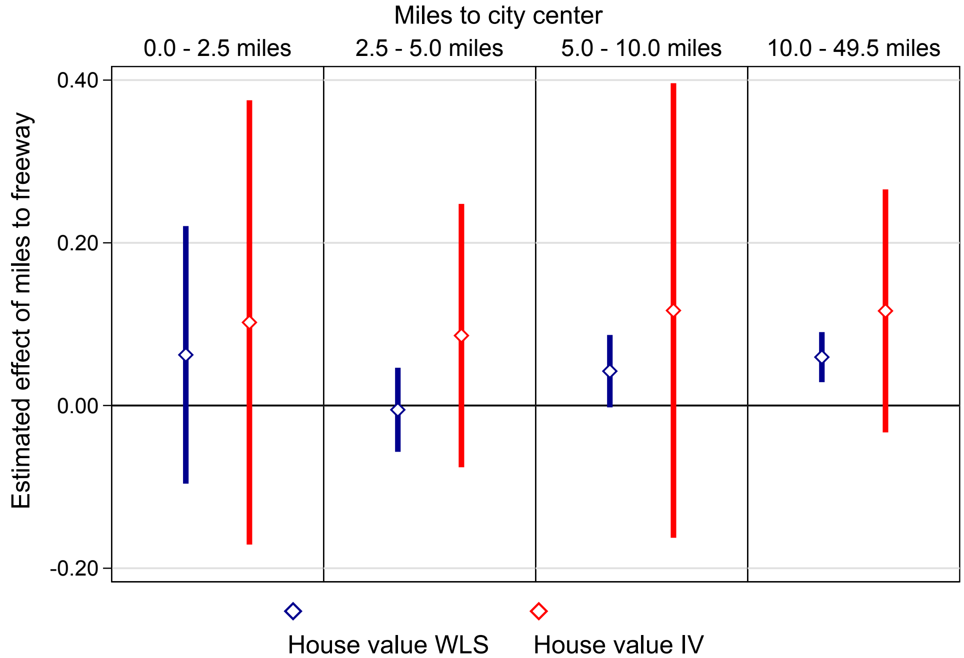

Figure D.5: House prices increased more farther from freeways

Each point is an estimate from a separate fixed-effects regressions of the logarithm of the 1950–2010 change in consistent-tract average house price for owner-occupied housing units in single-unit structures only on distance to nearest highway in miles. All regressions include metropolitan area fixed effects. Lines extending from point estimates show 95 percent confidence intervals, robust to heteroskedasticity and clustering on metropolitan area.

the core areas of Chicago, tracts closest to freeways saw slower land value appreciation compared with tracts farther away. In the peripheral areas of Chicago, tracts closest to freeways saw faster land value appreciation compared with tracts farther away. These patterns seem consistent with reduced household and firm demand for land near highways in downtown Chicago.

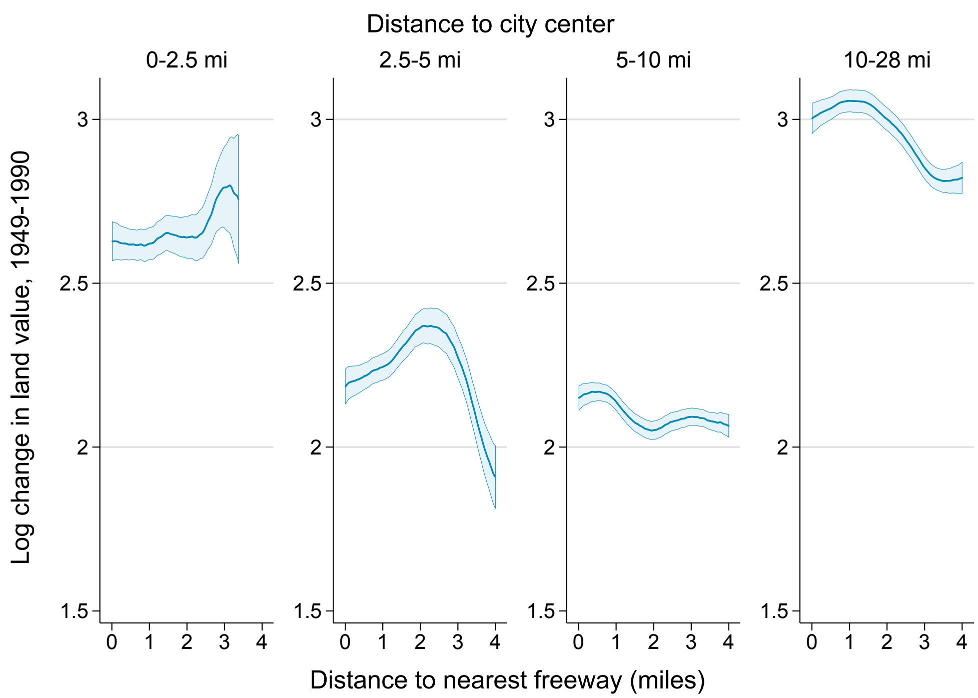

Figure D.6: Land value growth in Chicago, 1949–1990

Lines show kernel-weighted local polynomial smooths of the 1949–1990 change in the natural logarithm of appraised land value in the Chicago metropolitan area. Smooths use Epanechnikov kernel with bandwidth 0.3 and local-mean smoothing. Shaded areas indicate 95 percent confidence intervals.

E Solving for equilibrium

This section outlines the method to solve the equilibrium of the model for known parameter values. The methods described here for a closed city can easily be modified to solve for an open city.[67] Preference and production parameters $\{\alpha, \beta, \varepsilon\}$, location fundamentals $\{A_k, B_j\}$, land area (L_j), travel costs (d_{jk}), and total population (N) are known.

Our goal is to solve for the endogenous objects rents, wages, commuting flows, population, employment and land use $\{q_j,\ w_j,\ \pi_{jk},\ N_{Hj},\ N_{Wj},\ \theta_j\}$. The algorithm proceeds iteratively using an initial guess for location specific rents and wages denoted by $\{q_j^0, w_k^0\}$. Given this initial guess, the model admits closed form solutions for all endogenous objects, and allows for the calculation of updated vales of wages and rents, denoted by $\{q_j^1, w_k^1\}$. The algorithm then iterates until convergence. The required equations are given by the following.

1. Fraction of workers who chose each commuting pair:

$$\pi_{jk}^1 = \frac{\left(d_{jk}\left(q_j^0\right)^{1-\beta}\right)^{-\varepsilon}\left(B_j w_k^0\right)^{\varepsilon}}{\sum_{j'=1}^{J}\sum_{k'=1}^{J}\left(d_{j'k'}\left(q_{j'}^0\right)^{1-\beta}\right)^{-\varepsilon}\left(B_{j'} w_{k'}^0\right)^{\varepsilon}}.$$

2. Fraction of workers who chose a commute conditional on residential location:

$$\pi_{jk|j}^1 = \frac{\left(\frac{w_k^0}{d_{jk}}\right)^{\varepsilon}}{\sum_{k'=1}^{J}\left(\frac{w_{k'}^0}{d_{jk'}}\right)^{\varepsilon}}.$$

3. Residential population:

$$N_{Hj}^1 = N\sum_{k=1}^{J}\pi_{jk}^1.$$

4. Employment:

$$N_{Wj}^1 = \sum_{k=1}^{J}\pi_{jk}^1 N.$$

5. Residential land use:

$$L_{Hj}^1 = (1-\beta)\frac{N_{Hj}^1}{q_j^0}\sum_{k=1}^{J}\pi_{jk|j}^1\frac{w_k^0}{d_{jk}}.$$

6. Commercial land use:

$$L_{W_j}^1 = N_{W_j}^1\frac{(1-\alpha)}{\alpha}\frac{w_j^0}{q_j^0}.$$

7. Land use function:

[67] In the case of the open city, total population, N, is included as an endogenous variable. The algorithm requires an additional step to check that the expected utility is equal to the reservation utility. This condition is given by Equation 9.

$$\theta_j^1 = \frac{L_{W_j}^1}{L_{W_j}^1 + L_{Hj}^1}.$$

8. Production:

$$Y_j^1 = A_j \left(N_{Wj}^1\right)^{\alpha} \left(\theta_j^1 L_j\right)^{1-\alpha}.$$

9. Updated wages:

$$w_j^1 = \frac{\alpha Y_j^1}{N_{Wj}^1}.$$

10. Updated rents:

$$q_j^1 = \frac{(1-\alpha) Y_j^1}{\theta_j^1 L_j}.$$

F Imputation of missing travel times

The Census Transportation Planning Package does not record commute times for many origin-destination pairs, which are a required input into the quantitative model. We use a two-stage local adaptive bandwidth kernel estimator to impute missing values.[68] The method is based on a Gaussian kernel density estimator that works much like a moving average.

The estimate of the travel time, $\widehat{\tau}_{ij,}$ from an origin i to a destination j is

$$\widehat{\tau}_{ij} = \tfrac{1}{W_{ij}} \sum_{j'} I_{ij'} e^{-\left(\frac{D_{jj'}^2}{A\sigma_{ij}^2}\right)} \tau_{ij'},$$

where $\tau_{ij'}$ represents the observed travel time from the origin to a destination; $D_{jj'}$ is the distance between the destination being estimated, j, and other destinations, j'; $I_{ij'}$ is an indicator for whether the pair is observed or not, and W_{ij} is a constant that normalizes the sum of weights to 1:

$$W_{ij} = \sum_{j'} I_{ij'} e^{-\left(\frac{D_{jj'}^2}{A\sigma_{ij}}\right)}.$$

The constant A is a scale parameter that determines the average bandwidth used in estimating travel times and thus determines how much smoothing is introduced into the estimates. We allow the bandwidth to vary by origin-destination pairs through the term σ_{ij} in order to adapt to the local sparsity of the data near the destination point; i.e., locations with very little data nearby are given larger bandwidths. In the first stage, we calculate the adaptive bandwidth using a kernel density estimator with a fixed bandwidth. We calculate the bandwidths σ_{ij} used in the second stage as the reciprocal of this density estimate.

$$\sigma_{ij} = \frac{\sum_{j'} e^{-\left(\frac{D_{jj'}^2}{B^2}\right)}}{\sum_{j'} I_{ij'} e^{-\left(\frac{D_{jj'}^2}{B^2}\right)}}.$$

B is a constant that determines the sensitivity of the bandwidth to the local sparsity of the data. The constants A and B must be chosen. The proper choice depends on both the structure of the data and characteristics of the application to which the estimates are applied. These are often unobserved or unknown, so some judgment must be made.

Generally, the constant A should increase with the average sparsity of the data, while B should increase with variation in local sparsity. Bailey and Gatrell (1995) provide some guidance on choosing bandwidth parameters. We use $A = 1.5$ and $B = 1$. These values provide a reasonable amount of smoothing where data are sparse, but preserve detailed variation in locations where data are dense. Our final results are not sensitive to these choices.

[68]Various forms of adaptive bandwidth kernel density estimators are widely used and standard in a number of fields. Bailey and Gatrell (1995) provide an introduction.

G Instrumental variable estimates of freeway disamenities

In Section 10, we estimated freeway disamenities by fitting the freeway disamenity function to the calibrated neighborhood amenity values, B_j. One might be concerned that the location of the freeways are endogenous. We turn to an IV strategy using the same instruments as in the reduced form analysis: planned routes, shortest distance, railroads, and exploration routes. We run a first stage regression of distance to a freeway on the instruments. We then fit the recovered location amenities B_j to the disamenity function using the predicted distance to a freeway from the first stage regression.

Table G.1 shows results for different calibrated parameters. Panel (b) show the baseline least squares estimates, and panel (c) shows estimates using the predicted values from a first-stage IV regression. Note that standard errors on the IV estimates are not adjusted to account for the first stage regressions.

In most specifications, the IV estimates of the disamenity b_h are slightly larger compared with the least squares estimates. In the baseline specification (shown in the top row), the IV estimate suggests that there is an amenity reduction of 19.6 percent adjacent to a freeway, compared to the 17.5 percent reduction implied by the least squares estimate. In addition, the effect attenuates at a slower rate. The baseline IV estimate of .497, implies that the effect attenuates by 95 percent at 6 miles from the freeway compared to the distance implied by the least squares estimate of 2.4 miles.

Table G.1: Estimates of disamenity parameters using instruments

(a) Calibrated parameters				(b) LS				(c) IV			
κ	β	α	ϵ	b_h	(s.e.)	η	(s.e.)	b_h	(s.e.*)	η	(s.e.*)
0.002	0.950	0.970	4.000	0.175	0.012	1.284	0.131	0.196	0.009	0.497	0.036
0.001	0.950	0.970	4.000	0.173	0.012	1.357	0.143	0.187	0.009	0.519	0.039
0.004	0.950	0.970	4.000	0.181	0.011	1.147	0.110	0.215	0.008	0.456	0.030
0.002	0.930	0.970	4.000	0.165	0.014	1.748	0.218	0.125	0.010	0.522	0.063
0.002	0.970	0.970	4.000	0.192	0.009	0.919	0.077	0.264	0.008	0.470	0.023
0.002	0.950	0.980	4.000	0.177	0.012	1.285	0.130	0.196	0.009	0.499	0.036
0.002	0.950	0.960	4.000	0.174	0.012	1.284	0.132	0.197	0.009	0.495	0.035
0.002	0.950	0.970	2.000	0.299	0.015	0.850	0.074	0.478	0.007	0.184	0.009
0.002	0.950	0.970	6.000	0.125	0.011	1.815	0.226	0.097	0.008	0.546	0.064

This table shows the estimates and standard errors of the freeway disamenity parameters, b_h and η, for various calibrated parameter vectors, shown in columns 1-4. Columns 5-8 show the least-squares estimates. These are then followed by estimates using the predicted values from a first-stage IV regression in Columns 9-12. *Standard errors for the IV estimates are not corrected for first-stage regressions.

These results suggest that even accounting for the endogeneity of freeway locations, there is a strong correlation between neighborhood amenities and proximity to freeways. For the counterfactual results presented in the paper, we use the structural parameters obtained from the least squares estimate given that they are more conservative and have a more transparent mapping from the observed data.

H Sensitivity of barrier effect results

The barrier effect results in Table 6 are sensitive to both the scale and spatial attenuation of consumption spillovers parameters χ and ρ, as well as the calibration of the barrier cost, $c_{b,jj'}$.

Table H.1 shows sensitivity results. The first two columns report the calibrated consumption spillover parameters, and the next two columns show the calibration of the barrier cost. The last three columns contain the results of the counterfactual experiment where barrier costs are removed, including expected utility, population within 5 miles of the CBD, and population within the city limits of Chicago.

Table H.1: Sensitivity of barrier effect results to calibration

χ	ρ	miles	minutes	$\Delta\,\mathbb{E}[U]$	Δ <5mi	Δ city pop
0.144	0.738	3	2	1.030	1.154	1.059
0.144	0.500	3	2	1.017	1.115	1.047
0.144	0.900	3	2	1.039	1.181	1.065
0.100	0.738	3	2	1.020	1.091	1.032
0.200	0.738	3	2	1.045	1.255	1.112
0.144	0.738	2	2	1.015	1.089	1.041
0.144	0.738	4	2	1.041	1.181	1.058
0.144	0.738	3	1	1.010	1.058	1.022
0.144	0.738	3	3	1.066	1.296	1.111

This table shows the sensitivity to calibration for the counterfactual experiment of removing barrier costs. The first four columns show calibration choices, and the last three columns contain values of expected utility, population within 5 miles of the CBD, and population within the city limits. The results from the main text are shown in the first row.

The results presented in the main text are shown in the first row. In this case the spillover parameters were taken from Ahlfeldt et al. (2015), and the barrier costs were set such that trips under 3 miles had a barrier cost of 2 minutes of travel time when crossing freeways. Subsequent rows show results where individual parameters are adjusted and new counterfactuals are calculated.

All results remain quantitatively significant, but the results are sensitive to parameter choices. For example, when the time cost is adjusted from 2 minutes to 1 minute, the increase in expected utility when barrier costs are removed changes from 3 percent in the baseline to 1 percent. Conversely, when the time cost is increased to 3 minutes, expected utility increased by 6.6 percent in the counterfactual.

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