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Abstract

Why do freeways affect spatial structure? We identify and quantify the local disamenity effects of freeways. Freeways cause slower growth in central neighborhoods (where local disamenities exceed regional accessibility benefits) compared with outlying neighborhoods (where access benefits exceed disamenities). A quantitative model calibrated to Chicago attributes one-third of the effect of freeways on central-city decline to reduced quality of life. Barrier effects are a major factor in the disamenity value of a freeway. Local disamenities from freeways, as opposed to their regional accessibility benefits, had large effects on the spatial structure of cities, suburbanization, and welfare.

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

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Disclaimer: This Philadelphia Fed working paper represents preliminary research that is being circulated for discussion purposes. The views expressed here are solely those of the authors and do not necessarily represent the views of the Federal Reserve Bank of Philadelphia or the Federal Reserve System. Any errors or omissions are the responsibility of the authors. **Acknowledgements**: We thank Cameron Fen, Annette Gailliot, and David Mok-Lamme for outstanding research assistance; Jon Hallas, Cris McCollum, Roberto Sarmiento, and Kermit Wies for help finding travel survey data; Gabriel Ahlfeldt and Dan McMillen for sharing Chicago land value data; Nate Baum-Snow for sharing PR-511 data; and Richard Weingroff for advice on highway history and data. We received helpful comments from two anonymous referees, Nate Baum-Snow, Jonathan Dingel, Ed Glaeser, Jonathan Hall, Osborne Jackson, Matt Kahn, Jake Krimmel, Ed Kung, Jordan Riesenberg, Chris Severen, and many conference and workshop participants. First version: June 8, 2016.

1 Introduction

We identify freeway disamenities and quantify their effects on city structure and welfare. In our analysis, freeways have two effects on spatial structure. One, freeways improve regional job accessibility. Two, freeways reduce local neighborhood quality of life through, e.g., noise, pollution, and barrier effects. The net effect of freeways combines accessibility benefits *and* reduced local quality of life. We use two strategies to isolate the disamenity effects of freeways.

First, we use proximity to a city center as a proxy for employment access. In a simple monocentric city model, freeways have negative *local* effects near the city center. A negative local effect means that, conditioned on distance to the city center, a neighborhood next to a freeway declines more compared with one farther from a freeway. Intuitively, central neighborhoods near freeways already had superior *ex ante* job access, so disamenities outweigh modest accessibility benefits from new freeways. In contrast, freeways deliver more access benefits to peripheral neighborhoods, so the net local effects of suburban freeways are less negative, or even positive. We confirm these predictions using a consistent-boundary neighborhood panel across 64 U.S. metropolitan areas from 1950–2010. Negative local effects of freeways in central cities are easily explained by disamenities but more difficult to reconcile with standard models that focus exclusively on freeways' effects on reducing commuting costs. Through the lens of a simple monocentric city model, these results identify significant negative quality of life effects of freeways.

Second, we develop a quantitative spatial equilibrium model of city structure that considers the joint location decisions of employment and population with costly commuting, following Ahlfeldt et al. (2015). This framework takes into account endogenous job location and general equilibrium effects. We calibrate our model to neighborhood population, jobs, and travel times within the Chicago metropolitan area in the year 2000. The calibration yields residual neighborhood amenities; using these, we estimate neighborhood amenities are 18% lower next to a freeway, attenuating by 95% at 2.4 miles. This is a large effect, equivalent to three-quarters of a standard deviation in the city's overall neighborhood amenity distribution.

Our findings are important for understanding historical suburbanization, for evaluating mitigation policies such as capping freeways, and for understanding the *freeway revolts*, widespread protests in the 1950s and 1960s by central-city residents in response to early urban Interstate construction. To show this, we perform counterfactual simulations of our calibrated model mitigating all freeway disamenities while maintaining the commuting benefits of freeways. This experiment is analogous to real-world policies like Boston's "Big Dig" that buried a central freeway.

There are three main results. One, the welfare costs of freeway disamenities are large, equivalent to about 5% of income. The marginal benefits of mitigating disamenities are also much higher in central neighborhoods. These large, spatially concentrated benefits could exceed costs for targeted disamenity mitigation projects.

Two, one-third of the effect of freeways on central city population decline can be attributed to freeway disamenities. In the counterfactual simulation, population in the city of Chicago increases by about 8%—about one-third of Baum-Snow's (2007) estimate that freeways caused the population of U.S. central cities to decline by 25%. As Duranton and Puga (2015) note, while *relative* declines of central cities in response to freeways are consistent with standard models, it is more difficult to rationalize the large *absolute* declines of central cities. Our results help explain these absolute declines in central city populations, and thus depart from the consensus among economists that freeways affect spatial structure solely by reducing transport costs (Chandra and Thompson, 2000; Baum-Snow, 2007; Michaels, 2008; Duranton and Turner, 2012; Allen and Arkolakis, 2014; Redding and Turner, 2015). Instead, the amenity channel is a quantitatively important factor in the spatial effects of freeways.[1]

Three, barrier effects are a significant factor in the disamenity value of freeways. Barrier effects are increases in the cost of travel between neighborhoods severed by a freeway (Anciaes and Mindell, 2021). We use 1953–1994 changes in travel behavior from newly-rediscovered trip diary microdata to estimate that for short trips up to 3 miles that are interrupted by new freeways, travel flows decline and travel times increase. Using our estimates of barrier effects, we simulate the effect of mitigating barrier effects alone. Removing barrier effects for short trips crossing a freeway

[1] Ahlfeldt et al. (2019) contrast accessibility versus noise effects of a rail line in Berlin.

produces large welfare benefits, accounting for half of the total disamenity value of freeways.

These are the first estimates of freeway barrier effects on all-mode travel behavior, adding to a literature that studies barrier effects on wildlife (e.g., Forman and Alexander, 1998) and pedestrians (Downs, 1970). We complement work on freeway noise or pollution effects on health or housing prices (e.g., Robinson, 1971; Caro, 1974; Hoek et al., 2002; Gauderman et al., 2007; Parry et al., 2007; Currie and Walker, 2011; Rosenbloom et al., 2012). Our work instead emphasizes the implications of freeway disamenities for the spatial structure of cities (i.e., quantities) and welfare. Further, we identify a larger spatial scale for freeway disamenities compared with other estimates. For example, Anderson (2020) studies pollution effects on mortality up to 600 meters (0.4 miles) from freeways. In contrast, barrier effects extend up to 3 miles from freeways.[2]

We address several identification concerns in interpreting our evidence. One, central freeways may have been allocated to neighborhoods with inferior growth potential. If so, then low neighborhood population today might indicate unobserved factors instead of low quality of life. Instrumental variables estimates suggest freeways were *not* allocated to neighborhoods expected to decline. Additional narrative and statistical evidence suggest that freeways were in fact allocated to neighborhoods with superior growth potential. Two, central freeways may have attracted firms. If so, then low neighborhood population today might indicate strong demand for nonresidential uses. Using new estimates of historical neighborhood employment from Chicago and Detroit, we estimate null employment effects of freeways on central neighborhoods. In addition, we find inferior land and housing price growth near central freeways, and negligible local productivity gains.

In sum, the design of urban Interstates in U.S. cities had large external welfare costs and disparate spatial effects. While freeways likely created net benefits at regional or national scales (Duranton and Turner, 2012; Allen and Arkolakis, 2014), our analysis contributes to better estimates of the (non-construction) costs of the Interstate program. If policymakers had considered freeway disamenities by mitigating them on initial construction or routing freeways farther from

[2] Our results validate measures of urban isolation by Briant et al. (2015). They also provide an alternative interpretation of the role of road and rail corridors in racial segregation (Ananat, 2011). Quoting Schelling (1963), Ananat suggests railroads coordinate expectations among households, realtors, and others in maintaining racially segregation. Our results instead emphasize that freeways increase the "fundamental" cost of cross-neighborhood interaction.

downtowns, they likely would have increased the net benefits of the Interstate program (Boarnet, 2014). The large welfare costs of urban freeways also provide insight into why the freeway revolts were concentrated in central neighborhoods, and why mitigation initiatives today often focus on downtowns.

2 Background: Building the urban Interstates

Following the passage of the Federal-Aid Highway Act of 1956, initial freeway construction moved fast. Early national design standards (U.S. Congress, 1944; AASHTO, 1957) called for each city to feature several radial routes intersecting near the central business district (CBD) and one or more circumferential beltways. Planners faced few constraints and little opposition as they moved to build the Interstates. At the beginning of the Interstate era, state and federal highway engineers "had complete control over freeway route locations" (Mohl, 2004, p. 674).

But mass construction led to skepticism, then outright protests, which spread to at least 50 U.S. cities.[3] The freeway revolts set central-city residents (concerned about local quality of life) against planners (who viewed freeways as key to regional growth). In response, policy gradually ceded more control to local neighborhood concerns. Later highway acts required a public hearing, and economic, environmental, and historical preservation reviews.[4] By 1967, "the freeway debates and protests[...] began 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).

Central-city residents recognized the disamenities of early urban freeway construction. Famously, neighborhood advocates, including Jane Jacobs, fought the construction of central-city freeways such as the Lower Manhattan Expressway. While there are no systematic data on the precise timing and location of opposition to freeways, intriguingly, the growing revolts and evolving policy environment appeared to shape the allocation of freeways within cities. Especially by the

[3] Mohl (2004) studies revolts in Miami and Baltimore. 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). Highway planners faced "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).

[4] 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). See Table A.1 for a timeline of federal policy changes.

late 1960s, "freeway fighters successfully forced the adoption of alternative routes, and they even shut down some specific Interstate projects permanently" (Mohl, 2004, p. 675). Thus, deviations between planned freeways and completed freeways may provide clues about the location of the revolts and where freeway disamenities were most salient.

We provide broad quantitative support for Mohl's claim by comparing completed freeways to the 1955 "Yellow Book" plan for 50 U.S. cities (U.S. Department of Commerce, 1955). This was the first national publication describing planned freeway routes *within* each major U.S. metropolitan area.[5] Using these data, we compute the within-city, tract-level correlation between distance to the nearest completed freeway and distance to the nearest planned freeway.[6] (Section 4.1 and Appendix C describe these data.) If completed freeways are 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.

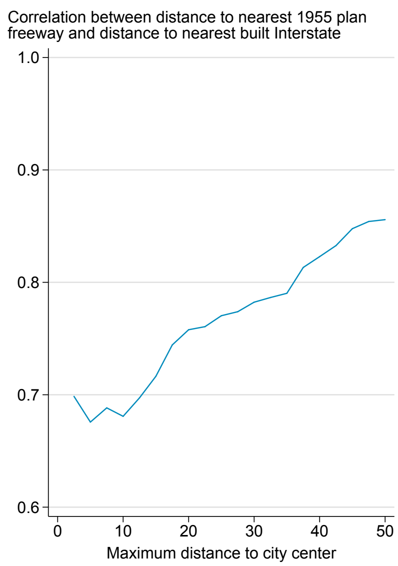
We find that *central* freeways were most likely to deviate from plan. Figure 1a shows correlation coefficients for groups of census tracts successively farther from the city center. The city's center is defined as a point in space using the 1982 Census of Retail Trade (Fee and Hartley, 2013). For tracts within 10 miles of city centers, the correlation between distances to the nearest planned freeway and the nearest completed freeway is 0.7: central freeways were more likely to deviate from plan. For tracts farther out, the correlation between planned and built freeways increases: suburban freeways were likely to be completed according to plan.[7]

In addition, deviations between planned and built freeways increased faster and farther in central neighborhoods over time. Figure 1b displays a similar exercise, except we group tracts according to the year that the nearest built freeway was first open to traffic. Freeways opened 1955–1957 were largely completed to plan ($\rho > 0.95$). However, this correlation fell to 0.86 by 1993 as new freeways deviated from plan. The decline in spatial correlation between planned and built routes

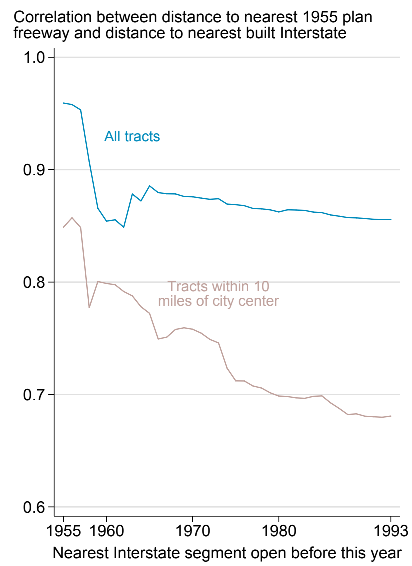
[5] Weingroff (1996) describes the history of the Yellow Book.

[6] These are (conditional) correlation coefficients from tract-level regressions of distance to the nearest completed freeway on distance to the nearest planned freeway, conditioned on metropolitan area fixed effects.

[7] In rural areas, plans were largely implemented as originally specified (Campbell and Hubbard, 2016).



(a) Completed freeways least resemble planned freeways in central areas



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

Figure 1: 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 panel (a), regressions use tracts with a maximum distance to city center of *x* miles, as indicated by the horizontal axis. In panel (b), regressions use tracts near Interstate segments open by year *x*, as indicated by the horizontal axis. Data are described in Section 4.1 and Appendix C.

was especially sharp in central neighborhoods, from 0.85 in 1955–1957 to 0.68 in 1993. This divergence is consistent with opposition to urban freeways and with the timeline of policy changes that ceded more power to neighborhood interests over the 1960s.

There is little evidence for alternative explanations of these patterns, e.g., that planners prioritized "easy to build" segments or discovered other factors, besides resident opposition, affecting central freeway construction. Despite their eventual extent, the revolts were largely unanticipated by planners, builders, and even later critics of the Interstate program. "[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"' (Weingroff, 2016).[8] 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 on idiosyncratic factors (Johnson, 1965). In fact, Appendix A shows that later freeways increasingly favored observed factors associated with *lower* costs—natural and historical rights of way such as coastlines, rivers, and rail lines. Later freeways also increasingly favored neighborhoods that were initially less dense, more Black, and less educated. This may have lowered land acquisition costs and preserved future tax revenues (Carter, 2018). Black or less-educated neighborhoods may have also been less able to take advantage of freeway-fighting policy reforms, consistent with the theory of Glaeser and Ponzetto (2018). Overall, these results suggest that central-city residents recognized the disamenities from new freeways and, in some cases, successfully opposed them.

3 Theory

We develop a spatial equilibrium model of freeway disamenities and city structure. The model considers the joint location decisions of employment and population in a city with costly commuting, following Ahlfeldt et al. (2015). In our model, freeways affect spatial structure through two margins: job access and quality of life. Then, we present a simplified version of the model with a predetermined CBD. The simplified model clarifies freeways' regional accessibility benefits versus their local disamenity effects and provides testable predictions of the effects of freeways. In Sec-

[8] See Appendix A.4 for additional narrative evidence.

tion 4, we test these simplified model predictions using panel data on neighborhoods. In Section 5, we estimate key parameters and calibrate the full model to cross-neighborhood variation in the Chicago metropolitan area in the year 2000. We use the resulting neighborhood amenity estimates to quantify freeway disamenities. Finally, in Section 6, we use counterfactual simulations of the full calibrated model to estimate the welfare and decentralization effects of freeway disamenities.

We contribute to a recent quantitative spatial literature by using similar methods to study the negative amenity effects of transportation infrastructure.[9] In addition, we use neighborhood amenities recovered from the structure of the model to estimate freeway disamenities. This method is related to approaches that use local wages and prices to study productivity and amenity factors across and within cities (e.g., Roback, 1982; Albouy and Lue, 2015).

3.1 Spatial model of freeway disamenities

A city has J neighborhoods each with land area L_j that may be split between consumption and production. There are iceberg commuting costs $d_{jk} \equiv e^{\kappa \tau_{jk}}$, where τ_{jk} is the travel time between neighborhoods j and k, and κ describes the relationship between travel time and costs. We assume a closed city: total population N is fixed and expected utility is endogenous. This allows for comparison of counterfactual experiments in terms of expected utility.[10]

Workers. Workers are homogeneous and have increasing preferences over consumption c, land l, and neighborhood amenities B_j.[11] Each worker m has an idiosyncratic preference $\nu_{jk,m}$ for a given home-work pair $\{j,k\}$, drawn from a Frechet distribution with shape parameter ε.[12] 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. Workers

[9] c.f. Allen and Arkolakis (2014), Ahlfeldt et al. (2015) and surveys by Redding and Rossi-Hansberg (2017) and Holmes and Sieg (2015). Donaldson (2018), Monte et al. (2018), and Severen (2019) study the effects of infrastructure investment on trade or commuting.

[10] It is possible to model an open city. In equilibrium, expected utility in the city equals an outside reservation utility, which means that location choice must occur in two stages: first, workers choose between living in the city and living in another city; second, workers draw their idiosyncratic preference shock and choose the best origin–destination pair within the city.

[11] We assume direct consumption of land and thus do not explicitly model housing production. This is equivalent to assuming mobile capital and a Cobb-Douglas form. For evidence in support of this assumption, see Thorsnes (1997), Epple, Gordon, and Sieg (2010), and Combes, Duranton, and Gobillon (2021).

[12] Some models include neighborhood-specific mean-shifting terms in the Frechet distribution. Ours subsumes these in B_j and A_k.

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

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

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

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

$$N_{Wk} = \sum_{j=1}^{J}\left[\frac{\left(\frac{w_k}{d_{jk}}\right)^{\varepsilon}}{\sum_{k'=1}^{J}\left(\frac{w_{k'}}{d_{jk'}}\right)^{\varepsilon}}N_{Rj}\right], \tag{2}$$

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

$$L_{Rj} = (1-\beta)\frac{N_{Rj}}{q_j}\sum_{k=1}^{J}\pi_{jk|j}\frac{w_k}{d_{jk}}. \tag{3}$$

Finally, expected utility is

$$E[u] = \Gamma\left(\frac{\varepsilon-1}{\varepsilon}\right)\left[\sum_{j'=1}^{J}\sum_{k'=1}^{J}\left(d_{j'k'}q_{j'}^{1-\beta}\right)^{-\varepsilon}\left(B_{j'}w_{k'}\right)^{\varepsilon}\right]^{1/\varepsilon}. \tag{4}$$

Freeway disamenities. B_j represents nearly all neighborhood amenities, including natural factors such as beaches and endogenous factors such as schools, shopping, or safety. The exception is job accessibility, which is handled explicitly by the commuting structure of the model. We assume $B_j = b_j g\left(\delta_{Fj}\right)$, where $g\left(\delta_{Fj}\right)$ describes the disamenity at a given distance to the freeway,

δ_{Fj}. For now, the disamenity is a simple function of distance to the freeway and does not depend on endogenous variables (see Section 6.3). The freeway disamenity is

$$g\left(\delta_{Fj}\right) = 1 - b_F e^{-\eta \delta_{Fj}}, \tag{5}$$

where b_F is the size of the disamenity and η describes its spatial attenuation. This is isomorphic to a cost that decays exponentially with distance to the freeway. Henderson (1977) and Nelson (1982) use similar forms to study the spatial costs of noise or pollution.

Production. A single final good is costlessly traded and produced under constant returns and perfect competition according to $Y_k = A_k L_{Wk}^{1-\alpha} N_{Wk}^{\alpha}$ in each location, where A_k is total factor productivity, L_{Wk} is total land used for production, N_{Wk} is total employment, and α is the labor share in production. Profit maximization yields total commercial land use in each location:

$$L_{W_k} = N_{W_k} \frac{(1-\alpha)}{\alpha} \frac{w_k}{q_j}. \tag{6}$$

We omit two potentially important features here. One, neighborhood productivity A_k is exogenous, abstracting 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. Two, there is no production amenity or disamenity from freeways analogous to the consumption disamenity (equation 5). This is consistent with null employment effects and negligible productivity effects of central freeways (Sections 4.3 and 5.2).

Equilibrium. To define equilibrium, assume that land area and travel costs $\{L_j, d_{jk}\}$ and total population N are exogenous; we observe these 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 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 2, (ii) land markets clear such that land demand (equations 3 and 6) sum to land supply L_j in each location, and (iii) total population equals N. Ahlfeldt et al. (2015) provide proofs of existence and uniqueness. In practice, the model is solved iteratively (see Appendix B).

3.2 Simplified model with central business district

Next, we present a simplified version of the model. Assume a location called the CBD contains all jobs with a single fixed wage: $A_k = 0$ for all $k \neq \text{CBD}$ and $A_{\text{CBD}} > 0$. This is the "monocentric city" assumption of Alonso (1964), Muth (1969), and Mills (1967). Equation 1 now defines residential population, N_j, given that commuting costs d_j now depend only on residential location choice:

$$N_j = \frac{\left(d_j q_j^{1-\beta}\right)^{-\varepsilon} \left(wB_j\right)^{\varepsilon}}{\sum_{j'=1}^{J} \left(d_{j'} q_{j'}^{1-\beta}\right)^{-\varepsilon} \left(wB_{j'}\right)^{\varepsilon}}. \tag{7}$$

Since land is only consumed by residents, rents depend on population:

$$q_j = (1-\beta)\frac{N_j}{L_j}\frac{w}{d_j}. \tag{8}$$

Consider two neighborhoods j, j'. Combining equations 7 and 8, the relative population density of j compared with j' is:

$$\frac{N_j/L_j}{N_{j'}/L_{j'}} = \left(\frac{B_j}{B_{j'}}\right)^{\zeta} \left(\frac{d_j}{d_{j'}}\right)^{-\beta\zeta}, \tag{9}$$

where $\zeta \equiv \varepsilon/(1+\varepsilon-\beta\varepsilon)$. Intuitively, the relative density of j increases with its amenity advantage $B_j/B_{j'}$ and decreases with its commuting cost disadvantage $(d_j/d_{j'})$.

Suppose we construct a freeway through the city. Then, using $d_{jk} \equiv e^{\kappa\tau_{jk}}$, the change in relative populations due to the new freeway is $\Delta \log \frac{N_j}{N_{j'}} = \zeta\left(\Delta \log B_j - \Delta \log B_{j'}\right) - \zeta\beta\kappa(\Delta\tau_j - \Delta\tau_{j'})$, where Δ denotes the first-difference operator. In words, the change in neighborhood i's relative size depends on the relative changes in amenity (the first term) and commute times (the second term).

First, consider the case where the freeway has *no disamenity effect*. Then, the first amenity term is zero, and the freeway's effect is to increase the relative size of the neighborhood that experiences superior commuting time savings. If neighborhood $1'$ is farther from the central business district than neighborhood 1, then commute time declines more for $1'$. Since $-\zeta\beta\kappa(\Delta\tau_1 - \Delta\tau_{1'}) < 0$, neighborhood $1'$ increases in relative size. This is a discretized version of the standard monocentric model prediction that locations farther from the city center grow because they benefit from superior reductions in commute time (e.g., Baum-Snow, 2007).

Second, consider the case where the freeway has a disamenity effect, as in equation 5. When the freeway reduces local quality of life, then the net effect of the access and amenity channels depends on *centrality*—that is, whether we compare neighborhoods near the city center, or neighborhoods on the suburban periphery. To see this, consider the geography of the simplified model city shown in Figure 2. Neighborhoods 1 and 2 are close to, and equidistant from, the CBD. Neighborhood 2 is farther from the freeway ($\delta_{F,2} > \delta_{F,1} = 0$). Define the *local effect* of the freeway as the contrast between a neighborhood next to the freeway and a neighborhood far from the freeway, conditioned on distance to the central business district, e.g., $\Delta \log N_1 - \Delta \log N_2$. The local effect of the freeway is composed of a disamenity effect and a commuting cost effect. The change in amenity value is strictly worse for neighborhood 1 due to the freeway disamenity, $\Delta B_1 < \Delta B_2$, so the disamenity channel has a strictly negative effect on the relative size of 1.

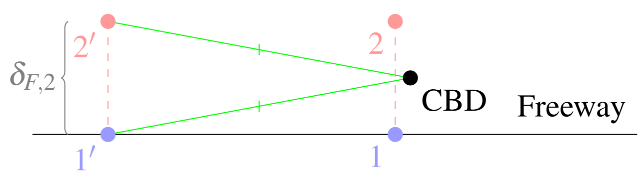


Figure 2: Geography of simplified model city

For neighborhoods 1 and 2 *near* the CBD, the relative commute time decline will be minimal. Thus, the net local effect of the freeway will be negative. In the limiting case, the line segments from both neighborhoods to the CBD are orthogonal to the freeway, so the decline in commute times for both is zero ($\Delta\tau_1 = \Delta\tau_2 = 0$). Since $\Delta B_1 < \Delta B_2$, the new freeway reduces the relative size of 1. This prediction departs from the the standard model: without freeway disamenities, the net local effect is zero.

Next, hold fixed the locations of 1 and 2 relative to the freeway, but consider $1'$ and $2'$ *far* from the CBD, as in Figure 2. The local effect of the freeway may be less negative, or even positive. The disamenity effect is identical to the city center case. But the job access effect will be more favorable in the city's periphery. That is, if $\Delta\tau_{1'} - \Delta\tau_{2'} \leq 0 = \Delta\tau_1 - \Delta\tau_2$, then the local effect of the freeway will be *less negative* for peripheral locations compared with the city center. This condition will be satisfied as long as commute times decline *more* for neighborhoods close to the new freeway compared with neighborhoods far from the new freeway. This seems likely, as for outlying neighborhoods, proximity to a freeway has clear benefits for reducing travel times.[13]

The local effect of the freeway in the suburbs might even be *positive* if the declines in commute time experienced by $1'$ are sufficiently more negative than those experienced by $2'$, i.e., $\Delta\tau_{1'} - \Delta\tau_{2'} < \frac{1}{\beta\kappa}(\Delta \log B_{1'} - \Delta \log B_{2'})$. That is, if the freeway's commute time benefits to $1'$ are large enough compared with the commute time benefits to $2'$ to overcome the disamenity effect, then the local effect of the (suburban) freeway will be positive.

Thus, our simplified model predicts that the local effects of freeways depend on *ex ante* centrality. In central cities, conditioned on proximity to the city center, neighborhoods near freeways will decline more than those farther from freeways. This is because, near city centers, the commuting benefits of freeways are small relative to the negative quality of life effects of freeways. This prediction is in contrast to standard models without a freeway disamenity channel, which do not predict negative local effects of freeways. In outlying areas, if freeways improve commuting times, then the local effects of freeways may be less negative, or even positive, as commuting access benefits offset freeway disamenities.

The simplified model abstracts from many potentially important margins. First, freeways may have been routed through neighborhoods expected to decline. However, narrative and statistical evidence suggests that planners targeted neighborhoods expected to grow (Section 2, Appendix A). Instrumental variables estimates support this conclusion (Section 4). These results suggest that negative selection is not a major concern.

Second, the simplified model abstracts from employment location. Freeways caused employment to suburbanize, though at a slower rate than population (Baum-Snow, 2020). But for evaluat-

[13]This will arise, if, for example, travel to the city center from the more-distant neighborhood 2 results in less travel distance *along* the freeway (and thus, less time savings). Alternatively, this condition will be satisfied if there is increased congestion and travel time from 2 *to* the freeway.

ing our simplified model predictions, the confounder is whether freeways increase *local* employment, *conditioned on neighborhood centrality*. If so, then local population declines near freeways may represent increased demand by firms. However, we do not find evidence of significant local job effects of central freeways using newly-digitized historical employment data (Section 4.3). Also, our calibrated model takes into account the location of jobs (Section 5). There, we do not find evidence of significant local effects of freeways on employment or productivity.

Third, we have said less about housing prices and housing supply. In the simplified model, housing prices follow population, following equation 8. But if housing supply was literally fixed amidst declining demand (Glaeser and Gyourko, 2005), then we would be unable to detect disamenities, because declining demand would register only in prices.[14] However, the depreciation of housing over 60 years likely mitigates this concern. We find similar results for housing and land prices compared with our results for population (Section 4.4, Appendix D.8). We also find that land rents implied by the full calibrated model closely predict appraised land prices (Section 5). These results are consistent with elastic housing supply, despite declines in demand.[15]

4 Reduced-form evidence

4.1 Data

We use data from multiple sources to test the simplified model prediction that freeway disamenities lead to more negative local effects in central versus outlying neighborhoods. That is, conditioned on distance to the city center, a central neighborhood next to a freeway declines more compared with one farther from a freeway.

One, we use a consistent-boundary census tract panel for 64 U.S. metropolitan areas between 1950 and 2010. Since tract boundaries change over time, these data are normalized to 2010 boundaries using area weights, or, in later years, block population weights. Metropolitan areas are core-

[14] The era of Interstate construction coincided with large overall declines in demand for living in central cities from non-freeway factors, e.g., increasing incomes (Margo, 1992; Kopecky and Suen, 2010); declines in quality of life (Cullen and Levitt, 1999; Collins and Margo, 2007); and changes in racial composition (Boustan, 2010).

[15] If local housing supply elasticity was positive but greater near freeways, then we might over-estimate the negative local effects of freeways.

based statistical areas as defined in 2010. Our analysis uses the 64 metropolitan areas with tract-level measures in 1950.[16] Census tables report tract population and housing characteristics in each census year. We compute each tract's distance to the city's center, a point in space defined using the 1982 Census of Retail Trade (Fee and Hartley, 2013).[17] We also spatially match tracts to natural features such as coastlines, lakes, rivers, and slope (Lee and Lin, 2018).

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). We also use information on the opening dates for each Interstate highway segment, up until 1993, from the PR-511 database compiled by the Federal Highway Administration. These data allow us to construct a time-varying measure of tract proximity to the expanding Interstate highway network. Most freeways were constructed between 1956 and 1969, the period originally authorized by the Federal-Aid Highway Act of 1956.

These and other data are described in Appendix C and elsewhere. In addition to the Yellow Book plan maps, we digitized or re-discovered several other databases. We digitized the 1947 Interstate plan and historical routes of exploration at a within-city spatial scale for our instrumental variables analysis (Section 4.2.) We also digitized summary data and rediscovered microdata from historical travel surveys conducted in 1950s Chicago and Detroit to estimate the effects of freeways on job growth and the barrier effects of freeways (Sections 4.3 and 6.3 and Appendix J.)

4.2 Evidence from population growth

We divide our 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. The median sample tract is quite close to a freeway: in the first bin, over three-quarters of tracts are within 1 mile of a freeway, and virtually

[16]These 64 metropolitan areas contained one-third of the total U.S. population in 2010. See Lee and Lin (2018) for details about the tract database, based on Manson et al. (2019) and Logan et al. (2014).

[17]Holian (2019) compares alternative measures of city centers and concludes that the 1982 Census is "probably the best measure of the [CBD] concept."

all tracts are within 2.8 miles of a freeway (Figure D.1). Even for tracts more than 10 miles from city centers, half are within 1 mile of a freeway.

We find that freeways have negative local effects in central neighborhoods but less negative or even positive local effects in outlying neighborhoods. Figure 3 summarizes these patterns for census tracts in all 64 sample metropolitan areas. Each line shows kernel-weighted local polynomial smoothed values of the 1950–2010 change in the natural logarithm of tract population against distance to a freeway. To account for variation in metropolitan growth, changes are centered around their metropolitan area means. Each line ends at the 99th percentile tract by distance to the nearest freeway. Fewer observations at greater distances to a freeway lead to larger standard errors.

These plots confirm the simplified model predictions. Population declined near city centers and increased in suburban areas following freeway construction. For neighborhoods within 5 miles of 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.

These patterns are evident in alternative visualizations. Figure D.2 shows a map of population changes in Chicago between 1950 and 2010. Large declines in population can be seen close to constructed freeways near the city center. Figure D.3 shows mean population changes across our 64-metro sample in 1- by 1-mile bins by distance to the nearest freeway and distance to the city center. This figure shows that results in Figure 3 and our subsequent regression analysis do not depend on our chosen discretization of distance.

Next, we can more formally analyze the patterns shown in Figure 3 with regression:

$$\Delta n_{g[m]} = \alpha_m + \beta_1 d_{Fg} + Z_g' \gamma + \varepsilon_g. \tag{10}$$

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_{Fg} is the distance from the neighborhood centroid to the nearest freeway, and Z_g is a vector of controls measuring fixed and persistent

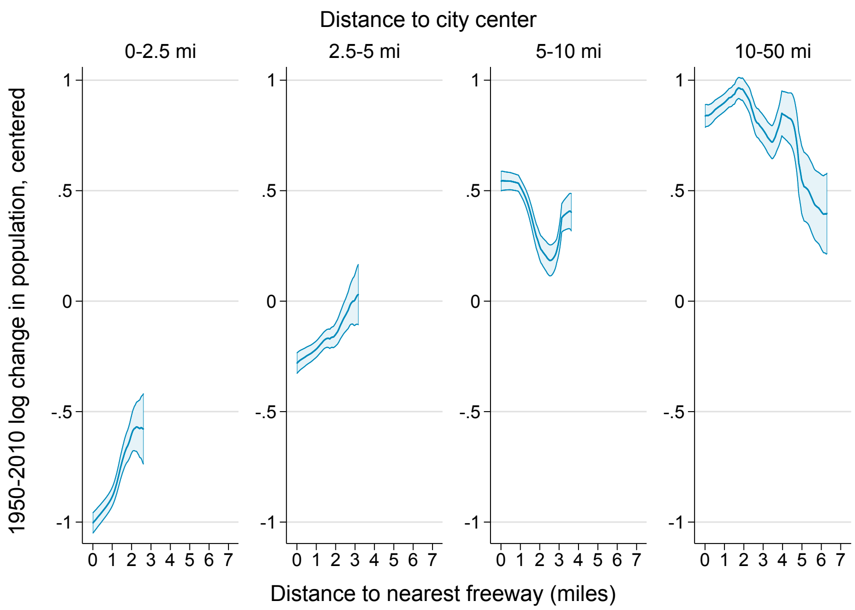


Figure 3: Neighborhoods near freeways declined in central areas and grew in the periphery

This figure shows kernel-weighted local polynomial smoothed values 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 smoothed values for a separate subsample conditioned on distance to the city center, as indicated by the panel titles. Smoothed values use Epanechnikov kernel with bandwidth 0.5 and local-mean smoothing. Shaded areas indicate 95% confidence intervals. Each smoothed-value line ends at the 99th percentile consistent-boundary tract by distance to the nearest freeway in 2010.

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 our four subsamples binned by distance to the city center. This flexible specification allows us to test whether the local effects of freeways vary by centrality, with weak parametric assumptions. A positive estimate $\hat{\beta}_1 > 0$ means that all else equal, neighborhoods farther from the freeway experienced higher population growth; i.e., freeways had negative local effects. Through the lens of the simplified model, 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 10.[18] Each column is a separate regression. The coefficient estimates have the expected sign and are precisely estimated. The coefficient on miles to freeway can be interpreted as the additional growth in log 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 27% 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 from the city center, tracts closest to freeways increased more in population compared with tracts farther from freeways.[19]

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 conditioned on distance to the city center noted in the column title and zero distance to the nearest freeway. Thus, population in freeway tracts within 2.5 miles of city centers declined by half, 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 indicators for average tract slope. The estimated coefficients on freeway proximity are similar when including these controls. (See Ta-

[18] 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.5 for similar results without weights.

[19] Of the 64 metropolitan areas in our sample, 38 have tracts beyond 10 miles. The changing metropolitan sample could account for the less-negative estimate in the fourth versus third column.

Table 1: Local effects of freeways on population

	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 ($\bar{\alpha}_\cdot$)	-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)
(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
Montiel Olea-Pflueger (F)	10.2	8.0	2.9	3.4
Sargan J test (p)	0.726	0.125	0.813	0.576

This table shows WLS and IV estimates. 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}—$p < 0.01$. WLS estimates reported in panel (b) include controls for neighborhood proximity to nearest park, lake, seaport, river, coastline, and city center in miles, and three categories indicating average neighborhood slope. IV estimates reported in panel (c) include controls and use four plan and historical route instruments. See Tables D.1 and D.2 for complete set of estimates.

ble D.1 for a complete set of estimates.)

Of course, freeways were not allocated randomly to neighborhoods. The main concern is if freeways were routed through neighborhoods with inferior growth potential, perhaps to aid struggling places.[20] This would provide an alternative explanation for our results.

We use both planned routes and historical routes as instruments, following the literature that estimates the causal effects of freeways and 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 freeway. The goal of the 1947 plan was to improve travel *between* distant cities and national defense (Baum-Snow, 2007; see Figure D.4). 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 planned-freeway instrument is determined by the number and orientation of nearby large metropolitan areas. For example, the north-south orientation of I-35 through Austin, Texas, is determined by the Austin's location compared with Dallas (north) and San Antonio (south), rather than neighborhood factors.

We also construct 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; the variant instrument shifts this route westward and northward.

We also use neighborhood proximity to historical routes as instruments. Identification relies on the premise that historical 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 Na-

[20] Existing evidence on this margin, 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 freeways. Other studies (Baum-Snow et al., 2017, Garcia-Lopez et al., 2015) suggest the opposite. This evidence is not directly comparable, as it comes from different time periods and countries. Our analysis also departs from earlier studies in that we consider the allocation of freeways to small geographic units—census tracts—compared with municipalities or metropolitan areas.

tional Atlas (U.S. Geological Survey 1970), and historical railroads in operation by 1898 by Atack (2015).[21] We re-digitized the plan and explorer route maps for this project. Baum-Snow (2007) and Duranton and Turner (2012) use cross-metropolitan area variation, so their instruments contain insufficient spatial detail for our analysis.

Table 1c shows instrumental variables estimates using all four instruments together. The estimation includes the same control variables as the specification reported in Table 1b. Table D.2 shows estimates using the planned and historical routes as instruments separately.

The IV estimates suggest that observed neighborhood declines near central freeways understate the negative causal effect of freeways. The IV and WLS estimates reveal qualitatively similar patterns: the negative local freeway effects (positive coefficients) estimated for city centers attenuate with distance to the city center. However, the IV estimates are larger, especially for neighborhoods closest to the city center. Whereas the WLS estimates imply that downtown neighborhoods adjacent to a freeway decline 27% more compared with a downtown neighborhood 1 mile from a freeway, the IV estimates imply that freeways caused downtown neighborhoods adjacent to freeways to decline 143% more than a downtown neighborhood 1 mile from a freeway. (In outlying areas, the IV estimates suggest the freeways also caused negative effects, although the standard errors are large and do not exclude zero or even positive effects.) The inflation of the IV estimates suggests that the causal effect of freeways is larger (more negative) than what simple growth rates suggest. In other words, freeways were generally allocated to neighborhoods that had high growth potential. 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 may have plowed through dense, mature neighborhoods and had very negative effects.

To test for weak instruments, we report the effective F-statistic of Montiel Olea and Pflueger (2013). For the two subsamples within 5 miles of city centers, where the simplified model predic-

[21] 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.

tions are sharpest, the effective F-statistics are 10.2 and 8.0. The former exceeds the rule-of-thumb critical value of 10 (Andrews et al., 2019), but both fall short of the Montiel Olea and Pflueger (2013) 10% critical values of 13.7 and 11.8, respectively. The historical routes alone appear to be stronger instruments. Table D.2 reports larger effective F-statistics for specifications using historical route IVs alone. This could be because the historical route instruments describe actual routes within cities. In contrast, the 1947 plan routes end at the metropolitan fringe, so the routing within cities is less well predicted. (The planned route IVs alone appear to be weak instruments.) Finally, we also test the overidentifying restrictions by reporting p-values from a Hansen (1982) test. We fail to reject the null hypothesis that the full set of instruments is valid. Taken together, weak instruments do not appear to be a major challenge for inferring large local declines near freeways in central neighborhoods.

We acknowledge uncertainty around the IV estimates. Our takeaway is that the primary selection concern—that freeways were routed through neighborhoods expected to decline—is inconsistent with the IV results. In addition, narrative evidence suggests that freeways were likely allocated to neighborhoods with more growth potential. Urban freeways, particularly in city centers, were built along previously less-developed and less-dense "corridors" left behind by previous radial development patterns. These patterns were codified by the American Association of State Highway and Transportation Officials (AASHTO) in the 1957 "Red Book."[22] This routing through undeveloped corridors is corroborated by our analysis in Appendix A.

4.3 Null effects of freeways on central neighborhood job growth

So far, we have inferred freeway disamenities from negative local effects of freeways in central versus suburban neighborhoods. If firms endogenously choose neighborhoods, then population declines may instead reflect increased competition from firms. For example, the growth of large employment centers near *suburban* highways seems to reflect improved productivity rather than

[22] "[M]ost cities have land areas outside the central core that lend themselves to the location of new highways. 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" (AASHTO, 1957, p. 89).

decreased amenity (Garreau, 1991). However, we find little evidence that increases in productivity or firm demand near *central* freeways confound our interpretation.

A challenge for evaluating productivity effects is obtaining historical neighborhood data. One possibility is data on employment. However, standard modern databases such as the Economic Census or Unemployment Insurance records suffer from poor industry and spatial coverage in the early 1950s. Instead, we use historical household travel surveys to identify job locations in the 1950s. These surveys record trip characteristics for a reference day or period, such as trip origins and destinations (latitudes and longitudes), the purpose of each trip, the mode of travel, and the time spent traveling.[23] We measure job locations by using trip destinations with the stated purpose of going to work.

We use surveys conducted in the Detroit metropolitan area in 1953 (Carroll, 2017) and the Chicago metropolitan area in 1956 (State of Illinois et al., 1959).[24] Estimates of jobs from these travel surveys match aggregates reported by other sources (see Appendix C). For modern estimates of jobs by census tract, we use the Census Transportation Planning Product (CTPP) from 2000 for Chicago and the 1994 Detroit travel survey (A.M.&P.G., 1995).

Table 2 shows regressions of the 1956–2000 (Chicago) and 1953–1994 (Detroit) change in tract employment on miles to a freeway and controls as in Table 1b.[25] The Chicago results show slower job growth near downtown freeways, and faster job growth near suburban freeways, although the estimates are not precise. Thus, we cannot reject null effects on jobs. The Detroit results are mixed. The OLS estimates suggest faster job growth near downtown freeways and suburban freeways, but the IV estimates suggest that freeways caused slower job growth near central freeways. Again, the estimates are imprecise, so we cannot reject null effects. Overall, we find little evidence that

[23] Also called "trip diary" or "origin-destination" surveys. Modern versions include the National Household Travel Surveys and the Census Transportation Planning Products.

[24] 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. For Chicago, we digitize summary information on employment by sector and zone, a small geographic unit unique to the travel survey, from Sato (1965).

[25] We visualize patterns of long-run population and job growth for census tracts in the Chicago metropolitan area in Figure D.10. We aggregate the tracts into two categories because of smaller sample sizes and to facilitate presentation. Table D.3 replicates population regressions for only Chicago and Detroit and show similar results compared with Tables 1b and 1c.

Table 2: Local effects of freeways on employment

	Chicago		Detroit	
	Distance to city center:		*Distance to city center:*	
	0–5 miles	5–28 miles	0–5 miles	5–21 miles
(a) OLS				
Miles to freeway	0.091	-0.059[b]	-0.315	-0.119
	(0.210)	(0.025)	(0.595)	(0.123)
(b) IV				
Miles to freeway	0.086	-0.082	0.960	0.723[b]
	(0.264)	(0.153)	(1.438)	(0.308)
Kleibergen-Paap LM test (p)	0.000	0.000	0.139	0.000
Montiel Olea-Pflueger (F)	50.7	8.2	3.7	9.7
10% critical value	14.4	11.6	13.2	10.6
Sargan J test (p)	0.000	0.021	0.024	0.066

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 include controls as in Table 1b.

central freeways caused local negative effects by attracting jobs.

4.4 Other evidence

In Appendix D, we discuss additional evidence identifying freeway disamenities. Our population growth 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 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 tend to be desirable regional destinations as they provide production benefits (job centers tend to be coastal) and consumption benefits (views, beaches, and moderate temperatures all complement recreational activities). Locations far from the coast may benefit more from freeway access, while locations near the coast would experience mostly the freeway disamenity. We find similar results: freeways have large negative local effects 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 (within 10 years) effects of freeways on population. These short-run effects are most negative for freeways completed in the 1950s and 1960s. In Section 2, we discussed narrative evidence 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 negative causal effects estimated with instrumental variables.

We also consider freeway effects 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 than the suburbs. These results again suggest the importance of freeway disamenities. 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. Data availability is a challenge; reliable historical estimates of housing and land prices for small geographies are scarce. In particular, reported housing prices from the 1950 Census of Population and Housing suffer from two defects: (i) the universe is owner-occupied units in single-unit structures, which tend to be scarce in downtown neighborhoods, and (ii) there are no available measures of house 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. Compared with our results for population or income, there is no clear pattern for house prices with respect to distance to the city center. This pattern could reflect unmeasured house size or quality.

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), we find that land values grew slower near freeways in central Chicago.[26] In outlying areas beyond 5 miles from Chicago's center, land

[26] An exception is that land values in tracts more than 3 miles from the nearest freeway in the intermediate city center bin—2.5 to 5 miles from Chicago's center—grew slower still. These data were generously shared by Gabriel Ahlfeldt and Dan McMillen.

values tended to grow faster near freeways.

Finally, Floberg (2016) documents corroborating evidence on land use in downtown Bridgeport, Connecticut. 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.

5 Quantitative model of freeway disamenities

We calibrate our full model that considers the joint location decisions of employment and population to quantify freeway disamenities. Externally calibrated parameters, along with tract population, employment, land area, and commute times from the Chicago metropolitan area in 2000, allow us to recover neighborhood amenities and productivities. We use these to estimate freeway disamenities.

5.1 Data and calibration

We use data on tract employment, worker population, land area, and tract-to-tract commute times from the 2000 CTPP for the Chicago metropolitan area. 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 Appendix A.

Imputing travel times. We use the CTPP to estimate commute times for tract pairs. To address missing data, we use a two-stage local adaptive bandwidth kernel estimator (see details in Appendix E). Our method uses observed times to nearby tracts from the same origin to predict commute times. Our results are robust to other methods, including alternative parameterizations of the kernel estimator and a separate regression imputation method that uses origin and destination fixed effects, distance, and controls for travel direction. In cross-validation, our imputed times predict observed times. Regression imputation leads to slightly larger estimates of the welfare effects of freeway disamenities, so our preferred imputation method produces conservative results.

Our preferred method has several virtues. One, we do not impute commute times using distances. Other methods may misrepresent the effects of freeways, since freeways affect commute times, conditioned on distance. Instead, our method nonparametrically accounts for unobserved features not captured by fixed effects and distance, including congestion. Two, by using times instead of flows, we avoid the problem of inferring infinite travel costs from missing tract-to-tract commuting flows (Dingel and Tintelnot, 2020).

Calibrated and estimated global parameters. We calibrate values for four global parameters. (Later, we explore sensitivity to these selections.) Using estimates from the literature, we set the consumption share to $\beta = 0.94$ (Brinkman, 2016; Davis and Ortalo-Magné, 2011; Davis and Palumbo, 2008) and the labor share in production to $\alpha = 0.97$ (Brinkman, 2016; Ciccone, 2002; Rappaport, 2008). Following Baum-Snow et al. (2019), we estimate the product $\varepsilon\kappa$ by regressing commute flows on commute times and origin and destination fixed effects. This regression follows from equation 1 and yields $\widehat{\varepsilon\kappa} = 0.019$ (95% CI [-0.0191, -0.0183]). Finally, we set $\kappa = 0.005$, which implies that the value of time spent commuting is approximately the wage rate (Van Ommeren and Fosgerau, 2009; Small, 2012). This is also the value used by Baum-Snow et al. (2019). In turn, this implies a value $\hat{\varepsilon} = 3.8$. This is between the estimates of 2.2 by Severen (2021) and 6.8 by Ahlfeldt et al. (2015). Since there is a wide range of estimates of ε and little consensus on its interpretation, we explore sensitivity to this value in subsequent analysis.

Estimated neighborhood productivity and amenity. Next, we estimate neighborhood productivities and amenities $\{A_k, B_j\}$. 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 ($d_{jk} = e^{-\kappa\tau_{jk}}$) (see Appendix F).[27] Recovered amenity values B_j are shown in Figure F.1. This map shows higher amenity neighborhoods located north of downtown, especially along Lake Michigan, and also throughout the suburbs.[28]

Our approach prioritizes intuition and transparency regarding identification. We acknowledge

[27] We choose land area, population, and employment because they are precisely measured quantities. The model could also be calibrated using land values, house prices, or wages.

[28] Note that we abstract from residential sorting. If higher income residents consume more land and sort into higher amenity neighborhoods, then we would underestimate the total variation in neighborhood amenities. See Appendix D.7 for discussion.

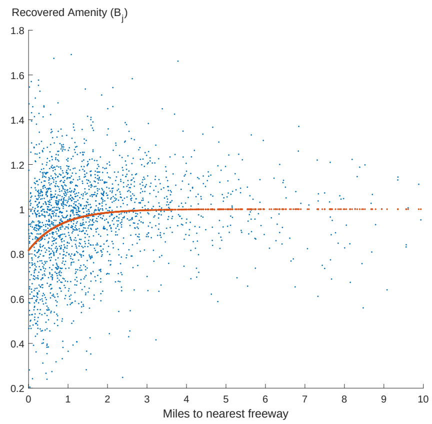
uncertainty surrounding the estimates of neighborhood amenities. We explore sensitivity to parameter choices in Section 5.2. In addition, oversimplification in quantitative spatial models may lead to biases in estimation or quantification. For example, Dingel and Tintelnot (2020) show that the granularity of travel data can cause uncertainty, while Severen (2021) shows that workplace amenities and origin–destination pair fixed factors can affect estimation results.

Validation with land prices. We validate the model and calibration using data on land values. While current data are not available for the entire Chicago metropolitan area, we can use appraised land values from the City of Chicago in 1990 (Ahlfeldt and McMillen, 2014 and 2018) to check if the model-generated rents predict observed prices. A regression of the natural log of land values on the natural log of model rents yields a coefficient of 0.75 (95% CI [0.68, 0.81]) and an R^2 of 0.38. The model-generated rents are highly predictive of observed prices, and the two series have similar variance. This result adds credibility to the calibrated model and allays concerns that features omitted from the model could confound the positive relationship between prices and population. See details in Appendix G.

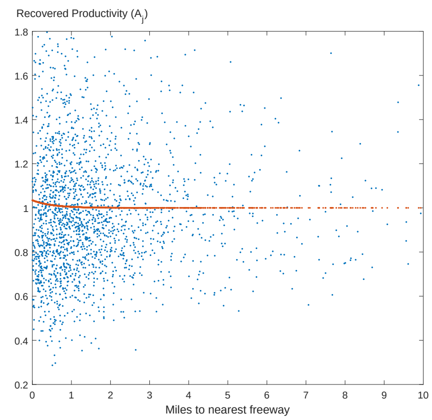
5.2 Freeway disamenity estimates

We estimate the freeway disamenity function (equation 5) using nonlinear least squares and neighborhood amenities B_j recovered from the calibrated model. We fit the function in levels, which is a consistent estimator of the parameters. (Alternatively, fitting the function in logs would require dropping zeros.) 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 4a shows recovered amenities for each tract versus distance to the nearest freeway. For our baseline calibration, we estimate that freeway neighborhoods have 18.4% inferior amenities ($\hat{b}_F = 0.184$), attenuating by 95% at 2.4 miles from the freeway ($\hat{\eta} = 1.24$).

These estimates complement the evidence in Section 4. There, freeway disamenities were identified by the dynamic local effects of freeways on population in central versus outlying neighborhoods. Here, freeway disamenities are identified from cross-sectional variation. Neighborhoods near freeways feature superior job access (i.e., low commuting times) but also low residential populations. The model infers that these freeway neighborhoods feature low quality of life. Inter-



(a) Amenity



(b) Productivity

Figure 4: Neighborhood amenity and productivity near freeways

Panel (a) shows recovered amenities B_j (blue) versus distance to the nearest freeway and a fitted disamenity function (red). Panel (b) shows the recovered productivities 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 asymptotically approaches 1.

estingly, the spatial scale of these estimates conforms with (i) local dynamic effects extending up to 3 miles from central freeways (Figure 3) and (ii) barrier effects for trips up to 3 miles (Section 6.3).

There is little correlation between freeway proximity and productivity. We estimate a quantitatively small 2% effect on productivity, attenuating by 95% 1.4 miles from the freeway (Figure 4b). These estimates are not statistically significant. Taken together with the results in Section 4.3, 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 3 shows baseline estimates in the top row, with subsequent rows showing sensitivity to calibrated parameters, fixing $\varepsilon\kappa = 0.019$. Amenity parameter estimates are significant and positive for all specifications. The Frechet parameter ε plays an important role in the estimates. For larger values of ε, the estimates of the disamenity are smaller. This relationship is mechanical, as for larger values of ε, smaller variation in amenities is needed to rationalize the data.

The last two columns of Table 3 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. A one

Table 3: 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
Baseline									
0.005	0.94	0.97	3.8	0.184	0.012	1.237	0.125	0.240	0.767
Robustness									
0.003	0.94	0.97	**6.3**	0.119	0.012	2.198	0.311	0.174	0.683
0.007	0.94	0.97	**2.7**	0.247	0.013	0.910	0.080	0.308	0.801
0.005	**0.91**	0.97	3.8	0.169	0.016	1.935	0.263	0.252	0.669
0.005	**0.97**	0.97	3.8	0.214	0.009	0.749	0.057	0.235	0.910
0.005	0.94	**0.98**	3.8	0.186	0.012	1.240	0.124	0.241	0.774
0.005	0.94	**0.96**	3.8	0.183	0.012	1.235	0.126	0.240	0.761

This table shows the estimates of freeway disamenity parameters b_F and η, varying calibrated parameters. Top row shows baseline estimates.

standard deviation increase is equivalent to a 24% increase in amenity value. The sensitivity of the coefficient of variation is similar to the parameter estimates: 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. In the baseline, the freeway disamenity is equivalent to a 0.77-standard deviation decrease in the overall neighborhood amenity distribution. This relative contribution of freeways to amenities is robust to calibration choices. In other words, changing ε affects the entire distribution of estimated neighborhood amenities, but not the contribution of freeways to those amenities.

These estimates likely understate the disamenity effects of freeways. In Section 4.2 and Appendix A we discuss statistical and historical evidence from across the U.S. that freeways were allocated to neighborhoods with high growth potential. In Appendix H we show results using instrumental variables. The IV estimates are slightly larger, but quantitatively similar. These results suggest that parameter estimates in Table 3 are conservative, and that we will likely understate the welfare and decentralization effects of freeway disamenities.

6 Counterfactual simulations

6.1 Effects of mitigating freeway disamenities

To understand equilibrium and welfare effects of freeway disamenities, we use our calibrated quantitative model to simulate a counterfactual policy. 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. This policy is similar to real-world policies such as Boston's Big Dig that mitigate disamenities by burying freeways.

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 city center (8.7% of the total metro working population) and 1,156,779 working residents living in the city of Chicago (28%). The policy simulation results are shown in Table I.2 for our baseline calibration and for alternative calibrations. The utility values and centralization measures are expressed as ratios relative to the baseline before mitigation.[29]

The aggregate expected utility gains from disamenity mitigation are 5.6%. While the magnitude is large, note that burying all freeways in the Chicago metropolitan area is a massive and costly policy intervention. Depending on calibration choices, estimated welfare gains range from +2.1% to +9.9%, with the results being most sensitive to the choice of ε.

There is also a large centralization effect from disamenity mitigation. Population grows 20.3% within five miles of the city center, at the expense of outlying areas. In the city of Chicago, population grows by 8%. The centralization result is robust, with increases in population ranging from 5.9% up to 8.8%. 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

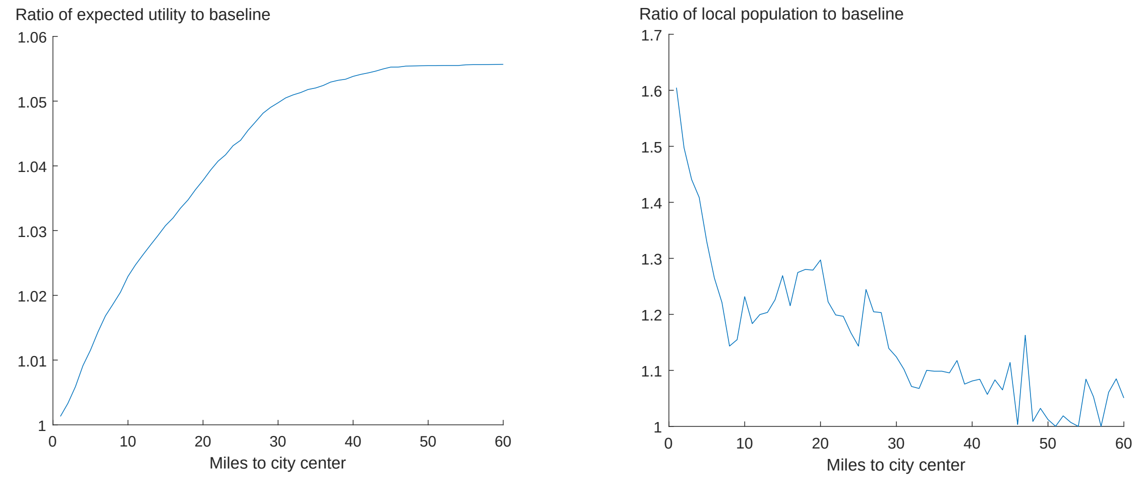
[29] Figure I.1 shows changes in population density under the counterfactual policy using our baseline parameters. There are large gains in population near freeways, especially in high-amenity neighborhoods on the north and northwest sides of Chicago.

have been 25% higher had freeways not been constructed. Thus, freeway disamenities were a quantitatively important factor in suburbanization.

Finally, welfare gains from mitigation are concentrated in central neighborhoods. We consider a policy where mitigation is only implemented in neighborhoods within a certain distance of the city center. Figure 5a shows 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, with little additional benefit from capping freeways beyond 30 miles from the city center. Next, Figure 5b shows effects on neighborhood population when neighborhoods *unilaterally* mitigate the freeway disamenity. We eliminate freeway disamenities only for neighborhoods at a given distance to the city center and report population growth 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%. However, if mitigation were only applied for locations between 10–11 miles from the city center, population gains would be smaller, around 20%. Generally, the benefits of mitigation decline with distance to the city center. These results provide insight into why the freeway revolts were concentrated in central neighborhoods and why support for mitigation today is often observed in central neighborhoods.

6.2 Benefits versus costs of disamenity mitigation

How do the benefits of freeway disamenity mitigation compare with costs? Boston's Big Dig cost $15 billion, but in addition to burying 1.5 miles of freeway through the city center, included the construction of a new 3-mile section of freeway and a tunnel under the Boston Harbor (Flint, 2015). 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. Nonetheless, a number of completed and proposed projects give insights into the magnitude of construction costs. For example, in Denver, an approved project that includes removing an existing 1.8 mile elevated freeway and a number of additional initiatives is priced at $1.2 billion (Murray, 2017). In Atlanta, a proposal to cap a 0.5 mile section of an already below-grade freeway has an



(a) Mitigation up to x miles from city center

(b) Mitigation at x miles from city center

Figure 5: Effects of mitigation by distance to city center

Panel (a) shows the effect on expected utility relative to the baseline for a policy that mitigates all disamenities within a given radius. Panel (b) shows the effect on local population relative to the baseline for a policy that mitigates the disamenity only at a given location.

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 5.6%, which corresponds to \$16.2 billion per year. This intervention would require mitigating 781 freeway miles, thus providing a benefit of \$20.7 million per mile per year. Using a discount rate of 7%, this suggests a lifetime benefit of \$296 million per mile, somewhat lower than the cost estimates above.[30]

Given the concentration of mitigation benefits in central neighborhoods, we 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 1.15%, or \$3.3 billion per year. However, this intervention requires miti-

[30]This is the discount rate recommended by the Federal Highway Administration. Rates used by States are often lower. Thanks to Jordan Riesenberg for identifying an error in calculating freeway mileage in the working paper version of this article.

gating only 30 miles of freeway, implying a benefit of $111 million per mile per year, or a lifetime benefit of $1.6 billion per freeway mile. This exceeds the costs estimates above. Thus, projects targeted to central neighborhoods that retrofit existing freeways could provide net benefits.

6.3 The role of barrier effects in freeway disamenities

We estimate that short trips up to 3 miles between neighborhoods severed by freeways decline by 20% and increase in travel time by 1–3 minutes. We incorporate these estimates in our quantitative model to estimate the contribution of barrier effects to the overall disamenity value of freeways. Mitigating barrier effects alone increases expected utility by up to 3%, or about half of the total gains from mitigating all freeway disamenities.

Freeways may reduce local quality of life through other channels. We do not attempt to separately model or account for noise or pollution effects. However, note that we identify a larger spatial scale for freeway disamenities compared with extant estimates of noise or pollution effects. Alternatively, land use exclusion could be an important channel. However, we find that land use is not a major contributor to reduced local quality of life (see Appendix K). Freeways cover roughly 0.5% of total land area in the Chicago metropolitan area, and only 2% of land use in central Chicago. Given the small share of land devoted to freeways, it is unsurprising the land use exclusion is a small part of the overall disamenity value of freeways.

Estimating barrier effects. Our main estimates of barrier effects use the Detroit travel surveys from 1953 and 1994 (Section 4.3). Using origin and destination latitudes and longitudes, we construct a panel of travel flows and times between census tract pairs. 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 equation 1. It differs in that travel flows vary over time, constant terms are subsumed into fixed effects, and origin-destination fixed effects affect travel flows.

$$\pi_{jkt} = \rho_{jt} \varsigma_{kt} \upsilon_{jk} e^{\mu \tau_{jkt}}. \tag{11}$$

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, and origin-destination fixed effects (υ_{jk}) capture pair-specific characteristics that are time invariant, such as pair distance and fixed transportation infrastructure. The parameter $\mu = -\varepsilon\kappa$ is the semi-elasticity of commuting flows with respect to travel times. We assume that travel times τ_{jkt} are a function of distance and the freeway 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 after 1953 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 less than a threshold distance Δ. (The PR-511 data measure which freeway segments opened to traffic between 1953 and 1994.) This expression omits the effects of distance and other fixed infrastructure as they are absorbed by origin-destination fixed effects, but it allows the effects of new freeways to vary by tract-pair distance. At short distances, this could be because of increased congestion or detours from fewer cross-freeway arterials. At long distances, benefits from increased speeds along freeways likely exceed any local disruptions to surface streets.

We use the Poisson pseudo-maximum likelihood (PPML) estimator by Correia et al. (2019) to estimate equation 11. A virtue of PPML is that the maximization of the likelihood function associated with equation 1 is numerically equivalent to a logit estimator (Guimarães et al., 2003). A second virtue is that it handles zeros appropriately. Appendix J describes our estimation method and results in detail. We perform separate estimations varying Δ to flexibly account for freeway effects that vary by trip distance.

Figure 6a 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% confidence intervals using standard errors clustered on origin–year, destination–year, and origin–destination pairs. The estimated parameter combines both the change in travel costs after the tract pair is "treated" with an intersecting freeway (v_1) with the response of trip demand (μ). Each connected point shows a separate estimate, 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% decline in the volume of trips between 1953 and 1994. Most

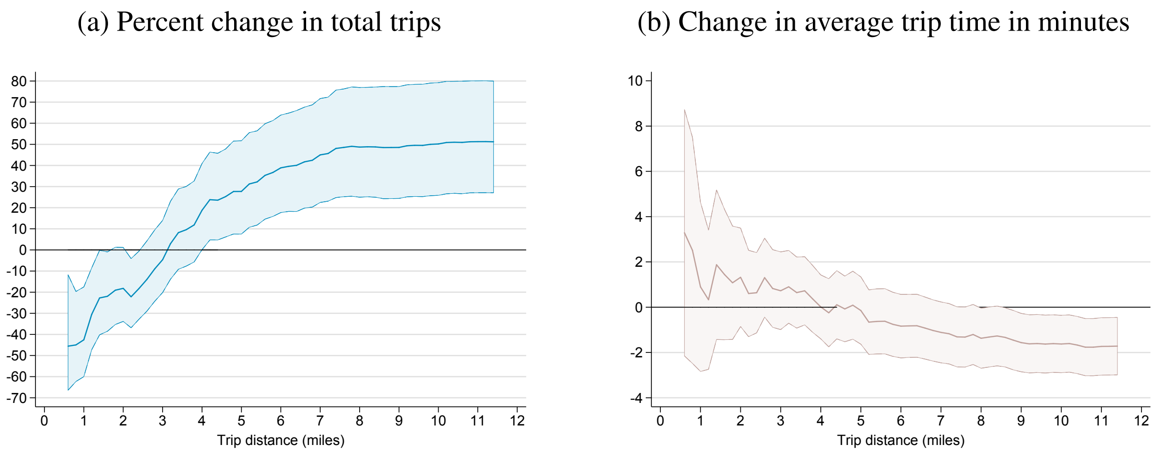


Figure 6: Effect of freeway crossing on volumes and times of trips

These panels display (a) PPML and (b) OLS 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, as indicated on the horizontal axis. The estimations include origin–destination, origin–year, and destination–year fixed effects. 90% confidence intervals shown.

trips are 2.5 miles or less and about a quarter of trips are shorter than 1 mile, so these effects are quantitatively important (see Table C.5.) In contrast, trips up to 6 miles crossing freeways are associated with increases in travel volumes of about 33%. Over longer distances, freeways that intersect tract pairs may offer a faster route than 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 6b. Note that unlike the estimates of changes in trip volumes reported in Figure 6a, the estimates in Figure 6b are using a restricted sample of tract pairs with a strictly positive number of households reporting trip times. If tract pairs with larger increases in trip times are more likely to have zero reporters, then we will under-estimate the time barrier effects of freeways. 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. The point estimates are imprecise, but they are consistent with the changes in travel flows shown in Figure 6a.

Freeway routes may have been selected to divide neighborhood pairs where travel flows were expected to fall. 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 J, we provide additional details and results, including estimates using binned distances. We also estimate barrier effects using cross-sectional data from Chicago in 2000. This regression does not include origin-destination fixed effects, given that the sample is a cross section. Using the Chicago cross-section, we estimate similar barrier effects (up to 1.6 minutes) but over larger distances (up to 8 miles).

Quantifying the role barrier effects. Next, we quantify the contribution of barrier effects to the overall disamenity value of freeways. The first step is to model access to local amenities. We use the specification for residential externalities developed by Ahlfeldt et al. (2015). Instead of modeling the freeway disamenity as an exponential decay function, consumption spillovers depend on proximity and population density of nearby areas. The amenity of a location j is

$$B_j = b_j \left(\sum_{j'=1}^{J} e^{-\rho \tau_{jj'}} \left(\frac{N_{Rj}}{L_j} \right) \right)^{\chi}, \tag{12}$$

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.[31] 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 overall amenities B_j as before. We then decompose overall amenity into an exogenous component b_j and an endogenous component using equation 12.

Barrier effects reduce amenities by increasing travel times $\tau_{jj'}$, thus reducing access to consumption amenities nearby.[32] We can write this as $\tau_{jj'} = \tau^*_{jj'} + c_{b,jj'}$, where $\tau_{jj'}$ is the observed travel time between locations, $\tau^*_{jj'}$ is the travel time without a freeway, and $c_{b,jj'}$ is the barrier

[31] Some formulations of residential spillovers use population as opposed to density. Census tracts vary in land area. Using density controls for land area and mitigates concerns about overstating spillover effects.

[32] Freeway barrier effects may operate not only through increased travel times but also through reduced origin-destination pair amenity. For example, a walking commute might be less amenable after the construction of a freeway because of noise, pollution, or safety concerns. If this origin-destination pair amenity channel is important, then our counterfactual simulations will underestimate the total benefits from removing barrier effects on both time and amenity.

cost after the freeway is built. Based on our estimates, we set $c_{b,jj'} = 2$ minutes for trips less than 3 miles that cross a freeway. Next, we use the calibrated model to quantify the magnitude of these barrier effects. We remove the barrier cost $c_{b,jj'}$ for short trips. Then, we recalculate the equilibrium and estimate the effect on both expected utility and decentralization.

We find that barrier effects are quantitatively important, accounting for about half of the total disamenities from freeways. Recall our baseline estimate was that total mitigation increased expected utility by 5.6%. When only barrier effects are removed, expected utility rises 2.9%, about half of the effect of total mitigation. In addition, population within 5 miles of the CBD increases 14%, compared with 20% for total mitigation. Population in the city of Chicago increases by 5.2%, compared with 7.9% for total mitigation. Thus, barrier effects alone may have played a large role in suburbanization. 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 Appendix L.) An implication of these results is that mitigation policies that do *not* address barrier effects are unlikely to significantly improve quality of life.[33]

We also evaluate the direct effects on neighborhoods within 2 miles of a freeway. Under total mitigation, rents increase by 4.7% and population increases by 8.3% in neighborhoods near freeways. Interestingly, these rent effects are quantitatively similar to the estimated effects on housing prices by Cervero et al. (2009) of replacing the Embarcadero and Central Freeways in San Francisco with surface boulevards.[34]

7 Conclusions

The freeway revolts were *prima facie* evidence of the importance of freeway disamenities, especially in central neighborhoods. Our results suggest that there were large disparate spatial effects

[33] In Table K.1, we show the effect on employment decentralization. In general, the effects on job location are minimal, with only a slight increase in employment near the center of the city.

[34] Both freeways were short stubs—about 1,400 and 2,000 feet, respectively—that terminated at surface streets, so their replacement probably had minimal effects on commuting times. Thus, they provide a comparable benchmark. Cervero et al. (2009) estimate that housing prices next to the former freeways increased about 7–13% following freeway replacement, and this effect attenuated to zero at around 2 miles from the former freeway corridor.

and welfare costs associated with freeway disamenities, particularly in central cities. Freeway disamenities, versus commuting benefits, likely played a significant role in the decentralization of U.S. cities. Targeted policies that bury highways in city centers could provide net benefits. Mitigating barrier effects seems especially important.

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 neighborhood development.

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A Appendix: More evidence from building the Interstates

A.1 Timeline of policy changes

Table A.1 summarizes key federal policy changes that affected the allocation of urban Interstates as described by DiMento and Ellis (2013) and Mohl (2008).

Table A.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.

A.2 Building the Interstates in Washington, DC

Figure A.1 illustrates an example of changes in highway allocation 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 canceled altogether in northwest and northeast D.C. There is also historical evidence of significant opposition to new freeways in these areas.

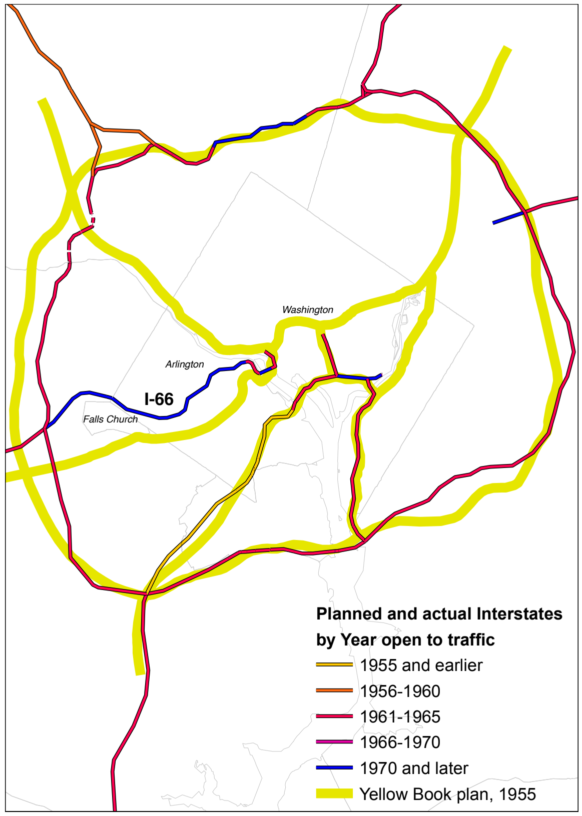


Figure A.1: Some highways deviated from initial 1955 plans or were cancelled

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.

A.3 The changing allocation of freeways

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

$$1(f_{g[m]t}) = \alpha_{mt} + Z_g' \beta_t + X_g' \gamma_t + \varepsilon_{gt} \tag{13}$$

where $1(f_{gt})$ is an indicator for whether tract g intersects a freeway by year t.[35] 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

[35]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.

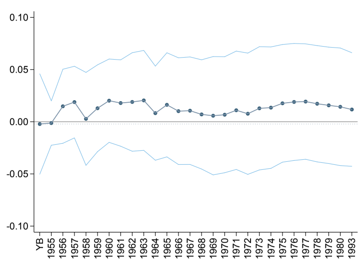
to the city center and for average slope. We also include a vector of initial tract characteristics measured in 1950 (X_g) which includes population density, education, race, income, housing prices and rents, and housing age. These characteristics are standardized to have mean zero and standard deviation 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 13 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.

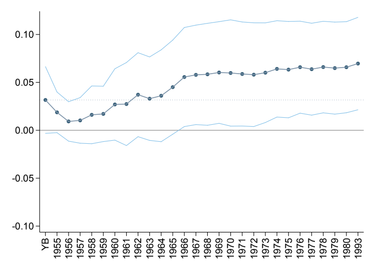
Figure A.2 shows estimates for selected regressors of interest from 28 year-by-year regressions.[36] The vertical axes measure the estimated coefficient of interest ($\hat{\beta}_{it}$). For the linear probability model, the coefficient can be interpreted as the increase (or decrease) in probability associated with a one-unit increase in the regressor indicated by the panel title, conditioned on the other regressors. 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 1950. (Coefficient estimates for other factors are reported in Table A.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 A.2a 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

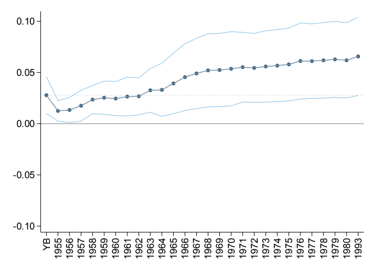
[36] Table A.2 displays estimation results for the Yellow Book of 1955 and the completed Interstate network as of 1956, 1960, 1970, and 1993. By 1993 about 95 percent of the eventual mileage had been completed. Table A.3 displays estimates from a corresponding logistic regression, with similar results.



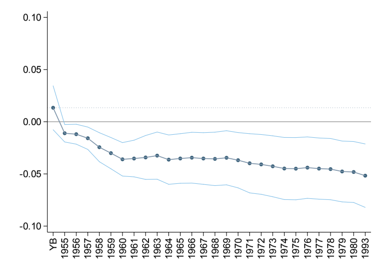
(a) Indicator for proximity to coast



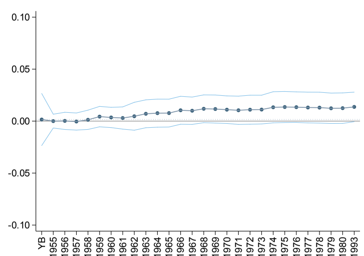
(b) Indicator for proximity to river



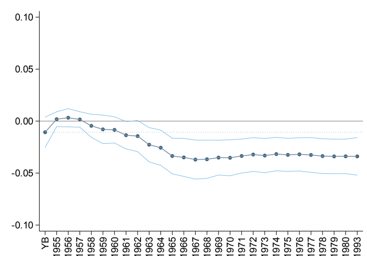
(c) Indicator for proximity to pre-1898 rail



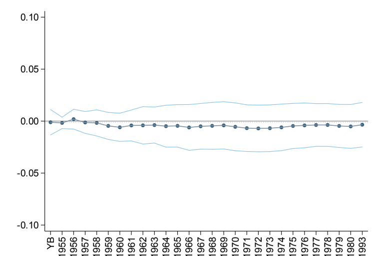
(d) 1950 population density



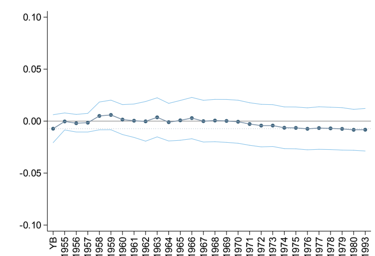
(e) 1950 population share Black



(f) 1950 population 25+ share college graduate



(g) 1950 median household income



(h) 1950 median house value

Figure A.2: 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 A.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.

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 A.2b shows that freeway construction became more likely near rivers through the mid-1960s. Figure A.2c 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 A.2d 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 4.

Figure A.2e 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 A.2f 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 A.2g and A.2h 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.[37]

In sum, freeway planning and construction evolved in response to the growing revolts of the

[37] 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 4.4 for details.

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.

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

	Yellow Book 1955	*Interstate highway open by:* 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

This table shows OLS estimates of equation (13). 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 A.3: Freeway factors: Logistic regression estimates

	Yellow Book 1955	*Interstate highway open by:* 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 A.2. See notes to Table A.2.

A.4 Additional narrative evidence

Planners had an immature understanding of the negative side effects of cars and limited-access roads in cities. For example, a 1924 plan for Detroit showed superhighways with a "'parkway' ambiance [...] 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 Bureau of Public Roads (BPR) 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 ease congestion and revitalize struggling downtowns. Lewis Mumford, later an important critic of urban freeways, initially "viewed the automobile as a beneficent liberator of urban dwellers from the cramped confines of the industrial city" (DiMento and Ellis, 2013, p. 38).

In personal correspondance, we asked what factors might have determined the priority order of Interstate construction. Weingroff (2016) replied:

> After the Federal-Aid Highway Act of 1956, the clock started ticking. BPR and the State highway officials believed they had to finish the entire 41,000 miles (the amount designated at the time) within the 13-year funding framework. BPR did not prioritize rural or urban routes or tell State highway agencies when each route should be built. Once the routes were designated, each State decided when to build their segments. Many States opted initially for the rural mileage, which was much easier to build because the routes did not involve extensive disruption of homes, businesses, and neighborhoods. Moreover, many State highway agencies were primarily rural oriented and had little experience with construction in cities, so they had a learning curve to overcome. Initially, in 1955-1956, no 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, with the expectation being that Interstate Construction funds would be available to complete all the segments within the timeframe.

B Appendix: 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.[38] 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:

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

[38] 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 4.

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:

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

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:

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

C Appendix: Data

C.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[39]. 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.[40]

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 C.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.

[39]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.

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

Table C.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			✓
	Tucson			✓
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	✓	✓	✓
MS	Jackson			✓
MT	Butte			✓
	Great Falls			✓
NC	Durham		✓	
	Greensboro		✓	
NE	Lincoln			✓
	Omaha	✓	✓	✓
NH	Manchester			✓
NJ	Camden	✓	✓	✓
	Trenton	✓	✓	✓
NY	Albany			✓
	Buffalo	✓	✓	✓
	Kingston			✓
	New York	✓	✓	✓
	Rochester	✓	✓	✓
	Schenectady			✓
	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			✓

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

C.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.

C.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), who 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. These data were generously shared by Nate Baum-Snow.

Table C.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)
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.

C.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., AASHTO, 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). Fifty metropolitan areas have both 1950 tract data and a Yellow Book map.

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.

C.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$\times$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, 2018) and McMillen (2015).

C.6 Chicago and Detroit travel surveys

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.

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 "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 to 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 responsible 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 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

Table C.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]	5-county[e]	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 Chicago Area Transportation Survey (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.

than our sample area)[41]. 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 (Table C.4). 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)[42]. The 1950 Census of Population also reported 983 thousand jobs reported by residents of Wayne County.

Table C.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

[41]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.

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

Table C.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.

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.[43]

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

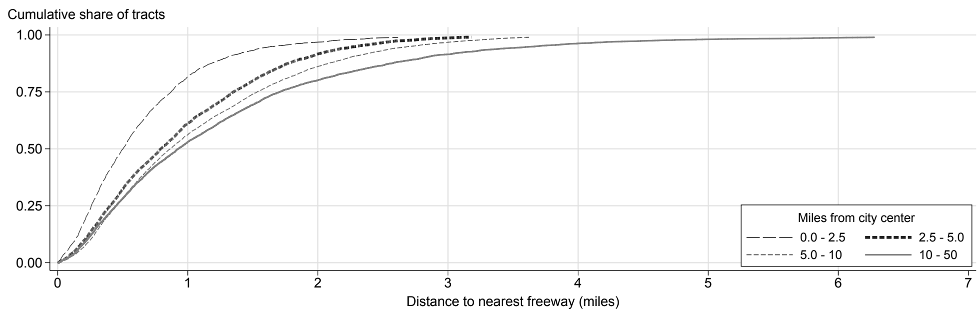
Table C.5: Summary statistics, 1953 and 1994, Detroit Metropolitan Area Traffic Study (DMATS)

	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

D Appendix: Other evidence from population, income, prices, land use, and jobs

D.1 Empirical cumulative distribution of neighborhood distance to freeway

Figure D.1: Cumulative distribution of neighborhood distance to freeway



This figure shows the empirical cumulative distribution of census tracts by distance to the nearest freeway and distance to the city center.

D.2 Changes in neighborhood population in Chicago

Figure D.2 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 to have experienced larger population increases than those farther away.[44]

[44] 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.

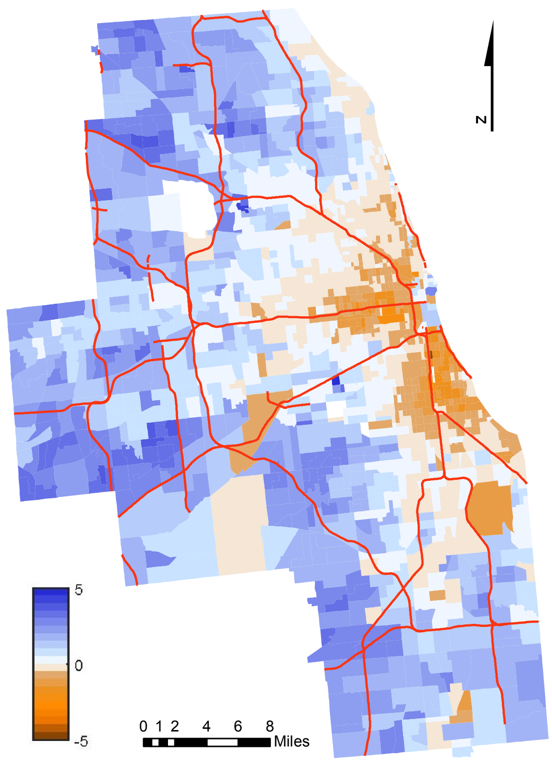


Figure D.2: Central neighborhoods declined in population, especially near freeways

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.

D.3 Heat plot of changes in neighborhood population

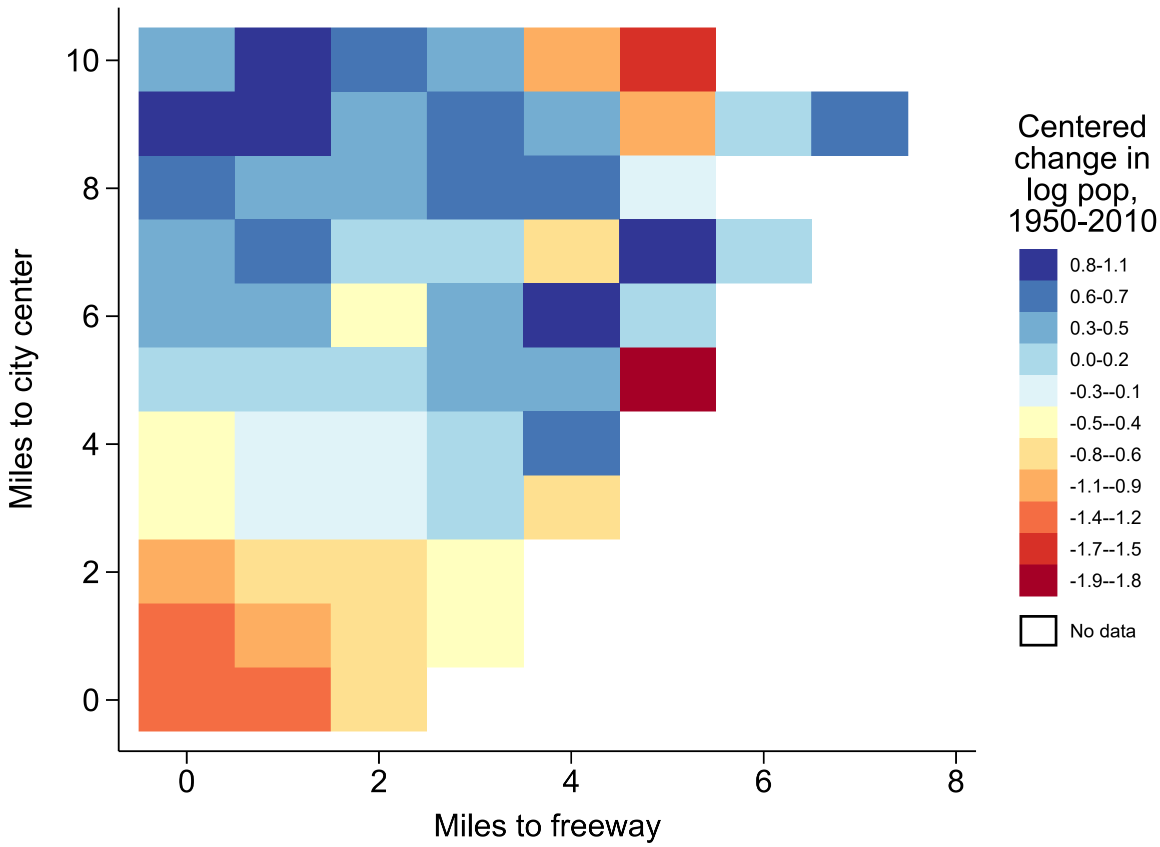


Figure D.3

This heat plot shows 1950–2010 mean changes in the centered natural logarithm of population for consistent-boundary census tracts in 64 metropolitan areas, in 1-mile wide bins. Sources: NHPN, NHGIS.

D.4 1947 Interstate plan

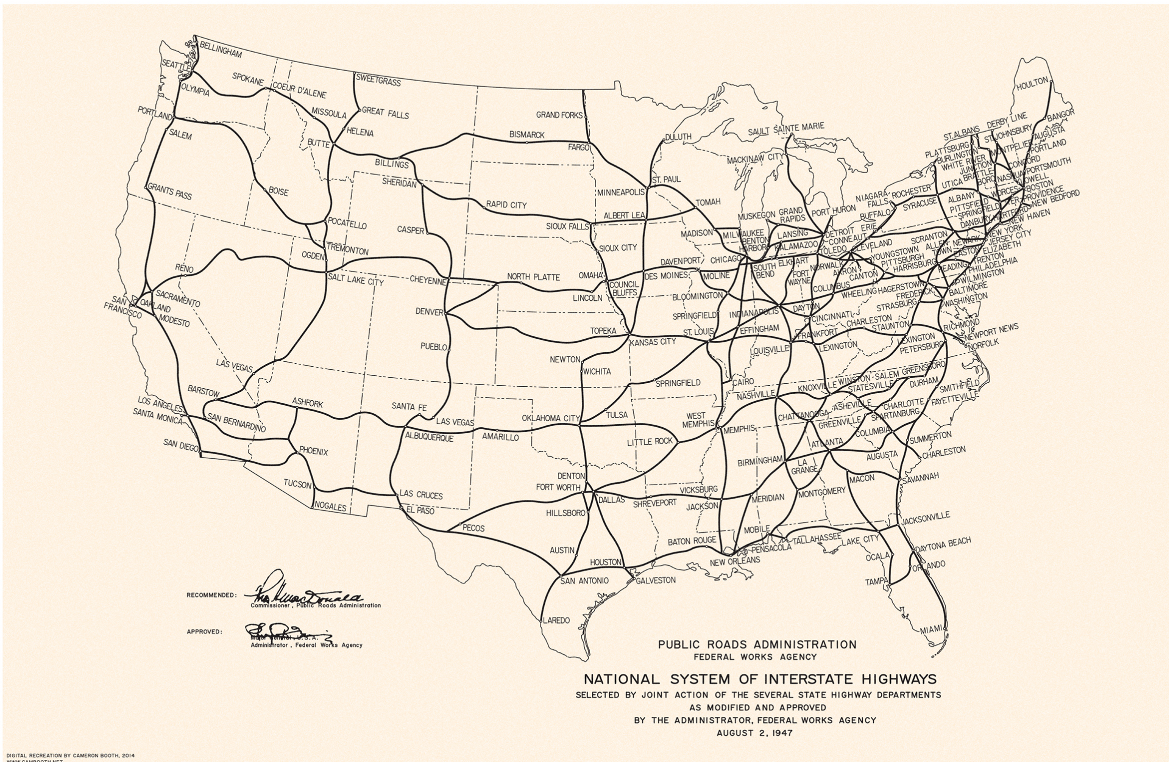


Figure D.4: 1947 highway plan

D.5 Robustness of population results

Table D.1 reports WLS estimates of equation (10), with controls for natural and historical factors. The estimated coefficients on miles to nearest freeway are also reported in Table 1, panel (b).

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 freeway	0.165^c	0.076^b	-0.205^c	-0.062
	(0.059)	(0.031)	(0.071)	(0.042)
Miles to city center	0.303^c	0.293^c	0.223^c	0.028
	(0.043)	(0.039)	(0.037)	(0.023)
Miles to nearest park	0.187	0.149^b	0.078	-0.170
	(0.118)	(0.059)	(0.048)	(0.109)
Miles to nearest lake	-0.021	0.014	0.012	0.014
	(0.023)	(0.012)	(0.013)	(0.012)
Miles to nearest port	0.041	0.033^a	0.054^b	0.005
	(0.040)	(0.017)	(0.025)	(0.028)
Miles to nearest river	0.018	-0.009	0.032	0.022
	(0.041)	(0.031)	(0.032)	(0.033)
Miles to nearest coastline	-0.041	-0.025	-0.044^a	0.013
	(0.044)	(0.017)	(0.022)	(0.021)
Average slope between 0 and 5 degrees	-0.585^c	-0.350^c	-0.055	0.599^a
	(0.123)	(0.103)	(0.206)	(0.308)
Average slope between 5 and 10 degrees	-0.339^c	-0.221^b	0.039	0.626^b
	(0.110)	(0.109)	(0.208)	(0.282)
Average slope between 10 and 15 degrees	-0.063	-0.035	-0.328	0.389
	(0.095)	(0.105)	(0.288)	(0.264)
R^2	0.149	0.119	0.122	0.062
Neighborhoods	2,312	3,482	5,561	5,173
Metropolitan areas	64	63	56	38

This table shows WLS estimates of equation (10). 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}—$p < 0.01$.

To illustrate the robustness of our main results, Figure D.5 reports coefficient estimates for other specifications. The baseline IV results reported in Table 1, panel (c) are shown in red on the left side of each panel. (The diamond 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 city center.

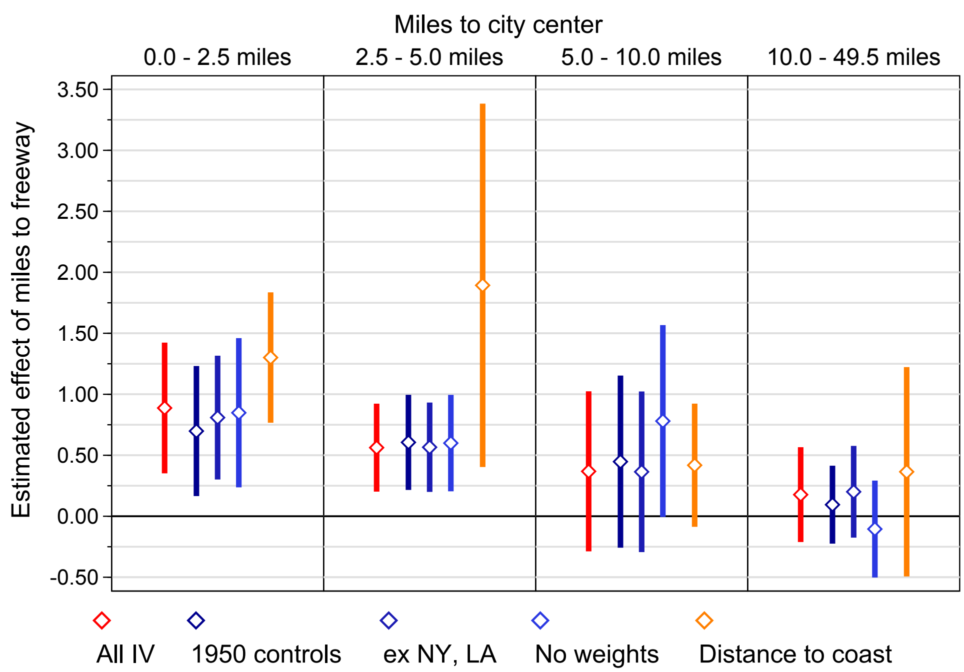


Figure D.5: Robustness of freeway effects on population

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.

Up to this point, we have considered only the access benefits of highways for commuting to the city center. However, this same analysis could apply to other regional level destinations. The fifth line in each panel of Figure D.5 reports coefficient estimates where the sample of neighborhoods is conditioned on distance to the nearest coastline instead of distance to the city center.[45] 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 city center. 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.

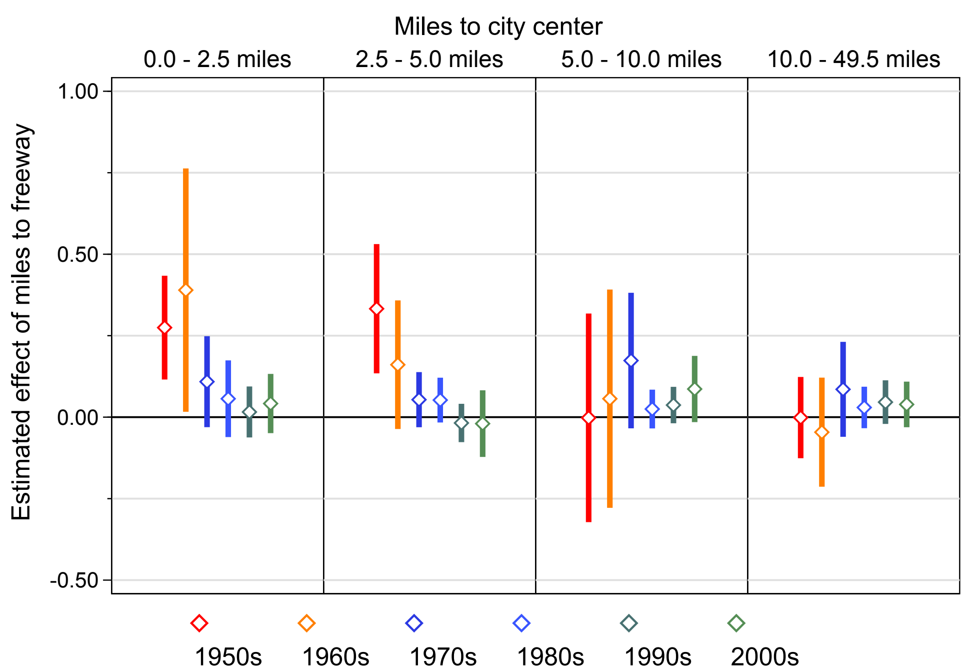


Figure D.6: Freeway effects on population largest in the 1950s and 1960s

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 tim-

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

ing of interstate construction. In this exercise we regress change in population in each decade on distance to the city center 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 includes only 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 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.6 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.6 IV estimates

Table D.2: Instrumental variables 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
Montiel Olea-Pflueger (F)	2.5	5.9	3.4	2.4
10% critical value	7.3	7.4	7.2	4.6
Sargan 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
Montiel Olea-Pflueger (F)	14.2	9.4	2.3	5.6
10% critical value	5.7	5.4	11.4	9.0
Sargan 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
Montiel Olea-Pflueger (F)	10.2	8.0	2.9	3.4
10% critical value	13.7	11.8	14.2	13.6
Sargan 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 (c). 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}—$p < 0.01$.

D.7 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.7 illustrate the effect of highway proximity on the relative change in income, separated by distance to the city center. Neighborhoods farther from highways had larger income growth, and this effect was somewhat larger near the city center. 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 city center. In addition, particularly near the city center, 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 city center. 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 mech-

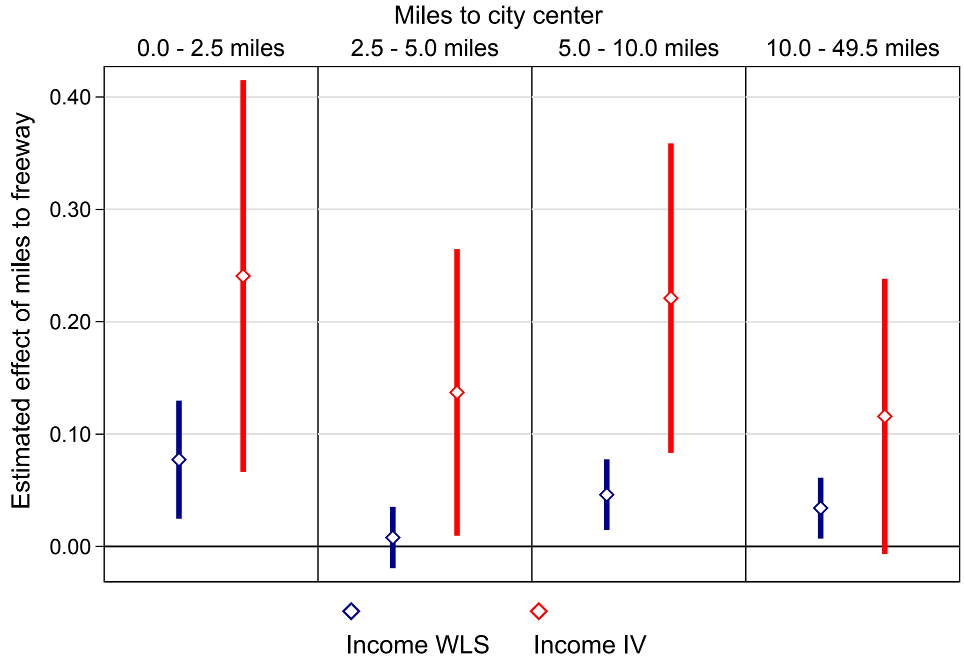


Figure D.7: Incomes increased more farther from freeways

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.

anisms and analysis by Lee and Lin (2018).

D.8 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 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.[46]

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.8. 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.

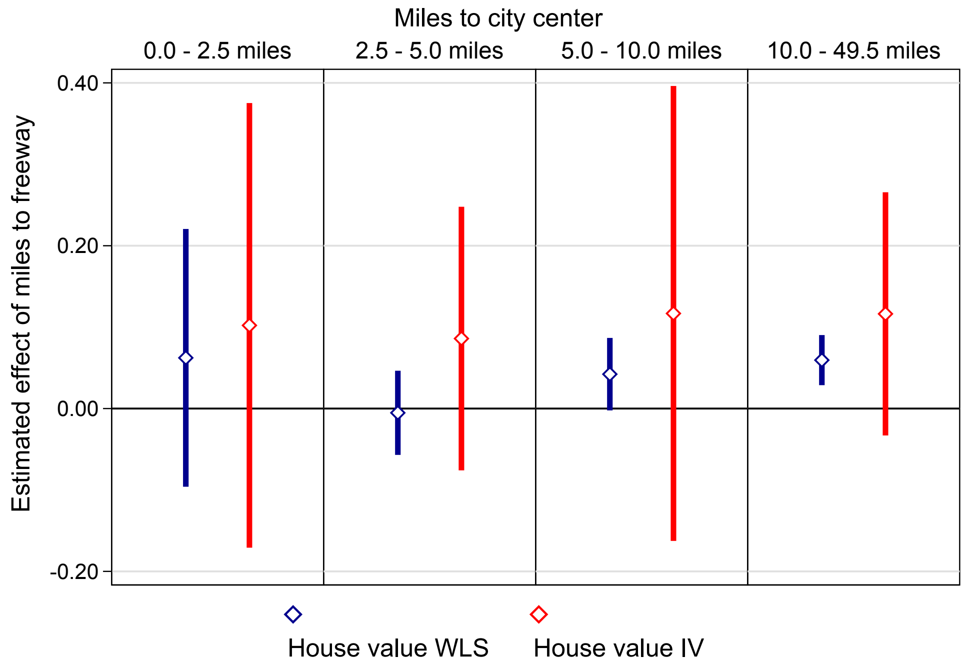


Figure D.8: House prices increased more farther from freeways

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.

To provide further evidence in light of the limitations of the census house-price data, we turn to

[46]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.

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.9.[47] Here the patterns are more clear compared with census housing prices. In 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.

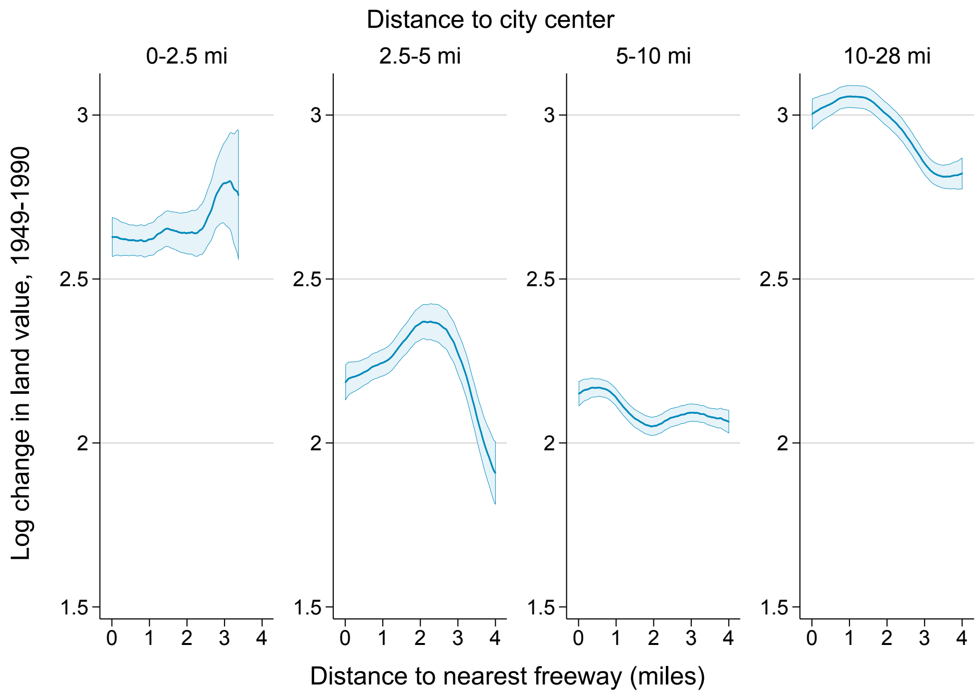


Figure D.9: Land value growth in Chicago, 1949–1990

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.

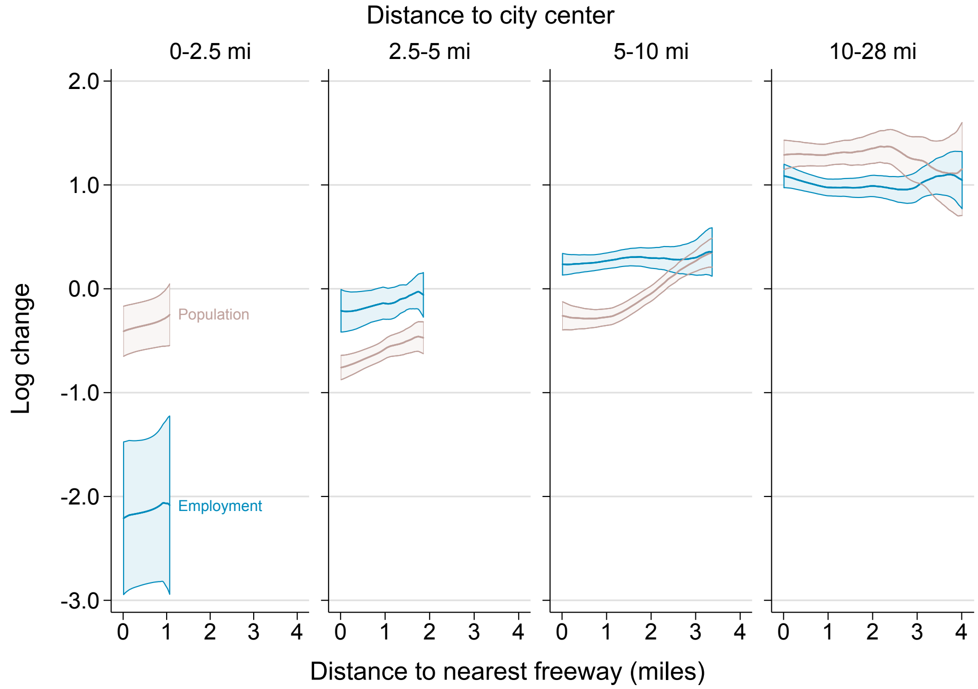


Figure D.10: Changes in population and employment in Chicago

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.

D.9 Changes in employment in Chicago

Figure D.10 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 D.2 and is similar to the pattern observed across all U.S. cities seen in Figure 3. 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.

[47]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.

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 D.10 does not support the hypothesis that increases in firm demand caused by freeways displaced households in central areas.

D.10 Changes in population in Chicago and Detroit

	Chicago		Detroit	
	Distance to city center:		*Distance to city center:*	
	0–5 miles	5–28 miles	0–5 miles	5–21 miles
(a) Change in population – OLS				
Miles to freeway	0.384^c	-0.014	0.095	0.015
	(0.078)	(0.031)	(0.151)	(0.036)
Neighborhoods	263	1,101	105	425
(b) Change in population – IV				
Miles to freeway	0.119	-0.120	0.463	0.358^c
	(0.098)	(0.168)	(0.351)	(0.106)
Kleibergen-Paap LM test (p)	0.000	0.000	0.031	0.000
Montiel Olea-Pflueger (F)	50.7	8.2	5.2	12.7
10% critical value	14.4	11.6	12.3	10.6
Sargan J test (p)	0.004	0.000	0.194	0.000

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

E Appendix: Imputing 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. To impute missing values, we use a two-stage local adaptive bandwidth kernel estimator. 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.

The method is based on a Gaussian kernel density estimator that works much like a moving average. If travel times are missing for a origin-destination pair, then the weighted average of travel times to nearby destinations from the *same origin* is used to impute the missing values. For example, if commute times from tract A to tract C are missing, but location B is near location C, then the missing commute time from A to C will be similar to the observed commute time from A to B. The weights are calculated using a two-dimensional Gaussian kernel and decline with distance between destinations. The algorithm is adaptive in the sense that it uses a first-stage to determine the sparsity of observed travel times around a destination and adjusts the kernel bandwidth accordingly.

This imputation method nonparametrically accounts for unobserved characteristics of origins, destinations, and the paths between them. This is important in our application where we need to account for the transportation network and other geographic features as well unobserved characteristics including congestion given the urban setting. Thus we believe this method has advantages over linear regression-based imputation or fast marching algorithms. The details of the imputation method follow.

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:

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

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:

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

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.

We tested the sensitivity of our results to underlying assumptions of our kernel estimator. We also tried a separate regression imputation method using origin and destination fixed effects, distance, and controls for travel direction. Our final results are robust to assumptions and methods.

F Appendix: Estimating neighborhood amenities

The following equations allow for the recovery of tract productivities A_k and amenities B_j. Rewriting equation 2, we solve for wages paid at each location iteratively:

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

Next, we use land market clearing and land demand by firms and workers (equations 3 and 6) to solve for land rents in each location:

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

We recover neighborhood amenities using wages and rents and combining equations 1 and 4:

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

Finally, profit maximization and zero profits yield neighborhood productivity:

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

Recovered amenity values B_j in the Chicago metropolitan area are shown in Figure F.1, with colors representing quantiles. The map shows higher amenity neighborhoods located north of downtown, especially along Lake Michigan, and also throughout the suburbs. This map also helps clarify the sources of identification for neighborhood amenities. The amenities are derived from a combination of density and job access. For example, neighborhoods near the central business district have similar access to jobs, but large variation in estimated amenities, despite all being relatively more dense than suburban locations.

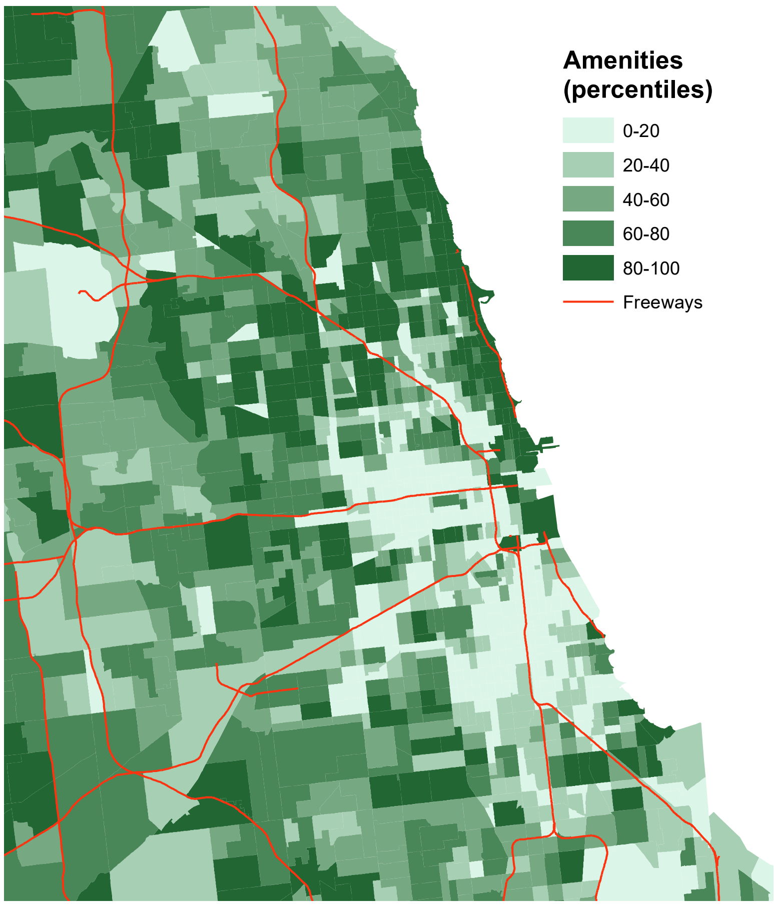


Figure F.1: Estimated neighborhood amenities in Chicago

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.

G Appendix: Validating model rent predictions

We calibrate the model using data only on population, land area, travel times and commuting flows. With these data we can recover neighborhood amenities and productivities. However, the model also makes predictions about land rents, and prices are often used in the literature to measure amenities. Unfortunately current data for land rents are not available for the entire Chicago Metropolitan Area. However, we can use the data on appraised land values for the city of Chicago in 1990 described in Appendix C to validate assumptions of the model.

Figure G.1 shows a plot of the land values versus the rents outputted by the model for census tracts in the city. We take the natural log of both variables and subtract out the mean to normalize the data. The regression line and equation are also included in the figure. The model rents are highly correlated with an R squared value of 0.38. The coefficient of 0.75 (95% CI [0.68, 0.81]) suggests that the model rents are slightly more dispersed than land values.

This result supports the validity of modeling assumptions by showing that the model is able to predict land prices of neighborhoods quite well despite not using data on prices directly in the estimation. The series are highly correlated and the fact that the coefficient is close to one is evidence that the model is correctly predicting the variation in prices.

Note that the ratio of values to rents need not be constant given that values are dependent on tax rates, interest rates, and expectations about appreciation, which may vary by neighborhood. This is confirmed by previous studies including Gyourko, Mayer, and Sinai (2013) and others that show that price-to-rent ratios can be correlated with location characteristics and price levels. Also, the data used for the model come from 2000 while the land value data are from 1990.

These results also allay general concerns that population and employment densities might not be linked strongly to rents as is assumed in the model. For example, sorting and heterogeneity may break this connection if high-income workers locate near freeways and consume more land per person. Likewise rents and population may be only weakly correlated if housing supply elasticities are either very large or very small. These are valid concerns that could affect the interpretation of welfare calculations. Nonetheless, the fact that the model-generated rents closely predict the variation and magnitude of land values lends credibility to the model and calibration.

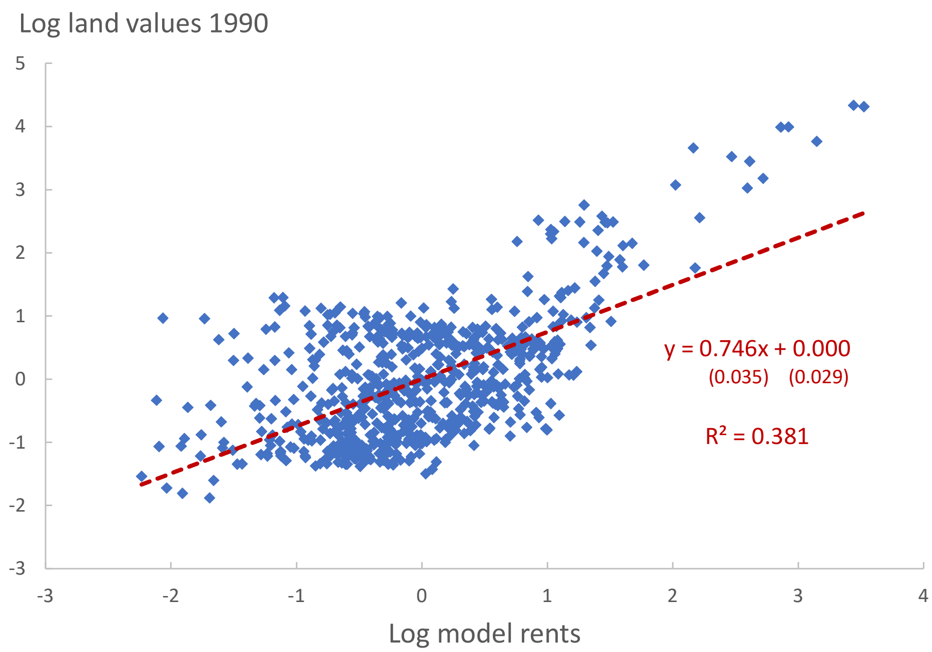


Figure G.1: Correlation of model rents and land values

This figure shows the natural logarithm of appraised land values for the city of Chicago in 1990 plotted against the natural logarithm of rents outputted from the calibrated model using data from 2000. The estimated regression line, coefficients, standard errors, and R^2 value are shown in red. The data are normalized to have a mean of zero.

H Appendix: Instrumental variable estimates of freeway disamenities

In Section 5, 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 H.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 20.3 percent adjacent to a freeway, compared to the 18.4 percent reduction implied by the least squares estimate. In addition, the effect attenuates at a slower rate. The baseline IV estimate of .444, implies that the effect attenuates by 95 percent at 6.7 miles from the freeway compared to the distance implied by the least squares estimate of 2.4 miles.

These results suggest that even accounting for endogenous freeway routing, 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.

Table H.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.*)
0.005	0.940	0.970	3.800	0.184	0.012	1.237	0.125	0.203	0.009	0.444	0.032
0.003	0.940	0.970	6.333	0.119	0.012	2.198	0.311	0.057	0.008	0.543	0.111
0.007	0.940	0.970	2.714	0.247	0.013	0.910	0.080	0.321	0.010	0.396	0.022
0.005	0.910	0.970	3.800	0.169	0.016	1.935	0.263	0.091	0.010	0.463	0.085
0.005	0.970	0.970	3.800	0.214	0.009	0.749	0.057	0.306	0.008	0.414	0.018
0.005	0.940	0.980	3.800	0.186	0.012	1.240	0.124	0.203	0.009	0.446	0.033
0.005	0.940	0.960	3.800	0.183	0.012	1.235	0.126	0.204	0.009	0.442	0.032

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.

I Appendix: Simulated mitigation policy

I.1 Changes in population

Figure I.1 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 on the north and northwest sides of Chicago.

I.2 Results of simulated mitigation policy

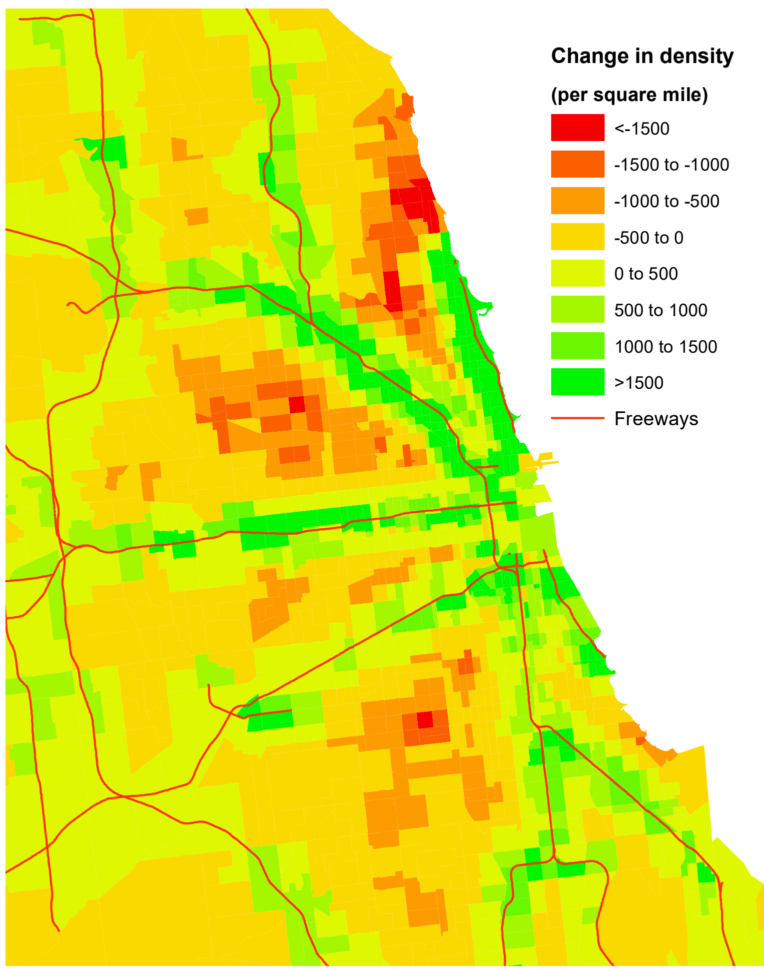


Figure I.1: Change in population density after mitigation of freeway disamenities

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

Table I.1: Results of simulated mitigation policy

κ	β	α	ε	$\mathbb{E}(U)$	pop. <5mi	pop. city
Baseline						
0.005	0.94	0.97	3.8	1.056	1.203	1.079
Robustness						
0.003	0.94	0.97	**6.3**	1.021	1.164	1.059
0.007	0.94	0.97	**2.7**	1.099	1.214	1.088
0.005	**0.91**	0.97	3.8	1.033	1.149	1.054
0.005	**0.97**	0.97	3.8	1.098	1.267	1.113
0.005	0.94	**0.98**	3.8	1.057	1.203	1.080
0.005	0.94	**0.96**	3.8	1.055	1.202	1.079

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.

J Appendix: Barrier effects

J.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 C.5).[48] 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 ($855 \times 855 =$) 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 estimates (Correia, 2015). Table J.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.

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.

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

Table J.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

J.2 Estimating barrier effects

Our main estimates of barrier effects use the Detroit travel surveys from 1953 and 1994 (described in section 4.3). Using origin and destination latitudes and longitudes, we construct a panel of travel flows and times between census tract pairs in 1953 and 1994.[49]

We use the Poisson pseudo-maximum likelihood (PPML) estimator by Correia et al. (2019) to estimate equation 11. A virtue of PPML is that the maximization of the likelihood function associated with equation 1 is numerically equivalent to a logit estimator (Guimarães, Figueirdo, and Woodward, 2003). A second virtue is that it handles zeros appropriately. With 855 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.[50] The PPML estimator assumes a multiplicative error η_{jkt} with $E[\eta_{jkt}|\alpha_t, \rho_{jt}, \varsigma_{kt}, \upsilon_{jk}, \tau_{jkt}] = 1$. Alternatively, one could estimate equation 11 by ordinary least squares after taking logs and assuming an additive i.i.d. error. However, this would restrict the sample to observations with strictly positive

[49] Summary statistics can be found in Appendix C.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.

[50] 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.

flows, leading to selection bias (Santos Silva and Tenreyro, 2006).[51]

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.

J.3 Other estimates of barrier effects

We present alternative estimates of barrier effects. First, compared with the regression results presented in section 6.3, 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 6.3, 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 J.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 6, panel (b). Although this is not precisely estimated, it is consistent with the sharp drop in actual flows shown in Figure 6, 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

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

to the PPML regression estimates shown in Figure 6, panel (a).

Table J.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

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 J.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 6.3 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.

K Appendix: Two different counterfactual experiments

Table K.1, Columnn (2) shows results for the counterfactual experiment described in Section 6.3.

Table K.1: Outcomes of three different mitigation experiments

	(1) Total mitigation	(2) No barrier effects	(3) Land use reclamation
$\Delta\mathbb{E}(U)$	1.056	1.029	1.001
Δ pop. 5 mi from city center	1.203	1.138	1.002
Δ pop. 10 mi from city center	1.077	1.041	1.001
Δ pop., Chicago city	1.079	1.052	1.001
Δ emp. 5 mi from city center	1.002	1.002	1.000
Δ emp. 10 mi from city center	1.001	1.001	1.000
Δ emp., Chicago city	1.001	1.001	1.000
Δ rent 2 mi from freeways	1.047	1.043	1.001
Δ pop. 2 mi from freeways	1.083	1.075	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.

Table K.1, Column (3) shows results for a counterfactual experiment where we account for land used by freeways.

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).[52] We estimate 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.[53]

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 3. 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 K.1 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 K.1. 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.

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

[53] Our exercise may overstate the contribution of land use exclusion, since downtown freeways seem more likely to economize on land.

L Appendix: Sensitivity of barrier effect results

The barrier effect results in Table K.1 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 L.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 L.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	1.029	1.138	1.052
0.144	0.500	3	2	1.017	1.101	1.041
0.144	0.900	3	2	1.039	1.163	1.057
0.100	0.738	3	2	1.020	1.083	1.029
0.200	0.738	3	2	1.044	1.228	1.096
0.144	0.738	2	2	1.015	1.079	1.035
0.144	0.738	4	2	1.040	1.161	1.051
0.144	0.738	3	1	1.010	1.052	1.020
0.144	0.738	3	3	1.064	1.264	1.097

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 6.4 percent in the counterfactual.

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