Highways and Pedestrian Deaths in US Neighborhoods*

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Abstract

Over 100,000 pedestrians have been struck and killed by vehicles on US roadways in the first two decades of the 21st century, representing an alarming public health issue. We examine the US Interstate Highway System's legacy in contributing to local pedestrian deaths using historical Interstate Highway plans as an instrument for local Interstate construction. Operating an Interstate through a census tract increased local pedestrian deaths significantly. Among 17,000 tracts bisected by Interstates, we estimate the average tract experienced 2.5 additional pedestrian deaths between 2001–2020 due to the presence of the Interstate. We find these deaths occur disproportionately in Black communities.

Keywords: Transportation ; Safety ; Traffic Fatalities ; Inequality

JEL classification: I1; H54; R41; R42; R48

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1 Introduction

As the most vulnerable road users, pedestrians face serious risks from vehicle crashes. Between 2000 and 2020 over 100,000 pedestrians died after being struck by a vehicle in the US, representing 13% of road deaths. Societal costs of vehicle collisions include property damage, bodily harm, and death. These costs are spatially concentrated in areas with road infrastructure that enables high vehicle speed and volume. Disadvantaged communities often live near this infrastructure, leading them to bear a disproportionate share of the costs. A history of prioritizing urban highway construction in the US — which raises both the quantity and speed of local vehicles — provides a possible explanation for the high rate of pedestrian deaths occurring close to highway infrastructure and the higher impacts borne by disadvantaged groups.

The federal Interstate Highway System represents an immense investment in US highway construction. In this paper, we study a particular local disamenity created by Interstates: the role of the US Interstate system in causing local pedestrian deaths. While previous research has shown Interstates impact property values (Kilpatrick et al., 2007), produce local pollution (Currie and Walker, 2011; Hoek et al., 2002), and have other negative externalities (Parry et al., 2007), we examine an extreme example of a local cost, pedestrian deaths. We apply an instrumental variable approach, using historical planning documents from the Interstate Highway System, to contribute a causal estimate of the partial effect of a freeway on local pedestrian deaths. We show that, relative to a counterfactual where a particular neighborhood did not receive an Interstate, local Interstate Highways cause a dramatic increase in local pedestrian deaths, both on and near Interstates. We then examine the racial distribution of these deaths, providing evidence that Black residents are more likely to be victims of Interstate induced pedestrian death.

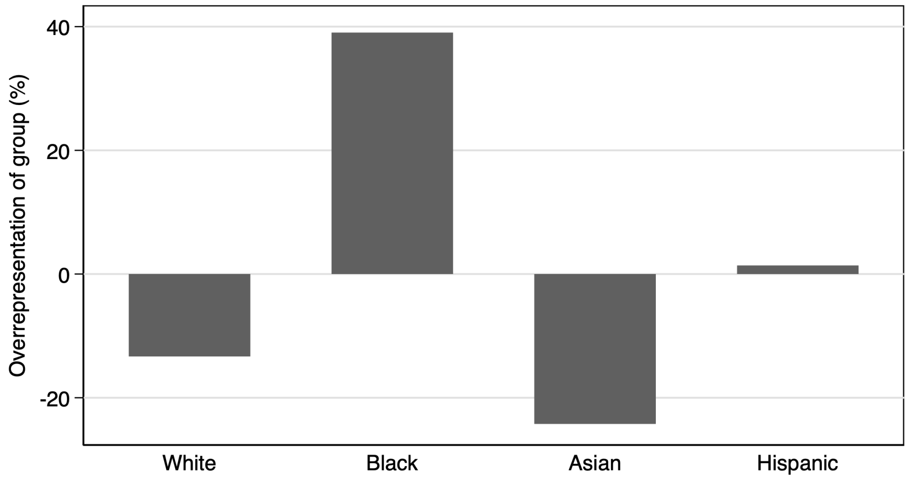
Our empirical strategy makes use of data on traffic fatalities from the Fatality Analysis Reporting System (FARS) for a 20-year period spanning 2001–2020. FARS aims to capture every motor vehicle crash that involves at least one fatality, but no data are available for non-fatal crashes. Using the location of crashes from FARS, we match collisions to the census tracts they occurred in. We then use data from the US Census to determine if an Interstate or other major roadway bisects these census tracts to estimate the effect of the Interstate Highway System on deaths. Of course, the Interstate road network was not randomly allocated over space. The placement of routes was heavily influenced by political and monetary considerations, creating a correlation between local conditions and the presence of an Interstate. As a solution to endogenous route placement, we instrument for the presence of an Interstate in a tract using the 1947 Interstate Highway plan. The historical plan, previously used as an instrument for actual highway construction in Baum-Snow (2007), Baum-Snow et al. (2017), Baum-Snow (2019), and Brinkman and Lin (2022), among others, was created at the national level to advance inter-city mobility as opposed to within-city transportation. Proposed routes in the plan provide an exogenous instrument for contemporaneous routes as they did not consider granular neighborhood demographics or other characteristics. Because Interstates broadly impact a region's transportation network, we focus on local neighborhood effects as opposed to investigating more aggregate impacts of Interstates such as their influence on total pedestrian deaths in the US. We further include contemporaneous controls for tract demographics to determine the extent that observable neighborhood sorting *after* Interstate construction contributes to the causal effect of Interstates on road deaths.

We find that census tracts with an Interstate experienced an additional 2.5 pedestrian deaths between 2001—2020 relative to a counterfactual where the tract did not have an interstate, but the Interstate system was completed elsewhere. We determine that at least 21% of this effect can be attributed to observable neighborhood sorting after Interstate construction by accounting for the role of endogenous Interstate route selection as well as endogenous household and firm sorting. The Interstate-induced pedestrian fatalities appear to be split between collisions on and off Interstates, with a spatial decomposition suggesting Interstates create a dangerous spillover effect up to 400 meters away from the road. We find evidence that changes in the built environment induced by Interstates, like "feeder roads" that funnel traffic to or from Interstate entrances and exits, and the higher vehicle speeds on these local roads drive the increase in pedestrian fatalities that occur off of Interstates. While Interstates likely induce large regional shifts in transportation systems, we find little evidence of treatment spillovers to nearby tracts. We find the causal effect of a local Interstate on the likelihood of dying as a pedestrian is more than twice as large for Black residents as it is for white residents, even after controlling for local demographics. The finding shows Black residents have far higher exposure to pedestrian safety risk and have carried a disproportionate burden of the pedestrian death costs of Interstates.

Our results suggest that, in addition to increasing the number of local pedestrian deaths, freeways contribute to the large gap between white and Black pedestrian deaths. Figure 1 illustrates the extent to which racial groups are over or underrepresented in pedestrian fatalities relative to their share of the US population. White and Asian populations are underrepresented by 13% and 24%, respectively. In contrast, Black populations are overrepresented by 39%. While the US population is 13% Black, 18% of pedestrian death victims are Black. The US road system is not only a significant source of mortality for pedestrians broadly but is systemically more dangerous for Black residents. The US Interstate Highway System, which has disproportionately bisected minority and low-income neighborhoods (Archer, 2020), provides a potential mechanism driving differences in highway exposure over space, creating these inequalities.

The primary contribution of this paper is to provide the first causal estimates of the effect of an Interstate Highway on local pedestrian deaths. The issue is particularly important because road deaths are a huge contributor to accidental death in the US, a source of death that has become an increasingly large portion of US mortality (Singer, 2022). The effect of highways on neighborhoods has garnered additional interest because the historical placement of highways has been racially discriminatory due to its connection with "slum clearance" and urban revitalization projects in history. More recently, initiatives to remove urban highways from some US cities have renewed interest in estimating the local costs of highways

Figure 1: Pedestrian Deaths Relative to Population Share



The bars indicate each group's share among pedestrian deaths divided by that group's overall share of the US population, multiplied by 100. Sources: National Highway Traffic Safety Administration, Fatality Analysis Reporting System, 2001-2020; 2010 US Census.

(Brinkman et al., 2022). We attempt to provide an accounting of a clear local externality of highways: pedestrian deaths.

Our work builds on a rich literature examining traffic safety and collision externalities — how the act of driving a vehicle increases risks for all other individuals on the road (Vickrey, 1968). This literature has examined policy interventions and driver behaviors that may be affected by the presence of an Interstate like drunk driving (Ruhm, 1996; Levitt and Porter, 2001; Hansen, 2015), speed limits (Van Benthem, 2015; Ang et al., 2020; Bauernschuster and Rekers, 2022; McCarthy, 1994), passing distances (Nehiba, 2018), the overall level of driving (Parry, 2004), interactions between vehicle types (de Palma et al., 2008), and other issues that affect road safety. Characteristics of vehicles and the built environment — the man-made attributes of the places people live and work — also factor into collisions. Increases in the size and weight of vehicles have had deleterious effects on traffic safety, specifically for pedestrians (White, 2004; Anderson, 2008; Anderson and Auffhammer, 2014; Tyndall, 2021; Edwards and Leonard, 2022).[1] While differences in the built environment like land use,

[1] The movement towards larger and heavier vehicles to promote the safety of the vehicle's driver, often

population density, pedestrian infrastructure, and other variables can all impact collision risk (Merlin et al., 2020), we are particularly interested in the role of road networks in collisions. Large arterial and multi-lane streets are associated with higher collision risks as well as more severe collisions (Dumbaugh et al., 2012). Larger roads, like limited access freeways, may have fewer pedestrian collisions because pedestrians are less likely to use these roads, but pedestrian collisions tend to be more severe on these high-speed roads (DiMaggio, 2015; Chen and Shen, 2016, 2019). Highway placement also affects the local built environment through the construction of on-ramps, feeder roads, and other features. We empirically consider the mechanisms relating Interstates to pedestrian deaths by examining how pedestrian fatalities vary with distance from a highway, distance to on/off ramps, as well as how Interstates impact the speed of fatal collisions.

We also contribute to the study of racial inequalities in traffic fatalities. The literature has found large disparities in traffic deaths across racial and ethnic groups (Morency et al., 2012; Pirdavani et al., 2017; Hwang et al., 2017; Yu et al., 2018; Roll and McNeil, 2022; Sanders and Schneider, 2022). As noted in Morency et al. (2012), it is unclear if these inequalities arise due to differences in spatial proximity to dangerous traffic or differences in behavior due to socioeconomic status. For example, Black communities are more likely to be near a highway, increasing exposure. Additionally, Black workers are more likely to walk to work than white workers, providing a behavioral mechanism that increases exposure. Our empirical methodology allows us to separately examine the exposure and behavioral mechanisms.

The remainder of the paper is organized as follows. Section 2 provides background information on the US Interstate Highway System. Section 3 describes the data. Section 4 describes the empirical strategy used to examine the effects of the Interstate Highway System on pedestrian fatalities. The results are provided in Section 5, and Section 6 concludes.

referred to as an "arms race," may be particularly harmful to pedestrians who do not participate in the race.

2 The US Interstate Highway System

The current Interstate Highway System includes over 78,000 kilometers of limited access highway. The highway network generates considerable societal benefits by enabling the movement of people and goods across the country at low costs. This connectivity encouraged growth in previously hard-to-access areas (Baum-Snow, 2007; Michaels, 2008), allowed easier migration across the nation, and boosted productivity (Herzog, 2021). Jaworski et al. (2023) estimated that eliminating the Interstate Highway System would lead to a $619 billion decrease in real GDP.

These economic gains have come with costs. Interstates induce vehicle travel demand, generating pollution, congestion, and traffic collision externalities. Interstates also influence the spatial organization of cities and neighborhoods, reducing mobility in the neighborhoods they bisect (Brinkman and Lin, 2022) and increasing urban sprawl (Baum-Snow, 2007). These disamenities lead to a number of negative outcomes including population reductions in central cities and lower housing values near Interstates (Brinkman and Lin, 2022).

The original concept of the Interstate Highway System was laid out in the 1944 Federal Aid Highway Act. The stated purpose of the Interstate Highway System was for inter-city travel of goods and people, as well as to provide international connections for trade with Canada and Mexico. The system of highways was also meant to serve a military purpose, enabling the quick movement of troops and military hardware within the continental US, and enabling rapid evacuation of dense urban centers in the event of a nuclear attack. The Interstate Highway System's use for intra-urban commuting was not a primary consideration. However, after a national network of highways was planned, local jurisdictions were subsequently given leeway to route local segments of Interstate Highways to meet perceived local needs.

The expansion of the Interstate Highway System advanced sporadically throughout the 20$^{\text{th}}$ century, with major Interstate construction spanning five decades. Brooks and Liscow (2020) marks the period of significant construction as spanning 1956-1993. Much of the

Interstate Highway System was built through rural areas where land acquisition was relatively simple. For acquiring private land, the government was required to pay fair market value to displaced landowners. Local officials plotting precise routes therefore had a financial incentive to build on undeveloped, low-cost land. However, connecting the system to established cities required building through urbanized land. Many proposed sections of the project were rerouted or altered in response to local political opposition, or due to prohibitive costs of obtaining high-value land. In many instances, Interstates were rerouted to areas deemed "slums" or "blighted," which had lower land values and were disproportionately Black, urban neighborhoods (Archer, 2020). Relative to white neighborhoods, these Black neighborhoods were not able to marshal effective political resistance to highway construction. Therefore, Black communities bore greater costs from the Interstate Highway System in terms of displacement and exposure to local highway externalities. Compounding this harm, Black residents were largely excluded from living in the new suburban areas that were unlocked by the Interstate Highway System (Taylor, 2019). The redirection of Interstates and exclusionary suburban policy led to an uneven distribution of the initial costs and benefits of the system.

3 Data

The primary source of data on traffic fatalities will be the federal Fatality Analysis Reporting System (FARS) maintained by the National Highway Traffic Safety Administration. We make use of 20 years of data spanning 2001-2020.[2] FARS provides information on every motor vehicle crash in the US that resulted in at least one death. For the death to be considered caused by the crash the victim must die from related injuries within 30 days of the crash. The data omits non-fatal crashes.

FARS includes an array of variables relating to each incident. Importantly, the data includes the latitude and longitude of every crash, which allows us to spatially aggregate

[2] 2001 is the earliest year in which FARS reports latitude and longitude coordinates of crashes.

crashes. The data also includes information on the characteristics of all individuals and vehicles involved in the crash. For our primary analysis, we remove all non-pedestrian fatalities from the data, where pedestrians exclude cyclists or any person operating a motorized or non-motorized means of conveyance. We then aggregate the data to the census tract level, calculating the total number of pedestrian fatalities in a census tract during the sample period. We make use of Census TIGER shapefiles to identify 2010 census tract boundaries. When collapsing fatality counts to tracts, we generate variables capturing the average characteristics of the crash and crash victims.

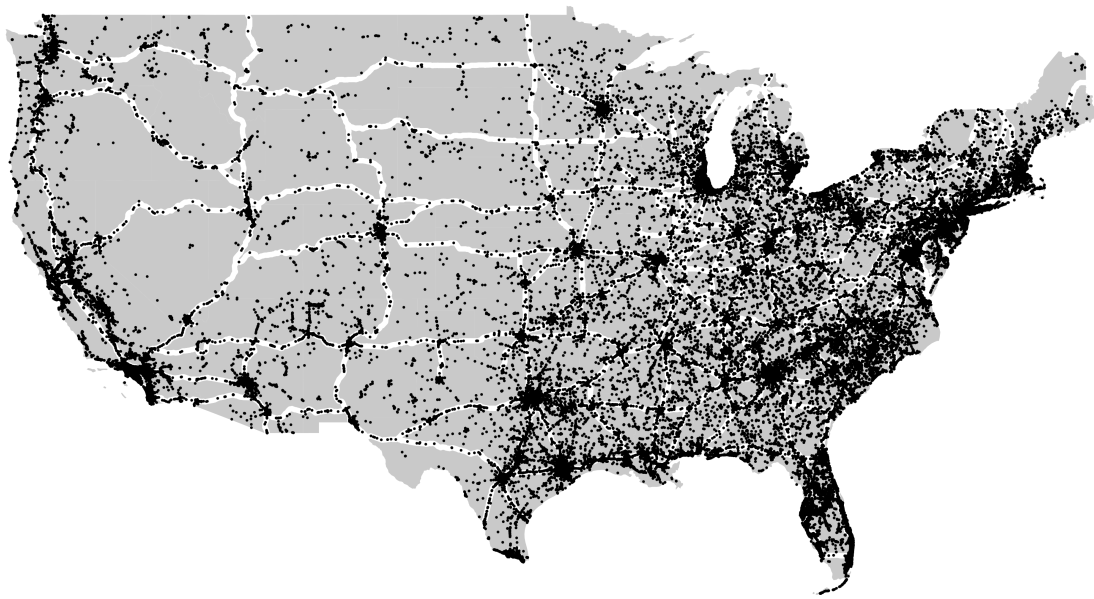
Figure 2 plots the location of all pedestrian deaths that occurred over the 2001–2020 study period. Deaths are concentrated in urbanized areas. The figure also plots the location of the current Interstate Highway System. While only providing correlative evidence, there is a clear spatial relationship between the location of Interstate Highways and the location of pedestrian deaths.

We make use of tract-level demographic control variables. Demographic information for tracts is gathered from the 2019 5-year American Community Survey (ACS). We gather variables for commuting modes, income, education, car ownership, and other socio-demographic variables. While we examine pedestrian fatalities dating back to 2001, the ACS estimates correspond to 2015-2019. The extent that census tract characteristics changed between 2001–2020 will introduce measurement error into our control variables. We test the robustness of our results using alternative vintages of ACS data in the Appendix. We drop any tract that contains fewer than 500 people, as these tracts are often missing demographic information. We drop an additional three tracts that are missing demographic information.

In addition to ACS control variables, we include a control variable for the number of jobs located in each tract. We calculate tract-level job counts from the 2019 Longitudinal Employer-Household Dynamics Origin-Destination Employment Statistics.

Summary statistics are provided in Table 1 for the 71,508 tracts that make up our final data set. Our main dependent variable is the number of pedestrian fatalities in a tract

Figure 2: Pedestrian Fatalities 2001-2020



Each dot indicates one pedestrian death. The white lines indicate the current Interstate Highway System.

between 2001 and 2020. The number of fatalities averaged 1.4 and ranged widely from 0 in many tracts to 90.

The US Census provides information on highways through the TIGER shapefile datasets. We use the 2020 "Primary Roads" shapefile. The data set maps all limited-access roadways in the US, which includes Interstate Highways as well as state-run highways. The data identifies the name of each road segment and whether it is part of the Interstate Highway System. We also make use of the Tiger "All Roads" shapefiles to identify the location of all limited access freeway on-ramps and off-ramps in the US.

We make use of an instrumental variable, discussed in more detail in Section 4, relating to the proposed Interstate Highway routes from the original 1947 federal plan. This instru-

Table 1: Tract Level Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.
Pedestrian fatalities	1.373	2.004	0	90
Pedestrian fatalities ON Interstate	0.143	0.651	0	58
Pedestrian fatalities OFF Interstate	1.23	1.802	0	32
Motorist fatalities	8.483	11.656	0	705
Local interstate	0.237	0.425	0	1
Planned local interstate	0.127	0.333	0	1
Local noninterstate highway	0.103	0.304	0	1
Population (1,000)	4.280	1.936	0.501	37.452
Population density (1,000/sq. km)	2.028	4.493	0	83.177
Number of jobs (1,000)	1.982	4.132	0	331.181
Commute share: car	0.849	0.153	0	1
Commute share: walk	0.034	0.060	0	0.876
Commute share: public transit	0.053	0.116	0	0.885
Median income (10,000)	3.327	1.381	0.250	15.132
Poverty rate	0.071	0.07	0	1
White population share	0.724	0.251	0	1
Black population share	0.139	0.218	0	1
Asian population share	0.049	0.089	0	0.938
Hispanic population share	0.167	0.215	0	1
College education share	0.595	0.175	0	1
No car share	0.048	0.097	0	1
N		71508		

Numbers are averaged over a cross-sectional sample of 71,508 unique census tracts.

mental variable was used in (Brinkman and Lin, 2022) and a digitized shapefile version of the planned routes has been publicly provided by the authors.[3] We omit Hawai'i and Alaska throughout this paper as they were not admitted as states until 1959 and therefore were excluded from the original Interstate Highway plan.

Figure 3A maps the 1947 Interstate Highway plan and the current Interstate Highway System that has been constructed. The 1947 plan is followed closely in most instances. There are some occasions where Interstates that were not planned have been built. For example, Interstate-70 running from Denver, CO through UT. There are few instances of planned Interstates that were never built. However, due to local planning decisions, Interstate paths

[3]The digitized version is available here: https://github.com/jeffrlin/freeway-data.

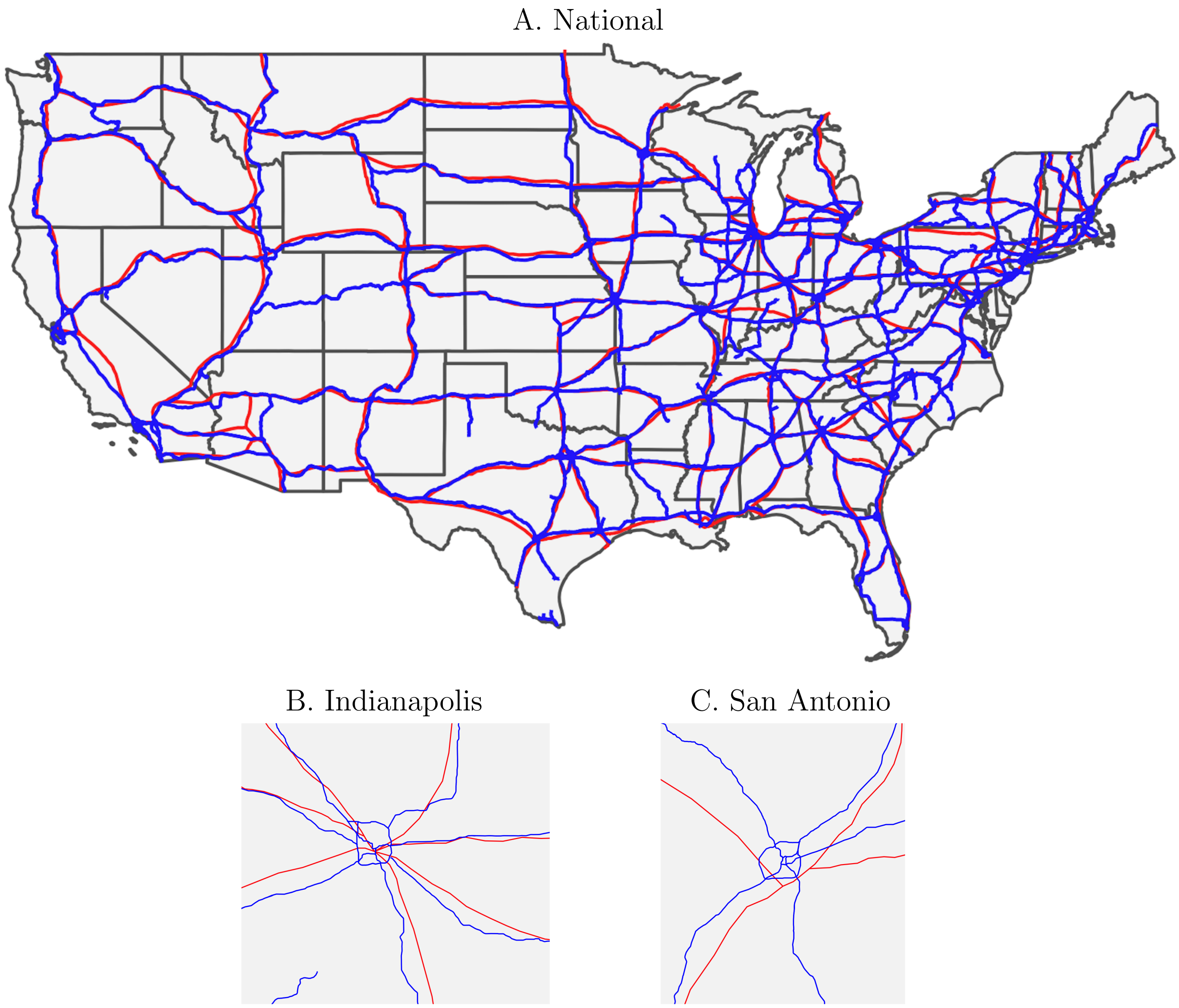
at the local level often deviate from the routes as planned at the national scale. To provide an example, Figure 3B and C show the planned and constructed Interstates for Indianapolis and San Antonio, respectively. Local authorities were given freedom to route Interstates as they preferred, and they often added additional connections in an attempt to improve local commuter traffic flow. Many cities elected to construct a highway that encircled the downtown area of their city, requiring significant construction through populated areas.

We combine the shapefiles for tract boundaries, constructed Interstate routes, planned Interstate routes, and highway access ramps. We generate a dummy variable for every tract in our sample indicating whether it is currently bisected by an Interstate Highway. We consider any overlap between an Interstate's center line and the boundaries of a tract to indicate the tract is bisected by an Interstate. We generate a similar variable for the planned Interstate shapefile and for the highway access ramps. Of 71,508 tracts in our sample, Interstate Highways bisect 16,961 tracts (23.7%) while only 9,079 (12.7%) are bisected by a planned Interstate route. Because the construction of actual routes followed more circuitous paths, the number of tracts with an Interstate exceeds the number with a route included in the original plan. 46.4% of tracts contain at least one highway access ramp.

4 Estimation Methodology

Using our cross-sectional, tract-level data we estimate the partial effect of an Interstate on local pedestrian fatalities during the 2001–2020 period. We confront two main threats to causal identification. First, as discussed above, the Interstate network was not randomly allocated over space, with route selection being influenced by both political and fiscal pressures (Rose and Mohl, 2012). Because of endogenous route selection, a naive comparison of tracts with and without Interstates would be unable to identify the causal effect of the Interstate on local deaths as tracts were systematically selected for treatment on both observable and unobservable characteristics and these characteristics may be correlated with pedestrian risk.

Figure 3: Original 1947 Interstate Plan vs. Actual Interstate System



| - Originally Planned Interstate | - Current, Completed Interstate

The current Interstate System and the Interstate system according to the 1947 federal plan are shown. The routes of the current system are highly spatially correlated with the 1947 planned routes.

A second, though less concerning issue, regards sorting.[4] A local highway produces noise and pollution, creating a local disamenity that may cause neighborhood sorting. Residents with lower socioeconomic status may therefore sort towards highways. This selection mech-

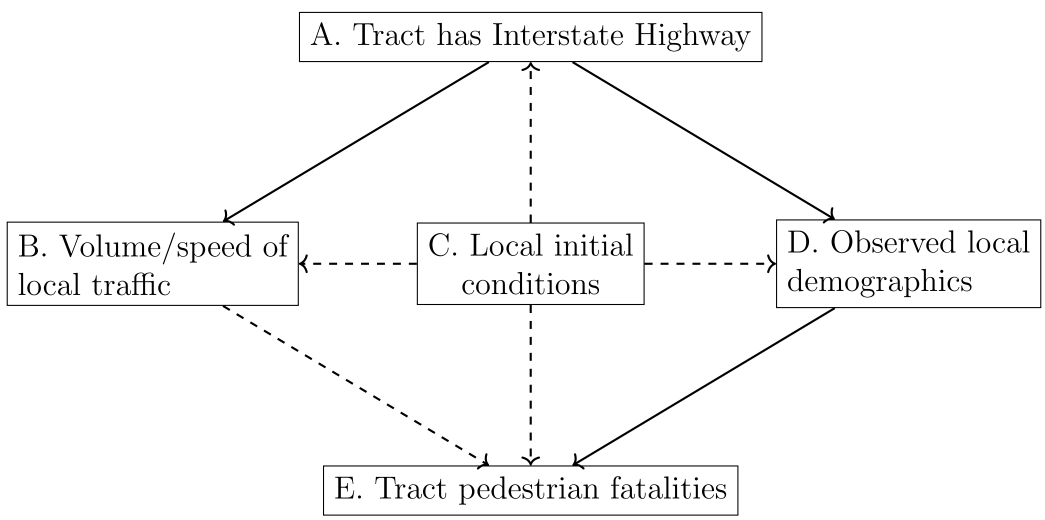
[4]This sorting is itself one potential mechanism that leads to higher pedestrian deaths within Interstate tracts.

anism may explain the higher pedestrian deaths because pedestrian deaths are correlated with socioeconomic status. We provide results where endogenous sorting is included as one mechanism that contributes to the overall rise in neighborhood pedestrian deaths. However, we also provide results that condition on observable neighborhood demographics and results suggest endogenous sorting is not the primary driver of results.

Figure 4 provides a Directed Acyclic Graph (DAG) to explicitly represent the causal paths present in our setting (Cunningham, 2022). We are interested in estimating the causal effect of a local Interstate on local pedestrian deaths. In terms of the DAG, we are interested in the effect of node A on E via both B and D. However, neighborhoods differ in their initial conditions (node C). If these conditions caused both a different probability of receiving an Interstate and were correlated with pedestrian death probability, then these initial conditions will spuriously affect the estimated relationship between nodes A and E. Initial demographic conditions may persist over time. For example, a persistently low-socioeconomic status neighborhood may have attracted an Interstate and may also suffer from higher rates of pedestrian deaths today because poor residents might walk more. Initial conditions might also include persistent features of the built environment such as street design. Second, households may sort away from or towards neighborhoods with an Interstate. Local demographics are therefore caused by both initial conditions and the causal sorting effect of a local highway. If sorting relative to Interstates is correlated with socioeconomic status, this would influence the estimated relationship between A and E via D.

By introducing an instrumental variable for Interstate allocation, we shut down the causal paths running through node C. By including a vector of local demographic controls, we shut down the causal path running through node D. In our primary specification, we do not include local demographic controls, as the total causal effect of an Interstate includes this sorting. Omitting demographic controls allows us to estimate the causal effect of an Interstate on average neighborhood conditions, rather than the causal effect on a representative individual. In some secondary specifications, we include these control variables for observed local demographics, which allows us to estimate the neighborhood effect of an Interstate independent of endogenous sorting as well as endogenous route selection. We also include control variables for the residential population, residential population density, as well as the population of jobs. One form of endogenous sorting is the general effect of an Interstate encouraging or discouraging the inflow of people and jobs. We are able to control for this outcome directly. By comparing the magnitude of the Interstate effect between these two specifications, we can determine a lower bound for the extent that sorting contributes to the causal effect. This comparison allows us to examine one potential mechanism driving the results.

Figure 4: Directed Acyclic Graph



The paths of causation that connect a local Interstate to pedestrian deaths are represented. Solid lines represent observable mechanisms while dashed lines represent unobservable mechanisms. We isolate the causal effect of A on E by proposing an IV to remove the effects of node C. We also provide results where we control for observable elements of node D in order to understand the relative importance of nodes B and D.

To overcome endogeneity of route selection, we instrument for Interstate placement using the 1947 Interstate Highway plan. This plan was created at the national level to promote defense and commerce between cities, not to facilitate travel within a city. The map was drawn at a national level, and therefore the proposed routes were orthogonal to local differences in neighborhood demographics, providing us with a plausibly exogenous instrument.

We assume an exclusion restriction wherein the precise placement of Interstate plans, within counties, is orthogonal to pre-plan tract characteristics that might cause differences in modern-day pedestrian deaths. Nationwide tract-level demographic data is not available in the pre-plan period to directly test this assumption. However, we will provide a robustness test that makes use of available 1950 Census data, and find our results are robust to conditioning on historical demographic information. Brinkman and Lin (2022) also explore the relationship between pre-plan tract characteristics and planned routes and draw a similar conclusion.

The Interstate Highway plan has been used as an instrumental variable for actual highway construction in prior research (Baum-Snow, 2007; Baum-Snow et al., 2017; Baum-Snow, 2019; Brinkman and Lin, 2022). The instrument was first used in Baum-Snow (2007) which instrumented for the total number of Interstate Highway paths in US cities using the number allotted to the city in the 1947 plan to examine the relationship between Interstates and urban sprawl. In a related work, Brinkman and Lin (2022) instrument for the location of existing freeways with the location of Interstates in historic plans to examine the local effects of new freeways on quality of life and political opposition to freeways. Our IV approach is most similar to Brinkman and Lin (2022), as we instrument for the existence of an Interstate at the granular tract level using the historic plans.

To study the possibility that Interstates caused endogenous neighborhood sorting across demographic groups, we directly control for a vector of tract-level demographics in secondary specifications. We examine the sensitivity of our results to demographic controls to determine if observable spatial demographic differences are an important component of the estimated effect of Interstates. Because we eliminate the influence of initial conditions through the instrument, the remaining variation in contemporaneous demographics can be attributed to neighborhood sorting.

The effect of an Interstate on pedestrian fatalities within a tract is estimated using the following system of equations.

$$\mathrm{I}_i = \alpha_0 + \delta \cdot \mathrm{P}_i + \phi \cdot X_i + \mu_j + \nu_i \tag{1}$$

$$\mathrm{F}_i = \beta_0 + \eta \cdot \widehat{\mathrm{I}_i} + \psi \cdot X_i + \mu_j + \epsilon_i \tag{2}$$

F is the total number of pedestrians who were killed in a motor vehicle crash between 2001 and 2020. Subscript i denotes a census tract, and j denotes a county. I is an indicator variable equal to one if the tract is currently bisected by an Interstate Highway. P is an indicator variable equal to one if the tract is bisected by a planned Interstate Highway from the 1947 plan. X is a vector of tract-level control variables and μ is a county fixed effect. The probability that a tract has an Interstate as predicted by the 1947 plan is estimated in the first stage (equation 1) while the second stage (equation 2) recovers estimates of the partial effect of an Interstate on pedestrian fatalities (η).

We estimate this system using two-stage least squares because, while F takes the form of a non-negative count variable, we are unaware of any instrumental variable or control function approaches for count models with fixed effects that are appropriate in a setting where the data are a cross-section and both the endogenous explanatory variable and excluded exogenous instrument are discrete.

The tract-level control vector (X_i) includes standard demographic controls for population, population density, median household income, the college education rate, racial and ethnic population shares, and the poverty rate.[5] These variables may be correlated with an area's built environment and demographic factors that lead individuals to walk more or walk in more dangerous areas. For example, population density is likely correlated with a tract's urbanization. We also include commuting mode shares to control for differences in the frequency that the local population walks to work, a control variable for the share of the

[5]We include contemporaneous demographic controls as opposed to controls dating to before Interstate construction due to historical data limitations. Census tracts, which became an official Census entity in 1940, covered only major cities at the time. However, the results are robust to including historical controls in the subset of tracts with available data (Table A7 and A8).

population that does not have a car, and a control variable for the number of jobs located in that tract. Additionally, we include a dummy variable for whether the tract is bisected by a highway that is not part of the Interstate Highway System. Controlling for non-Interstate Highway effects allows us to isolate the effect of Interstates specifically.

In our main specifications, we omit X_i, as local demographics may be endogenous to the Interstate itself due to sorting. Omitting X_i provides our estimate of the overall effect of the Interstate on neighborhood deaths, inclusive of the influence of demographic sorting. Because these contemporaneous demographic controls introduce a type of selection bias, they are a form of "bad controls" in the language of Angrist and Pischke (2009). Including these controls means that the estimated effect of an Interstate, η, will be equal to the causal effect on local pedestrian deaths less the effect of post-treatment sorting. While our primary focus is estimating the causal effect of Interstates on local pedestrian deaths (specifications that do not include controls), we are also interested in the extent that sorting contributes to these deaths. The difference in magnitude of the estimated effect of Interstates between specifications with and without controls can be viewed as a lower bound on the contributions of sorting to the total causal effect.

Our estimation strategy yields the Local Average Treatment Effect (LATE) corresponding to tracts that were treated by an Interstate Highway because of their location along a planned Interstate route. While some tracts on the planned route were able to resist construction of the freeway, we provide an estimate of how much higher pedestrian deaths are in tracts with Interstates, relative to an alternative history where the same tract was able to resist the Interstate's construction. This effect encapsulates the total change in local pedestrian fatalities spurred by operating an Interstate in a tract. Some of these deaths may involve pedestrians illegally crossing dangerous Interstates, but others occur due to changes in mileage, vehicle speeds, and built environmental factors (on and off ramps, wider roads, etc.) that Interstates induce in neighborhoods. Our analysis examines how these various factors contribute to pedestrian deaths.

Our empirical strategy assumes that national characteristics are held constant. While the Interstate Highway System as a whole likely contributed to national private vehicle adoption and increased mobility, we capture only the local impact of highway treatment by using non-Interstate treated tracts as our control group.

We estimate similar models to examine racial disparities. In these models, the outcome is the count of pedestrian fatalities for a race within a tract, normalized by dividing by the full US population of that race in millions. For example, one outcome variable is the number of white pedestrian deaths in a tract per million white US residents. These regressions estimate how adding an Interstate to a tract affects the white, Black, Asian, or Hispanic pedestrian deaths within that tract in terms of the contribution to the national death rate for that race. Large relative differences in estimated coefficients across races would suggest that Interstates differentially affect some populations.

5 Results

5.1 Effect of Interstates on Pedestrian Fatalities

We now estimate the causal effect of having an Interstate in a census tract on local pedestrian fatalities. The possibility that Interstates were not randomly allocated over space necessitates the instrumental variables strategy discussed in Section 4. Table 2 provides results for the first stage regression examining how having a planned Interstate in a census tract in the 1947 plan affects the probability that the tract is bisected by an Interstate today. Controls vary across specifications as shown, and all standard errors are clustered at the county level.

A planned local Interstate is a strong and statistically significant predictor of which census tracts currently contain an Interstate. Without any control variables or fixed effects, we find that a tract bisected by a planned Interstate was 27.6 percentage points more likely to contain an Interstate today (column 1). Our primary specification in column 2 adds county fixed effects, which reduces the estimated effect by only considering within-county variation. Column 3 adds a vector of local demographic controls. The inclusion of control variables further reduces the estimated effect of the highway plan to a 15.9 percentage point increase.

Table 2: First Stage Regression Results

	(1)	(2)	(3)
Planned local interstate	0.276**	0.179**	0.159**
	(0.011)	(0.010)	(0.010)
Local noninterstate highway			-0.260**
			(0.010)
Population (1,000)			0.011**
			(0.001)
Population density (1,000/sq. km)			-0.015**
			(0.004)
Number of jobs (1,000)			0.016**
			(0.002)
Commute share: car			0.142**
			(0.052)
Commute share: walk			-0.187*
			(0.091)
Commute share: public transit			0.105
			(0.078)
Median income (10,000)			0.001
			(0.003)
Poverty rate			-0.059
			(0.039)
White population share			-0.006
			(0.039)
Black population share			0.026
			(0.047)
Asian population share			0.202**
			(0.071)
Hispanic population share			0.080*
			(0.035)
College education share			-0.078**
			(0.027)
No car share			0.186*
			(0.079)
Constant	0.202**	0.215**	0.095
	(0.005)	(0.001)	(0.069)
County fixed effects	N	Y	Y
N	71508	71508	71508

Significance levels: $*$: 5% $**$: 1%. Robust standard errors clustered at the county level in parenthesis. Dependent variable is an indicator for whether a tract has an Interstate.

Table 3 presents OLS and IV estimates of the effects of local Interstates on pedestrian fatalities in a census tract. OLS results are in columns 1–2 while IV results are in columns 3–4. Controls vary across specifications, and every regression clusters standard errors at the county level. Each regression includes county fixed effects to control for time-invariant differences in characteristics across locations like regional climate. For the IV estimates, both specifications have a Kleibergen-Paap F statistic of the excluded instrument in excess of 270. These large F statistics combined with the strength of the results in the first stage suggest a strong instrument.

The OLS regressions estimate that a local Interstate increases pedestrian fatalities in a census tract by 0.73 and 0.96 deaths between 2001 and 2020 in columns 1 and 2, respectively. The magnitude of the effect becomes much larger in the IV regressions which correct for the non-random placement of Interstates across space. Our primary specification in column 3 estimates an Interstate causes 2.5 pedestrian fatalities over the period 2001–2020.[6] Put differently, for a neighborhood with an Interstate, our LATE estimates that 2.5 fewer pedestrian deaths would have occurred in that neighborhood if the Interstate had never been built, on average.

The introduction of contemporaneous demographic control variables at the census tract level in column 4 has a relatively minor impact on the estimated effect of an Interstate. The inclusion of county fixed effects means the demographic control variables are only able to capture within-county variation in demographics, which might be relatively small. We find that controlling for this within-county demographic variation does not significantly affect the impact of an Interstate on pedestrian deaths, but we delve further into what can be learned from the differences between estimates below. We obtain these control variables from the 2019 5-year ACS estimates, but we examine the robustness of the results to older 5-year ACS vintages in Appendix Table A3, finding similar results.

[6]These results are robust to both inverse hyperbolic sine and per-capita transformations of the dependent variable. See Appendix Tables A1 and A2. These results are also robust to excluding 2020 data that may be affected by the COVID-19 pandemic.

Table 3: Effect of a Local Interstate on Pedestrian Fatality Count

	(1)	(2)	(3)	(4)
Local interstate	0.960**	0.734**	2.516**	1.987**
	(0.050)	(0.039)	(0.265)	(0.228)
Local noninterstate highway		0.255**		0.583**
		(0.045)		(0.071)
Population (1,000)		0.151**		0.136**
		(0.007)		(0.007)
Population density (1,000/sq. km)		-0.053**		-0.033**
		(0.013)		(0.010)
Number of jobs (1,000)		0.098**		0.078**
		(0.014)		(0.013)
Commute share: car		-0.143		-0.285
		(0.301)		(0.305)
Commute share: walk		0.415		0.648
		(0.383)		(0.399)
Commute share: public transit		1.886**		1.768**
		(0.469)		(0.479)
Median income (10,000)		-0.032*		-0.035*
		(0.015)		(0.015)
Poverty rate		0.750**		0.856**
		(0.229)		(0.229)
White population share		-0.841**		-0.839**
		(0.287)		(0.279)
Black population share		-0.575		-0.614*
		(0.310)		(0.303)
Asian population share		-1.258**		-1.506**
		(0.359)		(0.353)
Hispanic population share		-0.213		-0.303
		(0.264)		(0.268)
College education share		-2.548**		-2.418**
		(0.168)		(0.164)
No car share		0.251		0.016
		(0.318)		(0.324)
County fixed effects	Y	Y	Y	Y
IV	N	N	Y	Y
Kleibergen-Paap F	-	-	319.726	274.945
N	71508	71508	71508	71508

Significance levels: $*$: 5% $**$: 1%. Robust standard errors clustered at the county level in parenthesis. Dependent variable is the number of pedestrian fatalities that occurred in the tract between 2001-2020.

Comparing the OLS to IV estimates suggests that the endogenous selection of Interstate routes is highly correlated with latent determinants of pedestrian deaths. Overall, we find that the correlative association between Interstates and local pedestrian deaths understates the causal effect. Nationally, Interstate construction was most difficult in urban areas, and local planning officials may have strategically favored undeveloped or low-density areas to locate Interstates, allowing Interstates to avoid areas with a high density of pedestrians.[7] Our IV specifications estimate a LATE for the tracts that gained an Interstate on account of being bisected by a route that was planned in 1947. Therefore, the impact of highway construction on this type of tract appears far greater than the effect among all tracts that actually received an Interstate.

Tracts that received an Interstate based on the 1947 plan likely experienced some degree of sorting. Highways and other large roads are disamenities that lower housing prices and increase pollution, suggesting that they will also affect who lives in a neighborhood with an Interstate contemporaneously. While our primary goal is to estimate the total impact of an Interstate on local pedestrian fatalities inclusive of this sorting (column 3), we are also interested in the extent that sorting contributes to this effect. To examine this mechanism, we estimate secondary specifications in columns 2 and 4 that include "post-treatment" controls.[8] In other words, we control for contemporaneous neighborhood characteristics and demographics to examine if this sorting is one of the main mechanisms driving the increase in pedestrian fatalities. The IV specification in column 3 estimates the total effect of an Interstate on average neighborhood pedestrian deaths, while the secondary specification in column 4 eliminates the variation in fatalities due to observable sorting of individuals and economic activity.[9] The difference in the magnitude of the effect between these two columns can be thought of as a lower bound to the portion attributable to sorting, in other words, at least 21% of the effect is due to sorting.[10]

[7] We find evidence supporting this hypothesis in Appendix Tables A4 and A5, which examine how our results vary with population density. We find larger in magnitude and stronger first-stage results for the least dense areas, suggesting that denser census tracts were more capable of resisting planned Interstate routes.

[8] In the language of Angrist and Pischke (2009), these are "bad controls," but their inclusion allows us to learn the extent that sorting contributes to the effect of Interstates.

[9] We further control for an area's built environment by controlling for "walkability" in Appendix Table A6, finding qualitatively similar results.

[10] While we are only able to control for observable differences across tracts, these differences do not drive

In addition to contemporaneous controls, we consider historical control variables from the 1950 Census in Appendix Tables A7 and A8; however, data coverage from the 1950 Census is poor. We attempt to validate that the routes in the 1947 Interstate plan are orthogonal to tract-level variables that could predict local pedestrian deaths today. Table A7 performs a balancing test comparing tracts with and without planned routes while Table A8 includes historical demographic variables as controls in our regression strategy. We find that demographics are largely balanced across tracts, with somewhat higher Black population share in tracts with a planned interstate. We find the results are robust to the inclusion of these historical control variables and fail to find a significant effect of historical Black population share on the probability that a tract has a planned Interstate route in the first stage. These results suggest that the planned routes are orthogonal to pre-plan characteristics, a finding similar to that of (Brinkman and Lin, 2022). We provide additional discussion in Appendix A.

One potential threat to identification in this setting is the possibility of treatment spillovers. Spatial spillovers in this context could violate The Stable Unit Treatment Value Assumption (SUTVA). Constructing an Interstate in a neighborhood likely impacts both the treated tract as well as the transportation network and flows in nearby tracts. To examine the influence of these potential spillovers, Table 4 drops control tracts that are near Interstates and therefore may have been partially treated.

Table 4, column 1 provides the full sample results for comparison. Column 2 drops all tracts that are untreated by the Interstate but are within a one km buffer of an Interstate. Columns 3-6 increase the buffer in one km increments to drop additional tracts. We find that dropping these tracts has no impact on results. Across the five reduced sample estimates, our estimated Interstate effect does not change by more than 8%. Because the Interstate system was so extensive, most tracts are relatively close to an Interstate — 63% of tracts

the results. Given the rich set of controls included, it seems unlikely that unobservable sorting can explain our results, suggesting a direct causal effect of Interstates on pedestrian deaths, independent of local demographic conditions.

either have or are within 5 km of an Interstate. The results suggest that spatial spillovers of Interstates are either small or their effect on control tracts is relatively uniform across space. As a corollary to this analysis, we also include the tracts removed here as "treated" tracts in Appendix Table A9, finding the average effect of an Interstate decreases as "treated" tracts become more distant. These conclusions are also consistent with the results presented in the next section which show that the effect of Interstates decays quickly over space.

Table 4: Effect of a Local Interstate on Pedestrian Fatality Count: Dropping Control Tracts Close to Interstates

	(1)	(2)	(3)	(4)	(5)	(6)
Local interstate	2.516**	2.406**	2.391**	2.436**	2.502**	2.698**
	(0.265)	(0.248)	(0.239)	(0.232)	(0.236)	(0.254)
County fixed effects	Y	Y	Y	Y	Y	Y
IV	Y	Y	Y	Y	Y	Y
Control observations dropped	None	Within 1 km	Within 2 km	Within 3 km	Within 4 km	Within 5 km
Kleibergen-Paap F	319.726	345.839	319.793	327.165	312.521	295.515
N	71508	63857	56482	50619	46341	43092

Significance levels: $*$: 5% $**$: 1%. Robust standard errors clustered at the county level in parenthesis. Dependent variable is the number of pedestrian fatalities that occurred in the tract between 2001-2020.

5.2 Spatial Decay and Built Environment Factors

Next, we further examine the mechanisms driving the relationship between Interstates and pedestrian fatalities. We consider the location of pedestrian deaths, observable changes in the built environment induced by Interstates (Interstate entrances and exits), and the speed of vehicles involved in pedestrian fatalities.

We theorize that the increase in pedestrian fatalities in tracts with Interstates estimated in Table 3 may occur both on the Interstate road itself as well as the surrounding area. While the Interstate itself may be dangerous because it is a wide, difficult to cross, high-speed route that creates a physical barrier for pedestrians, it may also lead to a more dangerous environment in the nearby vicinity. The larger roads may result in local spillovers within the tract by increasing local traffic volume, increasing local traffic speed, changing the built environment, or other factors. These spillovers may increase pedestrian fatalities off Interstates.

Table 5 explicitly tests for this possibility using FARS data on the type of road where the crash occurred. We estimate specifications that include only fatalities that occurred on an Interstate (including Interstate entrance ramps, exit ramps, and frontage roads) in columns 1–2 and only fatalities that occurred off Interstates, though still within the treated census tract, in columns 3–4.[11] We find that local Interstates increase pedestrian deaths both on and off of Interstates. We estimate a local Interstate caused an average of 1.1 pedestrian deaths on the Interstate and 1.4 deaths off the Interstate, suggesting local spillovers to other roads account for more than half of the deaths. Controlling for contemporary demographics reduces the magnitude of both estimates, but has a larger impact on fatalities that occurred off of Interstates because these deaths are more likely to be local residents. Recent research has also found that approximately 18% of pedestrian deaths on Interstates are disabled-vehicle-related crashes — crashes where a motorist has left their vehicle while on an Interstate before being struck (Wang and Cicchino, 2020). Further, if we isolate our analysis to only deaths that occur on Interstate ramps, we find about 6% of the on-Interstate effect is attributable to these ramp fatalities. By and large, on-Interstate deaths appear to be cases where an Interstate has bisected a neighborhood forcing pedestrians to attempt dangerous and illegal crossings.

Among deaths occurring off Interstates, we propose that the traffic and higher vehicle speeds induced by Interstates generate spillovers onto neighboring roads, which may also experience higher traffic and vehicle speeds due to their proximity to the highway. We expect this effect would decay as the distance from an Interstate increases. To test this hypothesis, we use the precise location of all pedestrian deaths occurring off of an Interstate and calculate the linear distance from the death to the Interstate. We then estimate a model similar to equation 2, replacing the left-hand side variable with the number of pedestrian deaths that occurred within a specific distance range from the Interstate. We use 100-meter

[11] The FARS data includes fatalities that occur on entrance ramps, exit ramps, and frontage roads within their definition of Interstate fatalities.

Table 5: Effect of a Local Interstate on Pedestrian Fatality Count that Occur On Vs. Off an Interstate

	(1)	(2)	(3)	(4)
	Fatality ON interstate		Fatality OFF interstate	
Local interstate	1.101**	1.051**	1.415**	0.936**
	(0.099)	(0.089)	(0.206)	(0.187)
Local noninterstate highway		0.087**		0.496**
		(0.020)		(0.067)
Population (1,000)		0.001		0.135**
		(0.002)		(0.007)
Population density (1,000/sq. km)		0.003		-0.036**
		(0.002)		(0.009)
Number of jobs (1,000)		0.010		0.068**
		(0.007)		(0.009)
Commute share: car		-0.148*		-0.138
		(0.068)		(0.295)
Commute share: walk		0.076		0.572
		(0.143)		(0.366)
Commute share: public transit		0.182*		1.585**
		(0.088)		(0.464)
Median income (10,000)		-0.009		-0.025
		(0.005)		(0.014)
Poverty rate		-0.072		0.928**
		(0.049)		(0.219)
White population share		-0.021		-0.819**
		(0.072)		(0.266)
Black population share		-0.026		-0.589*
		(0.071)		(0.288)
Asian population share		-0.340**		-1.166**
		(0.088)		(0.328)
Hispanic population share		-0.111*		-0.192
		(0.051)		(0.256)
College education share		-0.170**		-2.248**
		(0.036)		(0.151)
No car share		-0.244**		0.260
		(0.090)		(0.287)
County fixed effects	Y	Y	Y	Y
IV	Y	Y	Y	Y
Kleibergen-Paap F stat	319.726	274.945	319.726	274.945
N	71508	71508	71508	71508

Significance levels: $*$: 5% $**$: 1%. Robust standard errors clustered at the county level in parenthesis. Dependent variable is the number of pedestrian fatalities that occurred (either on or off an Interstate) in the tract between 2001-2020.

buckets, first counting the number of non-Interstate pedestrian deaths occurring up to 100 meters away from the Interstate, then counting those occurring between 100 and 200 meters from an Interstate and increasing at intervals of 100 meters. The majority of tracts contain no area that is adjacent to an Interstate, so their value for near Interstate deaths is zero. We directly control for the share of each tract that is within 100, 200, 300, 400 and 500 meters of an Interstate with separate control variables. Equation 3 provides the estimation equation.

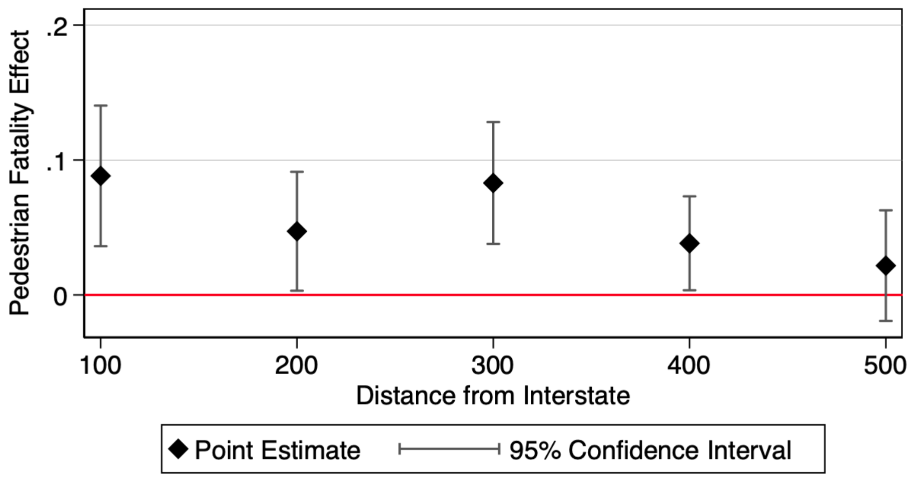
$$\mathrm{F}_i^d = \beta_0 + \eta \cdot \widehat{\mathrm{I}_i} + \psi \cdot X_i + \mu_j + \beta_1 A_i^{100} + \beta_2 A_i^{200} + \beta_3 A_4^{300} + \beta_4 A_i^{400} + \beta_5 A_i^{500} + \epsilon_i \qquad (3)$$

Equation 3 is identical to the main specification (Equation 2) with two exceptions. First, the dependent variable captures only the fatalities that occurred within d meters but more than d-100 meters of the Interstate. Second, control variables for the share of area in the tract that is within each 100 meter distance are added ($A_i^{100} + \ldots + A_i^{500}$). We provide IV results where $\widehat{\mathrm{I}_i}$ is instrumented with a dummy variable for a planned Interstate, consistent with the approach taken throughout the paper.

Figure 5 displays estimates of the effect of an Interstate on fatalities that occurred at varying distances from the Interstate within the treated tract. For example, the first point estimate indicates that 0.09 additional off-Interstate fatalities occurred between 0–100 meters from an Interstate while the second point indicates .05 additional fatalities 100–200 meters from the Interstate. We estimate significant and positive effects for the first four bins but fail to find a significant increase in pedestrian deaths for the 400–500 meter bin. The downward trend of the estimates as distance from the Interstate increases suggests that the effects of an Interstate on pedestrian fatalities are highly localized — occurring mainly within a few blocks of the Interstate. The finding suggests that roads proximal to an Interstate Highway present significant safety risks for pedestrians.

Given the rapid decay of the effect of an Interstate on pedestrian fatalities across space, we hypothesize that local changes in the built environment play a large factor in these deaths. To examine this possibility, we investigate how the presence of an Interstate ramp

Figure 5: Spatial Decay of Pedestrian Deaths



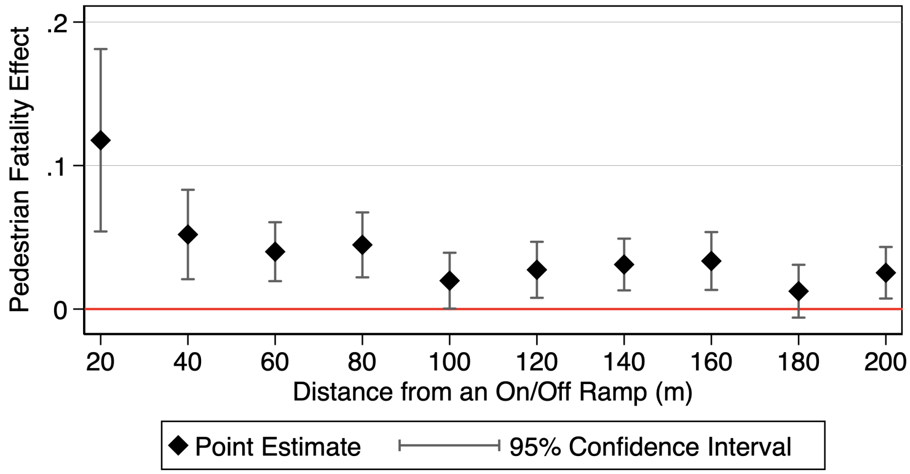
Each point estimate pertains to a separate regression. IV estimates are shown for the estimated number of pedestrian deaths occurring within bins according to distance from the Interstate, where the distances indicate the outer boundary of the 100 meter wide ring.

in a tract — a readily quantifiable change in the built environment — affects off-Interstate deaths. Because our measure of off-Interstate deaths does not include deaths that occur on ramps, these deaths can likely be attributed to changes in traffic flows and speeds caused by high-capacity roads that feed Interstates in the local area as opposed to ramps themselves increasing fatalities.

Figure 6 estimates our main IV regression equation but examines deaths occurring within distance bins around on and off ramps. For these regressions, the distance bins are reduced to 20 meter intervals. We find that Interstate caused pedestrian deaths are concentrated around ramps. Within 20 meters of an on/off ramp pedestrian fatalities *that did not occur on the ramp or Interstate* increase by 0.12. This effect decays quickly with distance from the ramp. These results provide evidence that one of the main channels that Interstates lead to off-Interstate pedestrian fatalities is through changes to the characteristics of the surrounding road network and built environment.

Interstates may also influence vehicle speeds leading to more frequent and severe collisions

Figure 6: Spatial Decay of Pedestrian Deaths Around On/Off Ramps



Each point estimate pertains to a separate regression. IV estimates are shown for the estimated number of pedestrian deaths occurring within bins according to distance from the on/off ramp, where the distances indicate the outer boundary of the 20 meter wide ring.

with pedestrians. Interstates of course have higher speed limits, but the roads surrounding Interstates could also have higher speed limits due to changes in the built environment discussed above. Drivers may also be more likely to exceed the posted speed limit near an Interstate as they transition onto or off the Interstate. We examine how a local Interstate in a tract affects the speeds of vehicles involved in collisions with pedestrian fatalities in Table 6. In this table, we modify our IV strategy to focus on collision-level microdata that reports the average speed across all vehicles that were involved in the crash.[12] Equation 4 provides the second stage estimation equation.

$$\text{Speed}_{ki} = \beta_0 + \eta \cdot \widehat{\text{I}_i} + \psi \cdot X_i + \mu_j + \epsilon_{ki} \tag{4}$$

Our unit of observation in these regressions is an individual collision k that occurred in tract i. We estimate the effect of having an Interstate in the tract on the average vehicle

[12]We also estimate a similar specification using the maximum speed of any vehicle in the collision as the dependent variable in Appendix Table A10.

speed in the collision. These vehicle speeds are provided in police reports and rely on the officer's guess of the vehicle speed based on the accounts of witnesses, drivers, and any other available information. Unfortunately, many collisions do not have speed information and must be dropped from the sample. A lack of data points across all tracts prevents us from running a tract-level estimation model.

In Table 6 columns 1 and 2 we include all collisions with speed data. We estimate a local Interstate causes the average speed of vehicles involved in a crash causing a pedestrian death to be 12.6 miles per hour higher in column 1 (which presents the cumulative effect inclusive of sorting). In columns 3 and 4 we limit the analysis to pedestrian fatalities that occurred off of Interstates to investigate how speeds change on local roads. We find that Interstates increase the average speed of vehicles involved in a crash causing a pedestrian death by 5.2 miles per hour in these off-Interstate collisions in column 3.[13] These results are consistent with past research and confirm that higher speeds, both on and off Interstates, are one mechanism through which Interstates lead to higher pedestrian fatalities (Wang and Cicchino, 2020).

As a placebo test, we also examine if local Interstates impact the probability that a driver consumed alcohol prior to being involved in a crash in Appendix Table A10, failing to find a significant effect. We also note that we estimate the average effect of an Interstate, and this effect may vary with Interstate characteristics like its width or whether it is elevated. While this heterogeneity is unlikely to bias our estimate of the average effect, it would be an interesting margin to examine if such granular data were available. To the extent that these characteristics are correlated with population density, Appendix Tables A4 and A5 may partially address this point by showing that Interstates in more densely populated areas result in relatively more pedestrian fatalities.

[13] After controlling for contemporaneous sorting in column 4 the estimate decreases slightly in magnitude and is statistically significant at the 10% level.

Table 6: Effect of a Local Interstate on Speed of Vehicles Involved in Pedestrian Fatalities

	(1)	(2)	(3)	(4)
	All fatalities		Fatality OFF interstate	
Local interstate	12.615**	11.111**	5.197**	3.629
	(1.784)	(1.783)	(1.968)	(1.948)
Local noninterstate highway		6.325**		5.879**
		(0.930)		(0.934)
Population		0.137*		0.212**
		(0.054)		(0.049)
Population density (1,000/sq. km)		-0.902**		-0.924**
		(0.168)		(0.165)
Number of jobs (1,000)		-0.104**		-0.089**
		(0.026)		(0.030)
Commute share: car		-1.933		-0.166
		(2.930)		(2.808)
Commute share: walk		-13.316**		-12.852**
		(3.538)		(3.645)
Commute share: public transit		0.218		-0.085
		(4.962)		(4.182)
Median income (10,000)		0.342*		0.208
		(0.174)		(0.189)
Poverty rate		-5.034*		-3.636
		(2.044)		(2.046)
White population share		-0.901		-1.130
		(1.512)		(1.422)
Black population share		-4.213*		-4.406**
		(1.647)		(1.604)
Asian population share		-5.725*		-4.324*
		(2.300)		(2.138)
Hispanic population share		-4.959**		-5.792**
		(1.210)		(1.235)
College education share		-5.952**		-7.053**
		(1.288)		(1.185)
No car share		-3.343		-3.448
		(2.784)		(2.612)
County fixed effects	Y	Y	Y	Y
IV	Y	Y	Y	Y
Kleibergen-Paap F stat	69.792	60.057	49.257	44.530
N	37926	37926	33683	33683

Significance levels: $*$: 5% $**$: 1%. Robust standard errors clustered at the county level in parenthesis. Dependent variable is the average reported speed of vehicles involved in a fatal pedestrian collision, in miles per hour.

5.3 Racial Disparities in Pedestrian Fatalities from Interstates

In this section, we examine the possibility that Interstates have exacerbated racial disparities in pedestrian fatalities. We estimate the partial effect of an Interstate on local deaths within specific racial groups, normalizing the number of deaths by that group's representation in the overall US population. We re-specify the dependent variable as the number of pedestrian deaths that occurred within a racial group per million US residents of that race. In other words, we estimate Interstate induced pedestrian deaths among white, Black, Asian, or Hispanic pedestrians in terms of the impact on the national death rate for that group.

Table 7 presents results with each column corresponding to the effects on a different population. All specifications are estimated using two-stage least squares with county fixed effects and standard errors clustered at the county level. We find that Interstates cause a similar-sized increase of 0.005-0.006 in White and Asian pedestrian deaths per million individuals while Hispanic deaths are slightly lower at 0.003. However, Interstates cause a far larger increase in Black pedestrian deaths per million. The estimated increase of 0.013 deaths per million Black US residents is more than double the effect for white deaths. We include control variables for local racial composition, suggesting the effect is not due to the endogenous racial make-up of neighborhoods with Interstates but is rather capturing that an exogenously imposed Interstate represents a larger safety threat to Black residents than it does to white, Asian, or Hispanic residents. Lower car ownership and higher rates of walking among Black communities may lead to higher exposure to the risks of a local Interstate.

Table A11 provides a similar analysis but specifies the dependent variable as the share of pedestrian fatalities within a tract that were of a specific race. Tracts with no pedestrian fatalities are omitted from this analysis because we cannot calculate shares when there are no observations. We regress the share of that tract's pedestrian fatalities that are of a particular race and find qualitatively similar results. A local Interstate is found to cause a 9.7 percentage point decrease in the share of local victims who are white, and an offsetting 8.4 percentage point increase in the share of victims who are Black.

Table 7: Effect of a Local Interstate on Victim Race

	(1)	(2)	(3)	(4)
	White	Black	Asian	Hispanic
Local interstate	0.005**	0.013**	0.006**	0.003**
	(0.001)	(0.002)	(0.001)	(0.001)
Local noninterstate highway	0.001**	0.004**	0.002**	0.001
	(0.000)	(0.001)	(0.000)	(0.000)
Population (1,000)	0.000**	0.001**	0.001**	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Population density (1,000/sq. km)	-0.000*	-0.000	-0.000**	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Number of jobs (1,000)	0.000**	0.000**	0.000**	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)
Commute share: car	0.000	-0.001	-0.004*	-0.003
	(0.001)	(0.002)	(0.002)	(0.002)
Commute share: walk	0.003*	0.006	0.002	-0.004
	(0.001)	(0.003)	(0.002)	(0.002)
Commute share: public transit	0.005**	0.010*	0.007**	0.004
	(0.001)	(0.004)	(0.003)	(0.003)
Median income (10,000)	-0.000**	0.000*	0.000**	-0.001**
	(0.000)	(0.000)	(0.000)	(0.000)
Poverty rate	0.002**	0.007**	0.004**	-0.004*
	(0.001)	(0.002)	(0.001)	(0.002)
White population share	0.001*	-0.002	0.000	-0.002
	(0.001)	(0.002)	(0.002)	(0.001)
Black population share	-0.002**	0.026**	-0.001	-0.004*
	(0.001)	(0.003)	(0.002)	(0.002)
Asian population share	-0.004**	-0.007*	-0.003	0.036**
	(0.001)	(0.003)	(0.002)	(0.005)
Hispanic population share	0.001	-0.006*	0.017**	-0.002*
	(0.001)	(0.002)	(0.002)	(0.001)
College education share	-0.006**	-0.013**	-0.009**	-0.003**
	(0.000)	(0.002)	(0.001)	(0.001)
No car share	-0.001	-0.004	-0.003	0.001
	(0.001)	(0.002)	(0.002)	(0.003)
County fixed effects	Y	Y	Y	Y
IV	Y	Y	Y	Y
Kleibergen-Paap F stat	274.945	274.945	274.945	274.945
N	71508	71508	71508	71508

Significance levels: $*$: 5% $**$: 1%. Robust standard errors clustered at the county level in parenthesis. Dependent variable is the number of pedestrian deaths with victims of the indicated race in a tract per million residents of the indicated race in the US.

5.4 Effect of Interstates on Motorist Fatalities

Finally, we examine how local Interstates affect motorist fatalities as well as the race of motorists involved in fatal collisions. Table 8 columns 1–2 (which are comparable to Table 3 columns 3 and 4) estimate that the presence of an Interstate increased motorist fatalities in a tract by 26.6–28.3 deaths between 2001–2020. The far larger magnitude of these effects relative to the estimates for pedestrian fatalities is to be expected due to pedestrian deaths only representing 13% of road deaths. Columns 3 and 4 of Table 8 examine how Interstates affect white and Black motorist fatalities in a tract per million US residents of each race, respectively. Columns 3 and 4 estimate that white and Black motorist fatalities experience nearly identical increases due to Interstates, suggesting that the racial disparities caused by Interstates may be unique to pedestrian fatalities.

While these motorist results demonstrate large effects, we consider the location of a motorist death as more incidental as compared to pedestrian deaths. A motorist might cross dozens of census tracts during a single trip, meaning the precise tract where the death occurred may not represent a tract where that motorist spends time or has a connection to. In contrast, pedestrians are likely to only travel within a specific set of tracts where they either live, work, or otherwise spend time. In this paper, we attempt to quantify the local safety disamenity presented by Interstate Highways, which seems particularly pertinent for pedestrians. Future work, which might adopt a more aggregate rather than locally focused methodology, will be important to understanding the causal effect of the Interstate Highway System on motorist deaths.

6 Conclusion

We estimate the impact of the Interstate Highway System on neighborhood pedestrian deaths in the US, with attention given to racial disparities in these fatalities. Using the 1947 Interstate Highway plan as an instrument for contemporaneous Interstate placement allows

Table 8: Effect of a Local Interstate on Total Fatalities and White and Black Victims Per Million, Among Motorists

	(1)	(2)	(3)	(4)
	Aggregate Fatality Δ		White Motorists	Black Motorists
Local interstate	28.314**	26.601**	0.084**	0.078**
	(1.885)	(1.904)	(0.006)	(0.007)
Local noninterstate highway		7.878**	0.025**	0.023**
		(0.518)	(0.002)	(0.002)
Population (1,000)		0.880**	0.003**	0.002**
		(0.052)	(0.000)	(0.000)
Population density (1,000/sq. km)		-0.060	-0.000	0.000
		(0.059)	(0.000)	(0.000)
Number of jobs (1,000)		-0.163**	-0.001**	-0.000
		(0.048)	(0.000)	(0.000)
Commute share: car		-17.372**	-0.060**	-0.018**
		(2.003)	(0.007)	(0.006)
Commute share: walk		-8.944**	-0.033**	0.019
		(2.755)	(0.009)	(0.010)
Commute share: public transit		-7.117**	-0.025**	0.012
		(2.202)	(0.008)	(0.009)
Median income (10,000)		0.117	0.000	0.001*
		(0.093)	(0.000)	(0.000)
Poverty rate		-12.895**	-0.047**	-0.017**
		(1.274)	(0.004)	(0.005)
White population share		-2.262	0.007	0.001
		(1.484)	(0.005)	(0.004)
Black population share		-9.144**	-0.028**	0.059**
		(1.660)	(0.005)	(0.007)
Asian population share		-17.565**	-0.046**	-0.030**
		(2.395)	(0.007)	(0.007)
Hispanic population share		-15.290**	-0.045**	-0.031**
		(1.424)	(0.005)	(0.004)
College education share		-17.586**	-0.057**	-0.043**
		(1.183)	(0.004)	(0.004)
No car share		-4.766**	-0.012*	-0.031**
		(1.397)	(0.005)	(0.007)
County fixed effects	Y	Y	Y	Y
IV	Y	Y	Y	Y
Kleibergen-Paap F stat	319.726	274.945	274.945	274.945
N	71508	71508	71508	71508

Significance levels: $*$: 5% $**$: 1%. Robust standard errors clustered at the county level in parenthesis. Dependent variables is either the number of motorist deaths in a tract (columns 1 and 2) or the number of motorist deaths in a tract of the indicated race per million US residents of that race (columns 3 and 4).

us to produce causal estimates of the effects of the system, examine the historical legacy of Interstates, and consider the role of neighborhood sorting in pedestrian fatalities.

We find that operating an Interstate in a census tract leads to an additional 2.5 pedestrian fatalities within that tract between 2001–2020. By including post-treatment controls, we disentangle the effect of neighborhood sorting that occurred due to the Interstate's placement from the effect of Interstates on local pedestrian fatalities caused by the volume and speed of local traffic. We find that at least 21% of the effect of Interstates on pedestrian fatalities is due to sorting on observable characteristics. The effects of Interstates are shown to be localized, with many fatalities occurring directly on Interstates and spillover effects decaying rapidly as distance from the Interstate increases. We find evidence that changes in the neighborhood built environment induced by Interstate construction and increases in vehicle speeds on local roads as a result of these changes drive the spillover effects. Further, we find evidence that the Interstate Highway System contributed to racial disparities in pedestrian fatalities. After correcting for endogenous route placement and controlling for contemporaneous tract demographics, we estimate that operating an Interstate in a tract increased Black pedestrian fatalities at more than double the rate of white pedestrian fatalities. While both groups are harmed by Interstate-induced pedestrian safety risks, Black residents are more vulnerable to being killed by a vehicle and carry a larger share of the costs.

We conclude that an alarming number of pedestrian deaths can be linked to the Interstate Highway System bisecting neighborhoods in the US. These deaths may be from an increase in overall pedestrian deaths, a spatial redistribution of deaths towards Interstates, or a combination of these effects. While Interstates provide a host of benefits, they play a role in the large number of, and racial disparities in, pedestrian fatalities in the US. With cities considering the demolition of some urban highways, our insights help quantify an important local disamenity of highways.

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Appendices

A Robustness Checks

In this appendix we provide results from several robustness checks. Table A1 provides results of the IV regression when the dependent variable is transformed with an inverse hyperbolic sine function. The estimates from Inverse Hyperbolic Sine transformed variables are often reported as semi-elasticities; however, we follow Bellemare and Wichman (2020) in deriving semi-elasticities for interpretation here. The results remain positive and statistically significant. The OLS specifications in columns 1 and 2 estimate that receipt of an Interstate increases local pedestrian fatalities by 40%–51%. Again, the IV specifications in columns 3 and 4 are larger in magnitude, estimating a causal effect of an Interstate on local pedestrian fatalities between 119%–163%. The difference in magnitude of the effect between column 3, which does not control for equilibrium sorting, and column 4, which eliminates the variation attributable to sorting, is consistent with our main specifications — sorting on observables appears to account for 27% of the effect.

Table A2 provides results where the dependent variable has been transformed into fatalities per 1,000 local residents. We find qualitatively similar results in magnitude and statistical significance to our main results in Table 3. The similarity of results is perhaps not surprising as tracts are drawn to contain approximately 4,000 people and we control for population in some specifications.

Table A3 replaces the 2019 ACS 5-year control variables with comparable controls from the 2014 ACS 5-year estimates. We find very similar results, suggesting that our results are not driven by the sampling period of the control variables.

Table A1: Effect of a Local Interstate on Pedestrian Fatality Count: Inverse Hyperbolic Sine Transformed Dependent Variable

	(1)	(2)	(3)	(4)
Local interstate	0.412**	0.337**	0.966**	0.786**
	(0.014)	(0.013)	(0.083)	(0.078)
Local noninterstate highway		0.142**		0.259**
		(0.016)		(0.026)
Population (1,000)		0.069**		0.064**
		(0.003)		(0.003)
Population density (1,000/sq. km)		-0.021**		-0.014**
		(0.005)		(0.003)
Number of jobs (1,000)		0.031**		0.024**
		(0.003)		(0.003)
Commute share: car		0.083		0.033
		(0.118)		(0.118)
Commute share: walk		0.341*		0.424**
		(0.144)		(0.149)
Commute share: public transit		0.901**		0.858**
		(0.190)		(0.194)
Median income (10,000)		-0.025**		-0.026**
		(0.007)		(0.007)
Poverty rate		0.240**		0.278**
		(0.086)		(0.085)
White population share		-0.314**		-0.314**
		(0.083)		(0.081)
Black population share		-0.199*		-0.213*
		(0.095)		(0.093)
Asian population share		-0.414**		-0.503**
		(0.116)		(0.112)
Hispanic population share		-0.070		-0.102
		(0.086)		(0.088)
College education share		-1.022**		-0.975**
		(0.055)		(0.056)
No car share		0.173		0.089
		(0.107)		(0.109)
County fixed effects	Y	Y	Y	Y
IV	N	N	Y	Y
Kleibergen-Paap F stat	-	-	319.726	274.945
N	71508	71508	71508	71508

Significance levels: $*$: 5% $**$: 1%. Robust standard errors clustered at the county level in parenthesis. Dependent variable is the inverse hyperbolic sine of the number of pedestrian fatalities that occurred in the tract between 2001-2020.

Table A2: Effect of a Local Interstate on Pedestrian Fatalities Per 1,000 Local Residents

	(1)	(2)	(3)	(4)
Local interstate	0.249**	0.202**	0.608**	0.524**
	(0.014)	(0.012)	(0.070)	(0.061)
Local noninterstate highway		0.071**		0.155**
		(0.012)		(0.021)
Population (1,000)		-0.047**		-0.051**
		(0.002)		(0.002)
Population density (1,000/sq. km)		-0.023**		-0.018**
		(0.004)		(0.003)
Number of jobs (1,000)		0.029**		0.024**
		(0.004)		(0.004)
Commute share: car		-0.214		-0.251*
		(0.118)		(0.118)
Commute share: walk		0.075		0.135
		(0.165)		(0.168)
Commute share: public transit		0.476**		0.446**
		(0.155)		(0.157)
Median income (10,000)		-0.005		-0.006
		(0.004)		(0.005)
Poverty rate		0.242**		0.269**
		(0.077)		(0.076)
White population share		-0.237**		-0.237**
		(0.079)		(0.078)
Black population share		-0.135		-0.145
		(0.090)		(0.089)
Asian population share		-0.314**		-0.378**
		(0.103)		(0.101)
Hispanic population share		-0.087		-0.110
		(0.069)		(0.070)
College education share		-0.658**		-0.625**
		(0.041)		(0.041)
No car share		0.354**		0.294*
		(0.118)		(0.116)
County fixed effects	Y	Y	Y	Y
IV	N	N	Y	Y
Kleibergen-Paap F	-	-	319.726	274.945
N	71508	71508	71508	71508

Significance levels: $*$: 5% $**$: 1%. Robust standard errors clustered at the county level in parenthesis. Dependent variable is the number of pedestrian fatalities that occurred in the tract between 2001-2020 divided by the tract's population in 1,000s.

Table A3: Effect of a Local Interstate on Pedestrian Fatality Count: ACS 2014 Estimates

	(1)	(2)	(3)	(4)
Local interstate	0.961**	0.725**	2.521**	1.930**
	(0.050)	(0.039)	(0.265)	(0.224)
Local noninterstate highway		0.258**		0.573**
		(0.044)		(0.069)
Population		0.156**		0.141**
		(0.007)		(0.007)
Population density (1,000/sq. km)		-0.055**		-0.036**
		(0.013)		(0.010)
Number of jobs (1,000)		0.099**		0.079**
		(0.014)		(0.013)
Commute share: car		-0.669*		-0.694*
		(0.292)		(0.297)
Commute share: walk		-0.371		-0.052
		(0.390)		(0.410)
Commute share: public transit		1.269*		1.220*
		(0.524)		(0.535)
Median income (10,000)		-0.058**		-0.059**
		(0.018)		(0.019)
Poverty rate		0.996**		1.061**
		(0.222)		(0.223)
White population share		-0.727*		-0.743*
		(0.312)		(0.313)
Black population share		-0.530		-0.580
		(0.343)		(0.347)
Black population share		-1.007**		-1.274**
		(0.376)		(0.393)
Hispanic population share		-0.333		-0.426
		(0.245)		(0.245)
College education share		-2.494**		-2.352**
		(0.164)		(0.157)
No car share		0.279		0.175
		(0.292)		(0.296)
County fixed effects	Y	Y	Y	Y
IV	N	N	Y	Y
Kleibergen-Paap F stat	-	-	320.052	273.663
N	71521	71521	71521	71521

Significance levels: $*$: 5% $**$: 1%. Robust standard errors clustered at the county level in parenthesis. Dependent variable is the number of pedestrian fatalities that occurred in the tract between 2001-2020.

Tables A4 and A5 examine how the effects of an Interstate vary based on population density. We separately estimate the specification in column 4 of Table 3 for each quartile of the population density distribution to roughly examine how urbanization levels impact the results. Table A4 presents results from the first stage while Table A5 presents results from the second stage.

Interestingly, Table A4 illustrates that the strength of the planned Interstate instrument decreases across the population density quartiles. The instrument appears strongest in low-density areas (column 1) and weakest in high-density areas (column 4). More densely populated areas may have been more able to form opposition to Interstate placement, thus creating more deviations from the 1947 plan in these areas. However, this facet of the data would suggest that much of the identification in our own as well as other empirical works using the 1947 Interstate Plan as an instrument comes from areas with lower population density. While this limitation alone is not a threat to identification, it should be considered when assessing the implications of empirical findings.

As can be seen in Table A5, the effect of an Interstate on pedestrian fatalities increases dramatically with density — an Interstate in a tract in the first quartile increases pedestrian deaths by 1.1 but an Interstate in a tract in the fourth quartile increases pedestrian deaths by 4.8. These results suggest Interstates cause more pedestrian deaths in more densely populated tracts.

Table A6 includes controls for an area's "walkability." Walkability data were obtained from the EPA's Smart Location Mapping database. The index ranks block groups according to their relative walkability as measured by factors like the diversity of employment types, occupied housing, commute modes, intersection density, etc. By controlling for walkability, we can remove some of the effect of urban form, which may be correlated with Interstate locations. Unfortunately, not all tracts are included in the EPA's data, and the included tracts skew towards more urban areas. Because missing data significantly reduces the sample size in Table A6, columns 1 and 3 replicate the main regression results in columns 3 and 4 of

Table A4: Effect of a Local Interstate on Pedestrian Fatality Count by Density Quartiles: First Stage

	(1)	(2)	(3)	(4)
	1st Quartile	2nd Quartile	3rd Quartile	4th Quartile
Planned local interstate	0.289**	0.095**	0.066**	0.048*
	(0.015)	(0.015)	(0.015)	(0.020)
Local noninterstate highway	-0.230**	-0.347**	-0.288**	-0.180**
	(0.012)	(0.014)	(0.015)	(0.015)
Population (1,000)	0.008**	0.008**	0.006**	0.007*
	(0.002)	(0.002)	(0.002)	(0.003)
Population density (1,000/sq. km)	-0.028	-0.131**	-0.136**	-0.010**
	(0.133)	(0.017)	(0.012)	(0.002)
Number of jobs (1,000)	0.027**	0.020**	0.015**	0.005**
	(0.005)	(0.002)	(0.002)	(0.001)
Commute share: car	0.183*	0.289**	0.292**	0.146
	(0.085)	(0.102)	(0.094)	(0.092)
Commute share: walk	-0.024	-0.119	0.051	0.196
	(0.143)	(0.142)	(0.131)	(0.126)
Commute share: public transit	0.004	0.309	0.551**	0.134
	(0.249)	(0.199)	(0.133)	(0.103)
Median income (10,000)	0.021**	-0.004	-0.013**	0.003
	(0.008)	(0.006)	(0.005)	(0.004)
Poverty rate	0.040	0.115	0.068	0.110
	(0.093)	(0.079)	(0.075)	(0.061)
White population share	0.027	0.012	-0.081	-0.081
	(0.051)	(0.084)	(0.072)	(0.058)
Black population share	0.171**	0.163	0.008	-0.062
	(0.065)	(0.093)	(0.085)	(0.067)
Asian population share	0.270	0.329**	0.242**	0.112
	(0.240)	(0.119)	(0.090)	(0.092)
Hispanic population share	0.218**	0.254**	0.213**	0.043
	(0.053)	(0.053)	(0.047)	(0.062)
College education share	0.018	0.039	-0.067	-0.052
	(0.048)	(0.048)	(0.045)	(0.052)
No car share	0.096	0.289*	0.132	0.150*
	(0.110)	(0.122)	(0.102)	(0.076)
County fixed effects	Y	Y	Y	Y
IV	Y	Y	Y	Y
N	17877	17877	17877	17877

Significance levels: $*$: 5% $**$: 1%. Robust standard errors clustered at the county level in parenthesis. Dependent is an indicator equal to one if the tract has an Interstate.

Table 3 but using the reduced sample. We find a positive and statistically significant effect of a local Interstate on pedestrian fatalities, but the magnitude of this effect is larger than that

Table A5: Effect of a Local Interstate on Pedestrian Fatality Count by Density Quartiles: Second Stage

	(1)	(2)	(3)	(4)
	1st Quartile	2nd Quartile	3rd Quartile	4th Quartile
Local interstate	1.127**	2.075**	2.304*	4.764
	(0.164)	(0.685)	(0.964)	(2.455)
Local noninterstate highway	0.496**	0.636**	0.664*	0.969*
	(0.079)	(0.231)	(0.288)	(0.429)
Population (1,000)	0.186**	0.103**	0.133**	0.175**
	(0.012)	(0.012)	(0.013)	(0.022)
Population density (1,000/sq. km)	-2.591**	-0.200	0.082	-0.002
	(0.611)	(0.127)	(0.139)	(0.023)
Number of jobs (1,000)	0.127**	0.103**	0.070**	0.029
	(0.030)	(0.025)	(0.015)	(0.016)
Commute share: car	0.233	-0.252	-0.319	-1.878**
	(0.379)	(0.514)	(0.528)	(0.645)
Commute share: walk	-0.647	0.498	0.826	-0.911
	(0.723)	(0.705)	(0.642)	(0.861)
Commute share: public transit	3.130	3.148**	1.617	0.008
	(1.689)	(0.957)	(0.844)	(0.801)
Median income (10,000)	-0.068	-0.021	0.005	-0.006
	(0.041)	(0.022)	(0.024)	(0.031)
Poverty rate	0.350	0.550	1.535**	-0.055
	(0.452)	(0.471)	(0.458)	(0.430)
White population share	-2.257**	-1.273	-0.471	-0.151
	(0.531)	(0.726)	(0.437)	(0.408)
Black population share	-1.624**	-0.574	-0.188	-0.127
	(0.594)	(0.783)	(0.455)	(0.419)
Asian population share	-1.049	-3.407**	-1.918**	-0.871
	(1.404)	(1.086)	(0.575)	(0.502)
Hispanic population share	0.324	0.422	0.802	-0.611
	(0.375)	(0.505)	(0.437)	(0.440)
College education share	-1.276**	-2.606**	-2.084**	-2.257**
	(0.245)	(0.287)	(0.278)	(0.351)
No car share	0.981	1.049	0.864	-0.232
	(0.567)	(0.917)	(0.562)	(0.603)
County fixed effects	Y	Y	Y	Y
IV	Y	Y	Y	Y
Kleibergen-Paap F stat	384.510	40.925	18.613	5.904
N	17877	17877	17877	17877

Significance levels: $*$: 5% $**$: 1%. Robust standard errors clustered at the county level in parenthesis. Dependent variable is the number of pedestrian fatalities that occurred in the tract between 2001-2020.

estimated in Table 3. Columns 2 and 4 include a control variable for the area's walkability index. The walkability index is estimated to have a positive and statistically significant effect on local pedestrian fatalities, likely capturing a change in behavioral choices in more walkable neighborhoods. In both regressions, the estimated magnitude of the effect of an Interstate on pedestrian fatalities increases when we control for walkability, but the results remain similar.

The exclusion restriction of the instrumental variable requires that the routes of planned Interstates in the 1947 plan are orthogonal to within-county variation in tract-level variables that might predict current levels of pedestrian death. Ideally, we would control for pre-1947 demographics. Unfortunately, historical tract-level US Census information does not cover the entire country but only extends to densely populated areas.

We investigated using 1940 census tract-level data, cross-walked to the 2010 tract boundaries of our sample. However, we were able to match fewer than 10% of our tracts to 1940 data. Using 1950 data, collected three years after the planned Interstate map, we are able to recover demographic information for 11,331 of our tracts (16%). Table A7 provides a balancing test of the subsample of tracts with data that were or were not bisected by a planned Interstate. We find demographics are generally balanced, though the Black population share is 46% higher in the tracts that were intersected by an Interstate. Because planned Interstates converge on central cities, and black populations are over-represented in central cities, we hypothesize the difference is mainly a mechanical function of Black population distribution, rather than the routes being targeted at Black neighborhoods intentionally. This issue is likely pronounced in this small sub-sample as it ignores the 85% of our sample that generally covers less urbanized areas. A discussion related to endogeneity concerns of the instrument is included in the online appendix to Brinkman and Lin (2022). As pointed out there, the 1947 plan represented city centers as circles on the map. The digitization of the map simply extended the trajectory of routes through the circles. Therefore, for central city areas, it is not possible that the routes were chosen with an intentional preference for

Table A6: Effect of a Local Interstate on Pedestrian Fatality Count: Walkability Index

	(1)	(2)	(3)	(4)
Local interstate	3.492**	3.663**	2.576**	2.709**
	(0.832)	(0.820)	(0.731)	(0.716)
Walkability index		0.078**		0.053**
		(0.013)		(0.011)
Local noninterstate highway			0.620**	0.665**
			(0.207)	(0.202)
Population (1,000)			0.126**	0.132**
			(0.014)	(0.014)
Population density (1,000/sq. km)			-0.053*	-0.057*
			(0.024)	(0.025)
Number of jobs (1,000)			0.062**	0.057*
			(0.023)	(0.023)
Commute share: car			-0.817	-1.003
			(0.587)	(0.552)
Commute share: walk			0.565	0.134
			(0.746)	(0.717)
Commute share: public transit			1.409	1.023
			(0.899)	(0.872)
Median income (10,000)			-0.017	-0.007
			(0.022)	(0.023)
Poverty rate			1.680*	1.435*
			(0.683)	(0.672)
White population share			-0.567	-0.532
			(0.297)	(0.304)
Black population share			-0.406	-0.443
			(0.403)	(0.404)
Asian population share			-1.306**	-1.447**
			(0.442)	(0.431)
Hispanic population share			-0.659	-0.778
			(0.498)	(0.493)
College education share			-2.982**	-3.081**
			(0.290)	(0.286)
No car share			0.917	0.982
			(0.930)	(0.921)
County fixed effects	Y	Y	Y	Y
IV	Y	Y	Y	Y
Kleibergen-Paap F	87.961	88.010	71.845	71.717
N	23569	23569	23569	23569

Significance levels: $*$: 5% $**$: 1%. Robust standard errors clustered at the county level in parenthesis. Dependent variable is the number of pedestrian fatalities that occurred in the tract between 2001-2020.

particular neighborhood types.

Table A7: Examining Instrument Demographic Balance using 1950 Census Data

	Mean of Tracts Bisected by a Planned Interstate	Mean of Tracts Not Bisected by a Planned Interstate
Black population share (1950)	0.073	0.050
White population share (1950)	0.924	0.946
Median years of schooling (1950)	10.461	10.800
Median income (1950)	3,404	3,494
Home-ownership share (1950)	0.678	0.669
Median home value (1950)	9,055	9,710
Observations	1,500	9,831

Using 2010 Census tract boundaries we estimate 1950 demographic variables by spatially crosswalking data from 1950 Census tract geographies.

Ideally, we would introduce controls for pre-plan demographics in our regression framework. Table A8 replicates our main IV regression strategy, but includes the 1950 covariates, and is therefore limited to the small subsample where data is available. Interestingly, we find our main result is highly robust within this small subsample. Our first-stage result remains strongly statistically significant after conditioning on 1950 demographics. The corollary estimate to our main result (column 2), shows a local Interstate caused 3.5 additional pedestrian deaths within a tract. This is larger than the 2.5 deaths estimated in the main specification (Table 3, column 3). However, the larger result is consistent with our finding of larger effects in denser areas (Table A5, column 4.)

Limited availability of historical census data prevent us from fully testing for the relationship between historical demographic conditions and the location of planned Interstates in our sample. However, the available data suggests our results are robust to controlling for historical conditions.

Table A9 broadens the definition of treated tracts, providing results that apply to a less local region. We redefine a treated tract as those within designated buffer zones of an Interstate and planned Interstate. This table serves as a corollary to Table 4 which dropped tracts near treated units to investigate spillovers. Column 1 provides our baseline result.

Table A8: Effect of a Local Interstate on Pedestrian Fatality Count, Using 1950 Demographic Control Variables with Reduced Sample

	No Contemporaneous Controls		Contemporaneous Controls	
	First-stage	Second-stage	First-stage	Second-stage
	(1)	(2)	(3)	(4)
Local interstate		3.513**		2.634*
		(1.229)		(1.314)
Planned local interstate	0.093**		0.071**	
	(0.021)		(0.020)	
Black population share (1950)	0.030	1.915	-0.050	1.970
	(0.683)	(2.459)	(0.619)	(1.237)
White population share (1950)	-0.026	2.033	-0.116	2.151
	(0.673)	(2.311)	(0.607)	(1.239)
Median years of schooling (1950)	-0.011	-0.053	-0.006	-0.042
	(0.009)	(0.046)	(0.008)	(0.031)
Median income (1950), $1,000s	-0.009	0.129	-0.004	0.068
	(0.012)	(0.076)	(0.010)	(0.059)
Home-ownership share (1950)	0.106	-0.738**	-0.007	-0.399
	(0.055)	(0.220)	(0.042)	(0.210)
Median home value (1950), $1,000s	-0.008**	-0.037	-0.004	0.029
	(0.003)	(0.027)	(0.003)	(0.021)
Local noninterstate highway			-0.341**	0.885*
			(0.024)	(0.435)
Population (1,000)			0.003	0.160**
			(0.003)	(0.016)
Population density (1,000/sq. km)			-0.029**	-0.036
			(0.006)	(0.036)
Number of jobs (1,000)			0.022**	0.053
			(0.002)	(0.032)
Commute share: car			0.209	-0.879
			(0.145)	(0.533)
Commute share: walk			-0.037	-0.008
			(0.186)	(0.948)
Commute share: public transit			0.089	0.770
			(0.162)	(0.794)
Median income (10,000)			0.008	-0.067**
			(0.006)	(0.025)
Poverty rate			0.082	0.750
			(0.103)	(0.448)
White population share			0.032	0.174
			(0.099)	(0.317)
Black population share			0.061	0.725*
			(0.113)	(0.347)
Asian population share			0.222	-0.542
			(0.143)	(0.478)
Hispanic population share			0.142	-0.019
			(0.094)	(0.452)
College education share			-0.144*	-1.831**
			(0.073)	(0.467)
No car share			0.219	-0.336
			(0.131)	(0.626)
Constant	0.427		0.336	
	(0.668)		(0.609)	
County fixed effects	Y	Y	Y	Y
Kleibergen-Paap F stat	-	19.894	-	13.111
N	11331	11331	11331	11331

Significance levels: $*$: 5% $**$: 1%. Robust standard errors clustered at the county level in parenthesis.

Column 2 includes tracts within 1 km of an Interstate as a treated tract, and columns 3–6 increase the size of the buffer by a km in each successive specification.

As the buffer zone increases, we find that the magnitude of the effect of Interstates on pedestrian fatalities in the treated area decreases. This pattern is in line with the spatial decay of the effect we found in Figure 5. The effects appear largest near Interstates, so including additional area in the treatment definition reduces the average impact of the Interstate on pedestrian fatalities.

Table A9: Effect of a Local Interstate on Pedestrian Fatality Count: Including Nearby Control Tracts as Treated Observations

	(1)	(2)	(3)	(4)	(5)	(6)
Local interstate	2.516**	2.464**	2.122**	1.863**	1.694**	1.677**
	(0.265)	(0.269)	(0.235)	(0.228)	(0.219)	(0.218)
County fixed effects	Y	Y	Y	Y	Y	Y
IV	Y	Y	Y	Y	Y	Y
Control observations added	None	Within 1 km	Within 2 km	Within 3 km	Within 4 km	Within 5 km
Kleibergen-Paap F	90.055	83.780	81.283	66.543	59.879	59.339
N	71508	71508	71508	71508	71508	71508

Significance levels: $*$: 5% $**$: 1%. Robust standard errors in parenthesis, clustered at county.

Table A10 tests for differences in driver behavior attributable to a local Interstate, among drivers involved in a crash that kills a pedestrian. We use the microdata but drop some observations because not all incidents include information on speed and/or alcohol. We repeat the main IV specification of this paper but use measures of vehicle speed rather than pedestrian deaths as our outcome variable. As a corollary to Table 6, columns 1–2 replace the average speed dependent variable with the speed of the vehicle that was traveling fastest. Results suggest that regardless of using the average or maximum vehicle speed and regardless of whether tract level demographic controls are included, a local Interstate increases the speed of the vehicle involved in the fatal pedestrian crash. The finding provides evidence that one of the causal mechanisms linking local highways to pedestrian deaths is that highways cause vehicles involved in pedestrian incidents to be traveling at higher speeds. As a placebo test,

Table A10 also tests for an effect of a local Interstate on the probability that a driver had consumed alcohol prior to being involved in the crash. Whether the driver had consumed alcohol is based on the judgment of the police officer recording the incident. We fail to find an effect of a local Interstate on the likelihood the involved driver had consumed alcohol.

Table A11 specifies the dependent variable as the share of pedestrian fatalities within a tract that was of a particular race. We omit tracts with no pedestrian fatalities from this analysis. While the absolute number of deaths was shown to increase across all groups, a local Interstate is found to cause a 9.7 percentage point decrease in the share of those victims who were white, and an offsetting 8.4 percentage point increase in the share of those victims who were Black.

Table A10: Effect of a Local Interstate on Driver Behavior in Crashes Involving Pedestrians

	Max Speed	Max Speed	Alcohol	Alcohol
Local interstate	12.659**	11.177**	-0.003	-0.000
	(1.794)	(1.795)	(0.018)	(0.022)
Local noninterstate highway		6.398**		-0.005
		(0.927)		(0.009)
Population		0.149**		-0.002*
		(0.054)		(0.001)
Population density (1,000/sq. km)		-0.894**		-0.000
		(0.170)		(0.002)
Number of jobs (1,000)		-0.105**		-0.000
		(0.027)		(0.000)
Commute share: car		-2.166		-0.048
		(2.961)		(0.044)
Commute share: walk		-14.057**		-0.001
		(3.545)		(0.061)
Commute share: public transit		0.554		0.030
		(4.956)		(0.072)
Median income (10,000)		0.355*		0.002
		(0.174)		(0.003)
Poverty rate		-5.121*		0.044
		(2.038)		(0.030)
White population share		-1.069		-0.028
		(1.515)		(0.022)
Black population share		-4.188*		-0.031
		(1.655)		(0.022)
Asian population share		-5.881*		-0.057
		(2.306)		(0.036)
Hispanic population share		-5.031**		0.009
		(1.148)		(0.017)
College education share		-5.935**		0.043**
		(1.278)		(0.016)
No car share		-3.433		0.004
		(2.817)		(0.042)
County fixed effects	Y	Y	Y	Y
IV	Y	Y	Y	Y
Kleibergen-Paap F stat	69.792	60.057	69.792	60.057
N	37926	37926	37926	37926

Significance levels: $*$: 5% $**$: 1%. Robust standard errors clustered at the county level in parenthesis. We include the standard tract level controls in each regression. Speeds are measured in miles per hour and Alcohol is a dummy variable taking a value of one if the reporting officer noted the involvement of alcohol in the crash.

Table A11: Effect of a Local Interstate on Victim Race, Outcome Variable is Share of Tract Fatalities of that Race

	(1)	(2)	(3)	(4)
	White	Black	Asian	Hispanic
Local interstate	-0.097**	0.084**	0.037	-0.000
	(0.032)	(0.026)	(0.021)	(0.011)
Local noninterstate highway	-0.033**	0.033**	0.015*	-0.006
	(0.012)	(0.009)	(0.007)	(0.005)
Population (1,000)	0.004**	-0.003**	0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.000)
Population density (1,000/sq. km)	-0.003**	0.002*	-0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.000)
Number of jobs (1,000)	0.002**	-0.001**	-0.000	-0.000*
	(0.001)	(0.000)	(0.000)	(0.000)
Commute share: car	0.102*	0.021	-0.067	-0.018
	(0.048)	(0.040)	(0.036)	(0.018)
Commute share: walk	0.052	0.116*	0.054	-0.054
	(0.067)	(0.056)	(0.060)	(0.029)
Commute share: public transit	-0.022	0.103	0.040	-0.004
	(0.069)	(0.057)	(0.052)	(0.026)
Median income (10,000)	-0.002	0.007**	0.009**	-0.004*
	(0.004)	(0.002)	(0.002)	(0.002)
Poverty rate	-0.035	0.050	-0.026	-0.001
	(0.039)	(0.032)	(0.028)	(0.017)
White population share	0.252**	-0.045*	0.004	0.007
	(0.035)	(0.019)	(0.029)	(0.010)
Black population share	-0.373**	0.580**	-0.008	-0.002
	(0.049)	(0.035)	(0.030)	(0.012)
Asian population share	-0.249**	-0.027	-0.026	0.494**
	(0.070)	(0.041)	(0.041)	(0.068)
Hispanic population share	0.034	-0.011	0.468**	-0.013
	(0.022)	(0.016)	(0.028)	(0.008)
College education share	0.028	-0.038*	-0.040*	0.012
	(0.023)	(0.017)	(0.016)	(0.010)
No car share	0.046	-0.102**	-0.095**	0.002
	(0.044)	(0.035)	(0.029)	(0.018)
County fixed effects	Y	Y	Y	Y
IV	Y	Y	Y	Y
Kleibergen-Paap F stat	230.883	230.883	230.883	230.883
N	41474	41474	41474	41474

Significance levels: $*$: 5% $**$: 1%. Robust standard errors clustered at the county level in parenthesis.