If you build it, they will drive: Measuring induced demand for vehicle travel in urban areas

Kent Hymel

California State University, Northridge, United States

ARTICLE INFO

ABSTRACT

This paper examines the causal link between highway capacity and the volume of vehicle travel in US urban areas. Estimates from a dynamic panel model suggest that highway capacity expansion generates an exactly proportional increase in vehicle travel. Moreover, induced vehicle travel is expected to revert traffic speeds to pre-expansion levels in approximately five years. To address the simultaneous relationship between lane mileage and highway capacity, this paper develops an identification strategy to account for possible endogeneity bias. A set of instrumental variables measures the degree of influence that state delegations have had on key transportation committees in the US congress. The instruments strongly correlate with highway capacity and are plausibly exogenous, considering the idiosyncratic legislative process in the US. These findings cast doubt on the effectiveness of expanding highways to eliminate traffic congestion, as the speed-related benefits of new capacity tend to be short-lived.

1. Introduction

The distribution of federal highway funds has long been the subject of intense debate among policymakers at all levels of government, and for good reason. Abundant evidence suggests that roads have had wide-ranging effects on, among other things, productivity (Fernald, 1999), trade (Duranton et al., 2014; Allen and Arkolakis, 2014), land use within cities (Baum-Snow, 2007; Duranton and Turner, 2012), and automobile externalities (Parry et al., 2007). Highway infrastructure spending has also played an important role in countercyclical fiscal policy and was a significant part of the 2009 stimulus bill (Leduc and Wilson, 2014). Although highways generate undeniable economic benefits, they are costly to build and spawn a host of negative externalities when they are not managed efficiently. Hence, transportation planners must carefully consider the behavioral response of drivers when expanding highway capacity. How will additional highway capacity change the volume, temporal distribution, spatial distribution, and speed of vehicular travel? This paper measures one such factor: the effect of highway expansion on the volume of vehicle travel in US urban areas.

There is little dispute among transportation researchers that expanding highway capacity increases vehicle use. This phenomenon is commonly known as *induced demand*, and it demonstrates a fundamental economic principle: individuals tend to consume more of a good as the price of the good falls. In other words, wider highways increase

traffic speeds and reduce the time cost of driving, thereby inducing additional vehicle travel. In the short run, when residential and employment locations are fixed, faster peak period highway speeds attract drivers from alternate routes, modes, and times of day. Then, in the long-run, faster speeds encourage additional social and economic behavior in areas made more accessible by the new highway capacity, which further increases traffic volumes.

Research studies since the 1960s have suggested that, because of induced demand, the hoped-for benefits from highway expansion tend to be short-lived and do not provide lasting relief to traffic congestion. Early studies by Downs (1962), Smeed (1968), and Thomson (1977) go so far as to argue that, over time and without any other offsetting deterrent, rush-hour traffic speeds tend to revert to their pre-expansion levels. The finding has even been dubbed the *Fundamental Law of Road Congestion* (Downs, 1962), which asserts that the elasticity of vehicle miles traveled with respect to lane mileage is equal to one, implying that driving increases in exact proportion to highway capacity additions.

A related strand of research examines the direct effect of travel-time savings on the demand for vehicle travel. Road improvements in congested areas induce additional travel indirectly, as drivers ultimately benefit from reductions in the generalized cost of driving brought about by faster traffic speeds (For a review of the literature, see Wardman (2012).). Thus, the elasticity of vehicle mileage with respect to travel-time may serve as a better predictor of future traffic volumes, as some

road improvements occur in uncongested areas and have little impact on the speed of traffic. But accurately measuring that elasticity is challenging as travel times can vary widely across the hours of the day and the days of the week. These practical difficulties have prompted many researchers to use lane mileage as a proxy for time-savings. Hence, induced demand elasticities based on capacity expansions should be correctly interpreted as average effects applying to the road types included in a given study.[1]

The existing literature on induced demand is extensive and includes case studies as well as analyses based on aggregate cross-sectional and time series data. Though rich in detail, findings from individual case studies can be hard to generalize and may have a limited ability to predict the effects of highway expansion in other metropolitan areas. Moreover, highway expansion impacts travel on a city's entire transportation network and may alter traffic on any secondary roads excluded from a case study. In contrast, studies that measure induced demand for a cross-section of metropolitan areas over many years can yield generalizable lessons that are relevant to the national highway policy debate. Nevertheless, estimates of the induced demand elasticity differ considerably across studies and depend on the time horizon, unit of measurement, and empirical model employed. Research using county or state-level panel data generate elasticity estimates that vary widely both within and across studies (Cervero, 2002). For other reviews of the induced demand literature see Goodwin (1996), Noland and Lem (2002), and Graham and Glaister (2004).

Many of these earlier studies, however, do not provide clear identification strategies, making it difficult to uncover the causal links between highway capacity and vehicle travel. Although capacity and travel are highly correlated, it is not plausible to assume that causality flows in a single direction. Transportation planners, for example, do not randomly select which highways to widen. Instead they prioritize improving highway segments with unacceptable levels of traffic congestion or in areas expecting economic growth. Initially, a newly widened highway will attract drivers from other routes, modes of travel, and times of day. Over time, however, vehicle speeds tend to regress as traffic increases, eroding the sought-after congestion relief and encouraging further capacity expansion. Thus, it is not plausible to assume that causality flows in a single direction: highway capacity itself is endogenously determined by the volume of vehicle travel and other factors.

Failing to control for endogeneity in a travel demand model will likely generate biased estimates of the induced demand elasticity, which casts doubt on the validity of studies lacking a credible identification strategy. That said, instrumental variables (IV) has been the prevailing approach to estimating the causal effects of highway expansion. The earliest IV-based studies proposed a variety of instruments for highway capacity, which include lagged values of highway capacity growth (Fulton et al., 2000), the amount of urban land area (Noland and Cowart, 2000), and a combination of political and environmental measures (Cervero and Hansen, 2002). But, good instruments have proven difficult to find and the reliability of the induced demand estimates from these early studies is uncertain, as none tested for bias from weak or invalid instruments.

There are, however, studies that focus attention on the causal relationship between vehicle travel and highway capacity. For example, Duranton and Turner (2011) used an early plan of the US Interstate highway system from 1947 along with a set of rail and exploration routes from the 1800s to generate instruments for urban area lane mileage. Their instrumental-variable based estimates of the induced demand elasticity range from 0.92 to 1.04, which are consistent with the fundamental law. In a set of related studies, the road mileage depicted on the 1947 plan serves as an instrument in much the same way

— Baum-Snow (2007) estimates the effect of highway provision on suburbanization, Michaels (2008) looks at trade barriers and labor market outcomes, and Hymel (2009) examines the effect of traffic congestion on metropolitan employment growth. In those studies, a key argument for instrument validity is the long span of time separating the 1947 highway plan from economic outcomes occurring decades later. Revisiting the map-based approach of Duranton and Turner (2011), Hsu and Zhang (2014) generate instruments from a map of planned highways in Japan, and also find induced demand elasticity estimates that support the fundamental law of road congestion, ranging from 1.24 to 1.34. Other studies have addressed causality with Granger causality tests (Fulton et al., 2000; Melo et al., 2012), simultaneous equations models Noland (2001); Hymel et al. (2010), and propensity scores (Graham et al., 2014). Table 1 summarizes the methodologies and research findings of induced demand studies that controlled for endogeneity bias by using instrumental variables.

To help disentangle causality, this paper examines how scarce highway funds are allocated across competing projects. Policymakers at the federal, state, and local level all exert influence over the distribution of highway funds. A key funding mechanism in the US is the Federal-Aid Highway Program, which redistributes motor-fuel tax revenue put into the Highway Trust Fund. Using a statutory formula, the program apportions federal funds among states to help pay for a variety of surface transportation programs. Key factors in the formula include the size of each state's road network, the amount of vehicle travel, fuel tax revenues, and air pollution levels.[2] Ultimately state and local transportation departments decide where to spend their own portion of the funds.

Do these formulas, written by Congress, accurately reflect highway transportation needs? If so, the funding formulas present further evidence that road building is not exogenous to vehicle travel. However, legislative politics can also create inefficiencies if the formulas redirect funds towards places with influential members of congress and away from places with high priority road projects. To answer that question, this paper develops a set of instrumental variables that measure the degree of influence each state's congressional delegation has had in the US House of Representatives and US Senate.

The remainder of this paper is organized as follows. Section 2 provides background information about highway finance and its relation to politics in the US. Section 3 describes this paper's identification strategy and provides evidence demonstrating the exogeneity and predictive strength of the instrumental variables. To estimate the induced demand elasticity, Section 4 develops a dynamic panel model of urban travel demand across US states between 1981 and 2015. The model addresses important statistical issues including unobserved heterogeneity, endogenous highway capacity, and the dynamic response of vehicle travel to highway capacity expansions. Section 5 presents a robust set of estimates which suggests that, over the long run, highway expansions generate an almost one-for one increase in vehicle travel. That is, the most trustworthy estimates of the induced demand elasticity are very close to one and add further support for the fundamental law of road congestion.

2. Background

Surface-transportation spending bills have historically been contentious, and there is little doubt that members of Congress have strong incentives to secure federal funds for their constituents and to oblige campaign contributors (Levitt and Poterba, 1999; Knight, 2005). Highways are tangible, long-lived, and conspicuous. Because of those features, members can, without much difficulty, point to a popular highway improvement and claim credit for securing the federal funds

---

[1] Hereafter the term *elasticity* refers to the response of vehicle travel with respect to highway capacity, unless otherwise noted.

[2] The US Federal Highway Administration reports the prevailing funding formula in its annual *Highway Statistics* publication.

Table 1
Induced demand elasticity estimates from earlier IV-based studies.

Study	Sample	Identification strategy	Estimator	Elasticity range	Time period
Internal instrument					
Fulton et al. (2000)	Counties in the US Mid-Atlantic (1969–1996)	Lagged growth in highway capacity	FE	0.56–0.59	Short run
			2SLS FE	0.46–0.51	Short run
External instruments					
Noland and Cowart (2000)	US urbanized ureas (1982–1996)	Urbanized land area inverse population density	2SLS FE	0.28	Short run
			2SLS FE	0.90	Long run
External instruments					
Cervero and Hansen (2002)	Urban counties in California (1976–1997)	Measures of geography, politics, and air quality	3SLS FE	0.79	5 year
External instruments					
Duranton and Turner (2011)	US Metropolitan Statistical Areas (1983–2003)	The 1947 US Interstate Highway system plan and mapped rail and exploration routes from 1835 to 1898	OLS	0.82–0.86	10 year
			FE	0.95–1.12	10 year
			2SLS ML	0.94–1.03	10 year
Internal instruments					
Melo et al. (2012)	US urbanized ureas (1982–2010)	Lagged levels and differences of the dependent & independent variables	GMM	0.98	Long run
External instruments					
Hsu and Zhang (2014)	Urban employment areas in Japan (1990–2005)	Japan's 1987 National Expressway Network Plan	OLS	1.02–1.17	3–5 year
			FE	1.13–1.14	
			2SLS ML FE	1.24–1.34	
Internal instruments					
Graham et al. (2014)	US urbanized ureas (1985–2010)	Lagged levels and differences of the dependent & independent variables	PS RE	0.77	Long run
			OLS	0.76	Long run
			FE	0.53	Long run
			GMM	0.61	Long run

Notes: Studies typically reported results using multiple estimators and model specifications. The following conventions distinguish the different estimators employed in a given study: OLS = pooled ordinary least squares; 2SLS = two-stage least squares; 3SLS = three-stage least squares; GMM = generalized method of moments; ML = Maximum likelihood; PS = propensity scores; FE = fixed effects; RE = random effects. The elasticity range refers to the minimum and maximum elasticity estimate for a given estimator across different model specifications. Time horizon refers to the length of time a study assumes will transpire before the effects of induced demand will play out.

that helped finance it (Grimmer et al., 2012). Moreover, members of congress who steer funds to their home districts, do so to court voters and win reelection (Levitt and Snyder, 1997). For example, drivers who endure intense traffic congestion would likely benefit from additional highway capacity funded by federal tax dollars. Likewise, individuals residing in congestion-free areas may also derive benefits from highway projects, which can stimulate regional economies and generate employment.

That said, increased federal funding for highways may not appeal to the voters who bear the external costs of vehicle use (e.g., air pollution and noise). In addition, new highway capacity can increase accessibility and widen the spatial size of metropolitan area labor markets, which may redistribute jobs and other economic activity from one place to another. The uneven distribution of these costs and benefits across individuals suggest that expanding capacity will generate both winners and losers. Consider a member of congress who secures funds for a highway project in their home district. Among that member's constituents, the number of winners (i.e., those deriving positive net benefits from the project) would be expected to exceed the number of losers. For one, paying for a local highway project with federal funds spreads the capital costs across all US taxpayers, while the district's constituents enjoy most of the benefits. That imbalance incentivizes members of congress to obtain federal funding for projects back home that would otherwise fail benefit cost tests. Moreover, incumbents seeking reelection would be unwise to sponsor highway projects that a majority of their electorate disapprove of.

Members of Congress use a variety of legislative maneuvers to secure Federal-Aid Highway funds for their constituents. First and foremost, members shape the funding formulas through the political process, which rewards states or districts with senior, high-ranking, and majority party members (Moore and Hibbing, 1996). Although the funding formulas calculate each state's allocation of highway funds using objective measures, Congress can also rewrite or adjust the formulas, and has typically done so when drafting long-term highway

funding bills (Shatz et al., 2011). Members of key transportation-related committees wield even more influence because they can markup or add amendments to any highway-related bill and can directly earmark funds for specific projects in their home district.

Groups from across the political spectrum have assailed both the formulas and earmark spending for inefficiently distributing highway funds. Estimates by Knight (2004) suggest that the annual cost of federal highway spending exceeds benefits by $7 billion while Cooper and Griffith (2012) estimate that nearly one quarter of formula funds were distributed according to factors unrelated to objective transportation needs in 2010. Instead the federal government distributed more than 10 billion dollars through a set of complicated bonuses and minimum guarantees, which were intended to promote equity across states.[3] The factors used in the funding formula have also been criticized for being poor indicators of actual highway needs. For example, prior to 1980, the formula used a state's total number of center-line road miles to measure the extent of the highway system. Although important, that variable does not adequately measure urban highway capacity, which depends not only on length, but also the number of lanes a road has.

Furthermore, the funding formulas have also created so-called "donor" and "donee" states, which result from the difference between fuel tax revenues a state pays into the highway trust fund and what it gets back in the form of grants. Between 1956 and 2016, the cumulative grant allocations given to eight predominantly rural states were more than twice as large as their cumulative payments into the trust fund (Zhu and Brown, 2013).[4] As many critics claim, the perennial imbalance between tax payments and highway grants suggests that the

---

[3] The apportionment formulas in effect during 2015 can be found at https://web.archive.org/web/20170831060313/https://www.fhwa.dot.gov/policyinformation/statistics/2015/fa4a.cfm.

[4] Of those eight donee states, six were mostly rural: North Dakota, South Dakota, West Virginia, Alaska, Vermont, and Montana. The two mostly urban states were Rhode Island and Hawaii.
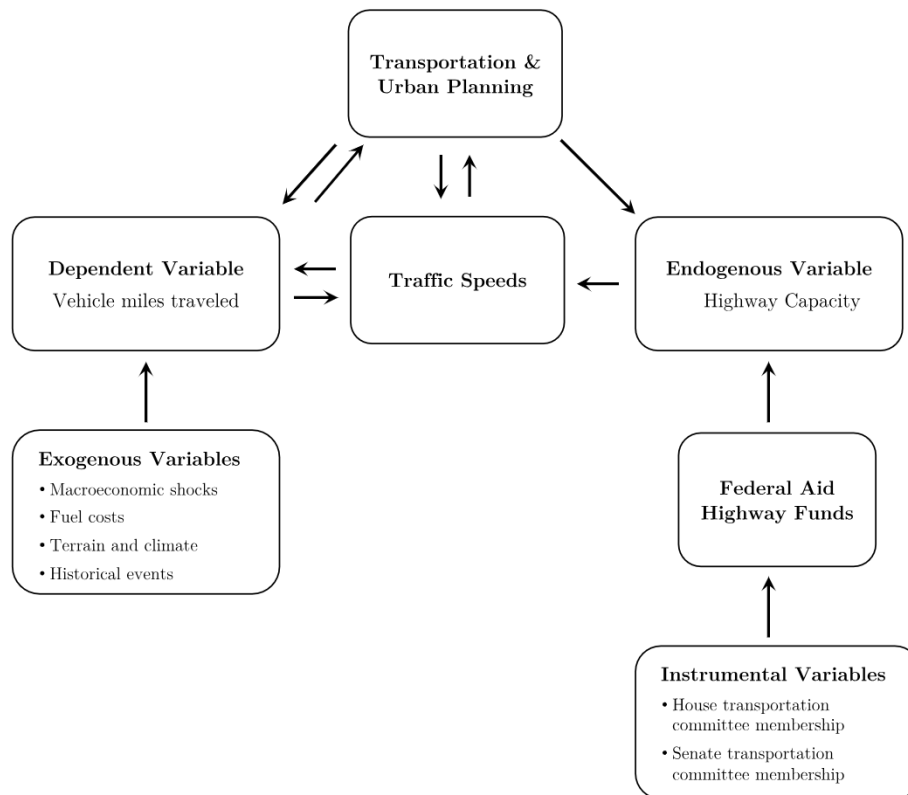
Fig. 1. Hypothesized causal relationships between the empirical model's key variables.

formulas are not efficiently allocating funds to the states where highway needs are greatest (Knight, 2004; Coopert and Griffith, 2012).

Although susceptible to manipulation and inefficient, the highway funding formulas distribute federal money to states based on a set of observable measures, which are straightforward to include in a regression model. Obtaining unbiased estimates of the induced demand elasticity, then, depends more critically upon accounting for unexplained variation in vehicle travel that correlates with the explanatory variables. The byzantine nature of the US legislative process bolsters the identification strategy developed in this paper: earmarks, vote-trading, and omnibus spending bills all serve to divert highway funds away from the most beneficial transportation projects and towards districts with influential delegates.

3. Research design

To help motivate the empirical model and identify an appropriate set of explanatory variables, this section develops a conceptual framework to illustrate the relationships between highway capacity, vehicle miles traveled, and other relevant factors. Fig. 1 presents a directed graph depicting the hypothesized causal linkages between vehicle miles traveled and its determinants. Each of the diagram's arrows indicate the presence and direction of a first-order causal link between two variables. The absence of an arrow linking two variables implies that no *direct* causal relationship exists, although the two items may be indirectly linked through one or more intermediate variables.

Consider equilibrium traffic speed, which is simultaneously determined by highway capacity, the demand for vehicle travel, and other traffic-demand management programs. As Fig. 1 shows, the amount of highway capacity has a direct positive effect on traffic speed in the short-run. However, the reverse causal effect does not follow the same path, because traffic speed effects highway capacity through intermediate channels. For example, suppose there was an abrupt and

permanent fall in fuel prices. Traffic congestion would worsen as individuals, responding to the price shock, undertake more vehicle travel. The decline in traffic speed would impact highway capacity indirectly, by influencing transportation officials' long-term plans for highway infrastructure improvements.

Developing an empirical model of these interrelated causal linkages is not straightforward, making it difficult to obtain credible estimates of the induced demand elasticity. To obtain precise and unbiased induced demand elasticity estimates, it is important to control for the many exogenous factors that help determine vehicle miles traveled. Exogenous variables that influence travel demand include macroeconomic and geopolitical events that occur at the national or global level. Such variables would include economic recessions, changes in US trade policy, and fuel price volatility.

The exogeneity of these variables, however, requires states to be *atomistic*. In other words, states must be numerous enough, such that a sizable economic shock occurring in one urban area will have little impact on the larger national economy. For example, suppose that mass layoffs at a single manufacturing plant cause that area's VMT to sharply decline. Atomism implies that the effect of the layoffs would remain localized, having no discernible impact on unemployment rates or VMT measured nationally. In the current context, a group of 51 states[5] spread over a wide geographic area is numerous enough to assume atomistic behavior and accept the exogeneity of variables measuring economic conditions. Specifications of the empirical model, described in the next section, include controls for fuel prices, per-capita income levels, and the rate of unemployment, which are reliable predictors of aggregate travel demand (Noland, 2001; Hymel et al., 2010).

Other exogenous factors that influence the demand for vehicle travel are fixed at the state level and remain relatively constant over a period of time. Examples of these so-called *fixed-effects* include a state's

---

[5] This figure counts the District of Columbia as a state.

terrain and climate, which influence the quality of alternate forms of transportation such as bicycling and walking. In a similar vein, historical events of the distant past can generate long-lasting effects because of path dependencies. Using panel data and the within-group estimator makes controlling for both state- and year-fixed-effects a straightforward task. Further details of the fixed effects approach are presented in Section 4 below.

3.1. Instrumental variables

Including a rich set of explanatory variables improves the precision of regression estimates and reduces omitted variables bias. But the key variable, highway capacity, is not exogenous, so obtaining unbiased estimates of the induced demand elasticity is challenging. To address the endogeneity of highway capacity, this paper uses an instrumental variables approach. A valid instrument for highway capacity must be strongly correlated with lane mileage and, conditional on the explanatory variables, it must also be exogenous to vehicle miles traveled. Measures of a state's level of political influence can serve as valid instrumental variables, and the evidence presented below demonstrates how they satisfy the strength and exogeneity requirements just mentioned.

In the US Congress, the House Committee on Transportation and Infrastructure (HCTI) and the Senate Committee on Environment and Public Works (SCEPW) yield the greatest influence over highway policy and the distribution of grants.[6] Members of these committees exert influence over highway legislation through two main channels: by shaping the funding formulas that distribute Highway Trust funds and by funding specific projects through earmark spending.

Procedural rules in the US House of Representatives give considerable control over the legislative agenda to individual committees. Acting as gatekeeper, each committee holds hearings and debates before approving (or rejecting) bills falling under its jurisdiction (Davidson et. al, 2013). Moreover, the HCTI retains the sole power to amend, revise, rewrite, or table a highway spending bill before moving it to the House floor for consideration by the full chamber. Once passed by the full chamber, legislation moves to the conference committee. At that stage, the HCTI regains a measure of influence as its members negotiate the final terms of highway spending bills with the SCEPW.

Institutional norms in Congress have also helped consolidate political power within committees. Historically, the House of Representatives has practiced *reciprocal deference*, a system of tacit vote-trading between committees (Krehbiel et al., 1987). In other words, expecting reciprocity, representatives would defer to the policy experts sitting on other committees and not oppose legislation outside of their own jurisdiction. As transportation is a relatively non-partisan issue, reciprocal deference would be expected to dampen opposition to distorted funding formulas and earmarked pet projects, thereby increasing highway funds allocated to HCTI members' districts. These idiosyncrasies of the US legislative system concentrate the power to shape highway policy within the HCTI and to a lesser degree the SCEPW. Indeed, research by Evans (2004), Knight (2002), and Lee (2003) find that members of key transportation committees garner more funding for their constituencies.

The predictive strength of the transportation committee instruments is also reinforced by intrinsic features of highway infrastructure. Urban highway investments and land-use decisions tend to restrict a city's subsequent infrastructure options, creating path dependencies. Consider adding a new link to an existing urban highway network. Some of the major roadway construction tasks – such siting, excavation and grading – permanently alter the terrain and have far reaching

effects on future land use patterns, which makes rerouting or deconstructing urban highways impractical.[7] Furthermore, population growth increases land rents and population densities, both of which raise the cost of constructing new urban highways and limits opportunities for lane widening. Thus, the prevailing spatial structure of an urban highway network depends in large part on initial conditions and the actions of influential policy makers from the past.

These arguments suggest that legislators serving on one of the transportation committees during the inception and early years of the US Interstate highway system would have an outsized and enduring effect on their district's ability to expand highway capacity many years later. This line of reasoning is central to the exogeneity argument in many induced demand studies that rely on historical maps to construct instruments for highway capacity (see e.g., Duranton and Turner, 2011; Garcia-Lopez et al., 2015; and Hsu and Zhang, 2014).

Long time lags also tend to separate the debut of new highway capacity from its associated authorizing legislation. So, construction on an earmark-funded project could continue well beyond its sponsor's term in congress. Likewise, changes to the funding formula in a multi-year reauthorization bill would have little near-term impact on capacity, as states use already obligated funds to pay for road projects. Over time, however, changes to the formula would have a greater impact on capacity as states adjust their transportation plans to accommodate any changes in federal grants. Coupled with path dependency, the long duration of highway legislation, planning, and construction suggest using deeply lagged measures of political influence to instrument for future highway capacity levels. Thus, the cumulative delegate-years of House and Senate transportation committee membership will serve as the political influence instruments. The starting point for measuring committee membership is 1956, which marks the inception of the Interstate Highway System.

The first instrumental variable $H_{i,t}$ measures the cumulative years of HCTI membership possessed by state i between 1956 and year t.

$$H_{i,t} = \sum_{j=1956}^{t} \sum_{k=1}^{435} h_{i,j,k}$$

$$h_{i,j,k} = \begin{cases} 1 & \text{if representative } k \text{ from state } i \\ & \quad \text{served on the House CTI in year } j \text{ otherwise.} \\ 0 \end{cases}$$

Similarly, instrument $S_{i,t}$ measures state i's cumulative membership on the Senate CEPW in year t.

$$S_{i,t} = \sum_{j=1956}^{t} \sum_{k=1}^{100} s_{i,j,k}$$

$$s_{i,j,k} = \begin{cases} 1 & \text{if senator } k \text{ from state } i \text{ served on the Senate CEPW in year } j \\ & \quad \text{otherwise} \\ 0 \end{cases}$$

The assignment data used to construct these two instruments is drawn from committee rosters published by *Congressional Quarterly*.

The political process that assigns delegates to committees could pose a threat to this identification strategy. For example, delegates from

---

[6] The House and Senate appropriations committees determine overall funding levels for federal agencies and programs, but they exert less influence over how grants are distributed across the states.

[7] A meta-analysis conducted by Cairns et al. (2002) examined more than 70 European roadway reallocation projects. They conclude that, contrary to public perception, converting roadways to more pedestrian and transit-friendly uses can reduce automobile use, without degrading driving conditions on the surrounding roads. These projects were usually accompanied by improvements to nearby transit facilities, bus-only lanes, and sidewalks, which presumably served a portion of the displaced automobile traffic. However, some of these conversions also improved the quality of alternate modes of transportation, which makes it difficult to disentangle the causal effect of reducing capacity from the concurrent effect of any improvements to the surrounding area.

districts with high levels of urban traffic congestion may self-select onto or be matched to one of the key transportation committees. If delegates from the most congested districts are systematically assigned to the HCTI or SCEPW, the validity of the instruments is questionable. However, that argument is not supported by any strong empirical evidence.

Congressional delegates, from both urban and rural districts, have a strong incentive to "bring home the bacon" for their constituents when seeking reelection. Highway projects – being tangible, conspicuous, and firmly rooted in one place – make particularly useful examples on the campaign trail, and incumbents do take credit for procuring the requisite funds (Evans, 2004; Grimmer et al., 2012). These features increase the value of seats on the transportation committees because members can earmark federal funds for highways more easily than non-members. Hence, demand for a seat on the HCTI or SCEPW is not limited to delegates from congested urban areas – rural districts may also benefit from extra funds, as construction can boost both employment and the demand for locally-produced inputs.

Indeed, research by Lee and Oppenheimer (1999) found that smaller and more rural states have historically been overrepresented on the HCTI relative to more urbanized states where most congestion occurs. Similarly, each US state has equal representation in the Senate, which amplifies the political influence of committee members from sparsely populated states. These aspects of the committee assignment matching process in the House and the structure of the Senate, do not support the argument that delegates from the most congested districts are systematically assigned to the HCTI or SCEPW.

Many factors unrelated to urban traffic conditions help determine committee assignments, including the delegate's party affiliation, level of seniority, reelection prospects, and policy-area expertise (Frisch and Kelly, 2006; Kellerman and Shepsle, 2009). For example, in the absence of term limits, long-serving members of Congress may be loathe to relinquish their valuable committee posts. Low turnover on committees also reduces the likelihood of seating delegates from states with the most urgent highway needs. Similarly, party and committee leaders make members pay "party dues", via fundraising, to secure a preferred committee assignment (Burgat, 2017). Such actions reward talented fundraisers instead of individuals with useful knowledge about transportation, and inhibits Congress from making intelligent highway policies. Together, these idiosyncratic aspects of the political process render arguments against instrument validity unconvincing.

First stage regression results, presented in Section 5 below, show that a state's cumulative years of representation on the HCIT and SCEPW are both reliable predictors of current lane-mileage, with the effect being stronger in the House. The relatively weaker effect of SCEPW representation is not surprising. The Senate, being more egalitarian, places less importance on member seniority than does the more hierarchical House, which in turn diminishes the importance of cumulative prior representation. Section 5 below presents further evidence from statistical tests supporting the exogeneity and strength of the instruments.

3.2. Data sources

The data used in this analysis include important transportation, geographic, and socioeconomic variables for US States and the District of Columbia measured between 1981 and 2015. The two key data series are drawn from the Federal Highway Administration's (FHWA) annual *Highway Statistics* publications. Highway capacity is measured as the total number of lane-miles residing within Census-defined urban areas in a given state, and is limited to freeways and other limited-access roads. The measure of highway use, vehicle miles traveled, is similarly limited only to driving in urban areas. Table 2 presents summary statistics.

For the early years in the sample, states typically estimated vehicle travel using a combination of fuel tax receipts, traffic counts, and

survey data. Evaluating these data, Schipper et al. (1993) found the FHWA's aggregate vehicle miles traveled measures correspond with those calculated from the National Personal Transportation Survey, lending them credibility. Moreover, as technology has progressed, estimates of vehicle miles traveled have become much more precise. States now use loop detectors embedded in highways and GPS or cell phone tracking to collect a wealth of highly detailed spatial and temporal traffic data.

4. Empirical model and estimation

The main goal of this research is to estimate the induced demand elasticity, but doing so is made difficult by three primary factors. First, including highway capacity or another endogenous variable in a travel demand regression model generates biased coefficient estimates. So, to account for endogeneity bias, this paper employs an instrumental variables approach as described in Section 3.

Second, unobservable factors that influence travel demand are likely to exist, and if not controlled for, will lead to omitted variables bias. To limit such bias, the specifications will include observable travel-demand factors that vary over time and across states. Moreover, the panel nature of the data also permits testing different model specifications for several sources of unobserved heterogeneity. In most specifications, the within-group panel estimator will control for time-invariant state-specific fixed effects (e.g., geographic area, topography, etc.). A set of year dummy variables will control for time-varying effects that are common to every state, (e.g., macroeconomic shocks, oil crises, etc.). Alternatively, travel demand trends may also differ across states, and to account for such heterogeneity some specifications will include state-specific linear time trends.

The third factor complicating estimation stems from transaction costs or other barriers that inhibit drivers from quickly adjusting to shocks, which raises the risk of misspecification if the dynamic nature of travel demand is not correctly modeled. To account for the dynamic nature of vehicle travel and to model driver adaptation, a partial adjustment model will provide estimates of the short and long run induced demand elasticities.

To illustrate the need for a dynamic model, first consider a simple static model of travel demand,

$$V_{it} = \alpha_i + \gamma C_{it} + \beta X_{it} + \varepsilon_{it}; \ (t = 1, ..., T; \ i = 1, ..., N) \tag{1}$$

where V_{it} is urban vehicle-miles traveled for state i in year t, C_{it} is urban lane mileage, and X_{it} is a set of exogenous variables that help determine travel demand. The state-specific effect, α_i, which may be correlated with X_{it}, accounts for time-invariant factors such as a state's climate or topography. Variables V and C are measured in logarithms, so γ measures the induced demand elasticity.

Static model (1) implies that VMT immediately adjusts to its equilibrium level following an increase in highway capacity. But a rapid and complete adjustment may not be feasible if there are transition or learning costs that impede some individuals from altering their driving behavior. For example, a commuter might respond to faster freeway speeds by moving their residence or by changing jobs. But such long-term adjustments are costly and it may take several years for the full effect of a capacity increase to be realized.

The beneficial effects of highway capacity expansion may generate time savings that endure for more than one period, providing a stream of future benefits. Thus, the present discounted value of an expansion is directly tied to the adjustment speed of travel demand. Accurate measures of how rapidly (or slowly) drivers adjust to changes in the transportation network have important policy ramifications. For example, the total time savings generated by capacity expansion depends on the duration of congestion relief, so reliable estimates of the long-run induced demand elasticity are key factors in cost-benefit analyses and long-term transportation planning models.

Panel data sets (with suitably long time series) can help model the

Table 2
Summary statistics (1981–2015).

	Min.	Max.	Mean	Std. Dev.	Source
Vehicle Miles Traveled (millions)	2665	335,539	48,415	52,093	FHWA
Urban Area Lane Miles	1122	253,319	38,534	42,392	FHWA
Population (millions)	0.405	39.144	5.367	5.995	US Census
State Unemployment Level	2.30	17.80	6.08	2.10	BEA
Gas price with taxes (2015 cents)	125.85	467.27	251.05	72.49	EIA
Income per capita (2015 dollars)	7127	73,890	27,106	12,314	BLS
Number of delegates on HCTI	0	9	1.27	1.54	Congress. Quarterly
Number of delegates on SCEPW	0	2	0.42	0.52	Congress. Quarterly

Notes: The summary statistics are based on data measured annually at the state level for the 50 US states and the District of Columbia. HCTI stands for the US House of Representatives Committee on Transportation and Infrastructure, and SCEPW is the Senate Committee on Environment and Public Works.

dynamic aspects of induced demand and can generate useful short and long-run elasticity estimates. Consider the dynamic panel model below, which adds the one year lag of V to model (1):

$$V_{it} = \alpha_i + \phi V_{i,t-1} + \gamma C_{it} + \beta X_{it} + \varepsilon_{it}. \qquad (2)$$

By including lagged VMT as an explanatory variable, the dynamic model can distinguish short-run changes in travel demand from long-run movement towards equilibrium in response to an increase in highway capacity. This model implies that the current level of VMT is not solely determined by contemporary factors, but also by the entire past history of the explanatory variables and error terms subsumed by the lagged VMT term. Hence, the coefficient for lagged VMT, ϕ, measures the year-to-year persistence of travel demand. A value of ϕ close to zero implies low persistence, while a value of ϕ close to one implies high persistence. In each of the partial-adjustment model specifications, vehicle miles traveled and lane mileage are measured in logs, so coefficient β measures the short-run induced demand elasticity, while $\frac{\beta}{1-\phi}$ measures the analogous long-run elasticity.

To control for unobserved heterogeneity common to each state, some specifications employ the within-group estimator, which demeans variables at the state level, wiping out the fixed-effects (α_i). Further, because ε_{it} is correlated with C_{it}, the strict exogeneity assumption does not hold, so (demeaned) model (2) will be estimated by two-stage least squares (2SLS) using the political influence variables (H_{it} and S_{it}) as instruments for endogenous highway capacity (C_{it}). To test the robustness of the results, other specifications of (2) include year dummies, a linear time trend, or state-specific time trends.

Drawing useful inferences from (2) crucially depends on the accuracy of the regression coefficients, and obtaining unbiased and consistent estimates of ϕ and γ is not straightforward. In (2) the fixed effect α_i appears in all observations for state i, and is thus correlated with $V_{i,t-1}$ making the lagged dependent variable endogenous. Taking first differences removes the state fixed effect:

$$\Delta V_{it} = \phi \Delta V_{i,t-1} + \gamma \Delta C_{it} + \beta \Delta X_{it} + \Delta \varepsilon_{it}, \qquad (3)$$

where Δ is the lag operator (e.g., $\Delta V_{it} = V_{i,t} - V_{i,t-1}$).

Although first differencing (2) removes the state fixed effect, the operation reintroduces endogeneity because $\varepsilon_{i,t-1}$ is a part of $V_{i,t-1}$. Hence, $\Delta V_{i,t-1}$ is correlated with the differenced error term, which renders the within-group estimator both inconsistent and biased (Nickell, 1981). This prevalent problem, commonly referred to as *Nickell Bias*, motivated a large literature on dynamic panel econometrics and brought about a range of estimators that are well-understood and frequently used in micro and macroeconomic applications (Arellano, 2003).

Thus, to address the dynamic-panel endogeneity problem, Section 5 also considers estimates produced by the generalized method of moments (GMM) estimator developed by Blundell and Bond (1998). In short, a GMM estimator combines the information contained in an observed sample of data with a set of restrictions implied by theory known as *population moment conditions*. The method generates estimates

by finding the parameter values that make a set of sample moments as close as possible to the corresponding population moments. As the name suggests, GMM is a very general estimation approach as it nests many common estimators (e.g., ordinary and two-stage least squares, maximum likelihood) along with the more complex dynamic panel estimators designed to address Nickell Bias. This useful property makes comparing the results from different models easier, as large-sample GMM estimators are consistent and asymptotically normal, provided both the dependent and independent variables are stationary (Newey and McFadden, 1994).

Although uncorrected Nickell Bias may lead to false inferences, the bias vanishes as the time dimension of the panel data set increases. So, with $T = 34$ annual observations per state, the degree of bias is expected to be small. Indeed, the calculated Nickel bias of the lagged coefficient when $T = 34$ and $\phi = 0.66$ equals -0.050 (Arellano, 2003). Nevertheless, results from the Blundell-Bond estimator will serve as additional robustness checks for the conventional least-squares based estimates.

5. Results

This section presents regression results and induced demand elasticities generated by the dynamic model using both least-squares and GMM based estimators. Across all models, urban vehicle miles traveled and urban lane mileage are in logarithmic form and measured per-capita. The set of control variables includes the state unemployment level, log income per-capita, and log real gas price. Some specifications also include year dummies, a linear time trend, or a set of state-specific time trends.

Table 3 presents estimates for various specifications of the dynamic panel model. Column (I) reports results from the pooled ordinary-least-squares (OLS) estimator, which ignores the panel nature of the data and fails to account for endogenous lane-mileage. That specification produces a statistically significant long-run induced demand elasticity estimate of 0.855, but otherwise performs badly. The unemployment and gas price coefficient estimates are not statistically significant and, contrary to expectations, the coefficient on the income variable has a negative sign. Estimates presented in column (II) are from the within-group estimator, which demeans each of the variables at the state level to control for fixed effects. The resulting long-run elasticity estimate falls slightly to 0.703, and coefficients for the other explanatory variables become statistically significant and take the expected signs.

The long-run induced demand elasticity estimates in columns (I) and (II) are similar in magnitude (0.855 and 0.703 respectively), but the component parts of the long-run estimate suggest substantially different adjustment speeds for drivers.[8] The lagged VMT coefficient from the pooled OLS model in column (I) equals 0.974, which implies that drivers adjust their behavior very quickly following a change in

---

[8] Recall that the expression for the long run elasticity is $\frac{\hat{\beta}}{1-\hat{\phi}}$ where $\hat{\phi}$ and $\hat{\beta}$ are the lagged VMT and lane-mileage coefficient estimates, respectively.

Table 3
Dynamic panel model estimates.

Model	(I)	(II)	(III)	(IV)	(V)	(VI)
Estimator	OLS	OLS	2SLS	2SLS	2SLS	2SLS
State fixed effects		Y	Y	Y	Y	Y
Year fixed effects				Y		
State specific trends						Y
Lagged dependent variable	0.974	0.744	0.663	0.653	0.656	0.639
	(210.93)	(54.92)	(15.99)	(13.06)	(11.38)	(6.41)
Log urban lane-miles per capita	0.0220	0.180	0.349	0.369	0.364	0.322
	(4.17)	(14.22)	(4.22)	(3.77)	(3.14)	(1.84)
State unemployment level	−0.0400	−0.310	−0.394	−0.232	−0.341	−0.331
	(-0.65)	(-4.40)	(-4.67)	(-2.14)	(-4.18)	(-2.91)
Log income per capita	−0.0145	0.0468	0.0463	0.0621	0.0832	0.161
	(-3.81)	(7.78)	(7.31)	(2.07)	(1.98)	(4.81)
Log gasoline price (2015)	−0.00577	−0.0261	−0.0438	0.0347	−0.0424	−0.0476
	(-1.11)	(-4.99)	(-4.32)	(0.96)	(-3.93)	(-3.09)
Linear trend					−0.00159	
					(-0.88)	
Long-run induced demand elasticity	0.855	0.703	1.038	1.063	1.056	0.892
IV diagnostic tests						
First-stage *F*-statistic			22.17	16.77	11.84	5.346
Sargan test statistic $\sim \chi^2$			0.91	0.67	1.60	0.11
First-stage coefficient estimates						
HCTI members per capita			0.0107	0.00897	0.00622	0.0144
			(5.160)	(4.235)	(2.930)	(3.190)
SCEPW members per capita			0.00314	0.00328	0.00395	0.00350
			(1.933)	(2.108)	(2.468)	(0.836)

Notes: The dependent variable is the natural logarithm of urban vehicle miles traveled per capita. The *t*-statistics in parentheses are based on robust standard errors clustered at the state level. Coefficients for the estimated model intercept, year dummies, and state-specific time trends are excluded for brevity.

highway capacity. But the within-group estimates in column (II) yield a smaller lagged coefficient estimate of 0.744, which implies a much slower adjustment speed. Although endogeneity bias renders these baseline results untrustworthy, they serve as reference point to help illustrate the substantial impact that instrumental variables and controls for fixed effects have on induced demand estimates.

The two-stage least squares (2SLS) estimates of the dynamic model are shown in columns (III) through (VI) of Table 3. The specification in column (III) is estimated with the two-stage least squares within-group estimator, using the political-influence instruments. The estimated short and long-run induced demand elasticities are 0.349 and 1.038 (Wald-test p-value < 0.0001) respectively, which provides strong support for the fundamental law of traffic congestion. Additionally, the estimates provide a measure of how quickly traffic levels respond to changes in highway capacity. Following an expansion, urban VMT is expected to achieve 58.0%, 73.4%, and then 90.1% of its ultimate equilibrium level of growth after a span of two, three, and five years respectively. In other words, these estimates predict that the effects of induced demand will set in quickly and new traffic will consume more than 90% of the new highway capacity after just five years.

Columns (IV), (V), and (VI) present estimates from other specifications of the partial adjustment model, and include year dummies, a linear time trend, and state-specific time trends respectively. Overall, the results are robust and change little with additional explanatory variables. When year dummies are included, the estimated long-run induced demand elasticity increases slightly to 1.063 (Wald-test p-value < 0.0001) compared to the estimate of 1.038 from specification (III). Similar results hold for specification (IV), which includes a linear time trend instead of year dummy variables. That specification yields a long run elasticity estimate of 1.056 (Wald-test p-value < 0.0001), which is also nearly identical to the estimates based on specifications (III) and (IV). In specification (VI), the lane mileage and autoregressive coefficient estimates are 0.322 and 0.539 respectively. Together those estimates yield a long-run elasticity estimate of 0.892, which is smaller

than the estimates from specifications (III), (IV), and (V). The most likely explanation for the smaller estimate would be the addition of 51 state-specific time trends, which significantly reduces the degrees of freedom and increases the likelihood of overfitting the model.

The lower panel of Table 3 presents the first-stage results and diagnostic tests to assess the predictive power and validity of the instruments. To make interpreting the coefficients easier, the political influence instruments are measured per 1,000,000 people, which is approximately twice the size of a congressional district in 1990.[9] The results suggest that a state's cumulative stock of HCTI members is a strong predictor of lane-mileage. The estimated coefficient from specification (III) equals 0.0107, which suggests that an additional seat on the committee increases urban lane-mileage per congressional district by 1.07 percent. The cumulative count of senators on the SCEPW has a statistically significant effect in specifications (IV) and (V), but the estimated coefficients are smaller than those for the HCTI instrument. The difference between the estimated coefficients is reasonable given the structure and institutional nature of Congress. For example, the US House of Representatives has historically possessed a greater share of authority over transportation policy, and during the 1974 to 2017 period, the House introduced 71% of the 1085 transportation-related bills that were eventually enacted into law.

Two diagnostic tests also provide support for the strength and exogeneity of the instruments. The first stage *F*-statistics for specifications (III)–(V), reported in the lower panel of Table 3 exceed the Stock and Yogo (2005) critical value of 8.68, showing no evidence of weak instrument bias. Furthermore, results from the Sargan test of the overidentifying restrictions also support instrument validity. The null hypothesis for the test states that the exclusion restrictions hold, and failure to reject the null hypothesis lends support to the validity of the

---

[9] The 1990 US Census counted 249.6 million people; dividing that number by 435 gives an average population per congressional district of 574,713.

Table 4
GMM estimator results.

	(GMM-I)	(GMM-II)	(GMM-III)	(GMM-IV)
Lagged dependent variable	0.786	0.906	0.854	0.828
	(64.24)	(343.98)	(66.41)	(40.72)
Log urban lane-miles per capita	0.187	0.0801	0.104	0.131
	(14.73)	(18.86)	(8.77)	(6.13)
Log income per capita	0.0309		0.0182	0.0624
	(6.91)		(4.14)	(3.47)
State unemployment level	−0.231		−0.179	−0.132
	(-15.38)		(-7.52)	(-5.21)
Log gasoline price (2015)	−0.0237		−0.0135	−0.0149
	(-10.87)		(-4.78)	(-4.61)
Linear trend				−0.00164
				(-2.68)
Long-run induced demand elasticity	0.874	0.852	0.714	0.758
External political instruments used?		Y	Y	Y
Sargan overidentifying restrictions				
– test statistic $\sim \chi^2$	49.64	50.68	48.94	47.29
– degrees of freedom	317	912	912	912
Second degree serial correlation				
– test statistic $\sim N(0,1)$	0.980	1.011	0.991	0.970

Notes: The dependent variable is log urban vehicle miles traveled per capita. The *t*-statistics in parentheses are based on robust standard errors clustered at the state level. Coefficients for the estimated model intercept, year dummies, and state-specific time trends are excluded for brevity. For the Sargan test, the overidentifying restrictions are valid under the null hypothesis. Arellano and Bond (1991) proposed the second degree serial correlation test for the first differenced errors and, under the null hypothesis there is no serial correlation. To reduce the number of instruments, each specification includes a maximum of 10 lags of any endogenous regressor in the instrument matrix.

instruments. None of the tests rejected the null hypothesis. To address potential bias in the standard errors, those reported in Table 3 are robust to heteroscedasticity and within state serial correlation in the error terms.

Overall, the results from the partial adjustment model are robust to adding year dummies or a linear time trend. The estimated urban lane-mileage coefficients for specifications (III), (IV), and (V) are 1.038, 1.063, and 1.056 respectively. Moreover, the three corresponding 99% confidence intervals all encompass the value 1.0, which lends support for the fundamental law of traffic congestion. With one exception, the estimated effects for the other control variables are all measured precisely and retain the expected signs. The gas price coefficient in specification (IV) is not statistically significant, but its effect is likely captured by the year dummy variables which control for shocks that are common across states in a given year.

5.1. Robustness checks

To further test the robustness of the partial adjustment model, Table 4 presents results generated by the GMM estimator of Blundell and Bond (1998) discussed above. In each of the specifications, the dependent variable is the natural logarithm of urban vehicle miles traveled per person. The two key explanatory variables are the one year lag of the dependent variable and the measure of urban lane mileage per person, also measured in logs. Only specification (GMM-I) excludes the political instruments, while (GMM-II) – (GMM-IV) utilize the instruments with different sets of control variables. Across all four specifications, coefficient estimates for the lagged dependent variable range from 0.79 to 0.90, suggesting that aggregate urban vehicle mileage exhibits strong persistence from year to year. This finding likely stems from the transaction costs associated with switching one's mode of transport, residence, or employment location. If these costs are substantial they would impede drivers from quickly adjusting their behavior in response to a change in lane-mileage.

The short-run effect of urban lane mileage on vehicle miles traveled

is measured precisely and ranges from 0.13 to 0.19 across models. The corresponding long-run estimates presented in the bottom panel of Table 4 are more than 4 times larger than their short-run counterparts ranging from 0.71 to 0.87. Although these induced demand estimates are somewhat smaller than their analogous within-group counterparts, they still suggest that induced demand rapidly erodes the time-savings benefits from highway expansion.

6. Conclusion

These findings offer persuasive evidence supporting the fundamental law of traffic congestion, and indicate that capacity expansion is not a viable long-term solution to urban traffic congestion. Across specifications of the dynamic model that controlled for endogenous lane-mileage and state fixed effects, the within-group estimator generated long-run induced demand elasticities ranging between 0.892 and 1.063, all with very small standard errors. These elasticities, along with the other coefficient estimates, are robust to the addition of year dummies, linear time trends, and state-specific time trends. The specifications that excluded the instruments, however, produced considerably smaller long-run elasticity estimates of 0.703 and 0.855. These findings suggests that failing to account for endogeneity can introduce sizable downward bias in the lane-mileage coefficient estimates. Furthermore, results from the dynamic model suggest that after five years, induced vehicle travel is expected to grow to 90% of its equilibrium level, quickly decreasing traffic speeds on the new roadway capacity.

Expanding capacity may be a poor strategy for managing congested freeways, but that finding is not a sufficient rationale for summarily rejecting highway expansion as a policy option. The elasticity estimates in this paper show that capacity expansion in urbanized areas generates long lasting outcomes, both positive and negative. One such outcome is the increase in total vehicle throughput that an expanded highway can serve. Work commutes, deliveries, and social engagements make up much of the induced vehicle travel, which generates beneficial economic activity. At the same time, additional vehicle trips generate offsetting costs in the form of carbon emissions, air pollution, and congestion. And, although these findings support the fundamental law, alone, they do not imply that capacity expansion is wholly unwarranted. Rather, the findings identify and measure some, but not all, of the benefits and costs generated by highways and vehicle travel, and can help officials improve the efficiency of transportation systems in urban areas.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.tranpol.2018.12.006.

References

Allen, Treb, Arkolakis, Costas, 2014. Trade and the topography of the spatial economy. Q. J. Econ. 129 (3), 1085–1140.

Arellano, Manuel, Bond, Stephen, 1991. Some tests of specification for panel data: Monte Carlo evidence and an application to employment equations. Rev. Econ. Stud. 58 (2), 277–297.

Arellano, Manuel, 2003. Panel Data Econometrics. Oxford university press.

Baum-Snow, Nathaniel, 2007. Did highways cause suburbanization? Q. J. Econ. 122 (2), 775–805.

Blundell, Richard, Bond, Stephen, 1998. Initial conditions and moment restrictions in dynamic panel data models. J. Econom. 87 (1), 115–143.

Burgat, Casey, 21 July 2017. Problems with the 'Committee Tax' in Congress. Brookings Institute. www.brookings.edu/blog/fixgov/2017/07/21/problems-with-the-committee-tax-in-congress/.

Cairns, Sally, Atkins, Stephen, Goodwin, Phil, 2002. Disappearing traffic? The story so far. In Proceedings of the Institution of Civil Engineers-Municipal Engineer 151 (1), 13–22 Thomas Telford Ltd.

Cervero, Robert, 2002. Induced travel demand: Research design, empirical evidence, and normative policies. J. Plann. Lit. 17 (1), 3–20.

Cervero, Robert, Hansen, Mark, 2002. Induced travel demand and induced road

investment: a simultaneous equation analysis. J. Transport Econ. Pol. 36 (3), 469–490.

Cooper, Donna, Griffith, John, 2012. Highway robbery: How Congress put politics before need in federal highway and transit funding. Center for American Progress available at. http://www.americanprogress.org/wp-content/uploads/2012/09/CooperRoadsReport-5.pdf.

Davidson, Roger H., Oleszek, Walter J., Lee, Frances E., Schickler, Eric, 2013. Congress and its members. Congressional Quarterly Press.

Downs, Anthony, 1962. The law of peak-hour expressway congestion. Traffic Q. 16, 3.

Duranton, Gilles, Turner, Matthew A., 2011. The fundamental law of road congestion: evidence from US cities. Am. Econ. Rev. 101 (6), 2616–2652.

Duranton, Gilles, Turner, Matthew A., 2012. Urban growth and transportation. Rev. Econ. Stud. 79 (4), 1407–1440.

Duranton, Gilles, Morrow, Peter M., Turner, Matthew A., 2014. Roads and trade: evidence from the US. Rev. Econ. Stud. 81 (2), 681–724.

Evans, Diana, 2004. Greasing the Wheels: Using Pork Barrel Projects to Build Majority Coalitions in Congress. Cambridge University Press.

Fernald, John G., 1999. Roads to prosperity? Assessing the link between public capital and productivity. Am. Econ. Rev. 89 (3), 619–638.

Frisch, Scott A., Kelly, Sean Q., 2006. Committee Assignment Politics in the U.S. House of Representatives. University of Oklahoma Press, Norman, OK.

Fulton, Lewis M., Noland, Robert B., Meszler, Daniel J., Thomas, John V., 2000. A statistical analysis of induced travel effects in the US mid-Atlantic region. J. Transport. Stat. 3 (1), 1–14.

Garcia-Lopez, Miquel-Angel, Adelheid Holl, Viladecans-Marsal, Elisabet, 2015. Suburbanization and highways in Spain when the Romans and the Bourbons still shape its cities. J. Urban Econ. 85, 52–67.

Goodwin, Phil B., 1996. Empirical evidence on induced traffic. Transportation 23 (no. 1), 35–54.

Graham, Daniel J., McCoy, Emma J., Stephens, David A., 2014. Quantifying causal effects of road network capacity expansions on traffic volume and density via a mixed model propensity score estimator. J. Am. Stat. Assoc. 109 (508), 1440–1449.

Graham, Daniel J., Glaister, Stephen, 2004. Road traffic demand elasticity estimates: a review. Transport Rev. 24 (3), 261–274.

Grimmer, Justin, Solomon, Messing, Westwood, Sean J., 2012. How words and money cultivate a personal vote: the effect of legislator credit claiming on constituent credit allocation. Am. Pol. Sci. Rev. 106 (4), 703–719.

Hsu, Wen-Tai, Zhang, Hongliang, 2014. The fundamental law of highway congestion revisited: Evidence from national expressways in Japan. J. Urban Econ 81, 65–76.

Hymel, Kent M., Small, Kenneth A., Van Dender, Kurt, 2010. Induced demand and rebound effects in road transport. Transp. Res. Part B Methodol. 44 (10), 1220–1241.

Hymel, Kent, 2009. Does traffic congestion reduce employment growth? J. Urban Econ. 65 (2), 127–135.

Kellermann, Michael, Shepsle, Kenneth A., 2009. Congressional careers, committee assignments, and seniority randomization in the US House of Representatives. Q. J. Pol. Sci. 4 (2), 87.

Knight, Brian, 2002. Endogenous federal grants and crowd-out of state government spending: Theory and evidence from the federal highway aid program. Am. Econ. Rev. 92 (1), 71–92.

Knight, Brian, 2004. Parochial interests and the centralized provision of local public goods: evidence from congressional voting on transportation projects. J. Publ. Econ. 88 (3-4), 845–866.

Knight, Brian, 2005. Estimating the value of proposal power. Am. Econ. Rev. 95 (5), 1639–1652.

Krehbiel, Keith, Shepsle, Kenneth A., Weingast, Barry R., 1987. Why are congressional committees powerful? Am. Pol. Sci. Rev. 81 (3), 929–945.

Leduc, Sylvain, Wilson, Dan, 2014. Fueling road spending with federal stimulus. FRBSF Econ. Lett. 2014, 25.

Lee, Frances E., 2003. Geographic politics in the US House of Representatives: Coalition building and distribution of benefits. Am. J. Pol. Sci. 47 (4), 714–728.

Lee, Frances E., Oppenheimer, Bruce I., 1999. Sizing up the Senate: The unequal consequences of equal representation. University of Chicago Press.

Levitt, Steven D., Poterba, James M., 1999. Congressional distributive politics and state economic performance. Publ. Choice 99 (1–2), 185–216.

Levitt, Steven D., Snyder Jr., James M., 1997. The impact of federal spending on House election outcomes. J. Polit. Econ. 105 (1), 30–53.

Melo, Patricia, Graham, Daniel, Canavan, Shane, 2012. "Effects of road investments on economic output and induced travel demand: evidence for urbanized areas in the United States. Transport. Res. Rec. J. Transport. Res. Board 2297, 163–171.

Michaels, Guy, 2008. The effect of trade on the demand for skill: Evidence from the interstate highway system. Rev. Econ. Stat. 90 (4), 683–701.

Moore, Michael K., Hibbing, John R., 1996. Length of congressional tenure and federal spending: were the voters of Washington State correct? Am. Pol. Q. 24 (2), 131–149.

Newey, Whitney K., McFadden, Daniel, 1994. "Large sample estimation and hypothesis testing. Handbook of econometrics 4, 2111–2245.

Nickell, Stephen, 1981. "Biases in dynamic models with fixed effects. Econometrica: Journal of the Econometric Society 1417–1426.

Noland, Robert B., 2001. Relationships between highway capacity and induced vehicle travel. Transport. Res. Pol. Pract. 35 (1), 47–72.

Noland, Robert B., Cowart, William A., 2000. Analysis of metropolitan highway capacity and the growth in vehicle miles of travel. Transportation 27 (4), 363–390.

Noland, Robert B., Lem, Lewison L., 2002. A review of the evidence for induced travel and changes in transportation and environmental policy in the US and the UK. Transport. Res. Transport Environ 7 (1), 1–26.

Parry, Ian WH., Walls, Margaret, Harrington, Winston, 2007. Automobile externalities and policies. J. Econ. Lit. 45 (2), 373–399.

Schipper, Lee, Figueroa, Maria Josefina, Price, Lynn, Espey, Molly, 1993. Mind the gap the vicious circle of measuring automobile fuel use. Energy Pol. 21 (12), 1173–1190.

Shatz, Howard J., Kitchens, Karin E., Sandra, Rosenbloom, 2011. Highway Infrastructure and the Economy: Implications for Federal Policy. Rand Corporation.

Smeed, Reuben J., 1968. Traffic studies and urban congestion. J. Transport Econ. Pol. 33–70.

Stock, James, Yogo, Motohiro, 2005. Asymptotic distributions of instrumental variables statistics with many instruments 6 Chapter.

Thomson, John M., 1977. Great Cities and Their Traffic 3 Gollancz, London.

Wardman, Mark, 2012. Review and meta-analysis of UK time elasticities of travel demand. Transportation 39 (3), 465–490.

Zhu, Pengyu, Brown, Jeffrey R., 2013. Donor states and donee states: investigating geographic redistribution of the US federal-aid highway program 1974-2008. Transportation 40 (1), 203–227.