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# Induced Travel Demand: Research Design, Empirical Evidence, and Normative Policies

Robert Cervero

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*Claims of induced travel demand have seemingly paralyzed the ability to rationalize road development in the United States. Methodological issues related to resolution of analysis, measurement, specification, and normative significance are raised in this article. Five types of empirical studies—facility specific, model forecasts, area studies using proxy elasticities, area studies using partial elasticities, and disaggregate analyses—are reviewed and critiqued. Efforts to simultaneously model road supply and demand relationships and identify interaction effects are also reviewed. Based on a meta-analysis, the preponderance of research points to an appreciable induced demand effect; however, problems related to research design continue to cast doubts about our understanding of this phenomenon.*

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HIGHWAYS AND TRAVEL: THE POLICY DEBATE

Few contemporary issues in the urban transportation field have elicited such strong reactions and polarized political factions as claims of induced travel demand. Expanding road capacity is said to spawn new travel and draw cars and trucks from other routes. Consequently, road improvements, critics charge, provide only ephemeral relief—within a few years, most facilities are back to square 1, just as congested as they were

prior to the investment. Failure to account for induced demand likely exaggerates the travel time savings benefits of capacity expansion. Reflecting on the history of highway policies in the United States, Wilfred Owens (1985), echoed this view: “Meeting the ever-growing needs for transport capacity has often proved to be a fruitless task, as the persistence in urban traffic jams attest” (p. 366).

Regional transportation plans, such as in the San Francisco Bay Area, have been legally challenged on the very grounds that they failed to adequately account for the possibility that new road investments might spawn new trips and induce sprawl. Road projects in virtually every U.S. state are being put on hold as charges of induced demand are mediated through the environmental-impact review process. The contention that “you can’t build your way out of traffic congestion” has become a rallying cry of many environmental advocacy groups. In a recent study of seventy metropolitan areas across fifteen years, the Surface Transportation Policy Project (STPP, 1999) concluded that metropolitan areas investing heavily in road capacity expansion

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fares no better in easing traffic congestion than metropolitan areas that did not. Although this and similar analyses can be criticized for inferring causality from simple correlations, such studies nevertheless resonate with politicians because results are comprehensible and transparent.

This article reviews and critiques the state of the field in studying induced travel demand. Three main topics are covered: (1) problems related to research design, (2) empirical evidence and the degree to which it overcomes various research design issues, and (3) policy implications of research conducted to date. Throughout, the focus is on metropolitan settings since the problems assigned to induced demand—like the inability to stave off traffic congestion and curb air pollution—are quintessentially urban in nature. Methodological issues pervade the induced demand policy debate and thus receive a fair amount of attention in this article. Emblematic is the issue of causality—might traffic growth induce road investments every bit as much as vice versa? Some observers point out that for a good century or more, road investments have not occurred in a vacuum but rather as a consequence of a continuing and comprehensive effort to forecast and anticipate future travel demand. Accordingly, road improvements act as a lead factor in shaping, and a lag factor in responding to, travel demand. A recent study by the Urban Transportation Center (1999) at the University of Illinois in Chicago lends anecdotal credence to this position. Using sixty years of data, the study showed that road investments in metropolitan Chicago could be better explained by population growth rates a decade earlier than vice versa. For both the Tri-state Tollway (I-294) and East-West Tollway (I-88), the study concluded that “major population gains occurred in proximity to the expressways over a decade before the construction of the respective expressways” (p. 8).

The core policy debate swirling around induced travel demand is whether it is significant enough in scale and scope to “matter,” and if so, what kinds of policy initiatives need to be introduced to best cope with it. A related policy concern is whether current long-range travel forecasting models adequately account for and encapsulate induced-demand effects. To the degree they do not, the purported benefits of adding or expanding roads (i.e., travel time savings) are likely overstated. To help guide policy on these matters, numerous empirical studies have been carried out seeking to gauge the degree to which induced demand exists, with the pace having picked up, in step with the increasingly heated rhetoric, in recent years. However, methodological and research-design concerns continue to raise questions about the validity and generalizability of past studies, thus hampering the ability to inform

highway investment policies. It is to these analytic challenges, and their implications for policy making, that we now turn.

#### ANALYTIC CHALLENGES

Four theoretical and methodological challenges are often encountered in the study of induced travel demand: (1) resolution, (2) measurement, (3) specification, and (4) normative significance. In combination, these concerns complicate the ability to draw policy inferences on the magnitude and scope of induced travel demand. The discussions that follow focus on matters of research design. For discussions on the economic theory of induced travel demand, see Cohen (1995), European Conference of Ministers of Transport (1998), Hill (1996), Lee et al. (1999), and Noland (2001).

##### *Resolution*

A fine-grain analysis of induced travel will often yield appreciably different results than a coarser grain analysis. Lee et al. (1999) reflect on how the resolution of analysis can influence induced demand estimates:

If the demand is for a single facility, then induced traffic will appear large in relation to previous volumes, because most of the change will be from diverted trips. At the regional level, induced traffic . . . would be a smaller share of total traffic growth because only trips diverted from other regions, plus substitutions between transportation and other goods, make up the induced share. (P. 71)

When diverted trips are netted out of the calculations, however, the induced-demand effect generally becomes bigger as the unit of analysis increases in size. This is because bigger geographic areas capture the impacts of an expanded artery on the capillaries that tie to it, that is, the additional traffic on feeder routes within a tributary area. This is revealed by the work of Hansen et al. (1993), wherein the effects of road capacity on traffic were studied for road segments, counties, and metropolitan areas in California. At the road segment level, estimated elasticities of vehicle miles traveled (VMT) as a function of lane miles were in the following ranges: 0.15 to 0.30 across a four-year horizon, 0.30 to 0.40 across a ten-year horizon, and 0.4 to 0.6 across a sixteen-year horizon.<sup>1</sup> At the county level, short-term elasticities were higher, in the range of 0.32 to 0.50. And at the metropolitan scale of analysis, short-term elasticities edged even higher: 0.54 to 0.61.

##### *Measurement*

Measurement problems abound in studying induced demand. Problems related to sources, spillovers,

boundaries, and variable definitions are reviewed below.

#### SOURCES

Traffic growth comes from numerous sources, some related to increased supply of roads and some not (e.g., exogenous factors like increased labor force participation and commuting among women). Some of the traffic gains spawned by a new road facility are *generative* (e.g., suppressed trips being released), and some are *redistributive* (e.g., diverted trips from parallel routes). Although it is easy to show that road improvements are followed by increased traffic, it is not always easy to show where the traffic came from.

In the near term and in reasonably congested settings, road improvements stimulate what Downs (1962, 1992) termed *triple convergence*—motorists switch modes, routes, and times of day to exploit available capacity. Among these redistributions, only mode shifts add new trips and are thus bona fide contributors to induced demand. Although route and schedule switches may reduce some of the travel time savings conferred by a project, they do not represent new vehicle travel (assuming trips do not become more circuitous). Trips might also lengthen as motorists opt to travel farther because of freer flowing traffic, for example, the discount retailer that is five miles farther from one's house than a higher priced competitor might be worth the extra travel as congestion wanes. Added to this are suppressed (and presumably lower value) trips that are unleashed by faster-moving traffic, also known as *latent demand*. New and longer trips are also part of the induced-demand equation. In the long term, new roads induce structural shifts, namely, the realignment of land development and a tendency toward higher car ownership as a result of more auto-centric landscapes and the decline in transit services. These components are also elements of induced travel.

Most technical studies on induced travel have sought to exclude diverted trips, some more successfully than others. Most have embraced definitions of induced travel similar to that of Schmidt and Campbell (1956): "the added component of traffic volume which did not previously exist in any form, but which results when new or improved transportation facilities are provided" (p. 4).

Short of placing an electronic tag on each traveler affected by a new road and monitoring his or her travel, disentangling the many contributors to increased travel—at least to a high degree of precision—can be a futile exercise (Bonsall 1996). Regardless, some studies have tried their hand at sorting out the sources. Pells's (1989) literature review suggested most redistribution occurred via time-of-day shifts; however, another U.K.

study (Howard Humphreys and Partners 1993) maintained route shifts were the predominant form of redistribution. A study by Kroes et al. (1996) of the opening of Amsterdam's Ring Road and the elimination of a major bottleneck at a tunnel crossing found, in the short run, significant amounts of diverted travel (from off-peak times and other routes) and little truly new travel (in the form of mode shifts or extra trips). On the basis of household surveys in California, Dowling and Colman (1995) suggested near-term redistributions are often a balance of both spatial and temporal shifts. DeCorla-Souza (2000) maintains the lengthening of trips, spawned mostly by land use adjustments, is often the most significant cause of induced travel (when measured as VMT).

Evidence on the redistributive effects of road investments on transit usage is particularly slim. This is partly because intermodal impacts of new highway construction likely unfold slowly and subtly—for example, transit service cuts prompted by a lowering of suburban densities spawned, in part, by highway-oriented patterns of development likely occur sporadically and over a number of years. Cross-elasticities of demand for transit as a function of highway travel times, experiences show, are quite varied—studies have recorded values of 0.36 in the San Francisco Bay Area, 0.42 in Montreal, and 0.84 in greater Chicago (Cervero 1990). Cross-elasticities generally increase with congestion levels, metropolitan size, and quality of transit services. Few studies have traced the path between road expansion, faster automobile speeds, and transit patronage losses. A study on linking a major arterial and freeway in Melbourne, Australia, found some evidence of modal shifts (Luk and Chung 1997). During a seven-year period spanning the opening of a critical network link in one of the region's fastest growing corridors, ridership on the Dandenong train line fell 14 percent compared with a region-wide decline of just 4 percent. Pells's (1989) study of the grade separation of the Westway in London found a 12 percent increase in daily traffic and a 19 percent increase in peak traffic in the corridor. The greater increase in the peak period was attributed to mode shifting from rail.

#### SPILLOVERS

Road facilities do not stand in isolation but rather are links in a hierarchical network. The interdependency of roads means improvements on any one link inevitably have some repercussion on travel demand on other links that feed into it. To capture the spillover impacts of road improvements, many analysts have chosen to study induced-demand impacts at either the metropolitan level or county level. At a metropolitan scale, virtu-

ally all route diversions will be internal to the unit of analysis.

#### BOUNDARIES

To cope with spillover problems, many recent analyses of induced demand have been conducted at an area-wide scale. Aggregation, however, poses potential bias problems since metropolitan and county boundaries are political artifacts that rarely correspond to true travel sheds.

Another boundary-related problem is the focus on roads under state jurisdiction at the exclusion of those owned and maintained by lower levels of government. Rigorous modeling of induced demand requires rich time-series data of travel and road capacity, information that in most instances only state departments of transportation regularly and consistently maintain. Consequently, most metropolitan and corridor-level studies have focused on how traffic changes along state-owned and state-maintained highways (Hansen et al. 1993; Hansen and Huang 1997; Noland and Cowart 2000; Fulton et al. 2000; Cervero and Hansen 2002). Many times, the collectors and arterials that feed into state highway are under local jurisdiction. By default, changes in travel on lower level facilities get ignored altogether. This poses the possibility of spurious inferences since one cannot rule out that purported induced traffic generated on state highways is not matched by reduced travel and shifts from local facilities.

#### VARIABLES

Nearly all induced-demand studies have measured the dependent variable, travel consumption, in terms of VMT. This is partly because VMT is thought to correlate strongly with the social costs of automobile travel. VMT is an imperfect measure for it ignores some elements of trip redistribution, such as rescheduling of trips from the shoulders to the heart of the peak. Although time-of-day shifts do not constitute new trips, they can deteriorate levels of service and thus, one might argue, deserve attention in the induced-demand debate. DeCorla-Souza and Cohen (1999) maintain that negative impacts of auto motoring do not vary significantly by time of day, and thus, the focus of induced-demand studies should be on daily VMT. Others contend that policy concerns like air pollution and greenhouse gas emissions are at least as strongly tied to vehicle hours of travel (VHT) as they are to VMT, thus the effects of new roads on cumulative hours logged should also be considered (Ewing 1995).

Supply-side variables used in induced-demand studies also face measurement problems. Lane miles of capacity are commonly used to represent the benefit of

a highway improvement. In truth, benefits are best expressed by outputs (e.g., travel time savings), not inputs (lane additions). An additional half-mile of lane on a crowded bridge crossing will provide much more benefit than a half-mile of lane in the uncongested exurbs. The notion that lane miles themselves capture supply improvements is presumptuous. The armature that accompanies a road improvement—for example, width of shoulders, provision of attenuation barriers, channelization—will also have a bearing on capacity and travel speeds. In some corridors, improved signal timing, smart-highway provisions (e.g., real-time message boards), ramp metering, or surface repavement might do as much to expedite traffic flows as adding two eleven-foot lanes.

Studies that have used lane miles as a proxy of transportation benefit have usually compiled data only for state facilities. Some have pooled more than two decades of data on state highway capacity to predict traffic growth. Cohen (1995) cautions that redesignations to and from state highways can further introduce statistical noise.

Cohen (1995), DeCorla-Souza (2000), and others have criticized studies that have used lane miles as predictors since travel demand is not tied directly to road capacity but rather the benefits it yields, generally expressed in travel time savings. As discussed later, studies that have employed lane miles as a predictor treat it as a stand-in, or proxy, for travel time savings for practical reasons. Lane miles can generally be measured with a fair degree of accuracy; however, measuring travel time is fraught with difficulties.

When used in elasticity form to gauge benefits, all measures of road improvements fail to adequately account for the scale of projects. For example, in an elasticity calculation, the proportional change in capacity of expanding a road from four to six lanes is expressed the same (in the denominator) for a one-mile road segment as a twenty-mile road segment; in both instances, the relative increase is 50 percent. Surely, the traffic-inducing impacts of a twenty-mile-long project will in most instances be greater than a one-mile-long project. For such reasons, one must be on guard about reading too much into elasticity estimates when passing judgments on the efficacy of road investments.

#### *Specification*

Early studies of induced demand generally tracked percentage increases in traffic without attempting to control for other possible contributory factors. More recent work has sought to isolate out the effects of road expansions on travel demand by statistically removing potential confounding influences. This has generally been done through pooled time-series/cross-sectional

analyses, that is, tracking trends in a panel of counties or metropolitan areas over multiple time points. A case in point is the work of Hansen et al. (1993) and Hansen and Huang (1997). They used econometric models to explain VMT as a function of lagged lane miles of California highways, controlling for factors like population, personal income, residential densities, and gasoline prices. Fixed-effect dummy variables were employed to adjust for the idiosyncratic patterns of VMT growth for a particular county or metropolitan area unaccounted for by the models.

One of the major specification problems confronted by all induced-demand studies is the conflation of cause and effect. Road investments are not made at random but rather as a result of conscious planning based on anticipated imbalances between demand and capacity. This implies that, irrespective of any traffic inducement effect, road supply will generally correlate with road use. Skeptics can easily claim that all or most of the observed relationships between traffic and road investment derive from good planning rather than traffic inducement.

The choice of control variables can also lead to spurious inferences. For example, a transit service variable might be used as a control since riding transit is an alternative to driving. However, one could also argue that poor transit service is an indirect consequence of road investments. In this case, controlling for transit service could mask a potentially important traffic inducement mechanism. Although a few studies, such as Luk and Chung (1997), have attempted to control for the effects of transit service levels on highway travel, none has simultaneously modeled how changes in demand for these two substitutable modes—cars and transit—jointly respond to road expansions. Invariably, such model misspecifications distort elasticity estimates to some degree.

#### *Normative Significance*

One can also question the normative significance of induced-demand research. Even if estimated elasticities are essentially correct, lane mile growth accounts for a small share of VMT growth (Kiefer and Mehndiratta 1998; DeCorla-Souza 2000). In a larger context, induced-demand effects are generally eclipsed by the effects of megatrends on VMT growth, such as rising car ownership, feminization of the labor force, and declining real costs of gasoline. To underscore this point, Heanue (1997) used historical data on travel speeds and a range of elasticity estimates by Goodwin (1996) to illustrate that induced demand accounted for between 6 and 22 percent of VMT growth in Milwaukee from 1963 to 1991. That is, more than three-quarters of VMT growth was attributable to exogenous factors like

maturing baby boomers and declining real gasoline prices that had nothing to do with road expansion.

Another normative concern is that induced-demand studies are partial, focusing only on the cost implications of road expansion. Presumably, the extra VMT induced by new roads is generating some additional surplus that may or may not offset congestion impacts (Small 1992; Hansen 1998; Lee et al. 1999). Whether new roads are on the balance beneficial to society cannot be informed by studies of induced demand; such weighty questions require a full accounting of benefits and costs. In Europe, the general consensus is that “deadweight” losses (in the form of diminished time savings) from induced travel outweigh the collective benefits from making new trips as congestion levels worsen (Standing Advisory Committee on Trunk Road Assessment [SACTRA] 1994; European Conference of Ministers of Transport 1998).

Normative questions also arise as to whether VMT increases spurred by road construction are necessarily bad. “One can certainly envision situations where adding lane miles, by removing some traffic bottleneck, results in both better traffic conditions and a higher VMT per state highway lane-mile ratio” (Hansen and Huang 1997, 217). From an air quality standpoint, higher VMT certainly means more tailpipe emissions; however, in some instances, these impacts might be more than offset by the air quality benefits of expediting flows and eliminating episodes of stop-and-go traffic.

#### NORMATIVE FRAMEWORK

Figure 1 traces the path of road-induced traffic impacts as defined in the literature. The causal chain works as follows: a road investment increases travel speeds and reduces travel times (and sometimes yields other benefits such as less stressful driving conditions, on-time arrival, etc.); increased utility, or a lowering of generalized cost, in turn stimulates travel, made up of multiple components, including new motorized trips (e.g., latent demand previously suppressed), redistributions (modal, route, and time-of-day shifts), and in the longer term, structural shifts such as land use adjustments and increased vehicle ownership rates (which in turn increase trip lengths and VMT). Some of the added trips are new or induced, and some are diverted. Although evidence on the induced-growth effects of new highways is limited (Dunphy 1996; Boarnet 1997; Sanchez et al. 1998; Cervero 2002), roads and prominent fixtures of America’s landscape that they serve—for example, big-box retail, edge cities, and corporate campuses—are clearly codependent. Some observers contend that only newly generated traffic should be treated as induced demand (as portrayed in

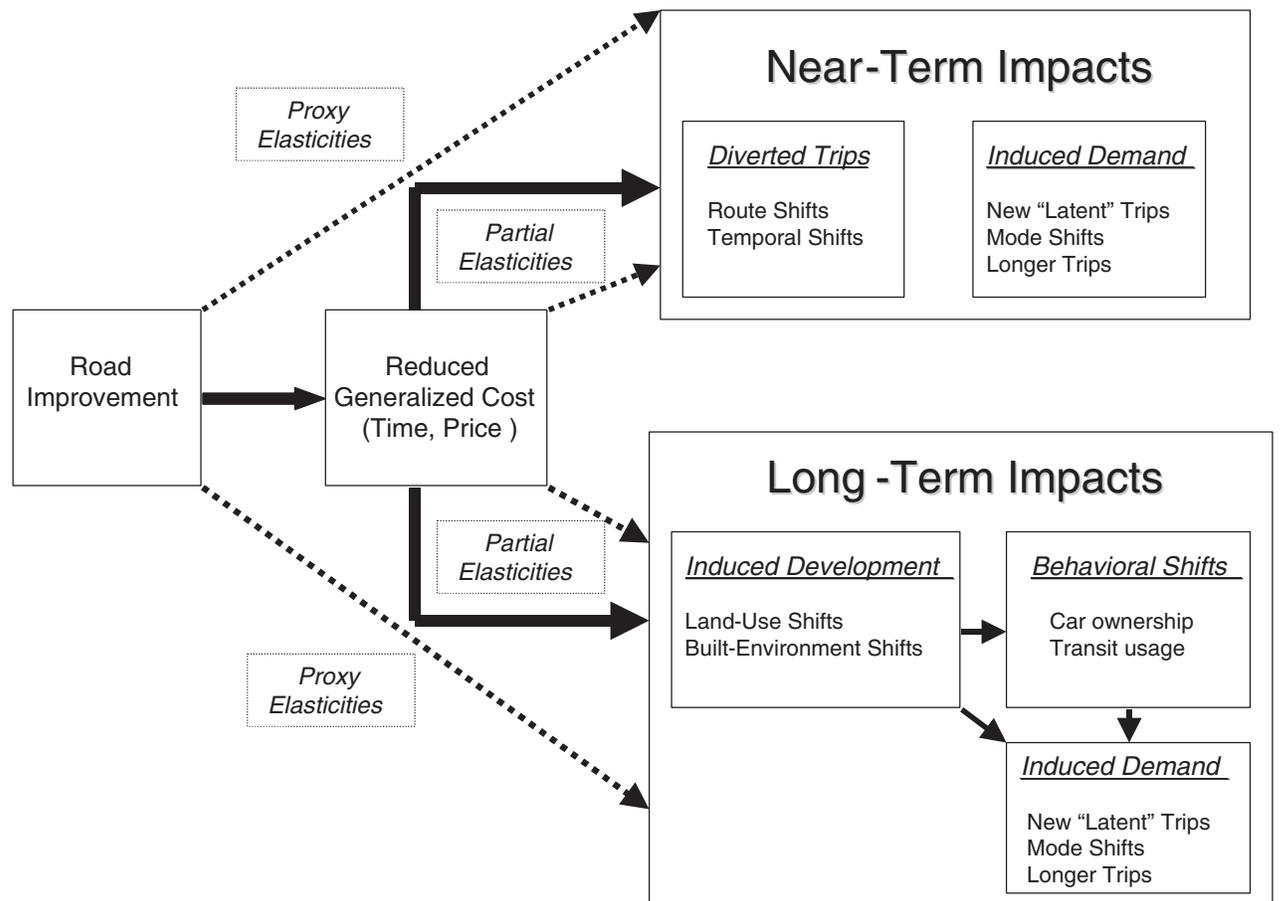


FIGURE 1. Path of Induced Travel Demand and Elasticity Measures

Figure 1) since only newly added VMT increases loads on highway networks. Others counter that all traffic, including redistributions, needs to be counted since route and time-of-day shifts can erode travel time savings even if they do not increase VMT. Most policymakers subscribe to this broader view, wanting to know total effects of new roads on traffic conditions—regardless if the traffic is new, redistributed, or a combination of the two.

To date, empirical studies of induced travel demand have relied on two types of elasticity estimates: *proxy* or *partial* measures.<sup>2</sup> Proxy measures gauge changes in travel demand as a function of changes in road capacity without weighing the fact that the effects of road investments get channeled through their impacts on generalized cost. Partial measures of elasticity can likewise be faulted for incomplete specification. Notably, they often fail to weigh the relative role of road improvements in

bringing about travel time savings and to gauge how this in turn induces travel. They model the intermediate step, but they ignore the relative importance of road improvements (*vis-à-vis* other possible predictors) as the catalyst. Accordingly, despite significant progress made in documenting induced-demand effects during the past few decades, our knowledge of this phenomenon remains incomplete and partial.

#### RESEARCH FINDINGS

This section reviews empirical studies of induced travel demand, with studies divided into five analytic approaches: (1) facility-specific analyses, (2) model forecasts, (3) area studies based on proxy measures, (4) area studies based on partial measures, and (5) disaggregate models. Findings are summarized in Tables 1 (facility-specific analyses) and 2 (aggregate-

TABLE 1. Summary of Facility-Specific Studies of Induced Travel Demand

Study	Setting	Data	Method	Facility Type	Variables		% Growth Attributable to Induced Travel		
					Demand	Supply	ST	IT	LT
Jorgensen (1947)	New York, Connecticut	TS	GC	New parkway	ADT	New facility	25-30		
Lynch (1955)	Maine	TS	GC	Turnpike	ADT	New facility			30
Mortimer (1955)	Chicago, IL	TS	GC	Expressway	ADT	New facility		3-33	
Frye (1964a)	Chicago, IL	TS	MP	Expressway	ADT	New facility			11 <sup>a</sup>
Frye (1964b)	Chicago, IL	TS	MP	Expressway	ADT	New facility			7 <sup>a</sup>
Holder and Stover (1972)	Texas	TS	GC	Highways	ADT	New facility	0-21 <sup>a</sup>		
Pells (1989)	London, UK	TS	MP	Highways	ADT	Widening			27 <sup>a</sup>
Pells (1989)	London, UK	TS	MP	Highways	ADT	Widening		25	56
Pells (1989)	London, UK	TS	MP	Highways	ADT	Upgrade			80
Pells (1989)	London, UK	TS	MP	Expressway	ADT	New facility		77 <sup>a</sup>	
Pells (1989)	London, UK	TS	MP	Tunnel	ADT	New facility			89 <sup>a</sup>
Hansen et al. (1993)	California	TS/CS	GC/Reg	Highways	ADT	Widenings	— <sup>b</sup>	— <sup>b</sup>	— <sup>b</sup>
Kroes et al. (1996)	Amsterdam, NL	TS	MP	Tunnel	ADT	New facility		4.5	
Luk and Chung (1997)	Melbourne, AU	TS	MP	Freeway link	ADT	New facility	0		
Mokhtarian et al. (2000)	California	TS/CS	MP	Highways	ADT	Widenings	0	0	

NOTE: TS = time series; CS = cross section; GC = growth comparison; MP = matched pairs; Reg = regression; ADT = average daily traffic; ST = short term (< 1 year); IT = intermediate term (1-5 years); LT = long term (> 5 years).

a. Thought to include significant amounts of diverted trips.

b. Presented as elasticities: 0.2-0.3 for short and intermediate term; 0.3-0.6 for long term.

TABLE 2. Summary of Area Studies and Simulations with Elasticity Estimates Based on Proxy Supply-Side Measures of Benefit

Study	Setting	Data	Method	Variables		Elasticity Estimates	
				Demand	Supply	Short Term	Long Term
Kassoff and Gendell (1972)	U.S. urban areas	CS	GA	VMT/capita	Capacity Index	< 0.58	—
Koppelman (1972)	20 U.S. cities	CS	Reg: OLS	VMT	Lane miles	0.13	—
Payne-Maxie et al. (1980)	54 U.S. metro areas	CS	Reg: OLS	VMT/capita	Lane miles	0.22	—
Hansen et al. (1993)	30 CA urban counties	TC/CS	Reg: OLS, DL, FE	VMT	Lane miles	0.46-0.50	—
Hansen et al. (1993)	CA metro areas	TC/CS	Reg: OLS, DL, FE	VMT	Lane miles	0.54-0.61	—
Hansen and Huang (1997)	32 CA urban counties	TC/CS	Reg: AR/DL, FE	VMT	Lane miles	0.30	0.68
Hansen and Huang (1997)	CA metro areas	TS/CS	Reg: AR/DL, FE	VMT	Lane miles	0.50	0.94
Noland and Cowart (2000)	70 U.S. metro areas	TS/CS	Reg: IV, DL, FE	VMT/capita	Lane miles/capita	0.66	0.81-1.00
Fulton et al. (2000)	220 counties: MD, NC, VA, DC	TS/CS	Reg: IV, DL, FE	VMT	Lane miles	0.3-0.6 (OLS)	0.47-0.89
Fulton et al. (2000)	220 counties: MD, NC, VA, DC	TS/CS	Reg: IV, AR	VMT	Lane miles	0.13-0.43 (AR)	—
Strathman et al. (2000)	48 U.S. urban areas	CS	Reg: IV	VMT/HH	Lane miles/capita	0.29	—
Cervero and Hansen (2002)	34 CA counties	TS	Reg: 2SLS, DL, FE	VMT	Lane miles	0.56	0.78-0.84
Cervero (2002)	24 CA corridors	TS/CS	Reg: OLS, 2SLS	VMT	Lane miles, speeds	0.10	0.39

NOTE: TS = time series; CS = cross section; GA = graphic analysis; Reg = regression; OLS = ordinary least squares; DL = distributed lag; FE = fixed effects; AR = autoregressive OLS; IV = instrument variables; MD = Maryland; NC = North Carolina; VA = Virginia; DC = Washington DC; 2SLS = two-stage IV estimation.

scale analyses with elasticity estimates). As noted previously, the studies reviewed in this article investigate induced demand from a metropolitan perspective; studies from rural settings or conducted at a national scale (e.g., Noland 2001) are not examined.

#### *Facility-Specific Analyses*

The finest grain analyses of induced demand have occurred at the project level. Most facility-specific studies compare observed traffic counts along an improved facility to what would have been expected had the project never been built. Expected volumes under the “null” might be based on trend extrapolation, travel-demand forecasts, or comparisons to a “control” corridor or regional trends. Results are not presented as elasticities but rather as percentages of traffic growth thought to be attributable to induced travel.

Facility-specific studies generally rate low in terms of internal validity but get high marks for transparency and accessibility to laypersons. One problem with some before-and-after project-level analyses is that they fail to sort out diverted trips from latent trips in gauging induced demand. Most project-level studies also ignore the repercussions of an expanded road segment on travel along lower order routes that connect to it. Since traffic increases on connecting and lower order facilities are overlooked, project-level studies are thought to have generally misstated induced-demand impacts.

Three types of facility-specific studies are summarized below: (1) growth comparisons, (2) quasi-experimental comparisons, and (3) regression analyses. This order matches, more or less, the degree to which potential confounding factors are controlled—from growth comparisons (the lowest) to regression analyses (the highest). Table 1 summarizes the research designs and findings of key facility-specific studies to date, particularly those that have sought to remove diverted trips from estimates. As shown, findings vary considerably.

#### *GROWTH COMPARISONS*

A *growth comparison* indexes past traffic trends to a factor, like car registrations, and estimates are made of what traffic would have been without a road improvement by factoring off known car registrations. Differences between recorded and expected volumes are considered to be newly generated traffic. One of the earliest growth comparison studies, by Jorgenson (1947), studied traffic trends on two newly opened parkways connecting New York City and New Haven, Connecticut. By indexing traffic trends to gasoline sales, Jorgenson estimated that the new parkway increased traffic in the corridor by 25 to 30 percent. Because the corridor included all significant alternate routes, it was pre-

sumed most of this gain represented new, not redistributed, travel.

Comparisons of forecasted and actual traffic have been widely used in the United Kingdom to estimate the induced-demand effects of individual projects. In its comprehensive review of British experiences, SACTRA (1994) issued a report that found travel forecasts were, on average, 10 to 20 percent below actual recorded traffic because induced traffic was overlooked. For a one-year forecast, the underprojection of traffic volumes along improved road facilities was found to be 5.7 percent. This compared with an average underprojection of 0.7 percent on alternative routes, suggesting induced demand represented about 5 percent of traffic growth (Goodwin 1996). In the long run, induced-demand effects were thought to be four times as high. How much of the forecast error was attributable to truly newly generated traffic versus forecast inaccuracies (beyond the failure to account for induced demand) was left unsaid.

#### *QUASI-EXPERIMENTAL COMPARISONS*

A criticism of past project-level comparisons is that little effort was made to introduce controls. Some studies have introduced quasi controls by comparing traffic trends between improved facilities and regions at large. Holder and Stover (1972) employed such an approach in a study of eight urban highways in Texas, finding “apparent induced traffic” ranged from 0 to 21 percent. Examining trends in pre- and postconstruction average daily traffic (ADT) for thirty-seven Texas urban corridors from 1955 to 1985, Henk (1993) obtained similar results.

A preferred approach to studying before-and-after traffic is matched-pair comparisons. This mainly involves comparing before-and-after screen line counts along an improved corridor versus a fairly comparable unimproved corridor.

Although matched-pair comparisons perform well in attributing traffic growth to road improvements, presuming matched facilities are nearly identical in all other respects, they say little about how much is diverted and how much is newly generated. Wide screen lines will partly solve this problem.

In a review of twenty matched-pair comparisons conducted for road projects in the United Kingdom, Goodwin (1996) calculated the average share of traffic growth attributed to induced traffic to be 25 percent, with a range of 7 percent to 66 percent. Moreover, the “unexplained” growth attributable to induced travel increased from an average of 10 percent for a very short time frame of less than one year to 33 percent during a five-year period. Pells’s (1989) review of seventy-eight published and unpublished U.K. studies that used con-

trol corridors, drawn mainly from the greater London area, found considerable variation in traffic impacts, with induced-demand effects (exclusive of redistributions) ranging from 0 to 80 percent.

British researchers have also used matched pairs to examine the polar opposite of induced demand—what is sometimes called *reduced demand* or *suppressed demand*. These studies gauge the effects of banning cars from streets (e.g., pedestrian-only districts); bridge closures (such as for repairs or because of natural disasters like earthquakes); rededication of lanes to buses only; and other measures that reduce, instead of expand, road capacity. In a study of more than one hundred cases of road-capacity reductions in Europe, North America, Japan, and Australia, Goodwin et al. (1998) found an average overall reduction of 25 percent, even after controlling for possible increased travel on parallel routes. This “evaporated” traffic was assumed to represent a combination of people forsaking low value-added (discretionary) trips and opting for alternative modes, including transit riding, walking, and cycling. Methodologically, matched-pair studies of reduced travel are likely less susceptible to confounding influences such as secular increases in traffic (e.g., due to demographic trends) since, unlike induced-demand analyses, the two effects move in the opposite direction (Cairns et al. 1998).

Although no comprehensive matched-pair studies of suppressed travel have been carried out in the United States, eighteen pairs of California highway corridors were recently matched to study induced travel from 1976 to 1996 (Mokhtarian et al. 2000). The study found statistically indistinguishable differences in ADT growth rates between improved and unimproved segments. The authors cautioned that the inability to choose truly random and similar control sites could have meant matched-pair models understated induced-demand effects.

#### REGRESSION MODELS

An alternative to matched pairs for controlling the influences of confounding factors that might explain traffic growth is multiple regression analysis. Because of data limitations, this has rarely been done at the facility-specific level. Perhaps the most rigorous study to date that longitudinally studied induced demand at the level of road segments is the work of Hansen et al. (1993). The authors probed the effects of adding highway capacity on VMT for a panel of eighteen California highway segments from 1970 to 1990, all of which were expanded during this time period. The analysis employed a “counterfactual” approach. First, log-linear

regressions were estimated that predicted traffic volumes as a function of capacities, controlling for secular growth in travel on all California state highways. The model was then applied under two scenarios. The first assumed that the capacity increases occurred (which they had), whereas the second—the counterfactual—assumed there were no capacity expansions. The difference in predicted traffic under the two scenarios was assumed to be the traffic induced by expansion. For individual segments, the elasticities of VMT with respect to lane miles were 0.2 to 0.3 during the first four years, increasing to 0.3 to 0.4 after ten years, and to 0.4 to 0.6 after sixteen years.

#### Model Forecasts

Another approach to estimating induced demand invokes the use of large-scale travel-demand forecasting models to derive traffic estimates. Differences between forecasted and actual volumes are assumed to represent latent trips. Implicitly, forecasting models are assumed to be valid except for their omissions of induced-demand effects.

Relatively few travel-model-based estimates of induced demand have been carried out to date, at least in the United States. One example is the work of Addison (1990). The author compared actual and forecasted traffic on several expanded and enhanced facilities in northern California. Addison used a conventional four-step model to forecast travel demand, but he did not directly measure induced demand, although he did find compelling evidence of its existence. In the case of a twelve-mile arterial upgrade to a grade-separated facility, Addison found that daily traffic on the improved section observed in 1985 exceeded 1985 forecasts by 21 percent, whereas peak-hour traffic was 25 to 30 percent higher.

The fact that relatively few studies have turned to four-step models to estimate induced-demand effects of specific projects or corridor improvements is partly a commentary on the standing of today’s forecasting tools. In many parts of the United States, travel-forecasting models are not up to the task of evaluating induced-demand effects because they do not embody elasticities or feedback mechanisms that weigh, directly or indirectly, generative impacts. The relationship between induced demand and travel-forecasting models was the focus of a report produced by the Transportation Research Board (1995) of the National Research Council. The consensus view was that contemporary travel-demand forecasting models fail to adequately account for induced-demand effects.

### Area Studies: Proxy Elasticities

Area studies aggregate data, usually by pooling cross-sectional cases across multiple time points, to relate lane mile additions (as a proxy of reduced travel costs) to VMT. Road capacity is usually measured on major facilities, such as freeways and arterials, which are under state jurisdictions. As aggregate analyses, covariation is captured by measuring differences in VMT across counties or metropolitan areas and across time points. Table 2 summarizes the research designs and core findings of key area studies reviewed in this article.

As noted, some observers argue that capacity additions are a poor indicator of the benefits conferred by a road improvement (Cohen 1995; DeCorla-Souza and Cohen 1999). Capacity releases suppressed demand only in crowded traffic conditions. Only by lowering the travel time “price” borne by motorists can traffic be induced. In truth, accurately measuring travel times across numerous time points can be a daunting task. Travel times vary considerably by time of day, day of week, and season of year; in contrast, a fixed amount of road capacity does not vary. In the case of regional analyses, the scale that DeCorla-Souza (2000) and others maintain is most appropriate for capturing the spillover effects of road improvements, travel times also vary widely geographically and across facilities.

For pooled time-series/cross-sectional studies, such as the works of Hansen and Huang (1997) and Noland and Cowart (2000), obtaining reliable annual travel time data for dozens of counties and metropolitan areas during several decades would be a Herculean effort. The risk of measurement errors is extremely high. To cope with the fact that lane mile additions is an imperfect proxy of benefit (e.g., in uncongested rural and exurban settings, additional lanes yield little if any travel time savings), many area analyses have used fixed-effects dummy variables. Fixed-effects variables absorb the unique nature of travel variation in certain places, such as sparsely populated counties, that are not statistically captured by other variables (including proxies such as lane miles) in an equation.

The first area studies that used road supply as a proxy for user benefit appeared in the early 1970s. For example, Kassoﬀ and Gendell (1972) studied the relationship between urban-area VMT per capita and road supply per capita for different-size classes of U.S. urban areas. The study found that as a “system supply index” nearly doubled, VMT per capita rose roughly 50 percent, implying an upper bound elasticity of 0.58. Other area studies from this period that regressed VMT on highway capacity across U.S. urban areas recorded lower

elasticities, in the range of 0.13 to 0.22 (Koppelman 1972; Payne-Maxie et al. 1980).

Among the first area studies that gauged the effects of lane miles of VMT in an econometric framework was the work by Hansen et al. (1993) and Hansen and Huang (1997). In both analyses, the authors used data on state highway travel demand and supply between 1970 and 1993 for thirty-two urban counties in California. The predictive models used population, per capita income, population density, and average gasoline price as statistical controls and ordinary least squares (OLS) estimation. The inclusion of fixed-effects variables helped absorb the influences of relevant but omitted variables, which Noland and Lem (2002) maintain is absolutely essential in induced demand studies since so many exogenous, difficult-to-measure factors have propelled VMT growth during the past several decades. The initial Hansen et al. (1993) study found lane mile elasticities in the range of 0.46 to 0.50. When county data were aggregated to the metropolitan level, elasticity estimates increased slightly, to a range of 0.54 to 0.61. The follow-up work introduced an autoregressive estimation structure, producing even higher elasticity estimates than the earlier one. At the metropolitan level, elasticity estimates were 0.5 in the short run and 0.9 in the long haul. The results of this study were embraced as empirical proof that no U.S. city can “build itself out of traffic congestion” (Dittmar 1998).

During the past five years, the work of Hansen and his associates has spawned numerous other studies, carried out using data from different areas of the country and different time periods, and usually with different model specifications. Fulton et al. (2000) pooled cross-sectional and time-series data to estimate VMT growth as a function of lane miles, population, and per capita income for counties in North Carolina, Virginia, and Maryland as well as the District of Columbia. Their base models produced short-run estimates in the range of 0.33 (Washington, D.C.) to 0.51 (Virginia), with estimates for all areas combined of 0.52 to 0.57.

Distributed lag model structures have recently surfaced to estimate long-term induced-demand effects (Hansen and Huang 1997; Noland and Cowart 2000; Fulton et al. 2000; Cervero and Hansen 2002). These models normally assume that lag effects taper according to an exponential function, with the strongest influences occurring immediately and impacts attenuating during longer lag periods. Long-run elasticities represent the cumulative sum of coefficients on log-transformed lagged variables. Fulton et al. (2000), for example, used a polynomial distributed lag model to estimate a long-run elasticity of 0.72 for three mid-Atlantic states and the Washington-Baltimore metropolitan area, ranging

from a low of 0.48 for Maryland counties and a high of 0.81 for Virginia counties. As in all studies to date, these long-run elasticities exceeded those in the short run.

#### *Area Studies: Partial Elasticities*

Other aggregate models have sought to relate traffic increases directly to travel time savings. Accordingly, elasticities take on negative signs in contrast to the positive elasticities of aggregate models that predict travel demand as a function of lane miles. As discussed earlier, these are considered to be partial estimates in that demand elasticities are imputed from estimated travel time savings; however, the degree to which these are purely attributable to road improvements (vis-à-vis factors such as economic downturns that reduce commuting volumes, etc.) is not always clear.

The United Kingdom's SACTRA report (1994), summarized by Goodwin (1996), is the most exhaustive review to date of how congestion relief stimulates travel. Culled from experiences documented in numerous unpublished and published studies across Great Britain, the report found that elasticities with respect to travel time ranged between  $-0.5$  and nearly  $-1.0$ .<sup>3</sup> DeCorla-Souza (2000) imputed elasticities of a similar magnitude using travel-demand forecasting models. On the basis of average region-wide travel speeds and VMT estimated for a beltway project in Memphis, Tennessee, the author estimated short-run travel time elasticities of  $-0.7$  for models without feedback from traffic assignment to trip distribution and  $-1.1$  when models included feedback. Because these estimates are at least as large as past estimates of travel time elasticities, he contends that travel forecasting models do adequately capture induced demand.

Several studies have measured elasticities of highway demand as a function of other price components, not all of which relate directly to road expansion. In a review of studies that measured the elasticity of VMT with respect to fuel prices, Goodwin (1992) found an average short-run elasticity of  $-0.16$  and a long-run elasticity of  $-0.30$ . Drawing from studies carried out on six highway corridors in Great Britain and pivoting off of fuel price elasticities, Goodwin (1996) estimated an average travel time elasticity of  $-0.28$  in the short term and  $-0.57$  in the long term.

Less empirical research has been carried out in the United States that relates highway VMT to changes in travel time in an econometric framework. One example is the work of Burright (1984), who estimated a simultaneous system of equations (using two-stage least squares) that predicted private vehicle miles per household as a function of three endogenous variables: travel time cost per household, bus trips per household, and

urbanized land area. Using a panel data set consisting of observations from 1968 and 1970 for twenty-seven urbanized areas, he estimated an elasticity of  $-0.51$ . Because standard errors were not reported, the precision of this estimate is unknown.

#### *Disaggregate Models*

Disaggregate analyses enjoy inherent advantages over aggregate analyses in studying travel behavior, including induced demand (Marshall 2000). By studying travel at the level of individual trip makers, they are less vulnerable to the spurious inferences sometimes encountered with aggregate-scale data. Put simply, people travel, not traffic zones.

Several recent studies have made headway in examining induced travel demand at a disaggregate level using data from the Nationwide Personal Transportation Survey (NPTS). Although data were drawn from across the United States, each observation represented the travel behavior of members of an individual household. Strathman et al. (2000) combined NPTS and Texas Transportation Institute (TTI) data on region-wide road capacities. Using different model specifications and data for twelve thousand respondents from forty-eight urban areas, they estimated cross-sectional elasticities of VMT with respect to per capita road capacity of  $0.29$ . Using the same data set, Barr (2000) imputed travel time elasticities ranging between  $-0.35$  and  $-0.58$ , with an average value of  $-0.44$ . How much of the travel time savings was attributable to road expansion, and thus was a measure of benefits conferred by road investments, was not specified in the analysis.

Both of these studies yielded lower elasticity estimates than those of most area studies, suggesting the possibility of ecological fallacies in inferring elasticities from aggregate data. However, cross-sectional studies have limitations as well. Studying variation in travel among people according to where they live and how well roads function in those areas might reveal the existence of a relationship, but does not establish causality, something that theorists contend only longitudinal data can provide (Asher 1983). The limitations of cross-sectional data are revealed by poor model fits. Among the fifteen equations Barr (2000) produced to estimate travel time elasticities, models explained less than 24 percent of the variation in household VMT. By comparison, most recent pooled time-series/cross-sectional models of induced demand have explained well over 90 percent of the variation in county-level VMT. (The poorer statistical fits from micro data, however, also suggest that aggregate-scale models of induced demand are guilty of aggregation biases and overstate goodness of fit.)

Combining data from two disparate databases, something that all disaggregate analyses have done to date, is also fraught with difficulties. First, there are possible resolution problems since the numerator of the elasticity calculation (trips) is measured on a disaggregate scale, whereas the denominator (lane miles) is aggregate in scale. Second, there is an assumption of concordance between the TTI and NPTS databases. There is no way to know, however, whether the corridors that were expanded matched the locations where NPTS respondents traveled most of the time and reaped travel time savings. This is similar to the discordance problem of using lane miles of highway to gauge induced-demand effects when it is not always clear the degree to which capacity additions bring about travel time savings. Finally, disaggregate analyses that use household travel diary data also totally ignore trips by commercial vehicles. This contrasts to aggregate-scale analyses that capture all forms of movement, including goods movements, on the facilities studied.

#### SORTING OUT THE CAUSAL CHAIN

Induced-demand research has recently turned to the question of whether road investments and travel demand are codependent, that is, do road improvements both induce and respond to travel demand? Studies that have sought to disentangle the two-way causality between road investments and travel demand have applied econometric approaches in this pursuit.

Several studies have used Granger causality tests to probe road supply-demand relationships. In a study of mid-Atlantic states, Fulton et al. (2000) found that lane mile growth “statistically” preceded growth in VMT, but not necessarily vice versa. In a California study, Cervero and Hansen (2002) found that lane mile expansions significantly accounted for VMT increases; however, unlike the study of mid-Atlantic states, they found that the relationship worked in both directions—past VMT increases also explained lane mile expansion. Their research suggested that causality works both ways—supply induces demand and likewise, demand induces supply.

Another approach to exploring causality has been the use of instrumental-variable estimation. Noland and Cowart (2000) built models to estimate elasticities using instrument variables, although they did not simultaneously model supply-demand relationships. To remove possible estimation biases, land area and population density were used as instruments to estimate lane miles, and reduced-form estimates were then used to predict variation in VMT per capita. An ideal instrument is one highly correlated with one endoge-

nous variable but not correlated with others. Conceptually, one could question the propriety of these instruments since both endogenous variables—VMT as well as lane miles—likely increase with increases in the area and use intensity of land. The authors derived fairly high long-run elasticities in the range of 0.65 to 0.90.

The Cervero and Hansen (2002) study also used instrument variables to resolve estimation bias problems; however, unlike previous work, this analysis introduced a simultaneous equation structure. Using data on thirty-four urban California counties from 1976 to 1997, VMT and lane miles on state highways were jointly estimated employing various exogenous variables related to topographic, meteorological, air quality, and political variables as instruments. The simultaneously estimated models revealed an elasticity of VMT with respect to lane miles of 0.56. Using a distributed lag model structure, the research estimated a long-run elasticity of 0.78 to 0.84. The research also showed evidence of “induced investments”: the elasticity of freeway and highway capacity with respect to VMT was 0.33. Presumably, state highway investment in any year was based on levels of travel demand that were anticipated—suggesting, in California at least, that road investments not only stimulated travel demand but responded to it as well.

A more recent study of twenty-four California freeway projects across fifteen years introduced a path model framework to sort out “induced demand,” “induced growth,” and “induced investment” effects (Cervero 2002). Recorded traffic increases along expanded freeways were explained in terms of both faster speeds and land use shifts. Because less than half of the recorded speed increases were statistically attributable to road improvements, a fairly modest long-term induced-demand elasticity of 0.39 was recorded. The longitudinal effects of rising VMT on roadway investments were of a similar order of magnitude. This path analysis produced elasticity estimates considerably below those of earlier studies, underscoring the fact that dramatically different results can be produced under different model specifications. Overall, models that have sought to account for two-way causality have yielded lower elasticity estimates (in absolute terms) than those based on single-equation analyses.

#### INTERACTION EFFECTS

Only recently have researchers sought to stratify their analyses of induced travel demand to account for interaction effects. Do road investments, for example, interact with levels of congestion to produce

large induced-demand outcomes? A Transportation Research Board (1995) committee took a firm position on this:

The largest induced traffic effects are expected for the construction of a new freeway in a congested corridor that currently does not have a freeway because the new facility would provide significant travel time savings during both peak and off-peak periods. (P. 149)

This section summarizes the degree to which empirical evidence bears this out.

#### *Congestion Levels*

It stands to reason that considerable pent-up demand will be unleashed by road expansion in a congested setting, whereas the impacts of adding lanes in free-flowing conditions will be negligible. In truth, evidence is scant. The SACTRA (1994) report suggested stronger induced-demand effects when a network is operating close to capacity but offered no empirical evidence to substantiate this position. Fulton et al. (2000) found some evidence that population densities and congested levels influenced the degree to which lane mile expansions induced VMT increases; however, the results were statistically insignificant. Henk (1993) showed that traffic volumes increased with volume-to-capacity levels as well as population density following road improvements in Texas cities, but his analysis did not separate out diverted from latent trips. Using a distributed lag model, Noland and Cowart (2000) found no difference in induced-demand effects (i.e., VMT elasticities as a function of lane miles) between highly congested and minimally congested metropolitan areas.

#### *Urban Setting*

Because urban areas are denser and more congested, they could be expected to experience greater induced-demand effects than suburban or rural settings. Again, the evidence is muddy. Using the 1995 NPTS, Barr (2000) found slightly higher travel time demand elasticities in urbanized areas (-0.36) than nonurbanized ones (-0.32). However, two other analyses suggested the opposite. Fulton et al. (2000) found the relationship between lane miles and travel to be statistically weaker in the case of the Washington-Baltimore metropolitan area than the states of Maryland, Virginia, and North Carolina, which contain many rural counties. In his review of British experiences, Goodwin (1996) found that urban roads averaged unpredicted traffic in the short term of 5.7 percent, whereas the average underprediction for rural roads was 13.3 percent. The inference was that induced demand accounted for more

than twice the share of traffic growth in rural settings as in urban ones.

#### *Metropolitan Size*

Big metropolises could be expected to register greater induced-demand effects since they generally suffer the nation's worst traffic congestion (Shrank and Lomax 2000). The evidence is also weak on this front. Noland and Cowart (2000) found, surprisingly, that induced-demand effects were highest for medium-sized metropolitan areas, followed by large and small ones. Barr (2000) also found no clear relationship; when travel time elasticities estimated from the NPTS data set were stratified by metropolitan size, values jumped around in a seemingly random fashion.

#### *Type of Facility*

A brand-new highway can be expected to generate more new traffic than the expansion of an existing facility, all things being equal. This is because a new facility will draw traffic during all hours of the day, whereas an improved one is likely to have little impact on off-peak conditions. This was supported by the findings of Ruiter et al. (1979, 1980) who, using simulation techniques, estimated substantially higher elasticities of VMT with respect to road capacity for the extension of a freeway linking Oakland, California, to its eastern suburbs in comparison to the expansion of older segments of the same freeway.

How might induced demand vary by facility type, for example, a beltway versus radial link? Luk and Chung (1997) postulate that "a radial route with added capacity could be less likely to generate demand than a circumferential route" (p. 2). Presumably, this is because a circumferential faces less competition in the sense that there are fewer parallel alternatives to cross-town facilities than radial ones. Of course, what matters is not the physical attributes of the facility but rather the amount of travel time savings it confers. A new circumferential freeway might add more marginal capacity for tangential journeys in the outskirts; however, a new limited-access radial facility might add more travel time savings along an urban corridor. In his study of thirty-seven Texas highway projects, Henk (1993) found a greater induced-demand effect on circumferential than radial highways and that the latent trips added to orbital facilities increased in direct proportion to population densities. An earlier study of fifty-four U.S. metropolitan areas found slightly higher elasticities of VMT with respect to beltway mileage (0.12) versus nonbeltway mileage (0.10) (Payne-Maxie et al. 1980).

## META-ANALYSIS

As case-based analyses, most metropolitan-scale studies of induced travel demand have limited external validity. One approach to generalizing research findings is a meta-analysis of elasticity estimates, that is, essentially calculating an arithmetic average across many studies. On the basis of a meta-analysis of more than one hundred road expansion projects in the United Kingdom, Goodwin (1996) estimated long-term elasticities (e.g., percentage increases in traffic as a function of travel time savings) of nearly  $|1.0|$ .<sup>4</sup> More recent analyses suggest this number is on the high side.

A simple unweighted arithmetic average of selected studies listed in Tables 1 and 2 reveals substantial variations based on the scale of analysis and methodological approach. In the case of facility-specific studies from the past twenty years that relied mainly on matched-pair comparisons, the percentage of traffic growth attributable to induced demand takes on the following mean values: short term = 0 percent, intermediate term = 26.5 percent ( $SD = 35$  percent), long term = 63 percent ( $SD = 28$  percent).<sup>5</sup> Not all of these studies successfully purged diverted trips from their calculations, thus these estimates could be on the high side. On the other hand, by not completely measuring impacts of road improvements on lower order and connecting facilities, some of the estimates could very well be too low. Based on facility-specific analyses, perhaps all that can be said with a fair degree of confidence is that induced-demand effects accumulate over time.

Area studies that have used lane miles as a proxy of road-conferred benefits have generally produced results suggesting greater impacts, as revealed by elasticities. A meta-analysis calculation of post-1980 area studies that have presented elasticities based on proxy (lane mile) estimates of benefits (drawn from Table 2, excluding disaggregate analyses) produced the following mean elasticities estimates: short term = 0.40 ( $SD = 0.18$ ), long term = 0.73 ( $SD = 0.20$ ). In comparison to facility-specific studies, not only are impacts more significant at the area scale of analysis, but results are also more similar, with lower standard deviations (relative to means, that is, lower coefficients of variation). Even when accounting for the fact that roads lag behind and respond to VMT growth, recorded induced-demand effects remain strong.

Two other types of elasticity estimates provide further insights—those based on travel time measures of benefits and those carried out at a disaggregate (household-level) scale. Drawing from recent British experiences, Goodwin (1996) concluded travel-time elasticities ranged from  $-0.5$  in the short term to nearly  $-1.0$  in the long term. In general, area studies that have presented

demand elasticities in either form—whether with respect to travel times (partial estimates) or lane miles (proxy estimates)—have yielded fairly similar estimates. If anything, evidence suggests that induced-demand effects might be slightly greater when gauged in terms of travel time. In contrast, disaggregate analyses of induced demand have generally yielded lower estimates. Both studies cited in this article that used NPTS data to study variations in VMT per household produced, in absolute terms, elasticity estimates in the range of 0.29 to 0.58.

If one interprets recently published studies on face value, the preponderance of research suggests that induced-demand effects are significant, with an appreciable share of added capacity being absorbed by increases in traffic, with a few notable exceptions (e.g., Strathman et al. 2000; Cervero 2002). To the degree that the many specification, measurement, and resolution issues raised at the beginning of this article have been problematic, these estimates have less credence. There remains some disagreement as to whether this is the case. All that can be said with certainty is that induced-demand effects exist (i.e., elasticities vary from zero), and they accumulate over time. So far, there has been greater consistency in gauging the *size* of induced-demand effects than in gauging their *scope*. Past efforts to explain variation by stratifying induced-demand estimates according to factors such as levels of congestion and urban versus rural setting have proven unsuccessful. This is an area worthy of more research attention.

## CONCLUSION

Despite the many methodological issues raised in this article, induced demand is “alive and well” in the United States and abroad. There is no question that road improvements prompt traffic increases, and these gains diminish travel time benefits to some degree. To what degree and under what circumstances, however, remains a matter of debate. Although the weight of evidence suggests induced demand is appreciable in most metropolitan settings of the United States and Europe, recent research based on micro data and multi-equation econometric modeling points to more modest impacts. Clearly, more studies across different settings and traffic conditions, ideally modeled according to normative theories of travel demand, are needed to refine our collective understanding of the complex relationship between road supply and travel behavior.

Refining our knowledge of the induced travel demand phenomenon is important for at least two policy-related reasons: to improve travel demand forecasts and to shape transportation decisions so as to pro-

mote the broader public good. In many parts of the United States, traditional four-step travel-forecasting models fail to embody “induced travel” and “induced growth” effects (Transportation Research Board 1995). Accordingly, forecasts run the risk of overstating the travel time savings benefits of future road proposals. Until trip generation techniques adequately account for latent trips, the traffic assignment step adequately captures route shifts, and dynamic feedback loops are created to account for land use shifts spurred by new roads, the art and science of travel demand forecasting will fall far short of the ideal. Progress is sorely needed on this front.

The public policy implications of induced demand are manifold. To the degree induced-demand effects are pronounced, supply-side solutions, alone, will yield short-lived benefits. The prospect of induced travel lends credence to a balanced, multimodal transportation policy. Under the right conditions, financial resources might be redirected from road capacity expansion to upgrading public transit, dedicating special lanes for car pools and van pools, or deregulating the marketplace to allow an array of commercial paratransit mobility options. Demand management might also be introduced in parallel with road improvements for purposes of moderating induced travel. This could run the gamut from encouraging employers to introduce modified work schedules to flexing zoning standards for parking to introducing roadway pricing (e.g., congestion tolls or high-occupancy toll lanes) (Ferguson 2000). Public pressures often stand in the way of any initiatives that ration scarce road supply through price signals, like congestion tolls. Regardless, it is up to transportation planners to remind citizens and public officials alike that demand management tools exist to moderate induced-demand effects, and whether they are introduced depends on the importance assigned to travel time savings (in addition to there being the political will to pass on higher charges).

In the long term, demand management might shift from the transportation to the land use sector. Proactive land use management and zoning controls near interchanges and along improved highway corridors can be effective at attenuating induced demand. Steps might take the form of keeping big trip generators away from congestion-prone corridors and encouraging commercial and office site designs that promote transit access, walking, and other alternatives to the drive-alone auto. Recent smart-growth initiatives in Maryland and Georgia explicitly call for cities and counties to consider possible induced-demand effects and mitigation strategies when preparing long-range integrated transportation and land use plans.

To date, transportation decision makers have been more sensitive to claims of induced demand abroad than in the United States. In Great Britain, the cumulative weight of evidence on induced demand effects has strongly influenced public policy. In 1998, the U.K. Department of Environment, Transport and the Regions published a White Paper, *A New Deal for Transport: Better for Everyone*, that jettisoned the previous policy of trying to accommodate traffic growth through the strategy of “predict and provide.” The amount of road infrastructure needed to meet unconstrained growth assumptions was deemed unsustainable—environmentally, financially, and socially (Noland and Lem 2002). Goodwin (1999) noted this has elevated alternative transportation programs, such as expanded transit services and demand management, to a higher status. The European Conference of Ministers of Transport (1998) has gone on record as supporting a balanced, multimodal approach to transportation investment that shifts more resources to modal alternatives, in large part because road investments provided ephemeral relief. So far, claims of induced demand have failed to resonate as deeply in America’s urban transportation policy circles.

Although studies of induced travel demand, despite their limitations, are essential, they do not tell the whole story. Whether the elasticity of VMT with respect to road investments is 0.10, 0.50, or 0.90 is less important than knowing which transportation investment and management strategies provide the greatest social and economic payoff. Analysts must be careful not to fall into a reductionist trap, that is, framing highway investment policies purely from the lens of induced travel. At least as much research attention needs to go toward investigating how to best invest and manage scarce urban transportation resources—for example, do benefit-cost estimates favor particular combinations of transportation strategies (e.g., road expansions, bus rapid transit systems, or value pricing) in particular corridors?

The many methodological dilemmas and shortcomings reviewed in this article suggest, despite some progress in recent times, considerable knowledge gaps remain in studying induced travel demand. Improvements in model specifications, ideally based on disaggregate data and normative modeling frameworks, will aid a lot in illuminating our understanding of this complex phenomenon. Given our limited understanding of interactive effects, studies conducted across a wider array of settings—for example, suburban versus urban areas, freeways versus surface streets—are needed. More important, urban simulation and travel-forecasting models need to incorporate induced-demand as well as induced-growth and induced-

investment effects to the maximum degree possible. Only then will decision makers be equipped with the kinds of knowledge necessary to rationalize road-building programs and pursue the right balance of multimodal transportation initiatives.

## NOTES

1. Elasticities represent an index of sensitivity, and in the context of induced travel demand, they measure the percentage change in travel demand given a 1 percent increase in roadway capacity (or some other measure of supply-side improvement). For example, an elasticity of 0.5 signifies that for every 1 percent increase in roadway capacity, there is a 0.5 percent increase in traffic—that is, roughly half of the added capacity gets absorbed by additional traffic.

2. A point elasticity takes the form of  $\partial \log(Y)/\partial \log(X)$  (i.e., the marginal change in Y given a marginal change in X) and has the advantage of yielding constant and symmetrical elasticity estimates of all points along a demand-curve surface. Arc elasticities are sometimes also presented, measured as  $(\partial Y/Y)/(\partial X/X)$ , but they yield different and not always symmetrical results at different points along the surface.

3. As a function of travel time, elasticities take on negative values, indicating that as travel times increase, the demand to travel declines.

4. The signs on some elasticities, such as travel demand as a function of travel time, were negative, whereas the signs on others, such as travel demand as a function of speed, were positive, reflecting difference in the directionality of relationships.

5. SD = standard deviation. Also, for studies that reported a range of estimates, the midpoint of the range was used in the calculation.

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