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Author
Cervero, Robert

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Induced Demand:
An Urban and Metropolitan Perspective

Robert Cervero, Professor
Department of City and Regional Planning
University of California
Berkeley, California

E-Mail: robertc@uclink.berkeley.edu

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Induced Demand:
An Urban and Metropolitan Perspective

Most studies of induced travel demand have been carried out at a fine to medium grain of analysis – either the project, corridor, county, or metropolitan levels. The focus has been on urban settings since cities and suburbs are where the politics of road investments most dramatically get played out. The problems assigned to induced demand – like the inability to stave off traffic congestion and curb air pollution – are quintessentially urban in nature.

This paper reviews, assesses, and critiques the state-of-the-field in studying induced travel demand at metropolitan and sub-metropolitan grains of analysis. Its focus is on *empirical* and *ex post* examinations of the induced demand phenomenon as opposed to forecasts or simulations. A meta-analysis is conducted with an eye toward presenting an overall average elasticity estimate of induced demand effects based on the best, most reliable research to date.

1. URBAN 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. Highway critics charge that road improvements provide only ephemeral relief – within a few year's time, most facilities are back to square one, just as congested as they were prior to the investment. Traffic is said to behave more like a gas than a fluid – it expands to fill available space. Regional transportation plans, such as in the San Francisco Bay Area, have been challenged on the very grounds that they failed to adequately account for the possibility new road investments might induce sprawl and the extra trips associated with it. Reflecting on the state of urban transportation policy in America, Wilfred Owens (1985, p. 366) echoed this sentiment: "Meeting the ever-growing needs for transport capacity has often proved to be a fruitless task, as the persistence in urban traffic jams attest".

The contention that "you can't build your way out of traffic congestion" has become a rallying cry of the Surface Transportation Policy Project (STPP). In a recent study of 70 metropolitan areas across 15 years, the Surface Transportation Policy Project (STPP, 1999) concluded that metropolitan areas that invested heavily in road capacity expansion fared no better in easing traffic congestion than metropolitan areas that did not. While this and similar analyses can be criticized for inferring causality from simple correlations, nevertheless such research often resonates with politicians because results are comprehensible and accessible. The downside of more sophisticated statistical analyses is that the results are often circulated among and appreciated by a fairly

small cadre of academic-types, not always the loudest voices shaping highway policies in America today.

The notion that new roads offer only short-term relief is today found in the popular media. A case in point is commentary by noted columnist Neal Peirce (1999) on the state of highway programs in metropolitan Washington, D.C.: "Widened roadways create excess capacity. Drivers anxious to cut their driving times switch from other roads. Where roads lead to less developed outer suburbs, homebuilders see opportunity, there's a rush of residents of the city and older suburbs, and congestion mounts."

As discussed in later in this paper, methodological issues pervade the induced demand policy debate. 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 at Chicago lends anecdotal credence to this position. Using 60 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 "major population gains occurred in proximity to the expressways over a decade before the construction of the respective expressways".

2. ANALYTICAL CHALLENGES

The study of induced demand at the project and corridor levels involves an *intra-metropolitan* scale of analysis. Studying travel between sub-areas (e.g., counties) or regions[1] involves *inter-metropolitan* comparisons. Both scales pose significant theoretical and methodological challenges. These challenges and what they imply for the study of induced travel demand are reviewed in this section.

2.1 Resolution

A fine-grain analysis of induced travel will often yield appreciably different results than a courser grain analysis. Lee *et al.* (1999, p. 71) 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.

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 geographical areas capture the impacts of an expanded artery on the capillaries that tie to it – i.e., 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 VMT as a function of lane-miles were in the ranges of: 0.15 to 0.30 over a four-year horizon; 0.30 to 0.40 over a 10 year horizon; and 0.4 to 0.6 over a 16 year horizon. 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.

2.2 Measurement

Measurement problems abound in studying induced demand at the intra- and inter-metropolitan scales. 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). Conventional wisdom holds some of the traffic gains spawn by a new road facility are *generative* (e.g., suppressed trips being released) and some are *redistributive*. Sources also are thought to vary between the near-term and long-term. While it is easy to show road improvements are followed by increased traffic, it is not always easy to quantify the *magnitude* and *sources* of the increase.

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. While 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. 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

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also part of the induced demand equation. Over 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".[2]

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). Accordingly most studies have ignored the sources of additional trips altogether, assuming that traffic gains are attributable to a host of factors. 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 & 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). Based on 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, spawn mostly by land-use adjustments, is the most significant cause of induced travel (when measured as VMT), though no evidence is presented. Within a regional context, he contends the impacts of highway improvements on the number of motorized trips and modal shifts are fairly modest, and the impacts on route shifts and regional growth are negligible to non-existent. Most consistent across studies is the position that improved roads trigger shifts from the off-peak to the peak.

Evidence on the redistributive effects of road investments on transit usage is particularly slim. This is partly because inter-modal impacts of new highway construction likely unfold slowly and inconspicuously – e.g., transit service cuts prompted by a lowering of suburban densities spawn, 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 drops in transit

patronage. A study of linking a major arterial and freeway in southeastern Melbourne, Australia found some evidence of modal shifts (Luk and Chung, 1997). Over a seven-year period spanning the opening of this critical network link in one of the region's fastest growing corridors, ridership on the Dandenong train line fell 14 percent compared to a regionwide 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, but no evidence was presented to support this.

Spillovers

Another measurement problem lies in the fact that the effects of expanding a particular highway segment can reverberate throughout a network, though how much and to what extend is generally unknown. Road facilities do not stand in isolation, but rather are links in a hierarchical network. The inter-dependency 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, virtually all route diversions will be internal to the unit of analysis.

Boundaries

To solve 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 maintained highways (Hansen *et al.*, 1993; Hansen and Huang, 1997; Noland and Cowart, 2000; Fulton *et al.*, 2000; Cervero and Hansen, 2001). 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 are not matched by reduced travel and shifts from local facilities.

Variables

Nearly all metropolitan-scale analyses have measured the dependent variable, travel consumption, in terms of VMT (vehicle miles of travel). 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 re-scheduling of trips from the shoulders to the heart of the peak period. While 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 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. Some 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 vehicle miles of travel (VMT), thus the effects of new roads on cumulative hours logged should also be considered (Ewing, 1996).

Supply-side variables used in induced demand studies also face measurement dilemmas. 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 – e.g., 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 re-pavement 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 re-designations 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,

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in an elasticity calculation, the proportional change in capacity of expanding a road from 4 to 6 lanes is expressed the same (in the denominator) for a one-mile road segment as a 20-mile road segment; in both instances, the relative increase is 50 percent. Surely, the traffic-inducing impacts of a 20-mile-long project will in most instances be greater than a 1-mile-long project.

2.3 Specification

Good, reliable research is based on well-specified, internally valid study designs – ones that are rooted in accepted theories and measure what they are suppose to measure (Hakim, 1987). Early studies of induced demand concentrated on gauging the traffic-producing impacts of new facilities. Induced demand was generally expressed as a percentage share of traffic growth. Most studies failed to control for the influences of exogenous factors that might have contributed to traffic increases.

More recent empirical 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 – i.e., 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-effects 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. Fixed-effect terms can be antidotes to model under-specification, helping to indirectly account for the influences of omitted (often exogenous) variables (e.g., female participation in the labor force) that likely have further propelled VMT growth.

One of the major specification problems confronted by all induced demand studies is the conflation of cause and effect. Until recently, efforts to measure induced demand effects could be criticized for ignoring issues of causality. Disentangling cause and effect in the interaction between road supply and travel demand is exceedingly difficult. 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.

2.4   Normative Significance

Another line of criticism questions the normative significance of research findings on induced demand. 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 swamped by the effects of mega-trends 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, over 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 balance beneficial to society cannot be informed by studies of induced demand; such weighty questions require a full accounting of benefits and costs.

Normative questions also arise as to whether possible VMT increases that following road expansion 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, p. 217). From an air quality standpoint, higher VMT certainly means more tailpipe emissions, however in some instances these impacts might be more than off-set by the air quality benefits of expediting flows and eliminating episodes of stop-and-go traffic.

3. TOWARD A NORMATIVE FRAMEWORK

A normative framework for gauging induced demand impacts is shown in Figure 1. The causal chain works as follows: a road investment increases travel speeds and reduces travel times (and sometimes yields other benefits like 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,

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Path of Induced Travel Demand

Near-Term Impacts

Diverted Trips

Route Shifts
Temporal Shifts

Induced Demand

New "Latent" Trips
Mode Shifts
Longer Trips

Road Improvement → Reduced Generalized Cost (Time, Price)

Long-Term Impacts

Induced Development

Land-Use Shifts
Built-Environment Shifts

Behavioral Shifts

Car ownership
Transit usage

Induced Demand
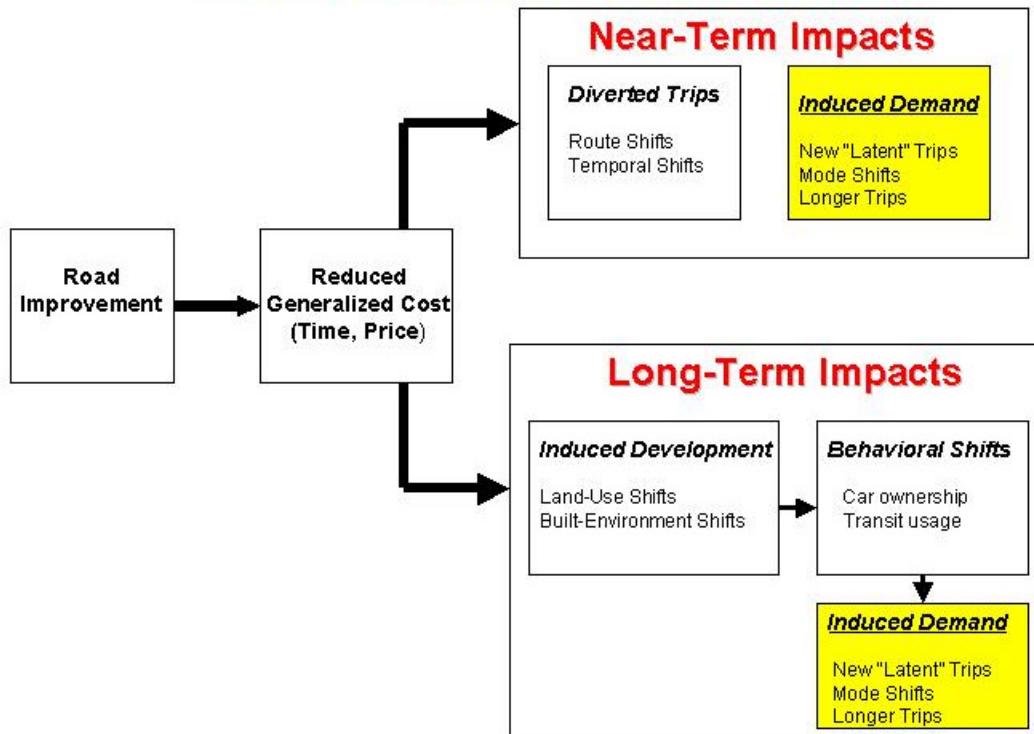
New "Latent" Trips
Mode Shifts
Longer Trips

Figure 1. Normative Path of Induced Travel Demand

including new motorized trips (e.g., latent demand previously suppressed), redistributions (modal, route, and time-of-day shifts), and over the longer term, more deeply rooted structural shifts like land-use adjustments and increased vehicle ownership rates (that in turn increase trip lengths and VMT). Some of the added trips are new, or induced, and some are diverted. While evidence on the induced-growth effects of new highways is limited (Dunphy, 1996; Boarnet, 1997), roads and prominent fixtures of America's suburban landscape -- big-box retail, edge cities, and campus-style executive parks – that they serve are clearly co-dependent. While many contend that only newly generated traffic should be treated as induced travel (as portrayed in Figure 1), others maintain all traffic, including redistributions, needs to be counted to demonstrate the futility of trying to relieve traffic congestion through road construction.

Figure 1 embodies basic microeconomic principles that hold motorists travel more when they derive utility in the form of speedier and safer roads. This lowers the generalized cost of using a facility. Thus, induced demand should be measured directly as a function of travel-time savings, which itself should be

measured directly as a function of road improvements. That is, an intermediate causal relationship exists – the effects of road improvements are channeled through the intermediate step of lowering generalized costs which in turn affects travel demand. Notationally:

$$\eta = (MQ/MGC)(GC/Q) \qquad Q = f(GC, C); \; GC = g(S,C) \quad (1)$$

> where: η = elasticity; Q = Quantity of trips; GC = Generalized Cost (including travel time); S = Supply of roads; C = Control vector.

In truth, empirical studies to date have relied on measures of induced demand that are either *proxies* or *partial*.   As shown in Figure 2, proxy metrics have gauged changes in travel demand, Q, as a function of changes in road capacity, S, without weighing the fact that the effects of road investments get channeled through their impacts on generalized cost. That is, they overlook the intermediate step of the causal path. Proxy measures express elasticities as:

$$\eta = (MQ/MS)(S/Q) \qquad Q = f(GC, S) \qquad\qquad (2)$$

Partial measures of elasticity can likewise be faulted for incomplete specification. Notably, they fail to weigh the relative role of road improvements in bringing about travel-time savings and to gauge how in turn this induces travel. While they model the intermediate step, they ignore the relative importance of road improvements (vis-à-vis other possible predictors) as the catalyst:

$$\eta = (MQ/MGC)(GC/Q) \qquad Q = f(GC, C) \qquad\qquad (3)$$

No metropolitan-scale studies of induced travel demand, as far as I know, have specified relationships as shown in Figure 1.   Accordingly, despite significant progress made in documenting induced demand effects over the past few decades, our knowledge of this phenomenon remains incomplete and partial.

4.   RESEARCH FINDINGS

At the broadest level, metropolitan and sub-metropolitan research on induced demand falls into two main categories: empirical studies and simulations. This paper focuses on empirical analyses, mainly those carried out over the past two decades. Empirical studies break down into three general groups: (1) those of specific facilities or projects, what Hansen *et. al.* (1993) called "facility-specific" studies; (2) those that study general pattern over a larger geographic territory, what Ruiter *et al.* (1979) termed "area" studies; and (3) those that examine individual travel behavior.
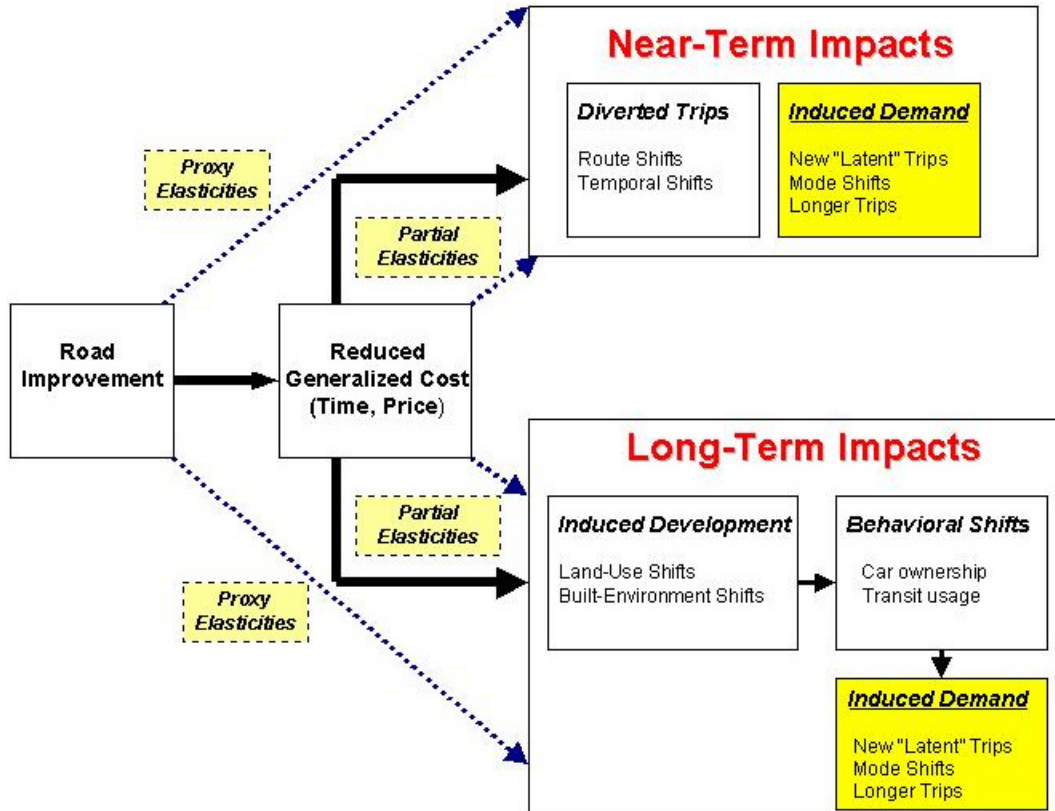
**Figure 2. Approaches Used to Measure Induced Demand Elasticities:
Proxy and Partial Elasticities**

Facility-specific studies probe relationships at a disaggregate, intra-metropolitan scale whereas area studies involve aggregate, inter-metropolitan comparisons. In the past few years, even more disaggregate studies have been conducted, focusing on travel at the household level as a function of road supply, however even these studies measure supply at a highly aggregate, typically metropolitan, level.

Empirical findings are summarized below, broken down by six analytical approaches: (1) facility-specific analyses; (2) model forecasts; (3) simulations; (4) area studies based on proxy measures; (5) area studies based on partial measures; and (6) disaggregate models.   Most of the findings summarized below are drawn from secondary sources – both original studies and a handful of syntheses of induced demand found in the urban transportation literature (Pells, 1989; Hansen, *et al*., 1993; Cohen, 1995; Goodwin, 1996; Noland and Lem,

2000). Findings are summarized in Tables 1 (facility-specific analyses) and 2 (aggregate-scale analyses with elasticity estimates).

4.1   Facility-Specific Analyses

The finest grain analysis of induced demand effects occurs 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.[3] Measuring elasticities is difficult for brand-new facilities since, after all, it is difficult to express a proportion when starting from a base of zero.[4]   Instead, elasticities are best suited for measuring impacts of lane additions – e.g., proportional change in VMT relative to a proportional change in lane miles (or the travel-time savings they induce).

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 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.

Below, three types of facility-specific studies are reviewed: (1) Growth Comparisons; (2) Quasi-Experimental Comparisons; and (3) Regression Analyses. This order matches 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 across studies.

Growth Comparisons

A "growth comparison" is the simplest approach to estimating the incremental gains in traffic in the aftermath of a road improvement. Past traffic trends are indexed to another factor, like car registrations, and estimates are made of what traffic would have been without a road improvement by factoring off of known car registrations. Differences between recorded and expected volumes are considered to be newly generated traffic. In hopes of removing diverted trips from the estimate, some analyses have used wide screenlines that span across parallel, alternate routes as well.

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

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

Key:

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)

Notes:

[a] Thought to include significant amounts of diverted trips

[b] Presented as elasticities: 0.2-0.3 for short and intermediate term; 0.3 to 0.6 for the long term

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. Jorgenson showed that gasoline sales growth for the state of Connecticut followed traffic growth in the corridor quite closely prior to the opening of the parkways. Gasoline sales growth after opening was used to estimate what traffic would have been without the new facility. From this, Jorgenson estimated the new parkway increased traffic in the corridor 25 to 30 percent.   Because the corridor included all significant alternate routes, it was presumed most of this gain represented new, not redistributed, travel. Another early study indexed traffic growth to increases in motor vehicle registrations to measure induced travel on a new expressway in Chicago (Mortimer, 1955).   The expressway was estimated to have increased traffic by 3 to 33 percent along three screenlines, each 4 to 5 miles in length. While the wide screenlines meant some route diversions were probably internalized (i.e., captured by screenline

counts), the study could not rule out that some of the traffic growth migrated from routes beyond the screenlines.

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, the U.K. Standing Advisory Committee on Trunk Road Assessment issued a report that found travel forecasts were, on average, 10 to 20 percent below actual recorded traffic because induced traffic was overlooked (SACTRA, 1994). For a one-year forecast, the under-projection of traffic volumes along improved road facilities was found to be 5.7 percent. This compared to an average under-projection of 0.7 percent on alternative routes, suggesting induced demand represented about 5 percent of traffic growth (Goodwin, 1996). Over 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.

While most project- and corridor-level analyses have defined induced demand in terms of shares of traffic growth, some early studies did estimate elasticities. Much of this earlier work, summarized in Ruiter *et al.* (1979), estimated lane-mile elasticities of a much smaller magnitude than more recent work (reviewed later) carried out at an area-wide scale – generally in the range of 0.01 to 0.15.

Quasi-Experimental Comparisons

A criticism of many project-level comparisons, particularly early ones, is that little effort was made to introduce "controls". Some studies used "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 zero to 21 percent. Examining trends in pre- and post-construction average daily traffic (ADT) for 37 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 screenline counts along an improved corridor versus a fairly comparable unimproved corridor. Induced traffic is measured as:

$$IT = [Q_{a(i)} - Q_{b(i)}] - [Q_{a(c)} - Q_{b(c)}] \qquad\qquad (4)$$

where: IT = induced traffic; Q = quantity of traffic; b = before time point; a = after time point; i = improved corridor; c = control corridor

While matched-pair comparisons perform well in attributing traffic growth to road improvements, they say little about how much is diverted and how much is newly generated. Again, wide screenlines will partly solve this problem. Matched-pairs also assume controls are virtually identical in every respect save for the absence of the road improvement, which is of course never the case. Often efforts are made to choose corridors that are similar in terms of a few key factors like geographic setting, levels-of-service, and types of facilities.

Among the earliest matched-pair analyses of induced traffic in the United States were comparisons of traffic growth on Chicago's Dan Ryan and Eisenhower expressways with control corridors (Frye, 1964a, 1964b). Frye found between 7 and 11 percentage point differentials in corridor traffic growth rates, and concluded most of the difference was explained by route shifts not captured by screenline counts.

In a review of 20 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 over a 5-year period. Pells's (1989) review of 78 published and unpublished studies that used control corridors in the greater London area found considerable variation in traffic impacts, with induced demand effects (exclusive of redistributions) ranging from 0 percent to 80 percent.

Several recent matched-pair studies have found no measurable induced demand effects. A fairly rigorous matched-pair analysis in Melbourne, Australia recorded no induced travel over a 10-year period as a consequence of linking a major freeway to a major arterial (Luk and Chung, 1997). After netting out estimated traffic gains due to route and time-of-day shifts, the authors recorded an annual traffic-volume growth rate of 1.7 percent along the improved corridor. The average traffic growth rate of two control routes was actually more, 2.7 percent, suggesting no new travel was generated by the network completion.
A recent matched-pair comparison of 18 California state highway segments over 1976 to 1996 period also found little evidence of induced demand (Mokhtarian *et al.*, 2000). The study found statistically and practically indistinguishable differences in ADT growth rates between improved and unimproved segments. The authors cautioned the inability to choose truly random and similar control sites could have meant matched-pair models understated induced demand effects (though one might expect any biasing effects to work in both directions).

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 to longitudinally study 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 18 California highway segments from 1970 to 1990, all of which were expanded over 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 the capacity increases occurred (which they had), while 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 10 years, and to 0.4 to 0.6 after 16 years.

4.2   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 new "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. While Addison used a conventional four-step model to forecast travel demand, he did not directly measure induced demand although he did find compelling evidence of its existence. In the case of a 12-mile arterial upgrade to a grade-separated facility, Addison found daily traffic on the improved section observed in 1985 exceeded 1995 forecasts by 21 percent while traffic in the peak was 25 to 30 percent greater.

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 no 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. In a more recent appraisal, Heanue (1997) was more charitable, arguing that the vast majority of VMT growth due to induced travel is accounted by travel models. A study of transportation and land-use scenarios in Sacramento estimated that integrated transportation and land-use models account for 50 percent of the induced travel effect (Rodier, *et al.*, 2001).

4.3   Simulations

Rather than modeling and estimating how road capacity shapes travel *ex post* (i.e., comparing actual and forecasted volumes), more recent large-scale modeling has focused on *ex ante* evaluations (i.e., simulations of future scenarios).   This normally involves simulating future travel with versus without major road improvements, and measuring the resulting differences in regional traffic levels. An advantage of simulations is that, unlike the real world, one specific variable can be changed while holding others constant. Thus the impact of adding two lanes on VMT can be directly and unambiguously measured. This approach, of course, faithfully assumes the conventional four-step travel-demand forecasting model (and integrated land-use allocation model, if used) embodies induced demand effects – i.e., it is internally valid.   As noted above, there is some disagreement on this question. While some metropolitan areas, like Portland and Sacramento, that are advancing the frontiers of travel-demand forecasting probably have models that significantly capture induced demand effects, many other metropolitan areas across the United States – big, medium, and small – probably do not.

One of the earliest and most systematic efforts to use transportation-planning models to simulate and predict the effects of road capacity expansion on vehicle travel is the work by Ruiter *et al.* (1979, 1980) (see Table 2). The authors estimated the effects on VMT of two highway improvements -- a new freeway and a freeway expansion -- in California. Their forecast models related trip generation rates to travel times, and were also able to account for impacts on modal splits as well as route and time-of-day distributions. For the new freeway, an elasticity of VMT with respect to vehicle miles of capacity of 0.38 was found. For the improved facility, a negative elasticity was actually produced (because added VMT was offset by reduced circuity for existing trips, and increased peak-period auto usage reduced off-peak travel).

Using the MEPLAN model and data from the Sacramento region, Rodier *et al.* (2001) employed a partial equilibrium approach to impute induced demand elasticities under a beltway improvement scenario. Arc elasticities of VMT as a function of beltway capacity (when accounting for trip generation, distribution, mode choice, and traffic assignment effects) were estimated to be 0.6 over a 15-year time horizon and 1.0 over a 40-year span.

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

Study	Setting	Data	Method	Variables Demand	Variables Supply	Elasticity Estimates Short-Term	Elasticity Estimates Long-Term
Kassoff, Gendell (1972)	US urban areas	CS	GA	VMT/capita	Capacity Index	<0.58	--
Koppelman (1972)	20 U.S. cities	CS	Reg: OLS	VMT	Lane-miles	0.13	--
Ruiter et al. (1979)	CA corridors	TS	MS	VMT	New facility	--	0.38
Ruiter et al. (1980)	CA corridors	TS	MS	VMT	Widening	--	0
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	--
	CA metro areas	TC/CS	Reg: OLS, DL, FE	VMT	Lane-miles	0.54-0.61	--
Hansen, Huang (1997)	32 CA urban counties	TC/CS	Reg: AR/DL, FE	VMT	Lane-miles	0.30	0.68
	CA metro areas	TS/CS	Reg: AR/DL, FE	VMT	Lane-miles	0.50	0.94
Noland, 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	.3-.6 (OLS) .13-.43 (AR)	0.47-.89 --
Strathman et al. (2000)	48 U.S. urban areas	CS	Reg: IV	VMT/HH	Lane-miles/capita	0.29	--
Cervero, Hansen (2001)	34 CA counties	TS	Reg: 2SLS, DL, FE	VMT	Lane-miles	0.56	.78-.84
Rodier et al. (2001)	CA corridors	TS	MS	VMT	New facility	--	0.6-1.0

Key:

TS = Time Series

CS = Cross-section

MS = Model Simulation

GA = Graphic Analysis

Reg = Regression
OLS = Ordinary Least Squares
DL = Distributed Lag
FE = Fixed Effects
AR = Auto-Regressive OLS
IV = Instrument Variables
2SLS = Two-stage IV estimation

4.4 Area Studies: Models Using Proxy Measures

Area studies aggregate data, usually by pooling cross-sectional cases over 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, like freeways and arterials, which are under state jurisdictions. As aggregate analyses, co-variation is captured by measuring differences in VMT across counties or metropolitan areas and over time points. Table 2 summarizes the research designs and core findings of key area studies reviewed in this paper.

What area studies of induced travel demand have most in common is the use of *elasticities* to gauge the relative sensitivity of VMT growth to lane-mile additions. This is partly because most regression analyses of induced travel demand to date have taken a log-linear form, yielding coefficient estimates that equal point elasticities.[5]

As noted earlier, 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). They note that capacity only releases suppressed demand in crowded traffic conditions. Only by lowering the travel-time "price" borne by motorists can traffic be induced.

In truth, accurately measuring travel times over 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 work of Hansen and Huang (1997) and Noland and Cowart (2000), obtaining reliable annual travel-time data for dozens of counties and metropolitan areas over a several decade stretch 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-effect dummy variables. Fixed-effects variables absorb the unique nature of travel variation in certain places, like sparsely populated counties, that are not statistically captured by other variables (including proxies like lane miles) in an equation.

The first area studies that used road supply as a proxy for user benefit appeared in 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. Hansen, *et al.* (1993) imputed an elasticity from this work with an upper bound of 0.58.[6] 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).[7]

Among the first area studies that gauged the effects of lane-miles of VMT in an econometric framework was the work by Mark Hansen and his colleagues from the University of California at Berkeley (Hansen, *et al.*, 1993; Hansen and Huang, 1997). In both analyses, the authors used data on state-highway travel demand

and supply between 1970 and 1993 for 32 urban counties in California. Induced-demand elasticities were estimated using single-equation models of the form:

$$\log(\text{VMT}_{it}) = a_i + \beta_t + S_k\ ?^k \log(x^k_{it}) + e_{it} \tag{4}$$

where:

VMT_{it} = vehicle-miles traveled in area i at time t;

a_i = fixed effect adjustment for area i

β_t = fixed effect adjustment for time t

x^k_{it} = value of explanatory variable k in area i and time t

$?^k$ = coefficient weight to capture distributed lag relationships

e_{it} = random disturbance term of area i and time t

The models estimated by Hansen and his associates used population, per capita income, population density, and average gasoline price as statistical controls and ordinary least squares (OLS) estimation. The inclusion of fixed-effect variables helped absorbed the influences of relevant but omitted variables, which Noland and Lem (2000) maintain is absolutely essential in induced demand studies since so many exogenous, difficult-to-measure factors have propelled VMT growth over the past several decades.

The initial (1993) study by Hansen and his associates 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 auto-regressive (Prais-Winsten) estimation structure and focused more on the time-lag structure of induced demand effects. This analysis produced even higher elasticity estimates than the earlier one. At the county-level of analysis, VMT elasticities (with respect to lane miles) of between 0.3 (near term) and 0.7 (intermediate to long term) were found. Aggregating up to the metropolitan level, elasticity estimates increased to 0.5 in the short run and 0.9 over the long haul. The results of this study received wide circulation and were quickly embraced by highway critics who claimed they now had empirical proof that America will never be able to "build itself out of traffic congestion" (Dittmar, 1998).

Over the past five years, the work of Hansen and his associates has spawn numerous other studies, carried out using data from different areas of the country and different time periods, and usually with different model specifications (Noland and Cowart, 2000; Cervero and Hansen, 2001). 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 been used by a number of authors to estimate long-term induced demand effects (Hansen and Huang, 1997; Noland and Cowart, 2000; Fulton, *et al.*, 2000; Cervero and Hansen, 2001). These models normally assume that lag effects taper according to an exponential function, with the strongest influences occurring immediately and impacts attenuating over 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 0.488 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.

DeCorla-Souza (2000) contends studies that have estimated induced demand as a function of capacity expansion have overstated elasticities for two reasons. One, only higher-order (state) facilities were included in the analyses. Two, diversion from lower-order facilities to higher-order ones were counted as induced travel. To the degree higher-order facilities average the worst traffic conditions, and thus are most subject to the effects of induced demand, the focus on state highways may be less problematic than appears. And to the degree induced demand is measured at a regional scale, the commingling of diverted and induced trips is not a problem to the degree route diversions are internal to the region.

4.5 Area Studies: Models Using Partial Measures

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 absent any effort to systematically express faster travel as a derivative of road improvements (vis-à-vis factors like mode shifts to upgraded transit services, economic downturns that reduce commuting volumes, etc.). Ideally, a path model is called for that measures travel-time savings as an intermediate step between initial road investments and consequent traffic increases. Nevertheless, a number of observers (Cohen, 1995; DeCorla-Souza, 2000) insist these studies provide the most accurate and valid basis for inferring induced travel impacts

The U.K.'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

the United Kingdom, the report found elasticities with respect to travel time ranged between –0.5 and nearly –1.0. DeCorla-Souza (2000) imputed elasticities of a similar magnitude using travel-demand forecasting models. Based on average regionwide travel speeds and VMT estimated for a beltway project in Memphis, Tennessee, DeCorla-Souza 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 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 long-run elasticity of –0.30. Drawing from studies carried out on six U.K. highway corridors 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. In weighing the combined findings of several literature reviews on price elasticity of demand for car travel throughout the industrialized world, Goodwin (1996) concluded that travel-time elasticities appear to be quite varied, ranging from –0.1 to –1.0; most recent studies have produced estimates that approach the upper boundaries of this range. Travel-time elasticities imputed from exogenous price variables, however, are apt to be inferior to those calculated directly from travel-time data. As Cohen reviews in these proceedings, imputed estimates of travel-time elasticities are likely inflated.

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 27 urbanized areas, he estimated an elasticity of –0.51. Because standard errors were not reported, the precision of this estimate is unknown.

While travel-time elasticities more closely reflect the normative model of Figure 1 than do lane-mile elasticities, nevertheless they face potential problems of their own. As noted, travel-time savings conferred by a road expansion might vary considerably by corridor, time-of-day, day-of-week, or season. An assumed "typical" travel-time savings may be a poor proxy benefit to the majority of motorists. Also, if one believes in the principle of time-budget theory, a road improvement should have an imperceptible effect on collective travel times. As the theory holds, people could be expected to maintain total travel-time budgets, thus by definition elasticities should gravitate toward zero in the case of regional scale analyses. Also, it is unclear the degree to which past studies have

successfully netted out the effects of other factors (e.g., changing fuel prices) that might have influenced variable in travel times. This is especially so in the case of studies cited by Goodwin (1996).   As shown in Figure 3, additions to road capacities can, *ceteris paribus*, be expected to increase travel speeds and thereby increase VMT. However, there might be one or more intervening factors, such as suburban location, that are explaining the co-movement in the capacity and speed variables. That is, road expansion might occur mainly in the suburbs (or sparsely populated areas more generally) where right-of-way is more easily acquired and where average travel speeds tend to be the highest. Unless the influences of such potential confounding factors are adequately controlled, the inferences drawn on the induced-demand effects of travel-time savings could be spurious. While many studies to date of induced-demand effects using lane-miles as predictors have sought to invoke an econometric framework to properly specify relationships, past studies using travel-time savings have generally paid less attention to specifying the complete chain of relationships. This could compromise the internal validity of the research findings.
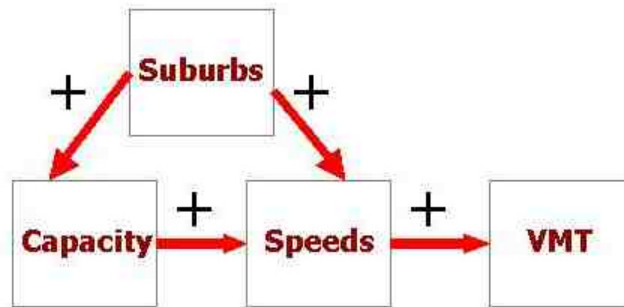

Figure 3. Possible Path Diagram of Factors Influencing Road Capacity, Travel Speeds, and VMT

4.6   Disaggregate Models

Disaggregate analyses enjoy inherent advantages over aggregate analyses in studying travel behavior, including that induced by road improvements (Marshall, 2000).   By studying travel demand 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 TAZs.

Several recent studies have made headway in examining induced travel demand

at a disaggregate level using data from the Nationwide Personal Transportation Survey (NPTS). While 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 regionwide road capacities. Using different model specifications and data for 12,000 respondents from 48 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 were attributable to road expansion, and thus were 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, however it does not establish causality.[8] Causal inferences, many contend, require some time-ordering of events, something which 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, pooled time-series/cross-sectional models of induced demand estimated by Hansen *et al*. (1993) and Cervero and Hansen (2001) explained over 99 percent of the variation in county-level VMT. Combining data from two disparate data bases is also fraught with difficulties. One, they face 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. Two, there is an assumption of concordance between the TTI and NPTS data bases. However, there is no way to know whether the locations where a region enjoys a generous supply of road capacity 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 whether capacity additions bring about travel-time savings.   Lastly, 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.

5. SORTING OUT THE CAUSAL CHAIN

To date, no metropolitan or sub-metropolitan scale study of induced travel demand has successfully used a normative model, such as depicted in Figure 1,

Table 3. Model Structures Used in Studies of Induced Demand Causal Relationships

Studies	Control Variables						Estimation	
	Population	Income per capita	Population Density	Employment Density	Gasoline Prices	Fixed Effects	Instrument Variables	Distributed Lags
Hansen *et al*. (1993)	✔	✔	✔		✔	✔		✔
Hansen & Huang (1997)	✔	✔	✔		✔	✔		✔
Fulton *et al*. (2000)	✔	✔				✔	✔	✔
Noland & Cowart (2000)	✔	✔	✔			✔		✔
Cervero & Hansen (2001)	✔	✔		✔	✔	✔	✔	✔

to measure induced travel demand in its many forms and shapes. Recently, however, research has probed the degree to which road improvements and travel demand are co-dependent – i.e., do road investments both induce and respond to travel demand? Studies that have sought to disentangle the two-way causality between road investments and travel demand have turned to econometric approaches in this pursuit. Table 3 summarizes the model structures employed by these studies.

To date, two approaches have been used to sort through the causal chain between road supply and demand: (1) Granger Causality Tests; and (2) Instrument-Variable Estimation. Both approaches are reviewed below.

5.1 Granger Causality Tests

Several studies have applied Granger Causality Tests to probe road supply-demand relationships (Granger, 1969). Fulton *et al*. (2000) applied GrangerTests to study the time-order of road improvements and VMT expansion. Using one and two year forward and backward lags to explain VMT growth as a function of lane-mile expansion, the authors found backward lags to be significant while forward lags were not. They inferred that in the mid-Atlantic states, lane-mile growth preceded growth in VMT, but not necessarily vice-versa. They cautioned that these findings do not imply lane miles caused increases in VMT.

Cervero and Hansen (2001) also used Granger Tests over two-year lags to examine road supply-demand relationships in California. Similar to Fulton *et al*. (2000) they found lane-mile expansions significantly accounted for VMT increases, however unlike the study of mid-Atlantic states, they found the relationship worked in both directions – past VMT increases also explained lane-

mile expansion. Their research suggested that, in California at least, causality works both ways – supply induces demand and likewise, demand induces supply.

5.2 Instrumental Variable Estimation

Instrumental-variable estimation, when applied in a simultaneous equation structure, is widely considered to be the most reliable and internally valid way to reducing estimation bias and capturing multivariate causal relationships (Pindyck and Rubinfeld, 1998). Several recent metropolitan-scale studies of induced demand have applied reduced-form instrument-variable models.

Noland and Cowart (2000) built models to estimate elasticities using instrument variables, though they did not simultaneously model supply-demand relationships.[9] 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 endogenous 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. This posed potential identification problems in specifying a simultaneous set of equations. The authors derived fairly high long-run elasticities in the range of 0.65 to 0.90.

A recent study by Cervero and Hansen (2001) also used instrument-variable estimation to resolve estimation bias problems, however unlike previous work this analysis introduced a simultaneous equation structure. Using data on 34 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, controlling for fixed effects as well as the tendency for travel to increase with population and per capita income and decline with employment density and gasoline prices. (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" – supply responding to demand. The analysis revealed an elasticity of freeway and highway capacity with respect to VMT of 0.33. Presumably, state highway investment in any year was based on levels of travel demand that were anticipated – suggesting, in California at least, road investments not only stimulated travel demand but responded to it as well. The analysis revealed that, also controlling for fixed effects, road investments rose with population size, carbon-monoxide emissions (lagged several years), temperature differentials, and democratic governorship, and declined with employment density. Prior-year slippages in air quality were interpreted to have added momentum to road investment under the premise that

congestion relief improves air quality. The influence of party affiliation reflected the time-lag structure in highway investments, with the impacts of a slowdown in highway construction engineered under Democratic leadership being felt by the time Republican governors were in office.

In general, models that have sought to account for two-way causality – with varying degrees of success -- in explaining induced demand effects have yielded somewhat lower elasticity estimates (in absolute terms) than those based on standard OLS estimation. This suggests some coefficient estimates from OLS models (e.g., Hansen and Huang, 1997) might be biased upwards. Still, econometric studies carried out in different parts of the country and that employ somewhat different model specifications have produced fairly consistent results (Noland and Cowart, 2000; Cervero and Hansen, 2001).

6. 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 (1996, p. 149) 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". This section probes the degree to which empirical evidence bears this out. It should be noted that the findings presented are inter-related and therefore partly redundant – e.g., studies on how elasticities differ by population densities also generally reflect the influences of congestion levels since streets in dense areas tend to be more crowded.

6.1 Congestion Levels

Do induced demand effects vary according to 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, though offered no empirical evidence to substantiate this position. The study also skirted the question as to what are the primary sources of induced demand – whether new latent trips, or schedule, route, or modal shifts. 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) uncovered evidence that traffic volumes increased with volume-to-capacity levels as well as population density following road improvements in Texas cities,

however 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.

6.2 Urban Setting

Because urban areas are denser and more congested, they could be expected to experience greater induced demand effects than suburban, exurban, or rural settings. Again, the evidence is muddled. Using the 1995 U.S. Nationwide Personal Transportation Survey (NPTS), Barr (2000) found slightly higher travel-time demand elasticities in urbanized areas (-0.36) than non-urbanized 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 that 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 under-prediction 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.

6.3 Metropolitan Size

Big metropolises could be expected to registered greater induced demand effects since they are known to 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 the 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.

6.4 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 – e.g., a beltway versus radial link? Luk and Chung (1997) postulate: "A radial route with added capacity could be less likely to generate demand than a circumferential route". Presumably this is because a circumferential faces less competition in the sense 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 37 Texas highway project, Henk (1993) found a greater induced demand effect on circumferential than radial highways, and the latent trips added to orbital facilities increased in direct proportion to population densities. An earlier study of 54 U.S. metropolitan areas found slightly higher elasticities of VMT with respect to beltway mileage (0.12) versus non-beltway mileage (0.10) (Payne-Maxie, *et al.*, 1980).

6.5   Summary

Overall, the few studies to date that have tried to statistically measure how road investments interact with other factors to induce travel demand have yielded inconclusive results. A literal interpretation of empirical findings would be that induced demand effects do not vary tremendously across settings – whether densely populated, highly congested urban areas or sparsely inhabited, less-congested exurbs. While common sense suggests this is not the case, so far the collective research community has been unable to jettison this "null hypothesis". This is probably more of an indictment of methodological tools and their inability to provide fine-grain insights into the induced demand phenomenon than an aspersion of the idea that induced demand impacts vary. Clearly, more and better research is needed on how induced demand effects vary across different settings and contexts.

7. META-ANALYSIS

As case-based analyses, most metropolitan-scale studies of induced travel demand have limited external validity. State-level and national analyses are more generalizable however the resolutions of these analyses raise questions of potential aggregation biases. One approach to generalizing research findings is a "meta-analysis" of elasticity estimates – i.e., essentially calculating an arithmetic average across many studies. Based on a meta-analysis of more than 100 road expansion projects in the United Kingdom, Goodwin (1996) estimated long-term elasticities (i.e., percent increases in traffic as a function of travel-time savings) of nearly |1.0|. More recent analyses suggest this number is on the high side.

Reviews of various empirical studies of induced demand by Ruiter *et al.* (1979), Pells (1989), Hansen *et al.* (1993), Cohen (1995), Goodwin (1996), and Noland and Lem (2000) found estimated elasticities with respect to road supply ranging from 0.1 to nearly 1.0. Studies reported by Goodwin (1996) and Noland and Lem (2000) were generally more recent than those of earlier reviews. Nevertheless, the reviews are essentially in agreement that road improvements induce demand; they disagree, however, with regards to the magnitude and sources of impacts. Empirical findings from studies cited in these reviews were dramatically affected by factors like the resolution and time expanse of analyses, the level of aggregation, measurement, and model specification. Why the magnitude of impacts varied so much across these studies was unanswered. While past reviews provide a good perspective into the evolution of induced demand studies, one must interpret summary findings with caution because they include studies of questionable reliability.

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 20 years that relied mainly upon matched-pair comparisons, the percent of traffic growth attributable to induced demand takes on the following mean values: short term = 0 percent; intermediate term = 26.5 percent (standard deviation = 35 percent); long term = 63 percent (standard deviation = 28 percent).[10] 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. As noted earlier, the difficulty of reliably measuring travel time benefits for a large geographic area have prompted most analysts who studied induced demand at an area-wide scale to employ lane-miles as a metric of supply-side benefit. 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 simulation studies and disaggregate analyses) produced the following mean elasticities estimates: short term = 0.48 (standard deviation = 0.13); long term = 0.80 (standard deviation = 0.12).[11] In comparison to facility-specific studies, not only are impacts more significant at the area scale of analysis, results are also far more similar, with low standard deviations. It is important to note that area studies that have attempted to resolve the simultaneous-equation biases of past work by accounting for two-way causality – namely the work of Cervero and Hansen (2001) and Noland and Cowart (2000) – have produce similar and consistent results. Even when accounting for the fact that roads lag behind and respond to VMT growth, recorded induced demand

effects remain strong. Overall, area studies using lane-miles as a predictor and econometric estimation approaches have yielded surprisingly consistent results -- ones that gravitate toward a central tendency.

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. Goodwin (1996) provides the most authoritative summary of travel-time elasticities from area studies – i.e., travel consumption as a function of travel times (what have been called "partial" elasticities in this paper). Drawing from recent British experiences, he concluded elasticities ranged from –0.5 in the short term to nearly -1.0 in the long term. DeCorla-Souza (2000) obtained even higher estimates (in absolute terms) in his recent simulations of travel in Memphis. 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.

Disaggregate analyses of induced demand have generally yielded lower estimates. Both studies cited in this paper 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 – the lower value from Strathman, *et al.* (2000) based on lane-mile per capita as a predictor and the higher value from Barr (2000) based on travel-time as a predictor. These estimates could be on the low side because of the use of cross-sectional data for inferring causal relationships as well as the questionable compatibility of data from the NPTS and TTI sources. While disaggregate studies get high marks for using a more appropriate "ecological unit" for studying induced demand, they too raise questions of reliability in the use of aggregate (metropolitan-wide) data on road supplies and travel times that do not necessarily correspond to conditions experienced by the very small sample of regional travelers surveyed by NPTS.

If one believes the results of recently published studies on face value, the preponderance of research suggests that induced demand effects are significant, with most added capacity being absorbed by increases in traffic. The pattern appears to be that within a year or two of a road improvement, on average, nearly half of the capacity added is filled by new traffic. And five or more years downstream, upwards of three-quarters or more of the extra capacity gets absorbed. To the degree that the many specification, measurement, and resolution issues raised at the beginning of this paper have been problematic, these estimates have less credence. There remains some disagreement as to whether this is the case.   As Pickrell and Cohen suggest in the proceedings, past elasticity estimates, based on lane-mile and travel-time inputs, have likely been inflated.   All that can be said with certainty is that induced demand effects exist (i.e., elasticities vary from zero) and they accumulate over time. To the degree the normative framework presented earlier (Figure 1) can be invoked in

future research, hopefully a greater degree of consensus can be achieved on the scope and magnitude of induced demand effects in coming years. 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 like levels of congestion and urban-versus-rural setting have proven unsuccessful. This is an area worthy of more research attention.

8. CONCLUSION

Those who have probed the theory of induced travel demand point out that diverted trips should be excluded from the measurement of induced demand. They argue that, in the short term, induced trips are: latent trips (suppressed trips that are now made because travel costs are lower); modal shifts (such as from transit, meaning new cars are added to the highway network); and destination shifts (which might lengthen, or shorten, trips). While this technical definition provides a sound basis for gauging net increases in traffic within a region, in political terms, the traffic additions motorists complain about several years after a road is improved includes diverted trips. When critics contend "pave it and they will come", they are referring to the increased traffic and return to former speeds a year or so later after a road improvement. Increased traffic and a return to congested conditions reflect the impacts of diverted trips as well as truly induced demand.

In the U.K., the cumulative weight of evidence on induced demand effects has clearly affected public policy. In 1998, the U.K. Department of Environment, Transport and the Regions (DETR) published a White Paper, *A New Deal for Transport: Better for Everyone*, that jettisoned the previous policy 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, 2000). Goodwin (1999) noted this has elevated alternative transportation programs, like expanded transit services and demand-management, to a higher status. So far, claims of induced demand have failed to resonate as deeply in America's urban transportation policy circles.

At times, the transportation research community's near-obsession with measuring induced travel demand has perhaps caused us to loose sight of "the forest through the trees". Whether the elasticity of VMT with respect to road investments is 0.10, 0.50, or 0.90 is less important than understanding which transportation investment and management strategies provide the greatest social and economic payoff. All too often, urban transportation research takes a reductionist perspective. While the induced demand phenomenon is important and not to be trivialized, far more energies need to go toward studying how America can best invest and manage scarce urban transportation resources –

e.g., should we be building more bus rapid transit systems, expanding value-pricing on former carpool lanes, or more closely integrating transportation and land use, and if so, when, where, and under what conditions?

Upon immersing oneself in the considerable literature on induced travel demand, one might get the feeling that in building roads to fight traffic congestion, America has been taking a step backward for every step forward. Are we really making little headway in relieving traffic congestion through road expansion? Statistics cannot answer this question, however anecdotally, traffic congestion was a lot worse during the pre-freeway era in the early part of the 20th century than the past half-century of freeway expansion. And it is a lot worse in many mega-cities of the developing world with meager road systems in place (even given their much lower vehicle ownership rates).

The problems people associate with roads – congestion, air pollution, and the like -- are not the fault of road investments *per se*. These problems stem mainly from the unborne externalities from the *use* of roads, new and old alike. They also stem from the absence of thoughtful and integrated land-use planning around new interchanges and along new corridors. In opposing any and all highway investments, even those backed by careful benefit-cost analyses, critics are fighting the wrong battles. Energies should instead be directed at curbing mis-pricing in the highway sector and at better managing land-use changes spawn by road investments.

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NOTES

[1] From a transportation planning and policy perspective, metropolitan and regional scales ideally reflect "commutersheds" – the geographic extent in which labor is drawn by firms and businesses in urban areas. In truth, metropolitan areas, as defined as metropolitan planning organizations (MPOs), are often spatially smaller than commutersheds.

[2] The authors called changes in routes without changes in origins, destinations, or modes "diverted traffic", changes in mode "converted traffic", and changes in desire lines due to changes in origins or destinations "shifted traffic".

[3] The ability to measure elasticities depends on the precision in which capacity expansions are measured. Many early studies expressed impacts in terms of percentage increases in traffic volumes over and beyond what was expected. Capacity expansion was not defined in percentage terms, thus elasticities could not be measured.

[4] Elasticities for new facilities can be imputed by gauging relative changes in traffic volumes to relative changes in total road capacity for a sub-area or corridor versus a specific facility.

[5] A point elasticity takes the form of $\partial \log(Y)/\partial \log(x)$ 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 $(\partial Y/Y)/(\partial X/X)$, however they yield different and not always symmetrical results at different points along the surface.

[6] The "system supply index" equaled 100,000 * ((5* freeway miles) + (arterial miles))/population.

[7] Relationships were generally not models in log-linear, but rather linear, form. Midpoint elasticities were estimated by multiplying the estimated partial regression coefficient on the road-capacity variable by the ratio of mean values of travel demand and road capacity.

[8] While the use of NPTS might be viewed as a "national" glimpse of the induced demand phenomenon, because these studies used metropolitan-level data to gauge road capacity they are actually "inter-metropolitan" in scale.

[9] Rather, the analysis used instrument-variable estimation in an attempt to remove endogeneity biases in the estimation of VMT as a function of lane-miles, however it did not model the concomitant effects of VMT changes on lane-mile additions, presumably because limited numbers of exogenous variables posed identification problems.

[10] For studies that reported a range of estimates, the midpoint of the range was used in the calculation.

[11] Excluded from this calculation are the estimates by Strathman *et al.* (2000), a disaggregate study, and Rodier *et al.* (2001), a model simulation. For studies that reported a range of estimates, the midpoint of the range was used in the calculation.