The effects of transportation infrastructure on cities: A review of the evidence

Lectures for EAERE Summer school, Venice, 2009[1]

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updated 10 July 2009

1. Introduction

We would like to understand the effect that transportation investments have on the way an economy operates, on the welfare of its residents, and on the environment. Ideally, our understanding is sufficient to be of use to the World Bank, where they must confront questions like "How many roads should we build in Kuala Lampur, and how should the network be arranged?"

We can imagine two classes of answers to this question. The first is positive. We can try to make statements like "Adding 1km of roads will cause 10 jobs to be created and 100 more km of driving per year". This class of answers allows us to assess whether infrastructure will achieve particular policy goals, and also to compare the cost of achieving policy goals across different policies. If we know that adding a road creates one in a city we can compare the cost of this road with an increment to educational attainment of tax policy which also creates one job. The second class of answers is explicitly normative. We can try to make statements like "Adding 1km of roads will be welfare increasing".

The current state of knowledge allows substantive positive statements about the effects of transportation investment and also an assessment of the cost effectiveness of these policies. We have a very limited ability to make absolute welfare statements about the effects of transportation infrastructure, but do somewhat better at ranking projects according to their effects on welfare. Our understanding of the effects of public transit is dramatically inferior to our understanding of the effects of roads.

In the abstract, the problem of understanding the effects of transportation investments can be resolved easily. We would consider a set of identical cities, treat half of them with roads systems, designing the systems carefully so that the sample variation allowed us to think about the implications of different extents and network designs, and watch what happened over the next two or three generations. Given such an experiment, it would fairly straightforward to resolve the questions I pose above.

While one can just barely imagine the World Bank 'treating' a random sample of cities with subways or similar investments, we probably should not wait until the results of this experiment are available until we make out infrastructure spending decisions. Thus, we are left to make what inference we can by observing the world around us, generated as it is by an equilibrium rather than an experimental process. Such data poses at least three important problems.

First, transportation investments, roads and public transit, are often very long lasting: Rue St. Jaques in Paris was built by the Romans, the US interstate System has so far lasted several decades. This means that an evaluation of these investments needs to be done over generational time scales in which land use, trade patterns, the locations of populations, and production patterns can adjust to the new infrastructure.

Second, since data are generated by an equilibrium rather than an experimental process, cross-sectional inference is problematic. If we see more development in cities with more roads, is this because the process which assigns roads to places favors those which are developing faster, is it because roads cause growth, or do roads attract more productive people and capital? The welfare implications of these three stories are completely different. In the first case, roads have no causal effect on the development process, in the second they do, in the third, they serve to move development from one place to another.

Third, it is difficult to make inference about the welfare effects of road investments in this environment. At a minimum, we confront the standard specification problems that beset any effort to make welfare statements: have we specified utilities reasonably, are we focusing on the empirically relevant margins of substitution, have we separated demand effects from supply effects, have we dealt with distributional issues reasonably, etc. However, since we consider long run problems, we also need to worry about migration and sorting. The good news is that many investments in roads can be expected to be capitalized into land prices, and land rent is relatively a straight forward to measure and interpret. More generally, in an environment with free mobility, anything that attracts people or economic activity to a city is probably good. [2]

These notes discuss the available econometric approaches to the problem of inferring the effect of road infrastructure on cities and their inhabitants and the results available in the extant literature.

2. Econometrics

To begin, consider a set of cities, indexed by $i \in \{1,..,N\}$ observed at some set of times $t \in \{1,...,T\}$. Let y denote the dependent variable of interest, this will be something like VMT or population or productivity. Let R denote the explanatory variable of interest, roads or transit, X a vector of other observed characteristics, ϵ unobserved characteristics, and Δ the difference operator, $\Delta_t y = y_t - y_{t-1}$. Our problem is to establish the nature of the causal relationship between R and y, if such exists.

A. *Equilibrium vs. Growth equations*

There are two main ways of thinking about estimating equations for these problems. The first is as an equilibrium equation. Here we think of our estimating equation as describing a static equilibrium. This gives rise to a levels on levels equation,

$$y_{it} = AR_{it} + BX_{it} + \epsilon_{it}, \tag{1}$$

or its first difference equivalent,

$$\Delta y_{it} = A\Delta R_{it} + B\Delta X_{it} + \Delta\epsilon_{it}, \tag{2}$$

Bento, Cropper, Mobarak, and Vinha (2005) illustrates the first, while Baum-Snow (2007) illustrates the second. This equation is right if we think that outcomes are determined by

[2] A caveat applies here: This is a partial equilibrium result. If we consider an economy with a system of cities then all cities may be too big. In this case attracting more people to a city may not be good. The empirical strategies I discuss below are consistent with partial equilibrium. We try to estimate the impact of changing infrastructure in one city, holding all other cities constant.

contemporaneous variables. The important conceptual point here is that these equations don't allow for frictions.

The alternative is something that looks like a growth equation. In this case, you have something like

$$\Delta y_{it+1} = AR_{it} + BX_{it} + \epsilon_{it}, \tag{3}$$

or

$$y_{it} = A_0 R_{it} + A_1 R_{it-1} + BX_{it} + \epsilon_{it}, \tag{4}$$

which looks an equation of motion derived from a growth model. We have a state variable R and we would like to know how y evolves in response to changes in R (our control variable is going to be something like investment in R). We use an equation like this if we are worried about the way that y adjusts to changes roads. Examples of this second approach are Burchfield, Overman, Puga, and Turner (2006), Duranton and Turner (2008*a*) and Chandra and Thompson (2000).[3]

The choice of equilibrium vs. growth equation should be governed by the details of your problem and the time-frame of interest. However, a cautionary note is in order. The theoretical literature describing growth of cities is different than the literature describing the growth of countries because it must allow for more mobility of factors across cities than across countries. This literature is difficult and still primitive.

B. *Estimation*

In order to think about our third question, how to isolate a causal effect of roads on our dependent variable, we need to discuss estimation techniques. In particular, we would like to estimate

$$y_{it} = AR_{it} + BX_{it} + \epsilon_{it}. \tag{5}$$

Our problem is that we suspect that R is not assigned to cities at random, and in particular, we hope that roads are assigned to cities which are experiencing rapid growth and fear that they are assigned to cities to prop them up after they receive some sort of negative shock. In either case, we have $cov(R,\epsilon) \neq 0$ and OLS does not give us an unbiased estimate of A.

One somewhat naive response to this problem is to resort to a growth equation. That is, to estimate

$$\Delta y_{it+1} = AR_{it} + BX_{it} + \epsilon_{it}, \tag{6}$$

as is done in Burchfield *et al.* (2006). This presents two problems. First, this equation describes a different process than the equilibrium equation, and so really represents a different hypothesis. Second, and probably of more practical importance, there is often persistence in the way that cities evolve. Thus, if we are concerned that $cov(R_t,\epsilon_t) \neq 0$, we worry that R_t and R_{t+1} are highly correlated and also ϵ_t and $epsilon_{t+1}$, which means

[3] As an aside, it is worth noting that people sometimes talk about these problems as being too complicated to allow us to describe equilibrium behavior, or for the system to ever reach equilibrium. If you really believe this, I think it complicates matters considerably. However, my view, which I think is orthodox, is that this type of talk indicates a fundamental misunderstanding of the notion of equilibrium. An equilibrium is simply a statement of how we think a group of people is going to behave. It is explicit in standard static models and in growth models. It is implicit in simulation models, and therefore may not be well understood. A system need not be stable, nor even converge to a stable path in order to be in equilibrium. It is in equilibrium if agents behave according to the rules we define.

that we ought to be concerned that $cov(R_t, \epsilon_{t+1}) \neq 0$. We should worry that this is going to make trouble in the growth equation: pre-existing conditions need not be exogenous. Burchfield *et al.* (2006) will provide an illustration of how this can make trouble.

A second approach is to look at a first difference estimator (or almost equivalent, a fixed-effect estimator). In this case, we write ϵ as the sum of two components, one city specific and time invariant, the 'fixed effect', and the other, city specific and time varying. Thus we have

$$y_{it} = AR_{it} + BX_{it} + \delta_i + \mu_{it}. \tag{7}$$

Taking first differences, we are left with,

$$\Delta y_{it} = A\Delta R_{it} + B\Delta X_{it} + \Delta\mu_{it}, \tag{8}$$

any time-invariant explanatory variable drop out, including the unobserved component δ. Thus, to estimate A we now require that $cov(R,\mu) = 0$, not the stronger $cov(R,\delta + \mu)$.

In words, it is OK if roads are assigned to cities on the basis of some constant but unobserved feature of cities. We have trouble only if roads are assigned on the basis of some time varying city specific trait. Thus, if roads are assigned to cities after they receive a productivity shock, we will confound the effect of roads with the effect of this shock.

There are a couple of responses to the endogeneiety problem in FD estimations. First, endogeneity leads to serial correlation in residuals. It is possible to test for this (see Chandra and Thompson (2000)). A second approach is instrumental variables estimation, which I discuss below. The third approach involves ad hoc robustness checks. For example, if we are concerned that cities which get positive shocks respond to roads differently than those that don't, we might restrict our sample to cities which had growth rates within one standard deviation of the mean and see if this affects our estimates of A, see Eid, Overmna, Puga, and Turner (2008) for an example in a different context.

Finally, we come to instrumental variables estimation. Instrumental Variables estimation requires that we estimate two equations. Loosely, in the first equation we are trying to simulate random experimental data, and in the second, we run the regression that we would really like to run.

Mechanically, IV requires that we estimate two equations, the first is a first stage or reduced form equation. In this equation we use our observed data and an instrument to generate random data,

$$R_i = CX_i + DZ_i + \mu_i. \tag{9}$$

In the second stage or structural equation, we base our estimate of the effect of roads on the relationship between predicted roads and y,

$$y_{it} = A\widehat{R_{it}} + BX_{it} + \delta_i + \epsilon_{it}. \tag{10}$$

The validity of this procedure hinges on finding a magic variable Z, our instrument, which has the following two properties:

$$cov(Z,R|X) \neq 0$$
$$cov(Z,\epsilon|X) = 0.$$

The first of these conditions requires that Z have some ability to predict R conditional on controls. We can assess whether this condition holds by looking at the first stage regression results. The second condition is the exogeneity condition. This is more difficult to check. If you have more than one instrument an over-identification test is available, passing it is logically equivalent to getting the same parameter estimate with each of your instruments. As such, it is informative only if your different instruments are sufficiently different form each other. If one instrument is the square of the other, the over-id test doesn't really teach me anything.

IV estimation is often done badly and does not lead to credible results. See Wooldridge (2001), Stock and Yogo (2005) for an introduction to theory and practice. For nice examples, look at Baum-Snow (2007), Michaels (2008), or Duranton and Turner (2008*a*).

3. The effects of roads

There is a reasonably extensive literature examining the relationship between infrastructure and interesting economic variables that describe the way cities work. This literature suggest the following four stylized facts about roads:

- Roads increase the productivity of adjacent land, partly at the expense of more remote areas.
- Roads increase the population density nearby land.
- Roads change the composition of production and population.
- Roads disportionately attract wealthier people.
- Roads decrease density in cities.
- An increase in roads causes an exactly proportional increase in city VMT.

The literature suggests the following stylized facts about transit,

- Transit increases the population of cities.
- Transit disportionately attracts poorer people to cities.
- An increase in transit does not affect city VMT.

A. *Literature*

The available literature is small enough that we can talk about most of the papers one at time.

B. *Bogart, 2009*

Bogart (2009) assembles county and parish level data for 18th century England which describes land rents at the parish level and whether there is a 'turnpike trust' near the parish or county. The presence of a turnpike trust indicates the presence of a private improved toll road, and there is good reason to think that it was much easier to move freight along such roads than along other roads. The data also indicate whether the particular Parish/County lay on a major trade route.

He observes these data at two points in time, about 1700 and about 1800, and thus is able to conducts a first difference regression:

$$\Delta y_{it} = A\Delta \text{Turnpike Indicator}_{it} + B\Delta \text{Observed Controls}_{it} + \Delta\mu_{it}. \tag{11}$$

He finds that a turnpike increases land rent within 5km by about 20%. This regression corrects for time invariant factors which affect both the incidence of turnpikes and land rent, but does not control for the possibility that turnpikes are assigned to places that are growing anyway. This is a particular problem since most turnpikes are built during the study period.

To resolve this issue he uses an indicator variable for whether the parish was on a major trade route in the 17th century, i.e. the century preceding the study period. He then estimates the system:

$$\begin{aligned} \Delta y_{it} &= A\Delta\ \widehat{\text{Turnpike Indicator}}_{it} + B\Delta \text{Observed Controls}_{it} + \Delta\mu_{it} \\ \Delta\widehat{\text{Turnpike Indicator}}_{i} &= A \text{ Old trade route}_i + B \text{ Observed Controls}_i + \gamma_i. \end{aligned}$$

This leads to a higher estimate of the effect of turnpikes on land rent.

The two very nice features of this paper are that it measures land rent directly and that it makes a serious attempt at dealing with the endogeneity of roads. With this said, I am sceptical of this instrument. I suspect that trade increased during the early parts of the industrial revolution and that this led land rents to increase along trade routes independent of whether they had roads. Thus, 'on a trade route' could contribute land rents independent of its effect on roads thereby invalidating the instrument.

C. *Duranton and Turner 2008a*

Duranton and Turner (2008*a*) examine the effect of roads on population and employment growth. Theory tells us that population and employment growth are directly related to land rent, so that these results also inform us about the effect of roads on the efficiency of production.

Duranton and Turner (2008*a*) construct data for all 275 1999 msa's in the continental US which records population and employment growth between 1980 and 2000, along with a measure of miles of major roads per msa and the count of large buses. They estimate a growth type regression equation, like equation 6, and instrument for roads with msa miles of the highway on the 1947 highway plan and miles of major 1898 railroad routes. They find that a 10% increase in msa road miles in 1980 causes about a 2% increase in population or employment over the period 1980-2000. Examining this finding in more detail, they find that this same 10% in 1980 causes about an 1.3% increase in population between 1980 and 1990, and about a 0.7% increase in population between 1990 and 2000. That is, roads affect growth in both decades, but at a decreasing rate. Assuming the effect of roads on population decays at the same constant rate in successive decades suggests that the total effect of a 10% increase in roads is about 3.5% population and employment growth of decay. This effect is about the same magnitude as the 80 year effect on population growth of a 10% increase in 1898 rail miles.

Repeating these regressions where the count of large buses is also included as an explanatory variable suggests that a 10% increase in the stock of large buses causes about a 0.8% increase in population and employment over the period 1980 to 2000. Since a percentage increase in buses is orders of magnitude less costly than a corresponding increase in roads, this suggests that for a wealthy country with an already well developed road network, purchasing buses is a more cost effective way to make cities attractive to people and jobs than is building roads. Unsurprisingly, Duranton and Turner (2008*a*) also find that cities with more roads tend to attract slightly wealthier immigrants, while cities with more buses tend to attract slightly poorer immigrants. A caveat is appropriate. The instrument used to identify the causal effect of buses is 'share of democratic voters in 1972'. While this instrument passes standard tests, one can imagine it affecting growth independently of its effect on buses.

In addition, Duranton and Turner (2008*a*) find that OLS estimates of the effect of roads are smaller than IV estimates. They conclude that this occurs because the equilibrium process for assigning roads to cities tends to assign roads to places which, conditional on other controls, are growing more slowly than average. That is, roads are built in response to negative shocks. While this is an implication of the difference between the IV and OLS results, by looking at the history of road building and employment shocks they confirm this finding directly. This is a very interesting finding, but one whose welfare implications are ambiguous. Planners may build roads after a shock because this is when land and labor are cheap, alternately, they may be using roads as way to attract people to places where they do not otherwise want to live.

Duranton and Turner (2008*a*) find no evidence that roads differentially effect employment in different sectors. This differs from findings in both Chandra and Thompson (2000) and Michaels (2008), although these authors are explicitly examining rural counties, while Duranton and Turner (2008*a*) examine exclusively urban counties.

Finally, to understand whether roads attract people to a city by making the city more attractive to workers or employers, or by increasing its access to other markets, Duranton and Turner (2008*a*) check whether road networks in nearby large cities affect population and growth in a city. They find no evidence to support this claim. This suggests that roads affect cities by making them more productive or attractive.

D. *Michaels 2008*

Michaels (2008) is principally interested in testing a hypothesis about 'globalization', in particular, that as trade costs decrease wages for skilled workers in places with lots of skilled workers should rise, and conversely that wages for unskilled workers should rise in places with lots of unskilled workers. To test this hypothesis he assembles data for 1972, 77, 82,87, and 92 that describes county level earnings in trucking and warehousing, in retail, and in skilled and unskilled manufacturing, along with data describing the interstate highway network. His measure of highways is an indicator variable indicating whether the county has a highway in a particular year. He conducts first difference type regressions and instruments for the presence of roads using the 1947 highway plan and a variable describing whether the county a county is due east or west or south or north of the nearest big city (as opposed to southwest etc.)

He finds that the addition of a road to a county has a statistically significant ability to predict increases in earnings in trucking in the contemporaneous five year period and that the magnitude of this effect ranges between 1-16% depending on the year and specification. This suggests that roads either increase the ability of these rural counties to trade with the outside, or cause them to specialize in moving goods.

He also finds that the addition of a highway causes an 8-10% increase in retail earnings in the period in which roads were added. This suggests that people in these counties are spending more, which in turn suggests that they are earning more. Thus, Michaels (2008) results are consistent with roads causing a large increase in real wages over the course of just a few years. Interestingly, Michaels (2008) finds that roads have only a small effect on the returns to skill in manufacturing and on the share of skilled workers in manufacturing. Results in Chandra and Thompson (2000) will suggest that this is because he was looking in the wrong place.

E. *Chandra and Thompson 2000*

Chandra and Thompson (2000) look at the effect of roads on economic activity in rural US counties. To do this they assemble panel data describing annual earnings data from 1969-93 by county for 1-digit SIC codes. Counties which contained an interstate highway in 1969 were excluded from the sample. The 185 counties which received a highway between 1969 and and 1996 are coded as 'highway' counties. 392 counties are 'adjacent' to highway counties and the remaining 1204 counties are control counties.

Our expectation is that the effect of a highway on economic development in a county depends on the length of time since the road has been built. As a silly example, we expect to see a big increase in construction employment around the time of construction and that this employment will quickly dissipate. To investigate this evolution, for each county year Chandra and Thompson (2000) construct an indicator variable R_{it}^T which is one if in year t county i has a road that is T years old. They construct the analogous set of indicator variables for counties adjacent a road county. They then perform the following regression for each industry,

$$\begin{aligned}\text{Earnings}_{it} = A_0 + A_1 \cdot \overline{R}_{it} + \\ &A_2\text{year dummies} + A_3\text{county dummies} + \\ &A_4\text{State level earnings (other industries)} + \epsilon_{it}.\end{aligned}$$

That is, they look at the effect on industry earnings in a county of having a road of age T, after conditioning out county mean earnings over the sample, national mean earnings for the year in question controlling for the level of economic activity in rural counties in the state year. They perform this regression for three different samples. In the first sample they use all of the counties in their sample. In the second they drop adjacent counties, so that they are comparing road counties with counties far from roads. In the third they drop road counties, so that they are comparing counties near roads with counties far from roads.

This is a very elegant research design and it allows them to trace out the economic evolution of these counties as a function of time since they received their roads and as a function of proximity to roads. The fundamental identifying assumption is that the arrival of roads is not correlated with changes in productivity. If this endogneity problem arises it implies serial correlation of errors. Chandra and Thompson (2000) test for this serial correlation and do not find it. Note that Chandra and Thompson (2000) estimate the effect of a road in the last five years on road county retail earnings at about 5%. This is quite similar to the corresponding estimate of 8-10% in Michaels (2008). This is consistent with both authors having resolved the endogenity problem, or less likely, with their having failed to resolve it and still getting the same answer.

Chandra and Thompson (2000) find that overall economic activity in road or adjacent counties increases by about 4% by 25 years after the road is constructed. However, this increase is unequally distributed across space, time and industries. Overall, adjacent counties lose about 3% of earnings 25 years after a road is built, while road counties gain about 8-10%. Thus, some of the benefit to road counties results from the migration of economic activity from neighboring counties. Roads also affect different industries differently. In road counties, construction and farming are harmed while retail, services, business services and government all grow, manufacturing is mostly unchanged. In adjacent counties, manufacturing and business services increases and most other industries decrease. After the construction of a road, it is several years before government earnings increase, but business services increase almost immediately and almost monotonically over 25 years.

If we think that business services requires more highly skilled labor than farming and construction, this is very similar to the sort of skill bias that Michaels (2008) searched for unsuccessfully. An further implication of this finding is that roads will differentially effect employment opportunities for different groups. With a road we will see more lawyers and bankers move in, and the farmers will move away, possibly to work in manufacturing in an adjacent county. This is consistent with the finding in Duranton and Turner (2008*a*) that roads disportionately attract wealthy people.

F. *Fernald 1999*

Fernald (1999) follows a literature begun by Aschauer (1989) which regresses measures of productivity on measures of road expenditure. The initial estimates in Aschauer (1989) did not recognize the probable endogeneity of these investments and were implausibly large. Fernald (1999) constructs annual data for 1947 until 1995 describing the value of productivity, road investment, and car use by industry. Somewhat reductively, he then regresses annual industry productivity on own year annual road expenditure. He deals with the endogeneity of these investments by exploiting a cross-equation constraint, in his model a weighted sum of industry errors must sum to the aggregate error, and estimating this structure explicitly.

He finds that interstate highway investment have a high return in national productivity during the period before 1973, about 10% per year, and an almost zero return after. He attributes this result to the fact that the interstate highway network was essentially complete after 1973, and that marginal improvements to the network were no longer as productive, and also to the fact that the interstate highway network became progressively more crowded after 1972, and thus (probably) more expensive to use.

Fernald (1999) results are not easy to reconcile with the results reported in Chandra and Thompson (2000), Michaels (2008), and Duranton and Turner (2008*a*). Each of these papers suggests that roads have important effects on earnings or productivity after 1973. Three resolutions of this puzzle exist. First, each of Duranton and Turner (2008*a*), Michaels (2008), and Baum-Snow (2007) find the the effects of roads a essentially zero when we do not control for endogeneity. Thus, it is reasonable to suspect that Fernald (1999) has failed to control for endogeneity in his estimates. Second, all of the other papers described here have considered much longer periods than the single years considered in Fernald (1999). It may Fernald does not watch long enough to see the effect of roads on the economy after 1973. That Chandra and Thompson (2000) find very small effects of roads in the year after they are built supports this story. Third, Fernald (1999) uses national data, while all of the other papers we have discussed use city or county level data. To the extent that city or county level road investments simply move economic activity from one place to another, it has no aggregate effect on productivity. Results is Chandra and Thompson (2000) provide some support for this. The fact that Fernald's 1973 cut-off coincides with an oil shock does and a recession does not. To the contrary, it suggests that the decrease in road investments may have been caused by the decrease in economic activity, rather than the converse.

At this point it is not clear how to resolve the puzzle. The preponderance of evidence supports the hypothesis that roads cause productivity to increase. However, this evidence is all based on small units of observation. The extent to which aggregate effects differ from marginal, city level effects merits further investigation.

G. *Baum-Snow 2007*

Baum-Snow (2007) conducts two exercises. In the first his unit of observation is a census tract. For each census tract in 1970 and 1990 he knows: population density; distance to the nearest limited access highway, the MSA in which it lies.[4], and the distance to the central business district. He finds that: conditional on distance to the CBD, moving 1 mile closer to a highway is associated with a 1-2% increase in density; conditional on distance to the CBD if a tracts distance to a highway decreases by 1 mile, its population decreases by 1-2% over twenty years. While it is possible that this reflects the fact highways are built through the densest areas and through areas where density is increasing, it is also possible that people are moving to highways. Indeed, Duranton and Turner (2008*a*) argue that highways are directed towards cities which experience negative shocks. If this result also holds at the sub-city level, then Baum-Snow (2007) is underestimating the ability of highways to attract population.

Note that the implication of Baum-Snow's finding finding for average density at the tract level is ambiguous. People accumulate near highways. Whether these people are drawn from denser or less dense tracts is unclear.

In his main exercise, for each msa Baum-Snow (2007) constructs population measures for constant area CBD population for 1950, 1970, and 1990. He also counts the number of limited access highways which lead in and out of the CBD, 'rays'. He then predicts changes in central city population as a function of changes in the number of rays, and a number of other controls. To correct for the fact that the number of rays may be a function of the population decentralization, he conducts an IV estimation in which he instruments for the number of ray in an MSA using the number of rays which occurred on a very early plan of the interstate highway system. He finds each additional ray causes about a 9% decrease in central city population density over the period from 1950-1990. Since the interstate highway system assigned between 2 and 3 rays to average city, this suggests that the interstate highway system caused an 18-27% decline in central city population density. This is most of the observed decrease over the period.

[4]An MSA is a 'metropolitan statistical area' as defined by the US census. Loosely, MSA'a attempt to approximate the extent of major US cities as collections of counties

It is worth noting that Baum-Snow (2007) finds that the estimated effect of rays is larger in magnitude in the IV than in the OLS. This is surprising. If we think that roads are assigned to places which are expanding, we should see that the effect of an average randomly assigned road (the IV) is SMALLER than the effect of a road assigned by the equilibrium process. This exactly the same finding as in Duranton and Turner (2008*a*).

In sum, Baum-Snow (2007) gives us a lot of insight into how highway networks shape cities. They cause people to concentrate near highways and further from central cities. He does not give us a lot of insight into the welfare implication of these changes. We suspect 'too much' migration out of central cities because congestion and pollution are unpriced. On the other hand, we know that people prefer these location because they choose them. These two effects work in opposite directions, and we cannot assess whether, at the margin, there are too many highways or not enough.

H. *Burchfield et al 2006*

Some of our only other direct evidence about the impact of transportation infrastructure on density. Burchfield *et al.* (2006) use data based on high altitude photographs in 1976 and satellite images from 1992 to characterize developed area, impermeable surface, in each of 275 1999 msa's. More specifically, for each msa they calculate the share of the surrounding square kilometer that is developed for an average house in the msa. As this index of sprawl increases, development is denser and as it decreases it is less dense. They conduct two types of regressions, levels on levels, as in equation 7 and also changes on levels as in equation 6. Their dependent variables are respectively the scatteredness of development in each msa and the scatteredness of development that occurred between 1976 and 1992. Their explanatory variables are, among several others, the per capita stock of streetcars in 1900 and a measure of the density of major roads.

They find that cities with more streetcars in 1900 were denser in 1992 and had denser new development between 1976 and 1990. This suggests that older denser cities that were laid out around public transit continue to be denser. While interesting, the principle practical value of this finding is probably just to remind us of how persistent are the effects of infrastructure spending.

They also find that road density per area does not affect the density of development. This is a perplexing result, and one which appears to directly contradict the results in Baum-Snow (2007). Baum-Snow (2007) and Duranton and Turner (2008*a*) suggest a resolution to this problem. Respectively, they find that roads have much smaller effects on density and population and employment growth in OLS estimates like equations 8 and 6 than in the corresponding IV estimates. That is, 'randomly' assigned roads have a bigger impact than roads assigned by the equilibrium assignment mechanism. It is reasonable to suspect something similar is behind the Burchfield *et al.* (2006) finding.

I. *Bento et al 2005*

Bento *et al.* (2005) looks at the ability of measures of urban form, measures of road density, and measures of public transit to explain outcomes in the NPTS, modal choice and vmt in vehicle and individual file.

Their data is based on 1990 NPTS, and their sample of cities is the 114 urbanized areas covered by this survey. For each urbanized area they calculate road density, miles of interstae highways per hectare, and the count of large buses.

Abstracting from non-linearities in their estimating equations, their estimating equation is of the form

$$\text{Individual Travel Behavior}_{ij} = A\text{Roads,Large buses}_j + B\text{individual controls} + \epsilon_{ij} \quad (12)$$

where i indexes individuals and j indexes urbanized areas. The measures of individual travel that they use are, probability of commuting to work (modal choice), Probability of car ownership, household vmt.

They find that road density is positively associated with the probability of riding a bus, increases vmt, and increases the likelihood of car ownership.

Since this is a cross-sectional regression it is not possible to infer a causal relationship from these results. It is possible that (1) infrastructure provision is determined by some unobserved feature that also affects transportation behavior, or (2) that individuals sort on the basis of infrastructure.

With this said, Bento *et al.* (2005) is one of the only papers that examines the link between urban form and driving behavior. They find small effects. In spite of inference problems, these are the best estimates available.

Making defensible causal inferences in this problem is very difficult. I think the way to do it would be to use a panel of individuals (so we could include an individual fixed effect) and to instrument for infrastructure.

J. *Duranton and Turner 2008b*

Duranton and Turner (2008*b*) investigate the effect that road and transit have on driving behavior. Their principle exercise is conduct a city level regression like the following:

$$\text{vmt}_i = A\text{lane miles}_i + B\text{controls} + \epsilon_i$$

To accomplish this, for each of 1983,1993,2003 they assemble msa level data extent of the urban and rural highway network, the network of urban arterial roads, along with the total vmt and total truck vmt on each system in each study year. As in their earlier work, they also record the count of large buses in each msa in each year.

The fundamental inference problem that they must resolve is that infrastructure is not assigned to cities at random.

To solve this problem they perform the regression above three ways. First as OLS in repeated the cross-sections. Second, as first differences the two repeated cross-sections (also pooling the data and using fixed effects), and finally using an IV estimator in which the 1947 highway plan, 1898 railroad routes, and the routes of major expeditions of exploration between 1820 and 1835 are used as instruments for the road network.

They conduct these regressions for all metropolitan interstate highways and vmt, for urban highways (oddly, much of the msa highway network is rural), and for urban arterial roads.

In every case they find that the road elasticity of vmt is positive, and almost never statistically different from one. That is, vmt increases in direct proportion to road miles.

This does not appear to result from substitution between road networks.

New traffic appears to have three sources. First, people change their driving behavior. A regression of household level annual VMT (from the NPTS) on lane miles shows that household vmt is increasing in lane miles. Second, people migrate to cities that are better provided with roads Duranton and Turner (2008*a*). Third, transport intensive activity increase with roads. That is employment shares in trucking and warehousing increase with roads as does truck vmt.

Finally, they find that the number of large buses in a city has no effect on vmt. This suggests that public transit is acting to increase capacity in exactly the same way as does increases in lane miles.

4. Conclusion

Understanding the effect of transportation infrastructure is both difficult and important. Our current state of knowledge suggests the set of stylized facts given above. These facts, together with information about the costs of given policy interventions provide a basis for assessing the cost effectiveness of different interventions. There is little basis for making direct calculations of the welfare impact of infrastructure.

Important areas for future research are to (1) improve our understanding of the effects of public transit on how cities develop, (2) to improve our understanding of the effects of how built environment affects travel behavior, (3) to improve our understanding of the way that infrastructure affects development outside of the US.

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