Report on Duranton and Turner, "Urban form and driving: Evidence from US cities"
QJE-25212-1

The paper potentially provides the best yet approach to an important economic question, namely: Can motor vehicle travel be changed significantly by dictating urban densities? The question has both descriptive and normative interest for the economics of cities.

The paper takes a dual approach. It derives a very nice rigorous quantitative theory, based mainly on aggregates within a reasonably small radius (10 km in their preferred specification). The theory helps them address both endogeneity and sorting as empirical problems. The paper then develops an excellent empirical strategy to estimate the desired relationship, taking endogeneity and sorting into account.

The empirical strategy is notionally based on the theory, which is somewhat helpful. However, the link is weakened by severe practical assumptions about the nature of sorting. The one I find most troubling is that the proxies we observe for the fraction of residents who are mobile over a 20-year period are exactly proportional to the actual fraction. Perhaps, as the authors assert, the main approach would survive relaxing them. But they make the interpretation of results (made in Section 7) less convincing.

The theory contains another strong assumption, namely that transportation cost is proportional to density to a power. Hence transportation cost approaches zero at low densities. This assumption is untenable at very low densities, yet the model is applied at low densities in Section 7 (highlighted in the Introduction). Even at median densities in medium-size MSAs, congestion is so small that I fear this assumption may vastly overstate the advantages of low densities to residents.

Throughout the paper, there are many places where the technical argument is flawed or unclear. It is difficult to determine whether the main arguments will still hold once they are corrected. These are described in detail below. I begin by expanding the point just made about low densities.

1. Form of equation (6) for travel cost:
   This equation implies that travel cost approaches zero as density does. Among other things, it implies that as density approaches zero, the number of trips N approaches infinity (from equation 3), and total vehicle mileage per household approaches either zero or infinity, depending on the sign of (ϕ-$\zeta\rho$). This is especially problematic when the resulting equation (8) for utility is used to describe how utility depends on density, as this assumption surely exaggerates utility achieved in very low-density areas.

   A more usual assumption would be that the congested portion of travel cost is a power function of density. Thus simply adding a constant to (6) would solve the problem. I'm not sure how much complication this would create in the subsequent equations. It would upset some of the neat results on the effects of particular parameter combinations, derived on pp. 9-10, but it might lead to more interesting and realistic results that show regions of density with different properties. For example, I conjecture that travel distance always decreases with

population density at low densities (conditional on θ), since congestion is not a factor there; whereas at higher densities the relationship depends on (ϕ-$\zeta\rho$) in the manner now stated just below equation (8).

2. Derivation of main theoretical result, eq (7):
I was unable to confirm eq (7) despite a lot of effort. There are at least three problems. First, the definition of $\overline{Y}$ as "mean travel distance" is ambiguous. I think this means that $\overline{Y}$ is the mean travel distance for all people living with the region R of size K that defines density at a point in space, i.e.

$$\overline{Y} = \frac{K}{X} \int_R Y(\theta) f_R(\theta) d\theta$$

where $Y(\theta)$ is given by (4) and $f_R(\theta)$ is the density function of preference parameter θ in that region. That would explain why the authors end up with an integral of $\theta^{\frac{1}{1-\rho}}$ in their definition of $\overline{\theta}$. Second, the equation defining $\overline{\theta}$ is notationally ambiguous, it being unclear what is meant by $j \in X$ (probably it means $\theta \in R$ in the notation I developed just above).

Third, it is unclear how the authors are handling the simultaneous determination of $\overline{Y}$ and τ in this process, since each depends on the other in equations (4) and (6). I tried computing $\overline{Y}$ from the equation I've written above, solving it and (6) for τ, then inserting that into (4). However it did not appear to be leading to (7) and finally I gave up.

3. Conclusions from equations (7) and (8):
In addition to the doubts I've expressed above about these two equations, the conclusions from them need to be qualified as to what exactly is being conditioned on. For the first conclusion at least, I believe it is conditioned on both θ and $\overline{\theta}$. Thus it is a statement about a hypothetical policy intervention, not about the equilibrium pattern observed empirically.

4. Sorting model (equation 15):
Generally, this is a good sophisticated model. I see two problems:
(a) There seems no good reason to suppose that everyone was in long-run equilibrium in 1990 but not in 2010. More likely, they were out of equilibrium in both years. This still leaves some opportunity for identification because in 2010 they will be closer to the equilibrium that would be predicted based on 2010 variables. So I think the strategy can still work, but needs a little more demonstration than the statement at the top of p. 14 than "generalizing … to describe this intuition precisely is straightforward and leads to an indistinguishable empirical strategy".
(b) The strategy is implement assuming the observed "proxy" for the share of residents s who have relocated over the 20-year period is, in fact, a perfect one: namely $\tilde{s} = s/\gamma_1$. This is not a proxy, it is just a change in units! To represent a proxy, one needs a stochastic relationship between s and $\tilde{s}$. I suspect this is feasible, and might lead to a different estimate of the role of sorting.

5. Discussion of results (Section 7A):
I found Section 7A extremely hard to follow. Formulas and auxiliary regressions are densely interspersed in verbal text form, without a lot of structure. For example, "We assume above

that $\theta = \bar{\theta} v$ " is hard to reconcile with the definition of how θ varies in (12). I suppose it is explained somewhere earlier in the text, but I could not easily find it. Another example: in equation (12) we have the empirical finding that β=-0.1, whereas later in the text it is stated that with sorting, β plus another quantity equals -0.1. I was unable to combine the three pieces of information given ($\theta = \bar{\theta} v$, eq 7, and eq 12) to obtain this latter result.

Another problem with this discussion is it does not facilitate seeing how sensitive these calculations are to the estimates, although this sensitivity or lack of it is mentioned in the discussion itself. It seems to me that for each parameter being measured, we need a formula in which the other quantities are either already determined or coefficients in a regression. We also need an estimate of standard error of the derived quantity. For example, writing the solution of (18) for ρ would clarify the statement that it is the ratio of two small quantities (both of which involve subtracting one much larger number from another), and would make it possible to translate this into a statement about the standard error of the derived estimate of ρ.

A third problem is that these auxiliary regressions might also be subject to endogeneity and sorting. How do we know such problems are not serious in these cases?

This whole subsection would benefit from a road map outlining the value of knowing these parameters, and the precision or lack thereof that is possible using these results.

6. Densification policies (p. 41):
The first policy considered is one of urbanization of rural population, not of changing urban densities. I can't see why I should trust results estimated on MSAs to evaluate results from such a policy. By contrast, the second policy considered is reasonable and enlightening.

However I cannot take seriously the cost-benefit analysis in the last paragraph of Section 7B. No realistic policy would proceed by tearing down housing; rather, it would alter new housing construction and/or the replacement of units scrapped for other reasons. The real costs of this policy are utility loss insofar as people are pushed into densities they do not prefer, offset perhaps by eliminating inefficiencies from market distortions that currently favor low densities (as many have argued). I suggest eliminating this paragraph, would also make the following subsection (Densification vs. other policies) obsolete - an advantage because there is not space in this paper to do justice to gas taxes and congestion pricing.

Expositional comments

p. 2 A minor point, but the "Brookings Institute" (actually Institution) does not "assert" statements that might appear in a blog on its website.

Equation (1): need to define Q and N, especially since notation i and N (later called "number of varieties") suggest discrete varieties but the theory treats i as continuous.

Eq (1) and the assumption that each variety requires a trip lead to the rather strong result (3), which shows that any increase in income W goes entirely to consumption of the numeraire (C) and none to increasing the varieties of goods. The authors might comment on how plausible this is, and how feasible it might be to relax it.

Eq. (4) and later: the term "total travel distance" could easily be confused with aggregate travel by all people. Here, it means total distance per unit time for a single resident, i.e. what is more commonly called household VMT or VKT (as it is called later in this paper).

p. 10 first line refers to "the accessibility externality", arising from (5). Is this really an externality? If so, what does this say about the optimality of the equilibrium?

Just below eq (12), should "$\beta>0$" read "$\alpha_1>0$"? Otherwise it's not clear why the statement is is this location.

p. 14, justification of subterranean geology as an instrument: One could imagine that elements of subterranean geology (e.g. aquifers, earthquakes, landslides) affect the ease of building highway infrastructure differently from that of building residences. If so, the instrument might affect driving through this direct channel rather than through density. I think it's reasonable to rule that out, but doing so is an assumption.

p. 26, "in column 5, our dependent variable is a measure of travel time". I assume this refers to the entry "household daily travel minutes" in Table 1. Somewhere a brief comment on how that is computed is in order.

p. 26, discussion of column 8: Why can't we simply measure (5) from this regression (perhaps done without controls) rather than indirectly as is done in Section 7?

p. 28 first paragraph, what are the "functional form restrictions" that the Heckman selection model imposes in its usual implementation?

p. 28 second paragraph: It is not only "unclear how these estimates should be interpreted", it's unclear what they are. Unless there is a compelling reason, I think this paragraph could be simply omitted.

p. 36: The third conclusion is that there is a small positive association between road density and household VKT. On the face of it, this seems incompatible with the conclusion of the authors' AER paper on induced demand. A comment would help.

p. 39 middle, the reference to equation (10) does not seem to fit the discussion. In fact, I could not begin to see how to compute the elasticity quoted in the text.

p. 42 last line: the word "price" is missing ("increase in gasoline")

Referee Report on

"Urban form and driving: Evidence from US cities"

by Gilles Duranton and Matthew Turner

QJE MS 25212

The paper estimates the effect of urban density on driving in the U.S. It starts by presenting a simple model of the effect of density on kilometers driven which helps to motivate the main regression specification. It then proposes a methodology to deal with sorting on unobserved characteristics, and to deal with the potential endogeneity of density using as an instrumental variable local geological characteristics. This is a well-done and professional piece of research. It constitutes the best estimation I know of the relationship between density and driving. The paper also presents a fairly complete set of robustness exercises that, in view, make the main results quite convincing.

Comments:

1. I did not like the model in Section 3. The model is very messy and not particularly illuminating of the main forces at play. It lacks prices, any form of agglomeration effects, and it just states the log-linear relationship between density and travel time, as well as travel costs and density. The whole discussion about imperfect mobility is also very hard to follow and not fully modeled. Overall my view is that such a model is useless. It is neither an equilibrium model that can give conceptual content to the estimated partial elasticity based on fundamental agent behavior, not a framework that can be used for quantitative analysis. The authors are also not successful in using it to convey clear intuition.

2. The empirical methodology is also not tightly connected with the model. It seems that the main goal of the model was to derive equation 9 and estimate it. The equation that the authors end up estimating is not exactly this equation. The estimation also does not take into account a variety of issues related to the effect of density in neighboring locations and general

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equilibrium effects. Perhaps more important, the main specification to deal with selection cannot be estimated with the proposed instrumental variables since it relies on changes in density over time. So the authors are forced to first show that the degree of selection seems minimal and then use a specification that does not correct for selection in their IV regressions. Ultimately, I am convinced that in this case selection is perhaps not a major concern. Still, in other cases this might not be the case and so the general methodology cannot be applied to those cases. This makes the overall empirical approach less attractive as a roadmap for estimating this elasticity.

3. There is a literature in economic geography and trade that has quantified models with commuting across locations as well as inter-regional goods trade. These models are based on estimates of gravity equations for commuting and trade flows that have been shown to match the data well. Those models imply a link between employment density and distance travelled as well as residential density and distance travelled. How do the results compare with those implied by this literature? Why not use a model in this class? Furthermore, some of that work has emphasized the difference between employment and residential density. Something that is viewed almost as synonymous in this paper. An explanation would be useful. Bridging the gap to these set of models in the Eaton and Kortum tradition might help make some of these estimates more useful for quantitative counter-factual analysis. In any case, linking results to estimated coefficients of the relationship between commuting and distance is essential.

4. The model tries to make the claim that the main estimated elasticity between density and driving is stable across a variety of specifications. However, it never shows results for different parts of the U.S., cities of different sizes, cities with different public transportation infrastructure, etc. Understanding the potential heterogeneity of this elasticity is important and would make the paper more interesting.

5. I was very disappointed with the last part of the paper where the authors "use" their estimation. First, since they lack a GE model their calculation of welfare effects is not valid. The section is called "Some simple general equilibrium implications". I do not understand how these implications are general equilibrium implications. The authors are just extrapolating from a

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linear coefficient! Second, the policy that they consider is not a real policy. What does it mean to reallocate the less dense decile? How is this policy supposed to be implemented? In my view the authors need to design a real transportation or zoning policy and evaluate its impact in the light of their estimates. One example could be the American 2050 plan and its associated megaregions and infrastructure projects. As it stands this section is just not up to standard for a top paper. The authors seem to have run out of steam.

This paper addresses a clear and important question: how does urban density affect how much people drive? This question has been the subject of much speculation and many studies with unreliable identification. Indeed, the belief that building denser cities is a good way to reduce car use is common among urban planners and politicians and is a tenet of the New Urbanism movement. This paper has the potential to inform and bring much-needed evidence to discussions on this topic.

Identifying the effect of density on driving involves significant challenges. People may sort into locations based on their preferences for density and driving, or urban development may be correlated with some unobserved factors that also influence driving. These issues are far from trivial but the authors do a good job of treating them. Furthermore, the authors outline the issues and explain how they are treated in a clear and convincing way.

The effect of density on driving is found to be negative but modest – 10% more density leads to around 1% less driving. Thus the authors show that changing the density of urban development is a very costly way of altering people's driving behavior. Treating the selection and endogeneity problems changes the coefficients only slightly, though this of course does not mean that they needn't be addressed in the first place.

The paper is well written and the analysis is nicely conducted and presented. Many of my comments are about how to improve the exposition or further details that readers may be curious about. My comments are listed below, beginning with the more substantive points.

Main comments

1.   The paper lacks a treatment of alternative modes of transportation such as walking and public transit. I guess the authors may intend these simply to be amongst the implicit mechanisms that determine how density affects driving. However, as the relationship is all about 'trips' it is natural for the reader to think of other modes of transportation. If they are intended to be implicit then this should at least be explained.

2.   In addition, alternative modes of transportation are fundamental to the relationship between density and driving. Denser cities tend to be easier to get around on foot and to have better public transit systems. If the effect of density depends on say the train network then this has a couple of important implications:

a.   Are the results a description of the density-driving relationship in the US or meant to inform policy? There could be a difference if older cities tend to be denser and to have more public transit. That is, a neighborhood built in 2016 with a certain density may have different characteristics than one built in 1850 that now has the same density. If for example these older

neighborhoods are more likely to have subways and we don't understand the effect of subways on driving, then the results would be less meaningful for policy.

b.   Selection into cities may be related to individuals' preferences for public transit.

c.   If to achieve the measured effect of density on driving a large amount of public money would have to be spent on building subways, then this would affect the calculations of the returns to density-promoting policies (in a direction that would strengthen the authors' argument).

If the authors just did some regressions with controls for measures of public transit, similar to the regressions with roads in Table 9, then that could be quite informative. There should be some data on train and subway networks as well as NHTS data on how many people commute by public transit.

3.   About the instruments:

a.   In arguing for the validity of the instruments, the authors state that the earthquake and landslide variables are not "concentrated in small geographical areas" and that the "aquifers are broadly distributed across the landscape". This is largely true but is only part of what the instruments need to satisfy. If an instrument is correlated with some factor – say climate, terrain, or culture – that has a direct effect on driving, then the exclusion restriction could be violated. The maps suggest that earthquakes are concentrated in the West, landslides on the West Coast and in the Appalachians, and aquifers largely in the Deep South, East Coast, and Rocky Mountains. Make a stronger argument in support of the exclusion restriction.

b.   Landslide risk presumably has a lot to do with slope. An area being hilly may well affect both urban development and transportation. Can you exclude this possibility? This especially concerning because the landslide instrument is far stronger than the others.

Other comments

4.   It's not immediately clear that the relationship between (log) density and (log) driving should be linear. I agree with the logic in the paragraph on page 8 that begins "The effect of density at a location on the demand for travel is ambiguous", but with the collection of mechanisms it left the impression of a relationship that could easily be non-linear and the introduction of equation (5) seemed like a jump. This unease was cured by the plot in Figure 2d, but more could be done on page 8 to argue that the relationship in (5) is reasonable.

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5.   I'm curious whether other measures of density could be informative. The various measures used (referring to the main measure and the alternatives in Table 4) all take density in a regular shape centered on the household's location. Given that residents in a suburb of an MSA should tend to commute in one particular direction, why not use MSA-level density and density in the MSA's core as well? People who work in New York and live in the suburbs, for example, may choose not to commute by car because of the hassles of driving the last couple of miles and then parking, even if the first part of the drive would be leisurely.

6.   In relation to the previous comment, is there data on VKT or commuting distances by place of employment that could be used as alternatives?

7.   Some additional points about the instruments:

a.   The authors state on page 18 that "aquifer prevalence is a good predictor of an aggregate measure of urban form" and of "local density". Give more detail on the measure of urban form and the sign of the effect on local density. (It may not be appropriate to do so in the data section, but it should at least be mentioned in the description of Table 8.) Aquifers must be a positive factor for urban development but also for farming, so it's not immediately obvious what the effect should be.

b.   Is earthquake risk a deterrent for building at all or for building tall buildings?

c.   For all three instruments it would be good to see brief explanations of why they would be expected to be relevant. In case these raise any concerns about the exclusion restriction, these should be discussed as well.

8.   Whether we should be considering MSAs or all of the US is not always perfectly clear. As far as I can tell it is always specified, but the context changes without attention being drawn to the fact, so it's easy for the reader to be confused. In describing the data and in the map in Figure 1, quantiles for the entire (US48?) population are given. Then in the main results it's a sample from the MSAs. Then the hypothetical policy proposals in Section 7B apply the coefficients derived from the MSA population to the entire US population. It may be better to make the context of the core points consistent, or simply do more to point out where they differ.

9.   Does the size of the main effect depend on the types of buildings? Is it related to the age of the structures, whether workplaces are separated from residential areas, how much parking there is, or other factors that could be affected by zoning?

10.   Four things from the paragraph split between pages 15 and 16:

a.   Until I read further I didn't know what "990 meter cells" meant. I take it they're squares with (roughly) 990-meter sides. Find a clearer way of wording this.

b.   The sentence with "matched to the cell with its centroid closest to the centroid" is more confusing than it needs to be. If you have a square grid of points and you have another set of points that you match to the nearest points in the grid, then this is the same as having a grid of squares and matching the points to the squares they fit within. Here the description refers to two sets of shapes by their centroids, which is a bit much to visualize. Just refer to the grid as shapes.

c.   Why 990 meters and not just one kilometer?

d.   Given the curvature of the Earth, it's impossible to have a regular grid of shapes with 990-meter sides or points all 990 meters apart. State what adjustment you make. It is a detail but may help clarify the geography in the mind of the reader.

11.   In describing panel (b) of Figure 2, you mention that the plots from the two data sources may differ because of a condition you use to drop observations from one of them. Can't you just observe the full sample as well? You needn't show it in the plot, but if the distribution with no observations excluded is practically identical to the other line, or even if it is closer, then you could write something more definitive.

12.   Is there data on car ownership? It would be interesting to see how it relates to the effects of income and age on VKT in Table 2. Plus it would be interesting to know how car ownership itself is affected by density.

13.   A few typos I noticed:

a.   "then must have" on page 10.

b.   In the second paragraph on page 21, I think "at low levels of driving" should instead be "at low levels of density".

c.   "Authors calculations".

d.   "The left panel shows a clear downward trend in driving at low levels of driving" – I think it should be "low levels of density" (on page 21).

e.   "they are may be correlated" (page 24).

f.   "sorting of household" (page 24).

g.   "Finally, column 7 gives … in millions of km per year" (page 40). I
think it's column 6 and billions of km.

h.   "gasoline price of elasticity" (page 42).