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Reply to Cox, Gordon, and Redfearn's Comment on "Did Highways Cause Suburbanization?"

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A REPLY TO: WENDELL COX, PETER GORDON, AND CHRISTIAN L. REDFEARN "HIGHWAY PENETRATION OF CENTRAL CITIES: NOT A MAJOR CAUSE OF SUBURBANIZATION," *ECON JOURNAL WATCH* 5(1), JANUARY 2008: 32-45. [LINK](#).

ABSTRACT

I take it as a compliment that my paper has generated sufficient interest to merit the comment by Cox, Gordon, and Redfearn. It is my intention that my paper's data construction and methods are sufficiently transparent such that others can evaluate and replicate my results at a relatively low cost. Indeed, I encourage Cox, Gordon, and Redfearn to look for themselves at the programs used for data construction and model estimation. A CD-ROM with all of the data construction and estimation results is available to any interested party from me upon request.

The comment takes the general view that my empirical finding that each additional highway passing through a central city causes about an 18 percent decline in its population cannot possibly be right because it is "too big". Further, the authors decry my lack of attention to other potential sources of population decentralization. On the second point, I do not dispute that forces other than transportation must be at play in driving population decentralization. These include increases in nonlabor income, changes in production and information technologies such that productivity spillovers can operate over larger distances and relative amenity values of cities and suburbs. Evaluating the importance of these mechanisms goes beyond the scope of my highways paper and requires at least one paper each. A working paper version of my *Quarterly Journal of Economics* article discussed these other mechanisms more extensively.

In the process of the research project, I devoted considerable attention to the size of the highway-penetration effect. To be confident about my results, I undertook an extensive set of empirical robustness checks, the most important

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and time consuming of which was to estimate a version of the regressions allowing identification to come only from time series variation in the highway infrastructure within metropolitan areas. In addition, I developed the companion theoretical paper (Baum-Snow 2007b), which was originally a part of the empirical paper, to evaluate how large a tractable theoretical model predicts the effects of highways to be. The simulation results from this paper demonstrate that the monocentric model mechanism alone generates conservative estimates that are remarkably similar to the empirical estimates arrived at completely independently. While I agree that the monocentric model could not possibly capture all aspects of the true data generating process, one may expect other mechanisms only to make the true treatment effect larger than that predicted by a monocentric model. The value of using such a model is that it is simple enough to generate qualitative and quantitative implications that are robust to specification.

I consider the main criticisms of my paper in turn:

1. *Using his estimates, Baum-Snow calculates that, had the interstate highway system not been built, the aggregate population of 1950 geography central cities would have grown by 8 percent between 1950 and 1990 rather than declined, as observed, by 17 percent. The magnitude of this increase cannot possibly be right because not enough housing could ever be built to increase population by 8 percent in a given built up geographic area.*

This claim cannot be true given that the 1950 geography of San Diego grew by 72 percent in population between 1950 and 1990. This city represents the maximum for change in log constant geography central city population reported in the Appendix table (Baum-Snow 2007a, 804). There are 11 other cities with more than 8 percent growth 1950 to 1990.

2. *It is not highways, it is rising incomes plus the automobile that caused suburbanization.*

I certainly agree that cars have been essential for population decentralization. But the fact remains that the car has come to dominate commuting in almost every U.S. metropolitan area, yet we saw much greater changes in the spatial distribution of the population in cities receiving more highways. If highways are not important, you have to tell a story as to what unobservables correlated with actual highways (and those in the 1947 plan) could generate the empirical pattern.

3. *The paper relies too heavily on the monocentric model.*

The paper in no way relies on the monocentric model for identification. It estimates a treatment effect which may be driven by any number of data-generating mechanisms. The paper is not an exercise in structural estimation, though if one wanted to limit oneself to the monocentric model, one could interpret the estimates structurally.

4. *MSA fixed effects are poor proxies for unobserved influences because of the long time span.*

I intended the fixed-effects analysis primarily as a robustness check on the long-difference analysis. Identification of the treatment effect of interest using the long difference estimator relies solely on exogeneity of the 1947 highway plan, conditional on appropriate control variables. The fact that the fixed-effects analysis delivers the same results as the long-difference estimates simply says that the number of rays in the 1947 plan is not correlated with any fixed (unobserved) MSA attributes that might have generated suburbanization. That is, the fixed-effects results provide support for the claim that the 1947 plan is exogenous. As seen in Table IV in my paper (791), the 1947 plan also appears exogenous to the MSA income distribution and population, as I argue it should be.

5. *Data from European cities does not indicate a correlation between central city highway penetration and city population loss.*

Cox, Gordon, and Redfearn provide a table showing population growth rates and highway infrastructure in 42 European cities. The table contains some interesting nuggets of information that merit further investigation. Indeed it is striking how similar the population declines of European cities have been to American cities. However, I am not convinced that their cursory investigation suffices to conclude that there is no relationship between transport infrastructure and the spatial distribution of the population in Europe. I break my critique of the authors' analysis into five parts.

- a. The data reported in Table 1, which indicates whether central cities had ray penetration as of 1990, does not match current maps that I've found. Amsterdam, Bremen, Genoa and a few other cities on the list currently have limited access highways serving their downtowns when measured using the same definition as in my paper. This recent construction may have been anticipated in 1990 with some population movement to the suburbs.
- b. The authors' analysis of the European data makes no attempt to control for the spatial size of the central areas for which population numbers are reported. Spatial size is the one variable for which it is essential to control in regressions. As an example, if these European cities have very large areas, then we would expect to find small or zero effects of highways on the spatial distribution of the population.
- c. The analysis of the European data does not take into account when the highways were built. Cox, Gordon, and Redfearn calculate the population change between its peak year and 1990, but in 19 of 42 cities in their sample the peak occurred in 1970 or 1980, plausibly after some of the

freeway rays were built. A more convincing analysis would examine the relationship between the change in population over a fixed time period and the change in freeway availability over this same period.

- d. I agree that it is remarkable that every single European city in Table 1 lost population in the period prior to 1990, whether they received highways or not. However, the authors misinterpret the counterfactual calculations I make for U.S. cities. That calculation shows that given my results, holding all other influences of 1950-1990 population changes the same, point estimates indicate that some U.S. cities would have grown in population absent highway construction, leading to aggregate population growth. Taking the calculation out of sample by applying it to cities in Europe would be appropriate only if all the other influences were the same as those in the United States. Clearly, that is not the case. For example, aggregate European population grew only about half as fast as aggregate US population did between 1950 and 1990. If this were the only difference between the two continents (which of course it is not), and noting that European cities received fewer roads on average than American cities over the period, it is absolutely consistent with my results that European cities absent new highways lost population.
- e. The list of cities in Table 1 excludes those that expanded geographically. As such, one may worry that the sample is not random. I would be more confident in the authors' results if they used the full sample of large European cities.

6. *Large tracts of undeveloped land existed in US central cities in 1950.*

The existence of greenfields is one of many reasons that it would be a bad idea to take structural estimation of a land use model viewed in isolation too seriously. Regardless of the prevalence of greenfields, the fact remains that I estimate a causal relationship between highways and suburbanization for US cities. To the extent that the true treatment effect of highways is a function of open space in central areas, I agree that one should be cautious about the external validity of the results.

In conclusion, it is my hope that my *Quarterly Journal of Economics* article spurs further investigation of the mechanisms driving the observed relationship between transport infrastructure and the spatial distribution of the population. I applaud Cox, Gordon, and Redfearn for investigating the topic with European data.

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