Does Traffic Congestion Reduce Employment Growth?*

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December 10, 2008

JOB MARKET PAPER

Journal of Urban Economics (In Press)

Abstract: This paper examines the impact of traffic congestion on employment growth in large U.S. metropolitan areas. An historic highway plan and political variables serve as instruments for endogenous congestion. The results show that high initial levels of congestion dampen subsequent employment growth. This finding suggests that increasing the efficiency of public infrastructure can spur local economies. Counterfactual estimates show that the employment-growth returns from modest capacity expansion or congestion pricing are substantial.

JEL classification: R11; R41; L91.

Keywords: urban growth; congestion; transportation.

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*I especially thank Jan Brueckner for his guidance and encouragement. I also thank David Neumark, Giuseppe Ragusa, Stuart Rosenthal, and Ken Small for their helpful comments. Nathaniel Baum-Snow graciously provided the Interstate Highway data. The University of California, Irvine Department of Economics and the University of California Transportation Center helped fund this research.

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1 Introduction

In the United States, urban vehicle-miles traveled increased 91% between 1982 and 2003. However, over this time period, freeway lane-miles only increased 41%.[1] One consequence of this disparity has been a rapid increase in congestion-related travel delay. According to a study by the Texas Transportation Institute (Schrank and Lomax, 2005), annual travel delay rose from 16 hours per driver in 1982 to 44 hours per driver in 2003.[2] If current trends in urbanization and population growth continue, congestion levels will increase. What impact will worsening congestion levels have on urban economies? Although many studies have measured congestion externalities borne directly by drivers, researchers have devoted less effort to identifying congestion's broader economic impact on urban areas as a whole.

Prior research regarding the effect of traffic congestion on economic growth is limited to a handful of empirical papers. One study by Boarnet (1997) looks at the effect of congestion on output in California counties between 1977 and 1988. He finds that increases in congestion have a negative and nonlinear effect on output, but finds that the effect of congestion on output is greater in highly congested counties. Another study by Fernald (1999) looks at the effects of road infrastructure and congestion on output in a cross section of U.S. industries between 1953 and 1989. He also finds evidence that congestion negatively affects output, but that this effect was only important after 1973. Although these two studies document congestion's negative impact on output, no research has looked at the effect of congestion on employment growth.

Using a cross section of U.S. metropolitan areas, this paper measures the causal impact of traffic congestion on aggregate employment growth. However, the task is difficult be-

[1]See Federal Highway Administration (1982, 2003)

[2]Census data from the Integrated Public Use Microdata Series (IPUMS) show a similar increase in commute times. For metropolitan workers, average one way travel times to work increased from 21 minutes in 1980 to 26 minutes in 2000, which translates into roughly 40 extra hours of driving each year. However, in the IPUMS data, it is not clear to what extent commute times have increased due to people living farther from their place of employment.

cause the two variables simultaneously determine one another. Workers generate congestion by driving to and from work during peak travel periods; at the same time, congestion discourages employment growth by raising workers' reservation wages and increasing shipping costs for goods. While the effects of population and employment growth on congestion have been measured,[3] the magnitude of congestion's negative feedback effect is not known. How much has congestion — itself caused by high *levels* of employment — dampened subsequent employment *growth*?

Measuring the magnitude of congestion's feedback effect on employment growth is the main empirical focus of this paper. In light of the simultaneous relationship just described, a set of historical variables serve as instruments for endogenous congestion. The first instrument is a measure of planned metropolitan highway capacity based on a 1947 plan of the Interstate Highway System. Empirical evidence in the paper suggests that the extent of planned highways is negatively correlated with urban congestion levels almost a half century later. This relationship is not surprising given that the Interstate Highway System represents a large fraction of urban highway capacity. However, to be a valid instrument, the measure from the highway plan needs to be orthogonal to temporally distant employment growth. This property seems reasonable, as the Congressional mandate for the 1947 plan makes no specific mention of promoting metropolitan employment growth decades later.

A second instrument for congestion measures each metropolitan area's past transportation-related influence in Congress. The measure is a count of prior congressional representatives assigned to the House Transportation Committee. The relationship between this measure and congestion is clear; members of Congress tend to promote projects and spending that benefit their constituents. One would therefore expect that metropolitan areas with greater historical representation on the Transportation Committee received more funding for road infrastructure and transit, which inhibits subsequent congestion. Again, it is reasonable to

[3] Downs (2004) discusses the increase in congestion levels and highlights its primary causes.

think that this measure is orthogonal to unexplained employment growth.

This paper's econometric model and choice of control variables follow the city growth literature, which examines population, employment, and income growth over long time periods. The literature focuses primarily on identifying and measuring the positive growth-benefits from economies of agglomeration. One body of research investigates the nature of agglomeration (e.g. the relative importance of localization and urbanization economies) and its impact on growth in industries within cities (Glaeser et al., 1992; Henderson et al., 1995). Other research examines how aggregate growth is affected by deeply-lagged city characteristics, which include the initial level of human capital (Glaeser et al., 1995; Simon, 1998; Simon and Nardinelli, 2002; Shapiro, 2006), natural characteristics (Beeson et al., 2001; Rappaport, 2007), and crime (Cullen and Levitt, 1999). The current paper measures congestion's negative impact on growth while controlling for the positive benefits afforded by economies of agglomeration. Therefore, besides congestion, the empirical work below also considers the effect of these other variables, which include human capital, crime, climate, and demographics.

The regression results suggest that increases in congestion significantly reduce subsequent employment growth. The best estimate of the annualized elasticity of employment growth with respect to per capita hours of travel delay is -0.02. To put this elasticity in perspective, the results imply that for Los Angeles, a 50% reduction in the level of congestion in 1990 would have generated roughly 100,000 additional jobs by 2003.

The plan of the paper is as follows. Section 2 discusses the econometric model and identification strategies. It also describes the measure of congestion and the instruments. Section 3 presents the results. Section 4 uses the results to calculate counterfactual estimates of changes in employment growth in response to different transportation policies designed to reduce congestion. Section 5 concludes.

2 Methods

This section presents a framework for studying employment growth in metropolitan areas. The basic empirical strategy involves regressing employment growth on initial employment, a measure of congestion, and other explanatory and indicator variables. This section also discusses the data sources, focusing on the congestion measure and its instruments.

Econometric Model

As mentioned previously, the econometric model follows the city growth literature, which explores how initial conditions determine subsequent economic growth. The cross-sectional model is:

$$ln(E_{i,2003}/E_{i,2003-k}) = \beta ln(CONG_{i,2003-k}) + ln(X_{i,2003-k})^{'}\theta + \phi ln(E_{i,2003-k}) + \nu_i \qquad (1)$$

The units of observation are U.S. metropolitan areas, which are indexed by i. The dependent variable, $E_{i,2003}/E_{i,2003-k}$, is employment growth between year 2003 and year $2003-k$; in the empirical implementation of Equation 1, k ranges from 13 to 21 years. On the right hand side of the equation, $CONG$ is a measure of congestion, X is a vector of exogenous explanatory variables, and ν is white noise. Because the variables are measured in logs, β measures the elasticity of employment growth with respect to the initial level of congestion.

Omitted explanatory variables may cause problems when estimating Equation 1. This omission may bias β, the coefficient of interest, if important unobservable city characteristics are excluded from X. To reduce such bias, Equation 2 below exploits the availability of panel data for metropolitan areas, and augments Equation 1 with area fixed-effects denoted by α. The panel data model describes employment growth in non-overlapping q year-long periods:

$$ln(E_{i,2003-(n-1)q}/E_{i,2003-nq}) = \beta ln(CONG_{i,2003-nq}) + ln(X_{i,2003-nq})'\gamma + \delta ln(E_{i,2003-nq}) + \alpha_i + \epsilon_{i,2003-nq}. \quad (2)$$

Again, i indexes the MSAs and $n = (1, 2, 3, \ldots)$ indexes the time periods. In the empirical implementation of Equation 2, q ranges from 1 to 5 years. For example, when $q = 5$ the first observation will be employment growth in MSA i between 1998 and 2003, the second observation will be employment growth between 1993 and 1998, and so forth. The main benefit of the panel data model is that it accounts for city-specific unobservable characteristics and that it utilizes a larger sample size than the cross-sectional model. However, the need for time-varying instruments for congestion is a drawback relative to the cross-sectional model.

Congestion Data

This subsection describes the congestion data in the study. The analysis is based on a sample of the 85 largest Metropolitan Statistical Areas (MSAs), observed between 1982 and 2003.[4] The sample of MSAs and the time frame are limited by the availability of reliable measures of congestion, but these limitations should not bias the results: congestion is minimal outside the 85 largest MSAs, and it was also minimal before 1982.

The measure of congestion is drawn from the 2005 Urban Mobility Report (Schrank and Lomax, 2005), produced by the Texas Transportation Institute (TTI). The TTI estimates time lost due to congested driving conditions for 85 large urban areas, starting in 1982.[5] This measure is based on data from the Highway Performance Monitoring System database

[4]MSA boundaries correspond to the 2003 Office of Management and Budget standards.

[5]The geographic urban area boundaries that the TTI uses are subsumed by the MSA boundaries. For most cities in the sample, the two geographic area boundaries closely correspond. However, some rural portions of MSAs extend beyond the urban area boundaries. In such cases, it is assumed that congestion levels in rural areas are negligible.

of the U.S. Federal Highway Administration (FHWA). Individual states collect the highway data according to guidelines set forth by the FHWA.

The measure of congestion used here is the annual aggregate amount of time lost due to congested driving conditions. The TTI generates this measure using the difference between free-flow and average actual speeds on individual highway segments at different times of day. Free flow speed is just the normal speed limit, assuming no other traffic is present. Average actual speed on highway h, calculated over time period τ, is a function of traffic volume V, capacity K, and physical road characteristics R:

$$\textit{actual speed}_{h,\tau} = f(V_{h,\tau}, K_{h,\tau}, R_{h,\tau}). \tag{3}$$

The speed function $f(\cdot)$ relating these four quantities comes from a traffic flow model, also produced by the TTI.

Using these free-flow and average actual speed measures, the TTI calculates travel delay as follows:

$$\textit{travel delay}_{h,\tau} = \frac{\textit{length}_h \cdot V_{h,\tau}}{(\textit{free flow speed}_h - \textit{average actual speed}_{h,\tau})}, \tag{4}$$

where $\textit{length}_h$ is the centerline mileage of highway h. Summing across highway segments and time periods gives total annual delay for a city.[6] In the empirical work, the measure of congestion in an MSA is travel delay per capita.

Note that this measure of congestion only accounts for time lost due to travel delay. It does not include other congestion-related costs that individuals may incur. For example, individuals with a low tolerance for congestion may sort themselves into less congested cities

[6]The TTI uses a stratified sample of data to estimate travel delay, because data is not available for every roadway segment for every time of day. Also, note that an explicit measure of accident-related travel delay is absent in Equation 4. The TTI uses physical attributes of highway segments (e.g., curvature and presence of shoulder) to estimate accident-related delay on individual highway segments and add this amount to their measure of recurring delay.

or into less congested places within a city. They may also rearrange their schedules to avoid rush hour. These behavioral distortions may be costly, but are not included in the congestion measure.

Endogeneity and Instrumental Variables

As discussed in the introduction, isolating the causal effect of traffic congestion on employment growth is challenging because the two variables are simultaneously determined. Furthermore, it is likely that persistent and unexplained factors affect both employment growth and congestion. Thus, instrumental variables will control for potential endogeneity bias in the estimates. The first instrument for congestion is based on the number of radial road-miles in a given MSA as proposed in a 1947 plan of the Interstate Highway System. Under a mandate from Congress, the Federal Bureau of Public Roads and state officials designed the Interstate Highway System. The stated purpose of the system was to link metropolitan areas, to promote national defense, and to further trade with Mexico and Canada.[7] Although the plan was modified somewhat after 1947, it largely determined the eventual network of Interstate highways. In the cross-sectional regressions, the instrument for base year congestion is planned radial road-miles per capita in each MSA. In the panel data model, the instrument employed is planned radial road-miles per capita interacted with a linear time trend. Figure 1 shows the original map of the planned highways.

It is reasonable to think that radial road-miles from this original plan are orthogonal to changes in employment decades later. The mandate from Congress did not specifically mention promoting employment growth as an impetus for the Interstate Highway System. However, one could argue that those who designed the plan (i.e., state and federal transportation planners) systematically included more Interstate highways in places that were

[7]Baum-Snow (2007) generated the idea of using the Interstate highway plan as an instrument for actual highway construction. For more historical details see U.S. Department of Transportation (1977). For the map of the highway plan, see U.S. Department of Commerce, Bureau of Public Roads (1955).

expected to have high employment growth, casting doubt on the validity of the instrument. Even if the plan's designers could accurately predict which cities would grow the most, there are several reasons why the radial road-miles measure is likely to be exogenous.

First, radial highways are segments of the highway network that emanate from city centers, and are designed to provide intercity access. By contrast, beltway highways (which were not drawn in the 1947 plan) provide intracity access, which tends to be more important for commuting. So although radial highways do benefit local residents, their intended effect on local commerce is incidental. Second, the highways are measured in road-miles, which is the centerline length of a particular segment. Road-miles are a measure of the extent of the road network, whereas lane-miles actually measure capacity. Planners anticipating future employment growth would be most concerned with providing an adequate level of freeway capacity. Third, to be a valid instrument, the radial road-miles measure must be orthogonal to employment growth conditional on congestion and the other control variables. It is reasonable to assume that road infrastructure mainly benefits firms and households by reducing the time cost of vehicular travel. Therefore, conditioning on congestion leaves little unexplained employment growth that could be correlated with the radial road-miles measure: the impact of radial-road miles works through congestion. These arguments, along with the distant origin of highway plan, lend credibility to this instrument.

The second instrument for congestion is a measure of each MSA's historical influence on transportation policy. The measure is a running total of Transportation Committee members in the House of Representatives. For the cross-sectional regressions, the measure is constructed as follows. For every session of Congress, beginning with the 80th session in 1947, congressional district boundaries are matched to 2003 MSA boundaries. Using a database of committee assignments (Nelson and Bensen, 1993), transportation committee members are matched to the 85 MSAs in this sample by year. Finally, the yearly member counts are summed by MSA between 1947 and various base years, which are 1982, 1986 and

1990. For the panel regressions, the instrument for the congestion level in a given base year is the MSA's cumulative number of transportation committee members from the prior 10 years.

The empirical work finds that these measures are negatively correlated with base year congestion levels. This finding suggests that transportation committee members garner transportation funding for their constituents, thereby inhibiting congestion formation. However, the political process that appoints committee members poses a threat to the validity of the instrument. For example, if representatives from areas with high expected employment growth rates are systematically appointed to the committee, the instrument may not be valid. This argument is unconvincing, however, for several reasons. For example, incumbency plays a prominent role in committee assignments. Long serving members of congress rarely relinquish their committee posts, which hinders the political process from making assignments based on expected employment growth. Additionally, incumbents on the transportation committee have the advantage of seniority, which gives them more power to procure transportation funds than do junior committee members. So at face value, this political influence measure seems to be a valid instrument. However, validity is less certain if Congressional leaders correctly forecast employment growth, and then act on those forecasts when making transportation committee assignments.

Control Variables and Data Sources

In the empirical implementation of Equations 1 and 2, the vector X includes several control variables. Beyond serving as controls, the effect of these variables on employment growth is also of interest. For example, the level of human capital may increase employment growth, because individual workers with high levels of human capital generate external benefits for other workers via economies of agglomeration. Similarly, X also includes a measure of crime, which may lead to out-migration and thus decrease employment growth in cities. Other control variables include a climate variable, demographic variables, and each MSA's share of employment in manufacturing. Although there may be other important factors that affect employment growth, reliable annual measures are not available for many American cities.

Data for the control variables are drawn from a variety of sources. The number of employees in each MSA is drawn from the annual County Business Patterns publications provided by the U.S. Census Bureau. The Bureau of Economic Analysis' Regional Economic Information System provides the metropolitan area population data, which is used to construct per capita measures. The total number of crimes in a metropolitan area is provided by the Bureau of Justice Statistics' Uniform Crime Reports. Demographic data, which includes racial and age distribution measures, are drawn from the U.S. Census Bureau. For the cross-sectional regressions, the human capital measure equals the percentage of the population with a high school diploma, which is drawn from the U.S. Census. For the panel regressions, the human capital measure is the number of two and four year colleges per capita, and it is provided by the Department of Education's Integrated Postsecondary Education Data System. The climate variable is the mean January temperature, which is calculated by the National Oceanic and Atmospheric Administration for each metropolitan area's principal airport. This control is essentially time invariant, and it is not included in the panel regressions. The manufacturing share variable is from the County Business Patterns publications, and it is not included in the panel regressions because it is not consistently defined following the 1997 switch to the North American Industrial Classification System. Table 1 reports summary statistics.

3 Results

This section presents the regression results based on the cross-sectional and panel models in Equations 1 and 2. For various specifications, OLS and limited information maximum likelihood (LIML) estimation techniques are used. The instrumental variables regressions use LIML, because studies have shown it to perform better than two-stage least squares in the presence of weak instruments (Hahn et al., 2004; Stock and Yogo, 2005).

Cross Sectional Estimates

Table 2 presents results from cross-sectional regressions with base years 1982, 1986, and 1990. These estimates portray the long-run response of employment growth to the initial level of congestion. This long-run feature of the model is an advantage; however, less precise estimates due to a small sample size are a drawback.

Equation 1 is estimated for a set of time periods that vary in length. In each regression, the unit of observation is log MSA employment growth between a particular base year and 2003. The explanatory variables are measured in the base year, and log per capita radial road-miles from the 1947 Interstate plan and historic transportation committee members serve as instruments for congestion. Table 2 also shows results from a regression that includes the congestion measure squared. For that regression, the instrument list also includes the square of each instrument and an interaction between the two instruments.

Conditional on the control variables, the results suggest that high levels of congestion tend to decrease employment growth in the long-run (where the long run is defined as either 21, 17, or 13 years). Starting from the left of Table 2, we see that specifications including the two primary instruments (columns 1–3) yield negative and statistically significant point estimates of β. Furthermore, these estimates increase with k, the number of years between the base and end of each period of employment growth under consideration, and range from -0.234 to -0.336.

Columns 4 and 5 present the base year 1990 specification with the two primary instruments in isolation. The congestion coefficient is -0.241 when only the transportation committee instrument is used, and is -0.231 when only the 1947 plan instrument is used.

This set of results lends credibility to the instrumental variables strategy. In a sense, they provide informal evidence that weak-instrument bias is not a problem: two very different instruments yield similar point estimates for the congestion coefficient.

The rightmost specification in column 6 includes congestion and its square, and the corresponding estimated coefficients are -0.186 and -0.083. Evaluating the implied elasticity at the mean value of log congestion per capita in 1990 yields a value of -0.054. Although this value is appreciably smaller than the other estimates, the coefficient on the square term suggests that increases in congestion have a stronger dampening effect on employment growth in more congested cities. Moreover, the point estimates in column 6 are more precise than the estimates presented in other columns.

Also, consider the estimated coefficients for the control variables. Across all specifications, the results suggest that high mean January temperatures are correlated with increased employment growth, and the elasticities range from 0.1 with base year 1990 to 0.4 with base year 1982. This result corresponds with evidence in Rappaport (2007), which documents a U.S. trend in migration to warmer climates. The results in Table 2 also suggest that high initial shares of employment in manufacturing are correlated with lower employment growth; however, the estimates are only marginally significant. The estimated effects of crime and human capital on employment growth are not statistically significant.

Select first-stage results in Table 3 also show that 1947 planned road-miles per capita and transportation committee members are both negatively correlated with congestion levels. The first-stage results are noteworthy in their own right; they suggest that roads planned or built long ago help inhibit congestion many years later. The evidence for this inhibiting effect is striking, considering other research suggesting that road building induces demand for driving (Noland, 2001; Cervero and Hansen, 2002). One explanation for this finding is that highway segments built long ago may facilitate future capacity expansion. For example, it is less costly to increase capacity by widening an existing roadway than to increase capacity by building an entirely new highway segment.

Table 2 presents results from tests of the overidentifying restrictions. The tests use Hansen's heteroskedasticity-robust J-statistic, which is distributed chi-square with degrees of freedom equal to the degree of overidentification. The null hypothesis of the test is that the overidentifying restrictions are valid. That is, assuming one of the two instruments for congestion is indeed uncorrelated with the error term, failure to reject the null hypothesis supports the validity of the other instrument. The p-values of the Hansen test statistics reported in Table 2 are above 0.5 in all overidentified specifications. Conditional on having one valid instrument, the tests support including both instruments to maximize the asymptotic efficiency of the estimates.

However, it should be noted that overidentification tests are biased and inconsistent if there are not enough valid instruments to exactly identify the relationship, and can also be sensitive to model specification. In light of these disadvantages, the conceptual arguments for instrument validity become paramount.

Also note that the values of the Kleibergen-Paap statistics[8] suggest weak-instrument bias may be a problem, especially in the specification with base year 1986. In instrumental variables regression, weak instruments can bias both the point estimates and the standard errors. When using LIML, one can formally test for weak-instrument bias in the standard errors using an approach proposed by Stock and Yogo (2005). However, that approach is only valid when model error terms are assumed to be iid, which is not the case in this situation. Nevertheless, the Stock-Yogo critical values give a rough sense for whether or not the estimated standard errors are too small.[9] The Stock-Yogo critical values for the LIML size distortion test at the 10% level is 6.46 when there is one endogenous regressor and two

[8]The Kleibergen-Paap statistic is a generalization of the first stage F-statistic, and is valid when the primary equation contains multiple endogenous regressors; the statistic is also robust to heteroskedasticity and within group serial correlation in the errors.

[9]Stock and Yogo do not provide a corresponding test for bias in LIML point estimates.

instruments. The critical value is 4.84 with two endogenous regressors and five instruments.[10] Although one cannot directly compare the Kleibergen-Paap statistics to these values, they do suggest that the estimated standard errors may be biased in some of the specifications.

Panel Estimates

The results from estimating Equation 2 are presented in Table 4. The dependent variable in each specification is employment growth across short time periods. In these regressions, employment growth over one to five year periods is considered. In addition to congestion, each of the panel specifications includes a measure of human capital (the number of two and four year colleges per capita), the crime rate, MSA dummy variables, and demographic controls. All of the controls are measured in the base year of the period of growth. In each specification, the robust standard errors are clustered at the MSA level to account for possible within group serial correlation in the residuals.

As explained earlier, the instruments for the initial congestion level in the panel data regressions are similar to those used in the cross sectional regressions. The first instrument is log planned radial road-miles per capita interacted with a linear time trend. The second instrument for the congestion level at time t is the MSA's cumulative number of transportation committee members between year t and year $t-10$.

The LIML results suggest that initial congestion levels have a negative and statistically significant effect on employment growth. The magnitude of β, the congestion elasticity, monotonically increases with the length of the time periods being analyzed. These estimates range from -0.024 when the period is one year to -0.051 when the period is five years. However, as the length of the periods of time increase, the precision of the results decreases. This phenomenon is attributable to the reduction in sample size that occurs as the periods of time become larger.

[10]This approach tests the null hypothesis that the actual significance level of a hypothesis test concerning β is less than 10 percent when the nominal significance level is 5 percent.

The effects of many of the other control variables are also estimated precisely. The results suggest that the initial level of colleges per capita increases employment growth; the estimated elasticity ranges from 0.01 to 0.05 across specifications. These results are similar in magnitude to the findings of Shapiro (2006), who estimates the elasticity of MSA employment growth with respect to the share of the population with a college degree to be 0.08. The results in Table 4 also suggest that crimes tend to decrease employment growth in MSAs, with elasticities that range from -0.01 to -0.02 across specifications.

The first stage regression estimates presented in Table 5 show that the planned 1947 highways and past representation on the Transportation Committee are negatively associated with congestion levels. This finding again suggests that highways planned and built in the past tend to inhibit congestion many years later.

The values of Hansen's J-statistic, presented in Table 4, fail to reject the null hypothesis that the overidentifying restrictions are valid. However, caution should be taken in interpreting the estimates due to potential weak instrument bias. Fortunately, some of the specifications yield adequately high Kleibergen-Paap statistics. Furthermore, the estimated standard errors and Kleibergen-Paap statistics from the panel regressions are conservative, as they account for heteroskedasticity and potential within group serial correlation in the residuals. Corresponding statistics that only account for heteroskedasticity are generally much more favorable, especially for the first-stage estimates. The non-clustered Kleibergen-Paap statistics are above 15 in all of the panel specifications.

Explaining Differences in the Estimates

Controlling for endogenous congestion yields estimates of β, the congestion coefficient, that are more negative than the corresponding OLS estimates in Tables 2 and 4. The LIML estimates from the panel regressions range from -0.024 to -0.051, while the OLS estimates are indistinguishable from zero. The more negative LIML estimates can be explained by the likely direction of the OLS bias. For example, suppose the congestion measure is positively correlated with the residuals in the employment growth equation — perhaps through an unmeasured city amenity that generates employment and traffic. This would induce positive bias, and draw the OLS estimate of β towards zero.

The difference between the elasticity estimates from the two models is also worthy of attention. At first glance, the estimates seem to depend on the specific time period under consideration. Estimates of congestion's negative effect on employment growth increase monotonically with the length of the time between base and end years. However, the annualized estimates of the congestion elasticity presented in Tables 2 and 4 are largely consistent with one another. Looking across both models, the annualized estimates are quite similar, ranging from -0.01 to -0.02. Beyond helping to reconcile the estimates, the pattern of the annualized congestion measures gives some insight into the temporal nature of congestion's effect on employment growth. Note that the annualized estimates in Table 4 tend to slightly decrease as the length of time between base and end years increases. This pattern suggests that the initial effect of a congestion shock is relatively large, but tends to dampen over time. The dampening may occur as cities respond to increased congestion levels, perhaps by building roads or expanding transit.

4 Policy Discussion

The estimates in the previous section suggest that in the long run, congestion dampens employment growth. This section uses those estimates to calculate counterfactual employment growth between 1990 and 2003 under two different transportation policies. The first counterfactual scenario involves expanding freeway capacity while the second scenario involves comprehensive congestion tolls, with the toll revenue returned in a lump-sum fashion. The following counterfactual estimates provide a rough measure of additional benefits that more efficient freeways would generate for the ten most congested cities in 1990.

To analyze the effect of capacity expansion and congestion pricing on employment growth, it is first necessary to estimate how much each policy would reduce congestion. Results from an auxiliary regression, which the following paragraph describes in detail, provide an estimate of how much congestion would decrease following capacity expansion. Alternatively, simulations and real world results provide an estimate of how much congestion would decrease with comprehensive tolls. These estimates, along with the elasticity of employment growth with respect to congestion, can be used to calculate counterfactual employment growth.

Counterfactual employment growth $\hat{E}_i$ in city i following a κ percent increase in capacity is:

$$\hat{E}_i = \kappa \times \varepsilon_i^{C,K} \times \varepsilon_i^{E,C} \times E_i + E_i, \tag{5}$$

where $\varepsilon_i^{C,K}$ is the elasticity of congestion with respect to capacity, $\varepsilon_i^{E,C}$ is the elasticity of employment growth with respect to congestion, and E_i is the actual change in employment for city i between 1990 and 2003. Similarly, counterfactual employment growth $\tilde{E}_i$ in city i following the imposition of congestion tolls that reduce congestion by μ percent is:

$$\tilde{E}_i = \mu \times \varepsilon_i^{E,C} \times E_i + E_i. \tag{6}$$

Although the counterfactual estimates in Equations 5 and 6 ignore complex changes in behavior and land use that accompany transportation policies, they can be used to estimate the first-order response of employment growth to changes in congestion.

The first step in generating counterfactual estimates is obtaining intermediate estimates of how congestion would respond to the policies under consideration. Estimating $\varepsilon_i^{C,K}$, the elasticity of hours of travel delay with respect to freeway capacity, is straightforward. The Texas Transportation Institute provides measures of the two most important determinants of congestion — volume (measured in vehicle miles traveled per person) and capacity (measured in freeway lane-miles per person). Estimates of $\varepsilon_i^{C,K}$ are based on an OLS regression of travel delay per person on volume, capacity, the interaction between volume and capacity, and Census division dummies. The elasticities for the ten most congested MSAs are presented in column 2 of Table 6.[11]

Estimating how much congestion pricing would reduce travel delay is difficult. No American cities have implemented comprehensive congestion tolls. However, estimates from sophisticated simulation models of travel behavior and the actual experience of European cities with cordon tolls provide a basis for this analysis. A simulation of traffic in the city of Cambridge, England by May and Milne (2000) shows that a cordon toll of 90 pence would reduce travel delay within the charge area by 70–90%. Another simulation by Safirova et al. (2003) shows that a comprehensive toll of seven cents per mile in Washington D.C. would increase speeds by 12% on the Beltway, reducing delays 37% during rush hour.

In addition, the experiences of London and Stockholm show that the imposition of congestion pricing significantly reduced travel delay. Santos and Fraser (2006) evaluate the London Congestion Charging Scheme, and they report that within the charge area, actual speeds increased 14–21% after the city introduced a £5 cordon toll. Similarly, Transport For London (2007) reported that the cordon toll initially reduced travel delays in the charge area by 30%. Stockholm also implemented a cordon toll, and during the initial six month trial period which began in 2006, the City of Stockholm reported reductions in travel delay of 30–50% (Stockholmsförsöket, 2006). Even though transportation systems differ in American and European cities, the evidence from congestion pricing schemes in Stockholm and London suggest that cordon tolls can achieve significant reductions in delay.

Table 6 presents two sets of counterfactual estimates of employment growth between 1990

[11] All variables are in logs. Interacting volume and capacity makes the elasticity of congestion with respect to capacity for MSA i a function of traffic volume in MSA i. The full set of regression results are not presented in tabular form, but are available upon request.

and 2003 for the ten most congested U.S. metropolitan areas. Column 3 contains estimates of the elasticity of employment growth with respect to congestion, $\varepsilon_i^{E,C}$, which are based upon the estimates in column 6 of Table 2. Counterfactual employment growth in column 5, $\hat{E}_i$, is based upon a 10% increase in freeway capacity in 1990. To put the 10% increase in perspective, actual freeway capacity growth for the ten most congested cities ranged from 8% to 26% between 1990 and 2003. Column 6 contains counterfactual estimates of employment growth $\tilde{E}_i$ based upon a road pricing scheme that reduces congestion by 50%, which is in the middle of the range of reductions that simulations and the experience of London and Stockholm suggest.

The results indicate that modest capacity expansion and road pricing policies have similar effects on employment growth in congested cities. Moreover, the amount of additional employment growth in each counterfactual scenario is not trivial. For the ten most congested cities, the estimates of employment growth following a 10% increase in capacity, seen in Table 5, are 4–11% higher than the actual amounts. Likewise, the increases in employment growth following road pricing that reduces congestion by 50% are 10–30% higher than the actual amounts. These results do not imply the superiority of one policy over the other, as this analysis does not account for costs. However, the purpose is to show that the potential effects on employment growth are substantial and that policy makers should include such benefits in future analyses of transportation policies.

5 Conclusion

This paper undertook the difficult task of measuring traffic congestion's feedback effect on employment growth. Although one would expect to find negative feedback, measuring the magnitude of the effect is challenging because the two variables are simultaneously determined. To avoid endogeneity bias, the analysis used a unique set of instrumental variables and found robust evidence that congestion dampens subsequent employment growth. The analysis also found that the dampening effect on growth is nonlinear and more intense in highly congested places. In the long run, these effects are substantial. For Los Angeles, the most congested MSA in 1990, annual travel delay was approximately 50 hours per person. The estimates imply that a 10% increase in congestion, for a city with delay comparable to that of Los Angeles, would reduce subsequent long-run employment growth by 4%. Thus, if the current trends in urbanization continue, additional cities will experience very high levels of congestion and the ensuing reduction in employment growth will be large.

The results of the present paper complement the findings of Boarnet (1997) and Fernald (1999) and, taken together, suggest that congestion has a broad negative impact on economic growth. The public policy implication is clear: reducing inefficient traffic congestion, though desirable in itself, has the added benefit of increasing employment growth. Cities can realize these benefits by expanding road capacity or by implementing congestion pricing, a possibility that should be taken into account in future cost-benefit analyses of such policies.

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Tables and Figures

Table 1: Descriptive Statistics

Variable	Mean	Median	Max.	Min.	Std. Dev.	Obs.
Employment (000)	1042.29	607.93	10283.68	37.84	1369.56	1870
Hours of Delay (000)	26.94	5.66	695.41	0.05	70.40	1870
1947 Interstate Miles	144.51	109.50	588.00	26.00	99.63	85
Transp. Committee Members	5.31	2.00	48.00	0.00	9.07	85
Number of Colleges	29.90	18.00	329.00	1.00	40.32	1700
Crimes (000)	100.68	58.39	1177.51	0.63	135.85	1870
Population (000)	1842.24	1052.26	18699.02	111.11	2458.83	1870

Notes: Descriptive statistics are based on the full sample, spanning years 1982 through 2003 and all 85 MSAs. Some of the regressions in this analysis are based on smaller samples. In some cases this is due to occasional missing data for some of the MSAs.

Table 2: Cross Sectional Results

The dependent variable is log employment growth between the base year and 2003.

	(1) Base Year 1982	(2) Base Year 1986	(3) Base Year 1990	(4) Base Year 1990	(5) Base Year 1990	(6) Base Year 1990
Log initial congestion per capita	-0.336	-0.327	-0.234	-0.241	-0.231	-0.186
	(0.120)	(0.174)	(0.099)	(0.201)	(0.129)	(0.056)
Log initial congestion per capita squared						-0.083
						(0.019)
Log initial employment level	0.208	0.173	0.169	0.174	0.166	0.078
	(0.113)	(0.127)	(0.084)	(0.176)	(0.116)	(0.051)
Log mean January temperature	0.415	0.307	0.108	0.110	0.107	0.163
	(0.230)	(0.189)	(0.094)	(0.126)	(0.089)	(0.071)
Manufacturing share of employment	-1.103	-1.211	-0.654	-0.651	-0.655	0.068
	(0.612)	(0.646)	(0.396)	(0.406)	(0.396)	(0.231)
Log high school graduates per capita			0.085	0.089	0.084	0.030
			(0.067)	(0.099)	(0.097)	(0.045)
Log crimes per capita	0.090	0.059	0.103	0.104	0.103	0.047
	(0.225)	(0.227)	(0.074)	(0.075)	(0.074)	(0.037)
Annualized congestion elasticity	-0.019	-0.023	-0.020	-0.021	-0.020	-0.016
OLS congestion coefficient	-0.002	-0.002	-0.008	-0.008	-0.008	-0.003
	(0.036)	(0.022)	(0.033)	(0.033)	(0.033)	(0.036)
OLS congestion squared coefficient						-0.017
						(0.011)
Controls and Census division dummies included?	yes	yes	yes	yes	yes	yes
Transportation committee instrument?	yes	yes	yes	yes	no	yes
1947 highway plan instrument?	yes	yes	yes	no	yes	yes
Observations	78	78	85	85	85	85
Hansen overidentification test p-value	0.68	0.56	0.75			0.75
Kleibergen-Paap statistic	9.35	3.98	8.31	2.61	9.15	6.21

Notes: Each model was estimated using LIML. Robust standard errors in parentheses. Demographic controls includes the percent of population aged 18–60, and the percent of population that is African American. The percent of the population with a high school degree variable is not available in 1982 and 1985.

Table 3: Select Cross-Sectional First-Stage Regression Results
The dependent variable is the base year level of per capita hours of travel delay.

	(1) Base Year 1982	(2) Base Year 1986	(3) Base Year 1990	(4) Base Year 1990	(5) Base Year 1990
Log initial employment level	0.729	0.570	0.905	0.978	0.833
	(0.132)	(0.154)	(0.076)	(0.089)	(0.069)
Log mean January temperature	0.381	0.206	0.067	0.289	0.141
	(0.473)	(0.411)	(0.205)	(0.247)	(0.234)
Manufacturing share of employment	-1.737	-2.235	-0.275	0.494	-0.270
	(1.358)	(1.627)	(0.962)	(1.194)	(1.044)
Log high school graduates per capita			0.598	0.583	0.610
			(0.166)	(0.171)	(0.237)
Log crimes per capita	-0.202	-0.210	0.287	0.198	0.135
	(0.514)	(0.608)	(0.215)	(0.230)	(0.235)
Log 1947 radial road-miles per capita	-0.331	-0.334	-0.201		-0.192
	(0.140)	(0.155)	(0.063)		(0.063)
Past Transportation Committee members	-0.016	-0.005	-0.013	-0.012	
	(0.010)	(0.010)	(0.006)	(0.008)	
Observations	78	78	85	85	85
R^2	0.29	0.30	0.49	0.42	0.47

Notes: Robust standard errors in parentheses. Each regression includes Census division dummy variables, the percent of population aged 18–60, and the percent of population that is African American. The percent of the population with a high school degree variable is not available in 1982 and 1985.

Table 4: Panel Model Results
The dependent variable is log employment growth over q years.

	$q =$ 1 year	$q =$ 2 years	$q =$ 3 years	$q =$ 4 years	$q =$ 5 years
Log initial congestion delay per capita	-0.024	-0.028	-0.039	-0.040	-0.051
	(0.010)	(0.010)	(0.018)	(0.024)	(0.031)
Log initial employment level	-0.020	-0.041	-0.056	-0.101	-0.164
	(0.018)	(0.019)	(0.032)	(0.054)	(0.079)
Log initial colleges per capita	0.017	0.015	0.053	0.034	0.134
	(0.006)	(0.006)	(0.011)	(0.015)	(0.028)
Log initial crimes per capita	-0.017	-0.018	-0.014	-0.005	-0.021
	(0.005)	(0.005)	(0.008)	(0.008)	(0.008)
Annualized congestion coefficient	-0.024	-0.014	-0.013	-0.010	-0.010
OLS congestion coefficient	-0.005	-0.008	-0.005	-0.018	-0.038
	(0.002)	(0.002)	(0.005)	(0.006)	(0.008)
MSA fixed effects and demographic controls?	yes	yes	yes	yes	yes
Observations	1615	850	510	425	340
Hansen overidentification test p-value	0.15	0.19	0.53	0.15	0.21
Kleibergen-Paap statistic	6.45	8.03	7.34	8.29	9.11

Notes: Each model was estimated using LIML. Robust standard errors clustered at the MSA level are in parentheses. Demographic controls includes the percent of population aged 18–60 and the percent of population that is African American.

Table 5: Panel Model First Stage Results

The dependent variable is log hours of base year travel delay per capita.

	$q =$ 1 year	$q =$ 2 years	$q =$ 3 years	$q =$ 4 years	$q =$ 5 years
Log initial employment level	0.919	0.016	-0.138	0.042	-0.071
	(0.288)	(0.065)	(0.074)	(0.059)	(0.075)
Log initial colleges per capita	0.320	0.556	0.634	0.513	0.597
	(0.080)	(0.181)	(0.177)	(0.208)	(0.268)
Log initial crimes per capita	-0.069	0.948	0.864	0.943	0.990
	(0.068)	(0.291)	(0.305)	(0.285)	(0.281)
Log 1947 road-miles $\times$ linear trend	-0.004	-0.008	-0.012	-0.017	-0.024
	(0.001)	(0.002)	(0.003)	(0.005)	(0.006)
Transp. Committee Members in prior 10 years	-0.002	-0.002	-0.002	-0.006	-0.008
	(0.009)	(0.009)	(0.009)	(0.010)	(0.011)
Observations	1615	850	510	425	340
MSA fixed effects and demographic controls	yes	yes	yes	yes	yes
R^2	0.74	0.75	0.72	0.77	0.80

Notes: Robust standard errors clustered at the MSA level are in parentheses. Demographic controls includes the percent of population aged 18–60 and the percent of population that is African American.

Table 6: Counterfactual Scenarios

This table presents counterfactual estimates of employment growth for the 10 most congested metropolitan areas in 1990. The estimates are based on the adoption of various transportation policies.

$$\hat{E}_i = 0.1 \times \varepsilon_i^{C,K} \times \varepsilon_i^{E,C} \times E_i + E_i$$

$$\tilde{E}_i = -0.5 \times \varepsilon_i^{E,C} \times E_i + E_i$$

	Elasticity of Congestion With Respect to Freeway Capacity	Elasticity of Employment Growth With Respect to Congestion	Actual Employment Growth between 2003 and 1990	Employment Growth With 10% Increase in Freeway Capacity	Employment Growth With Tolls That Reduce Congestion 50%
	$\varepsilon_i^{C,K}$ (2)	$\varepsilon_i^{E,C}$ (3)	E_i (4)	$\hat{E}_i$ (5)	$\tilde{E}_i$ (6)
Los Angeles-Long Beach-Santa Ana	-1.756	-0.466	567,983	614,428	700,235
San Jose-Sunnyvale-Santa Clara	-2.045	-0.415	78,512	85,178	94,811
San Francisco-Oakland-Fremont	-1.904	-0.399	321,437	345,833	385,488
Seattle-Tacoma-Bellevue	-1.994	-0.292	414,979	439,124	475,529
Detroit-Warren-Livonia	-1.967	-0.287	243,373	257,108	278,288
Houston-Sugar Land-Baytown	-1.954	-0.283	852,213	899,335	972,784
New York-Northern New Jersey-Long Island	-1.801	-0.278	725,558	761,919	826,506
Washington-Arlington-Alexandria	-1.976	-0.264	638,082	671,332	722,220
Chicago-Naperville-Joliet	-1.921	-0.262	734,909	771,968	831,351
San Diego-Carlsbad-San Marcos	-1.963	-0.248	406,612	426,444	457,129

Notes: The ten MSAs in this table had the highest levels of congestion in 1990 and are sorted in descending order. The letter i indexes MSAs. The elasticities in the second column depend on the actual amount of vehicle miles traveled in each of the 10 MSAs. Similarly, the elasticities in the third column depend on estimates from column 6 in Table 2 and the actual amount of travel delay in each of the 10 MSAs.

Figure 1: Map of the entire 1947 Interstate Highway plan