Does Traffic Congestion Reduce Employment Growth?*

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Abstract

This paper examines the impact of traffic congestion on employment growth in large U.S. metropolitan areas. Historical highway construction, political variables, and other traffic measures 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.

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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%. 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. 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 into 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, so 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 employment growth. However, the task is difficult because 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,[1] 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. The approach used here, instrumental variables estimation, helps avoid simultaneity bias. However, the chief challenge is finding strong instruments for congestion that are uncorrelated with unexplained employment growth. The difficulty is compounded because satisfying the exogeneity requirement may come at the expense of instrument weakness. Hence, the choice of instruments must strike a careful balance in order to minimize bias in the estimates.

The basic econometric model is cross sectional, and follows the city growth literature (Glaeser et al., 1992; Henderson et al., 1995; Glaeser et al., 1995; Simon, 1998), which uses deeply lagged variables to examine how initial city characteristics affect long-run population, employment, and income growth. Additionally, this paper considers a dynamic panel-data model, which uses shorter lags and includes area and year fixed effects. Besides congestion, the empirical work also examines other factors that affect employment growth: human capital, crime, climate, and demographics.

The cross-section regressions in this paper use two instruments for congestion. The first 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

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

specific mention of promoting metropolitan employment growth decades later.

A second instrument for congestion measures each metropolitan area's transportation-related influence in Congress. The measure is a running total, between 1947 and 1990, of 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 think that this measure is orthogonal to unexplained employment growth.

The panel-data regressions use three different instruments for congestion, the first of which is a measure of fatal automobile accidents. This measure proxies for dangerous road conditions and driving behavior. The rationale is that, in addition to causing fatal accidents, dangerous highways lower traffic speeds (either by causing accidents or by inducing cautious driving) and increase congestion. The second instrument, which measures the composition of traffic, equals the once-lagged ratio of registered large trucks to cars. Large trucks directly increase congestion because they take up more physical space on freeways and damage roads, leading to repairs that require congestion-producing lane closure. Such damage may also have an indirect effect, because road repair diverts funds away from capacity expansion and transit, both of which reduce congestion. The third instrument is a measure of historical highway capacity equal to the ten-year lag of freeway lane-mileage within a city. Although these instruments are discussed in greater detail below, there is little reason to suspect that they are correlated with unexplained employment growth.

Robust results from a variety of model specifications imply that congestion has a nontrivial dampening effect on employment growth in the long run and a smaller dampening effect in the short run. The best estimate of the elasticity of employment growth with respect to per capita hours of travel delay is -0.054 in the long-run and -0.040 in the short-run.

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:

$$ln(EMP_{i,t}/EMP_{i,t-k}) = \beta ln(CONG_{i,t-k}) + ln(X_{i,t-k})^{'}\theta + \phi ln(EMP_{i,t-k}) + \nu_i \qquad (1)$$

The units of observation are U.S. metropolitan areas, indexed by i. The subscript t indexes time. Hence, $EMP_{i,t}/EMP_{i,t-k}$ is employment growth in a city between year $t-k$ and year t. In the empirical implementation of the model, 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 city fixed effects and year fixed effects. These variables control for time-invariant city characteristics and country-wide macroeconomic trends, respectively. The panel data model takes the form of an augmented Dickey-Fuller regression[2]:

$$
\begin{aligned}
ln(EMP_{i,t}/EMP_{i,t-1}) \quad = \quad & \beta^S ln(CONG_{i,t-1}) + ln(X_{i,t-1})'\gamma \\
& +\psi ln(EMP_{i,t-1}/EMP_{i,t-2}) + \delta ln(EMP_{i,t-1}) \\
& +\alpha_i + \lambda_t + \epsilon_{i,t}.
\end{aligned} \tag{2}
$$

Again i indexes cities and t indexes years. EMP is the level of employment, $CONG$ is a measure of congestion, and X is a vector of explanatory variables. The symbols α and λ represent city and year fixed effects respectively. Although the ϵ term is assumed to be white noise, the empirical work will formally test the assumption of no serial correlation. Note that Equation 2 above contains employment growth lagged one year. This inclusion requires controlling for dynamic panel bias (Nickell, 1981), because the lagged dependent variable is correlated with the differenced error term. If the model's error term is serially uncorrelated, the two year lag of the dependent variable can serve as an instrument for the

[2]In principle, one could use Equation 2 to test for unit roots in the log employment level. However, the augmented Dickey-Fuller test requires spatial independence between the metropolitan areas in the panel. This assumption is overly restrictive, especially when considering metropolitan area employment levels. Appendix A documents a different test for unit roots, developed by Pesaran (2007), which allows for spatial dependence. The result of the test does not suggest a unit root in log employment levels.

endogenous one year lag.

The dynamic structure of the panel data model allows calculation of short and long-run elasticities. In Equation 2, congestion's coefficient β^S measures the short-run elasticity of employment growth with respect to the initial level of congestion. Using the coefficient on the lag term, the long-run elasticity of employment growth with respect to the initial level of congestion is:

$$\beta^L = \frac{\beta^S}{1 - \psi}. \tag{3}$$

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.[3] 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.[4] This measure is based on data from the Highway Performance Monitoring System database 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

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

[4]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.

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:

$$actual\ speed_{h,\tau} = f(V_{h,\tau}, K_{h,\tau}, R_{h,\tau}). \tag{4}$$

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:

$$travel\ delay_{h,\tau} = \frac{length_h \cdot V_{h,\tau}}{(free\ flow\ speed_h - average\ actual\ speed_{h,\tau})}, \tag{5}$$

where $length_h$ is the centerline mileage of highway h. Summing across highway segments and time periods gives total annual delay for a city.[5] 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 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.

[5]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 5. 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 adds this amount to their measure of recurring delay.

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. Instrumental variables can control for such endogeneity bias, provided the instruments are strongly correlated with congestion and orthogonal to unexplained employment growth. The literature provides few suggestions, although Boarnet (1997) used the proportion of accidents resulting in fatalities and vehicles per capita to serve as instruments for congestion. The present analysis does not use those exact instruments because the data are not available annually at the city level.

In regressions based on Equation 1, the first instrument for congestion is the number of radial road-miles in MSA i 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.[6] Although the plan was modified somewhat after 1947, it largely determined the eventual network of Interstate 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 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

[6] 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).

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, providing 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, and it 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. The yearly member counts are then summed by MSA between 1947 and various base years, which are 1982, 1986 and 1990. Finally, the MSA member counts are divided by base year populations.

The empirical work finds that this measure is negatively correlated with base year congestion. 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.

The two time-invariant instruments just described cannot be used in the fixed effects specification of Equation 2. Instead, the panel regressions use three different instruments, each of which varies across time and across MSAs. The first instrument is the ratio of vehicle fatalities on urban freeways to vehicle fatalities on other roads within an MSA. This measure proxies for the physical nature of the freeways (e.g., curvature, grade etc.) and the driving behavior of citizens (e.g., the propensity to drive while intoxicated). The second instrument in the panel regressions is the ratio of registered trucks to passenger cars within a state, lagged one year. The third instrument for congestion is the ten year lag of freeway lane mileage in an MSA. These three instruments are discussed in turn.

Dangerous freeways cause more fatalities, while increasing delay if they encourage people to drive more slowly or to leave larger headways. From this perspective, one would expect a positive correlation between vehicle fatalities and congestion-related travel delay. On the other hand, more congestion also means slower traffic speeds, which reduces the severity of accidents. Thus, the expected sign in the first-stage regression is not immediately clear. To deal with such potential nonlinearity empirically, the instrument list in the panel regressions includes a quadratic term in the fatality instrument.[7]

The second instrument in the panel regressions, the truck-to-car ratio, is related to congestion in several ways. For one, trucks take up more physical space on freeways, which reduces effective capacity and increases congestion. Also, trucks also tend to brake more slowly than cars, which causes truck drivers to leave larger headways, slowing down traffic. Finally, trucks cause more damage to roads than do passenger vehicles. Increased damage means that, all else equal, MSAs with more trucks must devote more resources to road repair, which itself can cause congestion. The empirical work below finds that, as expected, a higher truck-to-car ratio is positively related to congestion levels.

One problem with the truck-to-car ratio is that employment growth increases shipping, thereby raising the number of registered trucks within an MSA. Since this argument rules out using contemporaneous changes in the truck-to-car ratio as an instrument for congestion, a one-year lag is used instead.[8]

The third instrument for congestion is the ten-year lag of freeway lane-mileage in an

[7]Although the fatality instrument seems reasonable, consider the following potential argument against its validity. Suppose a given MSA has dangerous urban freeways and high levels of congestion. Also, suppose that the freeways are dangerous because the MSA's government is incompetent. If so, the unobservable incompetence would likely be correlated with other factors that determine employment growth. Such factors could include government corruption and poor provision of public services. These factors could dissuade in-migration of new firms. There are three main reasons why this line of reasoning is flawed. First, the instrument is the ratio of fatalities on urban freeways to fatalities on other roads. Hence, government mismanagement would have to affect urban freeway fatalities more than other fatalities for the argument to be valid. Second, the characteristics of urban freeways like curvature and grade are often inherited from older roads. These design features of dangerous roads would likely predate contemporary incompetent governments. Finally, Federal Aid Highways must adhere to uniform design standards that are out of the control of local governments. Together, these facts make the incompetence argument unconvincing.

[8]Using the one year lag of the instrument assumes that an increase in the number of trucks does not anticipate employment growth. This assumption is reasonable because it is unlikely that firms would invest in trucks prior to hiring additional workers. Furthermore, not all vehicles classified as trucks (e.g., large SUVs) are used for commerce; many trucks are used to commute.

MSA. The logic behind this instrument is similar to that for the 1947 plan. All else equal, metropolitan areas with more freeway capacity tend to have lower levels of congestion. The validity of this instrument hinges upon the extent to which transportation planners correctly anticipate long-run employment growth. However, like the Interstate Highway System, most roads are planned and constructed long before they are actually used. Although the instrument is the ten-year lag of actual freeway lane-mileage, the date when those freeways were planned is what bears on instrument validity. Assuming it takes P years to plan and construct a new freeway, the instrument would not be valid if planners correctly forecast employment growth more than $10 + P$ years into the future.

Using the lagged truck-to-car ratio and the ten-year lag of freeway lane-mileage as instruments requires serially uncorrelated error terms. The empirical work below formally tests for and finds no evidence of serial correlation across all specifications. In addition, tests of the overidentifying restrictions support the exogeneity of all instruments across model specifications.

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. The number of registered cars and trucks in each state is provided by the Federal Highway Administration's *Highway Statistics* publications. Fatal automobile accident data are from the National Highway Traffic Safety Administration's Fatal Accident Reporting 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. Most regressions use LIML, because studies have shown it to perform better than two-stage least squares in the presence of weak instruments (Bekker, 1994; Staiger and Stock, 1997; Stock and Yogo, 2005).

Cross Sectional Estimates

Table 2 presents results from cross-sectional regressions with base years 1982, 1986, and 1990. The advantage of these estimates is that they portray the long-run response of employment growth to the initial level of congestion, but the drawback is less precise estimates due to a small sample size.

Equation 1 is estimated using LIML for a variety of time periods. In each case, 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 radial road-miles from the 1947 Interstate plan and historic transportation committee members per capita instrument 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). Select first-stage results in the bottom panel of Table 2 also show that 1947 planned road miles and transportation committee members per capita are both negatively correlated with congestion levels. For the specifications without the squared congestion regressor, the estimates of the congestion coefficient range from -0.281 to -0.399. The rightmost specification in column 7 includes congestion and its square, and the corresponding estimated coefficients are -0.162 and -0.071. 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 7 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 higher employment growth, and that a high share of employment in manufacturing is 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.

Table 2 also presents tests of the overidentifying restrictions. When using LIML, one can test whether the restrictions are valid using an Anderson-Rubin (1950) test statistic, which is distributed chi-square with degrees of freedom equal to the degree of overidentification. This test statistic is the LIML analog to the Sargan test statistic. Failure to reject the null hypothesis that the overidentifying restrictions are valid lends support to the exogeneity of the instruments. The p-values of the Anderson-Rubin test statistics support instrument exogeneity across all overidentified specifications. 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.

Starting from the left of Table 2, we see that specifications including only the two primary instruments (columns 1–3) yield similar point estimates regardless of the base year. However, the values of the Kleibergen-Paap statistics[9] suggest weak-instrument bias may be a problem, especially in the specification with base year 1986. Unfortunately, when the model error terms are heteroskedastic, one cannot formally test for weak-instrument bias using the approach proposed by Stock and Yogo (2005). That approach tests for downward bias in the estimated standard errors for the endogenous variable, and it is only valid when

[9] The Kleibergen-Paap rank 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.

model error terms are assumed to be iid.[10] Nevertheless, the Stock-Yogo critical values give a rough sense for whether or not the estimated standard errors are too small. 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 instruments. The critical value is 4.84 with two endogenous regressors and five instruments.[11] Although we cannot directly compare the Kleibergen-Paap rank statistics to these values, they do suggest that the estimated standard errors may be biased in some of the specifications.

Columns 4 and 5 present the base year 1990 specification with the two primary instruments in isolation. The congestion coefficient is -0.290 when only the transportation committee instrument is used, and is -0.277 when only the 1947 plan instrument is used. In a sense, these results provide informal evidence that weak-instrument bias is not a problem: two very different instruments yield similar point estimates for the congestion coefficient.

Although the main results regarding congestion are robust to different instruments and specifications, it should again be stressed that weak instrument bias may be a problem.

Panel Estimates

The results from estimating panel Equation 2 are presented in Tables 3 and 4. What changes the most across these tables is the set of instruments for congestion. If the instruments are invalid or weak one would expect the point estimates to vary with the subset of instruments chosen. Hence, the tables include results using various combinations of the instruments or the instruments in isolation.

Each of the panel specifications includes MSA dummy variables. To control for contemporaneous macroeconomic shocks, some specifications also include year dummies or alternatively the lagged log change in U.S. GDP. Additionally, each specification includes

[10] When LIML is used, Stock and Yogo do not provide a test for bias in the estimated coefficient for the endogenous regressor.

[11] 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.

demographic controls, a measure of human capital (the number of two and four year colleges per capita), and the crime rate. All of the panel models include the endogenous one-year lag of the dependent variable. Therefore, to avoid dynamic-panel bias, each instrument list also includes the corresponding two year lag.

Columns 1–3 of Table 3 present the OLS results. In each specification, the estimated congestion coefficient is statistically indistinguishable from zero. The next three columns, labeled 4–6, present LIML estimates with the freeway fatality ratio and truck-to-car ratios instrumenting for congestion. In the specification that only controls for MSA fixed effects (column 4), the point estimate on the congestion coefficient (β^S) is precisely estimated and equals -0.040. In column 5, we see that controlling for year fixed effects yields a similar point estimate of -0.039. Further, the coefficients on the year dummy variables are jointly statistically significant with an F-value of 22.61. Replacing the year dummies with the lagged change in U.S. GDP lowers the estimated value of β^S to -0.028. Furthermore, the coefficient on lagged GDP indicates that changes in nationwide output anticipate changes in employment growth across cities.

Many of the other control variables are also estimated precisely. The results suggest that the initial level of colleges per capita increases employment growth while crimes tend to decrease employment growth. Finally, the estimates of the first stage congestion equation show that the urban freeway fatality measure and truck-to-car ratio are both positively correlated with congestion. Additionally, for two of the specifications (columns 4 and 6), the instruments are strong and have Kleibergen-Paap rank statistics greater than 15.

Moving to the right, the next three columns of Table 3 show results from specifications that expand the instrument list to include the ten year lag of log freeway lane mileage. The estimates on the congestion coefficient are again negative and range from -0.029 to -0.120. However, these estimates tend to be less precise, because the data for the freeway capacity instrument only exist after 1982. Hence, lagging the capacity variable ten years shortens the time series length, thereby reducing precision. The first-stage results confirm expectations that the ten-year lag of freeway capacity is negatively correlated with congestion levels, although the relationship is weak as indicated by the low Kleibergen-Paap rank statistic.

Across all specifications, the p-values of the Anderson-Rubin test statistics support instrument exogeneity. However, in some specifications, lagged variables serve as instruments. For the lagged instruments to be valid, it is crucial that the errors be serially uncorrelated. Tables 3 and 4 report results from tests for serial correlation in the error terms. The test for serial correlation comes from Godfrey (1994), and is valid for dynamic models estimated using an instrumental variables approach. The test was conducted as follows. After estimating Equation 2, the once-lagged residuals were retained. These residuals were then added as regressors and instruments to the original model, which was then reestimated. If a t-test indicates that the coefficient on the lagged residuals is statistically significant, serial correlation may be a problem. The p-values associated with these t-statistics are reported in the bottom panel of Tables 3 and 4; the values do not suggest serial correlation in the error terms.

Table 4 presents further evidence that the results are robust to using the instruments in isolation. Comparing the results across columns 1–6, the point estimates range from -0.030 to -0.041 when only the freeway fatality instrument is used. By contrast, if only the truck ratio instrument is used, the estimates range from -0.022 to -0.044. The specifications in columns 7–9 include only the ten year lag of freeway capacity. These estimates are larger and range from -0.086 to -0.131. However, they are based on a shorter time series and as a consequence are less precisely estimated.

The estimates in Tables 3 and 4 also yield long-run elasticities. Using Equation 3, the estimates of β^L range from -0.037 to -0.166. These long-run estimates are not appreciably larger than their short-run counterparts, which can be explained by the small estimates of ψ, the coefficient on the lagged dependent variable.

Although the statistical evidence supports instrument orthogonality, 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, these specifications produce point estimates for β^S that are statistically indistinguishable from other specifications that rely on somewhat weak instruments.

Explaining Differences in the Estimates

Controlling for endogenous congestion yields estimates of β^S, the congestion coefficient, that are more negative than the corresponding OLS estimates. The LIML estimates that are significant at the 1% level range from -0.022 to -0.044, 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^S towards zero. If congestion is indeed endogenous and the instruments are valid and not weak, then the LIML estimates presented here should be consistent.

The difference between the elasticity estimates from the two models is also worthy of attention. Although there is some overlap, the estimates of the congestion elasticity from the cross-sectional model are typically larger (ranging from -0.054 to -0.399) than those from the panel model (ranging from -0.028 to -0.131). However, some of the estimates have higher t-statistics, and are hence more credible, than others. For example, the most precise estimates from the cross-sectional model are from the regression that includes the congestion measure squared, which yield an elasticity estimate of -0.054 when evaluated at the mean level of congestion. This estimate is very similar in magnitude to the significant estimates from the panel model, which range from -0.022 to -0.108.

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. 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:

$$\hat{E}_i = \kappa \times \varepsilon_i^{C,K} \times \varepsilon_i^{E,C} \times E_i + E_i, \tag{6}$$

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:

$$\tilde{E}_i = \mu \times \varepsilon_i^{E,C} \times E_i + E_i. \tag{7}$$

Although the counterfactual estimates in Equations 6 and 7 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 5.[12]

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

[12]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.

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 5 presents two sets of counterfactual estimates of employment growth between 1990 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 7 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
Truck-to-Car Ratio	0.51	0.49	0.55	1.46	0.22	1870
Fatality Ratio	0.32	0.32	0.75	0.02	0.13	1870
Freeway Lane Miles	833.69	530.00	7170.00	20.00	977.44	1870
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	(7) Base Year 1990
Log initial congestion per capita	-0.362	-0.379	-0.281	-0.290	-0.277	-0.399	-0.162
	(0.127)	(0.197)	(0.121)	(0.216)	(0.156)	(0.216)	(0.072)
Log initial congestion per capita squared							-0.071
							(0.027)
Log initial employment level	0.225	0.207	0.211	0.219	0.208	0.318	0.109
	(0.121)	(0.145)	(0.105)	(0.188)	(0.141)	(0.178)	(0.064)
Log mean January temperature	0.429	0.317	0.124	0.127	0.123	0.243	0.162
	(0.240)	(0.209)	(0.094)	(0.125)	(0.093)	(0.131)	(0.067)
Manufacturing share of employment	-1.118	-1.271	-0.632	-0.628	-0.634	-0.513	-0.024
	(0.651)	(0.710)	(0.431)	(0.446)	(0.431)	(0.533)	(0.273)
Log high school graduates per capita			0.113	0.118	0.111	0.084	0.050
			(0.087)	(0.108)	(0.118)	(0.115)	(0.046)
Log crimes per capita	0.086	0.040	0.106	0.107	0.106	0.020	0.057
	(0.241)	(0.260)	(0.084)	(0.085)	(0.083)	(0.120)	(0.040)
Demographic controls	yes	yes	yes	yes	yes	yes	yes
First stage coefficients from congestion equation							
a. Log 1947 radial road miles	-0.274	-0.281	-0.173	-	-0.173	-0.164	-0.439
b. Log Transp. Committee Members per cap.	-0.016	-0.006	-0.012	-0.009	-	0.034	0.029
c. Interaction between a. and b.	-	-	-	-	-	-0.006	-0.006
d. Square of a.	-	-	-	-	-	0.011	0.039
e. Square of b.	-	-	-	-	-	-0.001	-0.001
Observations	78	78	85	85	85	85	85
Anderson-Rubin overid. test p-value	0.84	0.67	0.96	-	-	0.97	0.77
Kleibergen-Paap rk Wald F statistic	8.47	3.35	6.50	2.47	7.79	1.45	5.43

Notes: Each model was estimated using LIML. Robust standard errors in parentheses. Demographic controls includes the percent of population aged 18–60, the percent of population under age 30, 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: Panel Results
The dependent variable is log employment growth.

	(1) OLS	(2) OLS	(3) OLS	(4) LIML	(5) LIML	(6) LIML	(7) LIML	(8) LIML	(9) LIML
Log initial congestion delay per capita	-0.001	-0.002	-0.001	-0.040	-0.039	-0.028	-0.029	-0.120	-0.067
	(0.002)	(0.002)	(0.002)	(0.010)	(0.013)	(0.008)	(0.025)	(0.074)	(0.026)
Log initial employment level	-0.051	-0.043	-0.038	0.001	-0.025	-0.005	0.012	-0.023	0.053
	(0.009)	(0.011)	(0.008)	(0.014)	(0.011)	(0.012)	(0.045)	(0.046)	(0.044)
Lagged log change in employment	0.327	0.376	0.317	0.196	0.550	0.352	-0.106	0.096	-0.028
	(0.037)	(0.048)	(0.037)	(0.076)	(0.079)	(0.064)	(0.187)	(0.313)	(0.178)
Lagged log change in U.S. GDP			1.351			1.370			1.490
			(0.089)			(0.081)			(0.202)
Log initial colleges per capita	0.010	0.005	0.009	0.017	0.009	0.014	0.045	0.001	0.037
	(0.003)	(0.002)	(0.002)	(0.004)	(0.003)	(0.003)	(0.007)	(0.014)	(0.007)
Log initial crimes per capita	-0.012	-0.003	-0.003	-0.019	-0.009	-0.007	-0.003	-0.006	-0.010
	(0.003)	(0.003)	(0.002)	(0.004)	(0.004)	(0.003)	(0.006)	(0.009)	(0.006)
Demographic controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
MSA fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year fixed effects	no	yes	no	no	yes	no	no	yes	no
First stage coefficients from congestion equation									
a. Freeway fatality ratio	-	-	-	0.141	0.087	0.141	0.145	0.160	0.114
b. Freeway fatality ratio squared	-	-	-	0.058	0.048	0.057	0.032	0.035	0.023
c. Lagged ratio of trucks to passenger cars	-	-	-	0.495	0.243	0.503	0.495	0.123	0.419
d. 10 year lag of freeway lane mileage	-	-	-	-	-	-	-0.233	-0.174	-0.222
Observations	1615	1615	1615	1615	1615	1615	935	935	935
Number of MSAs	85	85	85	85	85	85	85	85	85
Anderson-Rubin overid. test p-value	-	-	-	0.30	0.56	0.39	0.25	0.33	0.31
Kleibergen-Paap rk Wald F-statistic	-	-	-	15.23	6.44	16.03	4.16	2.23	3.82
Serial correlation test p-value	-	-	-	0.40	0.25	0.35	0.22	0.41	0.23

Notes: Robust standard errors in parentheses. Demographic controls includes the percent of population aged 18–60, the percent of population under age 30, and the percent of population that is African American. The time series are shorter for models that include the 10 year lag of freeway lane mileage as an instrument. This is because the data is not available prior to 1982.

Table 4: Panel Results - Instrument Robustness Check
The dependent variable is log employment growth.

	(1) LIML	(2) LIML	(3) LIML	(4) LIML	(5) LIML	(6) LIML	(7) LIML	(8) LIML	(9) LIML
Log initial congestion delay per capita	-0.030	-0.038	-0.041	-0.044	-0.043	-0.022	-0.086	-0.131	-0.108
	(0.014)	(0.013)	(0.014)	(0.011)	(0.023)	(0.009)	(0.047)	(0.068)	(0.052)
Log initial employment level	-0.012	-0.025	0.012	0.005	-0.023	-0.012	0.079	-0.027	0.101
	(0.020)	(0.012)	(0.020)	(0.016)	(0.015)	(0.012)	(0.062)	(0.038)	(0.067)
Lagged log change in employment	0.191	0.547	0.361	0.193	0.551	0.349	0.093	0.211	0.111
	(0.073)	(0.085)	(0.070)	(0.077)	(0.079)	(0.062)	(0.218)	(0.304)	(0.236)
Lagged log change in U.S. GDP			1.380			1.366			1.747
			(0.089)			(0.078)			(0.380)
Log initial colleges per capita	0.015	0.009	0.016	0.017	0.009	0.013	0.051	0.000	0.040
	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.003)	(0.009)	(0.014)	(0.009)
Log initial crimes per capita	-0.017	-0.009	-0.009	-0.019	-0.010	-0.006	-0.014	-0.007	-0.018
	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)	(0.003)	(0.009)	(0.008)	(0.011)
Demographic controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
MSA fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year fixed effects	no	yes	no	no	yes	no	no	yes	no
First stage coefficients from congestion equation									
a. Freeway fatality ratio	0.171	0.091	0.171	-	-	-	-	-	-
b. Freeway fatality ratio squared	0.066	0.050	0.067						
c. Lagged ratio of trucks to passenger cars	-	-	-	0.510	0.264	0.521	-	-	-
d. 10 year lag of freeway lane mileage	-	-	-	-	-	-	-0.244	-0.184	-0.227
Observations	1615	1615	1615	1615	1615	1615	935	935	935
Number of MSAs	85	85	85	85	85	85	85	85	85
Anderson-Rubin overid. test p-value	0.19	0.28	0.59	-	-	-	-	-	-
Kleibergen-Paap rk Wald F-statistic	3.99	5.10	3.88	24.82	6.67	27.13	3.54	2.15	3.02
Serial correlation test p-value	0.42	0.31	0.18	0.46	0.301	0.64	0.387	0.789	0.15

Notes: Robust standard errors in parentheses. Demographic controls includes the percent of population aged 18–60, the percent of population under age 30, and the percent of population that is African American. The time series are shorter for models that include the 10 year lag of freeway lane mileage as an instrument. This is because the data is not available prior to 1982.

Table 5: 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	-1.745	-0.401	567,983	607,763	681,936
San Jose-Sunnyvale-Santa Clara	-2.123	-0.358	78,512	84,480	92,568
San Francisco-Oakland-Fremont	-1.952	-0.344	321,437	343,011	376,693
Seattle-Tacoma-Bellevue	-2.060	-0.253	414,979	436,567	467,374
Detroit-Warren-Livonia	-1.934	-0.248	243,373	255,061	273,592
Houston-Sugar Land-Baytown	-1.959	-0.245	852,213	893,097	956,585
New York-Northern New Jersey-Long Island	-1.915	-0.241	725,558	759,031	812,961
Washington-Arlington-Alexandria	-1.982	-0.228	638,082	666,983	710,978
Chicago-Naperville-Joliet	-1.856	-0.227	734,909	765,921	818,470
San Diego-Carlsbad-San Marcos	-1.854	-0.215	406,612	422,858	450,413

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 7 in Table 2 and the actual amount of travel delay in each of the 10 MSAs.

Appendix A

In the empirical implementation of Equation 2, there is little reason to believe that any of the control variables are non-stationary: most control variables are in logarithmic form and are measured per capita. However, employment growth may be nonstationary if positive shocks to the employment level have permanent, self-reinforcing effects via economies of agglomeration. To formally test whether log employment levels are nonstationary, consider the Pesaran (2007) test for unit roots in panel data. This test is appropriate for panel data with cross-sectional dependence and first-order serially correlated errors. Such cross-sectional dependence may arise when using MSA level panel data because of spatial proximity. Pesaran's test is based upon a cross-sectionally augmented Dickey-Fuller regression:

$$\begin{aligned} \Delta ln(EMP_{i,t}) &= a_i + b_i ln(EMP_{i,t-1}) + c_i ln(\overline{EMP}_{t-1}) \\ &\quad + d_{i,0}\Delta ln(\overline{EMP}_t) + d_{i,1}\Delta ln(\overline{EMP}_{t-1}) \\ &\quad + \delta_{i,1}\Delta ln(EMP_{i,t-1}) + \varepsilon_{i,t} \end{aligned} \tag{A-1}$$

where $\overline{EMP_t}$ is the average employment level across MSAs at time t. This regression is run for each of the MSAs in the sample. The t-statistics, denoted by t_i^*, associated with each estimated b_i are retained. The panel unit root test statistic, which Pesaran calls $CIPS^*$, is just the average of these t_i^*s. The null hypothesis of the test assumes that all of the series are nonstationary, while the critical values of this $CIPS^*$ statistic are given by Pesaran (2007). Using the full sample of 85 MSAs, the $CIPS^*$ statistic is estimated to be -2.338. This value exceeds the 1% level critical value of -2.170, leading to a rejection of the null hypothesis of nonstationarity. Finally, it should be noted that Pesaran finds some Monte Carlo evidence that his test has good power properties when the number of time periods is greater than 20. Together, this evidence suggests that there is not a unit root in the log employment level.