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**The Direct and Indirect Economic Effects of
Transportation Infrastructure**

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I. Introduction

The notion that highways boost economic activity is a popular one. States such as Iowa and Wisconsin have promoted highway policy as an economic development tool (Dalton 1991; Forkenbrock and Plazah 1986). Benefit-cost analyses of particular highway corridors have, at times, claimed large long-term economic gains (e.g. Seskin 1990; Weisbrod and Beckwith 1992). Yet for years, economists have argued that the common perception of a link between highways and economic development is, at best, incomplete.

In their classic study of highway impacts, Mohring and Harwitz (1962) contended that the offices, shops, and residences in a particular highway corridor might easily have located elsewhere had the highway never been built. More recently, Forkenbrock (1990) and Forkenbrock and Foster (1990) have argued that, in the case of particular highway corridors, much of the purported economic benefit from the highway is simply a redistribution of activity from nearby areas.

This paper examines how road and highway investments redistribute economic activity by dividing the economic impacts of transportation infrastructure into a direct and an indirect effect. The direct effect is the impact near a street or highway. The indirect effect is any impact that occurs at locations more distant from the corridor.

To be more specific, for purposes of this paper, the direct effect is the economic impact within the same jurisdiction that contains the street or highway. The indirect effect is the economic impact outside of the jurisdiction that contains the street or highway. Since this paper

uses data on California counties, the direct economic effect of a county's transportation infrastructure includes economic impacts within the same county. The indirect effect is any impact that street or highway capital in one county has on economic conditions in other counties.

Those studies that examine economic impacts near particular corridors (e.g. Seskin 1990; Weisbrod and Beckwith 1992) measure only a direct effect.¹ Yet, as Forkenbrock and Foster (1990) persuasively argue, nearby economic impacts are only part of the story. This paper uses data on street and highway infrastructure in California counties from 1969 through 1988 to verify the existence of both a direct and an indirect effect of road and highway capital. The results show that ground transportation infrastructure has partially opposing direct and indirect effects. This suggests that, as both Forkenbrock and Foster (1990) and Mohring and Harwitz (1962) hypothesized, some of the economic activity associated with transportation infrastructure investments would have occurred elsewhere had the road or highway not been built.

II. Background

The empirical test in this paper follows in the tradition of recent production function studies of public infrastructure. Such studies typically test the hypothesis that output or productivity within a metropolitan area, state, or nation depends in part on public capital stocks within the same jurisdiction. Since those studies rarely examine how infrastructure stocks in one jurisdiction affect economic activity in other jurisdictions, they are not able to separate direct and

¹ Both Seskin (1990) and Weisbrod and Beckwith (1992) are corridor studies, and both focus on economic impacts within the region surrounding proposed highway improvements. Yet Seskin does report that during construction of proposed freeway improvements in Boston, some retailing activity was forecast to shift away from the downtown area. Still, the primary focus of both studies is on nearby, or direct, economic impacts.

indirect effects.² The innovation here is to examine not only how output in a jurisdiction depends on its own street and highway capital stock (a direct effect), but also how output is influenced by other jurisdictions' street and highway stocks (an indirect effect). Before we continue further, some discussion of production function studies of public capital is necessary.

A. Production Function Studies of Public Capital

Highway infrastructure accounts for almost one-third of the United States' non-military public capital stock (Gramlich 1994), and thus the large literature on the economic effects of public capital can be (and has been) applied to the particular case of highways.³ The metropolitan area studies of Eberts (1986), Eberts and Fogarty (1987), and Duffy-Deno and Eberts (1991) suggested a positive link between a metropolitan area's infrastructure stock and its output, personal income, and private investment. Yet it was Aschauer's (1989) finding of large positive elasticities of private sector productivity with respect to public infrastructure which sparked public attention. His results implied that a marginal dollar invested in public capital would produce greater economic benefits than a marginal dollar invested in private capital. Authors such as Lemer (1992) and Nathan (1992) cited this as evidence of the importance of infrastructure investment as an economic development tool.

Yet some analysts suggested that the magnitude of Aschauer's public capital elasticities were implausibly large, and that difficulties in the econometric use of time series data might

² A notable exception is the study by Holtz-Eakin and Schwartz (1995), which is discussed below.

³ The U.S. Department of Transportation (1992) reviewed the literature on production function studies of public infrastructure, with particular attention to the special case of highway infrastructure. See also McGuire (1992) for a discussion of how production function studies of public capital can be applied to the particular case of highways.

undermine the reliability of his findings. (See, e.g., Aaron 1990, Jorgenson 1991, and Tatom 1991.) To overcome some of the difficulties inherent in time series estimation of aggregate production functions, researchers turned to panels of state data.

Holtz-Eakin (1994), using a panel of data on U.S. states, estimated regressions of the form shown below.

$$\begin{aligned} \log(Q_{s,t}) = & \beta_1 \log(L_{s,t}) + \beta_2 \log(K_{s,t}) & (1) \\ & + \beta_3 \log(G_{s,t}) + f_s + y_t + \varepsilon_{s,t} \end{aligned}$$

where Q = output

L = employment

K = private sector capital stock

G = public sector capital stock

f_s = a state-specific variable

y_t = year-specific dummy variables

$\varepsilon_{s,t}$ = an independent and identically distributed (i.i.d.) error term

"s" indexes states; "t" indexes years

The regression in (1) includes a unique variable for each state. These state effects (the f_s shown above) can help capture time invariant factors that influence state output, such as geography, climate, and natural resource endowments. If those time invariant effects are correlated with public infrastructure stocks, omitting the state effects risks biasing the econometric estimates.

Holtz-Eakin (1994) found that, when controlling for unique state effects, the elasticity of public capital was not significantly different from zero in a linear production function framework of the sort shown in equation (1). Similar results have been obtained by Evans and Karras

(1994), Garcia-Mila, McGuire, and Porter (1996), and Kelejian and Robinson (1994). The result, which appears somewhat robust, is that specifications that correct for time series difficulties and unique state effects show no marginal effect from infrastructure on national or state output or productivity.⁴ The same result has been obtained in studies that examined only highway infrastructure (Holtz-Eakin and Schwartz 1995).

B. Two Views of Indirect Highway Effects

As mentioned earlier, the idea of indirect highway effects is simply an adaptation of the classic argument that economic activity near a highway is, in part, activity that would have occurred elsewhere (Forkenbrock and Foster 1990; Mohring and Harwitz 1962). In this view, if direct (or nearby) economic impacts are positive, that is due, in part, to negative indirect effects at other locations. Yet there is a second view of indirect road and highway effects which was most recently articulated in the context of production function studies of public capital.

Munnell (1992) hypothesized that public capital (highways included) creates positive cross-state spillovers. This could occur when infrastructure investments in one state benefit persons in other states.⁵ In this view, indirect highway effects are positive, not negative, as areas outside the immediate jurisdiction share in the economic benefits of street and highway projects.

⁴ In particular, the two most important econometric corrections are differencing when using time series data (e.g. Tatom 1991) and controlling for unique unobservable effects when using panel data (e.g. Holtz-Eakin 1994). Garcia-Mila, McGuire, and Porter (1996) conducted a thorough specification search in their state panel data study. They found that the preferred specification uses differences in the variables and controls for unique state effects.

⁵ Note that this is consistent with a large amount of work in the public goods literature which assumes such spillovers. See, e.g., Case, Hines, and Rosen (1993, p. 288) for a discussion.

In fact, such positive spillovers are one justification for the large federal subsidies which have characterized highway finance since even before the 1956 Interstate Highway Act.

Holtz-Eakin and Schwartz (1995) tested Munnell's hypothesis of positive output spillovers for the case of highway capital by defining an effective highway capital which depends on highway infrastructure in a state plus some fraction of highway infrastructure in neighboring states. The definition of effective highway capital is shown below in matrix notation.

$$h^e = h + \delta \mathbf{W} h_e \quad (2)$$

where h = a column vector of highway capital stocks in each of the 48 contiguous states
 \mathbf{W} = a (48x48) matrix such that $w_{i,j} = 1$ if states "i" and "j" share a border, 0 otherwise⁶
 h^e = a column vector of effective highway capital stocks
 δ = a scalar parameter to be estimated

Effective highway capital, h^e , includes any spillover effects of the sort hypothesized by Munnell (1992). Transforming (2) such that only observable variables are on the right-hand-side, one gets $h^e = (\mathbf{I} - \delta \mathbf{W})^{-1} h$. Holtz-Eakin and Schwartz (1995) included h^e in a Cobb-Douglas production function. They estimated the parameters of the regression which is shown below in matrix and column vector notation.

$$\begin{aligned} \log(Q_t) = & \beta_1 \log(L_t) + \beta_2 \log(K_t) \\ & + \beta_3 \log(\mathbf{I} - \delta \mathbf{W})^{-1} \log(H_t) + f + y_t + \varepsilon_t \end{aligned} \quad (3)$$

where Q = state output
 L = employment
 K = private capital stock

⁶ Holtz-Eakin and Schwartz (1995) normalized the \mathbf{W} matrix by dividing each element $w_{i,j}$ by the number of states that border "i" and also by dividing each element by the land area of state "j". For purposes of this background discussion, the normalization is not important, and readers are referred to Holtz-Eakin and Schwartz (1995) for a more extended discussion of the definitions for \mathbf{W} .

H = state highway capital stock
 y_t = year-specific intercepts
f = a vector of time invariant state-specific variables
 ε = an i.i.d. disturbance
"t" indexes years

The coefficient δ is a spillover parameter and thus measures the indirect effect of highway capital in neighboring states. Holtz-Eakin and Schwartz (1995) rejected the hypothesis that highway capital has positive output spillovers, and in some of their specifications the spillover parameter was significantly negative.

Yet despite the fact that Holtz-Eakin and Schwartz (1995) rejected Munnell's (1992) hypothesis, theoretically any indirect effect from highway capital is the net of two offsetting effects -- the shift of economic activity discussed in, e.g., Forkenbrock and Foster (1990) and the spillover economic benefits hypothesized by Munnell (1992). Given that, the sign of an indirect effect depends on both the magnitude of the two offsetting effects and the geographic scale of the data. This paper uses county data for California, providing considerably more geographic detail than most previous production function research. The empirical work that follows tests both the for the existence of direct and indirect economic effects from street and highway capital and for the signs of those effects.

III. An Empirical Test

A. Background

For comparison with recent production function research, the empirical test is conducted in the context of an aggregate production function. A county production function is modified to include both the street and highway capital stock within each county, and the street and highway capital stock in other counties, as shown below.

$$Q = f(L, K, H, H_o) \quad (4)$$

where Q = county output

L = county employment

K = private capital stock in the county

H = street and highway capital stock in the county

H_o = street and highway capital stock in other counties

As an example, if highways contribute to output partially by drawing output away from other areas, the coefficient on H should be positive in a specification based on (4), while the coefficient on H_o would be negative. The key is defining other counties (i.e. H_o) in a sensible way. I follow Holtz-Eakin and Schwartz and define H_o as the sum of the street and highway capital in neighboring counties. This assumes that street and highway capital redistributes economic activity across county borders. While that is not the only possibility, it is intuitive, allows some comparison to prior research, and is in the spirit of the discussion in Forkenbrock and Foster (1990). Since the goal here is primarily to test the hypothesis that direct and indirect

economic effects exist, rather than to give an exhaustive examination of possible definitions of H_o , the empirical test will examine indirect effects between neighbor counties.

B. Data

Data for California counties from 1969 through 1988 are used to estimate the parameters of a log-linear form of (4). Specifically, data are available on gross county product, employment, private sector capital stock, and street plus highway capital stocks. Gross county product is derived by apportioning state product to counties based on total county personal income, which is consistent with the methodology used by the Southern California Association of Governments to estimate county product within their region. Private capital stock is constructed by apportioning Munnell's estimates of California private capital to counties.⁷ The apportioning methodology is the same as that used in Munnell (1990), which in turn follows Da Silva Costa, Elson, and Martin (1987). Highway and street capital stock is constructed using a perpetual inventory method based on annual highway and street expenditures, in each county, starting in 1957. Employment in each county is available from the Census Bureau's *County Business Patterns* for each year. See Boarnet (1995, Appendix A) for a detailed description of the data sources and the methods used to construct the county product, private capital, and highway and street capital variables.

The transportation capital stock variables, H and H_o , include both state highway and local street capital. This differs from Holtz-Eakin and Schwartz (1995), who included only state

⁷ I thank Douglas Holtz-Eakin and Alicia Munnell for providing the state private capital data.

highway capital, and it also differs from studies of highway corridors such as the work of Forkenbrock and Foster (1990). The reasons for including all ground transportation infrastructure are several. First, since the geographic scale of this study is considerably smaller than that of Holtz-Eakin and Schwartz (1995), one cannot as easily assume that indirect effects are due mostly to the provision of highway infrastructure. The possibility that cross-county travel can be facilitated by local streets should also be allowed in the empirical specification. Second, indirect effects need not be due strictly to cross-county travel. The productive benefits of transportation capital could be due to facilitating within-county travel. Again, that suggests that street capital should be included in the test. Third, the purpose here is to simply test the hypothesis that indirect effects exist. Given empirical support for that hypothesis, it is an appropriate topic for future work to examine how much of any direct and indirect effect can be attributed to state highway stocks versus the amount due to local street and road infrastructure.

A preliminary analysis of the data revealed that in all but three counties output was larger than the value of the stock of street and highway capital in 1988. The three counties where the value of street and highway stocks exceeded output were all sparsely populated rural counties which, due to their geographic size, had large transportation capital stocks but very little economic activity.⁸ Since these counties are outliers when compared with the rest of the sample, they were dropped from the analysis that follows.⁹ The empirical work that follows is based on

⁸ Those counties were Alpine, Mono, and Sierra. Mono spans a large portion of the Mohave Desert near the Nevada border, while Alpine and Sierra are in sparsely populated areas of the Sierra mountains. In 1988, Alpine County had a population of 1,100, Mono County had 9,100 persons, and Sierra County had a population of 3,300.

⁹ The idea that Alpine, Mono, and Sierra counties are outliers is supported both by examining a plot of output per employee versus street and highway capital per employee, and by a Chow test which suggests a structural break in the parameters for the outliers when compared with the rest of the sample. The regression in equation (6), below, was fit on the 3 outlier counties and the other 55 counties. The Chow test statistic for parameter stability across the

panel data for the remaining 55 counties from 1969 through 1988. Descriptive statistics for the data are shown in Table 1.

C. Estimation and Results

As mentioned earlier, past production function studies of states has established the importance of controlling for unique state effects (Evans and Karras 1994; Garcia-Mila, McGuire, and Porter 1996; Holtz-Eakin 1994; Kelejian and Robinson 1994). Assuming a log-linear Cobb-Douglas specification for equation (4) gives the regression shown below.

$$\log(Q_{c,t}) = \beta_1 \log(L_{c,t}) + \beta_2 \log(K_{c,t}) + \beta_3 \log(H_{c,t}) + \beta_4 \log(H_{o,c,t}) + f_c + y_t + \varepsilon_{c,t} \quad (5)$$

where Q, L, K, H, and H_o are as defined in equation (4)
 f = a vector of county-specific variables
 y = a vector of year-specific intercepts
 ε = an i.i.d. error term¹⁰

"c" indexes counties; "t" indexes years

two sub-samples is 19.36, which is distributed as F_{23,1114}. The hypothesis of coefficient stability across the two groups of counties is rejected at better than the 1% level. Given that, it is appropriate either to drop the three outlier counties or to parameterize or otherwise control for how those counties differ from the rest of the sample. In other work (Boarnet 1996), the outliers are included in the analysis by controlling for how those counties differ from the rest of the sample. That is done by examining indirect effects across counties of similar population density and per capita income. The result from that research is consistent with the work presented here. Both techniques support the hypothesis of negative indirect street and highway effects.

¹⁰ One could also model a first order spatially correlated error. In matrix notation, this would assume that, for any year, $\varepsilon = \rho \mathbf{W} \varepsilon + u$, with u being a classical spherical error term. Regressions were tested with \mathbf{W} defined such that $w_{i,j} = 1$ if counties "i" and "j" border, 0 otherwise, which is the same \mathbf{W} matrix that is used in the empirical work below. Specifying such a spatially autocorrelated error structure does not substantively change any of the regression results reported in this paper. Maximum likelihood estimates for the model with a first order spatially correlated error are available upon request from the author.

Note that the variables f_c control for any unique, time-invariant county characteristics which affect output but which are not otherwise measured with the data. The dummy variables y_t allow for common time effects, and thus control for the macroeconomy.

For any county, "i", the variable H_o is implemented as $\sum_j w_{i,j} H_j$, where $w_{i,j}$ are weights. The weights are defined such that $w_{i,j}$ equals one if counties "i" and "j" share a border; otherwise $w_{i,j}$ equals zero. Thus for any county "i", the street plus highway capital in other counties (H_o) is the sum of street plus highway capital in all counties that border "i".¹¹ Given this definition for H_o , the regression in equation (5) can be written in matrix notation as

$$\log(Q_t) = \beta_1 \log(L_t) + \beta_2 \log(K_t) + \beta_3 \log(H_t) + \beta_4 \log(WH_t) + f + y_t + \varepsilon_t \quad (6)$$

where $W = a (55 \times 55)$ matrix with elements $w_{i,j} = 1$ if counties "i" and "j" share a border, otherwise $w_{i,j} = 0$
 t subscripts indicate years
 all variables are column vectors of observations for the 55 counties for a given year

The definition of H_o in equation (6) differs slightly from the one used by Holtz-Eakin and Schwartz (1995). As mentioned earlier, they defined an effective highway capital equal to $(\mathbf{I} - \delta \mathbf{W})^{-1} \mathbf{H}$, where \mathbf{I} is an identity matrix and \mathbf{W} is a matrix that defines how states (in the case of their study) neighbor each other. The Holtz-Eakin and Schwartz (1995) formulation allows highway capital in one state to effect immediate neighbors in a "first-round effect", and then effect the neighbors of those immediate neighbors in a "second-round" effect, and so on. The

¹¹ While the idea of summing neighbors' street and highway capital is intuitive, one might also wish to take a weighted sum. One possibility is to weight the $w_{i,j}$ by the inverse of the number of neighbors, such that the variable H_o is the average of street and highway capital in adjacent counties. Using that definition for H_o does not change the results of any regressions reported in this paper.

parameter δ measures how the neighbor relationship decays from the "first round" or immediate neighbors, to neighbors of immediate neighbors, and so on. The use of the variable **WH** in equation (6) restricts attention to only immediate (or "first round") neighbors.

Either definition can test for indirect effects from street and highway capital. Attention here is restricted to first-round neighbor effects both because that allows a specification that is linear in the parameters and because focusing only on immediate neighbors is a more conservative test of indirect effects, since any higher order neighbor effects are not measured.

The unique county variables, f_c , can either be treated as part of the intercept (fixed effects estimation) or part of the error term (random effects estimation). Since random effects assumes that the f_c are part of the error term, that requires that the f_c be orthogonal to the other independent variables. The Hausman test statistic for fixed versus random effects for the specification in equation (6) is 18.82, which follows a chi-squared distribution with four degrees of freedom. At better than the 1% level, this rejects the hypothesis that the county effects are orthogonal to the other independent variables, suggesting that fixed effects estimation should be used.

Fixed effects coefficient estimates for (6) are shown in Table 2. The coefficients on all four inputs are statistically significant at the 1% level. The coefficients on labor and private capital are 0.62 and 0.20 respectively, which is consistent with the range of magnitudes obtained in other production function studies (e.g. Aaron 1990; Garcia-Mila, McGuire, and Porter 1996; Holtz-Eakin 1994). The coefficient on own street and highway capital (**H**) is significantly positive, and the coefficient on neighbor counties' street and highway capital (**WH**) is

significantly negative. This supports the hypothesis of opposing direct and indirect economic effects from transportation infrastructure, and suggests that some of the output increases associated with street and highway capital are a redistribution of activity from neighboring counties.

Garcia-Mila and McGuire (1992) and Munnell (1992) have both noted that fixed effects estimation identifies parameters based on short-run year-to-year fluctuations in the data. As such, the technique does not capture any long-run relationship between public capital and output. To avoid this problem, Holtz-Eakin and Schwartz (1995) suggest transforming the specification in (6) into what they call long differences.

Standard first differences specifications can be obtained by subtracting from the equation for year "t" the equation for the previous year, "t-1". While that eliminates the time invariant fixed effect (the f_c), it still identifies parameters based only on short-term year-to-year fluctuations. To overcome that problem, Holtz-Eakin and Schwartz suggest transforming the data by differencing over several years. Adapting that approach, consider subtracting from the equation for year "t" the equation for ten years earlier, "t-10". For example, subtract the equation for 1969 from the equation for 1979. The differences are now ten year changes, which should be sufficient to capture long-run relationships between county output, own county street and highway capital, and neighbor counties' street and highway capital.¹² Similarly, one could express

¹² Research on lagged adjustment models of urban economies gives justification for using ten year differences. Carlino and Mills (1987) estimated a lagged adjustment model of population and employment growth in counties. They found that, for both county population and county employment, approximately 15% of the gap between actual and equilibrium values was closed in ten years. Grubb (1982) and Luce (1994) estimated similar models for cities. They found adjustment speeds which implied that the cities in their samples closed between 30% and 100% of the gap between actual and equilibrium values in ten years. These results suggest that ten years is a sufficient time period to capture a reasonable amount of long run change.

equation (6) as the difference between 1980 and 1970 values. Given the available data, ten such differenced specifications can be obtained -- the difference between 1969 and 1979, between 1970 and 1980, and so on up to and including the difference between 1978 and 1988 values.

Furthermore, given that the regression coefficients in (6) are assumed to be time invariant, one can pool each of the ten equations. This gives an equation with 550 observations, with each observation based on ten year differences in the levels of the variables.¹³

Expressing the data in differences rather than levels has the added advantage of ameliorating any spurious correlations due to unit roots in the series. Previous analysis of national time series data has suggested that the relationship between output and public capital might be a spurious one due to common trends in the levels of the variables (Tatom 1991). Statistical tests for such problems usually require longer time series than are available here.¹⁴ Given that, the most cautious approach is to estimate a differenced specification. Taking ten-year differences transforms the specification into differences while preserving the long-run relationship between the variables.

The results of estimating equation (6) in ten year differences, while pooling all observations, are shown in Table 3. The results are essentially the same as in Table 2. The coefficients on labor and private capital are significantly positive. The coefficient on own street

¹³ Note that pooling differences across ten different ten-year time periods avoids the serial correlation problem discussed in Holtz-Eakin and Schwartz (1995, p. 463).

¹⁴ Davidson and MacKinnon (1993, chapter 20) note that the small sample properties of tests for non-stationarity and spurious correlation rely on restrictive assumptions, including the assumption of serially uncorrelated errors in the test regressions, which are not likely to hold in practice. Thus the tests are most reliable when the asymptotic properties, which depend on the length of the time series, can be used.

and highway capital is significantly positive and the coefficient on neighbors' street and highway capital is significantly negative, again suggesting opposing direct and indirect output effects.

Absent a full structural model, there are three reasons why the associations in Tables 2 and 3 more likely show the effect of street and highway infrastructure on output rather than the reverse causal link. First, the fixed county effects control for any time invariant characteristics which might cause more wealthy counties to either obtain more highway funding or spend more local money on roads. Second, using the vector autoregression techniques described in Holtz-Eakin, Newey, and Rosen (1986), changes in highway plus street capital stocks were regressed on lagged changes of both highway plus street capital stocks and county output. The null hypothesis that the coefficients on output equaled zero could not be rejected in regressions with two, three, four, or five lags, suggesting that the primary channel of causality does not flow from county output to highway capital stocks.¹⁵ Third, the indirect effect (the negative coefficient on neighbor counties' street and highway capital) argues that causality is from transportation infrastructure to output. That is consistent with transportation infrastructure investments which shift economic activity from place to place. The reverse causal link, namely that higher county output reduces street and highway infrastructure stocks in neighboring counties, does not have an equally compelling explanation.

¹⁵ Only regressions with two, three, four, and five lags were tested. The test statistic follows a chi-squared distribution. For five lags, the statistic is 0.0032 with 5 degrees of freedom. For four lags, the statistic is 0.0026 with 4 degrees of freedom. For three lags, the statistic is 0.0039 with 3 degrees of freedom. For two lags, the statistic is 0.0011 with 2 degrees of freedom.

Section IV: Interpretation

Given the magnitudes on own and neighbor county street and highway capital in Tables 2 and 3, one might be tempted to conclude that the direct and indirect effects are of equal and opposing magnitude. Yet there are two reasons why one should be skeptical of such a conclusion. First, note that there are several possible ways to define the w_{ij} . If the concern is to measure correlation between particular neighbors, the magnitude of the non-zero elements of \mathbf{W} is not of vital importance. An argument based on coefficient magnitudes, on the other hand, requires that the magnitudes of the weights be correct. Since it is difficult to determine *a priori* the proper magnitudes for the w_{ij} , the cautious approach is not to read too much into the magnitudes of the coefficients on own county and neighbor counties' street and highway capital.

Second, if transportation infrastructure is productive for counties (as the positive coefficient on own county street and highway capital suggests), then theoretically the direct and indirect effects ought not perfectly cancel. A contrived example can help clarify this point. Suppose that a county, call it "A", gets an increase in transportation infrastructure. Further suppose that the new infrastructure enhances the returns to private factors of production in County A. Some firms might then move to County A from other counties in order to benefit from the more productive environment in County A. Even if all increased output in A is due to the production of firms which moved into the county, the direct effect of increased output in A must be larger than the indirect effect of decreased output in the other counties where the migrant firms were previously located. That's because each firm that moved into County A is more productive, and thus can produce more output (given the same private inputs) than they could

have outside of A. After all, the increased productivity in County A is what caused the firms to move in the first place.¹⁶

Overall, theory suggests that if street and highway capital is productive for counties, the net of the direct and indirect effects ought to be greater than zero sum. Thus the results in this work are in conflict with the recent state-level studies which found that public infrastructure, transportation infrastructure included, is not productive (e.g. Evans and Karras 1994; Garcia-Mila, McGuire, and Porter 1996; Holtz-Eakin 1994; Kelejian and Robinson 1994). It is not likely that the difference in findings is due to econometric specification, since the specifications used here adopt the same techniques used in the recent state-level studies.

While a full reconciliation of this work with the results from state studies is a topic for future research, one possibility will be mentioned here. If the coefficient on public capital is biased downward due to noisy data, it is possible that state studies give elasticities that are insignificantly different from zero when public capital actually has a small marginal effect on output or productivity. At the state level, the direct and indirect county effects partially cancel. Thus the coefficient on state infrastructure is the net of any within-state direct and indirect effects, and ought to be smaller than the direct (county) effect measured in this study. Equivalently, with county data, the coefficient on own county transportation infrastructure ought to be larger than comparable estimates from state studies. Given that, even with noisy data, it might be possible to estimate statistically significant transportation infrastructure

¹⁶ For a more formal discussion of this, see Boarnet (1996).

elasticities with county data using techniques that do not yield significantly positive elasticities with state data.

Section V: Policy Recommendations

Despite the fact that the evidence here is consistent with productive street and highway capital, the recent skepticism about using public infrastructure as an engine of economic growth (e.g. Holtz-Eakin 1993; Krol 1995) is well founded. Even if transportation infrastructure is productive for counties (and a full reconciliation of this research with previous studies awaits future work), policy-makers ought to be cognizant of the possibility of economic losses outside the immediate project area. If studies of particular corridors measure only direct economic effects, those studies might overestimate the total economic gains from a project, as Forkenbrock and Foster (1990) suggested.

This raises the question of how to consider economic impacts in highway project analysis. Determining the magnitude of even the direct (or nearby) economic impact has always been difficult, since it is hard to control for the *ceteris paribus* condition of what would have happened had the highway not been built. The lesson here is that determining economic impacts is even more difficult than corridor studies or project analysis would suggest. Even if the *ceteris paribus* condition can be enforced, one must account for both direct and indirect economic effects.

This places a considerable burden on project analysis, both since it requires that the study consider impacts in places remote from the project and because it might not be *a priori* obvious

where to look for indirect effects. While this paper gives evidence of indirect economic effects across neighboring counties, other locational patterns are also possible. Overall, economic impacts generated from project analyses that do not attempt to measure indirect effects must be viewed with caution. Future research and practice should consider more carefully how to measure both the direct and indirect economic effects of transportation infrastructure.

Until that is done, some caution is required when interpreting evidence of street or highway economic benefits. Since most studies of transportation projects are only designed to illuminate the direct, or nearby, effects, policy-makers should be aware that investments that appear to bring economic gains might also generate economic losses in areas remote from the project.

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Table 1: Descriptive Statistics

Variable	Mean	Standard Deviation	Minimum	Maximum
<i>Levels</i>				
Q: County Output	7.38	1.68	4.16	11.98
L: Employment	10.09	1.83	6.68	15.10
K: Private Capital	7.07	1.59	3.86	11.63
H: Own County Street and Highway Capital	5.87	1.13	4.02	9.54
WH: Neighbor Counties' Street and Highway Capital	7.68	0.90	5.84	9.94
<i>Ten Year Differences</i>				
Q: County Output	0.40	0.22	-0.66	1.16
L: Employment	0.47	0.20	-0.13	1.10
K: Private Capital	0.43	0.24	-0.63	1.29
H: Own County Street and Highway Capital	0.13	0.12	-0.15	0.51
WH: Neighbor Counties' Street and Highway Capital	0.13	0.08	-0.04	0.40

Note: Output, private capital, and street and highway capital are in logs of millions of dollars. Employment is in logs.

Table 2: Fixed Effects Regression Results
 Dependent Variable = log(county output)

independent variable	
Labor (L)	0.62 (0.025)
Private Capital (K)	0.20 (0.020)
Own County Street and Highway Capital (H)	0.16 (0.040)
Neighbor Counties' Street and Highway Capital (WH)	-0.21 (0.059)

Note: All variables are in logs. Standard errors are in parentheses below coefficients. Coefficients on year dummy variables not shown. Number of observations = 1100.

Table 3: Regression Results, Ten Year Differences
 Dependent Variable = $\log(\text{county output})$

independent variable	
Labor (L)	0.69 (0.036)
Private Capital (K)	0.18 (0.030)
Own County Street and Highway Capital (H)	0.22 (0.056)
Neighbor Counties' Street and Highway Capital (WH)	-0.23 (0.08)

Note: All variables are ten-year differences of logs. Standard errors are in parentheses below coefficients. Coefficients on year dummy variables not shown. Number of observations = 550.