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The University of California Transportation Center

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Transportation Infrastructure, Economic Productivity, and Geographic Scale: Aggregate Growth versus Spatial Redistribution

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Abstract

Recent cross-state studies of public infrastructure suggest that infrastructure is not economically productive. Yet it is possible that public capital influences economic activity largely by shifting that activity from one location to another. If that is the case, infrastructure can be productive at small geographic scales but not productive over large areas. This paper tests that hypothesis with a production function study of highway and road capital in California counties for the years 1969 through 1988. The results show that county output is positively associated with highway capital in the county, but negatively associated with highway capital in neighboring counties. This suggests that the productive effects of highway capital are largely a shift in economic activity from one county to another.

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I. Introduction

Prompted initially by the work of Aschauer (1989), economists have given increasing attention to the link between public capital and the private sector economy. Yet Aschauer's work has met with a raft of criticism, summarized most recently by Gramlich (1994). While several different critiques have been offered, one of the most damning is the evidence from cross-sectional studies on U.S. states. Recent panel studies that control for unique state characteristics (Holtz-Eakin, 1994; Kelejian and Robinson, 1994) show no effect from public capital on private sector productivity or output. Holtz-Eakin (1994) concludes that public infrastructure does not boost private-sector economic output at the margin. While this result is intuitive, especially in a nation with a well developed infrastructure stock, there is another explanation that is consistent with the panel data results, but which has different policy implications.

It is possible that public capital is productive at a geographic scale that is smaller than states. Public capital might boost private sector productivity or output largely by moving economic activity from one location to another nearby location. If that is the case, public capital would appear productive at small geographic scales (e.g. metropolitan areas or cities), but would appear unproductive at larger geographic scales (e.g. states.)

The hypothesis stated above, namely that infrastructure is productive at sub-state levels, is based on the idea that the primary effect of public capital is to give one local area an advantage over other local areas in the same state. The hypothesized local advantage is largely zero-sum at a state level, but can be very real for local areas.

This hypothesis will be tested for the case of highway and road infrastructure. Using a panel of California counties, this paper shows that output in a county is larger, ceteris paribus, when that county's own stock of highway and road capital is larger, but county output is smaller when the stock of highway and road capital in neighboring counties is larger. This suggests not only that highway and road capital is productive at the county level, but also that the output gains are at the expense of neighboring counties. This is consistent both with the common political perception that good highways and streets lead to local economic growth and with evidence that public infrastructure is not productive at the level of states.

II. Background

The literature on infrastructure and economic development can be loosely divided into two groups -- the studies that use time series data, usually for the United States in post-World War II years (e.g. Aschauer 1989; Munnell 1990b) and the studies that use panel data for U.S. states (e.g. Munnell 1990a; Garcia-Mila and McGuire 1992; Holtz-Eakin 1994; Kelejian and Robinson 1994). While the distinction is not perfect, it is often the case that the time series studies find large and significantly positive elasticities of output or productivity with respect to public infrastructure (Aschauer 1989; Munnell 1990b), while the panel studies find much smaller elasticities.

The panel studies done by Garcia-Mila and McGuire (1992) and Munnell (1990a) found significantly positive elasticities for public capital, although those elasticities were typically smaller than the estimates from time series studies. Neither Garcia-Mila and McGuire (1992) nor Munnell (1990a) corrected for unobserved state heterogeneity. Holtz-Eakin (1994) provides a compelling argument for controlling for cross-state heterogeneity, and when the appropriate econometric techniques are used, the elasticity of output with respect to public capital is not statistically different from zero (Holtz-Eakin 1994; Kelejian and Robinson 1994).

Two interpretations of these panel results have been offered in the literature. The first, which pre-dates the work of Holtz-Eakin (1994) and Kelejian and Robinson (1994), notes that the estimated magnitude of the public infrastructure elasticity generally gets smaller when moving from national to state data. One possible explanation, articulated by Munnell (1992), is that the benefits of infrastructure spill over across state borders, so that state-level studies do not measure the full effect of public capital.

A second interpretation, offered by Holtz-Eakin (1994) and Kelejian and Robinson (1994), among others, suggests that the cross-state studies are more reliable than time series studies, especially when the cross-state studies control for unobserved heterogeneity. Those authors conclude that the elasticity of output with respect to public capital is close to zero. This reasoning is consistent with three other pieces of evidence. First, Tatom (1991) suggested that the time series evidence was driven by spurrious correlations due to non-stationary data. When he corrected for non-stationarity by first differencing, the estimated elasticity of public capital was not significantly different from zero (Tatom 1991). Second, an interpretation of zero (or negligibly small) public capital elasticities is consistent with the work of Hulten and Schwab (1984), who used growth accounting to examine productivity differences between U.S. regions. Third, Holtz-Eakin and Schwartz (1994) recently tested the hypothesis that public infrastructure creates economic benefits that spill over across state boundaries, and they found no evidence to support that hypothesis.

Overall, the most reasonable interpretation of past studies is that the time series evidence is suspect due to problems with non-stationarity, and the cross-sectional evidence suggests small and probably zero elasticities from public capital. This is consistent either with no economic effects from public capital, or with effects which redistribute economic activity within a state. If public capital is productive, current evidence suggests that it must be productive by giving some places a local competitive advantage over other, possibly nearby, places. Thus any productivity effect from public capital would be apparent at small geographic scales, but would on net be zero for larger units of observation.

This paper tests that idea by following much previous work and estimating a production function that includes a measure of public infrastructure. The innovation here is that the data are on counties. This allows an examination of the intra-state effects of public capital.

The empirical work presented here focuses only on highway and road capital. There are several good reasons to restrict the analysis to highways and roads.

First, street and highway capital was often one of the major components of "core infrastructure" in previous work (e.g. Aschauer, 1989; Garcia-Mila and McGuire, 1992; Holtz-Eakin, 1994). Those authors hypothesized that if any public capital was a useful input for private-sector production, highway infrastructure would be a prime candidate for producing such economic benefits.[1]

Second, economic development is often used as a justification for major highway projects. Despite the ambiguous empirical evidence on this matter, 36 states considered economic development as a factor in highway project evaluation as of 1987 (Stephanades and Eagle, 1987.) For policy reasons, a test of the productive effects of highway infrastructure is useful even absent the existing debate in the economic literature.

Third, there is evidence that employment growth clusters near highways (Boarnet, 1994). Given that, and the finding that public capital (highways included) are not productive at the state level, this raises the possibility that the economic gains observed near highways come at the expense of other areas.

Fourth, a reasonable view of the economic benefits of highway and road infrastructure suggests that those benefits will shift economic activity from one location to another, rather than increasing aggregate economic activity. If highways facilitate travel and goods movement between previously poorly connected cities, then those highways can increase trade between cities. Highways that are built when the transportation system is poorly developed can help change a system of somewhat closed city or regional economies into trading partners, facilitating aggregate economic gains. While this might be a reasonable model of the initial stages of highway building in the U.S., it is unlikely that modern highway projects offer any meaningful improvement in inter-city transportation.

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[1] Garcia-Mila and McGuire (1992) separate highway infrastructure from other public capital, and Holtz-Eakin (1994) reports that his results do not change when only highway infrastructure is included in the public capital variable. Gramlich (1994) notes that most of the reduction in public capital investment since 1970 is due to lower state and local investments in highways and education. Thus even studies that do not disaggregate the public capital variable report results that are likely driven in large part by highway capital investment.

Instead, modern projects often have the largest impact on a nearby area, possibly reducing congestion or allowing increased traffic volumes in the immediate area served by the highway. Thus highways often increase accessibility within a region, facilitating the movement of workers, goods, and information within the possibly small geographic area immediately served by the highway. The geographic scale of economic benefits from highways, if there are any, would be somewhat small. The same is likely even more true of local street and roadway capital. If firms make location decisions from a short-list within the same metropolitan area or sub-state region, highway and road projects that improve access in one locale might increase economic activity largely by drawing it away from another nearby locale.

That is the hypothesis tested in this paper. That hypothesis predicts that highway and road infrastructure will be shown to be productive for observations that are smaller than states (counties, in this case) when using the same econometric techniques that found that public capital was not productive for states and larger regions.

III. Data and Study Area

This study uses data on gross county product, employment, private capital stock, and highway and street capital stock for all California counties from 1969 through 1988. The data sources and the construction of the gross product, private capital, and highway capital variables are described in Appendix A. All variables except employment are measured in 1982 dollars.

Figure 1 shows the time series of California gross product per worker and highway plus street capital per worker during the study period. Output per worker varies as expected with the business cycle, and only becomes larger than the 1969 value in 1987. Highway and street capital per worker declines during the study period.[2]

Since the time series starts in 1969, it misses the rapid infrastructure buildup, and contemporaneous productivity growth, which likely drive the results of many national time series studies (e.g. Aschauer, 1989; Munnell 1990b). This can be an advantage, since any spurious correlation created by a common post-World War II trend in the time series variables could possibly be less severe with only the last half of the post-World War II cycle represented in the data. More importantly, with panel data one can use cross-sectional variation to add information not available in the time series trends.

In preliminary data analysis, it became clear that some rural counties have a large stock of highway and street capital given their population and economic activity. In particular, there are three counties whose highway and street capital exceeded their gross product in 1988. Those are Alpine, Mono, and Sierra, which are all sparsely populated counties near the Nevada border. (In 1988, Alpine had 1,100 residents, Mono had 9,100 residents, and Sierra had 3,300 residents.) Since all three counties are outliers when compared with the rest of the dataset, they are excluded from the analysis that follows. This leaves observations on 55 counties for 20 years.

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[2] The total stock of highway and street capital in California increased during the study period. Measured in constant 1982 dollars, California highway and street capital was $34.8 billion in 1969 and $48.9 billion in 1988.

IV. County Production Functions

County gross product is modeled as a function of labor inputs, private capital inputs, and highway capital stock. The basic model follows much previous work (e.g. Aschauer 1989; Holtz-Eakin 1994; Munnell 1990a and 1990b) by using a Cobb-Douglas production function, shown below in log-linear form.

$$\log(GCP_{i,t}) = \alpha_0 + \alpha_1\log(L_{i,t}) + \alpha_2\log(K_{i,t}) + \alpha_3\log(H_{i,t}) + u_{i,t} \qquad (1)$$

where GCP = gross county product
L = labor inputs (employment) in the county
K = private capital stock
H = highway and street capital stock

"i" indexes counties, "t" indexes time

Previous research has shown that the results of a production function such as (1) are sensitive to the assumptions made about the error term, $u_{i,t}$ (Holtz-Eakin, 1994; Kelejian and Robinson, 1994). In a panel data setting, the most obvious difficulties are controlling for unobservable differences across the observations and common time effects. The regressions shown in Table 1 control for time and county effects in a variety of ways.

The regression in column (1) is simply OLS applied to equation (1). Column (2) adds a linear time trend to the basic model in equation (1), and column (3) replaces the linear time trend with year dummy variables. The results do not appreciably differ from column (1).

The regressions in columns (1) through (3) are comparable to the work in Garcia-Mila and McGuire (1992) and Munnell (1990a). Unlike those studies, this model gives a point estimate for the elasticity of output with respect to public capital which is not significantly different from zero.

Column (4) controls for state fixed effects by estimating equation (1) in first differences. Highway plus street capital is significantly positive, although the estimated elasticity is implausibly large when compared with the coefficient on private capital. Columns (5) and (6) add time effects to the first differences model. The results suggest that highway capital in column (4) partially proxies for the omitted time trends. Highway plus street capital is statistically significant in column (6), which uses the more flexible representation of time allowed by year dummy variables.

One potential difficulty with the first differences model in columns (4) through (6) is that the parameter estimates are identified by the time series variation for particular counties, rather than fully exploiting the cross-sectional variation. Random effects estimation uses more of the cross-sectional information, but requires the restrictive assumption that county effects are uncorrelated with all independent variables. For comparison, columns (7) through (10) show both fixed and random effects models that correspond to equation (1). Since the random effects models use more of the cross-sectional variation, they will be preferred, but only if the orthogonality assumption for the county effects can be justified by a Hausman test. Columns (7) and (8) include no year effects, while columns (9) and (10) include year dummy variables.

There are few differences between the estimated coefficients in columns (7) through (10). In fact, the Hausman test does not reject the assumption of orthogonal county effects in the models with or without time controls.[3] This also suggests that the coefficient vectors do not vary appreciable when going from fixed to random effects estimation.

All variables are statistically significant in the models in columns (7) through (10), the elasticity of highway plus street capital is never larger than the elasticity of private capital, and the coefficients sum to very close to one, suggesting constant returns across the private and public inputs.[4] Thus the model appears to perform well and suggests that highway plus street capital is productive at the county level.

V. The Effect of Highway and Street Capital on Neighboring Counties

If highways and roads are productive largely by shifting economic activity from one area to another in a state, highway investments in a county ought to not only create increases in output in that county, but also economic losses elsewhere. This can be tested, as long as one makes assumptions about where to look for the losses in output. A good first guess is that highway and street investment might reduce output in neighboring counties. This assumes that the growth associated with highway and street capital comes at the expense of growth that would have occurred in neighboring counties. While, in general, the alternative location for firm location or expansion could be anywhere, assuming that growth occurs at the

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[3] The Hausman statistic shown in column (8) tests the null hypothesis that the county effects in column (8) are uncorrelated with the independent variables, and thus tests the random effects model in column (8) versus the fixed effects model in column (7). The Hausman statistic shown in column (10) tests the validity of the random effects model in column (10) versus the fixed effects model in column (9).

[4] A Wald test of the hypothesis that the coefficients on ln(L), ln(K), and ln(H) sum to one cannot be reject for any of columns 7 through 10.

expense of neighbors is as good of an assumption as any, and allows a more powerful test of the idea that highway infrastructure is productive largely by moving economic activity from one place to another.

The production function in Equation (1) can be generalized to include a measure of highway and street infrastructure in neighboring counties, as shown below.

$$\log(GCP_{i,t}) = \alpha_0 + \alpha_1\log(L_{i,t}) + \alpha_2\log(K_{i,t}) + \alpha_3\log(H_{i,t}) + \alpha_4\log(\mathbf{W}^*H_{i,t}) + u_{i,t} \qquad (2)$$

where **W** is a 55x55 neighbor matrix with elements $w_{i,j}$
$w_{i,j} = 1$ if counties "i" and "j" are contiguous, 0 otherwise
$w_{i,i} = 0$

The variable **W***H is the first-order spatial lag of highway and street capital.[5] It is the sum of highway and street capital in all neighboring counties. In keeping with the hypothesis that highway plus street capital is productive largely by shifting economic activity from one county to another, we expect α_3 to be significantly positive and α_4 to be significantly negative.

Table 2 presents the results of estimating equation (2) using different fixed and random effects models. Notice that the coefficient on highway and street capital is significantly positive in all specifications except column (2) (first differences with a linear time trend), and the magnitude of the coefficient on log(H) is similar to what was obtained in Table 1.

The coefficient on log(**W***H) is significantly positive in column (1) and significantly negative in column (6). The model in column (1) is not preferred, since it includes no

[5] For a discussion of spatially lagged variables, see, e.g., Anselin (1988), chapter 3.

controls for time effects. The differences between columns (1) and (3), and also between the same specifications in Table 1 (columns (4) and (6) in Table 1) suggest that the results are influenced by omitting the time effects.

The models in columns (4) through (7) in Table 2 show the results of estimating equation (2) with standard fixed effects (deviations from means) and random effects. The specifications in columns (6) and (7) are preferred, since they include year dummy variables. In column (6), fixed effects with year dummy variables, the coefficient on log(W*H) is significantly negative. Furthermore, the Hausman test reported at the bottom of column (7) rejects the hypothesis that the county effects are uncorrelated with the error term. This suggests that fixed effects must be used, since random effects estimation can give inconsistent results.

In column (6), the coefficient on log(W*H) has approximately the same magnitude as the coefficient on log(H). Furthermore, the coefficients on log(L), log(K), and log(H) in column (6) sum to 0.98, suggesting very close to constant returns to scale over the public and private inputs within a county.[6] In short, in column (6), highway and street capital appears to be productive within a county, but those gains seem to be offset by losses created in neighboring counties.

VI. Other Econometric Issues

Before interpreting these results, we must discuss several possible econometric

[6] A Wald test cannot reject the hypothesis that the coefficients on ln(L), ln(K), and ln(H) sum to one in column 6 of Table 2.

difficulties. The first issue is the possibility of spurrious correlations due to common trends in the data. Since the time series is relatively short (only twenty years), most tests for non-stationarity will have questionable reliability.[7] In that situation, the most reliable solution is often to first difference the data, on the assumption that the series for some variables might not be stationary.

Given that, it is encouraging that the coefficient on log(H) is significantly positive in column (6) of Table 1 (first differences with year dummy variables). Yet it is disappointing that the coefficient on log(W*H) is not significant in the comparable specification in Table 2 (column 3).

As other authors (e.g. Holtz-Eakin 1994; Munnell 1992) have noted, first differencing obscures any long-run relationship between the variables. For that reason, Holtz-Eakin (1994) suggests using "long differences" over several years to provide more long run information.

Table 3 shows the results of fitting equation (2) in first differences using time periods that are longer than one year. In column (1), all variables are expressed as the difference between their 1988 and their 1969 values. The variables for highway and street capital in the county, log(H), and in neighboring counties, log(W*H), are both insignificant in that specification. Since the regression in column (1) has only 55 observations, there is some doubt about whether there are sufficient degrees of freedom to get reliable hypothesis tests.

For that reason, column (2) shows the result of fitting equation (2) in first differences,

[7] Davidson and MacKinnon (1993, chapter 20) note that the small sample properties of most tests for non-stationarity 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 often most reliable when the asymptotic properties, which depend on the length of the time series, can be used.

with differences taken at five year intervals.[8] Column (3) in Table 3 shows first differences on equation (2) over four year intervals.[9] Both columns (2) and (3) are attempts to keep long-run information by differencing over more than one year, while allowing more degrees of freedom than column (1).

In column (2), the coefficient on log(W*H) is significantly negative (at the 5% level) and the coefficient on log(H) is positive but not significant. In column (3), that pattern is reversed, with the coefficient on log(H) significantly positive while the coefficient on log(W*H) is negative and significant at only the 10% level. Taken together, columns (2) and (3) provide additional support for the hypothesis that highway and street capital is productive at the county level by shifting output from neighboring counties.

Another potential problem is heteroskedastic or serially correlated errors in equations (1) and (2). There are three reasons why the error structure might be non-spherical. First, the California counties included in the regression are very different in size, ranging from Modoc County, which had 9,200 residents and a gross product of $126 million in 1988 to Los Angeles County, which had 8,639,300 residents and a gross product of $160 billion in 1988. This could lead to heteroskedastic errors based on county size. Second, one might expect the error structure to be positively serially correlated or, if positive serial correlation is not a problem, differencing can induce negative serial correlation. Third, the error term in equations (1) and (2) could be spatially correlated. Since heteroskedasticity could be due to one or more of three processes, the most straightforward approach is to use standard errors

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[8] Data for the years 1969, 1974, 1979, and 1984 are used in the first differences model in column 2 of Table 3.

[9] Data for the years 1969, 1973, 1977, 1981, and 1985 are used in the first differences model in column 3 of Table 3.

that are robust in the presence of heteroskedasticity or serial correlation.

The robust standard errors described in Wooldridge (1989) were used for ln(H) in columns (6) and (9) of Table 1 (first differences and deviations from means with year dummy variables) and for ln(H) and ln(W*H) in column (6) of Table 2 (deviations from means with year dummy variables.) Those robust standard errors (not shown in Tables 1 and 2) differed little from the standard errors obtained from first differences and deviations from means, and no hypothesis test is changed when using the robust standard errors.

The robust standard errors for ln(H) and ln(W*H) are shown in columns (2) and (3) of Table 3. In column (2), the robust standard errors are smaller than those obtained from first differences, and with the robust standard error the coefficient on ln(H) is significantly positive at the 10% level. In column (3), the robust standard error for ln(H) is slightly smaller while the robust standard error for ln(W*H) is slightly larger. The hypothesis test for ln(H) does not change, but with robust standard errors, ln(W*H) is no longer significantly negative at the 10% level in column (3).

The last issue that must be discussed involves interpreting causality from the regressions reported in Tables 1, 2, and 3. This is a common problem with production function studies that use aggregate data, as Gramlich (1994, p. 1188), among others, has noted. The regressions in Tables 1, 2, and 3 suggest that highway plus street capital is correlated with county output, but does the infrastructure cause output, does output cause infrastructure, or is it some combination of both? While not being able to definitively answer this question without a structural model, there are two reasons why this study has clear advantages over previous studies that used cross-sectional data.

First, if each county is a small actor within the state, the allocation of highway funding from the state government might be exogenous to the county's economy.[10] Of course, one difficulty with this line of reasoning is that some counties likely exert political influence on the statehouse, and if those are the same counties with large and growing economies, this could cause simultaneity bias. In California, the most obvious "large actor" is Los Angeles County, which, in 1988, accounted for 30% of the state's population and gross product.

For that reason, the regressions in columns (6) (first differences with year dummies) and (7) through (10) (fixed and random effects with and without year dummies) in Table 1 were estimated without Los Angeles County. Los Angeles County was also omitted from the comparable regressions in Table 2 (columns 3-7) and from columns (2) and (3) in Table 3. The results are not shown here, because all but one hypothesis tests are unchanged by omitting Los Angeles County. In column (3) of Table 3, the coefficient on ln(W*H) is negative but not significant at the 10% level when Los Angeles County is omitted from the regression.

The second advantage that county data bring in interpreting causality relates to the neighbor county effects. The regressions in column (6) of Table 2 and columns (2) and (3) of Table 3 provide some evidence that county output is reduced, ceteris paribus, by larger stocks of highway and street capital in neighboring counties. That is consistent with public infrastructure that is productive largely by giving counties a competitive advantage over their neighbors. The reverse causal link would state that slow county growth causes neighboring

[10] Street capital is more problematic, since some of those funds come from local sources.

counties to build more highway capital, or that counties with large gross products cause their neighbors to build less highway capital. While possible, this argument is somewhat strained, giving credence to the idea that causality runs from infrastructure to output, rather than vice versa.

Overall, the evidence strongly supports the hypothesis that county highway and street capital is linked to county output. Furthermore, there are good reasons to believe that causality runs from transportation infrastructure to output, although a definitive answer to that question should ideally be based either on structural models or on data from individual firms or economic sectors that are small enough to not influence highway and street spending decisions. The regressions in Tables 2 and 3 also support the idea that highway and street capital shifts economic output from one county to another within the state, thus appearing productive at small levels of geography, but not for larger geographic units.

In summarizing the empirical work, note that the evidence is strong enough to call into question recent results (e.g. Holtz-Eakin 1994; Kelejian and Robinson 1994) that suggest that public capital is not productive. Furthermore, the empirical results offer an explanation for why public capital might be productive at small geographic scales (e.g. counties) but not at higher levels of geography (e.g. states). The implications of this for policy and future research are discussed in the closing section.

VII. Research Directions and Policy Implications

The evidence here suggests that one type of public capital is productive in part by giving localized areas a competitive advantage, and thus creating not only output gains but also losses in nearby places. At a minimum, this suggests the importance of considering geographic scale when considering the question of whether public infrastructure is productive. More generally, it suggests that public authorities must carefully consider whether public capital projects produce economic benefits that correspond to the funding source. This point can be most clearly illustrated by discussing how the results in this paper can inform highway project evaluation.

There are two schools of thought on how economic benefits should be considered in highway project evaluation. The policy community has often assumed that highway capital produces economic benefits, and as mentioned earlier, some states consider those economic benefits in project evaluation. Yet a literal reading of recent cross-sectional studies of public infrastructure suggests that there are no (or at best negligibly small) economic benefits from infrastructure, highway capital included. Based on those cross-sectional studies, many scholars have concluded that only user benefits (e.g. reduced congestion or increased travel volume) should be included in benefit-cost analyses of highway and road projects. Given the evidence presented in this paper, the truth lies somewhere in between those two positions.

Highway and street capital appears to create economic gains, but largely by shifting economic activity within a state. The question of whether economic benefits should be incorporated into benefit-cost analyses depends on the geographic scale being considered. In the case of highways (which are often funded with large state and federal subsidies) this suggests the possibility that local governments will wish to build projects based on economic gains that they perceive as real, but which are not evident at the geographic scale that corresponds to the funding source.[11]

More generally, this work illuminates the need for careful benefit-cost analysis of highway and other public capital projects. One of the criticisms of past production function research on public capital has been that it diverts attention from project analysis. Gramlich (1994 pp. 1193-1194) states correctly that the policy implications from state or national production function studies can do little other than illuminate possible infrastructure shortages. Even if such shortages exist, a broad federal program to fund more public capital could include many projects that would not pass a benefit-cost test. This is apparently one of the reasons that Gramlich (1994) concluded that production function studies of public capital have outlived their usefulness.

Yet this study's focus on the issue of geographic scale gives results which, unlike previous production function studies, reinforce the need for project analysis. The purported productive effects of highway and street capital seem to exist, but public authorities should be very cautious when considering such benefits, since the benefits appear to represent a geographic redistribution of economic activity. Rather than divert attention from analysis of specific projects, this reinforces the need to analyze projects, to be cognizant of effects that occur both within and outside the immediate project area, and to consider how those effects correspond to the funding source for the project. Future research and applied work should try to apply this insight to public infrastructure benefit-cost analyses that consider not only whether economic gains are created by a project, but also whether those gains are at the expense of nearby areas.

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[11] Gramlich (1994) discusses similar ideas. See pp. 1190-1192.

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Appendix A: Data Sources and Construction of Variables

Section 1: Gross County Product

Gross County Product was estimated for each county based on the relationship shown below.

$$GP_{c,t} = \frac{Y_{c,t}}{Y_{s,t}} * GP_{s,t} \qquad (1)$$

where GP = gross product
Y = total personal income
"c" indexes counties, "t" indexes years, "s" subscripts denote state values

Gross Product in California in each year is apportioned to counties based on each county's share of California's total personal income in that year. Note that GP_{ct} is Gross County Product, denoted by GCP in the text. Data for California state personal income were taken from the Bureau of Economic Analysis publication *State Personal Income: 1927-1987.* County personal income was obtained from the *California Statistical Abstract*, published annually by the California Department of Finance. California gross state product is also available in the *California Statistical Abstract.* The methodology described above is the same as the technique used by the Southern California Association of Governments (SCAG) to estimate gross product in their member counties.

Section II: Highway Capital Stock

The stock of highway and road capital is constructed by first estimating the stock for each county in a base year, then depreciating that stock while adding the value of new expenditures in each successive year. This is done according to the relationship shown below.

$$H_{i,t} = (1-\delta) H_{i,t-1} + I_{i,t} \qquad (2)$$

where H = highway and street infrastructure stock
I = investment in highway and street infrastructure
δ = a depreciation rate
"i" indexes counties, "t" indexes years

Highway and street expenditures are available for California counties starting in 1957. Thus 1957 is the base year. Total highway and road infrastructure stock in California counties is estimated for 1957 based on data from *Fixed Reproducible Tangible Wealth*, published by the Bureau of Economic Analysis. That report includes data on the value of the stock of highway and street capital in the U.S. for the years 1850 to 1989. The stock of national highway and road infrastructure for 1957 is apportioned to California based on the ratio of total highway and road mileage in California divided by total highway and road mileage in the nation. The highway and road mileage data for California and the U.S. are from *Highway Statistics, 1957*, published by the Federal Highway Administration. The estimated stock of California highway and street capital is then apportioned to counties based on each county's fraction of California highway and street mileage in 1957. The data on highway and street miles in California counties is from the *California Statistical Abstract*, published by the California Department of Finance.

Annual investments in highways and streets in each county were obtained from two sources. *Annual Report: Financial Transactions Concerning Streets and Roads of Cities and Counties of California*, published by the California Office of the Controller, provided data on street and road expenditures by cities and counties for each year from 1957 to 1988. *State Highway Program Financial Statements and Statistical Reports*, published by the California Department of Transportation, provided data on state highway expenditures in each county from 1957 to 1988. The two reports cover distinct budgetary catagories, so combining the two leads to no double counting. Furthermore, actual expenditure information was always used, rather than data on appropriations, since appropriations do not always equal expenditures.

A depreciation rate of 4.1% was used in equation (2). This is the rate computed by Holtz-Eakin (1993) for national infrastructure stocks. It is similar to other depreciation rates for non-residential capital (Holtz-Eakin 1993, p. 191.)

Section III: Private Capital Stock

The private capital stock estimates for each of the counties of California were calculated using the technique described in the appendix of Munnell (1990a), which is based on the methodology employed by Costa, Ellson, and Martin (1987).

Following Munnell (1990a), the private capital stock in each county in each year is given by

$$K_i = \frac{AGK_i}{\sum AGK_i} AGK + \frac{MFGK_i}{\sum MFGK_i} MFGK + \frac{NFNMFGK_i}{\sum NFNMFGK_i} NFNMFGK \qquad (3)$$

where:

AGK = an estimate of the capital stock in the agricultural sector in California

MFGK = an estimate of the capital stock in the manufacturing sector in California

NFNMFGK = an estimate of the capital stock in the manufacturing sector in California

AGK_i = proxy for agricultural capital stock in county "i"

$MFGK_i$ = proxy for manufacturing capital stock in county "i"

$NFNMFGK_i$ = proxy for nonfarm, nonmanufacturing capital stock in county "i"

The data on agricultural and manufacturing capital for California were provided by Douglas Holtz-Eakin, but are the same data described in Munnell (1990a). The data for California nonfarm, nonmanufacturing capital were available for several sectors, described below. Those data also are described in Munnell (1990a) and were made available by John Greenlees and Alicia Munnell. All capital stock data are in 1982 dollars.

The total capital stock in agriculture in California was distributed among counties based on the value of land, buildings, and equipment in agriculture. The value of land, buildings and equipment for 1969, taken from the *1972 County and City Data Book*, was used as a proxy to calculate the stock for 1969,70 and 71. The value of land, buildings, and equipment in agriculture in 1974, taken from the *1977 County and City Data Book* was used to calculate capital stock shares for 1972 to 1975. Stocks for 1976 to 1979 were based on 1978 data, taken from the *1983 Data Book*. Data for 1982, taken from the *1988 Data Book*, were used in estimating stocks for 1980 to 1984. The *1993 County and City Data Book* was not available yet so information on the value of land, buildings and equipment in agriculture from the *1987 Census of Agriculture* was used as a proxy for apportioning the agricultural capital stocks for 1985 to 1988.

The capital stock estimate in manufacturing was distributed among counties based on each county's share of the value added by manufactures. The value added by manufactures for 1967, taken from the *1972 County and City Data Book*, was used as a proxy to calculate the stock for 1969 and 1970. Data for 1972, taken from the *1977 County and City Data Book*, were used to calculate shares for 1971 to 1974. Stock calculations for 1975 to 1979 were based on 1977 data, taken from the *1983 County and City Data Book*. Data for 1982, taken from the *1988 County and City Data Book* were used in estimating stocks for 1980 to 1984. Because the *1993 County and City Data Book* was not yet available, information on value added by manufacturers from the *1987 Census of Manufactures* was used as a proxy for estimating the stocks for 1985 to 1988.

The estimate of private capital in the nonfarm, nonmanufacturing sector for the state of California provided by Alicia Munnell was divided among the counties according to the sum of estimates for the following subsectors: construction, mining, retail trade, wholesale trade, banking, trucking and warehousing, water transportation, electric services, gas services, telephone and telegraph, and services. The following equation describes the estimating procedure:

$$\begin{aligned} NFNMFGK_i = {} & (shCONSTR_i * CONSTRK) + (shMI_i * MIK) + (shR_i * RK) \\ & + (shW_i * WK) + (shB_i * BK) + (shTRUCK_i * TRUCKK) \\ & + (shBOAT_i * BOATK) + (shELEC_i * ELECK) \\ & + (shGAS_i * GASK) + (shTEL_i * TELK) \\ & + (shSVCS_i * SVCSK) \end{aligned} \tag{4}$$

where: sh prefix denotes county i's share of proxy variable
K suffix denotes state capital stock esimate for the given subsector

The air transportation and railroad subsectors were included in Munnell's (1990a) estimates of state nonfarm, nonmanufacturing private capital. Those two subsectors, which represent approximately 4.9% of the total California private capital stock estimate for the years 1969-1988, were excluded from our calculations because of no appropriate railroad or air transportation data were available that could act as proxy variables for counties.

The total stock of capital in construction was distributed among counties based on each county's share of the valuation of new housing units authorized by building permits, taken from the *County Fact Book*. Data for the valuation of new housing units authorized by building permits were available for every year between 1969 and 1988 except 1977 and 1978. The data for 1976 was used to apportion the share in construction for 1977 and the data for 1979 was used to apportion the share in construction in 1978.

The capital stock estimate for mineral industries in California was apportioned based on the value of shipments and receipts in mineral industries as taken from the *County and City Data Book* and the *Census of Mineral Industries*. The value of shipments and receipts for 1967, taken from the *1972 County and City Data Book*, was used as a proxy to calculate the stock for 1969 and 1970. Data for 1972, taken from the *1977 County and City Data Book*, were used to calculate shares for 1971 to 1974. Stock estimates for 1975 to 1979 were based on data furnished in the 1977 *Census of Mineral Industries*. Data from the *1982 Census of Mineral Industries* were used in estimating stocks for 1980 to 1984. The *1987 Census of Mineral Industries* provided the data for 1985 to 1988.

The value of retail trade assets (RK) was apportioned according to each county's share of retail trade sales, taken from the *County and City Data Book*. Retail trade sales data for 1967, taken from the *1972 County and City Data Book*, were used as a proxy to calculate the stock for 1969 and 1970. Data for 1972, taken from the *1977 County and City Data Book*, were used to calculate shares from 1971 to 1974. Stock estimates for 1975 to 1979 were based on 1977 data, taken from the *1983 County and City Data Book*. Data for 1982, taken from the 1988 *County and City Data Book*, were used in estimating stocks for 1980 to 1984. Because the *1993 County and City Data Book* is not yet available, the *1987 Census of Retail Trade* data were used as a proxy for estimating the stocks for 1985 to 1988.

The value of wholesale trade assets (WK) was apportioned according to each county's share of wholesale trade sales, taken from the *County and City Data Book*. Wholesale trade sales data for 1967 taken from the *1972 County and City Data Book*, were used as a proxy to calculate the stock for 1969 and 1970. Data for 1972, taken from the *1977 County and City Data Book*, were used to calculate shares for 1971 to 1974. Stocks for 1975 to 1979 were based on 1977 data, taken from the *1983 County and City Data Book*. Data for 1982, taken from the *1988 County and City Data Book*, were used to estimate stocks for 1980 to 1984. Because the *1993 County and City Data Book* was not yet available, the *1987 Census of Wholesale Trade* provided the data for 1985 to 1988.

The stock estimate in the banking sector (BK) were distributed among counties using each county's share of deposits in 1970, 1976, 1981 and 1986, taken from data in the *County and City Data Book* published in the years 1972, 1977, 1982 and 1988, respectively. Deposit data for 1970 were used as a proxy to calculate each county's share of banking capital stock for the years 1969 to 1973. Data for 1976 were used to calculate each county's share for the years 1974 to 1978. Stock estimates for 1979 to 1983 were apportioned based on 1981 data.

Data for 1986 were used in estimating stocks for 1984 to 1988.

Trucking and warehousing assets(TRUCKK) were distributed to counties based on each county's share of statewide truck travel as published in *Truck Miles of Travel On California State Highway System: 1974 to 1991*. Because this data source does not exist before 1974, the data were used to extrapolate estimates of truck travel for counties for the years 1969 to 1973. Assets in trucking and warehousing were apportioned based on these estimated proxies. In the remaining years, 1974 to 1988, the stock estimates were apportioned according to the annual report on statewide truck travel per California county.

The state estimates of assets in water transportation (BOATK) were apportioned among the counties based on data on the tonnage of commerce in California ports, furnished in *Waterborne Commerce of the United States* (1967, 1972, 1977, 1980, 1987). Waterborne commerce data in 1967 was used as a proxy to apportion the stock estimates for 1969 and 1970. The data in 1972 were used as a proxy to apportion the stock estimate for the years 1971 to 1974. The data in 1977 were used as a proxy to apportion the stock estimates for the years 1975 to 1979. The data in 1980 were used as a proxy to apportion the stock estimates for the years 1980 to 1983. Finally, the data in 1987 were used as a proxy to apportion the stock estimate for the years 1984 to 1988.

The proxy used to distribute assets in electric services (ELECK) was based on each county's share of population, taken from the *California Statistical Abstract*, for each of the years between 1969 to 1988. Similarly, total California gas services assets(GASK) were divided among counties based on each county's share of population.

The stock estimates for assets in telephone and telegraph were distributed among counties based on each county's share of telephones taken from *The County Fact Book* for the years 1969, 1972, 1977, 1980. The number of telephones in 1969 was used to apportion the stock estimates in telephones and telegraphs in 1969 and 1970. The number of telephones in 1972 was used to apportion the stock estimates in telephones and telegraphs in 1971, 1972, 1973 and 1974. The number of telephones in 1977 was used to apportion the stock estimates in telephones and telegraphs in 1975, 1976, 1977 and 1978. The number of telephones in 1980 was used to apportion the stock estimates in telephones and telegraphs in 1979, 1980, 1981. Following the breakup of the telephone companies in 1981, the maintenance of records on the number of telephones per county was discontinued. The telephone data from 1969 to 1980 were used to extrapolate estimates of the appropriate proxies for the remaining period, 1982 to 1988.

The estimate of California capital assets in the service sector (SVCSK) was apportioned using each county's share of total receipts in six selected services -- hotels, personal services, business services, auto repair services, amusement services, and legal services. Taken from the *1972 County and City Data Book*, the total receipts data for 1967 were used as a proxy to calculate the stock for 1969 and 1970. Data for 1972, taken from the *1977 County and City Data Book*, were used to calculate shares for 1971 to 1974. Stock estimates from 1975 to 1979 were apportioned based on 1977 data, taken from the *1983 County and City Data Book*. Data for 1982, taken from the *1988 County and City Data Book*, were used in estimating stocks for 1980 to 1984. Because the *1993 County and City Data Book* was not yet available, the *1987 Census of Service Industries* provided the proxy data for 1985 to 1988.

Section IV: Employment Data

The employment data for California counties are from *County Business Patterns* for the years 1969-1988.

Table 1: Regression Results
Dependent Variable = log(gross county product)

ind var	1. OLS	2. OLS	3. OLS	4. 1st diff's	5. 1st diff's	6. 1st diff's	7. FEM	8. REM	9. FEM	10. REM
log(L)	0.69 [a] 0.015	0.69 [a] 0.015	0.69 [a] 0.015	0.35 [a] 0.023	0.21 [a] 0.023	0.09 [a] 0.023	0.66 [a] 0.021	0.67 [a] 0.019	0.63 [a] 0.025	0.65 [a] 0.021
log(K)	0.23 [a] 0.016	0.24 [a] 0.017	0.24 [a] 0.017	0.10 [a] 0.018	0.007 0.018	0.04 [b] 0.018	0.19 [a] 0.020	0.20 [a] 0.019	0.19 [a] 0.020	0.20 [a] 0.019
log(H)	0.028 0.014	0.027 0.014	0.024 0.014	0.33 [a] 0.061	0.016 0.060	0.15 [b] 0.06	0.19 [a] 0.039	0.12 [a] 0.026	0.15 [a] 0.040	0.13 [a] 0.028
constant	8.70 [a] 0.25	8.71 [a] 0.25	8.70 [a] 0.25		0.033 [a] 0.002			7.79 [a] 0.46	7.91 [a] 0.80	7.75 [a] 0.46
time effects	none	linear trend	year dummies	none	linear trend	year dummies	none	none	year dummies	year dummies
county effects	none	none	none	fixed	fixed	fixed	fixed	random	fixed	random
R^2	0.99	0.99	0.99	0.29	0.08	0.56	0.99	0.99	0.99	0.99
N	1100	1100	1100	1045	1045	1045	1100	1100	1100	1100
Hausman (3 df) p-value								6.90 0.07		6.97 0.07

standard errors are shown below coefficient estimates

[a] statistically significant at 0.01 level

[b] statistically significant at 0.05 level

Table 2: Regressions with Spatial Lag of Highway Capital
Dependent Variable = log(gross county product)

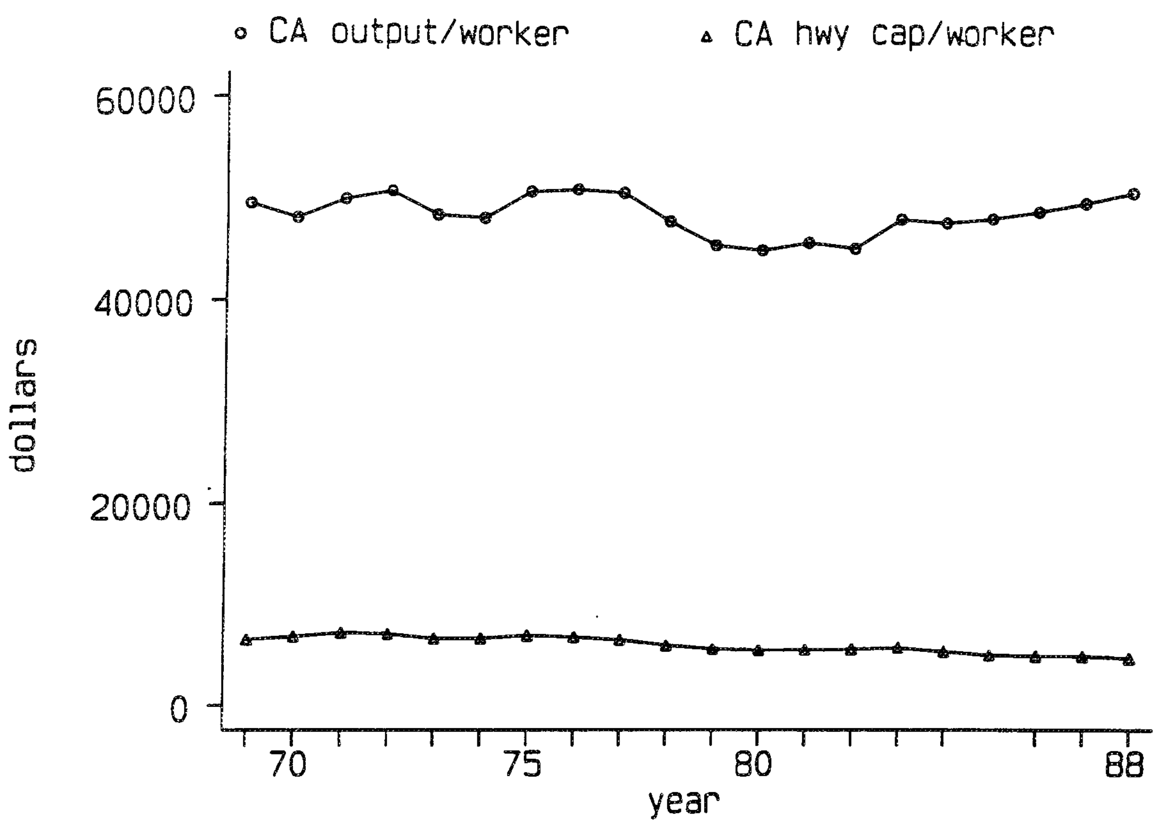
ind var	1. 1st diff's	2. 1st diff's	3. 1st diff's	4. FEM	5. REM	6. FEM	7. REM
log(L)	0.32 [a] 0.23	0.21 [a] 0.023	0.09 [a] 0.02	0.67 [a] 0.021	0.67 [a] 0.020	0.62 [a] 0.025	0.65 [a] 0.021
log(K)	0.09 [a] 0.018	0.005 0.018	0.04 [b] 0.018	0.20 [a] 0.020	0.20 [a] 0.019	0.20 [a] 0.020	0.20 [a] 0.019
log(H)	0.17 [b] 0.07	0.066 0.067	0.15 [b] 0.062	0.20 [a] 0.041	0.12 [a] 0.027	0.16 [a] 0.040	0.14 [a] 0.028
log(W*H)	0.39 [a] 0.10	-0.16 0.97	0.009 0.118	-0.047 0.052	0.0016 0.027	-0.21 [a] 0.059	-0.019 0.027
constant		0.03 [a] 0.002			7.76 [a] 0.59	11.908 [a] 1.394	8.00 [a] 0.60
time effects	none	linear trend	year dummies	none	none	year dummies	year dummies
county effects	fixed	fixed	fixed	fixed	random	fixed	random
R^2	0.30	0.08	0.56	0.99	0.99	0.99	0.99
N	1045	1045	1045	1100	1100	1100	1100
Hausman (4-df) p-value							18.82 8.5×10^{-4}

standard errors are shown below coefficient estimates
[a] statistically significant at 0.01 level
[b] statistically significant at 0.05 level

Table 3: Long Differences
Dependent Variable = log(gross county product)

ind var	1. 1988-1969	2. 5-year intervals	3. 4-year intervals
log(L)	0.70 [a] 0.074	0.65 [a] 0.08	0.37 [a] 0.05
log(K)	0.27 [a] 0.071	-0.007 0.054	0.15 [a] 0.04
log(H)	-0.02 0.09	0.22 0.13 (0.12)	0.22 [b] 0.11 (0.10)
log(W*H)	-0.15 0.12	-0.51 [b] 0.20 (0.19)	-0.29 0.17 (0.19)
constant	0.64 0.50		
time effects	none	year dummies	year dummies
county effects	fixed	fixed	fixed
R^2	0.89	0.80	0.79
N	55	165	220

standard errors are shown below coefficient estimates
robust standard errors are in parentheses
[a] statistically significant at 0.01 level
[b] statistically significant at 0.05 level
[a] and [b] superscripts denote significance levels using conventional, rather than robust, standard errors



CA output/worker
CA hwy cap/worker
60000
40000
20000
0
dollars
70
75
80
88
year

Figure 1