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PUBLIC-SECTOR CAPITAL AND THE PRODUCTIVITY PUZZLE

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ABSTRACT

A number of studies have suggested a quantitatively important relationship between public-sector capital accumulation and private sector productivity, with the most compelling evidence derived from analyses of state-level data. Estimates herein of production functions that use standard techniques to control for unobserved, state-specific characteristics, however, reveal essentially *no* role for public-sector capital in affecting private sector productivity. Only estimates of state production functions that do not include such controls find substantial productivity impacts. This result reconciles existing econometric estimates with the findings of Hulten and Schwab based on growth accounting techniques, as such techniques effectively control for state-specific effects. Region-level estimates are essentially identical to those from state data, suggesting no quantitatively important spillover effects across states.

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1. INTRODUCTION

Popular and professional perception of the role played by public sector capital has undergone a remarkable transformation. For the bulk of the postwar period, government capital budgeting decisions focused at best on the consumption benefits accruing from public goods and services, and at worst on the pork-barrel punch they carried. More recently, however, the public sector capital stock has emerged as a potent force for improved macroeconomic performance. Spurred by the work of Aschauer [1989a,1989b], a number of studies have examined the relationship between public-sector capital accumulation, especially "infrastructure" capital, and output or productivity in the private sector. The initial studies suggested a quantitatively important link between public sector capital and private productivity and buttressed the arguments of proponents of greater spending on public works.[1]

Perhaps the most compelling evidence in support of this thesis derives from empirical analyses of state data, and the purpose of this paper is to re-visit these analyses. Analysts have turned to the states only after exhausting other avenues for research. Aschauer [1989a], Holtz-Eakin [1988, 1989] and Munnell [1990a] use annual data for the United States to estimate production functions that include public-sector capital. Unfortunately, these data contain essentially a single observation: the concomitant slowdown in productivity growth and public sector capital accumulation in the early 1970s. It is tempting to infer a causal relationship from public-sector capital to productivity, but the evidence does not justify this step. It is just as easy to imagine the reverse scenario in which deteriorating economic conditions reduce capital stock growth. More generally, almost every post-war macroeconomic series (on quantities) has this characteristic shape, and it remains unclear as to the underlying causes of the slowdown.

A second potential source of information is cross-national studies of productivity growth (e.g. Aschauer [1989b]). Studies using these sources must face a relative paucity of comparable data and the difficulty of controlling for greatly different institutional arrangements across countries. The result has been rather unstable parameter estimates (see Tanzi [1990]).

This leaves the states as the most promising source of information on the spillovers from public sector capital. States exhibit a wide array of fiscal behaviors and the available samples are large enough to produce reliable estimates. Recent studies using state data have, however, largely produced controversy. On one hand, the econometric analyses of state data in Munnell [1990b] and Garcia-Mila and McGuire [forthcoming] attribute to public-sector capital an important role in explaining differences in states' economic performance. On the other hand, Hulten and Schwab [1978, 1991] use growth-accounting techniques to apportion regional economic growth. They find that the residual not accounted for by private inputs is at odds with regional patterns of public-sector investment.[2] Is the conflict the result of differing technique, the degree of aggregation, or some other factor?

In this paper, I argue that these disparate findings are easily reconciled. Estimates herein of production functions that use standard techniques to control for unobserved, state-specific characteristics reveal essentially no role for public-sector capital at the margin. The presence of such effects is quite likely on *a priori* grounds and is confirmed by the results of the statistical analysis. Importantly, the estimates of state production functions cited above do not include controls for these effects, while the focus in growth accounting on changes over time effectively controls for state-specific effects. Thus, the exclusion of these considerations from previous econometric studies is the apparent source of the conflicting evidence using state data. To further ensure that this feature of the estimation is the source of the difference, the analysis is repeated using regional aggregates as in, for example, Hulten and Schwab [1978,1991]. The regional results are essentially identical to those from state data.[3]

Section 2 is a discussion of the issues, while Section 3 presents the data and estimation results. The final section is a summary.

2. CONCEPTUAL ISSUES

The centerpiece of the analysis is an aggregate, state production function of the form:

$$q_{st} = \beta_0 + \beta_1 k_{st} + \beta_2 l_{st} + \beta_3 g_{st} + \varepsilon_{st} \tag{2.1}$$

where s indexes states, t indexes time periods, q_{st} is the logarithm of private output, k_{st} is the logarithm of private capital inputs, l_{st} is the logarithm of labor inputs, and g_{st} is the logarithm of public-sector capital.[4] From a policy standpoint, the basic issue is whether β_3 is positive and, if so, its quantitative importance. The quality of the estimate of β_3, however, depends upon the resolution of issues in specification and estimation. In what follows, I focus on issues in the specification of the error structure, endogeneity bias, restrictions on the coefficients to satisfy constant returns to scale (CRTS), and the effects of aggregation.

2.1 Specification of the Error Structure

The availability of panel data permits greater flexibility in the specification of the econometric error. As in other applications, a typical specification would be:

$$\varepsilon_{st} = f_s + \gamma_t + \mu_{st} \tag{2.2}$$

where f_s is a state-specific component, γ_t is a time-specific component, and μ_{st} is an i.i.d. error. The f_s capture those characteristics of the production function in each state that are unobservable (or omitted from the equation), but do not vary over time, while the γ_t control for shocks to the production function that are common to all states in each time period. In practice, the latter control primarily for business-cycle effects on output (or productivity), and virtually all investigators include dichotomous variables for each time period in the sample.[5]

In contrast, results suggesting that there are large output or productivity effects from government capital stem from econometric specifications that exclude state-specific effects. On the basis of reflection, it seems unlikely that such effects *not* be present. Aggregate production functions of the type in equation (2.1) ignore the effects of land area, location, weather, endowments of raw materials, and a myriad of other factors that result in differential productivity across locations. It would seem a sensible first step to check for the existence and importance of such effects.

Fixed Versus Random Effects.[6] One estimation strategy is to treat the state-specific effects as fixed, controlling for their presence through the use of state dummy variables or by entering all variables as deviations from state-specific means.[7] In doing so, the investigator essentially chooses to make inferences conditional upon the particular set of f_s present in the sample. In addition, the investigator ignores the information from cross-state variation in the variables, focusing instead on time-variation within each state. Thus, for example, Garcia-Mila and McGuire [forthcoming] explicitly reject controlling for state effects, saying "... we do not include state dummy variables in order that the cyclical variation over time does not dominate the long-run relationship we hope to estimate" (p. 9).

A second form of fixed-effects estimation is to transform the data through the use of differences over long periods of time. In the estimation below, for example, the sample period covers the 18 years 1969 to 1986. To eliminate state-specific effects, but avoid identification based on short-run (year-to-year) deviations from state-specific means, I estimate a variant of the production function in equation (2.1) in which each variable is entered as the change between 1969 and 1986. To the extent that public-sector capital is an important determinant of long-run productivity growth, it should be revealed by a focus on variation over a period of nearly two decades.

The alternative to fixed-effects estimation is to treat the state-specific effect as a component of the error term that contributes to its overall variance. The presence of the f_s, however, produces a correlation between error terms common to each state, requiring a generalized least squares (GLS) estimator to achieve efficient estimates. Clearly, the unconditional inference permitted by a random effects specification would, in general, be preferable, and it may be desirable to retain the cross-state information in the sample.

Unfortunately, in the presence of correlation between the right-hand-side variables and the state effects, the GLS estimator will be biased and inconsistent. As shown by Hausman and Taylor [1981], a comparison of the fixed-effects estimator and the GLS estimator serves as a test of the null hypothesis that the state effects are uncorrelated with observed quantities of capital and labor. In the results presented below, I utilize this test to check for the impact of these effects on the estimated production function.

2.2 Instrumental Variables Estimators

There are two likely sources of correlation between the right-hand-side variables and the error term. The first, noted above, occurs when the levels of the private and public inputs are chosen conditional upon the value of the state-specific effect. In this circumstance, the resultant correlation between inputs and the f_s, results in inconsistent estimates when the random effects estimator is employed. The fixed-effects estimator will provide consistent estimates, but as noted above these estimates will exploit only the time or "within-state" variation in the data.

An alternative solution that retains the information from cross-sectional variation is the use of instrumental variables. In an effort to retain information from cross-state variation, I construct an (IV) estimator that uses other states' input levels as instrumental variables. Specifically, the instrumental variables are constructed by first ordering the states alphabetically. Next define the first (alphabetic) "lag" of labor, for example, as the value of labor for the previous state in the list. Thus, the lagged value of labor for Georgia is the value for Florida. (The lag of Alabama -- first in the list -- is defined to be Wyoming.) The second lag is defined similarly. In the example, the second lag for Georgia is Delaware. (Again, the second lag for Alabama is defined to be Wisconsin, while the second lag for Arizona -- second on the list -- is Wyoming.) In implementing the estimator, I estimate equation (2.1) using first and second "lags" of labor, private capital, and public capital as instrumental variables.[8]

The discussion thus far has focused on the impact of simultaneity due to state-specific effects on the estimated production function, particularly the estimated spillovers of public sector capital. A second source of potential bias and inconsistency is the simultaneous determination of observed quantities of capital, labor, and output. In the presence of correlation of this type (between, for example, labor inputs and the μ_{st}), estimators such as the conventional fixed-effects estimator will be biased and inconsistent.[9] In these circumstances, Holtz-Eakin, Newey, and Rosen [1988] (HNR) propose using first-differences to eliminate the state-specific effects and an instrumental variables estimator to circumvent the simultaneity bias.[10] In the context of a production function estimate, simultaneity seems quite likely. To eliminate the state effects, the production function is estimated in first-differenced form:

$$\Delta q_{st} = \beta_0 + \beta_1 \Delta k_{st} + \beta_2 \Delta l_{st} + \beta_3 \Delta g_{st} + \Delta \gamma_t + \Delta \mu_{st} \tag{2.3}$$

and I employ the instrumental variables procedure of HNR to control for correlation between the variables (k_{st}, l_{st}, g_{st}) and the error term (μ_{st}). The instrumental variables employed are twice-lagged values of the right-hand-side variables (Δk_{st-2}, Δl_{st-2}, Δg_{st-2}).[11] The use of this estimation procedure thus avoids inconsistency stemming from both the presence of correlated state effects and the simultaneous determination of inputs and output.

2.3 Coefficient Restrictions

While the focus of this paper is the impact of alternative specifications of the error structure, a related issue is the degree to which the estimated production function satisfies the assumption of constant returns to scale. One possibility is that the production function satisfies CRTS in private inputs, i.e. $\beta_1 + \beta_2 = 1$. This yields a production function in intensive form:

$$(q_{st} - l_{st}) = \beta_0 + \beta_1 (k_{st} - l_{st}) + \beta_3 g_{st} + \varepsilon_{st} \tag{2.4}$$

Restricting the production function in this fashion reduces the number of parameters to estimate and may result in improved precision. Inappropriately assuming this form, however, may result in a specification error that leads to inconsistent parameter estimates. In what follows, I will test for the appropriateness of the production function in equation (2.2).[12]

Alternatively, one could imagine that the production function satisfies CRTS in all inputs, *viz.*:

$$(q_{st} - l_{st}) = \beta_0 + \beta_1(k_{st} - l_{st}) + \beta_3(g_{st} - l_{st}) + \varepsilon_{st} \tag{2.5}$$

where $\beta_1 + \beta_2 + \beta_3 = 1$. The argument in favor of CRTS is typically conceptual, arguing that if one simply duplicated the economic environment, one should get twice the output. Given the degree of aggregation and the sparse specification of the list of inputs, the degree to which CRTS is reasonable is an empirical question.

2.4 Aggregation

One final candidate source of the conflicting results in the literature is the level of aggregation. Hulten and Schwab [1978,1991] focus on regional growth accounting, while Munnell [1990b] and Garcia-Mila and McGuire [forthcoming] employ state-level data. While one might be tempted to explain the difference on this basis, Munnell [1991] argues exactly the opposite: that one would expect, and the evidence is consistent with, the notion that the estimated coefficient on public capital should rise with the level of aggregation. The intuitive basis for this is that in using states one misses a fraction of the spillover benefits from the public capital stock. As a result, estimates using regional data should show a greater role for public-sector capital. To investigate this possibility, the production function is estimated using both a panel of state-level data and a panel of region-level data.

3. DATA AND ESTIMATION

3.1 Data

The data set employed herein consists of output, labor, private capital, and state and local government capital for the 48 contiguous states over the years 1969 to 1986. Output is measured by the Bureau of Economic Analysis (BEA) state-by-state series on Gross State Product by private industries.[13] Estimates of private sector capital are from Munnell [1990b].[14] Labor inputs are BEA measurements of full-time and part-time wage and salary employees in private industries. Public sector capital data are new estimates of the sum of state government capital and local government capital, by state, for all governmental functions.[15,16] Sample statistics for the 864 observations on state-level data are shown in Table 1.

When converting the state data to eight regional aggregates, I follow the regional divisions used by the Regional Economic Measurement Division of the BEA. Descriptive statistics for the regional data, where the number of observations is 144, are contained in Table 2.

3.2 Results from State-Level Data

The estimation results using state-level data are reported in Table 3. To begin, consider column (1), which contains the results of ordinary least squares (OLS) estimation of equation (2.1). The estimated coefficient on labor is 0.497, while that on private capital is 0.359. As shown in the third row, the estimates indicate that the elasticity of private output with respect to public sector capital is a substantial 0.203 and the coefficient is precisely estimated. Thus, the simplest empirical analysis reinforces the qualitative and quantitative findings of Aschauer, Munnell, and others.[17] Given this, it is worth noting the similarity in results, despite the use of an alternative measure of output (private sector versus total) and new estimates of the public-sector capital stock. To the extent that differences emerge below, they do not arise from these sources.

The remainder of the rows provide information concerning the econometric specification and tests of restrictions on the coefficients. As shown in the table, the estimates in column (1) control for time effects, but not state effects.[18] Finally, the final two rows show chi-square test statistics of the null hypothesis that the estimated production function exhibits CRTS in private inputs or private plus public inputs, respectively. As shown, the null hypothesis is strongly rejected in both instances.[19]

The next step is to re-estimate the model while accommodating the existence of a state-specific component to the error structure. The estimates shown in column (2) do so, estimating the production function while controlling for fixed, state-specific effects. The results are quite striking. The estimates of the "private portion" of the production function are reasonably stable, and now easily satisfy the constraint of CRTS in private inputs. In contrast, the elasticity with respect to government capital is estimated to be negative (-0.0517) and is statistically different from zero at the 5 percent level of significance.[20] Since one cannot reject the hypothesis of CRTS in private inputs, I impose this constraint. The estimates in column (3) show that this has a negligible impact on the nature of the parameter estimates.

The primary objection to the conventional fixed-effects approach is that the use of deviations from state-specific means implies that the parameters are identified by annual variation over time within each state. As noted above, Garcia-Mila and McGuire, for example, explicitly reject the use of such relatively high-frequency variation as the basis for their analysis. The next columns represent two responses to this dilemma. Column (4) presents the results of estimating the production function using a single cross-section of "long-differences"; i.e. changes (in logarithms) between 1969 and 1986. As in the case of the conventional fixed-effects estimator, the point estimate of the impact of public capital is negative (but is statistically insignificant at conventional levels). Imposing CRTS in all inputs (column (5)) has no effect on this basic conclusion.

The second response is to treat the state-specific effects as random. If the state-specific effects are uncorrelated with the right-hand side variables, a GLS estimator that treats the state effects as a variance component is appropriate. As noted above, such an estimator has the advantage of continuing to exploit the cross-sectional information in the data while providing more efficient estimates of the parameters.

The GLS estimates are shown in columns (6) and (7) of Table 3.[21] The estimates in column (6) are from the unrestricted production function, while those in column (7) -- based on the test statistics in column (6) -- restrict the coefficients to satisfy CRTS in private inputs.[22] The moral is clear. As in the case of fixed effects, allowing for a state-specific component to the error structure dramatically reduces the putative importance of the public sector capital stock in the private production process. In this case, however, the conclusion holds when using both the time-variation and cross-section variation in the data.

As discussed above, a comparison of the fixed-effects and random-effects estimators serves as a formal test of the hypothesis that state-specific effects are uncorrelated with the regressors. Using the estimates in column (3) and column (7), the chi-square test statistic (with two degrees of freedom) is 27.6. Thus, the null hypothesis is easily rejected at the one percent level of significance. Accordingly, one must either rely on the fixed-effects estimates and ignore the cross-sectional information in the data or employ an instrumental variables procedure to circumvent the source of the inconsistency.

The remaining columns of the table are devoted to the instrumental variables estimators. Recall that difficulties may arise from either of two sources. First, the Hausman-Taylor test suggests that the levels of private inputs are made conditional on the value of the state-specific effect. In these circumstances, the fixed-effects estimator "solves" the problem, at the cost of ignoring information in cross-sectional variation. An alternative approach is to use an instrumental variable for the private inputs. In a similar fashion, the level of output and the inputs may be determined simultaneously, resulting in correlations between the right-hand-side variables and μ_{st}. Again, an instrumental variables estimator provides the opportunity for consistent estimates.

The estimates in columns (8) and (9) result when other states' input levels are used as instrumental variables. As above, the first column of this set is an unrestricted estimate, while column (9) imposes the appropriate restrictions; in this case that of CRTS in private and public inputs. Regardless of the constraints, the results again fail to suggest an important, positive relationship between public-sector capital and private productivity -- the point estimate of the elasticity is negative and insignificantly different from zero.

Clearly, the quality of an IV estimate depends on two factors: the degree of correlation between the instrumental variables and the regressors, and the absence of correlation between the instrumental variables and the errors. Given the fairly arbitrary construction of the IV estimator, it is useful to examine its performance in these areas. The symptom of problems in the first case is large standard errors. The precise estimates for the private inputs do not suggest a problem on this front. Regarding the latter, it is possible to implement a test of the over-identifying restrictions in this context. The estimates in column (9), for example, use four instrumental variables to estimate two parameters. A test of the hypothesis that the instrumental variables are uncorrelated with the errors produces a chi-square statistic with two degrees of freedom equal to 3.73, which is significant only at the 15 percent level. Thus, despite the fairly heroic nature of the procedure, the evidence suggests no particular deficiencies with the instrumental variables estimator. The point estimates reinforce the basic message from the previous columns.

The final columns of Table 3 present the results of the first-differenced, instrumental variable estimator proposed by HNR.[23] Focusing heavily on the time-variation in the data has one clear effect: the estimated elasticity with respect to labor inputs is dramatically larger than in the preceding columns. With regard to the primary focus of this paper, however, little is different. The estimated spillover elasticity between public capital and private sector production is negative, small in magnitude, and not significantly different from zero.

The evidence from state-level data is unambiguous. Findings of a statistically and economically significant, positive elasticity for public sector capital are an artifact of restrictions placed on the error structure. When using more appropriate techniques, the most plausible estimate of this elasticity is zero. The issue remaining is whether this finding (or others in this research area) is sensitive to the degree of geographical aggregation. To investigate this, I turn now to the results from regional data.

3.3 Results from Region-Level Data

Table 4 replicates the results in Table 3, using instead data for eight regional aggregates. The greatly reduced cross-section size, however, precludes the use of the long-differences, the instrumental variables estimator, and the HNR approach, so the number of results reported is smaller than before.[24]

There are two lessons to be learned from Table 4. The first concerns the impact of region-specific effects on the estimated elasticity with respect to public capital. In this regard, the regressions tell the same story as in the state-level data. One could virtually repeat the discussion in the preceding section; in the interest of conserving space I leave the comparisons to the reader and report only the bottom line: the best estimate of the spillover elasticity is roughly zero.

The second lesson comes from the striking similarity of the estimates in corresponding columns of Tables 3 and 4. Aggregation does not affect the point estimates to any great degree, suggesting that the use of regional data does not permit one to capture additional spillovers of any substantial magnitude. For public capital, this restates the conclusion that the coefficient is essentially zero. The lack of any effect on the coefficients for private capital and labor suggests an absence of significant externalities between the production possibility sets of private firms of the type that has occupied a central role in the literature in endogenous growth theory.

4. SUMMARY

The economic role of public sector capital, particularly "infrastructure" capital, has become the focus of recent discussions in a wide variety of academic, policy and popular settings. At one extreme are studies suggesting an "infrastructure crisis" of potentially apocalyptic proportions. This view has been buttressed by recent studies showing that the public sector capital stock has a large impact on private sector production. At the other end of the spectrum, analysts argue that the provision of public sector capital has little, if any, effect on private firms' production possibilities.

The issue is fundamentally empirical and a careful weighing of the evidence available from state-level data and region-level data indicates that the best estimate of the elasticity of private output or productivity with respect to state and local government capital is essentially zero. As a result, attempts to link the puzzling recent decline in productivity growth with slower governmental capital accumulation appear without merit. Previous findings of large, positive effects appear to be the artifact of an inappropriately restrictive econometric framework.

It would be wrong to conclude from this analysis that the large stock of public capital provides no benefits. The regression analysis indicates only that the productivity benefits in excess of direct provision of amenities are negligible. It would be a departure of common sense to argue that there are not important direct impacts from the provision of road networks, bridges, water supply systems, sewerage facilities, and the host of other infrastructure services. Similarly, there are presumably a wide array of capital expenditure projects that would survive a rigorous benefit-cost examination. Instead, the main message is that the use of aggregate data do not reveal sufficiently large linkages between public sector capital and private production activities to support the contention that government capital spillovers are the source of economy-wide variations in private productivity. Instead, future research in this area should be devoted to making more precise the microeconomic linkage between the provision of infrastructure and the nature of the production process.

Table 1
Summary Statistics for State-Level Data

Variable	Mean	Standard Deviation	Minimum	Maximum
Log Private Output	10.36	1.03	8.25	12.94
Log Private Employment	13.71	1.04	11.36	16.13
Log Private Capital	10.57	0.93	8.31	12.84
Log Public Capital	9.66	0.95	7.85	11.87
Growth of Private Output	2.97%	4.17%	-11.77%	16.98%
Growth of Private Employment	2.29%	3.31%	-10.82%	14.14%
Growth of Private Capital	3.10%	3.35%	-14.16%	24.18%
Growth of Public Capital	2.08%	1.71%	-1.25%	10.93%

Table 2
Summary Statistics for Region-Level Data

Variable	Mean	Standard Deviation	Minimum	Maximum
Log Private Output	12.48	0.69	10.80	13.41
Log Private Employment	15.81	0.70	14.05	16.77
Log Private Capital	12.60	0.66	11.20	13.66
Log Public Capital	11.73	0.66	10.24	12.54
Growth of Private Output	2.91%	3.43%	-6.24%	9.58%
Growth of Private Employment	2.29%	2.92%	-4.65%	8.82%
Growth of Private Capital	3.13%	2.83%	-8.98%	15.11%
Growth of Public Capital	1.99%	1.37%	-0.37%	4.95%

Table 3*

Estimates of State Production Function Dependent Variable:
Log Private Gross State Product

Variable	(1) OLS	(2) FIX	(3) FIX	(4) LONG	(5) LONG	(6) GLS	(7) GLS	(8) IV	(9) IV	(10) HNR	(11) HNR
Log Labor	0.497 (0.0144)	0.691 (0.0262)	--	0.643 (0.137)	--	0.659 (0.0225)	--	0.542 (0.0747)	--	0.911 0.0530	--
Log Private Capital	0.359 (0.0112)	0.301 (0.0302)	--	0.504 (0.142)	--	0.361 (0.0233)	--	0.472 (0.0653)	--	0.106 (0.0253)	--
Log Public Capital	0.203 (0.0190)	-0.0517 (0.0267)	-0.0557 (0.0228)	-0.115 (0.126)	--	0.00770 (0.0235)	0.0212 (0.0125)	-0.0150 (0.0660)	--	-0.102 (0.0606)	
Log Private Capital/Labor	--	--	0.306 (0.0245)		0.480 (.0113)	--	0.346 (0.0203)	--	0.685 (0.106)	--	0.104 (0.0252)
Log Public Capital/Labor	--	--	--		-0.130 (0.113)	--	--	--	-0.0174 (0.0821)	--	-0.0432 (0.0404)
Time Effects	Yes	Yes	Yes	No	No	Yes	Yes	Yes	Yes	Yes	Yes
State Effects	No	Fixed	Fixed	No	No	Random	Random	No	No	Differences	Differences
CRTS - Private[a]	60.32^{+++}	0.08	--	1.32	--	0.76	--	0.06	--	0.14	--
CRTS - Public[b]	250.42^{+++}	5.15^{++}	--	0.09	--	5.32^{++}	--	0.02	--	1.68	--

*For definitions of variables, see text.
[a]Test statistic for the hypothesis that coefficients on private labor and private capital sum to unity.
[b]Test statistic for hypothesis that coefficients on private labor, private capital, and public capital sum to unity.
$^{+++}$Significant at 1 percent level.
$^{++}$Significant at 5 percent level.
$^{+}$Significant at 10 percent level.

Table 4*

Estimates of Regional Production Function Dependent Variable:
Log Private Gross Regional Product

Variable	(1) OLS	(2) FIX	(3) FIX	(4) GLS	(5) GLS
Log Labor	0.563 (0.0453)	0.722 (0.0510)	--	0.698 (0.0451)	--
Log Private Capital	0.253 (0.0208)	0.272 (0.0566)	--	0.312 (0.0442)	--
Log Public Capital	0.201 (0.0479)	-0.120 (0.0418)	-0.123 (0.374)	-0.0497 (0.0372)	--
Log Private Capital/Labor	--	--	0.276 (0.0501)	--	0.348 (0.0387)
Log Public Capital/Labor	--	--	--	--	-0.0231 (0.0324)
Time Effects	Yes	Yes	Yes	Yes	Yes
Region Effects	No	Fixed	Fixed	Random	Random
CRTS - Private[a]	15.54^{+++}	0.03	--	0.10	--
CRTS - Public[b]	3.66^{+}	9.45^{++}	--	2.25	--

*For definitions of variables, see text.
[a]Test statistic for the hypothesis that coefficients on private labor and private capital sum to unity.
[b]Test statistic for the hypothesis that coefficients on private labor, private capital, and public capital sum to unity.
+++Significant at 1 percent level.
++Significant at 5 percent level.
+Significant at 10 percent level.

Endnotes

1. Just what is "quantitatively important" is in the eye of the beholder. See Holtz-Eakin [1988].

2. See also Duffy-Deno and Eberts [1989] and Eberts [1986, 1990a, 1990b] for studies of regional and municipal data.

3. In independent work, Eisner [1991] addresses some of these issues. His investigation, however, provides no formal test for the presence of state-specific error components, does not attempt to identify the parameters using lower frequency time-series variation, and does not explore aggregation to the regional level.

4. A production function in which the level of public-sector capital influences the technical efficiency index produces a specification such as that in equation (2.1).

5. Munnell [1990b] does not use time-effects, instead including the unemployment rate as a measure of cyclical conditions. The results presented below suggest that this choice has little impact on the estimates of the other coefficient.

6. See Hsiao [1986] for a thorough discussion of these issues.

7. The time-effects are treated as fixed throughout.

8. The regressions contain time dummy variables. As a result, correlation between the regressors does not stem from common movements over time.

9. See Nickell [1981].

10. Using first differences has the additional advantage of eliminating unit roots and common trends in the data.

11. One can use either the twice-lagged levels or the twice-lagged first-differences. In this application, the choice has little effect on the estimates reported below. Note that using the lagged levels as instrumental variables retains the use of cross-sectional information.

12. Aschauer [1989], for example, assumes CRTS. Munnell [1990] presents both unrestricted estimates those from imposing the constraint, but presents no test of whether the data are consistent with the constraint.

13. Munnell [1990b] and Garcia-Mila and McGuire [forthcoming] employ Gross State Product, thus including the "output" of the government sector in each state. Eliminating this component focuses on the spillover of public capital onto private output. In this way, the estimates below reflect the "excess" return to public capital. (See Griliches [1979] for a discussion of this issue in the context of research and development expenditures.) In principle, this seems preferable, but in practice it appears to have little effect on the estimates.

14. I thank her for providing these data.

15. Public sector capital stocks are estimated using the perpetual inventory method. Benchmark capital stocks are imputed to each state, and constrained to sum to equal the BEA estimate of aggregate capital in 1960. Holtz-Eakin [forthcoming] describes the construction of these data and the differences with the estimate of Munnell in greater detail. In practice, the use of these estimates yields essentially the same results as those obtained using the capital stock series constructed by Munnell.

16. One might wish to resist attention to certain categories of "infrastructure" capital such as highways, sewers, etc. Using such a measure of capital has little effect on the basic message of this paper.

17. Using capital for highways, sanitation and sewerage, and water supply as a measure of "infrastructure," the estimated coefficient is 0.086 with a standard error of 0.016.

18. Omitting the time effects from the equation has essentially no effect on the estimates.

19. Imposing CRTS on the basis of *a priori* considerations does not eliminate the apparent importance of public capital. The coefficient is 0.058 when CRTS in private inputs is imposed and 0.136 when CRTS in all inputs is imposed. In both cases the coefficient is precisely estimated.

20. The estimate that results when using the infrastructure capital stock is -0.0354 with a standard deviation of 0.0230.

21. The use of infrastructure capital produces a point estimate of -0.00155 with a standard deviation error of 0.0209.

22. State-specific effects are an important fraction of the overall variance. Using column (5), for example, the f_s contribute roughly 85 percent of the variance of ε_{st}.

23. The asymptotic properties of the HNR estimator are derived as the cross-section size approaches infinity. With only 48 states, there is reason to be cautious concerning the adequacy of the sample size.

24. Using these data, one cannot reject the null hypothesis of uncorrelated region-specific effects, thus reducing the importance of these estimation strategies.

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