Assessing the Productivity of Public Capital with a Locational Equilibrium Model

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Abstract: This paper employs Roback's locational-equilibrium model of public-goods pricing, cross-sectional data from the Census of Population and Housing, and SMSA-level estimates of public capital stocks in order to examine the productive contribution of public capital. I find that public capital has a small positive impact on private output.

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1. Introduction

The provision of public goods is a basic function of government. Perhaps the most visible type of public good is government capital, which includes such items as infrastructure--roads, bridges, sewers, and so on--as well as other government buildings and equipment. A natural question to ask is whether and to what extent publicly provided capital affects private-sector production.

In recent years, a number of empirical studies have attempted to quantify the relationship between public capital accumulation and private-sector output and productivity growth. Almost without exception, these studies proceed by estimating a cost or production function using time-series or longitudinal data, and then measuring the shadow value or output elasticity of government capital. The resulting estimates of public capital productivity vary widely, with some authors obtaining a significant measured effect of public capital on private output and others finding essentially no effect.[1]

A fundamental problem associated with the standard methodology is that it requires data on inputs and input prices. Consider a typical estimation strategy, which involves fitting a production or cost function to a panel of U.S. states. In order to do this, one needs state-level measures of prices and/or quantities of public and private capital and labor inputs; however, there are almost no data on private capital stocks or total private investment at the state or local level. In constructing their own measures of these variables, researchers have therefore been forced to take one of two routes: either they confine their attention to the manufacturing sector, for which estimates of investment exist, or they attempt to build state-level capital stock series for all sectors by somehow apportioning national estimates among individual states. Neither approach is terribly palatable, however--manufacturing represents less than twenty percent of aggregate production in the United States, and the allocation schemes used to compute total state capital

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[1] Canonical examples of the production function approach are Aschauer (1989) and Munnell (1990); both authors find large output elasticities of public capital. Garcia-Milà and McGuire (1992) present evidence that highway capital has a significant positive effect on output. Research in the production function vein that finds little or no evidence of public capital productivity includes Evans and Karras (1994) and Holtz-Eakin (1994), among others. The cost function approach is exemplified by Lynde and Richmond (1992) and Morrison and Schwartz (1996); both studies find significant productive effects of public capital on private output. Except for the papers by Aschauer and Lynde and Richmond, which use aggregate U.S. data, all of the studies listed above employ state-level data. Two papers by Eberts (1986, 1990) examine evidence on public capital stock productivity using SMSA-level data.

Exceptions to the typical production/cost function approach are provided by Holtz-Eakin and Lovely (1995) and Fernald (1999). The first paper develops a general-equilibrium model in order to assess the effect of public infrastructure on industry- and firm-level returns to scale, as well as on output variety; the second examines whether increased road and highway capital has a larger productive effect on industries that employ relatively more motor vehicles. An important contribution of these papers is that they attempt to highlight the specific channel through which public infrastructure influences the private production process, and are therefore better able to assess whether a causal relationship between public capital and private-sector productivity is at work.

stocks yield series which are probably not well suited for time-series or longitudinal analyses.[2]

These studies also suffer from the usual specification problems that attend the estimation of production or cost functions. In many cases, estimates of the productive contribution of public capital are sensitive to the inclusion of state-specific fixed effects and choice of functional form.

In this paper, I use a somewhat different approach in order to assess the productivity of publicly provided capital stock. Specifically, I employ a variant of Roback's (1982) model of public goods pricing, which uses measured interregional worker wage and land rent gradients in order to assess the productive and/or amenity value of a site-specific public good. The basic advantage of this method is that it does not require estimates of private capital or labor inputs; although it does require data on regional public capital stocks, existing measures are less objectionable than those for private capital. I can therefore obtain an "independent" estimate of the productive effect of private capital, that is, one which does not directly rely on a production or cost function estimate. In addition, a minor contribution of this paper is its use of the Roback model for something other than the computation of quality-of-life indexes; while the model has demonstrated its value in such exercises, its potential usefulness in assessing the productive value of site-specific characteristics has remained relatively unexplored.[3]

The balance of this paper is organized as follows. In section two, I outline a variant of the Roback model and describe how it can be implemented empirically using cross-sectional data from the U.S. Census. Section three presents the paper's findings, which are based on urban-level data. Finally, section four compares my estimates of public capital stock productivity with those found by previous researchers, notes several caveats regarding the interpretation of my results, and provides suggestions for additional research.

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[2] For example, Munnell (1990) constructs a state-level private capital stock series by allocating a share of the BEA's estimate of the national capital stock to each state; for an individual state, the share used is computed either as the fraction of national output or of the national *book* value of capital stock (as reported in the quinquennial industrial censuses) accounted for by the state's industries. Note that, because these shares change only once every five years, the growth rate of capital in a state over a five-year period is a constant fraction of the growth rate of the aggregate capital stock over that period. Furthermore, using an output-based share to allocate aggregate capital is justified only if the average productivity of capital is identical across all states; this need not be the case in a locational equilibrium, however. Nevertheless, numerous state-level studies--including Holtz-Eakin and Lovely (1995)--have employed this measure of state private capital stocks.

[3] Beeson and Eberts (1989) and Rauch (1993) provide notable exceptions.

2. Methodology

The theoretical framework that I employ in this paper is a modified version of the canonical Roback public goods pricing model.[4] Essentially, the Roback model is a spatial-equilibrium model in which land rents and wages differ across regions according to the presence or absence of site-specific characteristics. These site-specific characteristics can be productive or unproductive; they can also have amenity value, *i.e.*, they can affect consumers' utility directly as opposed to affecting utility solely through their effect on wages and land rents. An empirical implementation of the model involves fitting hedonic wage and rent regressions, that is, regressions that relate worker wages and housing expenditures (in lieu of land rents, which are not readily observed) to worker and dwelling characteristics and to a set of site-specific characteristics. As I will show, this can in principle allow me to calculate the implied productive effect of the site-specific characteristics from the characteristics' estimated influence on wages and rents. The remainder of this section describes this procedure in detail.

Theoretical model

The model assumes a world in which there are two classes of agents, workers and firms. Firms produce a traded composite consumption good, whose price is normalized to unity and set in a national or world market. The model is frictionless in the sense that workers and firms (labor and capital) are assumed to be able to migrate freely across regions. Firms in the traded-good sector have identical constant-returns-to-scale production technologies, while workers have identical preferences and inelastically supply a single unit of labor in return for a wage w. Both workers and firms demand land; for workers, this is used in the production of housing services, while for firms land is an input in the production function. The rental cost of a unit of land is denoted by r. Finally, regions are characterized by a vector s of site-specific characteristics.

The problem facing a representative worker is to maximize utility subject to his or her income (which is the wage w) and the rental price of land r. Utility can also depend on the site-specific characteristics s if they have positive or negative amenity value. In equilibrium, there is no incentive for workers to migrate; this implies that utility is equalized across regions. If we write the worker's indirect utility function as $v(w,r;s)$, this condition can be expressed as:

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[4] The modifications are due to Beeson and Eberts (1989) and Gyourko and Tracy (1991). The discussion in this section summarizes (with emendations) the relevant portions of these papers, as well as of Rauch (1993).

Isoutility locus: $$v(w,r;s) = v_o\,, \tag{1}$$

where v_o denotes the common level of utility across locations.

Firms are characterized by a unit cost function $c(w,r;s)$ which is allowed to depend on the site characteristics s if they augment or detract from productivity. The equilibrium condition for firms is that there be no incentive to locate elsewhere; this implies that unit costs for the traded-goods sector are equal to one (the price of the traded good) everywhere, or:

Isocost locus: $$c(w,r;s) = 1. \tag{2}$$

Under the usual assumptions regarding the partial derivatives of $v(.)$ and $c(.)$, equations (1) and (2) determine a unique equilibrium rent and wage for a region conditional on the region's level of s. On a graph with land rents on the y-axis and wages on the x-axis, the isocost locus will slope down (higher wages in a region must be compensated for by lower rents in order to equalize costs), while the isoutility locus will slope up (higher rents raise housing costs and lower utility, and must be compensated for with higher wages). The equilibrium level of wages and rents in a region is given by the common intersection of the two loci in (r,w) space.

Variations in s over space induce wage and rent differentials across regions. The sign and magnitude of these differentials depend on whether the site-specific characteristic s is a productive or nonproductive amenity or disamenity. Rather than formally deriving the comparative statics of the system (see Beeson and Eberts (1989) for a complete treatment), I sketch an intuitive explanation of the model's predictions.

Amenities affect equilibrium wages and rents through their effects on utility. A site-specific characteristic that has amenity value--*i.e.*, a characteristic s for which $v_s'(.)$ is positive--shifts the (upward-sloping) isoutility locus back: for a given level of land rents, lower wages are accepted in high-amenity regions. This can be thought of as resulting from workers' willingness to migrate to high-amenity areas; the resulting increase in total labor supply and increased demand for land for housing drives wages down and rents up until the migration flow is eventually choked off. Disamenities have the opposite effect: the isoutility locus is shifted out in low-amenity locations.

A productive site-specific characteristic--a characteristic s for which $c_s'(.)$ is positive--shifts the isocost locus out. For a given level of land rents in an area, the presence of a productive s allows firms to pay higher wages, as workers and sites are more productive. The resulting increase in demand for land and labor pushes up land rents and wages until there is no more incentive for firms to move to the area in order to take advantage of the productive area characteristic.

Finally, site characteristics with both amenity value and productive value will affect both loci. To summarize:

1. An increase in the level of a productive site characteristic that has no amenity value should raise wages and rents in a region (the isocost locus shifts out along a fixed isoutility schedule);

2. An increase in the level of a pure amenity raises rents and lowers wages (the isoutility locus shifts back along a fixed isocost curve); and,

3. An increase in the level of a productive characteristic that is also an amenity unambiguously raises land rents, but the effect on wages is uncertain (the isoutility curve shifts back while the isocost locus shifts out).

The model can, in principle, be used to test whether observed wage and rent gradients are occurring along a fixed isoutility curve, that is, whether a given characteristic *s* has no amenity value. Consider the derivative of (1) with respect to *s*:

$$v_w'.(dw/ds) + v_r'.(dr/ds) + v_s' = 0. \tag{3}$$

If *s* has no amenity value, $v_s' = 0$ by definition. Let L_H denote the quantity of land consumed for housing, and note that total income is equal to *w* under the assumption that labor is inelastically supplied.[5] Divide (3) through by *w* and $v_w'(.)$ and apply Roy's identity; this yields:

$$(d \ln w)/ds + (r.L_H/w).(d \ln r)/ds = (d \ln w)/ds - \sigma_L.(d \ln r)/ds = 0 \tag{4}$$

where σ_L is the share of land rents in labor income.

In a similar fashion, the productive effect of a characteristic *s* can be assessed by manipulating the isocost locus (2). Differentiating (2) with respect to *s* gives:

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5 The analysis is unchanged if consumers also receive capital income so long as portfolios are sufficiently diversified. This is required in order to ensure that the capital income of a region's residents is not affected by changes in their region's site characteristics.

$$c_w'.(dw/ds) + c_r'.(dr/ds) = -c_s'.$$

Applying Shephard's lemma to this equation yields an expression for the percentage reduction in costs that results from an increase in a productive characteristic *s*:

$$\theta_N.(d \ln w/ds) + \theta_L.(d \ln r/ds) = -c_s'(.)/c(.), \tag{5}$$

where θ_N and θ_L denote the cost shares of labor and land, respectively. Setting $c_s'(.)$ equal to zero provides a test of the proposition that a site-specific characteristic does nothing to augment productivity in a region. In order to implement this test and the test defined by equation (4), above, I require estimates of the parameters σ_L, θ_N, and θ_L; I use national averages, which I calculate either from the Census sample directly or from national accounts or other data.

I require an additional refinement to this basic setup in order to render it suitable for estimation.[6] The derivation above was expressed in terms of land rents, which are typically unobserved; instead, I have data on housing expenditures. The two can be related as follows. Let total housing expenditure $p_H.Q_H$ be denoted by E_H; following previous authors, define it to be the sum of three components: land expenditures $r.L_H$, structures expenditures $p_B.Q_B$, and expenditures on utilities $p_U.Q_U$ (utilities include such items as water, electricity, and fuel). Then, under the assumption that all variation in total housing costs results from differences in land rents, the expression

$$(d \ln E_H)/ds = \gamma.(d \ln r/ds) \tag{6}$$

relates the log derivatives of housing expenditures and land rents, where γ represents the ratio of land rents to total flow housing costs, or

$$(r.L_H)/(r.L_H + p_B.Q_B + p_U.Q_U).$$

A potential problem arises in that utilities costs are likely to be affected by the amount of public

[6] The model can be extended in several ways, *viz.*, it can be modified to allow for nontraded goods, the explicit production of housing, and a nontrivial labor supply decision by consumers; see Roback (1982) and Beeson and Eberts (1989) for details. The empirical implementation of these extensions is not feasible given available data, however.

capital--specifically, public infrastructure capital--that is present in an area; in this case, the assumption that variation in housing expenditures results solely from variation in land rents is invalid. This suggests that we should use a measure of housing expenditures *exclusive* of utilities costs, in which case the definition of γ must be modified accordingly. I return to this point below when I discuss the model's empirical implementation.

Theoretical effects of local-area taxation

In order to use the model outlined above to assess the productivity of public capital, we must address a further complication, namely, the presence of differential rates of taxation across regions. The Roback model is designed to measure the effect of publicly available goods on interregional wage and rent gradients. Goods provided by governments are typically financed through current or future taxation, however.[7] While the existing level of public capital stock can be taken as predetermined in terms of firm and worker location decisions, it is *a priori* plausible that areas with high levels of public capital will also face large current tax burdens, the intuition being that areas with large public capital stocks (say, in per-person terms) might tend to be areas in which government, including taxation, is on a larger scale as well.

The presence of local-area taxation presents two separate problems. First, the values *dr/ds* and *dw/ds* in the derivation above represent partial derivatives inasmuch as they describe the effect of a change in a site-specific characteristic holding fixed all other characteristics. The level of taxation, however, can be interpreted as a site-specific characteristic as well--one which is very likely to be correlated with public capital levels--and so must be controlled for in an empirical analysis.

Second, taxes will directly enter the model's formulation, since the presence of corporate and personal income taxes, excise and sales taxes, and property taxes will affect workers' utility and firms' costs. The required modifications involve equations (4) and (5). Assume that income and property taxes at site *i* are given by τ_i and π_i, respectively.[8] The proper statement of the condition expressed by equation (4) is then:

$$(1-\tau_i).(d \ln w)/ds - (1+\pi_i).\sigma_L.(d \ln r)/ds = 0. \qquad (4')$$

7 An exception arises when a region receives grants from a higher level of government.

8 Although sales taxes enter the indirect utility function through their effect on the price of the traded consumption good, they can be ignored here so long as the change *ds* in the site-specific good occurs with taxes held constant.

Similarly, the cost shares θ_N and θ_L in equation (5) must be corrected for the presence of indirect taxes.[9]

Empirical implementation

My goal is to obtain estimates of $(d \ln w)/ds$ and $(d \ln r)/ds$, where s denotes a measure of the flow of services from a region's public capital stock. As mentioned above, these derivatives should be *partial* derivatives in that we want them to capture the effect of a change in public capital on wages and rents holding all other site-specific characteristics constant.

I follow what has now become a more-or-less standard procedure for implementing the Roback model. This procedure involves using individual-level data to fit hedonic regression models that relate log worker wages to worker characteristics and log housing expenditures to housing characteristics. I include measures of region-specific public capital in these reduced-form regressions, together with a portfolio of other potentially relevant regional characteristics. The required derivatives can then be recovered from the regression coefficients on the public capital terms.[10]

The wage regressions are modified Mincerian earnings equations of the form

$$\ln w_{ij} = \beta_w . X_{ij} + \delta_Z . Z_j + \delta_K . K_j + u_{ij}, \tag{7}$$

in which the wages w of individual i in region j are projected on his or her own characteristics X_{ij}, a measure of public capital K_j, and other site-specific characteristics Z_j. The hedonic regressions for housing expenditures E_H take a similar form, namely,

$$\ln E_{H,\,ij} = \beta_H . H_{ij} + \rho_Z . Z_j + \rho_K . K_j + e_{ij}, \tag{8}$$

where expenditures on housing unit i in region j are projected on the unit's own characteristics H_{ij} and the same set of site-specific variables that enters the wage regressions. My analysis proceeds at the urban

9 Gyourko and Tracy (1989) develop a version of the Roback model that explicitly incorporates local-area public finance considerations. I describe the predictions of their model and compare their empirical findings to my own results in section three, below.

In practice, the average state and local income tax rate is very small (about 2.5 percent in 1979), while measures of property tax rates that correspond to the theoretical concept cannot be obtained. I therefore use equation (4), rather than (4′), in the empirical analysis.

10 Strictly speaking, the regression coefficient from the housing regression is an estimate of $(d \ln E_H)/ds$; the expression in equation (6) relates this to the desired derivative $(d \ln r)/ds$.

level; a region is therefore defined to be a Standard Metropolitan Statistical Area (SMSA). The basic source of data for earnings, housing expenditures, and worker and housing unit characteristics is the 1980 Census of Population and Housing; data on public capital stocks, local-area public finance, and other SMSA characteristics were collected from outside sources (described in section three, below) and merged with the Census data.

The dependent variable for the wage regressions is the natural logarithm of average weekly wages, defined as wage and salary income in 1979 divided by weeks worked in 1979. The set of worker characteristics X_{ij} included in the regressions consists of years of completed schooling, potential labor market experience (defined as age *minus* years of completed schooling *minus* six) and its square, sex, race, marital status, disability status, and 52 industry indicators (which roughly correspond to industry definitions at the two-digit SIC level). I also include interactions of sex with potential experience, marital status, and race, and control for whether a worker resides in a central city. An individual enters the sample if he or she meets certain criteria for minimum and maximum earnings, average weekly wage, reported industry, and age.[11] I further restrict the sample used in the wage regressions to include full-time workers (*i.e.*, workers who were employed for at least 50 weeks and who reported working more than 25 hours per week on average in 1979) and workers whose place of residence is the same as their place of work; these restrictions are informed by the theoretical model, which assumes that workers supply labor inelastically and face the same region's labor and housing markets. I also omit government workers from the wage regressions; this follows Gyourko and Tracy (1991), who argue that the level of taxes and government services in an area do not exogenously determine the wages of workers in the public sector.[12]

The choice of a dependent variable for the hedonic housing regressions presents a more complicated problem. For renter-occupied housing, the Census reports two measures of housing expenditures: contract rent (the agreed-upon rent for the housing unit) and gross rent, which is calculated as

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11 These criteria are that wages be neither topcoded nor imputed and that workers reported their industry, had nonnegative potential labor market experience, were between 16 and 65 years of age, and had wage income greater than 101 dollars and average weekly wages less than or equal to 2,500 dollars and greater than or equal to 36 dollars. The wage thresholds are the same ones used in Card and Krueger (1992).

12 It is possible to explicitly model commuting and the labor supply decision--see Hoehn, *et al.* (1987) and Beeson and Eberts (1989); these extensions are not interesting in this context, however. In practice, the only sample restriction that has any significant influence on the wage regressions is the requirement that workers be employed full-time; omitting government workers and commuters has virtually no effect. Dropping commuters from the sample has little impact since cross-SMSA commuters typically account for only a small percentage of all workers (about four percent for my sample of SMSAs); this is presumably because commuting sheds are taken into account in defining SMSA boundaries. Note, though, that only half of all Census respondents are asked questions about place of work; hence, the number of observations is automatically halved by screening commuters from the sample.

contract rent *plus* reported utility and fuel costs paid by the renter. For owner-occupied housing, the Census reports figures on house value (land and structure), together with data on utility and fuel purchases.

Several previous studies (Beeson and Eberts (1989), Blomquist, Berger, and Hoehn (1988), and Rauch (1993), for example) employ gross rent in the housing hedonics, with gross rent for owner-occupied units computed as the sum of a housing service flow (imputed from the reported value of the house) and the cost of utility and fuel purchases. While the rationale for using this measure is not discussed, it appears to be largely informed by data considerations. Contract rent, as defined by the Census, can include utility and fuel purchases if utilities are included as part of the rent. It is not possible to separate these utility charges from the reported contract rent figure; thus, the only way to ensure consistency is to measure all housing expenditures as inclusive of utility payments. As I noted above, however, this makes it difficult to assess the effect of site-specific characteristics on land rents if the characteristics also affect expenditures on utilities.[13]

A second alternative involves only using an imputed service flow based on house value in the regressions (as is done by Gyourko and Tracy (1991), for instance); this requires us to confine our attention to owner-occupied housing. By excluding renter-occupied housing we lose almost half of all housing units, and also restrict the sample in a potentially relevant way. We do, however, obtain a measure of housing expenditures that is closer to the theoretically preferable sum of land rents and structures costs.[14] In addition, this measure is free of the measurement error that is present in reported utility expenditures (see U.S. Bureau of the Census (1983), pp. K14, K41).

As neither alternative is unambiguously preferable, I experiment with both measures: first, the natural log of gross rents (with gross rents for owner-occupied units constructed by adding reported monthly utility and fuel costs to an imputed monthly contract rent value, computed as the reported value of the house times an estimated user cost) and second, the natural log of house values as reported for owner-occupied units.[15]

The set of housing characteristics H_{ij} included in the regressions consists of data on condominium

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[13] The choice of a gross rent measure by previous authors is particularly odd given their preoccupation with estimating the amenity value of climate in constructing quality-of-life measures.

[14] In the loglinear regression specifications I employ, it does not matter whether the dependent variable is house value or imputed housing service flows, since the user cost figure used to impute the service flow is identical for all regions.

[15] The user cost was defined to be 7.85 percent per annum (converted to a monthly rate), and is based on estimates by Peiser and Smith (1985) (see column 9 of their table 2). Rent and house values are collected and reported as intervals; following Census practice, the categories are converted to dollar values using the midpoint of the interval. See the data appendix for details.

status, lot size, number of floors, rooms, bedrooms, and bathrooms, the year in which the housing unit was built, the number of units located at the same address, whether the unit is located in a central city, the unit's water source and type of sewage disposal facilities, and whether the unit has an elevator, central air and/or central heating. In most cases, these variables are categorical and are therefore entered as unrestricted indicator terms. Regressions that include renter-occupied units contain an indicator for renter status; I also interact this indicator variable with all other variables in the regression. I include a housing unit in the sample if its rent or value is reported, if its lot size is less than 10 acres, if there is no commercial or medical office on the unit's property, and if the unit is not a group quarters. I also omit tents, boats, and mobile homes.

It is now well known that the estimated standard errors of coefficients on variables that take on constant values within subgroups are biased downward when estimated viâ OLS. In my regressions, public capital values K_j and site characteristics Z_j are equal for all observations in a given SMSA; hence, the *t*-statistics for these variables' coefficients will be overstated by OLS, typically to a very large degree.[16] In the results reported below, I have corrected for this problem by estimating (7) and (8) using the Huber routine from the STATA statistical package.

It is worth emphasizing that the assumptions of the Roback model--identical preferences and identical production technologies across all regions--are rather heroic. For example, the presence of self-sorting through selective migration might imply that individual preferences in a region will vary in nonrandom ways, particularly in ways that are related to the site-specific characteristic *s*. Our confidence in the empirical results therefore depends on whether we believe that the independent variables in the hedonic regressions provide sufficient controls for such factors--which is another way of saying that the methodology requires the estimated derivatives $(d \ln w)/ds$ and $(d \ln r)/ds$ to be true measures of the partial effect of the site characteristic on wages and land rents.

3. Results from SMSA-level data

I begin by presenting estimates of the hedonic wage and rent regressions. I use an extract from the one percent Census public-use tape (the "B" sample) for the regressions; the sample includes 40 SMSAs for which public capital stock data are available. Cell sizes for the wage regressions range from a minimum of

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[16] See Moulton (1986) and Deaton (1994) for a discussion of this problem.

246 observations to a maximum of 7,312 observations; cell sizes for the rent regressions range from 751 to 30,041 observations for the regressions that include both renter- and owner-occupied units and from 225 to 11,588 observations for the regressions that include owner-occupied units only. In all, there are 71,442 observations in the wage regressions and 254,291 observations in the rent regressions (a total of 139,394 observations remain when renter-occupied units are omitted).[17]

The data on public capital stocks are described in Eberts, Park, and Dalenberg (1986) and cover 40 SMSAs for which there exist sufficiently long public investment series.[18] The capital stocks are computed using the perpetual inventory method, and are expressed in 1967 dollars. The dataset also contains series for several subcategories of public capital, namely, water distribution and treatment, street and highway, and sewer capital stocks. I employ the natural logarithm of each measure's per-capita value in 1979 in the regressions; note that this implicitly assumes that the service flow from public capital is proportional to the stock.[19]

I also include a portfolio of tax controls in the regressions. Data on government revenues at the SMSA level are only available quinquennially in the *Census of Governments*; I use estimates from the 1977 *Census*, which covers the 1977 fiscal year (calendar years 1976-1977). Ideally, the tax measures should be expressed as average rates; in practice, however, it is impossible to obtain estimates of tax rates for SMSAs, which are typically composed of a central city and one or more counties and which sometimes cross states. I therefore consider two classes of tax controls: the first includes measures expressed in log per-capita terms, while the second deflates by SMSA personal income, thereby coming closer to the notion

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17 I should note that it is a somewhat disingenuous to note the regressions' large sample sizes, since the parameters of interest--the coefficients on the public capital stock variables--are effectively estimated with only 40 degrees of freedom (this being the number of SMSAs covered by the sample). Having a large number of observations in the regressions is useful only in that it yields more precise estimates of the coefficients on individual worker and housing characteristics, and therefore generates more precise estimates of the conditional means of wages and rents across SMSAs.

18 I thank Randall Eberts for his kindness in providing me with these data.

19 It is not theoretically clear whether public capital enters the production function in per-worker or per-capita terms. I prefer a per-capita measure, as the entire population of a region has access to its supply of public capital (and thus contributes to its congestion); similarly, public capital is not exclusively used in private production. In practice, the distinction is immaterial as regional population and workforce are nearly collinear.

In assuming that the flow of capital services is proportional to the stock, I am ignoring issues raised by Hulten (1996) and Boarnet (1997) concerning the effectiveness of infrastructure utilization.

Finally, a further problem with the capital stock data is that some infrastructure capital is owned by regulated private utilities, and is not therefore considered "public" capital stock. This appears to only affect one subcategory (water infrastructure) in a very limited number of cases; unfortunately, there is no remedy for the problem.

of an average rate.[20] For all tax controls, I combine the measure for an SMSA with the corresponding measure for the state(s) in which the SMSA is located, the rationale being that the location decision of firms and workers should depend on the overall level of taxation prevailing in a region.[21]

Altogether, I use five sets of tax controls: first, the natural logarithm of general revenues (from own sources) per capita; second, log nontax, property, and nonproperty tax revenues per capita; third, the ratio of general revenues to personal income; fourth, the ratio of property tax and other revenues to personal income, and fifth, the ratios of income tax, sales and gross receipts taxes, property tax, and other revenues to personal income. The data appendix contains additional details regarding the construction of these series.

Wage regression results

The first column (column zero) of table 1 reports the coefficients on the log public capital terms when no tax controls are included in the regression. (The coefficients on the individual worker characteristics are unsurprising and not reported.) A larger total capital stock in an SMSA raises the wages of workers in the SMSA by a small amount: 0.07 log points for each log point increase in public capital per capita. When the total stock is separated into infrastructure and noninfrastructure capital, the overall effect is slightly larger, with both categories acting to raise wages. Among the infrastructure components, only street and highway capital appears to have an effect on wages.[22]

Columns one through five in the table report the public capital coefficients that obtain when various tax measures are included in the regressions. Adding the log of general revenues per capita (column one) reduces the point values of the coefficients sharply, rendering almost all of them insignificant at conventional levels; a noticeable exception is the coefficient on highway capital, which is unaffected by

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[20] Of course, a proper measure of the tax rate would not use personal income as the base; for example, the correct definition of the average property tax rate is the ratio of property tax collections to the total value of real and personal property, while the sales tax rate should have the total value of taxable sales activity in the denominator. Even income taxes should be expressed relative to individual and corporate income, not personal income. Unfortunately, such data are unavailable, so I compromise by using personal income.

[21] When an SMSA is located in more than one state, I weight the state-level tax rates by the share of SMSA population located in each state.

[22] I also experimented with different infrastructure specifications which break out each infrastructure term separately, *i.e.*, water capital *versus* nonwater capital, highway capital *versus* nonhighway capital, and so on. The coefficients on the infrastructure terms (not reported) were quite similar to those presented in table 1, which is unsurprising considering that there is little correlation between the individual infrastructure components.

any of the tax controls. When log general revenues per capita are separated into property tax, nonproperty tax, and nontax components (column two), the impact on the public capital coefficients is lessened--the coefficient on infrastructure capital actually rises slightly relative to the benchmark specification with no tax controls. In the regressions summarized by columns three through five of table 1, I include tax measures which are expressed as shares of SMSA personal income. The public capital terms manifest little variation across these specifications.

The data in table 1 indicate that the only tax control with any major effect is the log of general revenues per capita. There are two reasons, however, to be wary of using a per-capita measure of tax collections as a proxy for the tax burden faced by a region's residents. First, revenue per capita is highly correlated with personal income per capita, so that even if public capital does positively affect regional productivity (and therefore regional incomes), some of the effect could be washed away by including per-capita revenues. Second, what matters in terms of an area's tax burden is the relation of taxes to one's ability to pay--even if taxes are a constant share of income, revenues per capita will vary across SMSAs to the extent that the level of personal income per capita does.[23]

It is worth considering the effect of the tax controls themselves; table 2 summarizes the coefficients on these terms. Of the per-capita measures, higher property taxes tend to raise wages, while nonproperty taxes tend to lower wages; the fact that the two components enter with different signs suggests that it is improper merely to include a single, combined general revenues term. The results obtained from the specifications that include tax controls that are expressed as ratios to personal income also indicate that a detailed breakdown is best--the overall general revenues term has a statistically insignificant negative effect on wages; however, this masks a negative effect of nonproperty tax revenues (particularly income tax revenues) and a positive effect of property taxes.

Do the signs and magnitudes of the tax terms conform to what we would expect *a priori*? In a series of papers, Gyourko and Tracy (1989, 1991) assess the effect of local-area taxation on wages and rents in the central cities of U.S. SMSAs. The model developed in their paper is a straightforward extension of the basic Roback formulation; its conclusions can be summarized as follows. First, higher personal income tax rates should raise wages in a region; the idea is that these taxes are like disamenities that do not affect firms' costs directly (because a firm's concern is with the pretax wage). Higher *corporate*

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[23] On the other hand, if regional public capital rises with regional income or wealth *pari passu* (*i.e.*, some variant of Wagner's law is at work), then the correlation between wages and public capital could merely reflect public capital's proxying for how well-off a region is. In that case, revenue per capita is probably a better measure to use.

income tax rates, on the other hand, shift the isocost locus inward, thus reducing wages.[24] The observed reduction in wages that attends higher income taxes can therefore be reconciled with the model's predictions if we assume that the negative effect of corporate income taxes overwhelms the positive effect of personal income taxes. Second, a sales tax is like a site-specific disamenity that also reduces firm productivity, and so has an ambiguous influence on wages; this is compatible with the statistically insignificant negative effect that I obtain. Finally, Gyourko and Tracy argue that property taxes will be completely passed through to land prices, and should therefore have no effect on wages. The fact that I find a positive relationship between property taxes and wages is therefore somewhat puzzling, although as I argue below this result is consistent with the type of omitted variable bias that is likely to be present in these estimates.[25]

Rent regression results

I now turn to the rent regressions, beginning with the specifications which employ gross rent (reported or imputed contract rent *plus* utilities expenditures) as the dependent variable. Table 3 reports the coefficients on the public capital terms from these regressions. When no tax controls are present (column zero of the table), the measured effect of total public capital stock on gross rent is positive. The contributions of infrastructure and noninfrastructure capital are roughly equal; note that the joint statistical significance of the two measures is quite high. Among the infrastructure components, water distribution and treatment capital and street and highway capital act to raise rents (the effect of streets and highways is statistically insignificant, however). Higher levels of sewer capital, on the other hand, appear to depress rents.

Including the taxation variables alters the results significantly. The log per-capita tax measures (see columns one and two of table 3) reduce the point estimates of many of the coefficients sharply, rendering them indistinguishable from zero at conventional significance levels. The other sets of tax controls also tend to reduce the measured effect of public capital on gross rents, although the coefficients

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24 How this works is somewhat easier to see in the context of a model that uses an indirect profit function instead of a unit cost function in order to characterize firm equilibrium. In the model described in section two, the effect of a corporate income tax acts through the suppressed cost-of-capital term. The required (posttax) return to capital is assumed to be set on a national or world market. Higher corporate income taxes reduce the posttax return to capital in a region, which means that the pretax return must rise; this acts like a positive cost shock and shifts the isocost locus back. Intuitively, capital (firms) will exit a high-tax region until the posttax marginal product of capital in the region rises to the level of the required rate of return; the resulting reduction in demand for land and labor depresses rents and wages.

25 It is important to note that the property tax measure used here suffers from two flaws. First, it is not properly a tax rate in that it is calculated relative to personal income, not property value. Second, property taxes do not only encompass taxes on real property, but also include levies on certain types of personal property that account for about 12 percent of the total base (it is not clear how--or whether--Gyourko and Tracy correct for this). Unfortunately, I am unable to compare my results with Gyourko and Tracy's as they exclude the property tax rate from their wage regressions on *a priori* grounds.

on infrastructure capital (particularly water and highway capital) remain positive and statistically significant when a detailed set of controls is used (see columns four and five).

Table 4 presents the coefficients on the tax variables. Although the ratios of income and sales tax revenues to personal income (set five) fail to enter individually, they are typically jointly significant, implying that a detailed breakdown is probably preferable.

There is disturbingly little congruence between the predicted effect of the tax variables on rents and the effect that is actually observed. According to the model, income, sales, and property taxes should all act to lower rents. In practice, however, the property tax terms have a strong and statistically significant *positive* influence on rents, while the effect of higher sales taxes is also to increase rents. It is not clear what drives these results. One possible explanation is that the ratio of property taxes to personal income proxies for an area's property values; it might also be the case that the property tax measures capture in part the overall level of government services provided in a region (recall that this tax measure also had a smaller, positive effect on wages). The fact that most of the tax measures have a larger effect on the coefficient for noninfrastructure capital gives further circumstantial support for the latter explanation, as there is more likely to be a direct connection between noninfrastructure capital and government service provision. Again, it is quite likely that this result stems from the presence of omitted variable bias; I discuss this possibility in more detail below.

I argued in section two that using gross rent as the dependent variable in the rent regressions is problematic inasmuch as gross rent includes utilities expenditures, which might be affected by the presence of public infrastructure. In the regressions summarized in table 5, therefore, I restrict the sample to owner-occupied housing and define the dependent variable to be the natural logarithm of reported house value. The coefficients from the house value regressions have a pattern of signs and significance levels that is very similar to that of their counterparts in the gross rent regressions.[26]

It is not surprising that the coefficients from the gross rent regressions are typically smaller in magnitude than the coefficients from the housing value regressions. If it is true that the only source of variation in gross rents and housing values is land rents, then the coefficients reported in tables 3 and 5 should be identical up to a scale term equal to the ratio of housing costs net of utilities expenditures to overall housing costs. This appears to be the case, and a battery of *t*-tests lends formal support, although

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[26] The same is true of the tax control coefficients (not shown).

the *t*-tests' inability to reject the hypothesis is largely thanks to the coefficients' generous standard errors.[27]

Effect of omitted variable bias

My analysis assumes that the coefficients on the public capital terms in the hedonic regressions capture the true partial effect of public capital on wages and land rents. However, the regressions are almost certainly contaminated by omitted variable bias. It is important, therefore, to assess the probable direction of this bias.[28]

Say that the true model is given by

$$y = X_1.\beta_1 + X_2.\beta_2 + \varepsilon,$$

but we erroneously omit the set of variables X_2 from the regression and instead estimate

$$y = X_1.b_1 + e.$$

The expected value of b_1 is then given by

$$E(b_1) = \beta_1 + (X_1{}'X_1)^{-1}X_1{}'X_2.\beta_2. \tag{9}$$

Note that each column of the matrix $(X_1{}'X_1)^{-1}X_1{}'X_2$ contains the vector of coefficients from a regression of the corresponding column of X_2 on the matrix of included variables X_1.

Now apply this general example to the hedonic regressions. Assume that we have already controlled for all relevant individual worker or housing characteristics, so that any remaining variation in the dependent variable is the result of site-specific factors.[29] I first consider a simple case where these

[27] The average value in my data of the ratio of housing costs net of utilities expenditures to overall housing costs (which can only be computed for owner-occupied housing units) is approximately 0.76. The largest *t*-statistic from the *t*-tests equals 1.4; note that the tests assume no correlation between the coefficients from the house value regressions and the coefficients from the gross rent regressions.

[28] The discussion of omitted variable bias that follows is standard; see Greene (1993, pp. 245-6), for example.

[29] The dependent variable *y* can therefore be thought of as the vector of coefficients that would obtain on a set of SMSA dummies if they were included in the wage or rent regressions. These fixed effect estimates will be unbiased so long as the coefficients on the worker and housing characteristics are uncorrelated with any omitted variables.

factors include government capital G_K, some measure of local-area taxation T, and an index of government service provision G_S. The true model is therefore

$$y = \beta_T.T + \beta_K.G_K + \beta_S.G_S + e, \tag{10}$$

where y denotes the (conditional) SMSA-wide mean of either rents or wages. The regressions I estimate, however, omit G_S. Denote the partial correlations of T and G_K (the included variables) with government services by γ_T^S and γ_K^S, respectively. In other words, γ_T^S is the correlation between taxes and government services *conditional on* government capital's being held constant (and similarly for γ_K^S), so that we can think of the γ terms as coefficients from an OLS regression of the form

$$G_S = \gamma_T^S.T + \gamma_K^S.G_K + u_S. \tag{11}$$

From equation (9), the expected value of the coefficients that obtain when we estimate (10) without including government services can therefore be written as

$$E(b_T) = \beta_T + \gamma_T^S.\beta_S$$
$$E(b_K) = \beta_K + \gamma_K^S.\beta_S.$$

What will be the direction of the bias? First, consider the coefficient on the tax term. If government services are productive amenities, then the coefficient on government services in the hedonic rent regressions will be unambiguously positive--in the context of the Roback model, the isocost curve shifts out, while the isoutility curve shifts back. Hence $\beta_S > 0$ for the rent regressions. The effect on wages, however, is ambiguous; the productive aspect of services tends to raise wages, while the amenity aspect tends to depress them. If the productive component of government services is sufficiently large relative to the amenity component, however, wages will rise with the level of services. What about the sign of γ_T^S? It seems likely that the correlation between taxes and services (with government capital held constant) will be positive, inasmuch as higher tax revenues are used to finance larger levels of government services. On balance, then, the estimated coefficients on the tax terms in the rent regressions will be biased upward; the same might also be true in the case of the wage regressions so long as government services are sufficiently productive (*i.e.*, we have $\beta_S > 0$ in the wage hedonics).[30]

Moreover, there is likely to be a positive correlation between the level of government services and the amount of public capital in a region even if taxes are held constant.[31] If so, $\gamma_K^S > 0$ in equation (11), and the direction of the bias in the government capital coefficients from the wage and rent regressions once again depends on the sign of the coefficient on government services from the "true" model.

It is also quite possible that there is a positive relationship between how well-off a region is and its residents' demand for government capital--*i.e.*, something like Wagner's Law is at work. For example, say there exists a region-specific productive characteristic Z which, together with government services, we erroneously omit from the regression. The true model is therefore

$$y = \beta_T.T + \beta_K.G_K + \beta_S.G_S + \beta_Z.Z + e,$$

and the expected values of the estimated coefficients are

$$E(b_T) = \beta_T + \gamma_T^S.\beta_S + \gamma_T^Z.\beta_Z$$
$$E(b_K) = \beta_K + \gamma_K^S.\beta_S + \gamma_K^Z.\beta_Z$$

where γ_T^S, γ_T^Z, γ_K^S, and γ_K^Z are once again partial correlation coefficients that satisfy regression-like relationships of the form

$$G_S = \gamma_T^S.T + \gamma_K^S.G_K + u_S$$
$$Z = \gamma_T^Z.T + \gamma_K^Z.G_K + u_Z.$$

As we have assumed that Z is a productive characteristic, a higher level of Z acts to raise land rents and wages in a region (the isocost locus shifts out). Thus, β_Z will be positive in both the rent and wage regressions. The direction of the bias that results from omitting Z will therefore depend on the signs of γ_T^Z and γ_K^Z.

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[30] This provides a possible explanation for the fact that property taxes enter the wage regressions, even though theory predicts that property taxes should only affect land prices.

[31] For example, there might be a "production function" relating tax revenues and government capital which takes the form $G_S = f(T, K_G)$; thus, we can produce more hospital services by raising taxes and hiring more doctors, or we can give the same number of doctors larger, better-equipped hospitals.

We would expect γ_T^Z to be positive, since it seems likely that an increase in Z (by raising incomes in a region) will act to increase taxes even if the level of government capital is held fixed.[32] Again, this provides an explanation of why property tax measures enter the wage regressions in spite of the theoretical prediction that they should not; intuitively, wealthier areas demand more government and pay more taxes for it.

It is not clear what sign γ_K^Z will take, however. This correlation might even be close to zero if governments tend to raise taxes in order to pay for additional or existing public capital; in other words, the level of taxation in a region might be a good proxy for the demand for public goods, capital included, on the part of the region's residents. If γ_K^Z is zero, the coefficients on public capital from the wage and rent regressions will be biased only because government services are omitted from the regressions.

Another source of omitted variable bias deserves mention, though it is harder to assess its effects. It is likely that certain worker characteristics are correlated both with worker productivity and with demand for public goods. Age is a good example: experience and cohort effects influence worker earnings, while we might expect younger workers--who are more likely to have school-age children--to demand higher levels of public education. An age effect might even enter the rent regressions to the extent that life-cycle considerations affect the choice of housing tenure or housing size. Although the wage regressions I estimate do control for age (and the rent regressions control for housing tenure and size), there could be other such characteristics which are omitted. This, of course, will induce a spurious correlation between regional wage and rent gradients and public capital levels.

Implied amenity and productive effects

Overall it appears reasonable to conclude that higher levels of infrastructure capital act to raise wages and rents in a region, while noninfrastructure capital has little or no effect. This is consistent with the story that public infrastructure acts as a productive site characteristic. I can further assess the implied productive contribution of infrastructure capital by using the expressions derived in section two together with the coefficients from the wage and rent regressions. Since the estimates from the imputed housing service flow regressions are conceptually preferable to those from the gross rent regressions, I use them exclusively in

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32 This is true even if the tax measures are expressed as ratios to personal income so long as the "budget share" of government is an increasing function of income, that is, if government is a luxury good. Interestingly, this appears to be the case for my sample: the ratio of general revenues to personal income in an SMSA rises with the log of SMSA personal income.

the calculations that follow.[33]

First, however, I require estimates for several parameters, namely, the share of land rents in labor income and the shares of labor and land in production costs. Roback (1982, p. 1273) estimates that 19.6 percent of the total value of a housing unit is accounted for by the value of the land on which the house is situated. I can therefore compute the share of land rents in labor income by applying this figure to an estimate of the budget share of housing expenditures (net of utilities costs) in wage income, which equals 0.19 in my Census data. Values for the cost shares of land and labor are computed from Bureau of Labor Statistics estimates, and equal 4.0 percent and 66.1 percent, respectively. (The data appendix contains details of all calculations.)

I begin with a test of the proposition that public capital has no amenity value; under the model's assumptions, this implies that we should be unable to reject the hypothesis embodied in equation (4)--*i.e.*, the left-hand side of equation (4) should be statistically indistinguishable from zero. I compute equation (4) and its associated standard error under the assumption that the only sources of measurement error are the regression coefficients; the results (not shown) are somewhat mixed. The *t*-statistics for total public capital do not allow us to reject the null hypothesis of zero amenity value for any specification, though this is largely thanks to the estimates' rather large standard errors. The point estimates and *t*-ratios are both very small for total infrastructure and highway capital, which is stronger evidence that these components of the public capital stock do in fact have little amenity value. A similar story can generally be told for total noninfrastructure capital; while the *t*-statistic associated with this component is statistically significant in one specification, the point estimates are much smaller when they are based on regressions in which the infrastructure components are broken out separately.

I reject the hypothesis of a stable isoutility locus for the water and sewer components of total infrastructure. Note that the observed wage and rent differentials induced by changes in these components are consistent with sewer capital's being a disamenity, which is puzzling; water distribution and treatment capital, on the other hand, appears to augment productivity in addition to having amenity value (see tables 1 and 5).

Next, I can estimate the implied productive contribution of public capital, that is, the percentage cost reduction that obtains from a one percent increase in public capital per person. Table 6 presents the

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[33] In order to use the coefficients from the gross rent regressions, I would have to apply a scaling factor equal to the ratio of total housing costs to housing costs *less* utilities expenditures (see above). Rather than introduce another step--and thus another possible source of measurement error--to the calculation, I prefer to base the analysis on the imputed housing service flow regressions.

full range of figures.[34] The results for water and sewer capital are consistent with the conclusion presented in the preceding paragraph, namely, that water capital apparently has some productive value while sewer capital has none (or negative value). Highways and streets consistently appear to raise productivity, while infrastructure as a whole makes a statistically significant positive contribution to productivity in all but one of the specifications. On balance, I believe that the specifications used to generate the estimates reported in column five of the table are preferable. As mentioned above, I favor the tax controls which are expressed as ratios to personal income; it also seems best to use a detailed set of revenue categories. The estimates from this specification imply that the output elasticity of total public capital is about 0.08. For all infrastructure capital, the estimated elasticity is 0.12, while the elasticity for highway capital is equal to 0.07.

4. Conclusions

The results presented above indicate that the elasticity of output with respect to public capital is positive and statistically significant, and principally reflects the contribution of infrastructure capital. How do these findings compare with those of other researchers?

Table 7 summarizes the implied output elasticities (where they can be computed) from a number of previous studies. With the exception of Eberts (1986), all are at the national or state level; note that Eberts (1986) and Morrison and Schwartz (1996) use manufacturing data, while the other studies use broader sector definitions. Unlike Evans and Karras (1994) and Holtz-Eakin (1994), I find that public capital does indeed have an impact on private-sector production, though my elasticity estimates are somewhat smaller than those found by Aschauer (1989) and Morrison and Schwartz (1996).[35] A surprising result is that the output elasticity estimate for total public capital that I report in column zero of table 6 (which is based on the regressions that contain no tax controls) is almost identical to the corresponding value found by Munnell (1990). The estimate from my preferred specification is roughly half as large as hers, however,

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[34] With a constant-returns-to-scale production technology, the percentage cost reduction can be interpreted as an equivalent percentage increase in total factor productivity, and hence as the elasticity of output with respect to public capital (holding population fixed).

[35] Note that Morrison and Schwartz fit a separate cost function to each of the four Census regions and do not impose constant returns to scale. The estimated percentage cost reduction that they find is therefore not equivalent to an output elasticity, and varies rather considerably by region; hence, the value reported in table 7 is very approximate.

although the estimates for highway capital are similar. The results in Eberts (1986) are particularly relevant, as his is the only study that employs SMSA-level data as well as the same public capital stock estimates that I use; he finds that manufacturing output rises by 0.04 percent for every one percent increase in total public capital and 0.03 percent for every one percent increase in infrastructure capital. Although these elasticities are smaller than the values I find, Eberts notes that manufacturing firms in a given SMSA have to share the full stock of public capital with other firms and households located in the SMSA, so the elasticity of *total* output might well be larger than that of manufacturing output alone.[36]

In the end, are these results convincing? When I began this study, I had a strong prior belief that public capital had *no* measurable productive effect on private output. This was largely because I subscribed to Hulten's (1990, p. 105) view that, while the presence or absence of a network of infrastructure investments might well have important implications for private-sector productivity, "*adding* to an existing network will rarely have the same return" (his italics). Moreover, I was extremely skeptical of estimation strategies that rely on what are likely to be seriously mismeasured inputs and often fragile functional forms. Nevertheless, using a methodology that is very different from the standard production or cost function-based approach, I found small but statistically significant output elasticities for public capital.

That said, I should note several caveats regarding my results (in addition to the problems resulting from omitted variables that I noted above). First, my estimates are based on a single cross section of Census data, and cover only 40 SMSAs. This latter criticism cannot really be addressed--there are few cities whose public investment data are long enough to permit the estimation of a capital stock series--and is probably unimportant, since the SMSAs used in the study represent a reasonably diverse group in terms of age and regional location. The former criticism, however, is probably important. I would be much more convinced by these results were they estimated over a panel of SMSAs, primarily because such a procedure would permit the inclusion of controls for SMSA fixed effects, which would in turn better allow me to determine whether the public capital terms are merely proxying for other characteristics (such as how wealthy an SMSA is). Unfortunately, such an extension would be difficult to implement: the public-use extracts from the 1970 Census do not contain sufficiently detailed data on housing characteristics, and the SMSA public capital series do not extend past 1983, so we cannot easily extend the analysis with another year's Census. In addition, a better understanding of the rate at which changes in area characteristics are

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[36] My estimates for the elasticity of highway capital also appear to be roughly consistent with Fernald's (1999) point estimates for the 1974-1989 period.

capitalized into land values and wages would be required before undertaking a longitudinal analysis of this sort.

Second, the tax variables used in the regressions are not wholly satisfactory. This is particularly bothersome since including the tax terms represents my only attempt to control for third factors that might be the true source of the correlation between public capital and rents and wages. Given available data, however, it is not clear how the tax measures could be improved.

Finally, I emphasize that I have largely ignored an important practical question, namely, whether the measured positive effect of public capital on private output lends support to the view that government should increase its spending on infrastructure. Answering this would require knowledge of the social cost of raising additional public funds, including any excess burden that might obtain from imposing higher tax rates.

Data Appendix

This section summarizes the procedures used to construct several variables employed in the analysis.

1 *Estimation of housing value.* The Census collects and reports housing values as a categorical variable. A total of 24 intervals are given, with the first category defined as "less than $10,000" and the final category defined as "$200,000 or more." I map each range of values into a single number employing the same procedure that the Census Department uses in computing its published statistics on housing values, *viz.*, each closed interval is mapped to its midpoint, the lower open interval is set to $7,500, and the upper open interval is set to $250,000. The Census procedure is documented in pages K52-K53 of the *1980 Census User's Guide Glossary*.

2 *Computation of cost shares.* The cost share for labor is taken from unpublished Bureau of Labor Statistics estimates (a table of "Basic industry data for the nonfarm business sector" dated August 9, 1994). The cost share for land is computed by applying the share of land in current capital cost (table C-22 of U.S. Bureau of Labor Statistics, 1983) to the cost share for capital (defined as one *minus* labor's share) for private nonfarm business; note that the BLS adjusts for indirect taxes in constructing these series. I average the computed shares over the period 1977 to 1979 in order to smooth out trade-cycle effects.

The ratio of land's share in current capital cost to the share of rental residential capital and other capital structures during the 1970s is equal to 0.209 in the BLS data, which suggests that Roback's (1982) estimate of 0.196 for the ratio of site value to total house value is quite reasonable.

Previous authors (Beeson and Eberts, 1989, Rauch, 1993) use a value for land's share equal to 6.4 percent, which is taken from Mills and Hamilton (1989, p. 92). This estimate is unsatisfactory, however--the original source for the figure is Keiper, *et al.* (1961, p. 102), which applies an assumed rate of return to a measure of land values in 1956. In addition, these authors estimate the cost shares as shares of national income, which is also unsatisfactory.

3 *Computation of the share of housing expenditures in wage income.* I use the Census data on wage and salary income and the estimated flow value of housing services for owner-occupied units to compute the share of land and structures expenditures in wage income. I computed the measure over all SMSAs (area types one, two, and three) using the 1980 Census's 1/1000 public use tape (the "A" sample), and restricted the sample used in the calculation to include households for which wage and salary income represented 85 percent or more of total household income. I also omitted households with wage and salary income less than the fifth percentile of wage and salary income for this group.

Using similar data, Rauch (1993) estimates the ratio of *gross* rents to wage income to be equal to 0.30; if 76 percent of gross rent goes to purchase structures and land services (this is roughly what I estimate in my sample), then a comparable figure is 0.23, though it is unclear whether and how Rauch deals with outliers. Beeson and Eberts (1989) estimate the fraction of income spent on housing (gross rent) to be 0.27 (implying a structures-land share of 0.21); they apparently use total income--not labor income--in their calculations, however.

4 *Tax controls.* The source for all revenue data is the 1977 *Census of Governments*, volumes four and five. State-level data come from table 47 of volume four; SMSA-level data from table 12 of volume five. SMSA personal income estimates for 1976 come from table six of the Bureau of Economic Analysis publication *Local Area Personal Income, 1974-1979*; state personal income and population data come from table 46 of the *Census of Governments*.

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Table 1

Coefficients on public capital stock from SMSA-level wage regressions

Capital measure	Tax control set (see code definitions, below) 0	1	2	3	4	5
Total	0.069[*]	0.020	0.049[a]	0.089[*]	0.077[a]	0.087[**]
	(0.023)	(0.036)	(0.026)	(0.044)	(0.038)	(0.029)
Infrastructure	0.046[*]	0.024	0.057[*]	0.056[a]	0.066[*]	0.071[**]
	(0.020)	(0.022)	(0.025)	(0.029)	(0.026)	(0.025)
Noninfrastructure	0.036[*]	0.013	0.026	0.047[a]	0.036[a]	0.041[*]
	(0.015)	(0.023)	(0.016)	(0.024)	(0.021)	(0.016)
Joint significance:						
- Both	0.0050	0.5296	0.0621	0.0906	0.0428	0.0083
Water	0.005	–0.012	–0.000	0.007	0.005	0.002
	(0.009)	(0.010)	(0.007)	(0.010)	(0.008)	(0.007)
Highways	0.059[*]	0.058[*]	0.057[*]	0.059[*]	0.058[**]	0.055[*]
	(0.027)	(0.027)	(0.021)	(0.025)	(0.021)	(0.021)
Sewers	–0.007	–0.002	0.015	–0.006	0.019	0.031
	(0.018)	(0.018)	(0.020)	(0.018)	(0.022)	(0.023)
Noninfrastructure	0.025	–0.013	0.001	0.029	0.010	0.010
	(0.018)	(0.022)	(0.015)	(0.022)	(0.018)	(0.014)
Joint significance:						
- All	0.0022	0.2202	0.0208	0.1100	0.0183	0.0026
- Water/highways/sewers	0.0314	0.1313	0.0184	0.1007	0.0102	0.0029

Tax control definitions: 0 = None; 1 = Log general revenues per capita; 2 = Log nontax, property, and nonproperty revenues per capita; 3 = General revenues (ratio to personal income); 4 = Property and nonproperty revenues (ratios to pers. income); 5 = Income, sales, property, and other revenues (ratios to personal income). Tax data are from the 1976-77 fiscal year.

Notes: Standard errors in parentheses. [**] denotes significant at the one percent level or better; [*] denotes significant at the five percent level or better; [a] denotes significant at the 10 percent level or better. "Joint significance" row reports *p*-values for *F*-test that indicated subset of coefficients is zero.

Table 2
Tax controls from SMSA-level wage regressions

Tax controls	Total	Infrastruct./ noninfrastruct.	Water/highway/sewer/ noninfrastructure
	Public capital stock measure included in regression		
1976 log per capita measures			
1. General revenues (from own sources)	0.098*	0.085a	0.124**
	(0.047)	(0.045)	(0.042)
2. Nontax	–0.004	–0.013	–0.003
	(0.043)	(0.043)	(0.033)
Property tax	0.081**	0.083**	0.092**
	(0.023)	(0.022)	(0.023)
Nonproperty tax	–0.048	–0.069a	–0.046
	(0.037)	(0.041)	(0.037)
1976 ratios to personal income			
3. General revenues (from own sources)	–0.273	–0.277	–0.082
	(0.504)	(0.475)	(0.385)
4. Nonproperty tax revenues	–0.840a	–0.963*	–0.772*
	(0.498)	(0.471)	(0.367)
Property tax revenues	1.254a	1.342*	1.836**
	(0.636)	(0.543)	(0.530)
5. Income tax revenues	–1.324*	–1.470**	–1.308*
	(0.573)	(0.542)	(0.487)
Sales tax revenues	–0.735	–0.843	–0.251
	(0.908)	(0.925)	(0.834)
Property tax revenues	1.469*	1.591*	2.295**
	(0.702)	(0.616)	(0.599)
Other revenues	–0.404	–0.498	–0.365
	(0.960)	(0.934)	(0.775)

Notes: Standard errors in parentheses. ** denotes significant at the one percent level or better; * denotes significant at the five percent level or better; a denotes significant at the 10 percent level or better.

Table 3

Coefficients on public capital stock from SMSA-level rent regressions
Gross rent definition

Tax control set (see code definitions, below)

Capital measure	0	1	2	3	4	5
Total	0.364**	–0.063	0.010	0.158	0.107	0.080
	(0.084)	(0.072)	(0.084)	(0.148)	(0.109)	(0.104)
Infrastructure	0.218[a]	0.003	0.084	0.108	0.167**	0.162**
	(0.122)	(0.067)	(0.058)	(0.110)	(0.060)	(0.051)
Noninfrastructure	0.175[a]	–0.054	–0.024	0.057	0.013	–0.003
	(0.098)	(0.048)	(0.045)	(0.100)	(0.055)	(0.047)
Joint significance:						
- Both	0.0005	0.5266	0.3011	0.5131	0.0271	0.0114
Water	0.135**	0.022	0.058*	0.085*	0.076**	0.065**
	(0.050)	(0.021)	(0.024)	(0.038)	(0.024)	(0.015)
Highways	0.053	0.059	0.058	0.067	0.091[a]	0.085[a]
	(0.087)	(0.072)	(0.045)	(0.091)	(0.046)	(0.044)
Sewers	–0.145[a]	–0.116[a]	–0.085[a]	–0.150[a]	–0.053	–0.023
	(0.082)	(0.068)	(0.048)	(0.084)	(0.042)	(0.037)
Noninfrastructure	0.189*	–0.039	–0.009	0.084	0.011	–0.013
	(0.077)	(0.053)	(0.049)	(0.091)	(0.054)	(0.050)
Joint significance:						
- All	0.0001	0.1812	0.0273	0.1015	0.0018	0.0002
- Water/highways/sewers	0.0172	0.2856	0.0201	0.0541	0.0009	0.0001

Tax control definitions: 0 = None; 1 = Log general revenues per capita; 2 = Log nontax, property, and nonproperty revenues per capita; 3 = General revenues (ratio to personal income); 4 = Property and nonproperty revenues (ratios to pers. income); 5 = Income, sales, property, and other revenues (ratios to personal income). Tax data are from the 1976-77 fiscal year.

Notes: Standard errors in parentheses. ** denotes significant at the one percent level or better; * denotes significant at the five percent level or better; [a] denotes significant at the 10 percent level or better. "Joint significance" row reports *p*-values for *F*-test that indicated subset of coefficients is zero.

Table 4
Tax controls from SMSA-level rent regressions: Gross rent definition

Tax controls	Public capital stock measure included in regression: Total	Infrastruct./ noninfrastruct.	Water/highway/sewer/ noninfrastructure
1976 log per capita measures			
1. General revenues (from own sources)	0.858**	0.855**	0.781**
	(0.117)	(0.117)	(0.097)
2. Nontax	–0.022	–0.039	–0.042
	(0.084)	(0.081)	(0.075)
Property tax	0.355**	0.363**	0.334**
	(0.072)	(0.076)	(0.069)
Nonproperty tax	0.294*	0.271*	0.245*
	(0.126)	(0.111)	(0.111)
1976 ratios to personal income			
3. General revenues (from own sources)	2.930[a]	3.137*	2.531[a]
	(1.594)	(1.466)	(1.279)
4. Nonproperty tax revenues	–0.154	–0.470	–0.411
	(1.349)	(1.222)	(1.056)
Property tax revenues	11.056**	11.435**	10.714**
	(1.854)	(1.610)	(1.585)
5. Income tax revenues	–1.201	–1.635	–1.248
	(1.510)	(1.164)	(1.124)
Sales tax revenues	3.672	3.372	3.175
	(2.267)	(2.011)	(1.993)
Property tax revenues	11.907**	12.283**	11.773**
	(1.443)	(1.185)	(1.158)
Other revenues	–0.239	–0.539	–0.695
	(2.188)	(2.220)	(2.180)

Notes: Standard errors in parentheses. ** denotes significant at the one percent level or better; * denotes significant at the five percent level or better; [a] denotes significant at the 10 percent level or better.

Table 5

Coefficients on public capital stock from SMSA-level rent regressions
Imputed housing service flow definition

Capital measure	Tax control set (see code definitions, below)					
	0	1	2	3	4	5
Total	0.538**	–0.077	0.043	0.220	0.140	0.103
	(0.185)	(0.158)	(0.184)	(0.237)	(0.184)	(0.184)
Infrastructure	0.401a	0.090	0.241*	0.229	0.353**	0.356**
	(0.199)	(0.126)	(0.116)	(0.183)	(0.104)	(0.075)
Noninfrastructure	0.207	–0.111	–0.063	0.043	–0.045	–0.072
	(0.187)	(0.109)	(0.103)	(0.170)	(0.089)	(0.077)
Joint significance:						
- Both	0.0052	0.5568	0.1186	0.4031	0.0047	0.0001
Water	0.233*	0.090	0.146*	0.162*	0.163*	0.148**
	(0.092)	(0.051)	(0.067)	(0.079)	(0.066)	(0.053)
Highways	0.192	0.178	0.163a	0.209	0.179*	0.153*
	(0.122)	(0.109)	(0.086)	(0.125)	(0.079)	(0.076)
Sewers	–0.371*	–0.309*	–0.259	–0.371*	–0.187a	–0.114
	(0.143)	(0.130)	(0.107)	(0.144)	(0.097)	(0.090)
Noninfrastructure	0.250a	–0.050	–0.005	0.107	0.003	–0.042
	(0.134)	(0.108)	(0.105)	(0.153)	(0.100)	(0.093)
Joint significance:						
- All	0.0011	0.0754	0.0212	0.0441	0.0055	0.0067
- Water/highways/sewers	0.0058	0.0543	0.0109	0.0216	0.0026	0.0040

Tax control definitions: 0 = None; 1 = Log general revenues per capita; 2 = Log nontax, property, and nonproperty revenues per capita; 3 = General revenues (ratio to personal income); 4 = Property and nonproperty revenues (ratios to pers. income); 5 = Income, sales, property, and other revenues (ratios to personal income). Tax data are from the 1976-77 fiscal year.

Notes: Standard errors in parentheses. ** denotes significant at the one percent level or better; * denotes significant at the five percent level or better; a denotes significant at the 10 percent level or better. "Joint significance" row reports *p*-values for *F*-test that indicated subset of coefficients is zero.

Table 6

Estimated output elasticities of public capital stock
SMSA-level regressions

	Tax control set					
Capital measure	0	1	2	3	4	5
Total	0.154**	–0.002	0.041	0.103[a]	0.079[a]	0.078[a]
	(0.040)	(0.040)	(0.041)	(0.056)	(0.045)	(0.042)
Infrastructure	0.111*	0.034	0.086**	0.083[a]	0.115**	0.119**
	(0.042)	(0.029)	(0.029)	(0.042)	(0.027)	(0.022)
Noninfrastructure	0.066	–0.014	0.004	0.040	0.015	0.013
	(0.039)	(0.027)	(0.023)	(0.038)	(0.023)	(0.019)
Water	0.050*	0.010	0.029*	0.037*	0.036*	0.031*
	(0.020)	(0.012)	(0.014)	(0.017)	(0.014)	(0.012)
Highway	0.078*	0.074*	0.071**	0.081*	0.075**	0.067**
	(0.030)	(0.028)	(0.022)	(0.030)	(0.021)	(0.021)
Sewer	–0.080*	–0.064*	–0.042	–0.079*	–0.025	–0.003
	(0.031)	(0.029)	(0.025)	(0.031)	(0.024)	(0.024)
Noninfrastructure	0.067*	–0.019	–0.000	0.041	0.007	–0.002
	(0.030)	(0.026)	(0.023)	(0.034)	(0.023)	(0.021)

Tax control definitions: 0 = None; 1 = Log general revenues per capita; 2 = Log nontax, property, and nonproperty revenues per capita; 3 = General revenues (ratio to personal income); 4 = Property and nonproperty revenues (ratios to pers. income); 5 = Income, sales, property, and other revenues (ratios to personal income). Tax data are from the 1976-77 fiscal year.

Notes: Standard errors in parentheses. ** denotes significant at the one percent level or better; * denotes significant at the five percent level or better; [a] denotes significant at the 10 percent level or better.

Table 7

Output elasticities of public capital (estimates from previous studies)

Study	Capital measure	Elasticity	Sample	Specification	Fixed effects?
Aschauer (1989)	Total nonmilitary Core infrastructure	0.3 - 0.4 0.24	National	Cobb-Douglas prod. fn.	N/A
Munnell (1990)	Total state and local Highways Water and sewer Other	0.15 0.06 0.12 0.01	State panel	Cobb-Douglas prod. fn.	No
Eberts (1986)	Total Total infrastructure	0.04 0.03	SMSA panel, mfg. only	Translog prod. fn.	No
Evans and Karras (1994)	Total and infrast. components	Negative or zero	State panel	Cobb-Douglas	Yes
Holtz-Eakin (1994)	Total	Negative or zero	State panel	Cobb-Douglas	Yes
Lynde and Richmond (1992)	Total nonmilitary	Positive	National	Translog cost fn.	N/A
Morrison and Schwartz (1996)	Infrastructure	0.12 - 0.25 (approx.)	State panel, mfg. only	Generalized Leontief cost fn.	Yes

Note: "Fixed effects?" refers to whether an attempt is made to control for time-invariant region-specific factors, either with random- or fixed-effect methods. N/A = not applicable.