What Have We Learned From Three Decades of Research on the Productivity of Public Capital?*

Pedro R.D. Bom†
Tilburg University

Jenny E. Ligthart‡
Tilburg University, Georgia State University,
University of Groningen, and CESifo

October 8, 2009

Abstract

The last three decades have witnessed a great deal of research effort devoted to measuring the private output elasticity of public capital, but no consensus has emerged yet. This paper reconciles the findings of the literature by quantitatively analyzing a sample of 67 studies for the 1983–2008 period. We derive an average output elasticity of 0.146 and identify several sources of variation in the reported estimates. The short-run output elasticity at the intraregional level amounts to 0.085. In the long run, and after accounting for interregional spillover effects, the contribution of public capital to output increases by a factor of three. The results suggest that the high output elasticities found in the early time-series literature are compatible with long-run (cointegrating) estimates found more recently. We also show that the observed estimates are significantly inflated by bidirectional publication bias.

JEL codes: E62, H54, R54

Keywords: public capital, infrastructure, public investment, meta-analysis, meta-regression analysis, (nonlinear) publication bias

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*Part of this paper was written while the first author was visiting the Federal Reserve Bank of Atlanta and the second author was a Visiting Professor at Georgia State University. The authors would like to thank Patricia Melo, Mark Rider, Jan-Egbert Sturm, participants at the 2007 Research Conference of the Netherlands Network of Economics in Utrecht, the 2008 Workshop on Meta-Analysis in Economics and Business in Nancy, the 2008 IIPF conference in Maastricht, the 2nd Annual Conference of the Portuguese Economic Journal in Évora, and seminar participants at the Federal Reserve Bank of Atlanta, Georgia State University, and Tilburg University.

†CentER and Department of Economics, Tilburg University, P.O. Box 90153, 5000 LE Tilburg, The Netherlands. Phone: +31-13-466-2068, Fax: +31-13-466-4032, E-mail: `p.r.duartebom@uvt.nl`.

‡Corresponding Author: CentER and Department of Economics, Tilburg University, P.O. Box 90153, 5000 LE Tilburg, The Netherlands. Phone: +31-13-466-8755, Fax: +31-13-466-4032, E-mail: `j.ligthart@uvt.nl`.

1 Introduction

What is the quantitative effect of public capital[1] on private output? Providing a solid answer to this question is of vital importance to policymakers as well as macroeconomic researchers. In the policy arena, the debate on the productivity of public capital has flared up globally following the recent world economic and financial crisis. Most industrialized countries have adopted fiscal stimulus measures to address the growing economic crisis. Specifically, three-quarters of G-20 countries have increased public spending on infrastructure, predominantly on transportation networks (IMF, 2009). On the research side, many structural macroeconomic and regional general equilibrium models feature the output elasticity of public capital as a fundamental parameter; knowledge of its exact magnitude is thus required to numerically analyze fiscal policy shocks.

The literature has devoted a great deal of effort to measuring the output elasticity of public capital by estimating a production function that includes public capital as an input.[2] Aschauer (1989a-b, 1990) uses this approach in an attempt to explain the productivity growth slowdown in the United States in the 1970s.[3] Indeed, in the United States and various OECD countries, investments in the public capital stock fell and aggregate labor productivity growth declined slightly later. Aschauer (1989a) found that a 1 percent increase in the public capital stock increased private output by 0.39 percent, suggesting that public capital is an important determinant of output. Since then, many studies have been undertaken for the United States and various other OECD countries. More recently, attention has also been focused on the productivity effects of public capital in developing countries (e.g., Ram, 1996).

Despite all these measuring efforts, remarkably little consensus has emerged in the literature. Indeed, the output elasticity of public capital differs substantially across studies, ranging from -1.7 for New Zealand to 2.04 for Australia (see Figure 1). In between these

[1]Public capital is generally defined as the tangible capital stock owned by the public sector excluding military structures and equipment. See Section 2.2 for a further discussion.

[2]See the literature reviews by Munnell (1991, 1992), Gramlich (1994), Pfahler et al. (1996), Button (1998), Sturm et al. (1998), Button and Rietveld (2000), Mikelbank and Jackson (2000), IMF (2004), and Romp and De Haan (2007).

[3]Mera (1973) was the first to estimate a production function including some form of public capital, which he refers to as 'social capital,' for nine Japanese regions. This work was followed by Ratner (1983), Da Costa, Ellson, and Martin (1987), and Aschauer (1989a). The latter is the seminal paper in the field.

extremes, a nonnegligible share of the reported estimates are not statistically different from zero. Although the majority of estimates are positive and cluster within a smaller range of values, it is virtually impossible to get an idea of the size of the output elasticity of public capital by glancing through the literature. The first objective of our paper is therefore to precisely quantify the 'true' contribution of public capital to private sector production.[4] Moreover, it is not at all clear from the literature how differences in estimates are related to differences in study characteristics. In view of this, the second objective of our paper is to identify the sources of heterogeneity of reported estimates. To this end, we apply meta-analytical techniques to a sample of primary studies. Drawing on Stanley and Jarrell (1989) and Stanley (2001), meta-analysis can be defined as a body of statistical methods to summarize, evaluate, and analyze empirical results across studies; it presents a systematic and objective way to explain and control for the study-to-study variation.[5] Our study is the first to employ meta-analysis to quantify the output elasticity of public capital.

Because we are interested in the true output elasticity of public capital, it is crucial that our sample consists of primary studies of relatively homogeneous quality that measure the same parameter. In view of this, we propose and apply new procedures to arrive at an appropriate meta-sample. Instead of using all available observations, which is common in meta-regression analyses, we use a single observation per study. This strategy allows us not only to control for dependency across multiple observations taken from a single study, but also to increase—by focusing on the highest quality estimates only—the accuracy of the true effect. The focus of our analysis is on studies that model the stock of public (infrastructure) capital as an input into production, that is, the so-called production function approach.[6] Our meta-sample of 67 studies spans the 1983–2008 period and consists of single-country studies (on 12 different countries) and various cross-country studies. Because our meta-sample contains unobserved

[4]Note that the term 'true' is used in this paper in a broad sense, referring to either a unique effect or the average of a heterogeneous effect. As we will see below, the true output elasticity of public capital is actually rather heterogeneous, consisting of both observed and unobserved heterogeneity components.

[5]Meta-analysis has a long-standing tradition in psychology and medical research. Environmental and transport economists were the first to apply meta-analysis in economics in the 1980s. Since then, it has been picked up by other fields in economics such as labor economics (e.g., Card and Krueger, 1995), industrial organization (e.g., Button and Weyman-Jones, 1992), and international economics (e.g., De Mooij and Ederveen, 2003; and Rose and Stanley, 2005).

[6]See Sturm et al. (1998) for an overview of other approaches to estimating the output elasticity of public capital.

heterogeneity across observations, we employ a random effects meta-regression model. For comparison purposes, we also derive estimates based on the standard fixed effects model.

When estimating the average output elasticity of public capital, we must bear in mind that researchers/editors are more likely to report/publish statistically significant results than insignificant ones. This so-called publication bias is widely recognized to inflate the size of the estimated coefficients available to the meta-analyst. Meta-analytical techniques enable us to filter out publication bias, which is typically assumed to be unidirectional. In practice, however, measurements often take on both positive and negative values, suggesting that publication bias can be of a bidirectional form. Another aim of our study is to extend the literature on publication bias by designing a simple framework that allows bidirectional publication bias of a linear form. In addition, we model nonlinear publication bias (which allows publication bias to be less severe for more precise estimates), which has been little dealt with in the literature.[7] We correct our true effect estimate for linear publication bias and compare it with that based on a nonlinear publication bias correction.

We find an unconditional (average) output elasticity of public capital of 0.146, which is far below Aschauer's estimate. This gap can too a great extent be explained by the substantial degree of heterogeneity across estimates of primary studies. A substantial part of the (observed) heterogeneity is explained by study design parameters, such as the empirical model, estimation technique, type of public capital, and level of aggregation of public capital data. The conditional short-run output elasticity of public capital at the intraregional is estimated at 0.085. In the long run, and after taking into account interregional spillover effects, the contribution of public capital to private production increases to 0.268. The results suggest that the high output elasticities found in the early time-series literature (e.g., Aschauer, 1989a) are compatible with long-run (cointegrating) estimates found more recently. Finally, primary estimates are significantly inflated by bidirectional publication bias; linear publication bias is shown to yield a better model fit than nonlinear publication bias.

The remainder of the paper is structured as follows. Section 2 presents the main methodology employed in the literature to estimate the output elasticity of public capital. In ad-

[7] The work by Stanley and Doucouliagos (2007) is a notable exception.

dition, it reviews the key methodological issues that are raised in the literature. Section 3 describes the meta-sample and analyzes publication bias informally. Section 4 sets out the meta-regression model. Section 5 discusses the meta-regression results. Section 6 concludes.

2 The Production Function Approach

Following the work of Aschauer (1989a), the production function approach is the most widely used in measuring the output elasticity of public capital. This section describes the empirical methodology underlying the production function approach. In addition, it discusses the main methodological issues raised in the empirical literature. The latter serves as input into defining the explanatory variables used in the meta-regression analysis of Sections 4 and 5.

2.1 The Empirical Model

The corner stone of the production function approach is a technological relationship that incorporates the stock of public capital of region/country i at time t (denoted by G_{it}) as an input:

$$Y_{it} = A_{it}F\left[K_{it}, L_{it}, G_{it}\right], \tag{1}$$

where Y_{it} is aggregate private sector output of region/country i at time t, A_{it} is an index of (Hicks-neutral) factor productivity,[8] K_{it} denotes the stock of (non-residential) private fixed capital, and L_{it} denotes employment (typically measured by total hours worked). In this setup, an increase in public capital may affect output directly (i.e., $\partial Y_{it}/\partial G_{it} > 0$), but also indirectly through its effect on the marginal productivity of private factors of production (i.e., $\frac{\partial^2 Y_{it}}{\partial K_{it}\partial G_{it}}$ and $\frac{\partial^2 Y_{it}}{\partial L_{it}\partial G_{it}}$). An example of an indirect effect is the 'crowding in' or 'crowding out' of private investment spending by public capital.[9] The general idea of the production function approach is that the services of public capital—which are hypothesized to boost private output—are proportional to the stock of public capital, which is usually assumed to

[8]The technology index may potentially depend on G_{it}. In the context of a Cobb-Douglas production function (see below), it does not make a difference whether public capital is treated as a third input or as a factor affecting the technology index.

[9]The sign of the relationship between public capital and private capital productivity is not a priori clear and thus has to be determined by empirical evidence.

be a pure public good. Note that the production function approach is partial in nature; for example, the financing method of public investment spending is not taken into account.

For empirical purposes, a specific functional form of (1) is chosen. The Cobb-Douglas production function is commonly used:

$$Y_{it} = A_{it} K_{it}^{\alpha} L_{it}^{\beta} G_{it}^{\theta}, \qquad \alpha, \beta, \theta > 0. \tag{2}$$

The parameter of interest is the *partial* output elasticity of public capital, which is defined as:[10]

$$\theta \equiv \frac{\partial \ln Y_{it}}{\partial \ln G_{it}} = \frac{\partial Y_{it}}{\partial G_{it}} \frac{G_{it}}{Y_{it}} > 0.$$

The definition of θ assumes that all other inputs and technology are held constant; any indirect effects of public capital on output therefore cannot be measured. Note that the Cobb-Douglas functional form imposes a unitary elasticity of substitution between factors of production, which is relaxed by more flexible functional forms like the translog.[11] In addition, technological progress in (2) is always of the Hicks-neutral type, that is, technology contributes equally to the productivity of all factors.

To arrive at an equation that can be estimated by linear estimation methods, we take natural logarithms on both sides of (2). Subsequently, we assume that $\ln A_{it} = a_0 + \chi_t + \nu_i + \varepsilon_{it}$, where a_0 denotes a constant, χ_t is a time-specific effect (representing shocks common to all units, for example, technological progress), ν_i denotes an unobserved unit-specific fixed effect (e.g., the effect of climate or geographical location on productivity), and ε_{it} is an error term. In its most basic form, the regression equation is:

$$\ln Y_{it} = a_0 + \chi_t + \nu_i + \alpha \ln K_{it} + \beta \ln L_{it} + \theta \ln G_{it} + \varepsilon_{it}. \tag{3}$$

The basic model has been extended in several directions. Some studies modify (3) by distinguishing various subcomponents of public capital (Section 2.2) or incorporating the capital

[10] Traditional growth theory typically assumes diminishing marginal productivity of factor inputs, which restricts α, β, and θ to be less than unity. In addition, returns to scale restrictions may be imposed (see below).

[11] The transcendental logarithmic (translog) production function includes quadratic and interaction terms for each input. Early adopters of the translog specification are, amongst others, Merriman (1990), Pinnoi (1994), and Damalgas (1995).

stock of neighboring jurisdictions (Section 2.4). To account for the effect of short-run fluctuations on factor use, many studies add a measure of the business cycle (Section 2.7). Besides the basic factors of production, other inputs may contribute to private output (Section 2.8). Taking all these extensions into consideration yields equation (3) in comprehensive form:

$$\begin{aligned} \ln Y_{it} &= a_0 + \chi_t + \nu_i + \alpha \ln K_{it} + \beta \ln L_{it} + \sum_{k=1}^{P} \theta_k \ln G_{it}^k + \gamma \ln Z_{it} \\ &+ \vartheta \sum_{j=1}^{R} b_{ij} \ln G_{jt} + \psi CU_{it} + \epsilon_{it}, \end{aligned} \tag{4}$$

where G_{it}^k represents component k of the public capital stock, θ_k is the output elasticity of capital stock component $k = 1, ..., P$, Z_{it} denotes other inputs into production, G_{jt} is the public capital stock installed in jurisdiction $j = 1, ..., R$, b_{ij} is the weight given to the public capital stock in jurisdiction j, ϑ is a parameter measuring the jurisdictional spillover effects of public capital, CU_{it} represents the capacity utilization rate, and ϵ_{it} is an error term. As will be demonstrated below, most studies estimate a special case of (4).

In estimating θ (or, alternatively, θ_k), authors have employed time-series, cross-section, and panel data models. The time-series approach fixes i for one jurisdiction (typically a country) and exploits the time variation; χ_t is either replaced by a linear time trend (i.e., $\chi_t = \phi t$) or is not included. The seminal work of Aschauer (1989a) analyzes time-series data and sets $\chi_t = \phi t$ and $\theta_k = \gamma = \vartheta = 0$. Cross-section studies keep t fixed and exploit the variation across jurisdictions (typically states/regions, e.g., Da Costa et al., 1987). In this context, ν_i cannot be estimated. Finally, panel data models (featuring both i and t) are employed either at the regional level for a single country or at the country level for country groupings (which we refer to as a transnational study). In the panel context, some studies treat the unobserved unit-specific effect (ν_i) as fixed effects, for example, Evans and Karras (1994a), or as random effects, for example, Andrews and Swanson (1995). Alternatively, various authors use pooled ordinary least squares (OLS) so that ν_i is simply ignored. A few studies (e.g., Boarnet, 1998) exclude ν_i by long-differencing the data. Time-specific fixed effects in the panel setup are represented by means of time dummies. Many panel data models, however, include a linear time trend to capture technological progress.

2.2 Defining Public Capital and Output

The Introduction provided a very general definition of public capital, which emphasized the government's ownership role and the stock nature of the capital.[12] More specifically, public capital consists of core infrastructure (i.e., roads, railways, airports, and utilities, such as sewerage and water facilities), hospitals, educational buildings, and other public buildings.[13] Core infrastructure is generally perceived to be more productive than other types of public capital, such as educational and office buildings and hospitals. Indeed, studies employing a broad definition of public capital (which necessarily includes less productive components) typically find a lower output elasticity of public capital than studies focusing on core infrastructure only. Mas et al. (1994), for example, disaggregate public capital in core and non-core components. Sturm and De Haan (1995) include various subcomponents of public capital into the equation all at once [see (4)] as well enter them one at a time. Some studies focus on transportation infrastructure only (e.g., Garcia-Milà and McGuire, 1992), which is a subcomponent of core infrastructure.

In countries with a fiscally decentralized government structure (e.g., the United States), different layers of government may be involved in the provision of public capital. Consequently, authors studying federal countries have consolidated their public capital stock data to differing degrees. The majority of studies employ public capital stocks defined at the national level including public capital provision at *all* levels of government (e.g., Aschauer, 1989a), whereas others deal with capital stocks estimated for regions based on consolidated regional data (e.g., Garcia-Milà and McGuire, 1992). Some studies only consider capital that is owned by local/regional governments (e.g., Evans and Karras, 1994a), and thus do not take into account regionally installed capital owned by the federal/central government. We are aware of only a few studies estimating capital stocks at the city/metropolitan level (e.g., Duffy-Deno and Eberts, 1991; and Kemmerling and Stephan, 2002).

[12] Statistics on the public capital stock are not readily available. To arrive at an estimate of the public capital stock, researchers determine an initial value of the capital stock to which they add gross investment flows and subtract technical depreciation of the existing capital stock (based on the expected life spans of its components). See Sturm and De Haan (1995) for further details on this so-called perpetual inventory method.

[13] Some authors use a very broad definition of public capital by also including health and welfare facilities (e.g., Mera, 1973). The latter components are hard to measure, explaining why the broad definition is rarely used in empirical analyses.

Some cross-sectional or panel studies explicitly control for differences in the size of jurisdictions (i.e., countries in a transnational context and regions in a federal setting). A few authors (cf. Prud'Homme, 1996) define public capital relative to the surface area of the region to take into account that larger regions have installed more public capital (i.e., the scale effect). De La Fuente and Vives (1995) include the surface area as a separate regressor. To address congestion effects, a minority of research papers define all variables in the equation in per capita terms.[14]

The output measure used as dependent variable varies across studies. Most studies use real *gross* output of the private sector (e.g., Ratner, 1983) or real Gross Domestic Product (GDP) exclusive of public sector output (e.g., Finn, 1993).[15] When the data are at the state level for the United States, real Gross State Product (GSP) is employed. Some studies employ (real) gross value added of the private sector (which equals *net* output of the private sector because intermediate inputs have been subtracted). Although the literature primarily deals with measuring the contribution of public capital to private output, some studies nevertheless employ a measure of total output (including public sector production). The latter is typically the case of studies using data for emerging markets or developing countries, where the only available measure of output is total GDP (e.g., Ram, 1996).[16]

2.3 Returns to Scale Restrictions

Incorporating public capital into the production function raises the issue of returns to scale in production. The majority of studies impose some form of restriction on the coefficients of the production function. Various studies assume constant returns to scale in private inputs (i.e., $\alpha + \beta + \gamma = 1$; see Mas et al., 1994; Otto and Voss, 1994; and Kavanagh, 1997), giving rise to increasing returns to scale across all inputs (i.e., $\alpha + \beta + \sum_{k=1}^{K} \theta_k + \vartheta + \gamma > 1$). Aschauer

[14]Note that if a constant returns to scale restriction is imposed upon a logarithmic specification of the regression equation, all variables end up being defined per unit of capital or per employee hour anyway.

[15]Private output may also be defined according to the economic sector where it is generated. Da Costa et al. (1987), for instance, report estimates for the manufacturing and non-agricultural sectors together with an estimate for all sectors.

[16]Because government output is typically not exchanged on markets, it is hard to measure. The 1993 United Nations System of National Accounts measures the value of government output (and of non-market output more generally) based on the inputs used in production. In applied econometric analyses, the wage bill of the public sector is typically used to approximate government output.

(1989a) argues that congestion effects may be severe enough to render the assumption of increasing returns to scale across all inputs inappropriate. Therefore, various studies impose a constant returns to scale restriction across all inputs. In equation (4), this implies that $\alpha + \beta + \sum_{k=1}^{K} \theta_k + \vartheta + \gamma = 1$, which yields decreasing returns with respect to private inputs taken together (i.e., $\alpha + \beta + \gamma < 1$).

Aside from these theoretical considerations, various studies (e.g., Ratner, 1983) employ constant returns to scale restrictions to help alleviate multicollinearity between explanatory variables in the model. From a statistical viewpoint, multicollinearity does not bias parameter estimates (but only affects their standard errors). Only together with model misspecification can returns to scale restrictions systematically affect parameter estimates. For example, imposing constant returns to scale in private inputs introduces biased estimates if the true model is characterized by *decreasing* returns to scale. In this case, the output elasticity of private capital is overestimated. In addition, the output elasticity of public capital may be upward or downward biased, depending on whether the contemporaneous correlation between private capital and public capital is negative or positive.[17]

2.4 Spillover Effects of Public Capital

The first author studying the output effect of public capital in a regional context is Mera (1973), who uses very broadly defined public capital. It was not until Aschauer's (1989a) analysis that various authors[18] started applying his methodology to regional data using a standard definition of public capital. They find elasticities at the regional level that are much smaller than those from analyses using aggregate data for a single country. This finding can be attributed to spillover effects of public capital, that is, some of the beneficial effects of

[17] Assuming that the 'true' production function is given by (3) with $\alpha + \beta = 1 - \kappa$ for $0 < \kappa < 1$ (i.e., yielding *decreasing* returns to scale in private inputs) and setting $\chi_t = \nu_i = 0$ (for purposes of simplification), we find:

$$\ln(Y_{it}/K_{it}) = a_0 + \beta \ln(L_{it}/K_{it}) + \theta \ln G_{it} + \tilde{\varepsilon}_{it}, \qquad \tilde{\varepsilon}_{it} = \varepsilon_{it} - \kappa \ln K_{it}.$$

Incorrectly assuming constant returns to scale in private inputs yields an upward biased estimate of θ if $\ln G_{it}$ and $\tilde{\varepsilon}_{it}$ are positively correlated, which is true if $\ln G_{it}$ is negatively correlated with $\ln K_{it}$.

[18] Authors that take a regional approach are: Munnell (1990b), Eisner (1991), Garcia-Milà and McGuire (1992), Evans and Karras (1994a), and Holtz-Eakin (1994).

public capital accrue to neighboring regions.[19]

Most studies do not include neighboring regions' public capital stock in the home jurisdiction's production function. The small number of studies (e.g., Boarnet, 1998) that do typically employ the weighted public capital stock of neighboring jurisdictions, where the weights are exogenously given. Estimating this extended equation [see (4)] yields an estimate of ϑ. Alternatively, some studies (e.g., Mas et al., 1994, 1996) include neighboring regions' public capital into the definition of G_{it} (so that ϑ cannot be estimated separately). No consensus has been reached on the significance of spillover effects. The study by Holtz-Eakin and Schwartz (1995b) finds little evidence of spillover effects, whereas Boarnet (1998) obtains a significantly negative spillover coefficient. This is not surprising because studies at this level of aggregation measure only the *net* effect. Backwash effects, such as congestion and resource exploitation, or displacement effects (i.e., new infrastructure shifts economic activity to other locations) may exceed any positive gross benefits of infrastructure.

2.5 Stationarity of Variables

Some of the early studies (including the work of Aschauer, 1989a,b) have been criticized for not properly accounting for common trends. Generally, time series on private output, the public capital stock, and the other private inputs contain a unit root; in other words, they are nonstationary. If this is the case, the usual test statistics have non-standard distributions so that standard inference procedures may give rise to misleading results. In particular, one may find spurious (or non-existing) relationships between private output and factor inputs.

To address these nonstationarity concerns, some studies (e.g., Aaron, 1990; Tatom, 1991; and Sturm and De Haan, 1995) eliminate stochastic time trends in variables by taking first differences. The literature criticizes the method of first differencing by pointing to the information loss that may arise, that is, a possible long-run equilibrium relationship between a set of nonstationary time series (in which case variables are so-called cointegrated) is discarded. This information loss shifts the focus of the analysis away from the long-run effects of public capital to the short-run effects. But the growth rate of private output in a particular year

[19] The theory on fiscal federalism demonstrates that in a Nash equilibrium, these spillovers are not internalized. Consequently, both regions end up with a less than socially optimal public capital stock.

is not strongly correlated with the growth rate of the public capital stock during that same year as lagged effects are likely to be important.[20]

Instead of first differencing, the nonstationary variables should be tested for cointegration. In the mid-1990s, various authors either employed Engle and Granger's (1987) single-equation cointegration test (e.g., Otto and Voss, 1994) or Johansen's (1988) Vector Autoregressive (VAR)-based approach (e.g., Otto and Voss, 1996). A cointegrated model can be interpreted as a long-run equilibrium model, in which the coefficients measure long-run effects.[21] Given the multi-year nature of public capital projects, it seems reasonable to expect to find higher output elasticities of public capital in long-run (cointegrated) models than in short-run models. In Section 5, we compare output elasticities of cointegration studies with those of the early time-series literature (which are possibly spurious given that no cointegration tests were conducted).

2.6 Endogeneity Concerns

Equations (3) and (4) assume the public capital stock to be strictly exogenous, implying that the causality runs from public capital to private output. Some authors (e.g., Munnell, 1992; and Gramlich, 1994) argue that public capital is likely to be an endogenous variable. In this case, public capital is correlated with the regression disturbances, causing the estimated coefficients to be biased. The direction of the endogeneity bias is unclear, however. On the one hand, a higher rate of output growth may boost tax revenue, which facilitates an increase in public investment. On the other hand, the fiscal authority may follow a policy rule according to which public investment reacts anti-cyclically to changes in private output.

Some authors solve the endogeneity problem by using an Instrumental Variables (IV) estimator (e.g., Holtz-Eakin, 1994; and Baltagi and Pinnoi, 1995). If the positive (negative) effect running from output to public capital dominates, then OLS estimates of θ are higher (lower) than IV estimates. Baltagi and Pinnoi (1995), for instance, use panel data for the United States to arrive at a pooled OLS estimate of θ of 0.16, whereas the IV estimate is only

[20] Equations estimated in first differences often yield implausible large coefficients for private inputs (see Sturm and De Haan, 1995).

[21] If a cointegrating relationship between nonstationary variables exists, the model can be estimated in logarithmic *levels* by standard linear estimation techniques.

0.02. Some authors (e.g., Evans and Karras, 1994a) also control for the endogeneity of other factors of production, usually labor. A few studies (e.g., Ai and Cassou, 1995) derive moment conditions from a dynamic model with optimizing firms to estimate production function parameters using the generalized method of moments (GMM) estimator.

During the last decade, various authors have employed VAR models with a view to capturing the dynamic interactions between private output, labor, public capital, and private capital.[22] The VAR approach itself, however, does not solve the endogeneity problem. VAR models are typically reduced-form models, in which contemporaneous effects are concentrated out. If the VAR model is to be given a structural interpretation, then the contemporaneous effects need to be uncovered. The latter requires assumptions essentially equivalent to those necessary to define appropriate instruments in an IV context.

2.7 The Business Cycle

To measure the effect of the business cycle on factor use, some studies incorporate a capital utilization rate—or, alternatively, the unemployment rate—into the regression equation.[23] See, for example, Aschauer (1989a), Hulten and Schwab (1991a), and Sturm and De Haan (1995). The effect of the capacity utilization rate on output is hypothesized to be positive, whereas it is negative in the case of the unemployment rate. Because authors use log-linear empirical models [see (4)], capacity utilization enters the equation in an additive fashion. In some cases (e.g., Ratner, 1983), authors incorporate variables into their analysis that are pre-adjusted for business cycle effects. Delorme et al. (1999) deal with the capacity utilization issue by using a production frontier approach. In this way, the expansionary effect of public capital on the production possibilities frontier can be disentangled from the production-inefficiency reducing effect of public capital formation. Note that in an efficient steady state, the production frontier approach is equivalent to the standard production function approach.

[22] The VAR approach models every endogenous variable as a function of its own lagged value and the lagged values of the other endogenous variables. McMillin and Smith (1994), Otto and Voss (1996), Batina (1998), Flores de Frutos et al. (1998), Pereira and Roca Sagales (1999), Sturm et al. (1999), Ligthart (2002), and Pereira and Roca Sagales (2003), amongst others, employ the VAR approach.

[23] Some studies (e.g., Garcia-Milà and McGuire, 1992) control for the business cycle by simply including time fixed effects. Note that time fixed effects not only capture the business cycle but also many other time-idiosyncratic shocks.

On average 37 percent of the studies do not adjust for the business cycle. Unless a long-run (cointegrating) relationship is estimated, ignoring the business cycle may lead to downward biased estimates of θ. Intuitively, public capital is perceived to be less productive because the economy is inside the production possibilities frontier during an economic downturn. Indeed, public capital formation may close the output gap in economic downturns without necessarily implying an observable effect on the production possibilities. As the economy approaches an efficient equilibrium, the effect of public capital becomes more apparent.[24]

2.8 Omitted Variables

Various authors have criticized Aschauer's model for being misspecified due to the omission of relevant explanatory variables. Omitting variables that help explain changes in output may bias the estimate of θ if those variables are also correlated with public capital. The study by Vijverberg et al. (1997) proposes to add imported raw materials to equation (4). Some studies (e.g., Garcia-Milà and McGuire, 1992) include education as an input. Tatom (1991) makes a case for including energy prices in the production function to account for supply shocks. For example, the rising oil prices of the 1970s—representing a negative supply shock—may have depressed output and capital use. Gramlich (1994) criticizes Tatom's approach for mixing production functions and cost functions. Instead of including energy prices, which typically feature as an argument of the cost function, a measure of the quantity of energy use in production should be employed. Most studies, however, do not include any Z_{it} variables and resort to estimating a standard production function such as equation (3).

3 Descriptive Analysis of the Meta-Sample

This section describes the meta-data set—which will be used in the regression analysis of Sections 4 and 5—and analyzes informally whether publication bias is present in the sample.

[24]Technically, $\ln G_{it}$ is negatively correlated with ε_{it} in (3). This problem can be solved either by including a measure of the business cycle in the model—as in (4)— or, more explicitly, by employing a production frontier approach.

3.1 The Meta-Sample

Table A.1 shows the studies that are included in our meta-data set, which covers estimates of the output elasticity of public capital using (or based on) the production function approach. In total, 67 studies are coded and included in the meta-data set. Eight papers are unpublished, whereas the remaining 59 are published in academic journals, professional journals, or books. Out of 38 published journal papers seven are published in top-20 journals (based on the journal quality ranking of Kodrzycki and Yu (2006)). The data set encompasses single-country studies for 12 different countries and six cross-country (or transnational) analyses. In total, 30 studies (45 percent) are based on data for the United States.

To obtain a sample of studies as representative as possible of the true population of available studies, we use a variety of searching methods.[25] We start by checking the references in overview papers, among others, Sturm et al. (1998) and Romp and De Haan (2007), which together provide a very comprehensive coverage of relevant papers up to 2004.[26] From these sources, we obtain 47 usable references (see below). We then search for papers citing Aschauer (1989a) in Thomson's *Web of Science*, which allows us to add seven papers to our meta-data base. We also use the Internet search engine *Google Scholar* and search for words such as 'public capital' and 'public infrastructure,' each in combination with 'output' or 'productivity,' which yields another 13 papers (of which six are working papers).

We strive for including in the meta-sample only those studies that give rise to a comparable measure of the output elasticity of public capital. Therefore, the following identified studies using a production function approach had to be dropped. First, studies that use physical measures of public capital or employ public investment to proxy the public capital stock. Second, studies based on the translog production function (which do not yield a single measure of the output elasticity). Third, studies that do not distinguish between private and public infrastructure capital. Finally, studies that take a pure VAR approach, except those based on Johansen's VAR approach to estimate parameters of cointegrated variables. Pure VAR studies are dropped from the meta-sample not only because they usually report reduced-form

[25] See White (1994) for a review of the general procedures for searching and retrieving papers.

[26] In addition, we also checked the overview papers by Pfahler et al. (1996), Button (1998), Mikelbank and Jackson (2000), and IMF (2004).

parameter estimates—which are different from the structural parameters of a production function—but also because the lag structure of these models gives rise to a multitude of parameter estimates (one for each lag).[27]

We include only one estimate per study in the meta-data set. The issue of how many estimates (also known as 'measurements') to include when each study reports more than one is still controversial. Some authors (e.g., Bijmolt and Pieters, 2001) claim that all available measurements should be used, whereas others (e.g., Stanley, 1998; and Van der Sluis et al., 2005) are strong believers of selecting only one measurement per study. Including all measurements from each study raises two statistical problems. First, it creates dependency among measurements taken from a single study. Indeed, the estimates are based on the same data set using typically the same empirical methodology. Second, studies with a large number of measurements would receive a disproportionate weight in the sample, giving rise to sampling bias (cf. Stanley, 2001). In our sample, the total number of data points is 569, yielding an average number of 8.5 per study. The distribution of available data points across studies is highly skewed, however. The four papers with the largest number of estimates account for 129 estimates (23 percent of total), the first 10 papers report 235 estimates (41 percent) and the top half of the ranked sample yields 472 estimates (83 percent). Kamps's (2006) study reports the largest number of estimates (59 in total). Only seven studies report one estimate.

Aside from the above-mentioned statistical considerations, there is a more fundamental reason why we use only one measurement per study, that is, we want to measure 'the' true output elasticity of public capital. To uncover the value of this parameter as accurately as possible, only those measurements that come closest to the true effect should be employed. In each study, no more than one measurement can reasonably meet this criterion. Often, the authors themselves consider many of their estimates senseless, which can therefore be discarded upfront. Take panel data studies as an example. For comparison purposes, researchers typically report pooled OLS, random effects, and fixed effects estimates. If the fixed effects model

[27] On a more practical note, many VAR studies report impulse responses only (and thus neither report parameter estimates nor standard errors). The latter are used as weighting factor in the meta-analysis, see Section 4. Consequently, any study that does not report standard errors had to be dropped (e.g., Mera, 1973).

is statistically preferred, as is often the case, then both OLS and random effects estimates are inconsistent. Unless the sole objective of the meta-analysis is to explain the heterogeneity created by the use of different statistical methods, these inconsistent estimates should obviously not be used in a meta-analysis.

Including only one measurement for each study raises the issue of selecting a measurement from multiple measurements. In a few cases, the authors come up with what they consider their 'preferred estimate.' More often than not, however, the choice of the measurement is not clear-cut. In such cases, we apply a set of predefined selection rules. First, we select the estimate of the output elasticity of public capital that uses the most aggregated concepts of private output and public capital.[28] Second, if there are separate estimates for different time periods, we choose the longest. Third, we let consistency prevail over efficiency and pick the estimate that results from the most sophisticated econometric method (cf. Stanley, 1998). For instance, IV is preferred over OLS estimation and panel fixed effects are considered to be superior to pooled OLS and panel random effects. Finally, we choose the estimate from the most parsimonious model as long as the imposed restrictions are not rejected statistically. Following Stanley (1998), when multiple measurements still remain, we average across them. Consequently, we also need to average any moderator variable we want to include, which makes it harder to interpret the coefficients derived from a meta-regression analysis. In view of this, we use this strategy only in a few cases in which differences in estimates are caused by study characteristics that are not included in our set of moderator variables (see Section 4).

Estimates in our sample vary from -0.161 to 1.160 (Table A.1), with an arithmetic (or 'simple') average of 0.198 and a standard deviation of 0.074. Compared to Figure 1, the range of estimates has become smaller, but still showing quite some variation. Indeed, we expect a substantial amount of variation given that studies differ along several dimensions. Seven studies find negative estimates, whereas the remaining 60 studies report positive coefficients. We find that the median of 0.165 is smaller than the sample average; thus the distribution is asymmetric, potentially indicating publication bias.

[28] Public capital may be broken down by the type of public capital (core and non-core) and the level of government (local or federal) that provides it. See Section 2.2.

3.2 Publication Bias

Publication bias means that journals are more likely to publish studies reporting statistically significant results. Papers reporting insignificant results are either not submitted for publication (i.e., self-censoring by the author(s)) or rejected by the editors/referees (i.e., censoring by peers). Even though papers are not published in academic journals they may still be available as Working Papers or unpublished reports. Some authors (cf. Begg, 1994) suggest to include in the meta-sample as many unpublished studies as possible with a view to minimize the perverse effects of publication bias. This strategy, however, is unable to completely eliminate publication bias. Indeed, self-censoring by authors may be quite pernicious, inducing authors from making their findings available altogether.

Is there publication bias in our sample? To get an informal answer, we employ a funnel plot depicting the inverse of the standard error on the vertical axis and the estimated effect size on the horizontal axis (Figure 2). In the absence of publication bias (and other sources of heterogeneity), estimates should lie symmetrically around the true effect. The plot should look like an inverted funnel, which is wider at the bottom than at the top. Intuitively, estimates based on small samples are usually less precise and may thus be located farther away from the true effect. The top panel of Figure 2 shows that estimates tend to concentrate on the right-hand side of the funnel, suggesting unidirectional publication bias (also known as type I selection bias; see Stanley, 2005). We also notice that the base of the funnel is rather wide, potentially indicating bidirectional publication bias (so-called type II selection bias). The bottom panel of Figure 2 presents an alternative funnel plot, which now measures the standard error on the vertical axis and adds 95 percent confidence bounds. It can be seen that roughly half of the data points appear to lie outside the 95 percent bands. Positive (negative) estimates seem to increase (decrease) with the standard errors, indicating that publication bias may be bidirectional. The next section formally measures publication bias in the context of a meta-regression model.

4 The Meta-Regression Model

As the funnel plots in Section 3 demonstrated, the true output elasticity of public capital is rather heterogeneous. This section develops a meta-regression model to find the determinants of excess variation in the sample of estimates of the output elasticity of public capital across studies while controlling for publication bias. Both linear and nonlinear publication bias corrections are considered.

We start by postulating a relationship between each estimate $\hat{\theta}_i$ and its population parameter (denoted by θ_i):

$$\hat{\theta}_i = \theta_i + \varsigma_i, \qquad \forall \ i = 1, ..., N, \tag{5}$$

where N denotes the total number of observed estimates taken from both published and unpublished papers. In the absence of publication bias, the term ς_i captures sampling error and is such that $E[\varsigma_i|\theta_i] = E(\varsigma_i) = 0$ and $V[\varsigma_i|\theta_i] = \text{se}(\hat{\theta}_i)^2$, where $\text{se}(\hat{\theta}_i)$ is the standard error of $\hat{\theta}_i$ (which we approximate by its estimated counterpart as reported in primary studies). If publication bias is present, however, the error term ς_i is correlated with any selecting factor. We assume that only statistical significance (determined by the estimated standard error) causes publication selection, so that ς_i is correlated with $\text{se}(\hat{\theta}_i)$. Assuming that $\varsigma_i = g(\text{se}(\hat{\theta}_i)) + \mu_i$, we can write (5) as follows:[29]

$$\hat{\theta}_i = \theta_i + g(\text{se}(\hat{\theta}_i)) + \mu_i. \tag{6}$$

If publication selection is plaguing the meta-sample, then we should observe a relationship between each estimate $\hat{\theta}_i$ and its standard error $\text{se}(\hat{\theta}_i)$. The function $g(\cdot)$ describes such a relationship and satisfies $g(0) = 0$; that is, infinitely precise estimates do not contain publication bias. Furthermore, we assume bidirectional publication bias, meaning that positive (negative) estimates increase (decrease) with the standard error, that is, $g'(\text{se}(\hat{\theta}_i)) > 0$ if $\hat{\theta}_i > 0$ and $g'(\text{se}(\hat{\theta}_i)) < 0$ if $\hat{\theta}_i < 0$. The functional form of $g(\cdot)$ is unknown a priori. Following Card and Krueger (1995), it has been common in the meta-analysis literature to assume a linear functional form, that is, $g(\text{se}(\hat{\theta}_i)) \equiv \delta \text{se}(\hat{\theta}_i)$, where δ is a parameter measuring publication

[29]The term μ_i features $E[\mu_i|\theta_i, \text{se}(\hat{\theta}_i)] = E(\mu_i) = 0$ and $V[\mu_i|\theta_i, \text{se}(\hat{\theta}_i)] = \text{se}(\hat{\theta}_i)^2$.

bias. Stanley and Doucouliagos (2007) note, however, that $g(\cdot)$ is most likely to be nonlinear and suggest to approximate it by the squared standard error, that is, $g(\text{se}(\hat{\theta}_i)) \equiv \delta\text{se}(\hat{\theta}_i)^2$. The nonlinear publication bias correction is intuitively more appealing than the linear case, because it assumes that publication bias is less severe for more precise estimates. The linear correction, in contrast, assumes that the magnitude of the bias due to publication selection is invariant to the size of the standard error.[30]

Here, we extend both approaches to allow bidirectional publication bias. In the nonlinear case, for instance, we assume $g(\text{se}(\hat{\theta}_i)) \equiv \delta_p\text{se}(\hat{\theta}_i)^2 D_{pi} + \delta_n\text{se}(\hat{\theta}_i)^2 D_{ni}$, where D_{pi} (D_{ni}) is a dummy variable that equals one if $\hat{\theta}_i > 0$ ($\hat{\theta}_i < 0$) and zero otherwise and δ_p and δ_n are publication bias parameters to be estimated. The functional form for the linear publication bias case is similar, with the only difference that the square of $\text{se}(\hat{\theta}_i)$ is dropped. If publication bias is bidirectional, then δ_p is positive and δ_n is negative. If publication bias is absent, then $\delta_p = \delta_n = 0$.

We subsequently define $\theta_i = \theta_0 + \lambda_i$, where θ_0 is the unknown (average) true output elasticity of public capital. The parameter λ_i describes the heterogeneity of reported estimates, which is partly explained by M moderator variables:

$$\lambda_i = \sum_{j=1}^{M} \phi_j D_{ji} + \eta_i, \tag{7}$$

where D_{ij} is a (centered) dummy variable that equals one if the i-th estimate is obtained from a study described by characteristic j and zero otherwise (see below) and ϕ_j denotes its coefficient. The error term η_i represents the remaining *unobserved* heterogeneity and satisfies $E(\eta_i|D_{ij}) = E(\eta_i) = 0$ and $V(\eta_i|D_{ij}) = \sigma_\theta^2$.

Using (7) together with the definitions of θ_i and $g(\cdot)$ yields the most general equation to be estimated:

$$\hat{\theta}_i = \theta_0 + \sum_{j=1}^{M} \phi_j D_{ji} + \delta_p\text{se}(\hat{\theta}_i)^h D_{pi} + \delta_n\text{se}(\hat{\theta}_i)^h D_{ni} + \xi_i, \tag{8}$$

where $h = 1$ if publication bias is assumed to be linear and $h = 2$ if a nonlinear publication bias approach is taken. The new error term ξ_i ($\equiv \eta_i + \mu_i$) consists of unobserved heterogeneity

[30]This can easily be seen by substituting the linear functional form for $g(\cdot)$ in (6) and dividing by $\text{se}(\hat{\theta}_i)$.

(η_i) and sampling error (μ_i), and satisfies $E[\xi_i|D_{ij}, \text{se}(\hat{\theta}_i)] = E(\xi_i) = 0$ and $V[\xi_i|D_{ij}, \text{se}(\hat{\theta}_i)] = \sigma_\theta^2 + \text{se}(\hat{\theta}_i)$.

Equation (8) embeds four cases. The first case assumes *homogeneity*, that is, all studies are unbiasedly estimating a unique population elasticity of public capital, which implies $\lambda_i = 0$ and $\theta_i = \theta_0$ for all i; in terms of equation (8), this means that $\sum_{j=1}^{M} \phi_j D_{ji} = \eta_i = 0$. The second case assumes that all (if any) *heterogeneity is random and unobserved*—as measured by the error component η_i—which is a valid assumption if, for instance, all studies are unbiasedly estimating the mean of a random distribution of population effects. In terms of equation (8), this implies $\sum_{j=1}^{M} \phi_j D_{ji} = 0$ and $\lambda_i = \eta_i$ for all i, where $V(\eta_i) = \sigma_\theta^2 \geq 0$. The third case assumes that any *heterogeneity is observed* (as measured by M moderator variables; see below for a digression), which is a valid assumption if studies are unbiasedly estimating different but non-random population elasticities, or if estimates are (observably) biased toward a unique population elasticity, or both. In terms of equation (8), this assumption implies $\eta_i = 0$ for all i. Finally, the fourth case—which consists of the most general version of model (8)—allows for *both* observed and unobserved heterogeneity.

Cases three and four control for observed heterogeneity by including M moderator variables. Table 1 presents the complete list of variables that we use—including the observed frequency—and covers several dimensions, like the definition of output, the type of public capital, data and model aggregation, type of data, empirical model, and estimation method. The key dimensions reflect the observable heterogeneity across measurements as discussed in Section 2. In addition, we also include country dummies to control for dependency across measurements for the same country. The dummies ensure that the within (and not the between) variation is captured across estimates.

Because the error term ξ_i is heteroscedastic, we estimate equation (8)—and all its special cases—using weighted least squares, where the optimal weights (w_i) depend on how heterogeneity is modeled. Cases one and three assume no unobserved heterogeneity (i.e., $\sigma_\theta^2 = 0$). For these so-called fixed effects models, we define the weight assigned to observation i as $w_i \equiv 1/\text{se}(\hat{\theta}_i)$. Cases two and four give rise to random effects models, since the error term also consists of an unobserved-heterogeneity component, whose variance $(\sigma_\theta^2 \geq 0)$ needs to be estimated.[31] The weights are then given by $w_i \equiv 1/\sqrt{\text{se}(\hat{\theta}_i)^2 + \sigma_\theta^2}$. To choose between the fixed effects and random effects models, we apply the Q-test of homogeneity (cf. Shadish and Haddock, 1994). In a regression context, this reduces to testing the significance of σ_θ^2, which can be done by noting that the residual sum of squares of the fixed effects model is χ_l^2 distributed under the null hypothesis of homogeneity, where l is the number of degrees of freedom of the model (cf. Raudenbush, 1994).

Additionally, we investigate the existence of a genuine empirical effect by relating a study's t-value to its degrees of freedom (df). If the true population effect is unique and different from zero, then (absolute) t-values should according to Card and Krueger (1995) grow at a rate equal to the square root of the degrees of freedom of the regression model. Stanley (2005) points out, however, that severe publication bias may undermine this relationship. Therefore, we apply OLS to the following modified Card and Krueger (1995) equation:

$$\ln \left| \frac{\hat{\theta}_i^*}{\text{se}(\hat{\theta})_i} \right| = \gamma_0 + \gamma_1 \ln \text{df}_i + \omega_i, \tag{9}$$

where $\hat{\theta}_i^*$ is $\hat{\theta}_i$ corrected for both heterogeneity and publication bias (as specified in each model), γ_0 and γ_1 are parameters to be estimated, and ω_i is an error term. If such a publication bias correction is appropriate, then a genuine empirical effect implies $\gamma_1 = 1/2$.

5 Meta-Regression Results

This section presents the meta-regression results. Section 5.1 tests for unobserved heterogeneity in our sample. Subsequently, Section 5.2 controls for observed heterogeneity by including various moderator variables.

5.1 Homogeneity versus Unobserved Heterogeneity

Let us consider the first and simplest of the four cases, that is, the homogeneity case. Table 2 reports the results for both the linear and nonlinear publication bias correction and compares these with the case without publication bias correction. In the context of publication bias, we

[31] We estimate σ_θ^2 as follows: $\hat{\sigma}_\theta^2 = \frac{N[Q/(N-M-2)-1]}{\sum_{i=1}^N w_i}$, where Q is the residual sum of squares of the fixed effects model (Raudenbush, 1994). Both Q and the weights (w_i) are obtained from the fixed effects model.

consider three special cases: (i) unidirectional publication bias (i.e., $\delta_p = \delta_n$); (ii) bidirectional publication bias, in which case δ_p and δ_n are estimated unrestrictedly; and (iii) symmetric bidirectional publication bias in which case $\delta_p = -\delta_n$. Table 2 shows that the true (fixed) effect estimate is much lower than a simple average of the estimates (even without correcting for publication bias). Intuitively, smaller estimates are typically more precise and therefore more heavily weighted (and thus part of the publication bias problem is solved automatically). Publication bias is shown to be strong, bidirectional, and symmetric for both the linear and nonlinear case. As expected, the publication bias correction is stronger for the linear case, because it assumes that high-precision estimates are as likely to be subject to publication bias as low-precision estimates. The hypothesis of homogeneity of the measurements is strongly rejected; there is a substantial amount of heterogeneity that is better dealt with in the random effects model. Finally, the df-test suggests that there is no relationship between t-statistics and the degrees of freedom, reflecting that observed heterogeneity has not been controlled for yet.

The Q-test suggests that the meta-sample is characterized by a substantial amount of heterogeneity. We use the estimate of σ_θ^2 to adjust the weights. The results are reported in Table 3. The estimate of the true effect in the random effects model is again smaller than a simple average of measurements, but somewhat larger than under fixed effects, reflecting the proportionally smaller weight that is assigned to precise (but small) estimates. Just like in the fixed effects model, publication bias is strong, bidirectional, and symmetric for both the linear and nonlinear publication bias specifications. Again, as expected, the correction is larger under linear publication bias. Our preferred estimate at this point—given that we have not controlled for observed heterogeneity—is 0.091, which corrects for symmetric bidirectional publication bias of a linear form. Although nonlinear public bias is intuitively more appealing, the linear publication bias specification yields a better model fit.[32]

[32] In principle, equation (8) could include the linear term $\text{se}(\hat{\theta}_i)$ together with the nonlinear term $\text{se}(\hat{\theta}_i)^2$ to test whether publication bias is linear or nonlinear. In practice, however, such a test is unlikely to be very informative, because the two terms are highly correlated.

5.2 Addressing Observed Heterogeneity

Table 4 controls for *observed* heterogeneity by including a set of moderator variables in both fixed and random effects models. Initially, all moderator variables of Table 1 are included in the equation. We apply a general-to-specific approach to reduce the model to a parsimonious specification for two reasons: (i) many of the moderator variables are not significantly different from zero; and (ii) the number of degrees of freedom is small if all moderator variables are included. In each step of the procedure, we delete the least significant moderator variable until we are left with a model in which the variables are significant at least at the 10 percent level. We apply an F-test to the null hypothesis of the joint insignificance of the excluded variables. Because we want to capture some overarching sources of heterogeneity, we try to put as much structure to the empirical model as possible. In view of this, we also test various 'aggregation restrictions.' We construct three composite variables. First, we test for equality of the coefficients of *Coint-single*, *Coint-VAR*, and *Spurious* (which all capture long-run effects in time series based on an identified or presumed cointegrating relationship) and *Long-diff* (which represents long-run relationships in panel data studies) and encompass them in the new variable *Long-run*. Our results indicate that in all cases we could not reject the null hypothesis, suggesting that studies usually taken as spurious have probably found long-run cointegrating relationships.[33] Second, we test for equality of the coefficients of *Pfrontier* and *Cap-util* and aggregate them in the variable *Cycle*. Finally, we test and impose symmetric publication bias. Both the aggregation and publication bias symmetry restrictions are tested simultaneously with the exclusion restrictions.

All restrictions taken together cannot be rejected. The Q-test indicates that some (possibly unobserved) heterogeneity is still present, suggesting therefore that the random effects model is preferable (and is therefore considered our benchmark model). Comparing the results of Table 4 to those in Table 2, we see that the variance of the unobserved-heterogeneity component is now much smaller. Again, we find evidence of symmetric publication bias. The magnitude of the publication bias, however, is much smaller in the presence of the moderator

[33] This result supports the suspicion of Erenburg (1998), who finds long-run output elasticities of public capital similar in magnitude to those in the early time-series literature.

variables than without, indicating that the publication bias term in Tables 2–3 is capturing much of the observed heterogeneity. Consequently, the publication bias parameter is biased upward in the latter case. The model with a linear publication bias correction yields a marginally better fit than that with nonlinear publication bias. After controlling for observed heterogeneity, the df-test shows a positive relationship between t-values and the degrees of freedom. We can reject the hypothesis that γ_1 is zero, but cannot reject the hypothesis that it equals a half under nonlinear publication bias at the 5 percent level (we marginally reject it for linear publication bias).

The estimation method is an important factor explaining heterogeneity in our sample. The coefficient of the *Long-run* variable is positive and highly significant. Public capital is more productive in the long run than in the short run for two possible reasons. First, much of the output effect of public capital is lagged and lasts for several periods (partially reflecting the multi-period nature of big infrastructure projects). Second, feedback effects from private output to public capital formation are captured in the long run. The variable controlling for the endogeneity of public capital (i.e., *Endo-pub*) turns out to be insignificant, however, suggesting that either contemporaneous feedback effects are not a major concern or the endogeneity bias is bidirectional.

The type of public capital used in the analysis matters. The coefficient of *Regional-loc* is significantly positive. Public capital provided by local governments is more productive than capital provided by the central government, which may reflect the ability of local governments to better target public investment to the most productive alternatives. In the fixed effects model, transportation capital is less productive than other components of public capital if the model features both transportation and non-transportation capital. If transportation capital is included alone, it is not significant anymore. Intuitively, the transportation variable is likely to pick up the effect of other (more productive) components of public capital. Somewhat surprisingly, the variables *Core* and *Core-disag* are notably insignificant. If core capital were truly more productive, then estimating an equation that includes core capital but excludes non-core capital will lead to a downward bias in the estimated parameter of core capital. *Core-disag* may turn insignificant because it consists of some relatively low-productive components (e.g., transportation capital). Of course, it is also quite possible that core capital is simply not more productive than other components of public capital. In the random effects model, none of the components of public capital are significant.

The econometric specification explains a substantial amount of heterogeneity across studies. Compared to estimating an unrestricted production function, both constant returns to scale in private and in all inputs (i.e., *CRTS-priv* and *CRTS-all*, but especially the former) lead to higher output elasticities of public capital. Note that the significant and sizable coefficient of *CRTS-priv* falls from 0.32 to 0.095 (while the rest of the results are unaffected) if we drop the study by Picci (1999), which receives a large weight because of its small standard error. Taking into account that only four studies in our sample impose constant returns to scale in private inputs, it is possible that other (observed or unobserved) study-specific effects are inflating the estimate of *CRTS-priv*. The positive coefficient of *CRTS-priv* may also reflect a combination of production function misspecification and a negative contemporaneous correlation between public capital and private capital. The latter may either stem from crowding-out of private capital (e.g., Aschauer, 1989c) or from an anti-cyclical policy rule (which induces the government to increase public investment when private investment is depressed).[34] Note that this argument is coherent with the small (and mostly insignificant) coefficient of *CRTS-all*.

The variable *Cycle* consists of studies that either use a production frontier approach or control for the business cycle by including some measure of capacity utilization (or simply unemployment). As expected, studies that control for the business cycle find a higher output elasticity of public capital. Including a time trend—which is often used as a proxy for technological progress—also leads to larger estimates of the output elasticity of public capital in the model with nonlinear publication bias. Intuitively, technological progress makes a given amount of public capital more productive and therefore less investment in public capital is required to generate a given level of output. Controlling for omitted variables, such as *En-*

[34]The *stocks* of public and private capital are typically positively correlated not only over time within a given jurisdiction (reflecting the time trends in these variables) but also across jurisdictions (reflecting jurisdiction-specific effects). This positive correlation in stocks is compatible with a short-run negative correlation between the *flows* of private investment and public investment, though, which may be uncovered if these time/jurisdiction-specific effects are accounted for in the empirical model.

ergy and *Education*, matters for the results. Studies that include energy prices tend to find lower estimates, whereas studies that incorporate some measure of education obtain larger estimates.

We find a significantly negative coefficient for *Regional-data*. Studies using data at the regional level fail to internalize the positive spillover effects of public investment between regions, thus giving rise to smaller output elasticities. One way of internalizing these spillovers is by redefining public capital of a given region to include the capital of neighboring regions. Indeed, studies that take this approach (which is captured by the variable *Spillovers*) obtain larger output elasticities. The coefficient of the *Spillovers* variable is almost equal (in absolute value) to that of the *Regional-data* variable.

The *unconditional* (average) true output elasticity of public capital in the benchmark model amounts to 0.146 after correcting for linear publication bias and controlling for observed heterogeneity. This value of θ_0 is somewhat smaller than the value of 0.171 that we find for nonlinear publication bias correction. The results in Table 4 tell us, however, that the true output elasticity of public capital is fundamentally heterogeneous. To facilitate the quantitative interpretation of those results, Table 5 presents estimated output elasticities of public capital *conditional* on three key study characteristics: the time horizon (short run versus long run), the level of government that provides public capital (regional/local versus national), and jurisdictional spillover effects (excluded versus included). In deriving the conditional θ, we correct for linear publication bias.[35] Table 5 presents three main results. First, the conditional true effect is again quite heterogeneous, ranging from a minimum of 0.04 in short-run models to a maximum of 0.29 in long-run models. The latter value is not too far off from estimates found in the early literature. Second, the short-run output elasticity of public capital at the intraregional level is small (about 0.085); in the long run, and after taking into account interregional spillover effects, the contribution of public capital to private

[35] All results are obtained assuming an unrestricted model (*CRTS-all*=*CRTS-priv*=0)—including measures of energy and education (*Education*=*Energy*=1) and excluding a time trend (*Trend*=0)—which is estimated with regional data (*Regional-data*=1) that are based on the broadest measure of public capital (*Transp-disag*=0) while controlling for the business cycle (*Cycle*=1). The cell indicated by a dagger—which represents the model specification that researchers typically have in mind—is derived assuming further a long-run model (*Long-run*=1), featuring nationally installed public capital (*Regional-loc*=0) without externalities (*Spillovers*=0). The other entries in the table follow from varying the dummy variable of the relevant study characteristic in the chosen model type.

production increases by a factor of three. Finally, the conditional output elasticities of public capital in the random effects model are smaller than those in the fixed effects model, which is line with the results for the unconditional elasticities found in the two models. Note that the conditional value of θ in the benchmark specification is 0.165, which is somewhat larger than its unconditional value.

6 Conclusions

This paper has assessed the output elasticity of public capital by means of a meta-regression analysis. The analysis focuses on studies employing the production function approach, which analyzes the output elasticity of public capital by including public capital as an input into the production function. The meta-sample is composed of 67 studies for the 1983–2008 period. Both random and fixed effects meta-regression models were estimated. The meta-regression controlled for bidirectional publication bias of a linear form and compared the results for those based on a nonlinear publication bias correction.

The average true output elasticity of public capital is positive and significant despite the wide variation in primary estimates. Publication bias is shown to be significant and appears to be bidirectional and symmetric. Because the sample contains a substantial amount of unobserved heterogeneity, the random effects model is preferred over the fixed effects model. After correcting for linear publication bias, the unconditional (average) output elasticity of public capital turns out to be 0.146. The unconditional (average) output effect is 0.171 if publication bias is of the nonlinear form. In view of the better model fit, the linear publication bias correction is preferred, however.

The meta-regression indicates that the sample also features a substantial amount of observed heterogeneity across estimates, which is predominantly explained by study design parameters, such as the empirical model, estimation technique, type of public capital, and level of aggregation of public capital data. The conditional short-run output elasticity of public capital at the intraregional level is shown to be 0.085. In the long run, and after taking into account interregional spillover effects, the contribution of public capital to private production increases by a factor of three. In addition, studies that impose constant returns to scale restrictions across private inputs, control for the business cycle, and incorporate some measure of education find larger output elasticities of public capital, whereas studies that include energy prices tend to find lower estimates. The results suggest that the high output elasticities found in the early time-series literature are compatible with long-run (cointegrating) estimates found more recently.

The conditional (average) output elasticity of public capital in the benchmark specification—which captures typical study characteristics—amounts to 0.165, which is not that far off from its unconditional value of 0.146. These values imply a marginal productivity of public capital for the United States in the range of 28.8–32.6 percent in 2001.[36] In that same year, the real long-term rate of interest—which is assumed to reflect the marginal productivity of private capital if markets are perfect—amounted to 2.6 percent, suggesting that investment in public capital should be encouraged from a macroeconomic point of view.

A fruitful research avenue is to perform a meta-analysis on estimates from studies using the behavioral approach, that is, cost functions and/or profit functions. To the best of our knowledge, such work has not been undertaken yet. Finally, in future work, we intend to control for dependency across studies caused by identical or very similar data sets.

[36]In the United States, the public capital-to-GDP ratio amounted to 50.6 percent in 2001. See Kamps (2006).

Appendix

Table A1. Studies Included in the Meta-Data Set

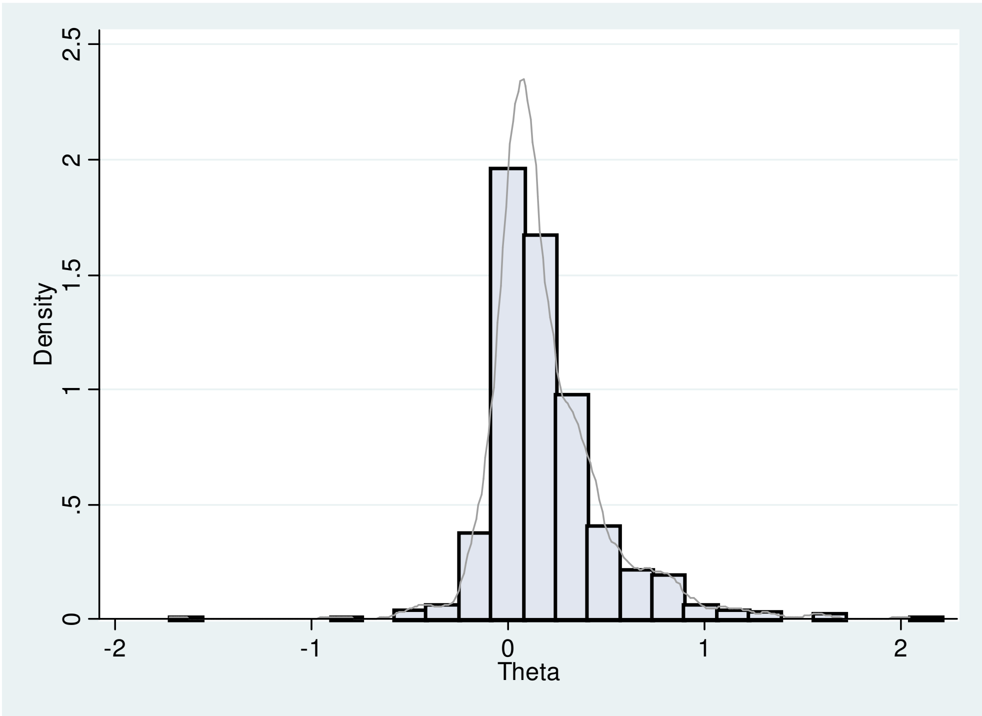
No.	Author(s)	Country	Estimate $\hat{\theta}_i$	St.Error $se(\hat{\theta}_i)$
1	Ratner (1983)[a]	US	0.277	0.099
2	Da Costa, Ellson, and Martin (1987)	US	0.281	0.091
3	Aschauer (1989a)	US	0.400	0.026
4	Ram and Ramsey (1989)	US	0.240	0.055
5	Munnell (1990a)	US	0.330	0.066
6	Munnell (1990b)	US	0.150	0.017
7	Eisner (1991)	US	0.165	0.018
8	Tatom (1991)	US	0.042	0.127
9	Berndt and Hansson (1992)	Sweden	0.687	0.220
10	Garcia-Milà and McGuire (1992)	US	0.044	0.005
11	Bajo-Rubio and Sosvilla-Rivero (1993)	Spain	0.190	0.007
12	Finn (1993)	US	0.158	0.077
13	Mas, Maudos, Pérez, and Uriel (1993)	Spain	0.066	0.118
14	Munnell (1993)	US	0.170	0.018
15	Eisner (1994)	US	0.270	0.068
16	Evans and Karras (1994a)	US	−0.055	0.053
17	Evans and Karras (1994b)	7 OECD countries	0.079	0.104
18	Ferreira (1994)	67 countries	0.260	0.058
19	Holtz-Eakin (1994)	US	−0.022	0.131
20	Mas, Maudos, Pérez, and Uriel (1994)	Spain	0.306	0.130
21	Otto and Voss (1994)	Australia	0.381	0.143
22	Ai and Cassou (1995)	US	0.321	0.095
23	Andrews and Swanson (1995)	US	0.110	0.018
24	Baltagi and Pinnoi (1995)	US	0.020	0.069
25	De la Fuentes and Vives (1995)	Spain	0.212	0.064
26	Holtz-Eakin and Schwartz (1995a)	US	0.112	0.040
27	Holtz-Eakin and Schwartz (1995b)	US	−0.007	0.025
28	Sturm and De Haan (1995)	The Netherlands	1.160	0.451
29	Garcia-Milà, McGuire, and Porter (1996)	US	−0.058	0.075
30	Holtz-Eakin and Lovely (1996)	US	−0.144	0.064
31	Khanam (1996)	Canada	0.170	0.060
32	Mas, Maudos, Pérez, and Uriel (1996)	Spain	0.141	0.041
33	Prud'Homme (1996)	France	0.087	0.043
34	Otto and Voss (1996)	Australia	0.168	0.080

(Continued on next page)

Table A1 (*Continued*)

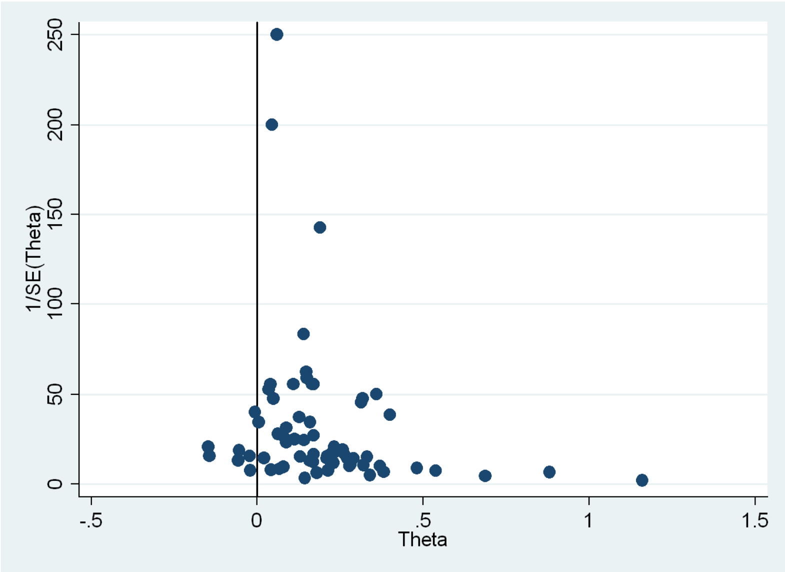
No.	Author(s)	Country	Estimate $\hat{\theta}_i$	St.Error $se(\hat{\theta}_i)$
35	Crowder and Himarios (1997)	US	0.231	0.048
36	Kavanagh (1997)	Ireland	0.144	0.298
37	Kelejian and Robinson (1997)	US	−0.023	0.064
38	Moreno, Artís, López-Bazo, and Suriñach (1997)	Spain	0.049	0.021
39	Vijverberg, Vijverberg, and Gamble (1997)	US	0.481	0.111
40	Boarnet (1998)	US	0.257	0.052
41	Erenburg (1998)	US	0.290	0.070
42	Flores de Frutos, Diez, and Amaral (1998)	Spain	0.210	0.070
43	Nourzad (1998)	US	0.340	0.203
44	Otto and Voss (1998)	Australia	0.059	0.004
45	Delorme, Thompson, and Warren (1999)	US	0.213	0.132
46	Picci (1999)	Italy	0.359	0.020
47	Bonaglia, La Ferrara, and Marcellino (2000)	Italy	0.005	0.029
48	Charlot and Schmitt (2000)	France	0.317	0.021
49	Dessus and Herrera (2000)	28 LDCs	0.130	0.065
50	Yamano and Ohkawara (2000)	Japan	0.148	0.016
51	Yamarik (2000)	US	0.088	0.032
52	Alonso-Carrera and Freire-Séren (2001)	Spain	0.126	0.027
53	Owyong and Thangavelu (2001)	Canada	0.881	0.150
54	Stephan (2001)	Germany and France	0.112	0.040
55	Kemmerling and Stephan (2002)	Germany	0.170	0.037
56	Ligthart (2002)	Portugal	0.370	0.100
57	Rubio, Roldán, and Garcés (2002)	Spain	0.040	0.018
58	Stephan (2003)	Germany	0.537	0.134
59	Rodríguez-Valez and Yarias Sampedro (2004)	Spain	0.160	0.029
60	Cantos, Gumbau, and Maudos (2005)	Spain	0.062	0.036
61	Kataoka (2005)	Japan	0.313	0.022
62	Kawaguchi, Ohtake, and Tamada (2005)	Japan	0.180	0.160
63	La Ferrara and Marcellino (2005)	Italy	−0.161	0.048
64	Berechman, Ozmen, and Ozbay (2006)	US	0.035	0.019
65	Cadot, Roller, and Stephan (2006)	France	0.085	0.040
66	Kamps (2006)	22 OECD countries	0.221	0.060
67	Creel and Poilon (2008)	6 EMU countries	0.140	0.012
	Average		0.198	0.074

[a] The estimates considered are those replicated by Tatom (1991) using revised data for the same period. Ratner's (1983) original estimate amounts to 0.056.



Notes: The histogram consists of 569 estimates obtained from the 67 studies described in Table A1. The gray line corresponds to the kernel density estimate of the (unconditional) distribution of estimates, which is derived using the Epanechnikov kernel function.

Figure 1: Histogram



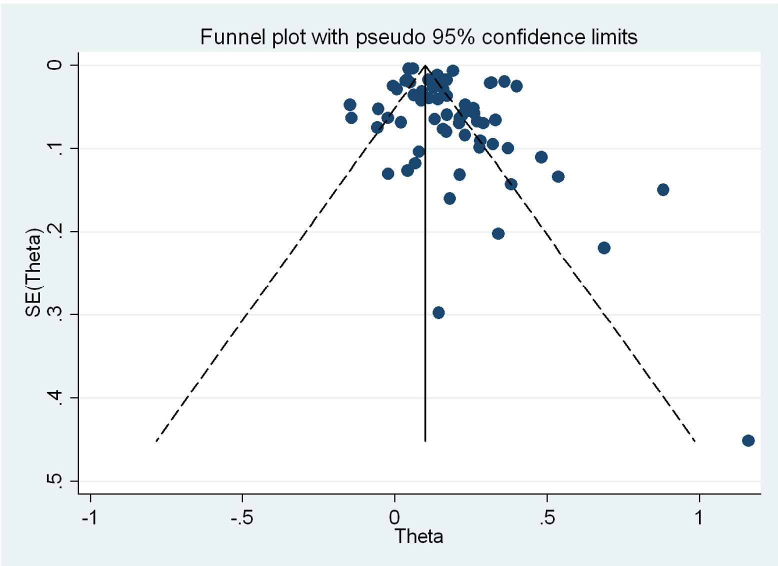


Figure 2: Funnel Plots

Table 1: List of Moderators Used in the Meta-Regression Model

Empirical Dimension	Dummy Variable (D_j)	Definition (1 if ..., 0 otherwise)	$\sum_{i=1}^{N} D_{ji}$[a]
Country fixed effect	Country k	Data are for country k[b]	–
Definition of output	Private	Dependent variable is private sector gross output at the country or state level	40
Type of public capital	Core	Only core capital is used[c]	8
	Transp	Only transportation capital is considered	2
	Core-disag	Core capital is used but non-core capital is also included in model	2
	Transp-disag	Transportation capital is used but non-transportation capital is also included in model	9
	Regional-loc	Only regional/local capital is considered	13
	Spillovers	Public capital of a given region is defined to include public capital of neighboring regions	3
Data aggregation	Regional-data	Regional data are used	39
Model aggregation	Transn	A transnational production function is estimated	6
Type of data	Cross	Cross-section data are used	2
Empirical model	Trend	A time trend is included	18
	Pfrontier	A production frontier model is employed	2
	Cap-util	Capacity utilization is controlled for	23
	CRTS-all	Constant returns to scale on all inputs is imposed	23
	CRTS-priv	Constant returns to scale on private inputs is imposed	4
	Per-cap	All variables in the model are divided by the population size	3
	Area	Public capital is expressed relative to the surface area of the region/country	5
	Energy	Energy prices are controlled for	2
	Education	A measure of education is included in the model	4
	Spill-disag	Public capital of neighboring areas is included as an additional regressor	5
Estimation method	Spurious	Equation is estimated in levels without cointegration test	8
	Coint-single	Cointegration relationship is found using a single-equation model	4
	Coint-VAR	Cointegration relationship is found using Johansen's VAR analysis	2
	FE	Unit-specific fixed effects are employed	18
	RE	Unit-specific random effects are employed	4
	TE	Time effects are employed	20
	Long-diff	Equation is estimated with the variables in long differences	3
	Endo-pub	Public capital is assumed to be endogenous	11
	Endo-oth	At least one variable other than public capital is assumed to be endogenous	7
	Efficiency	A set of efficiency conditions is estimated by GMM	3

[a]Each number in this column indicates the number of ones in the corresponding dummy variable; [b]Country-specific fixed effects are included for those countries for which there is more than one estimate (number of estimates for each country in parentheses): Australia (3), Canada (2), France (3), Germany (2), Italy (3), Japan (3), Spain (11), and United States (30); and [c]Core capital is a subset of non-military public capital consisting of two main components: transportation capital (highways, mass transit, etcetera) and public utilities (water and sewers, electrical and gas facilities, etcetera).

Table 2: Meta-Regression Results: Homogeneity (Fixed Effects)

	No PB	Linear PB			Nonlinear PB		
		Unidirectional	Bidirectional	Symmetric	Unidirectional	Bidirectional	Symmetric
θ_0	0.098***	0.076***	0.072***	0.072***	0.095***	0.096***	0.095***
	(0.023)	(0.023)	(0.023)	(0.021)	(0.023)	(0.023)	(0.022)
δ	–	1.993***	–	–	9.768***	–	–
		(0.609)			(3.106)		
δ_p	–	–	2.682***	2.658***	–	11.955***	13.035***
			(0.604)	(0.454)		(3.573)	(3.548)
δ_n	–	–	-2.540***	-2.658	–	-31.063**	-13.035
			(0.660)	–		(14.115)	–
R^2	–	0.646	0.699	0.699	0.612	0.632	0.628
N	67	67	67	67	67	67	67
Q	1292.596***	–	–	952.797***	–	–	1176.469***
$\hat{\sigma}_\theta^2$	0.006	–	–	0.005	–	–	0.006
F-test:							
$\delta_p = -\delta_n$	–	–	0.020	–	–	1.600	–
			(0.896)			(0.210)	
df-test:							
γ_1	-0.066	–	–	0.211	–	–	0.053
	(0.100)			(0.149)			(0.110)
R^2	0.001	–	–	0.041	–	–	0.004

Notes: The dependent variable in all equations is the output elasticity of public capital taken from study i. The regression considers three cases: no publication bias (PB), linear PB, and nonlinear PB. The values in parentheses are heteroscedasticity-robust (or White) standard errors in the case of estimates and p-values in the case of F tests. ***, **, * denote significance at the 1, 5, and 10 percent level, respectively. The dependent variable in the degrees of freedom (df)-test is the logarithm of the t-statistic corrected for the respective type of publication bias (i.e., zero, linear, and nonlinear). To save on space, the constant (γ_0) of the df-test is not reported.

Table 3: Meta-Regression Results: Unobserved Heterogeneity (Random Effects)

	No PB	Linear PB			Nonlinear PB		
		Unidirectional	Bidirectional	Symmetric	Unidirectional	Bidirectional	Symmetric
θ_0	0.157***	0.099***	0.105***	0.091***	0.135***	0.145***	0.134***
	(0.017)	(0.025)	(0.024)	(0.017)	(0.017)	(0.016)	(0.017)
δ	–	1.236***	–	–	6.051***	–	–
		(0.419)			(1.767)		
δ_p	–	–	1.646***	1.902***	–	6.886***	8.043***
			(0.407)	(0.276)		(1.967)	(2.189)
δ_n	–	–	-2.642***	-1.902	–	-27.999**	-8.043
			(0.709)	–		(13.189)	–
R^2	–	0.594	0.729	0.724	0.597	0.670	0.645
N	67	67	67	67	67	67	67
F-test:							
$\delta_p = -\delta_n$	–	–	1.080	–	–	2.450	–
			(0.302)			(0.122)	
df-test:							
γ_1	-0.066	–	–	0.078	–	–	-0.037
	(0.100)			(0.119)			(0.111)
R^2	0.001	–	–	0.007	–	–	0.001

Notes: The dependent variable in all equations is the output elasticity of public capital taken from study i. The regression considers three cases: no publication bias (PB), linear PB, and nonlinear PB. The values in parentheses are heteroscedasticity-robust (or White) standard errors in the case of estimates and p-values in the case of F tests. ***, **, * denote significance at the 1, 5, and 10 percent level, respectively. The dependent variable in the degrees of freedom (df)-test is the logarithm of the t-statistic corrected for the respective type of publication bias (i.e., zero, linear, and nonlinear). To save on space, the constant (γ_0) of the df-test is not reported.

Table 4: Meta-Regression Results: Observed and Unobserved Heterogeneity

	Fixed Effects		Random Effects	
	Linear PB	Nonlinear PB	Linear PB	Nonlinear PB
θ_0	0.155***	0.175***	0.146***	0.171***
	(0.011)	(0.009)	(0.014)	(0.011)
Long-run	0.121***	0.128***	0.128***	0.135***
	(0.027)	(0.037)	(0.029)	(0.036)
Regional-loc	0.047**	0.049**	0.058**	0.065*
	(0.020)	(0.024)	(0.028)	(0.035)
Transp-disag	-0.063**	-0.069**	-0.050	-0.046
	(0.028)	(0.030)	(0.033)	(0.036)
Spillovers	0.070**	0.098***	0.046	0.067*
	(0.026)	(0.029)	(0.040)	(0.041)
Regional-data	-0.078***	-0.067***	-0.071***	-0.083**
	(0.017)	(0.023)	(0.026)	(0.035)
CRTS-all	0.018	0.029*	0.020	0.029
	(0.016)	(0.018)	(0.019)	(0.022)
CRTS-priv	0.366***	0.385***	0.322***	0.341***
	(0.034)	(0.047)	(0.066)	(0.076)
Education	0.104***	0.107***	0.082**	0.088**
	(0.022)	(0.024)	(0.033)	(0.036)
Energy	-0.170***	-0.152***	-0.159***	-0.150***
	(0.050)	(0.045)	(0.056)	(0.047)
Cycle	0.089***	0.093***	0.089***	0.092***
	(0.016)	(0.020)	(0.019)	(0.025)
Trend	0.024	0.035*	0.021	0.032
	(0.016)	(0.018)	(0.021)	(0.022)
$\delta_p = -\delta_n$	1.109***	4.941**	1.119***	4.431**
	(0.277)	(2.176)	(0.289)	(2.046)
R^2	0.952	0.944	0.897	0.874
N	67	67	67	67
Q	152.666***	177.598***	–	–
$\hat{\sigma}^2_\theta$	0.001	0.001	–	–
F-test:				
All restrictions	0.780	0.730	0.690	0.540
	(0.712)	(0.759)	(0.802)	(0.917)
df-test:				
γ_1	0.274***	0.314**	0.289***	0.300**
	(0.092)	(0.129)	(0.099)	(0.134)
R^2	0.094	0.086	0.096	0.067

Notes: The dependent variable is the output elasticity of public capital. Estimates are presented for both linear and nonlinear publication bias (PB) and for both fixed and random effects models. The values in parentheses are heteroscedasticity-robust (or White) standard errors in the case of estimates and p-values in the case of F-tests. ***, **, * denote significance at the 1, 5, and 10 percent level, respectively. Note that the dummies are centered. The random effects model uses the $\hat{\sigma}^2_\theta$ obtained from the fixed effects model. The second F-test in the table tests whether or not the excluded explanatory variables are jointly insignificant. The dependent variable in the degrees of freedom (df)-test is the logarithm of the t-statistic corrected for the considered type of publication bias. See equation (9). To save on space, the constant (γ_0) of the df-test is not reported.

Table 5: Conditional Output Elasticities of Public Capital

Time period	Government level	No Externalities Fixed	No Externalities Random	Externalities Fixed	Externalities Random
Short run	Regional	0.099	0.085	0.169	0.141
	National	0.052	0.037	0.122	0.083
Long run	Regional	0.220	0.222	0.290	0.268
	National	0.173	$0.165^{\dagger}$	0.243	0.211

Notes: The labels fixed and random denote fixed effects and random effects, respectively. The conditional output elasticities of public capital are based on the estimated parameters of Table 4 using the linear publication bias correction. All results are obtained assuming an unrestricted model (*CRTS-all*=*CRTS-priv*=0)—including measures of energy and education (*Education*=*Energy*=1) and excluding a time trend (*Trend*=0)—which is estimated with regional data (*Regional-data*=1) that are based on the broadest measure of public capital (*Transp-disag*=0) while controlling for the business cycle (*Cycle*=1). The benchmark result (indicated by a dagger) is obtained assuming further a long-run model (*Long-run*=1), featuring nationally installed public capital (*Regional-loc*=0) without externalities (*Spillovers*=0). The other results are derived by varying the dummy variable for the relevant study characteristic.

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