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How Productive is Public Capital? A Meta-Analysis*

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Abstract

The paper analyzes the contribution of public capital to private output using several meta-analytical techniques. Both fixed and random effects models are estimated by Weighted Least Squares. Sample overlap across studies is explicitly controlled for by employing a 'full' Generalized Least Squares estimator. The weighted average output elasticity of public capital amounts to 0.08 after correcting for publication bias. A substantial part of the heterogeneity across studies is explained by study design parameters, such as econometric specification, estimation technique, empirical model, type of public capital, and level of aggregation of public capital data. The large elasticities of public capital found in the early literature seem to be caused by either unidentified (but present) cointegrating relationships or spurious relationships in national time series.

JEL codes: H540

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

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1 Introduction

Discussions among academics and policy makers about the contribution of the public capital stock to private output growth have been ongoing during the last two decades. Recently, these debates have revived within the European Union (EU), following the renewed interest in fiscal policy rules—in the form of ceilings on the deficit-to-GDP and public debt-to-GDP ratios—and their potential negative impact on public capital formation. Indeed, in many instances, policy makers find it easier to cut back on infrastructure investment rather than on current expenditure. To provide input to the public capital debate, it is of importance to have insight into the stylized facts on the linkage between private output and public capital formation.

Many authors have tried to determine the contribution of public capital to private output by estimating a production function that includes the public capital stock as an input (the so-called production function approach). Aschauer (1989a, 1989b, 1990) was one of the first to investigate this issue for the United States in an attempt to explain the productivity growth slowdown in the 1970s.[1] 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). The findings of these studies generally range from no significant effect to a strongly positive effect of public capital on output. Some studies, though not that many, even find significantly negative results. So far, researchers have not attached much priority to reconciling these differences.

Although various authors have reviewed the literature on the productivity of public cap-

[1] 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 work of Ratner (1983) and Da Costa, Ellson, and Martin (1987).

ital,[2] none of them has applied a systematic meta-analysis yet.[3] The aim of the paper is to fill this gap. 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. A problem with conventional reviews of the literature is that studies are difficult to compare, owing to differences in the empirical model, econometric specification, estimation method, and data definitions. Meta-analysis presents a more systematic and objective way to summarize empirical results. It allows us to explain the wide study-to-study variation by the researcher's choices made on research design in the analysis. In this way, a *meta*-output elasticity of public capital can be derived, which researchers and policy makers can use in their analyses without conducting any empirical research themselves.[4]

Our meta-sample pertains to studies employing public capital as an input into production. The sample covers all relevant studies up to and including the year 2006, yielding a meta-data set of 76 studies. Instead of using all available observations, we include a single observation per study, which 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 estimate. We compute both fixed and random effects estimates of the true underlying output elasticity while controlling for publication bias. In contrast to the work of Stanley (2005), we explicitly test for bidirectional publication bias that is potentially asymmetric.

Besides a simple meta-analysis, we conduct a more complex meta-regression analysis. We test for a large set of potential determinants of heterogeneity across studies. We estimate a fixed and random effects meta-regression model using Weighted Least Squares (WLS). In the fixed effects specification, the equation is multiplied by the inverse of the within-study

[2] See the studies 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] We are aware of only one study, that of Button (1998), who has made a first (but very incomplete) attempt to explain observed heterogeneity across studies on public capital. Button's (1998) meta-regression analysis covers only 26 studies (published during 1973–1994), which yields a meager total of 28 data points. He finds one significant moderator variable, that is, whether a study pertains to the United States.

[4] 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).

standard deviation. In the random effects specification, however, we use the sum of the within and between variation as weights. We control for dependency of estimates across countries by including country-fixed effects and correct for heteroscedasticity by employing White standard errors and also clustering of standard errors.

Our methodological innovation is that we account for the degree of sample overlap in a random effects model by using a 'full' Generalized Least Squares (GLS) procedure. Note that the WLS procedure—which is a stripped down version of GLS, which we call 'partial' GLS—leaves unused information on the error-covariances of the original estimates. Because various authors make use of identical or very similar samples, thus creating sample-dependency across estimates, the off-diagonal elements of the variance-covariance matrix are non-zero. We employ a simple procedure to calculate the degree of sample overlap, which is subsequently used to proxy error correlation. The error variances, in turn, are derived from the standard errors of the original estimates. By weighting the original measurements by the variance-covariance matrix obtained in this manner, a full GLS estimator is obtained. We show that a substantial amount of heterogeneity across studies is explained by study design characteristics such as the econometric specification, estimation technique, empirical model, type of public capital used, and level of aggregation of public capital data.

The remainder of the paper is structured as follows. Section 2 discusses definitions, presents various approaches used to estimate the output elasticity of public capital and discusses empirical results. In addition, it gives an overview of the criticisms launched against the most widely used methodology, that is, the production function approach. Section 3 describes the meta-sample and presents the meta-analysis results. Section 4 discusses and estimates publication bias and applies a publication bias correction to our meta-analysis. Section 5 sets out the meta-regression model and discusses the meta-regression results. Section 6 concludes.

2 Public Capital and Private Output

How do we define and measure the public capital stock? Which approaches exist to measure the productivity of public capital? What size of the output elasticity of public capital do empirical studies typically find? This section addresses these important questions before it ventures into a review of the methodological issues.

2.1 Defining Public Capital and Output

Gramlich (1994, p. 1177) defines *infrastructure capital* from an economic point of view as 'large capital intensive natural monopolies such as highways, other transportation facilities, water and sewer lines, and communications systems.' Although most of these systems are publicly owned, in some cases they are privately owned. For example, a firm that constructs its own road to connect itself to the main highway. The literature generally defines infrastructure capital based on ownership. It is the public component of infrastructure capital that most people have in mind when they talk about public capital. The issue of definitions is more subtle, however.

Most empirical studies employ a 'narrow definition' of public capital that includes the tangible capital stock owned by the public sector excluding military structures and equipment. More specifically, it consists of core infrastructure (i.e., roads, railways, airports, and utilities such as sewerage and water facilities), hospitals, educational buildings, and other public buildings. Some authors use a 'broad definition' of public capital by also including human capital investment (e.g., Garcia-Milà and McGuire, 1992), or health and welfare facilities (e.g., Mera, 1973). The latter components are hard to measure, explaining why most authors focus on the narrow concept of public capital.

To arrive at an estimate of the stock of public capital at a particular moment in time, 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).[5] The majority of studies employ public capital stocks defined at the national level including *all* levels of government (e.g., Aschauer, 1989a), whereas others deal with capital stocks estimated for regions (e.g., Garcia-Milà and McGuire, 1992). Some studies only consider capital that is owned by local governments (e.g., Evans and Karras, 1994a), which does not take into account regionally installed capital owned by the

[5] See Sturm and De Haan (1995) for further details on this so-called perpetual inventory method.

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).

The measure of output—which is used as the dependent variable in the econometric analysis, see Section 2.3—varies across studies. Most studies use either real *net* output of the private sector (e.g., Ratner, 1983)[6] or real Gross Domestic Product (GDP)—or, alternatively, real Gross State Product (GSP), when the data is at the state level for the United States—exclusive of public sector output (e.g., Finn, 1993). Because government output is typically not exchanged on markets, it is hard to measure. In the National Accounts, it is equated to the wage bill of the public sector. Although we are primarily interested in measuring the contribution of public capital to private output rather than total output, not every study employs a measure of output that corrects for 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).

2.2 Empirical Methodologies

The literature has distinguished various approaches to study empirically the link between private output and public capital. The production function approach, which is the most widely known and applied, considers the stock of public capital either as a separate input in private production (which we call the 'pure production function approach') or as a factor affecting multifactor productivity (which is known as the growth accounting approach; see Hulten and Schwab, 1991b). In both cases, public capital is assumed to be strictly exogenous. Evidently, because the growth accounting approach does not yield direct estimates of the output elasticity of public capital, it will not be covered in our empirical analysis. Closely linked to the production function approach is the production frontier approach, which departs from the former by taking into account that public capital may increase potential production without necessarily increasing actual production (e.g., Delorme et al., 1999). In an efficient steady state, the production frontier approach is equivalent to the standard production function

[6] *Net* output (which equals gross value-added) is obtained by subtracting the value of intermediate goods and services from *gross* output of the private sector.

approach. We take this distinction into account in our empirical analysis.

Another methodology is the Vector Autoregression (VAR) approach, which analyzes the relationships between public capital, private inputs, and private output without imposing a theoretical structure. The multi-equation VAR approach models every endogenous variable as a function of its own lagged value and the lagged values of the other endogenous variables and thus can assess whether there is any feedback from private sector variables to the public capital stock. We do not include pure VAR studies in our analysis because in this framework it is hard to disentangle the direct effect (i.e., the production elasticity) from the feedback effects. Some authors (e.g., Ligthart, 2002), however, employ Johansen's (1988) method—which makes use of the VAR technique—to check whether the variables in the production function are cointegrated (see Section 2.5). The latter studies will be included in our sample.

Other approaches are the following two. The cross-country growth regressions approach, which specifies a reduced-form equation to estimate—using cross-section or panel data—the relationship between per capita private output growth and the public investment-to-GDP ratio. The growth regressions approach should be distinguished from those studies explicitly embedding a production function in the framework (which we will call the 'production function-based approach'). We will classify the latter under the production function approach if an output elasticity of public capital is or can be derived. Last but not least, the behavioral approach—coined as such by Sturm et al. (1998)—which employs cost or profit functions to assess whether public capital reduces firms' production costs or increases firms' profits. This last approach does not specify a direct relationship between public capital and private output, so it will not be covered in the empirical section.

2.3 The Production Function Approach

The corner stone of the production function approach is a technological relationship that incorporates the stock of public capital at time t, denoted by G_t, as an input:

$$Y_t = A_t F\left[K_t, L_t, G_t\right], \tag{1}$$

where Y_t is real aggregate private output within some area (region or country), A_t is an index of economy-wide productivity, K_t denotes the stock of (non-residential) private fixed capital, and L_t denotes employment (typically measured by total hours worked). Equation (1) shows that public capital may affect aggregate real private output in two ways. First, a direct effect, that is, $\partial Y_t/\partial G_t > 0$. The idea is that the services of public capital are proportional to the stock of public capital—which is generally assumed to be a *pure* public good—and contribute to production in that way. Second, public capital may raise the marginal productivity of private factors of production, that is, $\partial^2 Y_t/(\partial K_t \partial G_t) > 0$ and $\partial^2 Y_t/(\partial L_t \partial G_t) > 0$.

Most studies employ a Cobb-Douglas production function:[7]

$$Y_t = A_t K_t^{\alpha} L_t^{\beta} G_t^{\theta}, \qquad \alpha, \beta, \theta > 0, \tag{2}$$

where $\theta \equiv \partial \ln Y_t/\partial \ln G_t$ is the output elasticity of public capital, which is hypothesized to be positive. This specification models public capital and private inputs as cooperative factors of production. By taking natural logarithms on both sides of (2), we get a linear relationship:

$$\ln Y_t = \ln A_t + \alpha \ln K_t + \beta \ln L_t + \theta \ln G_t. \tag{3}$$

Equation (3) can readily be estimated in logarithmic (log) levels or first differences of log levels (i.e., growth rates) to arrive at estimates of α, β, and θ. As can be seen from (3), the productivity index enters the equation in an additive way. Following Ratner (1983), many studies include a constant and a time trend as a proxy for technological progress (i.e., $\ln A_t = a_0 + a_1 t$, where $a_0, a_1 > 0$).

Incorporating public capital into the production function raises the issue of returns to scale in production. Imposing the restriction of constant returns to scale (CRTS) across all inputs, which is represented by $\alpha+\beta+\theta = 1$, yields the specification employed by the majority of studies:

$$\ln(Y_t/K_t) = \ln A_t + \beta \ln(L_t/K_t) + \theta \ln(G_t/K_t). \tag{4}$$

Equation (4) features decreasing returns with respect to private inputs taken together.[8] Instead of using private capital productivity, $\ln(Y_t/K_t)$, some studies subtract $\ln L_t$ from both

[7]Some studies use the more general translog production function, which includes also quadratic and interaction terms for each input. Early adopters of the translog specification are, amongst others, Merriman (1990), Pinnoi (1994), and Damalgas (1995).

[8]An alternative model assumes CRTS in both private inputs (represented by $\alpha + \beta = 1$; see Mas et al., 1994), allowing for increasing returns to scale across all inputs (i.e., $\alpha + \beta + \theta > 1$).

sides of (3) and impose CRTS so as to arrive at the logarithm of labor productivity as the dependent variable.

2.4 Some Empirical Results

The output elasticity of public capital can be used to derive the marginal productivity of public capital, that is, $\partial Y_t/\partial G_t = \theta(Y_t/G_t)$, which equals the effective rate of return on public capital.[9] To assess whether investments in public capital are worthwhile, policy makers generally compare the marginal productivity of public capital with the marginal productivity of private capital, which equals the real rate of interest in a competitive market.

Gramlich (1994) argues that the rate of return on public capital derived by Aschauer (1989a) is too large to be credible. Indeed, depending on the year of measurement of $(Y/G)_t$, returns vary between 60 to 80 percent. The marginal output gain of an additional unit of *private* capital estimated from Aschauer's equation amounts to 30 percent, suggesting a difference between public and private capital of a factor two to three. Aschauer (1990) points to the high rate of return found in R&D studies to justify the large output elasticities found in the early literature. Gramlich (1994) claims, however, that a large share of public capital is directed at less productive sectors of the economy, such as waste and pollution abatement, which is likely to contribute little to national output. There is no reason to be pessimistic about the empirical evidence. First, studies published in the 1990s find more realistic values of the output elasticity of public capital. Second, most studies find a positive and statistically significant output elasticity of public capital. Indeed, Ligthart (2002) derives an unweighted average of the output elasticity of public capital of 0.25 for OECD countries if the production function is estimated in logarithmic levels.

The first author studying the output effect of public capital in a regional context is Mera (1973), who analyzes nine Japanese regions, employing a broad definition of public capital. Since then, various authors[10] have found elasticities at the regional level that are much smaller

[9]Here it is assumed that public capital is remunerated based on its marginal productivity. Aaron (1990) argues that in the presence of government pricing inefficiencies and the absence of markets for government services this is not a very realistic assumption.

[10]Munnell (1990b), Eisner (1991), Garcia-Milà and McGuire (1992), Evans and Karras (1994a), and Holtz-Eakin (1994).

than those from analyses using aggregated data for a single country. This can be attributed to spillover effects, that is, some of the beneficial effects of public capital accrue to neighboring regions. In a Nash equilibrium, these spillovers are not internalized, so that both regions may end up with a less than socially optimal public capital stock. Spillovers can be formalized as follows:

$$Y_{i,t} = A_{i,t} K_{i,t}^{\alpha} L_{i,t}^{\beta} G_{i,t}^{\theta} G_{j,t}^{\eta}, \qquad \eta > 0, \tag{5}$$

where G_i is the public capital stock of the home region i, G_j is the public capital of the neighboring region j, and $\eta > 0$ is the spillover effect. On the significance of the spillover effect, however, no consensus has emerged in the literature yet. Studies by Holtz-Eakin and Schwartz (1995a, 1995b) and Boarnet (1998) find little evidence of spillover effects. Indeed, studies at the aggregate level 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.

The composition of the public capital stock matters for its effect on private production. Core infrastructure is more productive than other types of public capital, such as educational and office buildings and hospitals. Accordingly, empirical studies employing a broad definition of public capital (which necessarily includes less productive components), find a lower θ than studies focusing on core infrastructure (cf. Sturm and De Haan, 1995; and Ligthart, 2002).

2.5 Criticisms of the Production Function Approach

Various authors have criticized Aschauer's model for being misspecified due to the omission of relevant variables. 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 may have depressed capital use. Gramlich (1994) criticizes Tatom's approach for mixing production functions and cost functions. Instead of including energy prices, a measure of the quantity of energy use in production should be employed. The study by Vijverberg et al. (1997), for instance, includes imported raw materials in the production function.

Another specification issue concerns the modeling of the effect of the business cycle on factor use. For this purpose, some studies incorporate a capital utilization rate—or, alternatively, the unemployment rate—in the regression equation.[11] Because authors use log-linearized empirical models, capacity utilization enters in an additive fashion. Consequently, it affects all factors across the board, which is a restrictive assumption. Some studies, for example, Ratner (1983), have already adjusted the data and thus do not add a separate regressor. The majority of studies, however, do not correct for the business cycle. If one is interested in estimating long-run elasticities of output with respect to factor inputs, then it makes sense to disregard business cycle effects (e.g., Nourzad, 2000; and Ligthart, 2002). Instead, the aim of most studies is to estimate short-run elasticities of production. Not controlling for the business cycle in this case is likely to bias the estimates downwards.

Some of the early studies have been criticized for not properly accounting for common trends. Generally, time series on GDP and the public capital stock contain a unit root or, in other words, they are non-stationary. If variables are non-stationary, the usual test statistics have nonstandard distributions, implying that the application of standard inference procedures gives rise to misleading results. In particular, one may find spurious relationships between outputs and inputs. Some studies have, therefore, proposed to eliminate time trends in variables by taking first differences.[12]

Two criticisms were raised against first differencing. First, the growth rate of private output in a particular year is not strongly correlated with the growth rate in the public capital stock during that same year as lagged effects are likely to be important. Moreover, equations estimated in first differences often yield implausible coefficients for private inputs (see Sturm and De Haan, 1995). Second, by first differencing information on a possible long-run equilibrium relationship between a set of non-stationary time series (in which case variables are cointegrated) may be thrown away. This shifts the focus of the analysis away from the long-run effects of public capital to the short-run effects. Instead of first differencing, the variables should be tested for cointegration. In the mid-1990s, various authors have either employed Engle and Granger's (1987) cointegration test or Johansen's (1988) variant of this test, giving rise to mixed results.

[11] For example, Aschauer (1989a), Hulten and Schwab (1991a), and Sturm and De Haan (1995) were early adopters of this specification.

[12] See, for example, Aaron (1990), Tatom (1991), and Sturm and De Haan (1995).

Equations (1)–(4) assume G_t to be strictly exogenous, implying that causality runs from public capital to private output. Some authors (e.g., Munnell, 1992; and Gramlich, 1994) have pointed to the lack of attention paid to feedback effects. The direction of causality may run from private output to public capital rather than the other way around. Indeed, a higher rate of output growth may generate favorable budgetary conditions (via higher tax receipts), which facilitate an increase in public investment. During the last decade, various authors[13] have employed VAR models with a view to capturing the dynamic interactions between output, public capital, and private capital.

Some authors solve the endogeneity problem by using a more traditional econometric tool, namely the Instrumental Variables (IV) estimator. The choice of instrumental variables is usually not an easy task, but in a time-series or panel data context lags of the independent variables emerge as natural instruments to be employed. If a positive effect running from output to public capital exists, then Ordinary Least Squares (OLS) estimates of θ in a single equation model like (3) or (4) are known to be exaggerated. Therefore, IV estimates are likely to be smaller than OLS estimates. Baltagi and Pinnoi (1995), for instance, use panel data for the United States to arrive at a pooled OLS estimate of $\theta = 0.16$; the reported IV estimate of θ, however, is only 0.02.

3 Meta-Analysis

This section provides a description of the meta-data set and conducts a simple meta-analysis with a view to assess a meta-output elasticity of public capital.

3.1 The Meta-Data Set

Table A.1 (see Appendix) shows the set of studies reporting estimates of the output elasticity of public capital using (or based on) the production function approach.[14] In total, 76 studies

[13] 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.

[14] Studies dealing with translog production functions were ignored. Converting the estimated parameters to a single output elasticity is not straightforward. Trying to obtain the relevant standard errors is an even more daunting task.

were coded and included in the meta-data set. To obtain a sample of studies representative of the true population, we used a variety of searching methods.[15] We started by checking the references in the overview papers of Sturm et al. (1998) and Romp and De Haan (2007), which together provide a very comprehensive coverage of relevant papers up to 2004.[16] From these sources, we obtained 55 usable references.[17] We then searched for papers citing Aschauer (1989a) in Thomson's *Web of Science*, which allowed us to add eight papers to our meta-data base. We also used the Internet search engine Google *Scholar* and searched for words such as 'public capital' and 'public infrastructure,' each in combination with 'output' or 'productivity,' which yielded another 13 papers (of which six are working papers). Roughly 18 percent (14 out of 76) of the papers are unpublished. Out of 51 published papers, six are published in top-20 journals (based on the ranking of Kodrzycki and Yu (2006)). The data set encompasses single-country studies for 13 different countries and 10 cross-country analyses.

The issue of how many estimates to include in the meta-data set when each study reports more than one is still a controversial issue. Some authors claim that all available estimates (referred to as 'measurements') should be included (e.g., Bijmolt and Pieters, 2001), whereas others are strong believers of selecting only one measurement for each study (e.g., Stanley, 1998; and Van der Sluis et al., 2005). Including all measurements from each study raises two problems. First, it creates dependency among measurements taken from a single study, which we call 'measurement dependency.' Note that dependency across studies may also be present because some studies rely on identical or very similar data sets (which we refer to as 'sample overlap,' see Section 5). 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 706, yielding an average number of 9.3 per study. However, the distribution of available data points across studies is highly skewed. The four papers with the largest number of estimates account for 112 estimates (16 percent of total), the first 10 papers report 243 estimates (34 percent) and the top half of the ranked sample

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

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

[17] The initial data base of studies was much larger. Not all studies could be included owing to missing standard errors, which are a key input into the analysis.

yields 623 estimates (88 percent). Aschauer's (1989a) study reports the largest number of estimates (36 in total), thus receiving a weight six times larger than studies reporting only one estimate (of which only six are included in the sample).

Aside from statistical considerations, there is a more fundamental reason why we include only one measurement per study, that is, we want to measure 'the' true output elasticity of public capital (either measured in a fixed or random effects context). To uncover the value of this parameter as accurately as possible, only those measurements that come closest to the true effect should be used. 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.[18]

To address these issues, we include only one measurement for each study, which 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. 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. When the disaggregation of total public capital in subcategories proves significant, we select the broadest category. 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.[19] 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 5).

Estimates in our sample vary from -0.175 to 0.917, with an arithmetic (or 'simple')

[18]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 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.

[19]Some studies (e.g., Rosenthal, 1991) average across all measurements.

average of 0.193 and a standard deviation of 0.198, showing quite some variation. Indeed, we expect a substantial amount of variation given that studies differ along several dimensions. Eight studies find negative estimates, whereas 68 report positive coefficients. We find that the median of 0.159 is smaller than the sample average; thus the distribution is asymmetric, potentially indicating publication bias. Restricting our analysis to 65 papers applying the *pure* production function approach does not change the results much. Only when we exclude papers reporting possibly spurious estimates[20] do the mean and median (0.178 and 0.140, respectively) slightly decrease, though total and average variation remain large.

3.2 Pooling Estimates

When pooling estimates to arrive at a 'meta'-estimate, the issue of the degree of homogeneity of the estimates needs to be addressed. Two approaches to deriving a meta-estimate of θ can be distinguished. The first approach is the fixed effects model, which assumes that all studies are estimating a common true effect. More formally, denoting the estimate reported in each study in the meta-sample of size N by $\hat{\theta}_i$, and the unknown population (or 'true') effect that is estimated by θ_i, we can write:

$$\hat{\theta}_i = \theta_i + \mu_i, \qquad \text{for } i = 1, ..., N, \tag{6}$$

where μ_i is the sampling error satisfying $E(\mu_i|\theta_i) = E(\mu_i) = 0$ (where E denotes the expectations operator) if each estimate, $\hat{\theta}_i$, is unbiasedly estimating θ_i. The conditional variance of $\hat{\theta}_i$ is defined as $V(\hat{\theta}_i|\theta_i) = V(\mu_i)$ (which represents the within-study variance), whereas its unconditional counterpart is $V(\hat{\theta}_i) = V(\theta_i) + V(\mu_i)$. If all studies are estimating a common true effect (i.e., $\theta_0 = \theta_i$ for all i) then the conditional and unconditional variances of $\hat{\theta}_i$ are equal (i.e., $V(\hat{\theta}_i) = V(\mu_i)$). In other words, all variation is due to sampling error. The random effects model assumes that θ_i is drawn randomly from an iid$(\theta_0, \sigma^2_\theta)$ distribution, where σ^2_θ is the between-study variance (reflecting heterogeneity across studies). The unconditional variance of θ_i then becomes: $V(\hat{\theta}_i) = \sigma^2_\theta + V(\mu_i)$. Intuitively, the total variability of estimates across studies is composed of *pure* heterogeneity and sampling error.

[20] An estimate is considered to be potentially spurious if an equation is estimated in levels without testing for a cointegrating relationship.

For the fixed effects model it suffices to estimate θ, whereas for the random effects model we also need to estimate σ_θ^2. In both models, a simple unbiased estimator of θ is:

$$\bar{\theta} = \frac{\sum_{i=1}^{N} w_i \hat{\theta}_i}{\sum_{i=1}^{N} w_i}, \tag{7}$$

which is a weighted average of the sample estimates, $\hat{\theta}_i'$s, where the w_i's are weights. Although $\bar{\theta}$ is an unbiased and consistent estimator of θ for any choice of weights, there is only one estimator minimizing its variance, that is, $w_i = 1/V(\hat{\theta}_i)$ (see Hedges, 1994). Intuitively, more precise estimates receive more weight when averaged. In the fixed effects model, $V(\hat{\theta}_i) = V(\hat{\theta}_i|\theta_i)$, so that the weights are calculated from the standard errors of $\hat{\theta}_i$. In the random effects model, however, the weights are given by $w_i = 1/[\sigma_\theta^2 + V(\hat{\theta}_i|\theta_i)]$.[21] Obviously, the simple mean is just a specific case of (7), where the weights are chosen to be the same and equal to $1/N$.

Which model is preferable? On statistical grounds the question reduces to testing whether σ_θ^2 is statistically different from zero. If $\sigma_\theta^2 = 0$, then estimates differ from each other only due to sampling error, which suggests that a fixed effects model should be preferred. However, if $\sigma_\theta^2 > 0$, then a random effects model is called for. Because studies differ along several dimensions (i.e., functional specification, econometric technique, definitions of aggregate output and public capital variables, etcetera), sampling error is unlikely to be the sole factor of variation. Consequently, the random effects model becomes the more plausible candidate in our case. A more formal answer is given by the Q test of homogeneity (cf. Shadish and Haddock, 1994):

$$Q = \sum_{i=1}^{N} w_i \hat{\theta}_i^2 - \frac{\left(\sum_{i=1}^{N} w_i \hat{\theta}_i\right)^2}{\sum_{i=1}^{N} w_i}, \tag{8}$$

which under the null hypothesis of homogeneity is χ_{N-1}^2 distributed.

The left panel of Table 1 reports the estimates of θ for both fixed and random effects models and their 95 percent confidence bounds. For the whole meta-sample of 76 reported estimates, the fixed effects estimate of θ is 0.092, less than half of that obtained using a simple average. The true effect is between 0.088 and 0.097 with 95 percent confidence, implying that $\theta = 0$ can be rejected. The Q-test strongly rejects the null hypothesis of homogeneity, suggesting that

[21] Note that $\hat{\sigma}_\theta^2 = [Q - (N-1)]/c$, where $c \equiv \sum_{i=1}^{N} w_i - (\sum_{i=1}^{N} w_i^2 / \sum_{i=1}^{N} w_i)$ and Q is defined in (8). Note that we use $w_i = 1/V(\hat{\theta}_i)$ in the definition of Q.

Table 1: Fixed and Random Effects Estimates of the Output Elasticity of Public Capital

		No Publication Bias Correction					Publication Bias Correction				
			Confidence Interval					Confidence Interval[a]			
	N	$\bar{\theta}$	Lower	Upper	Q^b	$\hat{\sigma}^2_\theta$	$\bar{\theta}^c$	Lower	Upper	Q^b	$\hat{\sigma}^2_\theta$
All Studies											
Fixed	76	0.092	0.088	0.097	1161.3	0.007	0.064	0.029	0.099	819.6	0.005
Random	76	0.152	0.129	0.175			0.081	0.053	0.109		
All Studies Excluding VAR											
Fixed	74	0.090	0.086	0.095	1067.4	0.006	0.064	0.029	0.099	753.5	0.004
Random	74	0.147	0.124	0.169			0.075	0.049	0.102		
Pure Production Function											
Fixed	65	0.090	0.085	0.094	1031.4	0.006	0.064	0.028	0.100	740.9	0.004
Random	65	0.151	0.126	0.175			0.080	0.051	0.109		
Core Infrastructure											
Fixed	13	0.128	0.109	0.146	201.9	0.020	0.041	−0.057	0.140	160.3	0.001
Random	13	0.209	0.120	0.297			0.083	−0.046	0.211		
Regional Data											
Fixed	41	0.076	0.070	0.083	582.8	0.008	0.043	0.023	0.062	399.1	0.002
Random	41	0.110	0.080	0.141			0.066	0.034	0.099		
Non-Spurious[d]											
Fixed	68	0.106	0.101	0.112	1007.5	0.009	0.076	0.020	0.131	788.2	0.004
Random	68	0.144	0.118	0.170			0.080	0.051	0.110		

[a]Confidence intervals use White standard errors; [b]The p-values of all Q tests are smaller than 0.0001; [c]Publication bias corrected estimates of θ which are calculated using estimates of δ derived from equation (11′) of Tables 2-3; and [d]Spurious are those studies using time series in levels while not testing for cointegration.

we should employ the random effects model. In this model, the estimate of θ becomes 0.152, featuring a confidence interval of $[0.129, 0.175]$. Note that the confidence interval is now much wider, because under random effects the variance of $\bar{\theta}$ reflects not only sampling variation but also the term σ_θ^2. The estimate of θ hardly changes for the restricted sample that includes pure production function studies only. If potentially spurious results are also excluded the random effects estimate is 0.144, which is somewhat smaller than the estimate for either the complete sample or the pure production function. This apparently counterintuitive result is explained by the contribution of the heavily weighted (but small) estimate of 0.059 by Otto and Voss (1996), which is coded as a possibly spurious result.

Table A1 reveals three studies with extremely small values for the standard errors, namely, Garcia-Milà and McGuire (1992), Bajo-Rubio and Sosvilla-Rivero (1993), and Otto and Voss (1996). Consequently, these studies carry a large weight in the meta-analysis. If these measurements were excluded, we would find fixed and random effects estimates of 0.121 and 0.160, respectively. Consequently, the value of $\bar{\theta}$ would rise by not more than 5 percent in the random effects model, suggesting that outliers are not a serious problem.

4 Publication Bias

If fixed and random effects estimators are both consistent for the population effect θ, why is it that they produce such different estimates of the effect size? A possible explanation is that they are differently contaminated by publication bias, to which we will turn now.

4.1 The Nature of 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 are 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 and unpublished reports. Some authors (cf. Begg, 1994) suggest to include as many unpublished studies as possible to minimize the perverse effects

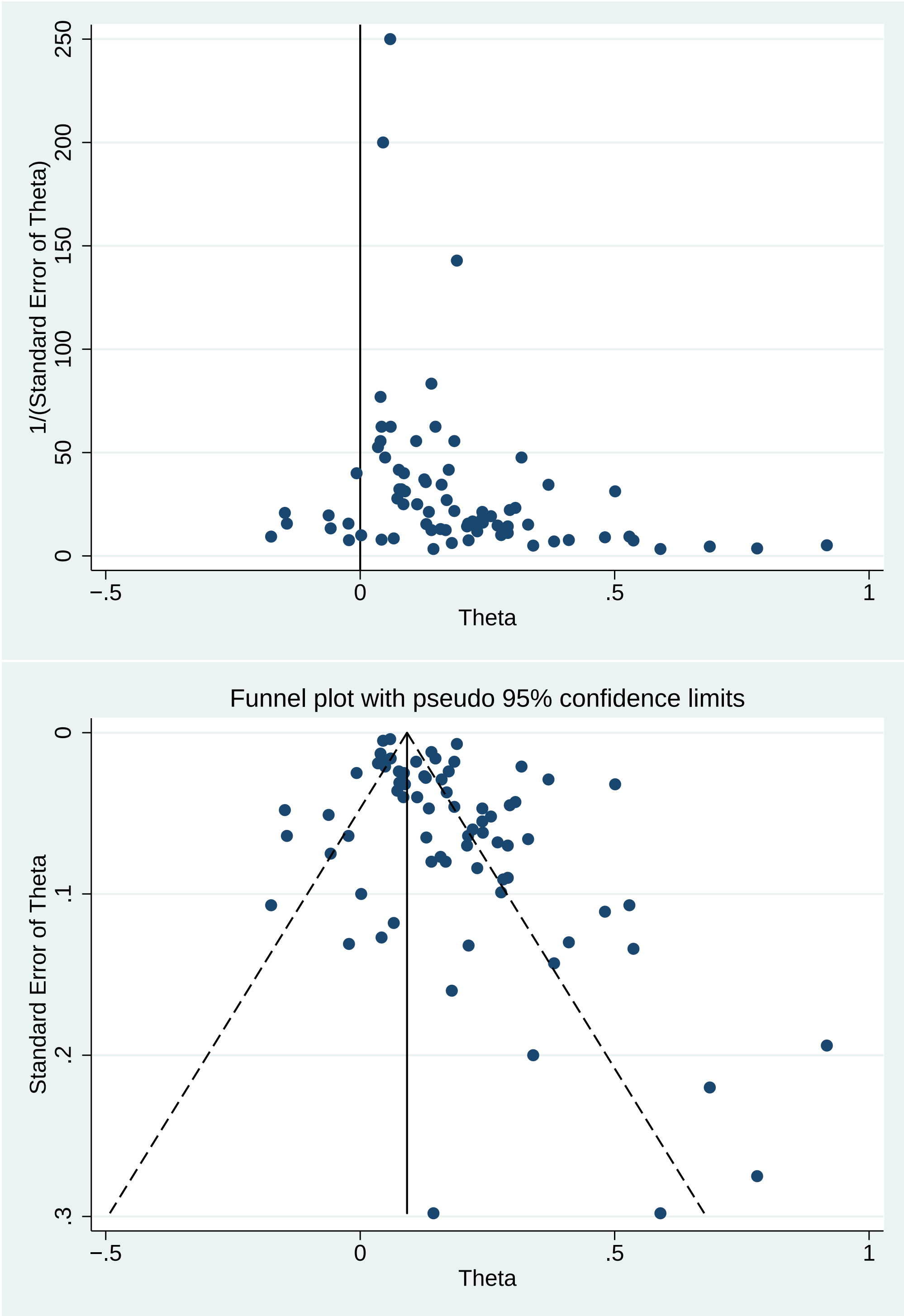

Figure 1: Funnel Plots

of publication bias. But even though it may reduce publication bias somewhat, it cannot be completely eliminated. Indeed, self-censoring by authors may be quite pernicious, which prevents them 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. In the absence of publication bias, 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 are therefore located further away from the true effect. The top panel of Figure 1 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 1 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.

4.2 Publication Bias Tests

We can formally test for the magnitude of publication bias while at the same time providing evidence of a genuine output effect of public capital. The starting point is equation (6), which is modified to include the standard error of each estimate ($\text{se}(\hat{\theta}_i)$) as a regressor with a view to estimating publication bias. We set up our model in a general way so as to be able to capture both fixed and random effects specifications. To differentiate the models, we introduce $\lambda_i \equiv \theta_i - \theta_0$. The fixed effects model (cf. Card and Krueger, 1995) assumes $\lambda_i = 0$ so that $\theta_i = \theta_0$, whereas the random effects model specifies $\lambda_i \sim \text{iid}(0, \sigma_\theta^2)$. We now get:

$$\hat{\theta}_i = \theta_0 + \lambda_i + \delta \text{se}(\hat{\theta}_i) + \mu_i, \tag{9}$$

where θ_0 is the true output effect and μ_i is the error term. As discussed above, if publication selection is plaguing the meta-sample, then we should observe some relationship between the estimates and their standard errors, that is, $\delta \neq 0$. In the absence of publication bias, estimates will lie symmetrically around the true value θ_0, which implies $\delta = 0$.

The error term μ_i in (9) is heteroscedastic given its obvious dependence on $\text{se}(\hat{\theta}_i)$. In this context, OLS estimation yields inefficient (but consistent) estimates of θ and δ. In the fixed effects model, $\text{se}(\hat{\theta}_i)$ provides a natural estimate of μ_i. Hence, we can divide both sides of (9) by $\text{se}(\hat{\theta}_i)$ to arrive at a study's standardized effect (i.e., the t-value) on the left-hand side of the equation. In other words, we multiply equation (9) by $w_i \equiv 1/\text{se}(\hat{\theta}_i)$.[22] In the random effects model, we apply $w_i \equiv 1/\sqrt{V(e_i) + \sigma_\theta^2}$ to both sides of (9). Estimating the weighted version of (9) by OLS produces WLS estimates of θ_0 and δ, which are consistent and efficient.

Stanley (2005) assumes symmetric publication bias and therefore takes absolute values of the left-hand side of the weighted fixed effects version of (9) to accommodate bidirectional publication bias, that is, $\hat{\theta}_i$ is expected to be positively (negatively) correlated with $\text{se}(\hat{\theta})_i$ when $\hat{\theta}_i$ is positive (negative). The more general version of Stanley's specification captures both fixed and random effects:

$$w_i|\hat{\theta}_i| = w_i \left[\theta_0 + \lambda_i + \delta \text{se}(\hat{\theta}_i) + \mu_i\right]. \tag{10}$$

Equation (10) forces the bidirectional publication bias to be symmetric, in which case δ does not depend on the sign of $\hat{\theta}_i$. It would be useful to test this restriction. Let us specify a more flexible model, in which asymmetric bidirectional publication bias is permitted:

$$w_i\hat{\theta}_i = w_i \left[\theta_0 + \lambda_i + \delta_p D_{pi} \text{se}(\hat{\theta}_i) + \delta_n D_{ni} \text{se}(\hat{\theta}_i) + \mu_i\right], \tag{11}$$

where D_{pi} (D_{ni}) is a dummy variable that equals one if $\hat{\theta}_i > 0$ ($\hat{\theta}_i < 0$) and zero otherwise. The uniqueness of the true effect can also be tested by interacting θ_0 with the D_{pi} (D_{ni}) dummies:

$$w_i\hat{\theta}_i = w_i \left[D_{pi}(\theta_p + \lambda_{pi} + \delta_p \text{se}(\hat{\theta}_i)) + D_{ni}(\theta_n + \lambda_{ni} + \delta_n \text{se}(\hat{\theta}_i)) + \mu_i\right], \tag{12}$$

where true effect uniqueness requires imposing the condition $\theta_p = \theta_n = \theta_0$.

[22] Note that in Section 3.2 the inverse variance approach was used in weighting the measurements instead of using $w_i \equiv 1/\text{se}(\hat{\theta}_i)$. In terms of estimators, both weighting schemes are identical. Suppose we premultiply (9) by a $N \times N$ weighting matrix T, which features the inverse of a study's standard errors on the diagonal. Applying OLS to this equation gives the vector of estimates (including θ_0): $\delta = [\text{se}(\hat{\theta})'\Omega^{-1}\text{se}(\hat{\theta})]^{-1}\text{se}(\hat{\theta})\Omega^{-1}\hat{\theta}$, where $\Omega^{-1} \equiv T'T$ and Ω is the variance-covariance matrix. The matrix Ω^{-1} has the inverse of the variances on the diagonal.

Table 2: Fixed Effects: True Effect Tests and Linear Publication Bias Correction

Equation	(9)	(10)	(12)	(11)	(11′)
θ_0	0.068	0.066	–	0.064	0.064
	(0.020)***	(0.020)***		(0.019)***	(0.019)***
	[0.018]**	[0.018]***		[0.019]***	[0.016]***
θ_p	–	–	0.064	–	–
			(0.020)***		
			[0.019]***		
θ_n	–	–	0.009	–	–
			(0.031)		
			[0.010]		
δ	1.915	2.246	–	–	2.536
	(0.522)***	(0.482)***			(0.371)***
	[0.519]***	[0.487]***			[0.407]***
δ_p	–	–	2.565	2.576	–
			(0.507)***	(0.499)***	
			[0.529]***	[0.524]***	
δ_n	–	–	−1.381	−2.353	–
			(0.634)***	(0.527)**	
			[0.261]***	[0.588]***	
R^2	0.356	0.371	0.707	0.706	0.706
N	76	76	76	76	76
Q-statistic	–	864.71	817.20	819.33	819.64
$\hat{\sigma}_\theta^2$	–	0.005	0.005	0.005	0.005
F-tests:					
$\delta_p = -\delta_n$ White	–	–	–	0.06	–
				(0.80)	
$\delta_p = -\delta_n$ Cluster	–	–	–	0.06	–
				(0.81)	
$\theta_p = \theta_n$ White	–	–	2.24	–	–
			(0.14)		
$\theta_p = \theta_n$ Cluster	–	–	5.31	–	–
			(0.03)**		
DoF-tests:					
γ_1	0.106	0.020	−0.037	0.011	0.007
	(0.106)	(0.104)	(0.113)	(0.113)	(0.110)
	[0.115]	[0.130]	[0.121]	[0.122]	[0.114]
R^2	0.0126	0.0004	0.0014	0.0001	0.0000

Notes: 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. The values in brackets are clustered standard errors. ***, **, * denote significance at the 1, 5, and 10 percent level, respectively. The dependent variable in the degrees of freedom (DoF) test is the logarithm of the t-statistic corrected for publication bias. To save on space, the constant (γ_0) of the DoF test is not reported.

Table 3: Random Effects: True Effect Tests and Linear Publication Bias Correction

Equation	(9)	(10)	(12)	(11)	(11′)
θ_0	0.085	0.089	–	0.087	0.081
	(0.022)***	(0.020)***		(0.020)***	(0.014)***
	[0.024]***	[0.025]***		[0.025]***	[0.014]***
θ_p	–	–	0.092	–	–
			(0.021)***		
			[0.025]***		
θ_n	–	–	−0.035	–	–
			(0.053)		
			[0.030]		
δ	1.443	1.666	–	–	2.066
	(0.391)***	(0.349)***			(0.233)***
	[0.379]***	[0.325]***			[0.214]***
δ_p	–	–	1.872	1.943	–
			(0.356)***	(0.347)***	
			[0.352]***	[0.348]***	
δ_n	–	–	−0.680	−2.399	–
			(0.793)	(0.534)***	
			[0.394]*	[0.618]***	
R^2	0.589	0.715	0.748	0.742	0.741
N	76	76	76	76	76
F-tests:					
$\delta_p = -\delta_n$ White	–	–	–	0.37	–
				(0.54)	
$\delta_p = -\delta_n$ Cluster	–	–	–	0.28	–
				(0.60)	
$\theta_p = \theta_n$ White	–	–	5.01	–	–
			(0.03)**		
$\theta_p = \theta_n$ Cluster	–	–	6.68	–	–
			(0.02)**		
DoF-tests:					
γ_1	0.066	0.057	−0.022	0.088	0.103
	(0.082)	(0.103)	(0.112)	(0.110)	(0.119)
	[0.087]	[0.100]	[0.116]	[0.117]	[0.124]
R^2	0.0082	0.0036	0.0005	0.0082	0.0093

Notes: 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. The values in brackets are clustered standard errors. The Q-statistic and $\hat{\sigma}^2$ can be found in Table 2. ***, **, * denote significance at the 1, 5, and 10 percent level, respectively. The dependent variable in the degrees of freedom (DoF) test is the logarithm of the t-statistic corrected for publication bias. To save on space, the constant (γ_0) of the DoF test is not reported.

An alternative way to test whether a genuine empirical effect exists is to relate a study's t-value to its degrees of freedom (df):

$$\ln \left| \frac{\hat{\theta}_i^*}{\mathrm{se}(\hat{\theta}_i)} \right| = \gamma_0 + \gamma_1 \ln \mathrm{df}_i + \mu_i, \tag{13}$$

where $\hat{\theta}_i^*$ is $\hat{\theta}_i$ corrected for publication bias and γ_0 and γ_1 are parameters to be estimated. If we cannot reject the hypothesis that $\gamma_1 = 0$, then either the true effect is zero or publication bias is too severe. A genuine empirical effect implies $\gamma_1 = 1/2$ (cf. Card and Krueger, 1995).

4.3 Estimation Results

Tables 2–3 present the estimates of the true effect and the size of publication bias of fixed effects and random effects models, respectively. Besides White standard errors, which correct for identified heteroscedasticity, we present clustered standard errors. For this purpose, we have defined 27 clusters (see Appendix A.1 for a description). The results show, however, that clustering does not affect our results much.[23] Column (1) estimates equation (9), yielding $\theta = 0.068$ in the fixed effects model and $\theta = 0.085$ in the random effects model. Both are highly significant. As indicated by the significant value of δ—and suspected from Figure 1—publication bias proves to be very significant in both models. Intuitively, even studies with very small sample sizes—and thus high standard errors of the estimates—manage to find a high enough θ such that a significant result is obtained. The fixed effects specification yields a larger parameter for publication bias than the random effects model. Not surprisingly, the estimate of δ with symmetric publication bias imposed (see column (2)) turns out to be significant in both models. Imposing symmetry raises the publication bias coefficient slightly.

Column (3) shows that the estimated true effect for positive estimates, θ_p, is positive, highly significant, and very much in line with previous estimates of θ. Perhaps more surprisingly, the estimate of θ_n is also positive in the fixed effects model, though not statistically different from zero, suggesting that negative estimates in the meta-sample are very likely to be just the result of a mixture of pure chance and publication selection. In the random effects model, θ_n is negative, but again insignificant. Column (4) imposes a symmetric true effect

[23]Clustering of measurements makes more of a difference in the meta-regression analysis of Section 5.

and shows estimates of positive and negative publication bias parameters, δ_p and δ_n, which appears quite symmetric; the F-test of the hypothesis $\delta_p = -\delta_n$ is not rejected. The hypothesis that publication bias is unidirectional (i.e., $\delta_p = \delta_n$) is naturally rejected (the F-test statistic is not reported). Imposing symmetric publication bias and true effect uniqueness (which we label (11′), see column (5)) does not change the point estimate of θ much, but increases the magnitude of publication bias somewhat.

Estimating (13) without publication bias correction yields an insignificant value of the γ_1 coefficient in both models (results are not reported), for which three potential explanations exist. First, a genuine empirical effect may be absent. The second reason is the presence of strong heterogeneity. Finally, γ_1 can be insignificant because of substantial publication bias, evidence of which has been presented above. To account for publication bias, we re-estimate equation (13) with the left-hand side variable corrected for publication bias (see the bottom panel of Tables 2–3). All γ_1's remain insignificant, however. Section 5 demonstrates that this is caused by omitted variables.

We can now use the estimate of δ from the last column of Tables 2–3 to correct the meta-θ for linear publication bias (see the right panel of Table 1). As compared to the case without publication bias correction, the difference between fixed and random effects estimates narrows, where the random effects estimates show a larger decline than the fixed effects estimates. As before, the Q-test strongly favors the random effects estimator. For the entire sample, we find a random effects meta-θ of 0.081, lying within the 95 percent confidence interval. Focusing on studies using the pure production function only, does not affect the estimate of θ much, whereas exclusion of VAR-based studies decreases it to 0.075.

5 Meta-Regression Analysis

The previous section showed that the true output elasticity of public capital is heterogeneous. However, heterogeneity has not been explicitly modeled yet. This section employs a meta-regression approach to find the determinants of the excess variation of estimates of the output elasticity of public capital across studies.

5.1 Methodological Framework

The starting point of the meta-regression model is the assumption that the true effect being estimated depends on K study-specific factors. Table 4 provides the complete list of moderators that are taken into consideration. In the context of symmetric bidirectional publication bias, we employ an extended version of (12) capturing both fixed and random effects:

$$w_i\hat{\theta}_i = w_i\left(\theta_0 + \lambda_i + \sum_{j=1}^{M}\phi_j D_{ji} + \delta \mathrm{se}(\hat{\theta}_i)(D_{pi} - D_{ni}) + \mu_i\right), \tag{14}$$

where ϕ_j's are estimated coefficients and D_{ji} (for $j = 1, 2, ..., K$) is a dummy variable that equals one if the i-th estimate is obtained from a study described by characteristic j and zero otherwise. We 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.[24]

We estimate equation (14) by OLS, using the weights defined earlier, which gives rise to WLS estimates of the parameters. Because the latter do not take into account the covariances across error terms they represent a partial GLS estimator. 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 small number of degrees of freedom (in our case 42) 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. Subsequently, we test for: (i) the joint insignificance of the excluded variables; and (ii) publication bias symmetry.

Because of the relatively large number of studies on the United States (41 percent of total), a few studies use identical data sets. For example, Munnell (1990b), Eisner (1991), and Baltagi and Pinnoi (1995) all use data for the 1970–1986 period. Other studies on the United States use slightly different sample periods, thereby yielding very similar data sets. Overlapping data sets generate sample dependency. For some smaller European countries, notably Italy and Spain, we also observe some overlap in data sets, again creating dependency

[24]We include country dummies D_{ji} (for $j = K + 1, ..., M$) if there at least two measurements for a single country. This yields $M - K - 1 = 9$ country dummies.

Table 4: List of Moderators Used in the Meta-Regression Model

Empirical Dimension	Dummy Variable	Definition (1 if ..., 0 otherwise)
Definition of output	Private	Dependent variable is private sector output at country or state level
Country fixed effect	Country i	Data are for country i[a]
	Developing	Data are for developing countries
Type of public capital	Core	Core capital is used[b]
	Transport	Transportation capital is considered
	Proxy	Capital is proxied by the public investment-to-GDP ratio
	Reg-loc	Only regional/local capital is considered
Data aggregation	Reg-da	Regional data is used
Empirical model	Pfrontier	A production frontier approach is employed
	PF-based	A transformation of a production function is used
	Gregr	A growth regression is considered
Econometric specification	Trend	A time trend is included
	Cap-util	Capacity utilization is controlled for
	CRTS	Constant returns to scale is imposed
Estimation method	Spurious	Equation is estimated in levels without cointegration test
	Coint-all	Cointegration relationship is found using one of the available cointegration methods
	Coint-VAR	Cointegration relationship is studied using Johansen's VAR analysis
	FE-levels	Unit-specific fixed effects with variables in levels are employed
	TE-levels	Time effects with variables in levels are employed
	FE-dif	Unit-specific fixed effects with variables in differences are employed
	TE-dif	Time effects with variables in differences are employed
	IV-GMM	IV or GMM estimators are used
	AR	An autoregressive residual correction is applied

[a]Country-specific fixed effects are included for those countries for which there is more than one estimate: Australia, Canada, France, Germany, Italy, Japan, Mexico, Spain, and United States; and [b]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 5: Meta-Regression Results

Equation	Partial GLS				Full GLS
	Fixed Effects		Random Effects		RE
	White	Cluster	White	Cluster	White
θ_0	0.120***	0.120***	0.125***	0.127***	0.086***
	(0.008)	[0.009]	(0.017)	[0.022]	(0.036)
Developing	-0.068***	-0.068***	-0.070***	-0.118***	-0.117***
	(0.023)	[0.023]	(0.025)	[0.024]	(0.043)
Core	0.073**	0.073	0.085*	0.064	0.078*
	(0.036)	[0.049]	(0.050)	[0.069]	(0.047)
Reg-loc	0.057**	0.057	0.066**	0.079**	0.075*
	(0.025)	[0.023]	(0.030)	[0.031]	(0.044)
Transport	–	–	–	-0.051*	–
				[0.028]	
Prox	–	–	–	0.082**	0.103*
				[0.031]	(0.055)
Private	–	–	–	–	-0.046
					(0.027)
Reg-da	-0.085***	-0.085***	-0.081***	–	-0.119***
	(0.018)	[0.014]	(0.027)		(0.040)
Pfrontier	0.099***	0.099***	0.088**	0.075***	0.132***
	(0.029)	[0.033]	(0.037)	[0.024]	(0.049)
Gregr	-0.085***	-0.085***	-0.092***	-0.171***	-0.148**
	(0.015)	[0.010]	(0.023)	[0.032]	(0.057)
PF-based	–	–	–	-0.059**	–
				[0.024]	
Trend	-0.039*	-0.039	-0.049*	-0.045**	-0.050*
	(0.020)	[0.026]	(0.026)	[0.020]	(0.030)
Spurious	0.051*	0.051***	0.060*	0.090***	0.118**
	(0.028)	[0.016]	(0.033)	[0.016]	(0.045)
Coint-all	0.124***	0.124***	0.105**	0.158***	0.096***
	(0.034)	[0.043]	(0.047)	[0.036]	(0.024)
Coint-VAR	0.066*	0.066	0.072	–	0.084**
	(0.039)	[0.047]	(0.056)		(0.038)
FE-levels	0.040*	0.040	0.039	–	0.059*
	(0.021)	[0.034]	(0.027)		(0.031)
IV-GMM	–	–	–	0.028**	–
				[0.012]	
CRTS	–	–	–	–	0.058**
					(0.025)
δ	1.779***	1.779***	1.818***	1.823***	1.514***
	(0.356)	[0.390]	(0.345)	[0.375]	(0.555)
R^2	0.933	0.933	0.872	0.874	0.776
N	76	76	76	76	76
Q	186.455	186.455	–	–	–
$\hat{\sigma}^2_\theta$	0.001	0.001	–	–	–
F-tests:					
$\delta_p = -\delta_n$	1.910	1.110	1.280	1.470	0.000
	(0.173)	(0.302)	(0.263)	(0.237)	(0.999)
$\phi_1 = ... = \phi_X = 0$	1.340	1.350	1.560	1.590	0.420
	(0.235)	(0.251)	(0.141)	(0.161)	(0.929)
DoF tests:					
γ_1	0.350***	0.350***	0.286***	0.286***	0.301***
	(0.102)	[0.076]	(0.075)	[0.050]	(0.103)

Notes: 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 random effects (RE) models (both partial and full GLS) use the $\hat{\sigma}^2_\theta$ obtained from the fixed effects partial GLS model. The second F-test in the table tests whether or not X excluded variables are jointly insignificant. The dependent variable in the degrees of freedom (DoF) test is the logarithm of the t-statistic corrected for publication bias. See equation (13). To save on space, the constant (γ_0) of the DoF test is not reported.

across studies. To address the sample overlap problem, we make use of full GLS estimation (Appendix A.2), which is applied to the random effects model. The GLS estimator weights the original measurements (our dependent variable) and the explanatory variables by a fully specified variance-covariance matrix. The degree of sample overlap is used to recover the off-diagonal elements of the variance-covariance matrix, which were assumed to be zero in the analysis of Sections 4.2–4.3. Because the information that is used to construct the clusters is basically the same as that needed to estimate the off-diagonal elements of the variance-covariance matrix, we do not use clusters in applying the full GLS estimator.

5.2 Regression Results

The last column of Table 5 presents the regression results for the random effects model estimated by full GLS, which we consider the benchmark model. The estimate of the true output elasticity of public capital is 0.086, which is slightly larger than the value of 0.081 found in the true effect tests of Table 3. Note that the parameter θ_0 is now measuring the true effect conditional on a given set of study characteristics, whereas in (9) it measures the unconditional true effect. Publication bias is significant and has the expected sign. It is, however, much smaller than the value found in the true effect tests (compare column (5) of Tables 3 and 5). The symmetry of publication bias could not be rejected. In general, the publication bias parameter becomes more precisely estimated and rises slightly in size when we impose the symmetry restriction.

We find 13 moderator variables that are significant (at least at the 10 percent level). Six of these variables are related to the estimation method and econometric specification. Studies allowing for a time trend find smaller output elasticities of public capital. A larger estimate of θ is found for studies: (i) estimating a model in levels that is likely to be spurious; (ii) including fixed effects in a levels specification; (iii) finding cointegration based on various methods, such as Engle and Granger's (1987) or Johansen's (1988) method; and (iv) imposing a CRTS restriction across all inputs. The coefficient of the *Coint-all* variable (measuring a cointegrating relationship among variables using various methods) is very large; it even exceeds the value of the conditional true effect. In addition, the coefficient of potentially spurious regressions (i.e., *Spurious*) is also large, partially explaining why the early literature found unreasonably large estimates. Many of the latter papers may also contain unidentified cointegrating relationships among the variables.

In line with the empirical partial results of Section 4.3, we find a significantly positive coefficient for studies employing core public capital, implying that railways and airports are more productive than public office buildings. In contrast, we find a significantly negative coefficient for studies using regional data, supporting the presence of spillover effects at the regional level that cannot be internalized at the national level. Public capital that is provided by local/regional governments seems to be more productive than that at the national level. Local governments apparently can better target public spending than national governments. The coefficient of the *Proxy* variable is significantly positive at the 10 percent level. Studies proxing public capital by the public investment-to-GDP ratio find larger output elasticities of public capital.

The type of empirical model affects the results too. Studies employing growth regressions yield smaller output elasticities than those employing a pure production function approach. Intuitively, the former is quite flexible in terms of the set of explanatory variables, which may include those variables that are strongly correlated with public capital.[25] Consequently, some of the variance of our dependent variable may be picked up by variables other than public capital. Studies based on the production frontier approach yield larger coefficients, however.

The specification incorporates country dummies to control for country-specific effects. Five of the nine country dummies (which are not reported) are significantly different from zero. Surprisingly, the dummy for the United States—found to be significant in Button's (1998) study—is only significant at the 10 percent level. Countries that are larger in size (in terms of surface area) seem to have smaller coefficients of the country dummies.[26] The dummy variable for developing countries is significantly negative, suggesting that the output elasticity of public capital for this country group is lower than for industrialized countries. In view of the typically low public capital-output ratios of developing countries, it is very likely

[25] Durlauf et al. (2005) argue that there is an abundance of potential growth determinants. Roughly 145 different proxies for growth determinants have been used in cross-country growth regressions.

[26] Regressing the estimated coefficients of the country dummies on the natural logarithm of country size yields an estimated slope coefficient of -0.168 and a R^2 of 0.44.

that their public capital is contributing more to productivity than in industrialized countries.

The R^2 of 0.78 shows that the model fits the data fairly well. Compared with the univariate true effect tests of Section 4, the inclusion of moderator variables explains some additional variation. Previously, some of this variation was picked up by publication bias, which plays a larger role in the univariate true effect tests. The F-test of joint insignificance of the excluded moderators cannot be rejected. Note that the degrees of freedom test now shows a significant γ_1 coefficient; it is not statistically different from 0.5 at the 5 percent level, providing evidence of a true effect.

5.3 Robustness Analysis

Columns (1)–(4) of Table 5 show the results obtained by applying the partial GLS estimator to the fixed effects and random effects models. Both regressions yield a larger true effect and a larger publication bias coefficient than in the full GLS specification. In addition, R^2 is larger than in the full GLS specification. Apparently, more variation is attributed to publication bias. Like before, we cannot reject the symmetry of publication bias. Dropping the country-specific fixed effects from the equation with White standard errors does not affect the size of the true effect. In the case of clustered errors, the true effect rises by 0.02 (fixed effects) and 0.01 (random effects) if the country-specific fixed effect is left out.

As compared with the full GLS specification, the set of significant moderator variables changes somewhat in the random effects specification with clustered errors. Five moderator variables are no longer significant: core public capital, regional data, Johansen's cointegration method, fixed effects estimation in levels, and the CRTS restriction. Three new significant variables enter (i.e., transportation capital, production function based studies, and IV estimation), potentially reflecting the effect of collinearity among the moderator variables.[27] In the fixed effects model with clustered standard errors, which is not our preferred specification, even more variables drop out. Quite some overlap exists with the dropped variables in the random effects model.

[27] Correlations above 0.75 were recorded between *Private* and *CRTS*, *IV-GMM* and *Private*, *Private* and *Spurious*, *Reg-da* and the US dummy, *Transport* and the US dummy, and *Spurious* and the Australia dummy.

6 Conclusions

This paper has assessed the output-elasticity of public capital by means of several meta-analytical techniques, including meta-analysis and meta-regression analysis. A sample was gathered of 76 studies focusing on the production function approach in a broad sense. The estimates of the output elasticity of public capital were corrected for publication bias. In the meta-regression analysis, both fixed and random effects models were estimated using Weighted Least Squares (or partial Generalized Least Squares). To address sample overlap across studies, a full Generalized Least Squares estimator was employed in the random effects model.

Publication bias is shown to be substantial and significant in both the fixed and random effects model of the simple meta-analysis. Moreover, publication bias appears to be bidirectional and symmetric. The true output elasticity of public capital is positive and significant no matter whether publication bias is corrected for. After correcting for publication bias, the output elasticity of public capital in the (preferred) random effects model turns out to be 0.081 compared with 0.064 in the fixed effects model.

Heterogeneity across estimates of the output elasticity of public capital is predominantly caused by differences in research design, such as the econometric specification, estimation technique, type of empirical model, type of public capital, and aggregation level of public capital. On the chosen estimation technique and econometric specification, the analysis indicates that larger meta-output elasticities are obtained in studies featuring cointegration, spurious relationships among variables, levels specifications estimated by fixed effects, and constant returns to scale restrictions. Smaller meta-output elasticities are found in studies employing time trends. The type of empirical model matters too. Production frontier studies yield larger output elasticities, whereas growth regressions give rise to smaller estimates of the output elasticity. On the kind of public capital and public capital data, the analysis indicates that core public capital features a larger output elasticity than specifications drawing on a more broadly defined concept of public capital. Studies employing regional data yield smaller estimates of the output elasticity of public capital, whereas studies focusing on locally installed public capital find a higher output elasticity than those dealing with centrally owned public capital.

Unlike Aschauer's estimate of 0.39, the meta-regression analysis yields a reasonable meta-output elasticity of public capital of 0.086 (which is slightly larger than the value found in the univariate true effect test). This implies a marginal productivity of public capital of roughly 17 percent in 2001.[28] In that same year, the real long-term rate of interest—which is assumed to reflect the marginal productivity of private capital in a perfect market—amounted to 2.6 percent, suggesting that investment in public capital should be encouraged from a macroeconomic point of view. In view of the above meta-regression results, the large estimates found in the early studies seem to be caused by either unidentified (but present) cointegrating relationships or spurious relationships among national time-series data.

Our analysis provides clear evidence in favor of public capital investment, but it is not without limitations. The Cobb-Douglas production function, on which the majority of studies is based, is quite restrictive. Extending the analysis by allowing for the much more flexible translog function is certainly an improvement. A key issue here is that output elasticities of public capital have to be calculated from the translog parameters. Although many studies carry out such computations for the elasticities themselves, few do so for the standard errors. Another fruitful research avenue is to combine the estimates from studies using the behavioral approach, that is, cost and/or profit functions, with those of the production function. To the best of our knowledge, such work has not been undertaken yet. Finally, in future work we intend to correct our meta-estimate of the elasticity of public capital for nonlinear publication bias.

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

Appendix

A.1 Constructing Clusters

In constructing the clusters, we took note of the following guiding principles. First, the number of clusters must be as large as possible given the size of the data set. Some authors, for example, Cameron et al. (2007), argue that at least 50 clusters are needed for accurate inference. In that case, the cluster robust standard error converges to the true standard error. Second, cluster sizes should be rather balanced. It would be natural to construct the clusters on the basis of countries. Given that we have only 13 single country studies, differing substantially in terms of the number of estimates, a small number of unbalanced clusters would result. For instance, the United States alone would yield a cluster with almost half of the sample, whereas many other countries would have one estimate only. In this context, correcting the standard errors by cluster would probably do more harm than good.

To arrive at a sufficient number of clusters, we define various clusters within the same country, especially for the United States. In defining the clusters, we use the degree of overlap of the samples underlying the studies. We allocate studies to a single cluster if the degree of sample overlap exceeds the pre-specified threshold of 80 percent. In a few cases (e.g., Ratner, 1983), we prefer to accommodate studies below the threshold in existing clusters rather than to isolate them in a one-study cluster. We thus employ 27 clusters, ranging from a minimum of one to a maximum of 14 estimates per cluster. On average, a cluster consists of 2.8 observations.

A.2 The Full GLS Estimator

To arrive at the full GLS estimator, we start with the following random effects model:

$$\hat{\theta}_i = \theta_0 + \lambda_i + \sum_{j=1}^{M} \phi_j D_{ji} + \delta \mathrm{se}(\hat{\theta}_i)(D_{pi} - D_{ni}) + \mu_i, \tag{A.1}$$

which can be written in compact form as:

$$\hat{\theta} = \mathbf{X}\psi + \mathbf{v}, \tag{A.2}$$

where $\mathbf{X}$ is defined as:

$$\mathbf{X} \equiv \begin{bmatrix} 1 & D_{11} & ... & D_{M1} & \text{se}(\hat{\theta}_1)(D_{p1} - D_{n1}) \\ 1 & D_{12} & & D_{M2} & \text{se}(\hat{\theta}_2)(D_{p2} - D_{n2}) \\ \vdots & \vdots & \ddots & \vdots & \vdots \\ 1 & D_{1N} & ... & D_{MN} & \text{se}(\hat{\theta}_N)(D_{pN} - D_{nN}) \end{bmatrix}, \tag{A.3}$$

and $\hat{\theta}$, ψ, and $\mathbf{v}$ are

$$\hat{\theta} = \begin{bmatrix} \hat{\theta}_1 \\ \hat{\theta}_2 \\ \vdots \\ \hat{\theta}_N \end{bmatrix}, \quad \psi = \begin{bmatrix} \theta_0 \\ \phi_1 \\ \vdots \\ \phi_M \\ \delta \end{bmatrix}, \quad \mathbf{v} \equiv \lambda + \mathbf{u} = \begin{bmatrix} \lambda_1 + u_1 \\ \lambda_2 + u_2 \\ \vdots \\ \lambda_N + u_N \end{bmatrix}. \tag{A.4}$$

Note that $E(\mathbf{v}) = \mathbf{0}$ and $V(\mathbf{v})$ (denoted by $\boldsymbol{\Omega}$) are defined as:

$$\boldsymbol{\Omega} \equiv \begin{bmatrix} \sigma_\theta^2 + \sigma_{u_1}^2 & \sigma_{u_2u_1} & \cdots & \sigma_{u_Nu_1} \\ \sigma_{u_1u_2} & \sigma_\theta^2 + \sigma_{u_2}^2 & \cdots & \sigma_{u_Nu_2} \\ \vdots & \vdots & \ddots & \vdots \\ \sigma_{u_1u_N} & \sigma_{u_2u_N} & \cdots & \sigma_\theta^2 + \sigma_{u_N}^2 \end{bmatrix}, \tag{A.5}$$

where $\sigma_{u_iu_j} \equiv C(u_i, u_j)$ (where C stands for covariance) if $i \neq j$ and $\sigma_{u_i}^2 = V(u_i)$ for $i = j$ because $C(\lambda_i + u_i, \lambda_j + u_j) = C(u_i, u_i) = V(u_i)$. The next step is to multiply both sides of equation (A.2) by the weighting factor $w \equiv 1/\sqrt{\boldsymbol{\Omega}}$:

$$\boldsymbol{\Omega}^{-1/2}\hat{\theta} = \boldsymbol{\Omega}^{-1/2}\mathbf{X}\psi + \boldsymbol{\Omega}^{\mathbf{-1/2}}\mathbf{v}, \tag{A.6}$$

where $E(\boldsymbol{\Omega}^{\mathbf{-1/2}}\mathbf{v}) = \mathbf{0}$ and $V(\boldsymbol{\Omega}^{\mathbf{-1/2}}\mathbf{v}) = \boldsymbol{\Omega}^{\mathbf{-1}}V(\mathbf{v}) = \boldsymbol{\Omega}^{\mathbf{-1}}\boldsymbol{\Omega} = \mathbf{I}_N$. Applying OLS to (A.6) provides consistent and efficient estimates of ψ. Of course, the above procedure is only operational if all elements of $\boldsymbol{\Omega}$ are known or can be estimated.

The third step consists of estimating the elements of $\boldsymbol{\Omega}$. In doing so, we assume that all sources of dependency across observations—except for sample dependency—have been controlled for in the meta-regression. The elements of $\boldsymbol{\Omega}$ on the main diagonal of (A.5) can be obtained in the same way as in Section 3.2. The formula differs slightly from that in footnote 21:

$$\hat{\sigma}_\theta^2 = \frac{Q-(N-M-2)}{c}, \qquad c \equiv \sum_{i=1}^{N} w_i - \left[\sum_{i=1}^{N} w_i^2 / \sum_{i=1}^{N} w_i\right], \tag{A.7}$$

where $\hat{\sigma}_{u_i}^2 \equiv (\mathrm{se}(\hat{\theta}_i))^2$ is defined as before and w_i are the weights derived from the fixed effects analysis. The off-diagonal elements are in some cases non-zero because there is sample overlap across studies. In formal terms, $\sigma_{u_i u_j} = \rho_{u_i u_j}\sqrt{\sigma_{u_i}^2}\sqrt{\sigma_{u_j}^2}$, where $\rho_{u_i u_j}$ is the correlation coefficient between u_i and u_j. We thus need an estimate of $\rho_{u_i u_j}$. To this end, we define $\hat{\rho}_{u_i u_j} \equiv n_{ij}/(n_i + n_j + n_{ij})$, where n_{ij} is the number of observations in overlapping samples i and j (i.e., the samples that give rise to $\hat{\theta}_i$ and $\hat{\theta}_j$) and n_i (n_j) is the number of observations used to estimate $\hat{\theta}_i$ ($\hat{\theta}_j$). If two samples do not overlap then $\hat{\rho}_{u_i u_j} = 0$, whereas if they completely overlap then $\hat{\rho}_{u_i u_j} = 1$.

We do not make an attempt to recover estimates for the fixed effects model because: (i) the random effects estimator is statistically preferred; and (ii) the fixed effects would require imposing further restrictions on $\mathbf{\Omega}$. The latter argument can be illustrated as follows. If we were to assume a fixed effects model, then σ_θ^2 would drop out from the matrix $\mathbf{\Omega}$. In that case, any two columns with $\rho_{u_i u_j} = 1$ (which assumes completely overlapping samples), would yield a singular $\mathbf{\Omega}$ matrix.

Table A1. Studies Included in the Meta-Data Set

No.	Author(s)	Country	Estimate $\hat{\theta}_i$	St.Error $\mathrm{se}(\hat{\theta}_i)$
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	0.240	0.047
4	Aschauer (1989b)	G-7 countries	0.410	0.130
5	Ram and Ramsey (1989)	US	0.240	0.055
6	Munnell (1990a)	US	0.330	0.066
7	Munnell (1990b)	US	0.060	0.016
8	Duffy-Deno and Eberts (1991)	US	0.081	0.031
9	Eisner (1991)	US	0.077	0.031
10	Tatom (1991)	US	0.042	0.127
11	Berndt and Hansson (1992)	Sweden	0.687	0.220
12	Garcia-Milà and McGuire (1992)	US	0.045	0.005
13	Bajo-Rubio and Sosvilla-Rivero (1993)	Spain	0.190	0.007
14	Finn (1993)	US	0.158	0.077
15	Mas, Maudos, Pérez, and Uriel (1993)	Spain	0.066	0.118
16	Munnell (1993)	US	0.040	0.013
17	Prud'Homme (1993)	France	0.073	0.036
18	Eisner (1994)	US	0.270	0.068
19	Evans and Karras (1994a)	US	−0.062	0.051
20	Evans and Karras (1994b)	7 OECD countries	−0.175	0.107
21	Ferreira (1994)	67 countries	0.185	0.046
22	Holtz-Eakin (1994)	US	−0.022	0.131
23	Mas, Maudos, Pérez, and Uriel (1994)	Spain	0.230	0.084
24	Otto and Voss (1994)	Australia	0.381	0.143
25	Ai and Cassou (1995)	US	0.174	0.024
26	Andrews and Swanson (1995)	US	0.110	0.018
27	Baltagi and Pinnoi (1995)	US	0.002	0.100
28	De la Fuentes and Vives (1995)	Spain	0.212	0.064
29	Holtz-Eakin and Schwartz (1995a)	US	0.112	0.040
30	Holtz-Eakin and Schwartz (1995b)	US	−0.007	0.025
31	Sturm and De Haan (1995)	The Netherlands	0.780	0.275
32	Garcia-Milà, McGuire, and Porter (1996)	US	−0.058	0.075
33	Holtz-Eakin and Lovely (1996)	US	−0.144	0.064
34	Khanam (1996)	Canada	0.140	0.080
35	Mas, Maudos, Pérez, and Uriel (1996)	Spain	0.086	0.025
36	Otto and Voss (1996)	Australia	0.168	0.080
37	Ram (1996)	53 LDCs	0.135	0.047
38	Crowder and Himarios (1997)	US	0.294	0.045
39	Kavanagh (1997)	Ireland	0.144	0.298

(Continued on next page)

Table A1 (*Continued*)

No.	Author(s)	Country	Estimate $\hat{\theta}_i$	St.Error $\mathrm{se}(\hat{\theta}_i)$
40	Kelejian and Robinson (1997)	US	−0.023	0.064
41	Khan and Kumar (1997)	95 LDCs	0.290	0.090
42	Nazmi and Ramirez (1997)	Mexico	0.129	0.028
43	Vijverberg, Vijverberg, and Gamble (1997)	US	0.481	0.111
44	Boarnet (1998)	US	0.257	0.052
45	Erenburg (1998)	US	0.290	0.070
46	Flores de Frutos, Diez, and Amaral (1998)	Spain	0.210	0.070
47	Moreno, Artís, López-Bazo and Suriñach (1998)	Spain	0.049	0.021
48	Nourzad (1998)	US	0.340	0.200
49	Otto and Voss (1998)	Australia	0.059	0.004
50	Ramirez (1998)	Mexico	0.590	0.298
51	Cadot, Roller, and Stephan (1999)	France	0.085	0.040
52	Delorme, Thompson, and Warren (1999)	US	0.213	0.132
53	Picci (1999)	Italy	0.501	0.032
54	Bonaglia, La Ferrara, and Marvelino (2000)	Italy	0.305	0.043
55	Charlot and Schmitt (2000)	France	0.317	0.021
56	Dessus and Herrera (2000)	28 LDCs	0.130	0.065
57	La Ferrara and Marcelino (2000)	Italy	−0.148	0.048
58	Nourzad (2000)	24 countries	0.529	0.107
59	Yamano and Ohkawara (2000)	Japan	0.148	0.016
60	Yamarik (2000)	US	0.088	0.032
61	Alonso-Carrera and Freire-Séren (2001)	Spain	0.126	0.027
62	Owyong and Thangavelu (2001)	Canada	0.917	0.194
63	Shioji (2001)	Japan	0.241	0.062
64	Stephan (2001)	Germany and France	0.112	0.040
65	Kemmerling and Stephan (2002)	Germany	0.170	0.037
66	Ligthart (2002)	Portugal	0.370	0.029
67	Rubio, Roldán, and Garcés (2002)	Spain	0.040	0.018
68	Stephan (2003)	Germany	0.537	0.134
69	Rodríguez-Valez and Yarias Sampedro (2004)	Spain	0.160	0.029
70	Cantos, Gumbau, and Maudos (2005)	Spain	0.042	0.016
71	Kataoka (2005)	Japan	0.185	0.018
72	Kawaguchi, Ohtake, and Tamada (2005)	Japan	0.180	0.160
73	Le and Suruga (2005)	105 countries	0.076	0.024
74	Berechman, Ozmen, and Ozbay (2006)	US	0.035	0.019
75	Creel and Poilon (2006)	6 EMU countries	0.140	0.012
76	Kamps (2006)	22 OECD countries	0.221	0.060
	Average		0.193	0.198

[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.

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