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Death to the Cobb-Douglas Production Function? A Quantitative Survey of the Capital-Labor Substitution Elasticity

Sebastian Gechert, Tomáš Havránek, Zuzana Iršová, and Dominika Ehrenbergerová *

Abstract

We show that the large elasticity of substitution between capital and labor estimated in the literature on average, 0.9, can be explained by three factors: publication bias, use of aggregated data, and omission of the first-order condition for capital. The mean elasticity conditional on the absence of publication bias, disaggregated data, and inclusion of information from the first-order condition for capital is 0.3. To obtain this result, we collect 3,186 estimates of the elasticity reported in 121 studies, codify 71 variables that reflect the context in which researchers produce their estimates, and address model uncertainty by Bayesian and frequentist model averaging. We employ nonlinear techniques to correct for publication bias, which is responsible for at least half of the overall reduction in the mean elasticity from 0.9 to 0.3. Our findings also suggest that failure to normalize the production function leads to a substantial upward bias in the estimated elasticity. The weight of evidence accumulated in the empirical literature emphatically rejects the Cobb-Douglas specification.

Abstrakt

V této práci ukazujeme, že vysokou elasticitu substituce mezi kapitálem a prací, odhadovanou v odborné literatuře v průměru na 0,9, lze vysvětlit třemi faktory: publikační selektivitou, použitím agregovaných dat a vynecháním podmínky prvního řádu pro kapitál. Průměrná elasticita při absenci publikační selektivity, použití desagregovaných dat a zahrnutí informací z podmínky prvního řádu pro kapitál činí 0,3. K dosažení tohoto výsledku shromažďujeme 3 186 odhadů elasticity ze 121 studií, kodifikujeme 71 proměnných, které odrážejí kontext, v němž autoři své odhady vytvářejí, a řešíme modelovou nejistotu bayesovským a frekventistickým průměrováním modelů. Ke korekci publikační selektivity, na kterou připadá nejméně polovina celkového snížení průměrné elasticity z 0,9 na 0,3, využíváme nelineární techniky. Naše zjištění rovněž naznačují, že absence normalizace produkční funkce vede ke značnému nadhodnocování odhadované elasticity. Poznatky shromážděné v empirické literatuře přesvědčivě vyvracejí Cobb-Douglasovu specifikaci.

JEL Codes: D24, E23, O14.
Keywords: Capital, elasticity of substitution, labor, model uncertainty, publication bias.

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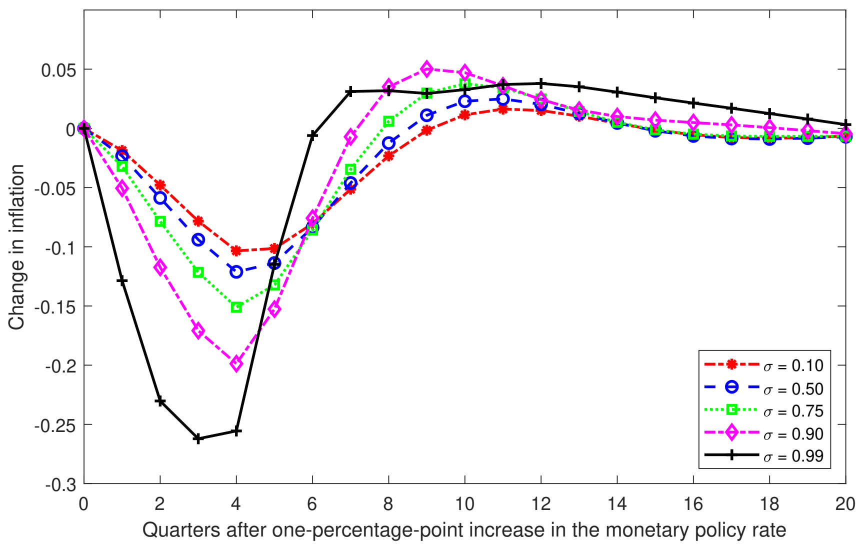
1. Introduction

A key parameter in economics is the elasticity of substitution between capital and labor. It is central to a host of problems related to economic growth and also monetary and fiscal policy. To start with, our understanding of long-run growth depends on the value of the elasticity (Solow, 1956). Klump and de La Grandville (2000) suggest that a larger elasticity in a country results in higher per capita income at any stage of development. Turnovsky (2002) argues that a smaller elasticity leads to faster convergence. The explanation for the decline of the labor share in income during recent decades that was put forward by Piketty (2014) and Karabarbounis and Neiman (2013) holds only when the elasticity surpasses one. The sustainability of growth in the absence of technological change is contingent on whether or not the elasticity of substitution exceeds one (Antras, 2004), and Cantore et al. (2014) show how the effect of technology shocks on hours worked is sensitive to the elasticity.

Second, the size of the elasticity may have practical consequences for monetary policy. Almost all models use the convenient simplification of the Cobb-Douglas production function, which implicitly assumes that the elasticity equals one. However, if the true elasticity is smaller, these models may overstate the strength of monetary policy (Chirinko and Mallick, 2017) and thus should imply a more aggressive campaign of interest rate cuts in response to a recession. For illustrative purposes, we choose the SIGMA model developed for the Federal Reserve Board by Erceg et al. (2008), as it is one of the very few DSGE models that actually allows for different values of the elasticity of substitution. In Figure 1, we see that the effectiveness of interest rate changes in steering inflation is higher with higher values of elasticity. However, in this paper we show that the Cobb-Douglas specification is at grave odds with the empirical evidence on the elasticity. Last but not least, the elasticity represents an important parameter in analyzing the effects of fiscal policies, including the effect of corporate taxation on capital formation, and in determining optimal taxation of capital (Chirinko, 2002).

Figure 1: The Elasticity of Substitution Matters for Monetary Policy



Note: The figure shows simulated impulse responses of inflation to a monetary policy shock. We use the SIGMA model of Erceg et al. (2008) developed for the Federal Reserve Board and vary the value of the capital-labor substitution elasticity while leaving other parameters at their original values. The model does not have a stable solution for σ larger than one.

Aside from convenience, the other reason for the widespread use of the Cobb-Douglas production function is that, at first sight, empirical investigations into the value of the elasticity have produced many central estimates close to 1. When each study gets the same weight, the mean elasticity reported in the literature reaches 0.9—at least based on our attempt to collect all published estimates, in total 3,186 coefficients from 121 studies. But we show that the picture is seriously distorted by publication bias. After correcting for the bias, the mean reported elasticity shrinks to 0.5. Moreover, some data and method choices affect the estimated elasticity systematically. If one agrees that sector-level data dominate more aggregated country- or state-level data and that including information from the first-order condition for capital dominates ignoring it, the implied mean estimate further decreases to 0.3. We recommend this value for the calibration of the elasticity.

The finding of strong publication bias predominates in our results. The bias arises when different estimates have a different probability of being reported depending on sign and statistical significance. The identification builds on the fact that almost all econometric techniques used to estimate the elasticity assume that the ratio of the estimate to its standard error has a symmetrical distribution, typically a *t*-distribution. So the estimates and standard errors should represent independent quantities. But if statistically significant positive estimates are preferentially selected for publication, large standard errors (given by noise in data or imprecision in estimation) will become associated with large estimates. Because empirical economists command plenty of degrees of freedom, a large estimate of the elasticity can always emerge if the researcher looks for it long enough, and an upward bias in the literature arises. A useful analogy appears in McCloskey and Ziliak (2019), who liken publication bias to the Lombard effect in biology: speakers increase their effort in the presence of noise. Apart from linear techniques based on the Lombard effect, we employ recently developed methods by Ioannidis et al. (2017), Andrews and Kasy (2019), Bom and Rachinger (2019), and Furukawa (2019), which account for the potential nonlinearity between the standard error and selection effort.

The studies in our dataset do not estimate a single population parameter; rather, the precise interpretation of the elasticity differs depending on the context in which authors derive their results. We collect 71 variables that reflect the different contexts and find that our conclusions regarding publication bias hold when we control for context. Because of the richness of the literature on the elasticity of substitution, we face substantial model uncertainty with many controls and address it by using Bayesian (Eicher et al., 2011; Steel, 2019) and frequentist (Hansen, 2007; Amini and Parmeter, 2012) model averaging. We investigate how the estimated elasticities depend on publication bias and the data and methods used in the analysis. Our results suggest that three factors drive the heterogeneity in the literature: publication bias (the size of the standard error), aggregation of input data (industry-level vs. country-level), and identification approach (whether or not information from the first-order condition for capital is ignored). In addition, the normalization of the production function used in recent studies typically brings much smaller reported elasticities, by 0.3 on average. We also find that different assumptions regarding technical change have little systematic effect on the reported elasticity and that estimations using systems of equations tend to deliver results similar to those of single-equation approaches focused on the first-order condition for capital.

As the bottom line of our analysis, we construct a synthetic study that uses all the estimates reported in the literature but assigns more weight to those that are arguably better specified. The result represents a mean estimate implied by the literature but conditional on the absence of publication bias, use of best-practice methodology, and other aspects of quality (such as publication in a leading journal). In this way we obtain an elasticity of 0.3, the best guess we can make about the parameter underpinned by half a century of accumulated empirical evidence. Defining best-practice methodology, of course, is subjective, and different authors will have different preferences on the various aspects of study design. But to arrive at 0.3, it is enough to hold two preferences: (i) industry-level data are superior to more aggregated country-level data, and (ii) including information from the first-order condition for capital is superior to ignoring it. To put these numbers into perspective, we once again turn to the Fed's SIGMA model, which employs a value of 0.5 for the elasticity of substitution (Erceg et al., 2008). This calibration corresponds to the mean estimate in the literature corrected for publication bias, without discounting any estimates based on data and methodology. The model employed by the Bank of Finland (Kilponen et al., 2016), on the other hand, uses an elasticity of 0.85, which is close to the mean estimate in the literature without correcting for publication bias. The calibration closest to our final result is that of Cantore et al. (2015), who use a prior of 0.4. Their posterior estimate is even lower, though, at below 0.2.

The remainder of the paper is structured as follows: section 2 briefly discusses how the elasticity of substitution is estimated; section 3 describes how we collect estimates of the elasticity from primary studies and provides a bird's-eye view of the data; section 4 examines publication bias; section 5 investigates the drivers of heterogeneity in the reported elasticities and calculates the mean elasticity implied by best practice in the literature; and section 6 concludes the paper. Appendixes A and B describe the bias-correction techniques designed by Furukawa (2019) and Andrews and Kasy (2019). Appendix C shows summary statistics of the variables that reflect study context, Appendix D presents robustness checks, and Appendix E includes the list of studies from which we extract estimates. The data and code are available in an online appendix at `meta-analysis.cz/sigma`.

2. Estimating the Elasticity

To set the stage for data collection and the identification of factors driving heterogeneity in results, we provide a short description of the most common approaches to estimating the elasticity of substitution between capital and labor. The concept was introduced by Hicks (1932) and almost simultaneously and independently by Robinson (1933), whose more popular definition treats the elasticity as the percentage change of the ratio of two production factors divided by the percentage change of the ratio of their marginal products. Under perfect competition, both inputs are paid their marginal products, so the elasticity of substitution can be written as

$$\sigma = \frac{d(K/L)/(K/L)}{d(w/r)/(w/r)} = -\frac{d\log(K/L)}{d\log(r/w)}, \tag{1}$$

where K and L denote capital and labor, r is the rental price of capital, and w is the wage rate. Under a quasiconcave production function, the elasticity attains any number in the interval $(0,\infty)$. If $\sigma = 0$, capital and labor are perfect complements, always used in a fixed proportion in the Leontief production function. If the elasticity lies in the interval $(0,1)$, capital and labor form gross complements. If $\sigma = 1$, the production function becomes Cobb-Douglas, and the relative change in quantity becomes exactly proportional to the relative change in prices. If the elasticity lies in the interval $(1,\infty)$, capital and labor form gross substitutes.

Although the concept of the elasticity of substitution was introduced in the 1930s, empirical estimates were only enabled by an innovation that came more than 20 years later: the introduction of the constant elasticity of substitution (CES) production function by Solow (1956), later popularized by Arrow et al. (1961). The CES production function can be written as

$$Y_t = C[\pi(A_t^K K_t)^{\frac{\sigma-1}{\sigma}} + (1-\pi)(A_t^L L_t)^{\frac{\sigma-1}{\sigma}}]^{\frac{\sigma}{\sigma-1}}, \tag{2}$$

where σ denotes the elasticity of substitution, K and L are capital and labor, C is an efficiency parameter, and π is a distributional parameter. The fraction $\frac{\sigma-1}{\sigma}$ is often labeled as ρ, a transformation of the elasticity called the substitution parameter. A_t^K and A_t^L denote the level of efficiency of the respective inputs, and variations in A_t^K and A_t^L over time reflect capital- and labor-augmenting technological change. When $A_t^K = A_t^L = A_t$, technological change becomes Hicks-neutral, which means that the marginal rate of substitution does not change when an innovation occurs.

The CES production function is nonlinear in parameters, and in contrast to the Cobb-Douglas case, a simple analytical linearization does not emerge. Thus the CES production function can be estimated (i) in its nonlinear form, (ii) in a linearized form as suggested by Kmenta (1967), or (iii) by using first-order conditions (FOCs). Kmenta (1967) introduced a logarithmized version of Equation 2 with Hicks-neutral technological change:

$$\log Y_t = \log C + \frac{\sigma}{\sigma - 1} \log \left[\pi K_t^{\frac{\sigma-1}{\sigma}} + (1-\pi) L_t^{\frac{\sigma-1}{\sigma}} \right] \tag{3}$$

and then applied a second-order Taylor series expansion to the term $\log[\cdot]$ around the point $\sigma = 1$ to arrive at a function linear in σ:

$$\log Y_t = \log C + \pi \log K_t + (1-\pi) \log L_t - \frac{(\sigma-1)\pi(1-\pi)}{2\sigma} (\log K_t - \log L_t)^2. \tag{4}$$

Estimation of σ via first-order conditions was first suggested by Arrow et al. (1961). The underlying assumptions involve constant returns to scale and fully competitive factor and product markets. The FOC with respect to capital can be written as follows:

$$\log\left(\frac{Y_t}{K_t}\right) = \sigma \log\left(\frac{1}{\pi}\right) + (1-\sigma)\log(A_t^K C) + \sigma \log\left(\frac{r_t}{p_t}\right). \tag{5}$$

Consequently, the FOC with respect to labor implies

$$\log\left(\frac{Y_t}{L_t}\right) = \sigma \log\left(\frac{1}{1-\pi}\right) + (1-\sigma)\log(A_t^L C) + \sigma \log\left(\frac{w_t}{p_t}\right), \tag{6}$$

where p is the price of the output. The two conditions can be combined to yield

$$\log\left(\frac{K_t}{L_t}\right) = \sigma \log\left(\frac{\pi}{1-\pi}\right) + (\sigma-1)\log\left(\frac{A_t^K}{A_t^L}\right) + \sigma \log\left(\frac{w_t}{r_t}\right). \tag{7}$$

In a similar way, one can derive FOCs with respect to the labor share $(wL)/Y$, the capital share $(rK)/Y$, or their reversed counterparts. The FOCs can be estimated separately as single equations, within a system of two or three FOCs, and as a system of FOCs coupled with a nonlinear or linearized CES production function. The latter approach (also called the supply-side system approach) has become especially popular in recent studies. León-Ledesma et al. (2010) assert that using the supply-side system approach dominates one-equation estimation, especially when coupled with cross-equation restrictions and normalization, which was suggested by de La Grandville (1989) and Klump and de La Grandville (2000). After scaling technological progress so that $A_0^K = A_0^L = 1$, the normalized production function can be written as

$$Y_t = Y_0 \left[\pi_0 \left(\frac{A_t^K K_t}{K_0}\right)^{\frac{\sigma-1}{\sigma}} + (1-\pi_0)\left(\frac{A_t^L L_t}{L_0}\right)^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1}}, \tag{8}$$

where $\pi_0 = r_0 K_0/(r_0 K_0 + w_0 L_0)$ denotes the capital income share evaluated at the point of normalization. The point of normalization can be defined, for instance, in terms of sample means. In other words, normalization means rewriting the production function in indexed number form (Klump et al., 2012). Normalization makes it possible to overcome the "impossibility theorem" as described in Diamond et al. (1978), i.e., to identify both the elasticity of substitution and the growth rates of biased technical change (León-Ledesma et al., 2010; Cantore and Levine, 2012), and has partly led to a revival of the CES production function in empirical research (Klump et al., 2007, 2008).[1]

Though the aforementioned approaches to estimating the elasticity dominate the literature, we also consider other approaches, in particular the translog production function. The translog function is quadratic in the logarithms of inputs and outputs and provides a second-order approximation to any production frontier (now omitting subscript t for ease of exposition):

$$\log Y = \log \alpha_0 + \sum_i \alpha_i \log X_i + \frac{1}{2}\sum_i \sum_j \alpha_{ij} \log X_i \log X_j, \tag{9}$$

where α_0 denotes the state of technological knowledge, and X_i and X_j are inputs, in our case capital and labor. The translog production frontier provides a wider set of options for substitution and transformation patterns than a frontier based on the CES production function. Due to the duality principle, researchers often employ the translog cost function instead:

$$\log C = \alpha_0 + \theta_1 \log Y + \frac{1}{2}\theta_2 (\log Y)^2 + \sum_i \beta_i \log P_i + \frac{1}{2}\sum_i \sum_j \varepsilon_{ij} \log P_i \log P_j + \sum_i \delta_i \log P_i \log Y, \tag{10}$$

where C denotes total costs and $i = K, L$, and P_i is the input factor price (that is, w and r). Using Sheppard's lemma, the following cost share functions can be derived:

$$S_i = \beta_i + \sum_i \varepsilon_{ij} \log P_j + \delta_i \log Y, \tag{11}$$

where S_i denotes the share of the i-th factor in total costs. In this case, Allen partial elasticities of substitution are most often estimated and are defined as

$$\sigma_{ij} = \frac{\gamma_{ij} + S_i S_j}{S_i S_j}. \tag{12}$$

We include estimates from all of the above-mentioned specifications, as each of them provides a measure of the elasticity of substitution between capital and labor, broadly defined. Then we control for the various aspects of the context in which researchers obtain their estimates. These aspects are presented and discussed in detail later in section 5, while the following section describes the dataset of the estimated elasticities.

[1] In fact, there is much more to normalization than representation in indexed number form, as shown in Cantore and Levine (2012) and De La Grandville (2009). The parameters in a non-normalized production function do not have economic interpretation (i.e., they are not deep): Cantore and Levine (2012) show that without normalization, only the elasticity of substitution is *dimensionless*; the other two key parameters—the efficiency and distribution parameters (see Eq. 2)—are *dimensional*, i.e., they depend on the elasticity of substitution and factor income shares and thus can change with the choice of units of inputs and outputs. Cantore and Levine (2012) show that next to the indexed number form representation, i.e., the form of deviation about a reference point, there is another option for creating dimensionless parameters, called re-parametrization. Nevertheless, most of the empirical literature we survey deals with normalization in the way shown in Eq. 8.

3. Data

We use Google Scholar to search for studies estimating the elasticity. Google's algorithm goes through the full text of studies, thus increasing the coverage of suitable published estimates, irrespective of the precise formulation of the study's title, abstract, and keywords. Our search query, available in the online appendix, is calibrated so that it yields the best-known relevant studies among the first hits. We examine the first 500 papers returned by the search. In addition, we inspect the lists of references in these studies and their Google Scholar citations to check whether we can find usable studies not captured by our baseline search—a method called "snowballing" in the literature on research synthesis. We terminate the search on August 1, 2018, and do not add any new studies beyond that date.

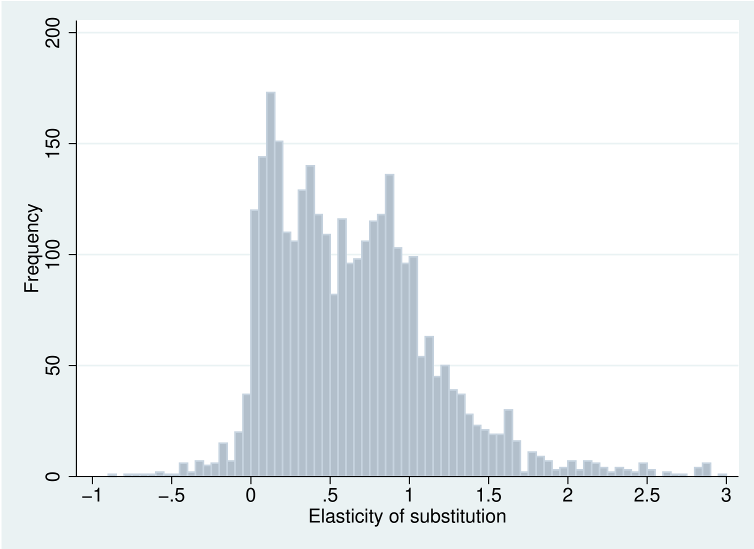
To be included in our dataset, a study must satisfy three criteria. First, at least one estimate in the study must be directly comparable with the estimates described in section 2. Second, the study must be published. This criterion is mostly due to feasibility, since even after restricting our attention to published studies the dataset involves the manual collection of hundreds of thousands of data points. Moreover, we expect published studies to exhibit higher quality on average and to contain fewer typos and mistakes in reporting their results. Note that the inclusion of unpublished papers is unlikely to alleviate publication bias (Rusnak et al., 2013): researchers write their papers with the intention to publish.[2] Third, the study must report standard errors or other statistics from which the standard error can be computed. If the elasticity is not reported directly but can be derived from the presented results, we use the delta method to approximate the standard error. Omitting the estimates with approximated standard errors does not change our results up to a second decimal place.

Using the search algorithm and inclusion criteria described above, we collect 3,186 estimates of the elasticity of substitution from 121 studies. To our knowledge, this makes our paper the largest meta-analysis conducted in economics so far: Doucouliagos and Stanley (2013), for example, survey dozens of meta-analyses and find that the largest one uses 1,460 estimates. Ioannidis et al. (2017) report that the mean number of estimates used in economics meta-analyses is 400. The literature on the elasticity of substitution is vast, with a long tradition spanning six decades and more than 100 countries. The list of the studies we include in the dataset (we call them "primary studies") is available in Appendix E. Out of the 121 studies, 39 are published in the five leading journals in economics. Altogether, they have received more than 20,000 citations in Google Scholar, highlighting the importance of the topic.

The mean reported estimate of the elasticity of substitution is 0.9 when we give the same weight to each study; that is, when we weight the estimates by the inverse of the number of observations reported per study. The simple mean of all the estimates is 0.8. We consider the weighted mean to be more informative, because the simple mean is driven by studies that report many estimates, typically the results of robustness checks, and we see little reason to place more weight on such studies. For both such constructed means, in any case, the deviation from the Cobb-Douglas specification is not dramatic, and one could use the mean estimate from the literature as a justification of why the Cobb-Douglas production function presents a solid approximation of the data. We will argue that such an interpretation of the data misleads the reader because of publication bias and misspecifications in the literature.

[2] A more precise label for publication bias is therefore "selective reporting", but we use the former, more common one to maintain consistency with previous studies on the topic, such as DeLong and Lang (1992), Card and Krueger (1995), and Ashenfelter and Greenstone (2004).

Figure 2: Distribution of the Estimated Elasticities



Note: Estimates smaller than −1 and larger than 3 are excluded from the figure for ease of exposition but are included in all statistical tests.

Figure 2 shows the distribution of the estimates in our dataset. Curiously, the distribution is bimodal, with peaks near 0 and slightly under 1, pointing to strong and systematic heterogeneity among the estimates. Three-quarters of the estimates lie between 0 and 1, 21% are greater than one, and only 4% attain a theoretically implausible negative value. At first sight, it is apparent that a researcher wishing to calibrate her structural model can find some empirical justification for any value of the elasticity between 0 and 1.5. There are a few extreme outliers in the data, thus we winsorize the estimates at the 5% level (our main results hold with different winsorization levels). In Figure 3 we show the box plot of the estimates. The elasticities vary not only across studies, but also within studies. Most studies report at least some estimates close to 1, giving further (but superficial, as we will show later) credence to the Cobb-Douglas specification.

Apart from the estimates of σ and their standard errors, we collect 71 variables that capture the context in which the different estimates are obtained. In consequence, we had to collect more than 220,000 data points from primary studies—a laborious but complex exercise that cannot be delegated to research assistants. The data were collected by two of the coauthors of this paper, each of whom then double-checked random portions of the data collected by the other coauthor in order to minimize potential mistakes arising from manually coding so many entries. The entire process took seven months, and the final dataset is available in the online appendix. Out of the 71 variables that we collect, 50 are included in the baseline model, while the rest only appear in the subsamples of the data for which they apply.

A casual look at the estimates reveals systematic differences among the reported elasticities derived from different data and identified using different methodologies. The most striking patterns are shown in Figure 4. For instance, while the mean of the estimates coming from the first-order condition for capital is 0.4, for the first-order condition for labor the mean is twice as much. The mean of the elasticities based on time series data is 0.5, while for cross-sectional data it reaches 0.8. Estimates based on industry-level data appear to be systematically smaller than those based on country-level data, and elasticities presented for individual industries are on average larger than estimates aggregated at the level of the entire economy. These patterns explain the bimodality of

Figure 3: Estimates Vary Both Across and Within Studies

Note: The figure shows a box plot of the estimates of the elasticity of substitution reported in individual studies. The box shows the interquartile range (P25–P75) and the median highlighted. Whiskers cover (P25 − 1.5*interquartile range) to (P75 + 1.5*interquartile range). The dots are remaining (outlying) estimates. Estimates smaller than −1 and larger than 3 are excluded from the figure for ease of exposition but are included in all statistical tests.

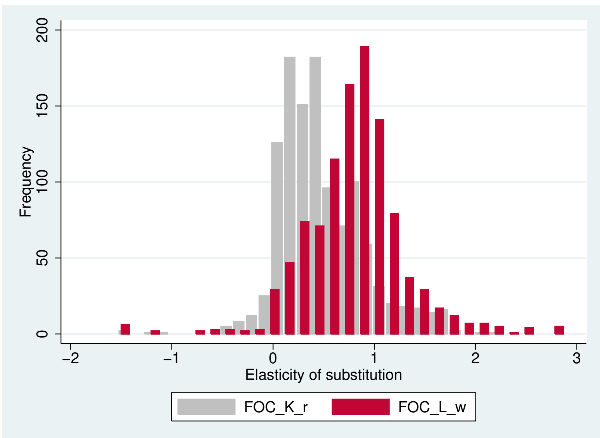
the overall histogram presented in Figure 2. Nevertheless, at this point we cannot be sure whether the differences are fundamental or whether they reflect correlations with other factors. A detailed analysis of heterogeneity is available in section 5. Some of the differences among the estimates may also be attributable to publication bias, an issue to which we turn next.

4. Publication Bias

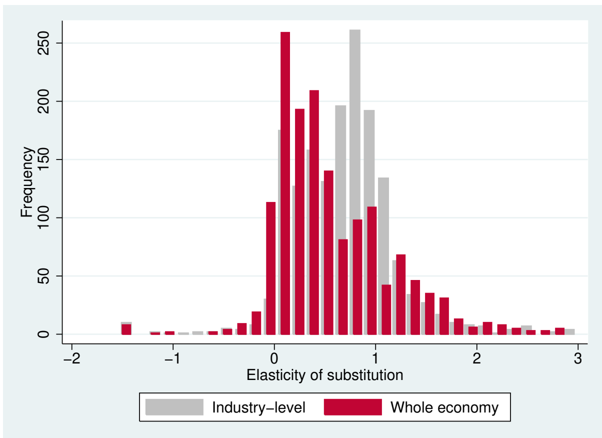
Theory and intuition provide little backing for a zero or negative elasticity of substitution between capital and labor, so it seems natural to discard such estimates. Previous researchers (most prominently, Ioannidis et al., 2017) have shown that such a censoring distorts the inference drawn from the literature, and here we document that publication bias is strong in the case of the elasticity of substitution. Even when the true elasticity is positive in every single estimation context, given sufficient noise in data and methods, both negative and zero (statistically insignificant) estimates will appear. For each individual author who obtains such estimates, it makes little sense to focus on them; it will bring her study closer to the truth if she finds and highlights a specification that yields a clearly positive elasticity. The problem is that noise in data and methods will also produce estimates that are much larger than the true effect, and such estimates are hard to identify: no upper

Figure 4: Prima Facie Patterns in the Data

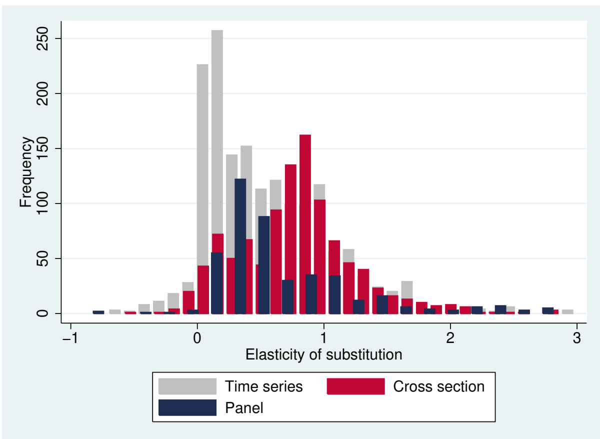
(a) Estimation form



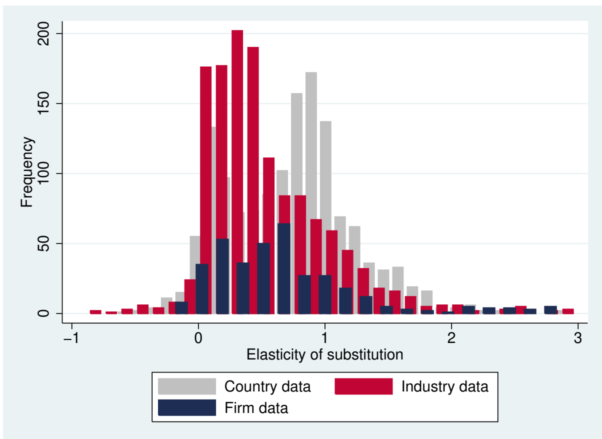
(b) Results aggregation



(c) Data dimension



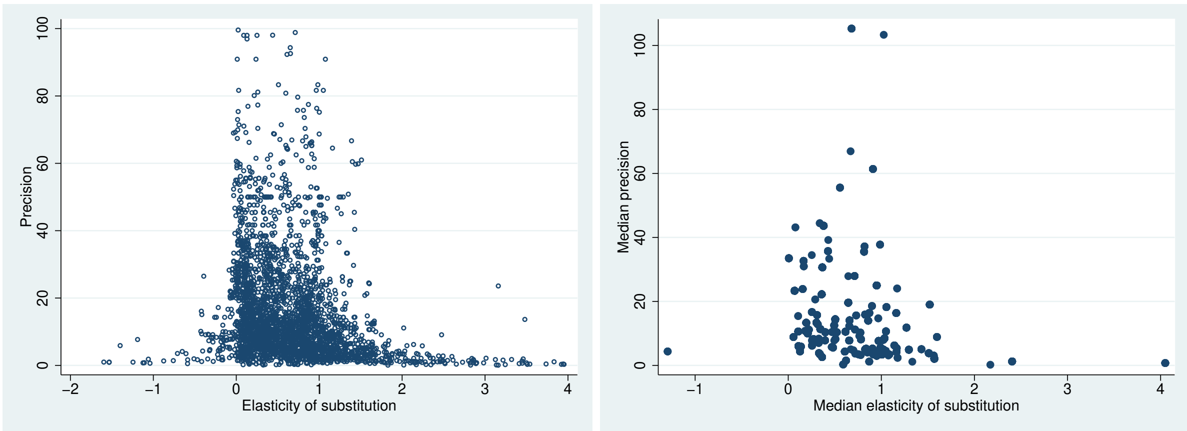
(d) Input data



Note: FOC = first-order condition. Estimates smaller than -1 and larger than 3 are excluded from the figure for ease of exposition but are included in all statistical tests.

threshold symmetrical to zero exists that would tell the researcher the estimates are implausible. If many small imprecise estimates are discarded but many large imprecise estimates are reported, an upward bias arises in the literature. Ioannidis et al. (2017) document that the typical exaggeration due to publication bias in economics is twofold. We find it remarkable that no study has addressed potential publication bias in the literature on the elasticity of substitution between capital and labor, one of the most important parameters in economics.

Figure 5: Negative Estimates of the Elasticity Are Underreported



Note: In the absence of publication bias, the scatter plot should resemble an inverted funnel symmetrical around the most precise estimates. The left panel shows all the estimates, while the right panel shows the median estimates from each study. Estimates smaller than -2 and larger than 4 are excluded from the figure for ease of exposition but are included in all statistical tests.

Figure 5 provides a graphical illustration of the mechanism outlined in the previous paragraph. In the scatter plot, the horizontal axis measures the magnitude of the estimated elasticities, and the vertical axis measures their precision. In the absence of publication bias, the scatter plot will form an inverted funnel: the most precise estimates will lie close to the true mean elasticity, while imprecise estimates will be more dispersed, and both small and large imprecise estimates will appear with the same frequency. (The scatter plot is thus typically called a funnel plot; Stanley and Doucouliagos 2010.) The figure shows the predicted funnel shape, still with plenty of heterogeneity at the top, but also shows asymmetry. For the funnel to be symmetrical, and hence consistent with the absence of publication bias, we should observe many more reported negative and zero estimates.

To identify publication bias numerically, we refer to the analogy with the Lombard effect mentioned in the Introduction: other things being equal, under publication bias authors will increase their effort (specification search) in response to noise (imprecision resulting from data or methodology). Thus, publication bias is consistent with finding a correlation between estimates of the elasticity and their standard errors. In contrast, if there is no bias, there should be no correlation, because the properties of the techniques used to obtain the elasticity ensure that the ratio of the estimate to its standard error has a *t*-distribution. It follows that estimates and standard errors should be statistically independent quantities. In any case, the intercept in the regression of the estimated elasticities on their standard errors can be interpreted as the mean elasticity corrected for potential publication bias (Stanley, 2005). It represents the mean elasticity conditional on the standard error approaching zero, and because in this specification publication bias forms a linearly increasing function of the standard error, the intercept measures the corrected estimate. The coefficient on the standard error measures publication bias and can be thought of as a test of the asymmetry of the funnel plot. So we have

$$\hat{\sigma}_{ij} = \sigma_0 + \gamma SE(\hat{\sigma}_{ij}) + u_{ij}, \tag{13}$$

where $\hat{\sigma}$ is the i-th estimated elasticity in study j, γ denotes the intensity of publication bias, and σ_0 represents the mean elasticity corrected for the bias.

In Table 1 we report the results of several specifications based on Equation 13. We cluster standard errors at both the study and the country level, as estimates are unlikely to be independent within these two dimensions; our implementation of two-way clustering follows Cameron et al. (2011). We also report wild bootstrap confidence intervals (Cameron et al., 2008). In all specifications, we find a statistically significant and positive coefficient on the standard error (publication bias) and a significant and positive intercept (the mean elasticity corrected for the bias). After correcting for publication bias, the mean elasticity drops from 0.9 to 0.5. The result is robust across all specifications with the exception of one, which suggests an even stronger bias and smaller corrected elasticity.

Table 1: Linear Tests of Funnel Asymmetry Suggest Publication Bias

	OLS	FE	BE	Precision	Study	IV
SE (publication	0.881^{***}	0.656^{***}	1.111^{***}	1.025^{***}	0.888^{***}	2.186^{***}
bias)	(0.086)	(0.201)	(0.190)	(0.115)	(0.094)	(0.413)
	[0.49; 1.21]	–	–	[0.59; 1.40]	[0.62; 1.22]	[1.20; 3.68]
Constant (mean	0.492^{***}	0.529^{***}	0.499^{***}	0.468^{***}	0.544^{***}	0.279^{***}
beyond bias)	(0.028)	(0.033)	(0.048)	(0.025)	(0.039)	(0.070)
	[0.38; 0.61]	–	–	[0.36; 0.61]	[0.44; 0.64]	[0.04; 0.47]
Studies	121	121	121	121	121	121
Observations	3,186	3,186	3,186	3,186	3,186	3,186

Note: The table presents the results of regression $\hat{\sigma}_{ij} = \sigma_0 + \gamma SE(\hat{\sigma}_{ij}) + u_{ij}$. $\hat{\sigma}_{ij}$ and $SE(\hat{\sigma}_{ij})$ are the i-th estimates of the elasticity of substitution and their standard errors reported in the j-th study. The standard errors of the regression parameters are clustered at both the study and country level and shown in parentheses (the implementation of two-way clustering follows Cameron et al., 2011). OLS = ordinary least squares. FE = study-level fixed effects. BE = study-level between effects. Precision = the inverse of the reported estimate's standard error is used as the weight. Study = the inverse of the number of estimates reported per study is used as the weight. IV = the inverse of the square root of the number of observations employed by researchers is used as an instrument for the standard error. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level. Standard errors in parentheses. Whenever possible, in square brackets we also report 95% confidence intervals from wild bootstrap clustering; the implementation follows Roodman (2019), and we use Rademacher weights with 9,999 replications.

The first column of Table 1 reports a simple OLS regression. The second column adds study-level fixed effects in order to account for unobserved study-specific characteristics, but little changes. (Adding country dummies would also produce similar results.) The third column uses between-study variance instead of within-study variance, and the estimate of the corrected mean remains not much affected. Next, we apply two weighting schemes. First, the precision becomes the weight, as suggested by Stanley and Doucouliagos (2017), which adjusts for the heteroskedasticity in the regression. Similar weights are used in physics for meta-analyses of particle mass estimates (Baker and Jackson, 2013). The corrected mean elasticity becomes a bit smaller, but is not far from 0.5. Second, we weight the data by the inverse of the number of observations reported in a study, so that each study has the same impact on the results. Again, the difference is small in comparison to other specifications. In the last column, we report the results of an instrumental variable (IV) regression. IV represents a crucial robustness check, because in primary studies, estimates and standard errors are jointly determined by the estimation technique. If some techniques produce systematically larger standard errors and point estimates, our finding of publication bias could be spurious. An intuitive instrument for the standard error is the inverse of the square root of the number of observations used

Table 2: Nonlinear Techniques Corroborate Publication Bias

	Bom & Rachinger (2019)	Furukawa (2019)	Andrews & Kasy (2019)	Ioaninidis et al. (2017)
Mean beyond bias	0.52 (0.09)	0.55 (0.21)	0.43 (0.02)	0.50 (0.06)

Note: Standard errors in parentheses. The method developed by Bom and Rachinger (2019) searches for a precision threshold above which publication bias is unlikely. The methods developed by Furukawa (2019) and Andrews and Kasy (2019) are described in detail in Appendixes A and B. The method developed by Ioannidis et al. (2017) focuses on estimates with adequate power.

in the primary study: the root is correlated with the standard error by definition but is unlikely to be very correlated with the use of a particular estimation technique. Using IV we obtain a larger estimate of publication bias and a smaller estimate of the mean elasticity corrected for publication bias, 0.3.[3]

The simple tests based on the Lombard effect and presented in Table 1 are intuitive but can themselves be biased if publication selection does not form a linear function of the standard error. For example, it might be the case that estimates are automatically reported if they cross a particular precision threshold. This is the intuition behind the estimator due to Bom and Rachinger (2019) presented in Table 2. Bom and Rachinger (2019) show how to estimate this threshold for each literature and introduce an "endogenous kink" technique that extends the linear test based on the Lombard effect. Next, Furukawa (2019) provides a nonparametric method that is robust to various assumptions regarding the functional form of publication bias and the underlying distribution of true effects. Furukawa (2019) suggests using only a portion of the most precise estimates, the stem of the funnel plot, and determines this portion by minimizing the trade-off between variance (decreasing in the number of estimates included) and bias (increasing in the number of imprecise estimates included). The stem-based method is generally more conservative than those commonly used, producing wide confidence intervals; the details are available in Appendix A.

Another nonlinear method to correct for publication bias is advocated by Andrews and Kasy (2019). They show how the conditional publication probability (the probability of publication as a function of a study's results) can be nonparametrically identified and then describe how publication bias can be corrected for if the conditional publication probability is known. The underlying intuition involves jumps in publication probability at conventional p-value cut-offs. Using their method, we estimate that positive elasticities are six times more likely to be published than negative ones. We include more details on the approach and estimation in Appendix B. Finally, the remaining estimate in Table 2 arises using the approach championed by Ioannidis et al. (2017), who focus only on estimates with adequate statistical power. We conclude that both linear and nonlinear techniques agree that 0.5 represents a robust estimate of the mean elasticity of substitution after correcting the literature for publication bias. Since the uncorrected mean equals 0.9, the exaggeration due to publication bias is almost twofold, consistent with the rule of thumb suggested by Ioannidis et al. (2017). Therefore, when we give the same weight to all approaches used in primary studies, the empirical literature as a whole provides no support for the Cobb-Douglas production function. But perhaps poor data and misspecifications bias the mean estimate downwards. We investigate this issue in the next section.

[3] The result is consistent with some estimation techniques or aspects of data influencing the point estimates and standard errors in opposite directions. In the next section we explicitly control for 71 aspects of study design, including data and methodology, and our final estimate also equals 0.3.

5. Heterogeneity

In section 2 and section 3 we discussed several prominent aspects of study design that might systematically influence the reported estimates of the elasticity. But many additional study characteristics can certainly play a role, and we need to control for them. To assign a pattern to the apparent heterogeneity in the literature, we collect 71 variables that reflect the context in which researchers obtain their estimates. The variables capture the characteristics of the data, specification choice, econometric approach, definition of the production function, and publication characteristics. (Moreover, the effects of different ways of measuring capital and labor are examined in subsamples of the main dataset and presented in Appendix D.) The variables, grouped in these categories, are discussed below and listed in Table C1 and Table C2 in Appendix C together with their definitions and summary statistics.

5.1 Variables

Data characteristics A central distinguishing feature of the studies concerns the level of data aggregation. Almost half (45%) of the studies employ country- or state-level data, which forms our reference category. We include a dummy variable equal to one if the study uses industry-level data (43% of the estimates) and firm-level data (12% of the estimates). We also include a dummy equal to one when the resulting estimate does *not* represent the whole economy but is reported at a disaggregated level for various industries. Moreover, we add controls for potential cross-country differences: a dummy for the US, developed European countries, and developing countries, as the substitutability between capital and labor may differ with the level of economic development and across institutional settings. For instance, Duffy and Papageorgiou (2000) suggest that capital and labor become less substitutable in poorer countries.

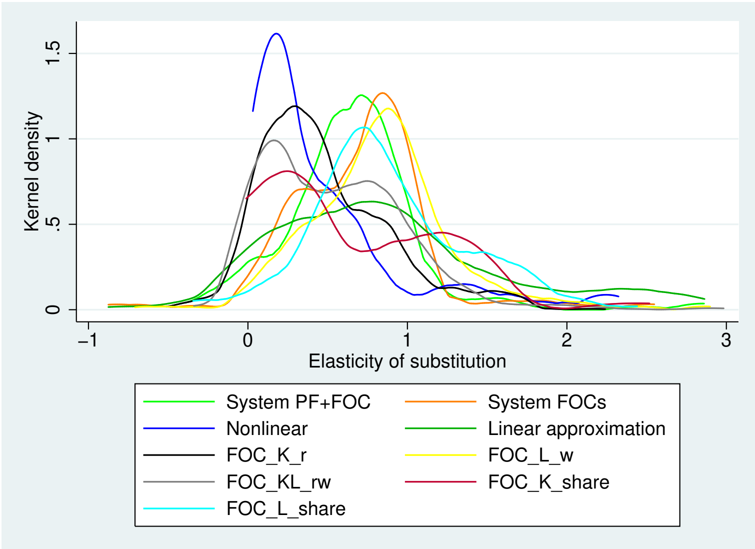
To account for potential small-sample bias, we control for the number of observations used in each study. We also include the midpoint of the data period to capture a potential positive trend in the elasticity over time, which could be due to economic development within a country, a changing composition of the inputs, or changes in their relative efficiency (Cantore et al., 2017). Regarding data frequency, 89% of the estimates employ annual data; we thus use annual data as the baseline category and include a dummy variable for the use of quarterly data. Moreover, we control for data dimension—whether time series, cross-sectional, or panel data are used. Most of the studies (around 53%) employ time series data, which we take as the reference category.

The final subset of variables covering data characteristics describes the source of data. Many estimates are based on data from the same databases—the largest number of studies employ data from the US Annual Survey of Manufacturers and Census of Manufacturers. The second largest group is the KLEM database by Jorgenson (2007), followed by the OECD's International Sectoral Database and Structural Analysis Database. We do not have a prior on how data sources should affect estimates, yet we still prefer not to ignore this potential source of variation and include the corresponding dummies as control variables.

Specification Concerning the specification of the various studies described in section 2, we distinguish between estimation via single first-order conditions (FOCs), systems of more than one FOC, systems of the production function plus FOCs, linear approximations of the production function, and nonlinear estimation of the production function. We also discriminate between the FOC for labor based on the wage rate, the FOC for capital based on the rental rate of capital, the FOC for the capital-labor ratio based on the ratio between the wage rate and the rental rate of capital, the

FOC for capital share, and the FOC for labor share in income. In total, this gives us nine distinct categories for estimation specification. We choose the FOC for capital based on the rental rate as the reference category because it represents the most frequently used specification (35%), though closely followed by the FOC for labor based on the wage rate (33% of the estimates). A special case of the FOC for capital is its inverse estimation, in which the resulting estimates are labeled user-cost elasticities; examples include Smith (2008) and Chirinko et al. (2011).

Figure 6: Estimation Form Matters for the Reported Elasticities



Note: A detailed description of the variables is available in Table C1.

The differences in the estimates derived from the various specifications are clearly visible in the data (Figure 6). While the mean of the estimates derived from the FOC for labor based on the wage rate reaches 1.1, estimates derived from the FOC for capital based on the rental rate of capital are on average only 0.5. Estimates obtained from the linear approximation of the production function also stand out, reaching a mean value of 1.1. Some of these patterns were noted early in the history of the estimation of the elasticity, for example by Berndt (1976), and later discussed by Antras (2004) and Young (2013). We attempt to quantify the patterns, while simultaneously controlling for other influences.

Regarding system estimations, two other important specification aspects can influence the reported elasticities: normalization and cross-equation restrictions. Normalization, suggested by de La Grandville (1989), further explored by Klump and de La Grandville (2000), and first implemented empirically by Klump et al. (2007), has been used by only a small fraction of the studies in our database. Normalization starts from the observation that a family of CES functions whose members are distinguished only by different elasticities of substitution needs a common benchmark point. Since the elasticity of substitution is defined as a point elasticity, one needs to fix benchmark values for the level of production, factor inputs, and the marginal rate of substitution, or equivalently for per capita production, capital deepening, and factor income shares. Normalization implies representing the production function in a consistent indexed number form (Klump et al., 2012). A proper choice of the point of normalization facilitates the identification of deep technical parameters; in other words, it overcomes the "impossibility theorem" by enabling the elasticity of substitution and growth rates of technical change to be identified at the same time (León-Ledesma et al., 2010). According to León-Ledesma et al. (2010), the superiority of system estimation compared to the single FOC approach is further enhanced when complemented with normalization. In their Monte Carlo experiment, they show that without normalization, estimates tend towards one.

Some estimations of systems employ cross-equation restrictions that restrict parameters across two or more equations to be equal, as in Zarembka (1970), Krusell et al. (2000), and Klump et al. (2007). To account for possible differences, we additionally include a dummy for cross-equation restrictions.

While the vast majority of estimates come from single-level production functions, estimates of the elasticity of substitution between capital and labor can also be found in studies using two-level production functions, including additional inputs such as energy and material (e.g., Van der Werf, 2008; Dissou et al., 2015). We control for two-level production functions as a special case. Moreover, when estimates of the elasticity rely on such two-level production functions, linear approximations of the production function, or a system of linear approximation in conjunction with share factors, researchers commonly report partial elasticities of substitution, for which we control as well. Our results are robust to excluding partial elasticities.

Econometric approach Our reference category for the choice of econometric technique is OLS. We include a dummy for the case when the model is dynamic, which holds for approximately one-quarter of all observations. The second dummy we include equals one if seemingly unrelated regression (SUR) is used—it is often employed for the estimation of systems of equations (11% of all estimates). An important aspect of estimating the elasticity, as pointed out by Chirinko (2008), is whether the estimate refers to a long-run or a short-run elasticity. Our reference category consists of explicit long-run specifications, that is, models in which coefficients are meant to be long-run and the specification is adjusted accordingly. We opt for long-run elasticities as a reference point as they are regarded as more informative for economic decisions. Explicit long-run specifications include estimations of cointegration relations or interval-difference models, where data are averaged over longer intervals to mimic lower frequencies; distributed lag models can also give a long-run estimate. Conversely, the short-run approach modifies the estimating equation to account for temporal dynamics. Examples include estimation of implicit investment equations, as in Eisner and Nadiri (1968) or Eisner (1969), differenced models, and estimation of short-run elements from error correction models or distributed lag models. The vast majority of estimates (70%) are meant to be long-run but the specification is unadjusted.

Production function components The fourth category of control variables comprises the ingredients of the production function. We include a dummy variable for the case when other inputs (energy, materials, human capital) are considered as additional factors of production, for instance by Humphrey and Moroney (1975), Bruno and Sachs (1982), and Chirinko and Mallick (2017). We include a dummy that equals one when a study differentiates between skilled and unskilled labor. We also subject the estimates to the following questions. Does the production function assume Hicks-neutral technological change (our reference category), Harrod-neutral technological change (i.e., labor-augmenting, LATC), or Solow-neutral technological change (i.e., capital-augmenting, CATC)? Are the dynamics of technological change important in explaining the heterogeneity? The growth rate of technological change can be either zero (our reference), constant or—with flexible Box and Cox (1964) transformation—exponential, hyperbolic, or logarithmic. According to the impossibility theorem suggested by Diamond et al. (1978), it is infeasible to identify both the elasticity of substitution and the parameters of technological change at the same time, so researchers tend to impose one of the three specific forms of technological change and implicit or explicit assumptions on its growth rate. We include the corresponding dummy variables.

We distinguish between estimates of gross and net elasticity, based on whether gross or net data for output and the capital stock are used. As pointed out in Semieniuk (2017), the distinction between net and gross elasticity is important with respect to the inequality argument of Piketty (2014): for his explanation of the decline in the labor share to hold, σ needs to exceed one in net terms. Elasticities based on net quantities should naturally yield smaller results (Rognlie, 2014). Finally, we include two additional dummies—first, for the case when researchers abandon the assumption of constant returns to scale; second, for the case when researchers relax the assumption of perfectly competitive markets.

Publication characteristics We include four study-level variables: the year of the appearance of the first draft of the paper in Google Scholar, a dummy for the paper being published in a top five journal, the recursive discounted RePEc impact factor of the outlet, and the number of citations per year since the first appearance of the paper in Google Scholar. We include these variables in order to capture aspects of study quality not reflected by observable differences in data and methods.

Moreover, we include two additional dummies. The first variable measures whether the study's central focus is the elasticity of substitution between capital and labor or whether the estimate is a byproduct of a different exercise, such as in Cummins and Hassett (1992) and Chwelos et al. (2010). The second variable equals one if the author explicitly prefers the estimate in question, and equals minus one if the estimate is explicitly discounted. Nevertheless, researchers typically do not reveal their exact preferences regarding the individual estimates they produce, so the variable equals zero for most estimates.

5.2 Estimation

An obvious thing to do at this point is to regress the reported elasticities on the variables reflecting the context in which researchers obtain their estimates:

$$\hat{\sigma}_{ij} = \alpha_0 + \sum_{l=1}^{49} \beta_l X_{l,ij} + \gamma SE(\hat{\sigma}_{ij}) + \mu_{ij}, \tag{14}$$

where $\hat{\sigma}_{ij}$ again denotes estimate i of the elasticity of substitution reported in study j, $X_{l,ij}$ represents the control variables described in subsection 5.1, γ again denotes the intensity of publication bias, and α_0 represents the mean elasticity corrected for publication bias but *conditional* on the definition of the variables included in X—that is, the intercept means nothing on its own, and μ_{ij} stands for the error term.

But using one regression is inadequate because of model uncertainty. With so many variables reflecting study design, including all of them would substantially attenuate the precision of our estimation. (We use 50 variables in the baseline estimation; the remaining 21 variables related to measurement of capital and labor and industry-level characteristics are included in the three subsamples presented in Appendix D.) One solution is to reduce the number of variables to about 10, which could allow for simple estimation—but doing so would ignore many aspects in which estimates and studies differ. Another commonly applied solution to model uncertainty is stepwise regression, but sequential t-tests are statistically problematic as individual variables can be excluded by accident. The solution that we choose here is Bayesian model averaging (BMA; see, for example, Eicher et al., 2011; Steel, 2019), which arises naturally as a response to model uncertainty in the Bayesian setting.

BMA runs many regression models with different subsets of variables; in our case there are 2^{50} possible subsets. Assigned to each model is a posterior model probability (PMP), an analog to information criteria in frequentist econometrics, measuring how well the model performs compared to other models. The resulting statistics are based on a weighted average of the results from all the regressions, the weights being the posterior model probabilities. For each variable we thus obtain a posterior inclusion probability (PIP), which denotes the sum of the posterior model probabilities of all the models in which the variable is included. Using the laptop on which we wrote this paper, it would take us decades to estimate all the possible models, so we opt for a model composition Markov Chain Monte Carlo algorithm (Madigan and York, 1995) that walks through the models with the highest posterior model probabilities. In the baseline specification we use a uniform model prior (each model has the same prior probability) and unit information g-prior (the prior that all regression coefficients equal zero has the same weight as one observation in the data), but we also use alternative priors in Appendix D.

Second, as a simple robustness check of our baseline BMA specification, we run a hybrid frequentist-Bayesian model. We employ variable selection based on BMA (specifically, we only include the variables with PIPs above 80%) and estimate the resulting model using OLS with clustered standard errors. We label this specification a "frequentist check" of the baseline BMA exercise. Third, we employ frequentist model averaging (FMA). Our implementation of FMA uses Mallows's criteria as weights, since they prove asymptotically optimal (Hansen, 2007). The problem is that, using a frequentist approach, we have no straightforward alternative to the model composition Markov Chain Monte Carlo algorithm, and it appears infeasible to estimate all 2^{50} potential models. We therefore follow the approach suggested by Amini and Parmeter (2012) and resort to orthogonalization of the covariate space.

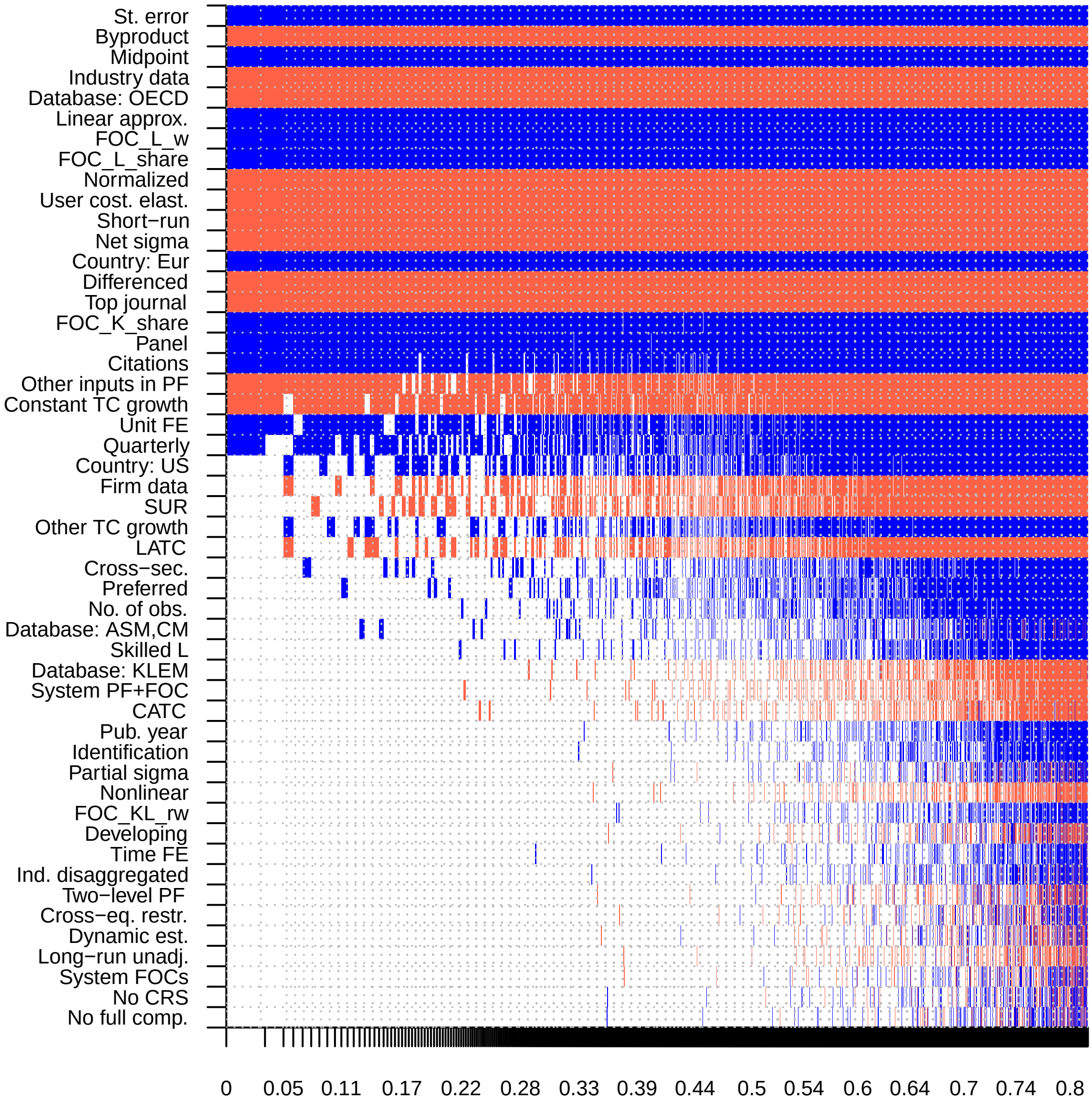
5.3 Results

Figure 7 illustrates our results. The vertical axis depicts explanatory variables sorted by their posterior inclusion probabilities; the horizontal axis shows individual regression models sorted by their posterior model probabilities. The blue color indicates that the corresponding variable appears in the model and the estimated parameter has a positive sign, while the red color indicates that the estimated parameter is negative. In total, 21 variables appear to drive the heterogeneity in the estimates, as their posterior inclusion probabilities surpass 80%. Table 3 provides numerical results for BMA and the frequentist check. In the frequentist check we only include the 21 variables with PIPs above 80%. Choosing a 50% threshold, for example, would result in including merely two more variables with virtually unchanged results for the remaining ones. Figure 8 plots posterior coefficient distributions of selected variables. The results of the FMA exercise are reported in Table D1 in Appendix D.

The first conclusion that we make based on these results is that our findings of publication bias presented in the previous section remain robust when we control for the context in which the elasticity is estimated. Indeed, the variable corresponding to publication bias, the standard error of the estimate, represents the single most effective variable in explaining the heterogeneity in the reported estimates of the elasticities of substitution (though several other variables also have posterior inclusion probabilities very close to 100% and are rounded to that number in Table 3). We observe that the publication bias detected by the correlation between estimates and standard errors is not driven by aspects of data and methods omitted from the univariate regression in Equation 13.

Data characteristics Several characteristics related to the data used in primary studies systematically affect the estimates of the elasticity. Our results suggest a mild upward trend in the reported elasticities, which increase on average by 0.004 each year. (The yearly change does not equal the

Figure 7: Model Inclusion in Bayesian Model Averaging



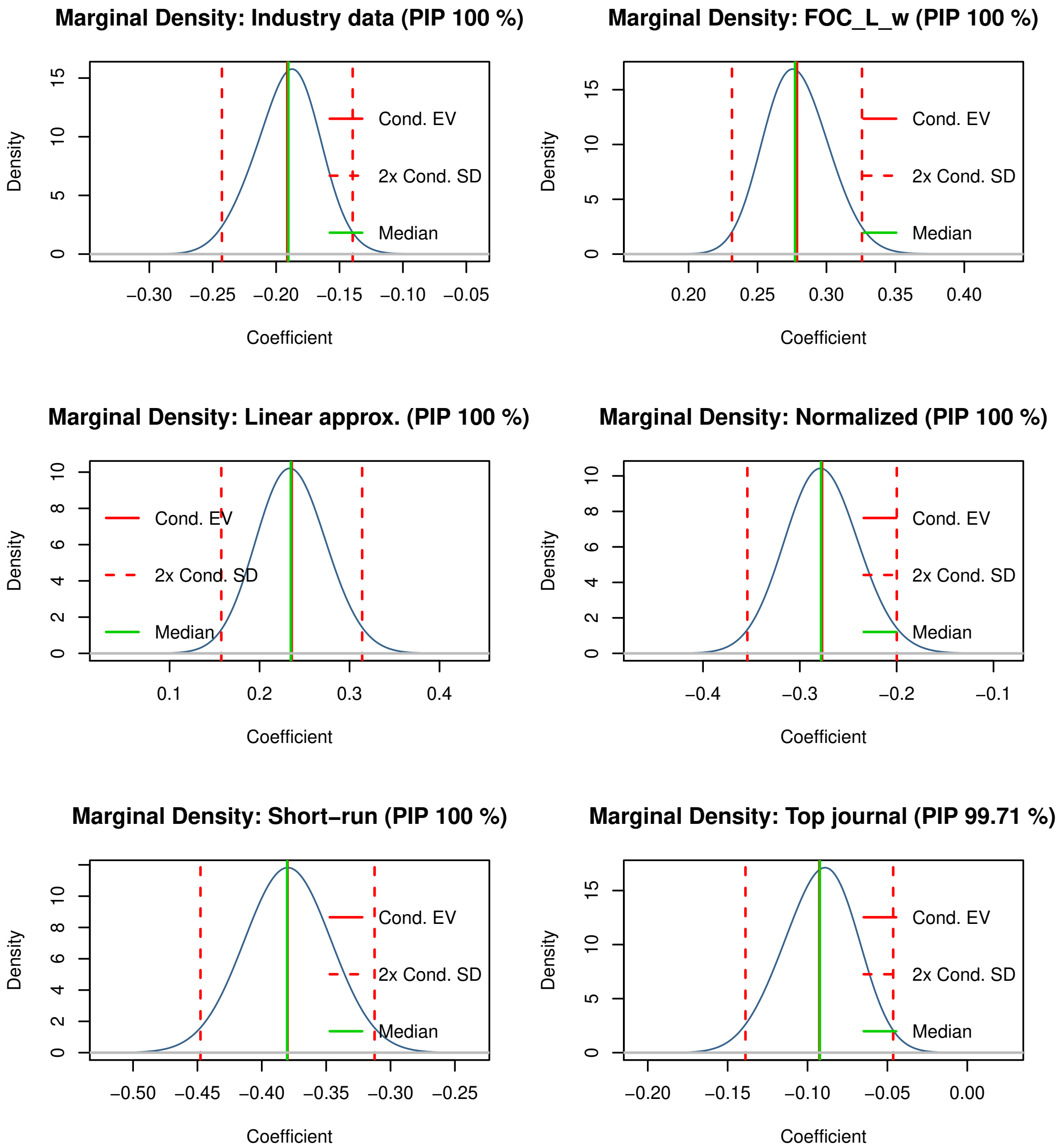
Note: The response variable is the estimate of the elasticity of capital-labor substitution. Columns denote individual models; variables are sorted by posterior inclusion probability in descending order. FOC = first-order condition. CATC = capital-augmenting technical change. LATC = labor-augmenting technical change. CRS = constant returns to scale. The horizontal axis denotes cumulative posterior model probabilities; only the 5,000 best models are shown. To ensure convergence we employ 100 million iterations and 50 million burn-ins. Blue color (darker in grayscale) = the variable is included and the estimated sign is positive. Red color (lighter in grayscale) = the variable is included and the estimated sign is negative. No color = the variable is not included in the model. Numerical results of the BMA exercise are reported in Table 3. A detailed description of all variables is available in Table C1 and Table C2.

Table 3: Why Do Estimates of the Elasticity of Substitution Differ?

Response variable:	Bayesian model averaging			Frequentist check		
Estimate of σ	Post. mean	Post. SD	PIP	Coef.	Std. er.	p-value
SE (publication bias)	0.614	0.038	1.000	0.633	0.042	0.000
Data characteristics						
No. of obs.	0.003	0.009	0.107			
Midpoint	0.118	0.022	1.000	0.123	0.036	0.001
Cross-sec.	0.009	0.023	0.160			
Panel	0.161	0.041	0.985	0.177	0.048	0.000
Quarterly	0.070	0.060	0.642			
Firm data	-0.033	0.049	0.363			
Industry data	-0.191	0.026	1.000	-0.191	0.064	0.003
Country: US	0.030	0.036	0.468			
Country: Eur	0.119	0.029	1.000	0.103	0.051	0.043
Developing country	0.000	0.003	0.014			
Database: ASM/CM	0.004	0.016	0.071			
Database: OECD	-0.277	0.039	1.000	-0.276	0.099	0.005
Database: KLEM	-0.003	0.014	0.042			
Disaggregated σ	0.000	0.003	0.012			
Specification						
System PF+FOC	-0.002	0.014	0.039			
System FOCs	0.000	0.003	0.008			
Nonlinear	-0.001	0.011	0.016			
Linear approx.	0.235	0.039	1.000	0.227	0.108	0.037
FOC_L_w	0.278	0.023	1.000	0.261	0.023	0.000
FOC_KL_rw	0.000	0.005	0.015			
FOC_K_share	0.230	0.064	0.993	0.212	0.253	0.402
FOC_L_share	0.209	0.038	1.000	0.204	0.064	0.001
Cross-equation restr.	0.000	0.004	0.010			
Normalized	-0.277	0.038	1.000	-0.289	0.066	0.000
Two-level PF	0.000	0.007	0.011			
Partial σ	0.001	0.012	0.017			
User cost elast.	-0.385	0.044	1.000	-0.368	0.061	0.000
Econometric approach						
Dynamic est.	0.000	0.003	0.009			
SUR	-0.027	0.041	0.348			
Identification	0.000	0.005	0.018			
Differenced	-0.111	0.025	1.000	-0.109	0.025	0.000
Time FE	0.000	0.006	0.013			
Unit FE	0.093	0.065	0.735			
Short-run σ	-0.380	0.034	1.000	-0.381	0.053	0.000
Long-run σ unadj.	0.000	0.002	0.009			
Production function components						
Other inputs in PF	-0.103	0.054	0.852	-0.128	0.070	0.068
CATC	-0.001	0.007	0.038			
LATC	-0.018	0.028	0.327			
Skilled L	0.006	0.029	0.061			
Constant TC growth	-0.078	0.040	0.844	-0.101	0.038	0.009
Other TC growth	0.029	0.045	0.332			
No CRS	0.000	0.002	0.008			
No full comp.	0.000	0.004	0.008			
Net σ	-0.376	0.048	1.000	-0.260	0.054	0.000
Publication characteristics						
Top journal	-0.092	0.023	0.998	-0.074	0.032	0.021
Pub. year	0.000	0.004	0.024			
Citations	0.033	0.014	0.916	0.037	0.018	0.040
Preferred est.	0.005	0.014	0.154			
Byproduct	-0.152	0.028	1.000	-0.143	0.075	0.059
Constant	0.059		1.000	0.071	0.143	0.619
Observations	3,186			3,186		

Note: σ = elasticity of capital-labor substitution, PIP = posterior inclusion probability. SD = standard deviation. FOC = first-order condition. CATC = capital-augmenting technical change. LATC = labor-augmenting technical change. CRS = constant returns to scale. The table shows unconditional moments for BMA. In the frequentist check we include only explanatory variables with PIP > 0.8. The standard errors in the frequentist check are clustered at the study level. A detailed description of all variables is available in Table C1 and Table C2.

Figure 8: Posterior Coefficient Distributions for Selected Variables



Note: FOC_L_w = 1 if the elasticity is estimated within the FOC for labor based on the wage rate. The figure depicts the densities of the regression parameters encountered in different regressions in which the corresponding variable is included (that is, the depicted mean and standard deviation are conditional moments, in contrast to those shown in Table 3). For example, the regression coefficient for Linear approximation is positive in all models, irrespective of specification. The most common value of the coefficient is 0.23.

regression coefficient because the variable is in logs; the precise definition is available in Table C1.) The finding resonates with Cantore et al. (2017), who point to a similar time trend. But the upward trend constitutes a poor reason to resurrect the Cobb-Douglas specification, because at this pace the specification will become consistent with the literature in about 175 years. Next, estimates of the elasticity that rely on industry-level data tend to be significantly smaller than those using country- or state-level data, a result corroborating the prima facie pattern in the literature shown in Figure 4(d) in section 3. Nerlove (1967) suggests that using country-level data, implicitly assuming the same technological levels across countries, can lead to an upward bias in the estimated elasticity.

Moreover, Chirinko (2008) discusses several drawbacks of aggregate data in comparison to firm- or industry-level data, including limited variation available for identification.

Concerning data dimension, our results suggest that panel data tend to yield larger estimates of the elasticity than time series data. The other prima facie pattern in the literature, the systematic and large difference between the results of time series and cross-section studies shown in Figure 4(c), breaks apart when controlling for other variables in BMA (the variable is statistically significant in FMA, but the estimated coefficient is small). Similarly, our results do not suggest that much of the differences between estimates can be explained by differences in data frequency.

Another prima facie data pattern, the importance of results aggregation presented in Figure 4(b), disappears in the BMA analysis. Elasticities computed for individual industries do not differ systematically from elasticities computed for the entire economy. Nevertheless, that is not to say that the elasticity does not vary across industries; we will return to this issue in Appendix D. Concerning cross-country differences, the reported elasticities tend to be larger in Europe than in other regions, but only by 0.1. Finally, our results suggest that datasets coming from the OECD database are associated with substantially smaller elasticities compared to all other data sources.

Specification A stylized fact in the literature on capital-labor substitution has it that estimations based on the first-order condition for labor deliver larger elasticities than estimations based on the first-order condition for capital; see Figure 4(a) in section 3. The BMA analysis corroborates this stylized fact and elaborates on it: when a system of FOCs is used, the results tend to be close to those derived from the FOC for capital. Omitting information from the FOC for capital, in contrast, exaggerates the reported elasticity by 0.2 or more. The FOC for capital thus seems to be more important for proper identification of the elasticity than the FOC for labor. The elasticity also becomes inflated by 0.2 when a linear approximation of the production function (using either the Kmenta or translog approach) is employed. As pointed out by Thursby and Lovell (1978) and León-Ledesma et al. (2010), linear approximations of the production function tend to be biased towards $\hat{\sigma} = 1$, as an elasticity of one usually serves as the initial point of expansion.

On the other hand, normalization of the production function systematically reduces the estimated elasticity by allowing for the identification of technological change parameters. Finally, if the FOC for capital is estimated in an inverse form (user cost elasticity of capital), the estimates tend to be on average much smaller. These results are robust across all the estimations we run: BMA, FMA, and the frequentist check. A similarly robust result is that the mean implied elasticity is 0.3 when made conditional on three aspects: (i) no publication bias, (ii) no country-level input data, and (iii) not ignoring information from the FOC for capital. We will expand and provide more details on the computation of the implied elasticity at the end of this section.

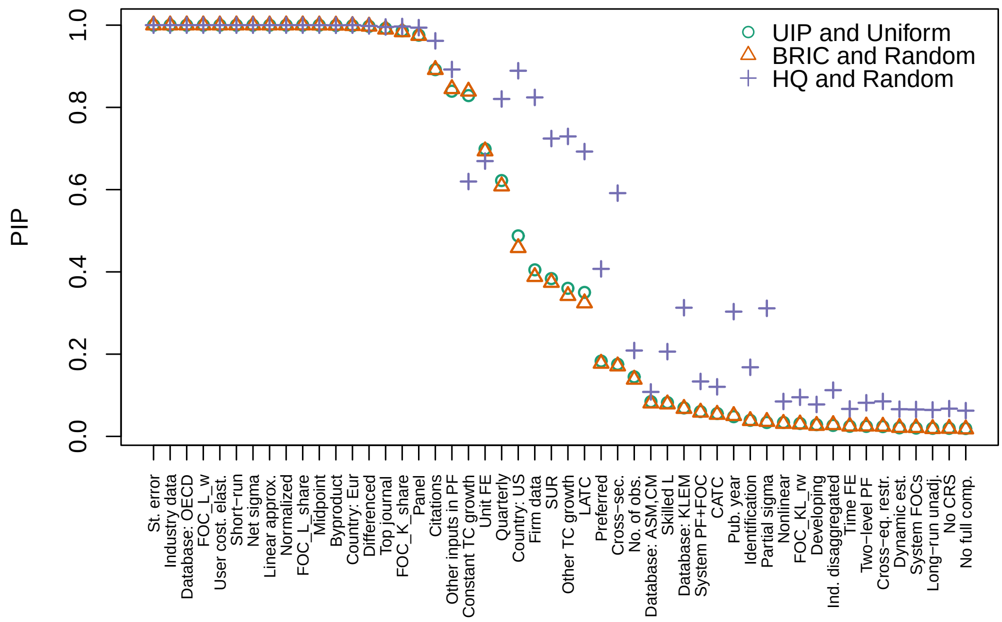
Econometric approach We find little evidence that the econometric approach used in primary studies is responsible for systematic differences in the reported elasticities. Naturally, short-run elasticities are smaller than long-run ones: estimations in differences tend to deliver elasticities that are smaller by 0.1; explicitly short-run estimations tend to deliver elasticities smaller by 0.4. Adjusted and unadjusted long-run estimates do not differ much from each other.

Production function components The results suggest that assumptions regarding technical change have little systematic effect on the resulting elasticities of substitution. Allowing for capital- or labor-augmenting technological change brings, on average, elasticities similar to the case when Hicks-neutral technological change is assumed. Allowing for constant growth in technological change (in comparison to no growth) decreases the estimate, but only by a small margin. The apparent irrelevance of assumptions on technological change for the estimation of the elasticity of substitution contrasts with Antras (2004), who argues that Hicks-neutral technological change biases the results towards the Cobb-Douglas specification. The irrelevance finding holds for both BMA and FMA and regardless of whether we include labor- and capital-augmenting technological change as separate dummies or jointly in one dummy. Including other inputs in the production function aside from labor and capital has a negative effect on the resulting size of the elasticity. When the elasticity is estimated in the net form, it tends to be smaller by 0.4 on average, but very few studies pursue this approach.

Publication characteristics Out of the five variables grouped together as publication characteristics, three are systematically associated with the magnitude of the reported elasticity. First, compared to other outlets, the top five journals in economics tend to publish slightly smaller elasticities. Second, studies that provide larger elasticities tend to receive more citations—potentially, such studies are more useful to researchers trying to justify the use of the Cobb-Douglas production function in their model, but it could also mean that studies reporting larger estimates are of higher quality. Third, the reported elasticity tends to be smaller if it does not represent the central focus of the study but is merely a byproduct of a different exercise. One can interpret the finding as further indirect evidence of publication bias against small estimates, or, alternatively, as evidence that more thorough examinations yield larger estimates.

Aside from our baseline BMA, FMA, and frequentist check, we run several sensitivity analyses with respect to different subsamples of data, control variables, priors, and weighting schemes. Regarding priors, Figure 9 shows that the implied relative importance of the variables changes little when different priors are used for BMA. In Appendix D, we also run BMA on weighted data: first, data are weighted by the inverse of the number of estimates reported by each study so that each study has the same weight (Figure D1); second, data are weighted by the inverse of the standard error (Figure D2). Our key results continue to hold in these specifications.

Figure 9: Posterior Inclusion Probabilities Across Different Prior Settings



Note: UIP (unit information prior) and Uniform model prior = priors according to Eicher et al. (2011). BRIC and Random = the benchmark g-prior for parameters with the beta-binomial model prior for the model space, which means that each model size has equal prior probability. HQ prior asymptotically mimics the Hannan-Quinn criterion. PIP = posterior inclusion probability.

5.4 Economic Significance and Implied Elasticity

We close the analysis with a discussion of (i) the economic significance of the variables identified as important by BMA and FMA, and (ii) the mean elasticity of substitution implied by the literature after taking into account the pattern that some data and method choices create in the reported estimates. Economic significance is explored in Table 4, which shows the effect on the reported elasticity when we increase the value of the corresponding variable by one standard deviation (the left-hand panel) and from minimum to maximum (the right-hand panel). Increasing from minimum to maximum perhaps makes more sense for dummy variables, while for continuous variables, such as the midpoint of data, the one-standard-deviation change is typically more informative. In the second and fourth columns, the table also casts the effects as percentages of the "best-practice" estimate implied by the literature, which we discuss below. It is apparent from the table that the variables with the largest effect on the elasticity are the standard error (publication bias), industry-level data (disaggregation), the FOC for labor (ignoring the FOC for capital), normalization of the production function, and of course the assumption of short-run or net elasticity. Changes in these variables can easily alter the resulting elasticity by 50% or more.

Table 4: Economic Significance of Key Variables

	One-std.-dev. change		Maximum change	
	Effect on σ	% of best practice	Effect on σ	% of best practice
Standard error	0.117	39%	0.461	154%
Byproduct	-0.047	-16%	-0.152	-51%
Midpoint	0.056	19%	0.588	196%
Industry data	-0.095	-32%	-0.191	-64%
Database: OECD	-0.069	-23%	-0.277	-92%
Linear approx.	0.062	21%	0.235	78%
FOC_L_w	0.132	44%	0.278	93%
Normalized	-0.061	-20%	-0.277	-92%
Short-run σ	-0.083	-28%	-0.380	-127%
Net σ	-0.059	-20%	-0.376	-125%

Note: The table shows *ceteris paribus* changes in the reported elasticities implied by changes in the variables that reflect the context in which researchers obtain their estimates. For example, increasing the estimate's standard error by one standard deviation is associated with an increase in the estimated elasticity of 0.117, more than a third of the size of the best-practice estimate (the one conditional on ideal data, method, and publication characteristics, as described in Table 5). Increasing the standard error from the sample minimum to the sample maximum is associated with an increase in the estimated elasticity of 0.461, more than one and a half times the best-practice estimate. A detailed description of the variables is available in Table C1 and Table C2 in Appendix C.

The mean implied elasticity is explored in Table 5. In essence, we create a synthetic study in which we use all the reported estimates but give different weights to different aspects of data, methodology, and publication. We have already noted that the implied elasticity is 0.3 when we hold three preferences: the estimate should be conditional on the absence of publication bias, the use of disaggregated (not country-level) data, and the use of information from the first-order condition for capital. Next, we augment the list of preferences to construct a best-practice estimate. For the computation we use the results of FMA because, unlike BMA, it allows us to construct confidence intervals around the implied elasticities (linear combinations of FMA coefficients and the chosen values for each variable). We compute fitted values of the elasticity by plugging in sample maxima for variables reflecting best practice in the literature, sample minima for variables reflecting departures from best practice, and sample means for variables where we cannot determine best practice.

We prefer large studies using newer data, so we plug in sample maxima for the number of observations and the midpoint of data. We prefer a system of production function together with FOCs for both capital and labor, tied with normalization and cross-equation restrictions. We also prefer the use of factor-augmenting technological change and joint estimation of equations by Zellner's method instead of OLS. As for the publication characteristics, we prefer studies that are highly cited and published in top journals. In contrast, we do not prefer linear approximation, byproduct estimates, elasticities that are supposed to be long-run but are not properly adjusted, and partial elasticities: we plug in zero for these variables. We do not have any strong opinion on the various sources of data or data dimension (whether time series or cross-sectional studies should be used, what data frequency should be employed). Thus, next to the central "best-practice" estimate we generate multiple estimates for these data and method choices. We also show implied elasticities for aggregated, country-level data, often used in the literature, and for short-run elasticity, net elasticity, and the use of a system of FOCs without a production function.

Table 5: Results from a Synthetic Study

	Implied elasticity	95% confidence interval
Best practice	0.30	(-0.01, 0.60)
Short-run	-0.11	(-0.38, 0.15)
Net σ	-0.02	(-0.30, 0.25)
Country-level data	0.50	(0.18, 0.81)
Quarterly data	0.42	(0.08, 0.76)
Time series	0.25	(-0.10, 0.60)
Cross-sections	0.32	(0.07, 0.56)
System of FOCs	0.35	(0.07, 0.64)

Note: The table shows mean estimates of the elasticity of substitution conditional on data, method, and publication characteristics. The exercise is akin to a synthetic study that uses all information reported in the literature but puts more weight on selected aspects of study design. The result in the first column is conditional on our definition of best practice (see the main text for details). The remaining rows change one aspect in the definition of best practice: for example the second row shows the result for short-run instead of long-run estimates.

The results, shown in Table 5, illustrate the high degree of uncertainty that such an exercise entails: the 95% confidence intervals for all estimates are approximately 0.6 wide. Our central estimate is still 0.3, which means that other aspects of best practice (on top of the three preferences made in the beginning) cancel each other out—even though now the estimate becomes barely statistically significantly different from zero at the 5% level. But even such a conservative estimation rejects the Cobb-Douglas specification in all cases. The implied short-run and net elasticities are close to zero. When one prefers quarterly data instead of showing equal treatment to estimates derived from data of different frequencies, the implied estimate increases to 0.4. A preference for time series data, cross-sectional data, or a system of FOCs without a production function would result in a smaller change in the elasticity. Even a preference for country-level data would only take the implied estimate to 0.5, with the upper bound of the 95% confidence interval at 0.8, making the result safely inconsistent with the Cobb-Douglas specification.[4]

[4] Different researchers may have different opinions on what constitutes best practice: even though we prefer to use disaggregated data in general, central bankers' structural models are mostly calibrated on country-level, quarterly data, so these dimensions may be more relevant for them. Even in the case of combining quarterly data and country-level data, the best-practice estimate yields a maximum of 0.62. Similarly, one may be interested in implied elasticities for different countries. For instance, the results for the US and developed European countries are slightly higher than those for other countries, with a baseline implied elasticity of 0.42 and 0.48. Deeper examination of cross-country differences in substitution elasticity is a topic of our current follow-up project.

6. Concluding Remarks

The Cobb-Douglas production function contradicts the data. This is the result we obtain after analyzing 3,186 estimates of the capital-labor substitution elasticity reported in 121 published studies. When we give the same weight to all the different approaches used to identify the elasticity, we find that the value most representative of the literature is 0.5, tightly estimated with the upper bound of the 95% confidence interval at 0.6. The representative value corresponds to the mean reported elasticity corrected for publication bias, a phenomenon that has not previously been addressed in the vast literature on the elasticity of substitution. The representative estimate further shrinks to 0.3 when one imposes the restrictions that identification must come from industry-level instead of aggregated, country-level data and that information from the first-order condition for capital must be considered instead of ignored. The representative estimate stays at 0.3 when we control for 71 aspects of study design and select a best-practice value for each aspect (plugging in mean values where no reasonable choice can be made). Such best-practice elasticity is imprecisely estimated, with the upper bound of the 95% confidence interval still at 0.6. Other researchers will have different opinions on what constitutes best practice. But no matter the preferences, after acknowledging publication bias, the Cobb-Douglas production function with the elasticity at 1 becomes indefensible in the light of empirical evidence.

We are not the first to highlight the disconnect between the Cobb-Douglas specification commonly used in macroeconomic models and the empirical literature estimating the elasticity of substitution. Chirinko (2008) and Knoblach et al. (2019) provide useful surveys of portions of the literature, and both studies suggest that the Cobb-Douglas production function is not backed by the available evidence. We argue that after controlling for publication bias and model uncertainty the true value of the elasticity decreases even more and the case against Cobb-Douglas strengthens.

Three caveats to the precise value of our central estimate, 0.3, are in order. First, the elasticities that we collect are unlikely to be independent, because they are frequently derived from the same or similar datasets. We partially address this problem by clustering standard errors at both the study and country level when controlling for publication bias and additionally compute wild bootstrap confidence intervals. Second, the value of 0.3 is a mean estimate and does not necessarily fit all situations and calibrations. While we do not find much evidence of systematic differences in the elasticity across countries and industries, in a companion project we are currently working on a more detailed examination of structural determinants of the elasticity. Third, we do our best to include all published studies estimating the elasticity of substitution, but we still might have missed some. Such an omission will not affect our results much as long as it remains random. We experimented with randomly omitting 50% of our data set, and the main findings continue to hold in such simulations.

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Appendix A: Furukawa's Method for Addressing Selective Reporting

Furukawa (2019) proposes the so-called stem-based correction method, which relies on the most precise studies, corresponding to the stem of the funnel plot. The method is nonparametric and fully data-dependent, and requires weaker assumptions for the underlying distribution of true effects and the publication selection process than other methods. Publication selection can be a function of the size of the estimates, their significance, or both at the same time, as imprecise null results are less likely to be published. By focusing on the n most precise estimates, Furukawa (2019) is able to account for various publication selection processes. The method extends the approach by Stanley et al. (2010), who suggest using 10% of the most precise estimates. Instead of selecting an arbitrary number of the most precise estimates, Furukawa (2019) suggests a formal method to calculate the optimal number n of the most precise studies to include by minimizing the mean squared error:

$$\min_n MSE(n) = Bias^2(n) + Var(n). \tag{A1}$$

With more studies used, the squared bias term increases, as less precise studies suffer from more bias, but the variance term decreases, as more information increases efficiency. An empirical analog of the bias term is estimated nonparametrically using two algorithms. The inner algorithm computes the bias-corrected mean given an assumed value of squared precision, and the outer algorithm computes the implied variance and ensures that it is consistent with its assumed value. The inner algorithm ranks and indexes studies in ascending order according to their standard error, se, and for each $n = 2, ..., N$ calculates the relevant bias squared and variance, given the assumed value of se_0:

$$\tilde{Bias}^2(n) = \frac{\sum_{i=2}^{n}\sum_{j\neq i}^{n} w_i w_j \beta_i \beta_j}{\sum_{i=2}^{n}\sum_{j\neq i}^{n} w_i w_j} - 2\beta_1 \frac{\sum_{i=2}^{n} w_i \beta_i}{\sum_{i=2}^{n} w_i}, \tag{A2}$$

$$Var(n) = \sum_{i=1}^{n} w_i, \tag{A3}$$

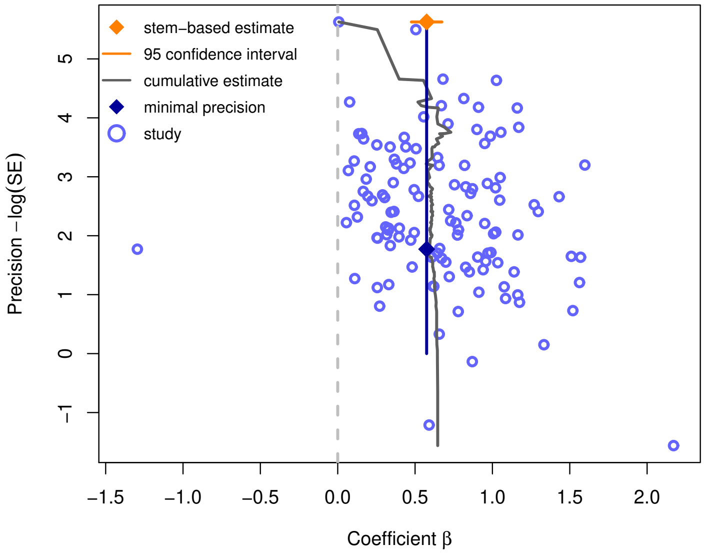
where $w_i = \frac{1}{se_i^2 + se_0^2}$. The optimal number of studies included is given by Equation A1. The stem-based corrected estimate follows:

$$\hat{b}_{stem} = \frac{\sum_{i=1}^{n_{stem}} w_i \beta_i}{\sum_{i=1}^{n_{stem}} w_i}. \tag{A4}$$

The outer algorithm then searches over se_0^2 so that the implied variance is consistent.

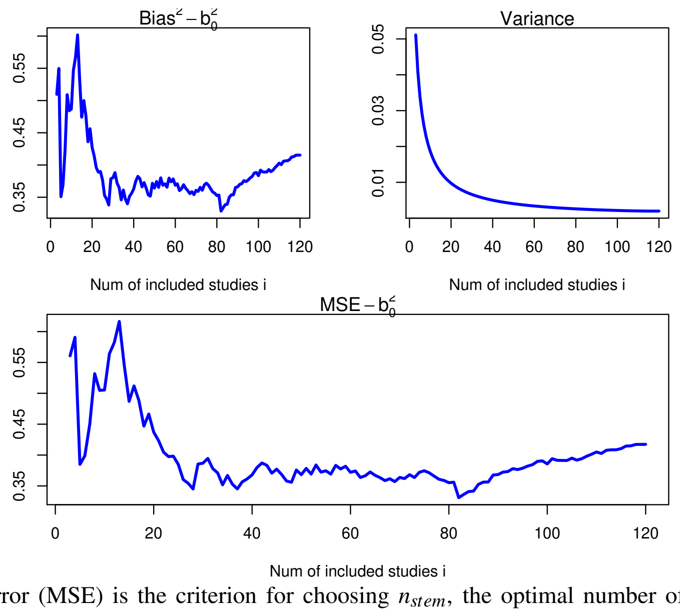
The stem-based method applied to the elasticity of substitution yields the following results: the mean underlying elasticity corrected for publication bias is 0.57 with a standard error of 0.05. Overall, 77% of the total information in the data is utilized, and the 83 most precise studies (out of 121) are included. Because the stem-based method uses study-level estimates (as preferred by Furukawa), we select median values from each study. Figure A1 visualizes the stem-based bias correction method. Figure A2 visualizes the bias-variance trade-off in order to minimize the mean squared error. When all estimates instead of median estimates are used, the mean corrected elasticity is similar, 0.55, but the standard error increases to 0.21.

Figure A1: A Graphical Illustration of Furukawa's Technique



Note: The orange (lighter in grayscale) diamond at the top corresponds to the stem-based estimate of the mean elasticity corrected for publication bias, with the orange line indicating the corresponding 95% confidence interval. The gray (lighter in grayscale) line denotes the estimate under various $n_{stem} \in 1, ..., N$. The blue (darker in grayscale) diamond indicates the minimum precision level that defines the "stem" of the funnel.

Figure A2: The Trade-Off Between Bias and Variance



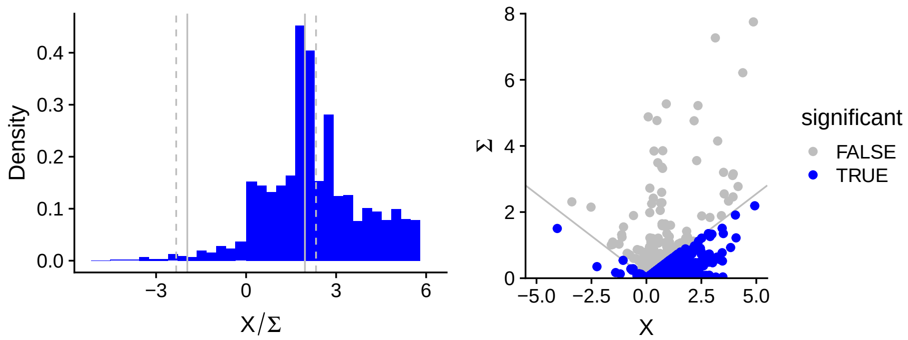
Note: The mean squared error (MSE) is the criterion for choosing n_{stem}, the optimal number of studies to include in the stem-based estimator. The relevant components of MSE—bias and variance—are plotted.

Appendix B: Andrews and Kasy's Method for Addressing Selective Reporting

Andrews and Kasy (2019) introduce two approaches for the identification of publication selection: the first one based on data from replication studies and the second one tailored for meta-analysis. They show that the meta-analysis approach delivers results similar to the approach based on replications. In the absence of publication bias, the distribution of the estimates from imprecise studies can be written as the distribution for precise studies plus noise; deviations from this form identify conditional publication probabilities. Andrews and Kasy (2019) identify publication probability similarly to Hedges (1992) using maximum likelihood: the conditional publication probability, $p(\cdot)$, is a step function with jumps at conventional critical values of the p-value.

When applied to our data, the method by Andrews and Kasy (2019) yields the following results. The bias-corrected estimate is 0.43 with a standard error of 0.017. We impose a cut-off at zero, that is, we compare the publication probability of negative vs. positive estimates regardless of their significance. (Allowing for other jumps in publication probability would yield even smaller estimates of the mean elasticity corrected for publication bias.) Our results also suggest that positive estimates are six times more likely to be selected for publication than negative estimates (Table B1). In the case of the elasticity of substitution, publication selection based on statistical significance is apparently less pronounced than selection based on the sign of the estimate, as suggested by the right panel of Figure B1.

Figure B1: A Graphical Illustration of Andrews and Kasy's (2019) Estimator



Note: The solid gray lines mark t-statistic equal to 1.96 in absolute value; the dashed gray line marks t-statistic equal to 2.33 in absolute value. We observe a jump at t-statistic equal to zero and then also jumps at conventional significance levels. The right-hand figure plots estimates X and their standard errors Σ; the gray line marks 1.96 in absolute value. Even though we observe discontinuity at the t-statistic corresponding to the 5% significance level, the right panel shows publication selection based on significance is not absolute, as some insignificant estimates (gray points) are reported.

Table B1: Results of Andrews and Kasy's (2019) Estimator

	$\bar{\theta}$	$\bar{\tau}$	DF	β_p
Estimate	0.430	0.489	12.809	0.158
Standard error	0.017	0.012	0.707	0.019

Note: $\bar{\theta}$ denotes the bias-corrected mean effect, $\bar{\tau}$ is a scale parameter, *DF* are degrees of freedom. β_p is a publication probability measured relative to the omitted category, in our case positive estimates. An estimate of 0.158 therefore implies that negative results are 15.8% as likely to be published as positive ones.

Appendix C: Description of Variables

Table C1: Definitions and Summary Statistics of Explanatory Variables

Variable	Description	Mean	Std. dev.
Data characteristics			
No. of obs.	The logarithm of the number of observations used in the regression.	4.28	1.51
Midpoint	The logarithm of the mean year of the data used minus the earliest mean year in our data.	4.71	0.48
Cross-sec.	= 1 if cross-sectional data are used (reference category: time series).	0.33	0.47
Panel	= 1 if panel data are used (reference category: time series).	0.14	0.35
Quarterly	= 1 if the data frequency is quarterly (reference category: annual).	0.11	0.31
Industry data	= 1 if industry-/sector-level data are used as input data (reference category: country-/state-level data).	0.43	0.50
Firm data	= 1 if firm-level data are used as input data (reference category: country-/state-level data).	0.12	0.32
Country: US	= 1 if the estimate is for the US.	0.58	0.49
Country: Eur	= 1 if the estimate is for a developed European country.	0.17	0.37
Developing	= 2 if the estimate is for a developing country; = 1 if the estimate is a common estimate for a collection of developed and developing countries (reference category: developed countries).	0.22	0.54
Database: OECD	= 1 if the data come from the OECD database.	0.07	0.25
Database: KLEM	= 1 if the data come from the Jorgenson KLEM dataset.	0.15	0.36
Database: ASMCM	= 1 if the data come from the Annual Survey of Manufacturers and/or Census of Manufacturers.	0.14	0.35
Disaggregated σ	= 1 if the elasticity is estimated on a disaggregated level (industry-specific elasticity).	0.52	0.50
Specification			
System PF-FOC	= 1 if the elasticity is estimated within a system of CES with FOC(s) or with cost share functions.	0.06	0.23
System FOCs	= 1 if the elasticity is estimated within a system of FOCs.	0.05	0.23
Nonlinear	= 1 if the elasticity is estimated within the CES directly via nonlinear methods.	0.04	0.20
Linear approx.	= 1 if the elasticity is estimated via Taylor series expansion (Kmenta approach or translog approach).	0.07	0.26
FOC_L_w	= 1 if the elasticity is estimated within the FOC for labor based on the wage rate (reference category: FOC for capital based on the rental rate of capital).	0.33	0.47
FOC_KL_rw	= 1 if the elasticity is estimated within the FOC of K/L based on w/r (reference category: FOC for capital based on the rental rate of capital).	0.18	0.39
FOC_K_share	= 1 if the elasticity is estimated within the FOC for capital based on the capital share (reference category: FOC for capital based on the rental rate of capital).	0.03	0.16
FOC_L_share	= 1 if the elasticity is estimated within the FOC for labor based on the labor share (reference category: FOC for capital based on the rental rate of capital).	0.04	0.19
User cost elast.	= 1 if the user cost of capital elasticity is estimated.	0.17	0.38
Cross-equation rest.	= 1 if cross-equation restrictions are employed when using system estimation.	0.08	0.28
Normalized	= 1 if normalization is applied to the CES.	0.05	0.22
Two-level PF	= 1 if a two-level CES function is estimated (due to more than two factors of production).	0.03	0.18
Partial σ	= 1 if some form of partial elasticity is used (Allen-Uzawa, Hicks-Allen, Morishima).	0.06	0.24
Econometric approach			
Dynamic est.	= 1 if dynamic methods are used for estimation (VAR, a distributed lag model or error correction model; reference category: OLS).	0.24	0.42
SUR	= 1 if a system of seemingly unrelated regressions is used (Zellner's estimation; reference category: OLS).	0.11	0.31
Differenced	= 1 if the coefficient is taken from a regression in first differences or log differences.	0.23	0.42
Time FE	= 1 if time-fixed effects are used for estimation.	0.06	0.24
Unit FE	= 1 if unit-fixed effects are used for estimation.	0.04	0.20
Identification	= 1 if instrumental variables are used for identification.	0.13	0.34
Short-run σ	= 1 if the coefficient is taken from an explicit short-run specification (reference category: explicit long-run specification—cointegration, low-pass filter, interval-difference model).	0.05	0.22
Long-run σ unadj.	= 1 if the coefficient is meant to be long-run but the specification is not adjusted accordingly (reference category: explicit long-run specification).	0.68	0.47
Production function components			
Other inputs in PF	= 1 if the production function includes other inputs such as energy, materials, and human capital.	0.13	0.34
LATC	= 1 if the production function includes labor-augmenting technological change, i.e., Harrod-neutral technological change (reference category: Hicks-neutral technological change).	0.29	0.63
CATC	= 1 if the production function includes capital-augmenting technological change, i.e., Solow-neutral technological change (reference category: Hicks-neutral technological change).	0.26	0.57

Continued on next page

Table C2: Definitions and Summary Statistics of Explanatory Variables (cont.)

Variable	Description	Mean	Std. dev.
Skilled L	= 1 if the production function distinguishes between skilled and unskilled labor.	0.02	0.13
Constant TC growth	= 1 if technological change is modeled with constant growth rates (reference category: no growth of technology).	0.30	0.46
Other TC growth	= 1 if technological change is modeled with nonconstant growth rates, e.g., logarithmic, linear (reference category: no growth of technology).	0.10	0.31
No CRS	= 1 if the authors assume nonconstant returns to scale.	0.09	0.36
No full comp.	= 1 if the authors do not assume factor markets to be perfectly competitive.	0.04	0.19
Net σ	= 1 if net elasticity is estimated (reference category: gross elasticity).	0.02	0.16
External info			
Top journal	= 1 if the study is published in a top five journal in economics.	0.31	0.46
Pub. year	The logarithm of the year when the first draft of the study appeared in Google Scholar minus the year when the first study on elasticity of substitution was written.	3.25	0.88
Impact	The recursive discounted RePEc impact factor of the outlet.	0.96	1.07
Citations	The logarithm of the number of per-year citations of the study since its first appearance on Google Scholar.	1.47	0.96
Preferred est.	= 1 if the estimate is preferred by authors or is explicitly considered to be better; -1 if it is considered inferior.	-0.04	0.47
Byproduct	= 1 if estimation of the elasticity is not the central focus of the paper but only a byproduct; = 0 if it is the central focus; = 0.5 if it is one of multiple main aims.	0.20	0.31
Measurement of variables			
y: index	= 1 if the input data for total output are in an index form.	0.03	0.18
y: other	= 1 if the input data for total output are measured differently than in gross domestic product or total value added (reference category: GDP, value added).	0.07	0.26
Labor-related			
Quality adj.	= 1 if the input data for labor incomes data are quality-adjusted.	0.22	0.41
Self empl.	= 1 if the input data for labor incomes data are adjusted for the income of self-employed people.	0.18	0.39
w: nominal	= 1 if the input data for the wage rate are nominal (reference category: the wage rate is in real terms).	0.09	0.29
w: direct	= 1 if the input data for the wage rate are measured directly (the wage rate calculated as total wages divided by the total number of employees).	0.14	0.36
L: hours	= 1 if the input data for labor are measured in hours.	0.25	0.44
L: years	= 1 if the input data for labor are measured in years.	0.07	0.25
L: FTE workers	= 1 if the input data for labor are measured by the full-time equivalent number of workers.	0.07	0.25
L: force	= 1 if the number of workers is measured as the total number of people in the labor force.	0.04	0.20
Capital-related			
Capacity adj.	= 1 if the authors control for capacity utilization in the regression.	0.09	0.28
r: quasi	= 1 if the input data for the rental rate of capital are measured as the quasi-rent, i.e., total output minus total wages divided by the total capital stock (reference category: measured as the user cost of capital, Hall-Jorgenson formula).	0.24	0.43
r: nominal	= 1 if the input data for the rental rate of capital are expressed in nominal terms.	0.01	0.09
K: IT	= 1 if IT capital is used only.	0.02	0.13
K: equipment	= 1 if the measure of equipment capital is used only.	0.07	0.26
K: structures	= 1 if the measure of structures, land or plant is used only.	0.04	0.17
K: residential	= 1 if the measure of capital includes the residential capital stock.	0.07	0.25
K: services	= 1 if capital is measured as service flow.	0.13	0.33
K: perpetual	= 1 if the input data for capital are measured via the perpetual inventory method.	0.36	0.48
K: index	= 1 if the input data for capital are expressed in an index form.	0.17	0.37
Industry-related			
Primary ind.	= 1 if the elasticity is estimated for the primary sector.	0.02	0.14
Secondary ind.	= 1 if the elasticity is estimated for the secondary sector.	0.62	0.49
Tertiary ind.	= 1 if the elasticity is estimated for the tertiary sector.	0.03	0.18
Materials	= 1 if the elasticity is estimated for the 2-digit industry in the "Materials" category of the GICS industry classification.	0.25	0.43
Industrials	= 1 if the elasticity is estimated for the 2-digit industry in the "Industrials" category of the GICS industry classification.	0.09	0.29
Consumer	= 1 if the elasticity is estimated for the 2-digit industry in the "Consumer goods" category of the GICS industry classification.	0.14	0.34

Note: Collected from published studies estimating the elasticity of substitution between capital and labor. When dummy variables form groups, we mention the reference category.

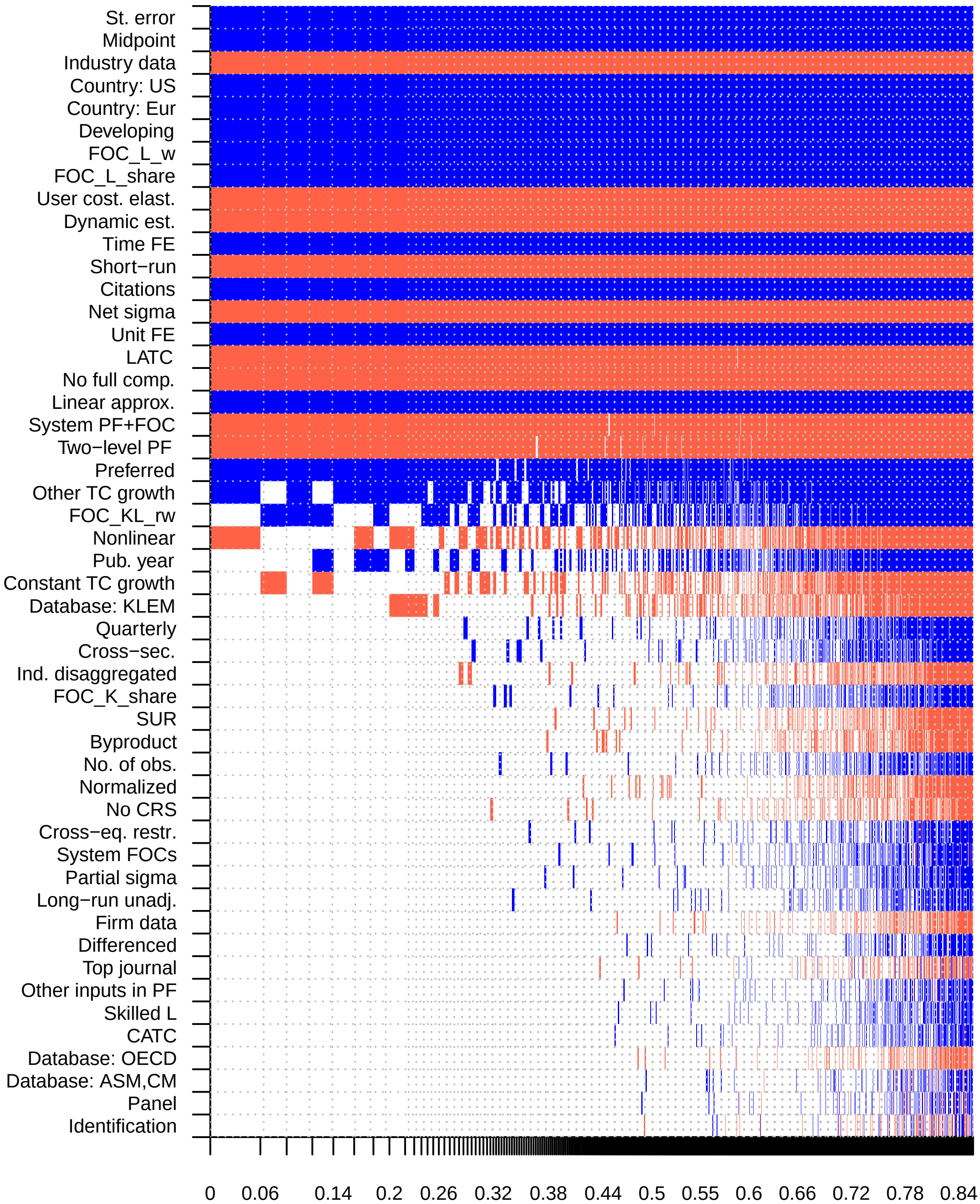
Appendix D: Robustness Checks

Table D1: Results of Frequentist Model Averaging

	Coef.	Std. er.	*p*-value
Standard error	0.557	0.042	0.000
Data characteristics			
No. of obs.	0.011	0.012	0.326
Midpoint	0.103	0.022	0.000
Cross-sec.	0.069	0.029	0.016
Panel	0.193	0.042	0.000
Quarterly	0.135	0.042	0.001
Firm data	-0.160	0.040	0.000
Industry data	-0.198	0.026	0.000
Country: US	0.121	0.031	0.000
Country: Eur	0.180	0.030	0.000
Developing	0.019	0.019	0.333
Database: ASM,CM	-0.031	0.037	0.402
Database: OECD	-0.301	0.044	0.000
Database: KLEM	-0.092	0.046	0.047
Disaggregated σ	0.043	0.024	0.077
Specification			
System PF+FOC	-0.111	0.059	0.061
System FOCs	-0.057	0.050	0.258
Nonlinear	-0.016	0.061	0.796
Linear approx.	0.268	0.050	0.000
FOC_L_w	0.324	0.032	0.000
FOC_KL_rw	0.007	0.032	0.832
FOC_K_share	0.226	0.063	0.000
FOC_L_share	0.251	0.048	0.000
Cross-eq. restr.	0.071	0.048	0.140
Normalized	-0.248	0.051	0.000
Two-level PF	-0.023	0.070	0.743
Partial sigma	0.130	0.055	0.018
User cost. elast.	-0.373	0.042	0.000
Econometric approach			
Dynamic est.	-0.005	0.029	0.854
SUR	-0.105	0.032	0.001
Identification	0.046	0.026	0.077
Differenced	-0.096	0.027	0.000
Time FE	-0.009	0.040	0.830
Unit FE	0.067	0.043	0.116
Short-run	-0.410	0.040	0.000
Long-run unadj.	-0.011	0.026	0.681
Production function components			
Other inputs in PF	-0.137	0.044	0.002
CATC	-0.003	0.026	0.904
LATC	-0.041	0.024	0.088
Skilled L	0.076	0.059	0.199
Constant TC growth	-0.032	0.025	0.191
Other TC growth	0.108	0.035	0.002
No CRS	-0.003	0.022	0.905
No full comp.	-0.022	0.042	0.598
Net sigma	-0.320	0.056	0.000
Publication characteristics			
Top journal	-0.085	0.025	0.001
Pub. year	0.032	0.015	0.038
Citations	0.037	0.011	0.001
Preferred	0.027	0.016	0.093
Byproduct	-0.130	0.032	0.000
(Intercept)	-0.123	0.130	0.342
Observations	3,186		

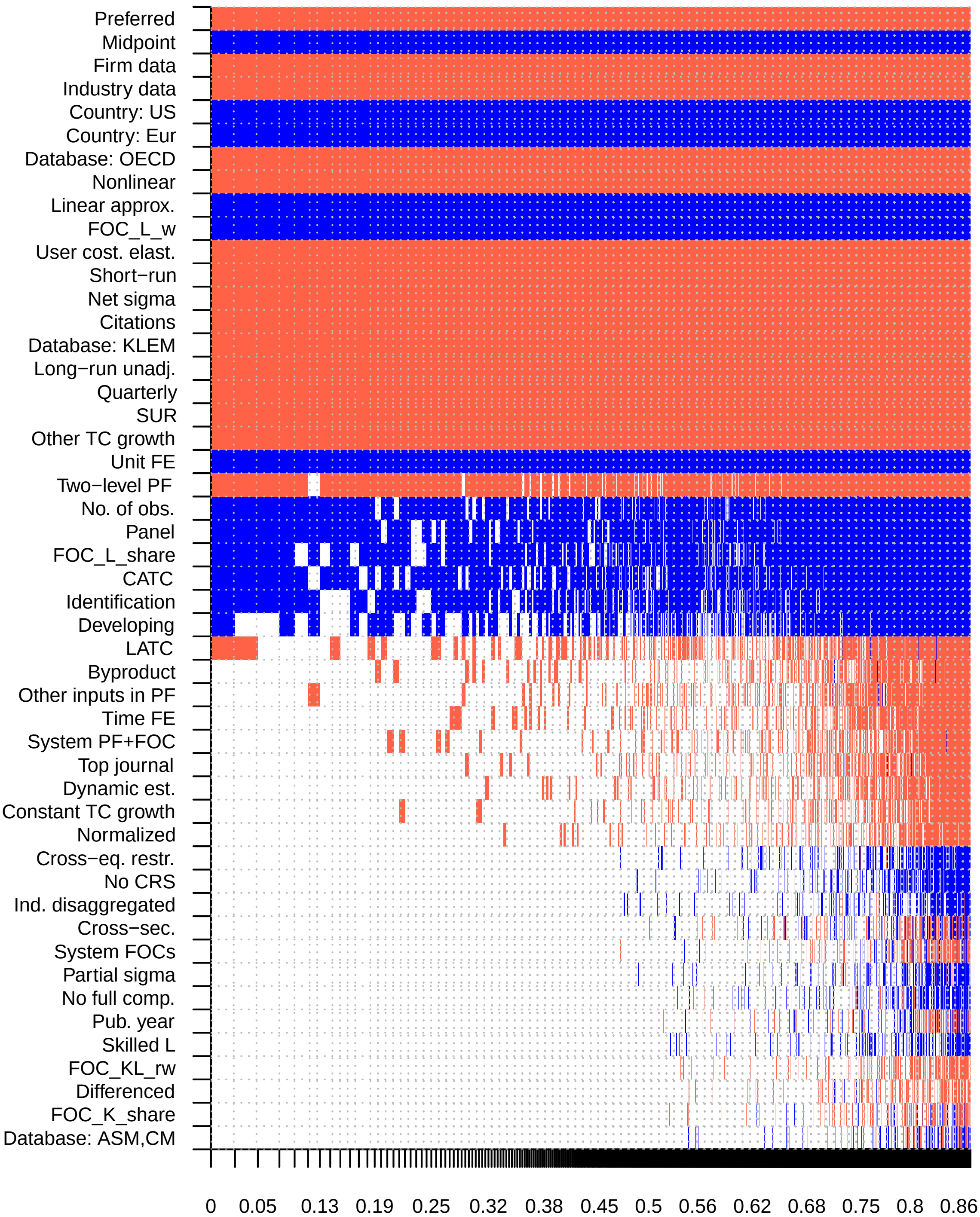
Note: Our frequentist model averaging (FMA) exercise employs Mallow's weights (Hansen, 2007) and the orthogonalization of the covariate space suggested by Amini and Parmeter (2012). Dark gray color denotes variables that are deemed important also in the BMA exercise. Light gray color denotes variables that are deemed important in the FMA but not the BMA exercise.

Figure D1: Model Inclusion in Bayesian Model Averaging, Weighted by the Inverse of the Number of Estimates per Study



Note: The response variable is the estimate of the elasticity of substitution. Columns denote individual models; variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes cumulative posterior model probabilities; only the 5,000 best models are shown. Blue color (darker in grayscale) = the variable is included and the estimated sign is positive. Red color (lighter in grayscale) = the variable is included and the estimated sign is negative. No color = the variable is not included in the model.

Figure D2: Model Inclusion in Bayesian Model Averaging, Weighted by the Inverse of the Standard Error



Note: The response variable is the estimate of the elasticity of substitution. Columns denote individual models; variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes cumulative posterior model probabilities; only the 5,000 best models are shown. Blue color (darker in grayscale) = the variable is included and the estimated sign is positive. Red color (lighter in grayscale) = the variable is included and the estimated sign is negative. No color = the variable is not included in the model.

Subsamples with Measurement Variables

As a complementary exercise to our baseline specification, we also run BMA analyses for subsamples of data in order to control for variables that are relevant only for a given subsample. We call these variables measurement variables. We need to create subsamples of the main dataset, because the variables relevant for the FOC for labor are not relevant for the FOC for capital, and vice versa. Regarding the estimates that utilize the FOC for labor, we include additional variables on how labor and the wage rate are measured. Regarding the estimates that utilize the FOC for capital, we include variables on how capital and the rental rate of capital are measured. Regarding industry-level estimates, we include the sector for which the elasticity was estimated, that is, the primary, secondary, and tertiary sectors; and, within the secondary sector, groups for industrial goods production, material goods production, and consumer goods production.

Concerning the measurement of labor, our reference category is measurement via the number of workers. We include a dummy equal to one if labor is measured using the number of hours worked. We also include a dummy variable that equals one if labor income is adjusted for self-employed labor income. As for the wage rate, we include dummy variables for the case when the rate is measured directly (in contrast to the situation when the wage rate is measured as the total amount paid to employees divided by the labor variable) and when the wage rate is used in nominal terms. In addition, we examine the effect of adjusting for changes in skill over time, for example, adjusting for the share of white- versus blue-collar workers.

Concerning the measurement of capital, our reference category is unspecified capital. We include dummies for specific measurements, including measurement as service flow, measurement via the perpetual inventory method, and capital stock in an index form. We code for special categories of capital stock: equipment, structures, IT, and residential capital stock. We include a separate dummy equal to one if the study controls for capacity utilization, either by adjusting the measurement variables or by adding it as a control. Underutilized capital would bias the results, since it biases the effect of input on output (Brown, 1966); nevertheless, only a small proportion of studies (Brown, 1966; Behrman, 1972; Dissou et al., 2015, among others) explicitly use this approach, for example by including capacity utilization indices.

Regarding the rental rate of capital, the baseline category comprises the user cost of capital, or, in other words, the standard Hall-Jorgenson formula (Jorgenson, 1963; Hall and Jorgenson, 1967), which appears in two-thirds of all the estimations. The Hall-Jorgenson formula calculates the user cost of capital as a function of the relative price of capital, the rate of return, and depreciation. We include a dummy for the case when the tax rate is an additional variable in the Hall-Jorgenson formula. The second most frequently used measurement is the quasi-rent approach, which calculates the rental rate of capital as the difference between total value added and total wages divided by the capital stock; this approach is used in 17% of cases, for example in Dhrymes (1965), Ferguson (1965), and Lovell (1973). Further, the rental rate of capital can be measured either in gross terms or in net terms and in real or nominal terms; nevertheless, the variability in nominal user cost is almost zero, so we do not include the corresponding variable.

In all subsamples we control for the measurement of output: first, we include a dummy variable that equals one if output is not measured as gross product or in value added terms, but in another way—for example, as the amount of sales. Second, we include a dummy for the case when output is used in an index form.

How does the addition of these variables affect our results? First, we include labor-specific variables, which capture how labor and the wage rate are measured, and run BMA on the subsample of data estimating the FOC for labor. The subsample covers less than half of the original dataset; the results are displayed in Figure D3. Only two of the newly included measurement variables are important for the explanation of the heterogeneity in the reported elasticities: direct measurement of the wage rate and measurement of labor as total labor force. The main drivers of heterogeneity remain the same, while the total explanatory power of the analysis increases only marginally.

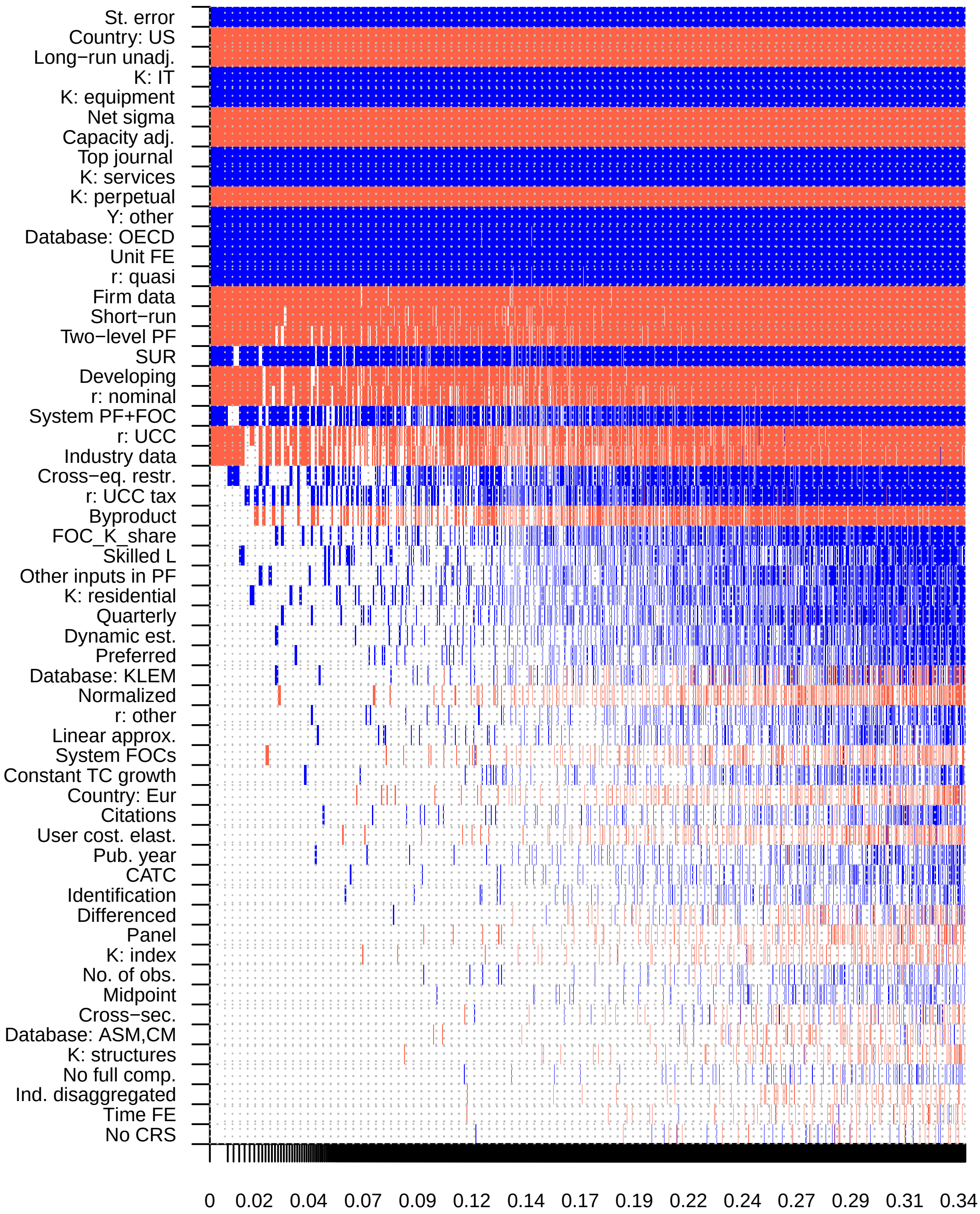
Concerning capital-related variables, we find that the type of capital under examination represents an important driver of the differences in results (Figure D4). IT capital and equipment capital are more substitutable with labor than other types of capital, such as buildings. When capital is measured as service flow, the estimates typically yield a larger elasticity of substitution. It also matters how the rental rate of capital, r, is computed, specifically whether the Hall-Jorgenson formula is used—we find that it yields smaller elasticities than do other approaches. The best-practice estimate derived from both subsamples and conditional on plugging in mean values for measurement variables would again equal 0.3, very far from the Cobb-Douglas assumption.

Finally, for the subsample of disaggregated elasticities we run the baseline BMA enriched with industry-relevant variables in Figure D5. We do not find any significant determinants that would suggest that the elasticity of capital-labor substitution differs systematically across sectors or industry groups (production of materials, production of industrial goods, production of consumer goods, and production of services). Given the number of variables in our analysis, it is infeasible to add more industry-specific variables, since that would create troubles with collinearity.

Figure D3: Model Inclusion in Bayesian Model Averaging, Labor-Specific Variables

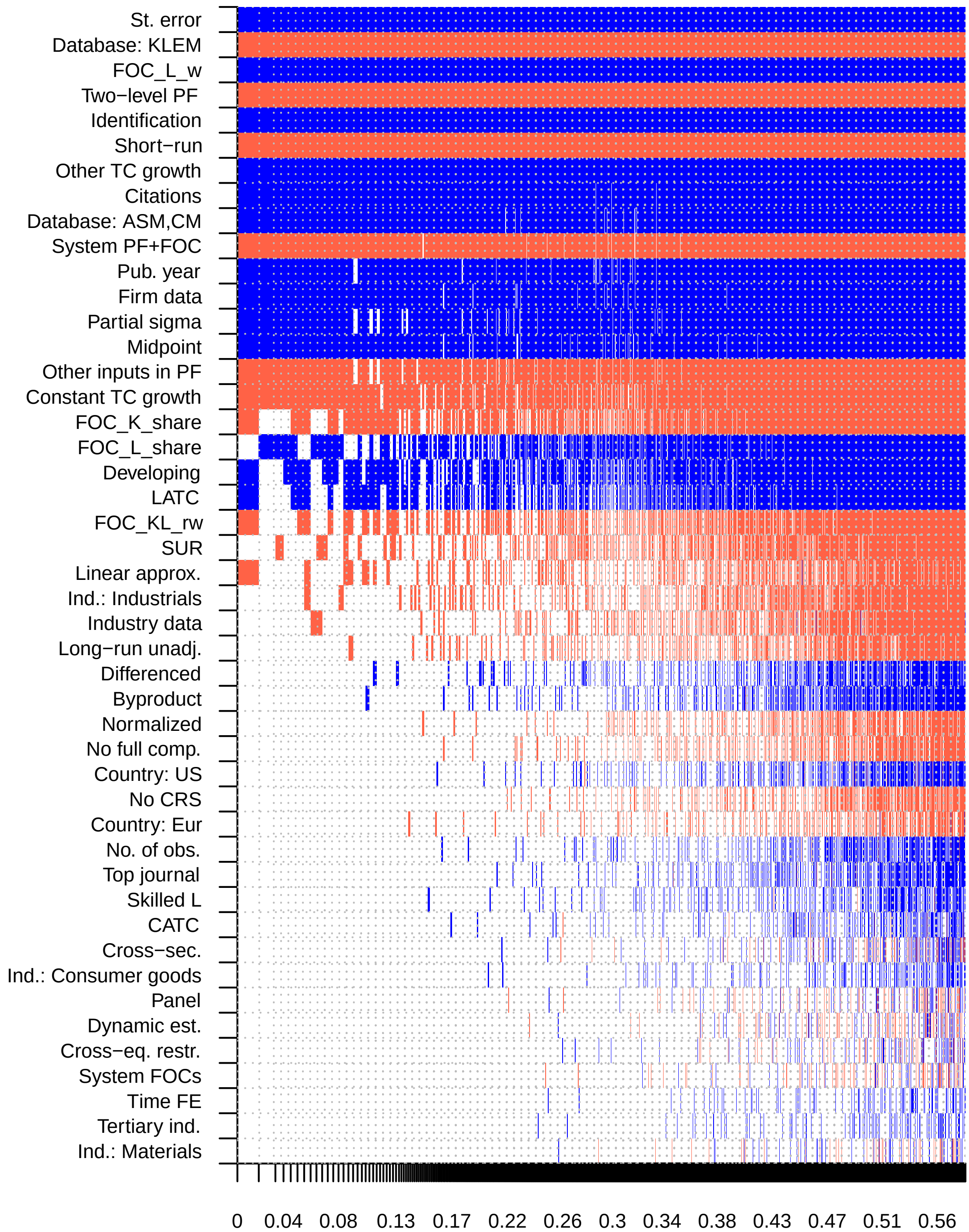
Note: The response variable is the estimate of the elasticity of substitution. Columns denote individual models; variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes cumulative posterior model probabilities; only the 5,000 best models are shown. Blue color (darker in grayscale) = the variable is included and the estimated sign is positive. Red color (lighter in grayscale) = the variable is included and the estimated sign is negative. No color = the variable is not included in the model.

Figure D4: Model Inclusion in Bayesian Model Averaging, Capital-Specific Variables



Note: The response variable is the estimate of the elasticity of substitution. Columns denote individual models; variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes cumulative posterior model probabilities; only the 5,000 best models are shown. Blue color (darker in grayscale) = the variable is included and the estimated sign is positive. Red color (lighter in grayscale) = the variable is included and the estimated sign is negative. No color = the variable is not included in the model.

Figure D5: Model Inclusion in Bayesian Model Averaging, Industry-Specific Variables



Note: The response variable is the estimate of the elasticity of substitution. Columns denote individual models; variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes cumulative posterior model probabilities; only the 5,000 best models are shown. Blue color (darker in grayscale) = the variable is included and the estimated sign is positive. Red color (lighter in grayscale) = the variable is included and the estimated sign is negative. No color = the variable is not included in the model.

Appendix E: Studies Included in the Dataset

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