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DISSERTATION THESIS

Empirical Essays on Monetary Policy and Financial Stability

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Declaration of Authorship

The author hereby declares that she compiled this thesis independently, using only the listed resources and literature, and the thesis has not been used to obtain a different or the same degree.

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Prague, March 22, 2024

Signature

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Abstract

This dissertation thesis consists of three empirical essays focusing on monetary policy, macroprudential policy, financial stability, and quantitative research synthesis of literature on a key economic parameter.

The first essay presents a meta-analysis of the elasticity of substitution between capital and labor. We identify factors responsible for large values of the elasticity estimated in the literature—on average, 0.9—and find that the mean elasticity conditional on the absence of these factors is 0.3. To obtain this result, we collect 3,186 elasticity estimates reported in 121 studies and codify 71 variables that reflect the context in which researchers produce their estimates. We find publication bias is responsible for at least half of the overall reduction in the mean elasticity from 0.9 to 0.3. The empirical literature thus rejects the Cobb-Douglas specification.

The second essay collects 1,555 estimates of the impact of short-term monetary policy rates on house prices from 37 individual studies. Several central banks have leaned against the wind in the housing market by increasing the policy rate preemptively to prevent a bubble. Yet, we show that the empirical literature provides mixed results: the estimated semi-elasticities range from -12 to positive values. We then relate the estimates to 39 characteristics of the financial system, business cycle, and estimation approach. We find that the mean reported estimate is exaggerated by publication bias.

The third essay explores the effect of higher capital requirements on bank credit growth in the Czech Republic, drawing on a unique confidential bank-level dataset. Our results indicate that higher additional capital requirements have a negative effect on the credit supply of banks maintaining lower capital surplus. We estimate the effect on annual credit growth to be between -1.2 and -1.8 pp. We emphasise the crucial role of bank capital surplus in the transmission of more stringent capital regulation.

JEL Classification	D24, E23, E32, E52, E58, G21
Keywords	Capital, capital regulation, credit growth, elasticity of substitution, house prices, interest rates, labor, meta-analysis, monetary policy, publication bias

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Chapter 1

Introduction

This dissertation thesis consists of three stand-alone essays. All three essays are empirical and address issues relevant from a policy perspective in central banks but also pertinent to the theory of economic growth.

The first and the second essays are linked together via their common methodology: a meta-analysis. A meta-analysis, as a quantitative research synthesis, has been on the rise in economics in recent years. As pointed out by Havranek *et al.* (2020), not only the number of published meta-analyses has increased dramatically, but many novel meta-analytical techniques have been developed in the last ten years, for example, Andrews & Kasy (2019); Ioannidis *et al.* (2017); Stanley & Doucouliagos (2017); Furukawa (2019); van Aert & van Assen (2021); Bom & Rachinger (2019). The methods focus mainly on examining the pattern of publication bias and how to correct for it. In both essays, we apply the state-of-the-art meta-analytical approaches to i) summarize the existing literature and synthesize significant heterogeneities (the first and the second essay) and ii) provide the basis for calibration of a key economic parameter (the first essay).

Both essays deal with publication bias that arises when different estimates have a different probability of being reported depending on sign and statistical significance. In most cases, theory and intuition provide little backing for estimates to be of a particular sign, so it seems natural to discard such estimates. However, such a censoring distorts inference drawn from the literature (for example, Ioannidis *et al.* 2017). Given sufficient noise in data and methods, both zero (statistically insignificant) estimates and estimates of an unexpected sign will appear. For each individual author who obtains such estimates, it makes little sense to focus on them. The problem is that noise in data and methods will also produce estimates that are of expected sign but are much larger than the true effect, and such estimates are hard to identify: a threshold symmetrical to zero that would tell the researcher the estimates are implausible does not exist. If many small imprecise estimates are discarded, but many large imprecise estimates are reported, an upward bias emerges on average. That presents a paradox: publication bias can be beneficial at the micro level of individual studies, but is detrimental at the macro level of the entire literature. Ioannidis *et al.* (2017) document that average effects reported in economics are exaggerated twofold. We find approximately the same distortion due to publication bias in the first essay and even more substantial distortion in the second essay. In both essays, we address publication bias in topics where it has not been examined before. Given the importance of the parameters in question, we consider it essential.

Second, both essays aim to examine heterogeneity in the existing literature and assign a pattern to it. We analyze 121 studies in the first essay and 37 in the second. The estimates coming from such a large number of studies vary widely, and a quantitative synthesis was missing. We collect several dozen variables that reflect the context in which researchers obtain their estimates. The variables capture the characteristics of the data, specification choice, econometric approach, and publication characteristics. Even though previous literature hinted at some differences in the estimated effects, by definition, they could compare only a few models, specifications, or countries. But based on the primary studies, we can build large databases of not only the reported results but also the factors that might have influenced those results. We are thus able to examine the heterogeneity in the estimated effects with much more power than the individual studies in the literature.

Next, the second and the third essays are linked thematically—they both examine central bank policies and their effect on financial variables. The second essay examines the effect of monetary policy on the critical variable from the perspective of financial stability—house prices. Many argued, and several central banks leaned against the asset price increases by raising the policy rate preemptively to prevent a bubble. Several researchers have already examined the nexus between monetary policy and financial imbalances and the assertions for an activist vs wait-and-see approach for central banks (for example, Assenmacher-Wesche & Gerlach 2010; Svensson 2014; 2017; Powell & Wessel 2021), but we are the first to examine whether a leaning-against-the-wind policy in housing markets is justified synthesizing all the available research in the field.

In addition to changes in monetary policy, the global financial crisis and its aftermath emphasized the importance of deploying macroprudential policies, which focus on mitigating risk in the financial system and increasing its overall resilience. It was shown that monetary policy, which ensures price stability, is insufficient for maintaining financial stability. The two policies, macroprudential and monetary, are also not independent. While they affect highly correlated variables such as bank rates and credit, they may have different objectives within the financial cycle. The tensions might become particularly acute in times of severe crisis or constrained monetary policy (Matheron & Antipa 2014). Given the relative novelty of the scope of macroprudential instruments, the third essay aims to contribute to the area of empirical research in this field by exploring their effects—specifically, the effects of new capital requirements—on credit growth.

In the following paragraphs, I will introduce individual essays in more detail. The first essay, titled *'Measuring capital-labor substitution: The importance of method choices and publication bias'*, published in the Review of Economic Dynamics, provides a large meta-analysis on a key parameter in many areas of economics—the elasticity of substitution between capital and labor. Among other things, the size of the elasticity has practical consequences for monetary policy. The effectiveness of interest rate changes in steering inflation depends on the assumptions central banks' models make about the value of the elasticity. 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 overstate the strength of monetary policy and should imply a more aggressive campaign of interest rate cuts in response to a recession. In this paper we show that the Cobb-Douglas specification is at odds with the empirical evidence on the elasticity. Aside from monetary policy, the value of the elasticity is central to other fields, including the theory of long-run growth and labour share. In fact, the evidence of declining labor share has revived interest in estimating the elasticity as some of the recent explanations depend critically on the value of the elasticity (Piketty 2014; Karabarbounis & Neiman 2014). The value of the elasticity has also implications for predictions on technological change and income inequality and consequences for fiscal policy.

In order to synthesize the vast literature on the substitution elasticity, we collect 3,186 estimates of the elasticity reported in 121 studies and codify 71 variables that reflect the context in which researchers produce their estimates.

The finding of strong publication bias predominates in our results. The mean elasticity reaches 0.9, but when we correct for publication bias, it shrinks to 0.5. With 71 control variables, we face substantial model uncertainty and address it by Bayesian and frequentist model averaging. Next to the publication bias, we show three main factors drive the heterogeneity in the literature: source of variation in input data (cross-country vs. industry-level variation), identification approach (whether or not information from the first-order condition for capital is accounted for), and normalization of production function. As the bottom line of the analysis, we construct a hypothetical study that assigns more weight to the estimates that are arguably better specified. The result represents a mean estimate conditional on the absence of publication bias and on the use of best-practice methodology. In this way, we obtain an elasticity of 0.3, with an upper bound of the 95% confidence interval at 0.6.

The second essay, titled *'When does monetary policy sway house prices? A meta-analysis'* and published in the IMF Economic Review, examines the relationship between monetary policy—specifically, short-term policy rates—and house prices. We collect 1,555 estimates extracted from 237 impulse responses reported in 37 individual studies covering 45 countries and 72 years. Our meta-analysis is unusual in that we collect and examine graphical results: the exact numerical results are rarely reported. Meta-analyses of graphical results are rare—a recent example is the meticulous survey by Fabo *et al.* (2021) on the effects of quantitative easing. As in the first essay, we explore potential publication bias. The expected effect of policy rates hikes on house prices is negative and zero as a psychological cutoff is not mirrored by a clear lower threshold, thus potentially causing a bias towards stronger effects. Using both linear and new nonlinear techniques, we indeed show that the mean estimate reported in the literature is exaggerated by publication bias.

To assign a pattern to large differences in estimated effects suggesting anything between a strong transmission to house prices on one hand and insignificant effects on the other, we relate the estimates to 39 characteristics of data, specification, estimation and countries. The finding of substantial publication bias is robust to controlling for this heterogeneity. Our results suggest that including controls correlated with policy rates (credit or money supply) significantly decreases the estimated effects of policy rates on house prices. Second, structural heterogeneity across countries is an essential factor in explaining the differences: for instance, the effects are more pronounced in countries with more developed mortgage markets and generally later in the cycle when the yield curve is flat and house prices enter an upward spiral. These country- and time-level characteristics can alter the implied impulse response by up to three percentage points.

The third essay focuses on the relationship between macroprudential policy and bank lending. The paper titled *'The effect of higher capital requirements on bank lending: The capital surplus matters'* and published in the Empirica, explores the effect of higher capital requirements on bank credit growth in the Czech Republic. The broader literature on the relationship between bank capital, capital regulation and lending provides mixed results, depending on the definitions and methodologies used. We contribute to the topic by analyzing a unique confidential bank-level dataset. Our detailed information on individual banks allows us to consider heterogeneity among banks and control for different effects with respect to banks' characteristics. Our results indicate that higher additional capital requirements have a negative effect on the credit supply of banks maintaining lower capital surplus. We estimate the effect on annual credit growth to be between 1.2–1.8 pp. We emphasize the crucial role of the excess of bank capital over the minimum capital requirement—the capital surplus—in the transmission of more stringent capital regulation.

The dissertation thesis consists of three papers, as is usual at the Insitute of Economic Studies, Faculty of Social Sciences, Charles University. However, during my doctoral studies, I have worked on several other papers that complement this thesis and that I briefly summarize below. As of March 2024, the papers I have coauthored have received altogether 368 citations according to Google Scholar. In the rest of the introduction, I will briefly introduce those papers as well.

The fourth paper, *'Does capital-based regulation affect bank pricing policy?'*, published in the Journal of Regulatory Economics and coauthored with Martin Hodula and Zuzana Gric, is closely linked to the third essay of the thesis, as it examines the effect of capital requirements on bank lending rates. Specifically, the paper tests whether a series of changes to capital requirements transmitted to a change to banks' pricing policy using a rich bank-level supervisory dataset covering the banking sector in the Czech Republic over the period 2004–2019. We estimate that the changes to the overall capital requirements did not force banks to alter their pricing policy. The impact on bank interest margins and loan rates lies in a narrow range around zero, irrespective of loan category. Our estimates allow us to rule out effects even for less-capitalised banks and small banks. The results obtained contradict estimates from other studies reporting significant transmission of capital regulation to lending rates and interest margins, and we engage in a deeper discussion of why this might be the case.

The fifth paper, *'A prolonged period of low interest rates in Europe: Unintended consequences'*, published in the Journal of Economic Surveys and coauthored with Simona Malovana, Josef Bajzik and Jan Janku, examines the potential adverse effects of a prolonged period of low interest rates on financial stability from multiple perspectives. First, we provide a unique comparison of natural rates of interest estimated using two approaches—with and without financial factors—for six large European countries inside and outside the euro area. Second, we provide a comprehensive review of the empirical literature, allowing us to identify and categorize financial vulnerabilities, which may be created and fueled by low interest rates. Third, we discuss a situation in which a prolonged period of low interest rates may lead to the point of no return by contributing to higher indebtedness, overvalued asset prices and underpriced risks, resource and credit misallocation, and lower productivity. With respect to all of that, we offer a few monetary policy considerations. Specifically, we suggest that (i) monetary policy should act symmetrically over the medium to long term, (ii) both the short-term and long-term costs and benefits of pursuing accommodative or restrictive monetary policy should be accounted for, and (iii) monetary and macroprudential policies need to be coordinated, and their interactions should be accounted for in order to find the best policy mix for the economy.

Sixth, the paper *'Does monetary policy influence banks' risk weights under the internal ratings-based approach?'*, published in the Economic Systems and coauthored with Simona Malovana and Vaclav Broz, studies the extent to which monetary policy may affect banks' perception of credit risk and the way banks measure risk under the internal ratings-based approach. Specifically, we empirically analyze the effect of different monetary policy variables on banks' risk weights for credit risk. We present robust evidence of a strong, statistically significant relationship between monetary policy easing and lower implicit risk weights of banks using the internal ratings-based approach. Further, we show that the recent prolonged period of accommodative monetary policy has been instrumental in establishing this relationship. The presented findings have important implications for prudential authorities, which should be aware of the possible side effects of monetary policy on how banks measure risk.

Seventh, the paper *'Estimating the effective lower bound for the Czech*

National Bank's policy rate, published in the Czech Journal of Economics and Finance and coauthored with Tomas Havranek, revisits the topic of monetary policy operating at the lower bound. The paper focuses on the estimation of the effective lower bound on the Czech National Bank's policy rate. The effective lower bound is determined by the value below which holding and using cash would be preferable to holding deposits with negative yields. This bound is approximated on the basis of the storage, insurance and transport costs of cash and the loss of convenience associated with cashless payments. This estimate is complemented by a calculation based on interest charges reflecting the impact of negative rates on banks' profitability. Overall, we get a mean of slightly below -1%, approximately in the interval (-2.0%, -0.4%). In addition, by means of a vector autoregression, we show that the potential of negative rates is not sufficient to deliver monetary policy easing similar in its effects to the impact of the Czech National Bank's exchange rate commitment during the years 2013-2017.

Last but not least, the paper *'The power of sentiment: Irrational beliefs of households and consumer loan dynamics'*, published in the Journal of Financial Stability and coauthored with Zuzana Gric and Martin Hodula, examines whether household sentiment can explain fluctuations in newly issued bank credit. We construct a novel measure of household sentiment using detailed data from the harmonized consumer surveys conducted in European countries. We differentiate between rational sentiment, which mimics dynamics in macroeconomic fundamentals, and irrational sentiment, which proxies households' optimism/pessimism on top of their rationally sourced beliefs. We show that shocks to the sentiment of households do have a measurable impact on the growth of consumer loans. Specifically, we assert a significantly positive role of irrational sentiment. Moreover, a closer examination reveals that the studied relationship is not symmetric over the business cycle—the effect of irrational sentiment is present only in periods in which a country's output is well above its potential.

The other two papers, published as working papers, deal with the effect of new climate policies on financial institutions' behaviour, and survey the opinions of professionals on the green transition. They are both currently under review in respected journals. This concludes the list of papers I have worked on during my doctoral studies that did not make it into the dissertation thesis. The following chapters will detail the first three essays presented in the introduction.

Chapter 2

Measuring Capital-Labor Substitution: The Importance of Method Choices and Publication Bias[1]

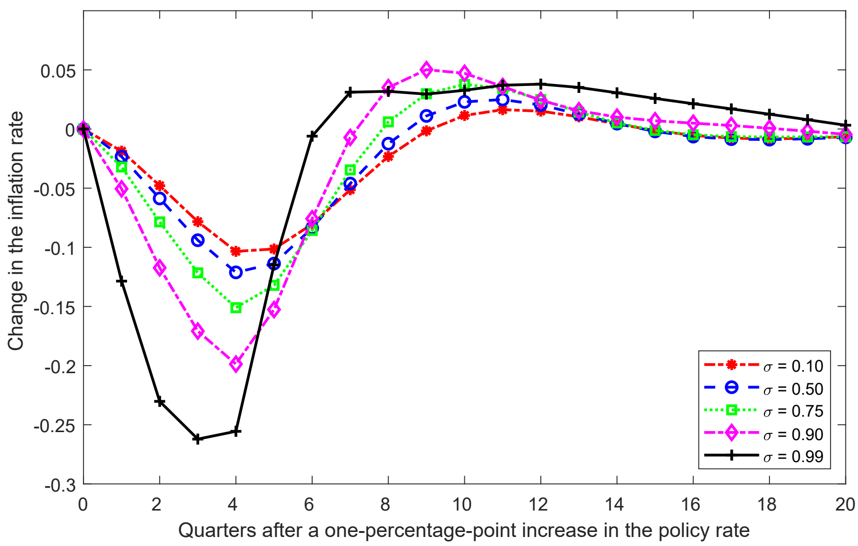
We show that the large elasticity of substitution between capital and labor estimated in the literature on average, 0.9, can be explained by three issues: publication bias, use of cross-country variation, and omission of the first-order condition for capital. The mean elasticity conditional on the absence of these issues 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 a 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.

[1]The paper was coauthored with Sebastian Gechert, Tomas Havranek and Zuzana Irsova and was published in the Review of Economic Dynamics (2022, 45, pp. 55-82). An earlier version of this paper circulated under the overly aggressive title "Death to the Cobb-Douglas Production Function." The authors thank the editor, two anonymous referees, Cristiano Cantore, Michal Franta and participants at various conferences for their useful comments. The paper was presented at the MAER-Net Colloquium 2018, Australia; FMM Conference: The Euro at 20, Germany; International Atlantic Economic Conference, USA; among others. The project was supported by the Czech Science Foundation (grant 21-09231S and 19-26812X) and Charles University (project Primus/17/HUM/16).

2.1 Introduction

A key parameter in economics is the elasticity of substitution between capital and labor. Among other things, the size of the elasticity has practical consequences for monetary policy, as Figure 2.1 illustrates. In the SIGMA model used by the Federal Reserve Board, the effectiveness of interest rate changes in steering inflation doubles when one assumes the elasticity to equal 0.9 instead of 0.5, yielding very different policy implications. We choose the SIGMA model for the illustration because, as one of very few models employed by central banks, it actually allows for different values of the elasticity of substitution. Almost all models use the convenient simplification of the Cobb-Douglas production function, which implicitly assumes that the elasticity equals one. If the true elasticity is smaller, these models overstate the strength of monetary policy and should imply a more aggressive campaign of interest rate cuts in response to a recession (Chirinko & Mallick 2017, make a related argument). In this paper we show that the Cobb-Douglas specification is at odds with the empirical evidence on the elasticity.

Figure 2.1: The elasticity of substitution matters for monetary policy



Notes: The figure shows simulated impulse responses of inflation to a monetary policy shock. We use a calibrated version of 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. This correction alone can imply halving the effectiveness of monetary policy in a structural model, as shown by Figure 2.1.

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 researchers command plenty of degrees of freedom in estimation design, a large estimate of the elasticity always emerges if the researcher looks for it long enough, and an upward bias in the literature arises. A useful analogy appears in McCloskey & Ziliak (2019), who liken publication bias to the Lombard effect in psychoacoustics: 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 & Kasy (2019), Bom & Rachinger (2019), and Furukawa (2019), which account for the potential nonlinearity between the standard error and selection effort.[2]

All the aforementioned techniques assume that in the absence of publication bias there is no correlation between estimates and standard errors: meta-analysis has its origins in medicine, where the exogeneity of the standard error is rarely questioned. In economics, however, the standard error can be endogenous for three reasons: it is itself an estimate (measurement error), publication bias may work through reporting artificially high precision (reverse causality), and some unobserved method choices may systematically influence both the

[2]Publication bias in economics has also been recently discussed, among others, by Havranek (2015), Brodeur *et al.* (2016), Bruns & Ioannidis (2016), Havranek & Irsova (2017), Havranek *et al.* (2017), Christensen & Miguel (2018), Astakhov *et al.* (2019), Bajzik *et al.* (2020), Blanco-Perez & Brodeur (2020), Brodeur *et al.* (2020), Cazachevici *et al.* (2020), Imai *et al.* (2021), Matousek *et al.* (2022), and Zigraiova *et al.* (2021).

point estimate and the corresponding standard error (omitted variables). No technique commonly used in economics meta-analyses allows us to get rid of the assumption. We employ study fixed effects, which filter out between-study differences, likely the most important source of endogeneity. We also employ the number of estimates as an instrument for the standard error, but some method choices can still be correlated with the size of the data set in primary studies.

A more fundamental solution is provided by psychology, where the newly developed p-uniform* technique (van Aert & van Assen 2021) analyzes the distribution of p-values instead of estimates and standard errors. The foundation of p-uniform* is the statistical principle that p-values are uniformly distributed at the mean underlying effect size: that is, when testing the hypothesis that the estimated coefficient equals the underlying effect. The idea of p-uniform* is to find a coefficient at which the distribution of p-values is approximately uniform; this is done by recomputing the reported p-values for different possible values of the underlying effect and then comparing the resulting distribution to the uniform one. Following this principle, the technique's test for publication bias evaluates whether p-values are uniformly distributed at the precision-weighted mean reported in the literature. All tests, including p-uniform*, suggest strong publication bias that substantially exaggerates the mean reported elasticity.

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 2020) and frequentist (Hansen 2007; Amini & 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), source of variation in input data (cross-country vs. industry-level variation), and identification approach (whether or not information from the first-order condition for capital is accounted for). Estimations using systems of equations tend to deliver results similar to those of single-equation approaches focused on the first-order condition for capital. 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.

As the bottom line of our analysis, we construct a hypothetical 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 related to quality (such as publication in a leading journal or a large number of citations). In this way we obtain an elasticity of 0.3 with an upper bound of the 95% confidence interval at 0.6. Though certainly not the definitive point estimate for the elasticity, it is the best guess we can make when looking at half a century of accumulated empirical evidence.

Defining best-practice methodology is subjective, and different authors will have different preferences on study design. But to arrive at 0.3, it is enough to hold two preferences: i) using variation across industries is superior to using variation across countries (which is substantiated, e.g., by Nerlove 1967; Chirinko 2008) and ii) including information from the first-order condition for capital is superior to ignoring it (and, for example, focusing exclusively on the first-order condition for labor). 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 the elasticity of 0.85, which is close to the mean estimate in the literature without correction 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.

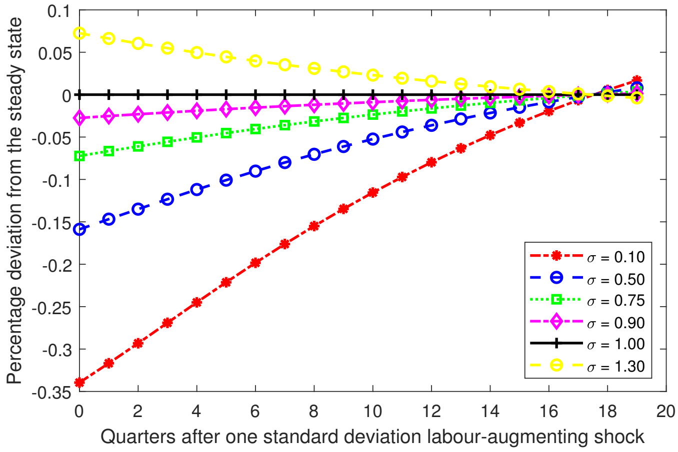
The elasticity of substitution between capital and labor is central to a host of problems aside from monetary policy. Our understanding of long-run growth depends on the value of the elasticity (Solow 1956). The sustainability of growth in the absence of technological change is contingent on whether the elasticity of substitution exceeds one (Antras 2004). Klump & de La Grandville (2000) suggest that a larger elasticity of substitution in a country results in higher per capita income. Turnovsky (2002) argues that a smaller elasticity leads to faster convergence. Nekarda & Ramey (2013) argue that the countercyclicality of the price markup over marginal cost also depends on the elasticity of substitution. 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).

But perhaps most prominently, the elasticity of substitution is a key parameter in the literature on the labor share. The evidence of a declining labor share has in fact revived general interest in estimating the elasticity because some of the explanations depend critically on the value of the elasticity (σ). Oberfield & Raval (2014) categorize these explanations into two groups: (1) mechanisms decreasing the labor share via changing factor prices and (2) mechanisms decreasing the labor share via changing technology. Regarding group (1), the explanations put forward by Piketty (2014) and Karabarbounis & Neiman (2014) hold only when the elasticity surpasses one. Then the global decline in the labor share can be attributed to an increasing capital-labor ratio, either via capital deepening (Piketty 2014) or as a response to falling investment prices (Karabarbounis & Neiman 2014). With $\sigma < 1$, however, declining prices of capital and increased capital accumulation raise the labor share. Yet, as we show in this paper, $\sigma < 1$ is consistent with the bulk of the empirical estimates of the elasticity. In this context, Glover & Short (2020a) assert that capital deepening cannot explain the observed decline; they point to issues that led to the high elasticity estimates of Karabarbounis & Neiman (2014). Regarding group (2), alternative explanations stress changes in automation, offshoring, directed technological change (as in Oberfield & Raval 2014; Eden & Gaggl 2018; Koh *et al.* 2016), a slowdown in labor productivity (as in Grossman *et al.* 2017), a rise in concentration (Autor *et al.* 2017), and demographic changes (Glover & Short 2020b); explanations that do not hinge on high values of σ.

The elasticity also has important effects on the short-run dynamics of the labor share. This channel can be illustrated by computing the response of the labor share to a labor-augmenting technology shock, as we do in Figure 2.2 based on the model developed by Cantore *et al.* (2014) and Cantore *et al.* (2015). In the case of the Cobb-Douglas production function the labor share remains constant, while with $\sigma < 1$ the share decreases after a labor-augmenting shock. As the figure illustrates, the response is highly sensitive to changes in σ. A model with a lower elasticity, consistent with our results, is able to match the actual dynamics of the data on the labor share better than the Cobb-Douglas case (Cantore *et al.* 2015).

The remainder of the paper is structured as follows: Section 2.2 briefly

Figure 2.2: The elasticity of substitution matters for the labor share



Notes: The figure shows simulated impulse responses of the labor share to a labor-augmenting technology shock. We use the model developed by Cantore *et al.* (2014) and Cantore *et al.* (2015).

discusses how the elasticity of substitution is estimated; Section 2.3 describes how we collect estimates of the elasticity from primary studies and provides a bird's-eye view of the data; Section 2.4 examines publication bias; Section 2.5 investigates the drivers of heterogeneity in the reported elasticities and calculates the mean elasticity implied by best practice in the literature; Section 2.6 concludes the paper. Appendix A.1 illustrates the working of publication bias and basic meta-analysis tools via a Monte Carlo simulation. The data, code, additional details, and robustness checks are available in an online appendix at `meta-analysis.cz/sigma`.

2.2 Estimating the Elasticity

To set the stage for data collection and 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 a 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

$$\sigma = \frac{d(K/L)/(K/L)}{d(w/r)/(w/r)} = -\frac{d\,\log(K/L)}{d\,\log(r/w)}, \tag{2.1}$$

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

$$Y_t = C[\pi(A_t^K K_t)^{\frac{\sigma-1}{\sigma}} + (1-\pi)(A_t^L L_t)^{\frac{\sigma-1}{\sigma}}]^{\frac{\sigma}{\sigma-1}}, \tag{2.2}$$

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 2.2 with Hicks-neutral technological change:

$$\log Y_t = \log C + \frac{\sigma}{\sigma-1} \log\left[\pi K_t^{\frac{\sigma-1}{\sigma}} + (1-\pi) L_t^{\frac{\sigma-1}{\sigma}}\right] \tag{2.3}$$

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

$$\log Y_t = \log C + \pi \log K_t + (1-\pi)\log L_t - \\ - \frac{(\sigma-1)\pi(1-\pi)}{2\sigma}(\log K_t - \log L_t)^2. \quad (2.4)$$

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:

$$\log\left(\frac{Y_t}{K_t}\right) = \sigma \log\left(\frac{1}{\pi}\right) + (1-\sigma)\log(A_t^K C) + \sigma \log\left(\frac{r_t}{p_t}\right). \quad (2.5)$$

Consequently, the FOC with respect to labor implies

$$\log\left(\frac{Y_t}{L_t}\right) = \sigma \log\left(\frac{1}{1-\pi}\right) + (1-\sigma)\log(A_t^L C) + \sigma \log\left(\frac{w_t}{p_t}\right), \quad (2.6)$$

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

$$\log\left(\frac{K_t}{L_t}\right) = \sigma \log\left(\frac{\pi}{1-\pi}\right) + (\sigma-1)\log\left(\frac{A_t^K}{A_t^L}\right) + \sigma \log\left(\frac{w_t}{r_t}\right). \quad (2.7)$$

In a similar way, one can derive FOCs with respect to the labor share $(wL)/Y$, 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 a 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 & 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

$$Y_t = Y_0 \left[\pi_0 \left(\frac{A_t^K K_t}{K_0}\right)^{\frac{\sigma-1}{\sigma}} + (1-\pi_0)\left(\frac{A_t^L L_t}{L_0}\right)^{\frac{\sigma-1}{\sigma}}\right]^{\frac{\sigma}{\sigma-1}}, \quad (2.8)$$

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 an indexed number form (Klump *et al.* 2012).

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 the second-order approximation to any production frontier (omitting now subscript t for ease of exposition):

$$\log Y = \log \alpha_0 + \sum_i \alpha_i \log X_i + \frac{1}{2}\sum_i \sum_j \alpha_{ij} \log X_i \log X_j, \tag{2.9}$$

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:

$$\begin{aligned}\log C = \alpha_0 + \theta_1 \log Y + \frac{1}{2}\theta_2 (\log Y)^2 + \sum_i \beta_i \log P_i + \\ + \frac{1}{2}\sum_i \sum_j \epsilon_{ij} \log P_i \log P_j + \sum_i \delta_i \log P_i \log Y,\end{aligned} \tag{2.10}$$

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

$$S_i = \beta_i + \sum_i \epsilon_{ij} \log P_j + \delta_i \log Y, \tag{2.11}$$

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

$$\sigma_{ij} = \frac{\gamma_{ij} + S_i S_j}{S_i S_j}. \tag{2.12}$$

We include estimates from all of the aforementioned 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 2.5, while the following section describes the dataset of the estimated elasticities.

2.3 Data

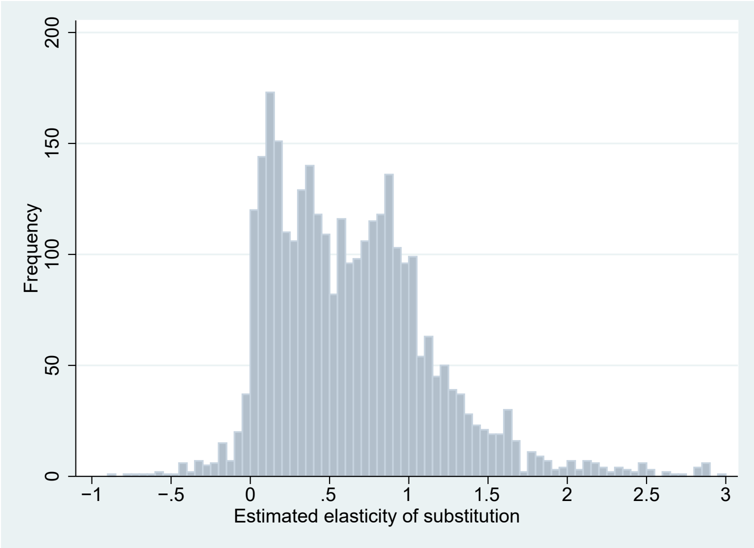
We use Google Scholar to search for studies estimating the elasticity. Google's algorithm goes through the full text of studies, thus increasing the coverage of suitable published estimates, irrespective of the precise formulation of the study's title, abstract, and keywords. Our search query, available 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 follow the guidelines for meta-analysis in economics by Havranek *et al.* (2020). 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 2.2. 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 a 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.[3] 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 & Stanley (2013), for example, survey dozens of meta-

[3] 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 & Lang (1992), Card & Krueger (1995), and Ashenfelter & Greenstone (2004).

Figure 2.3: Distribution of the estimated elasticities



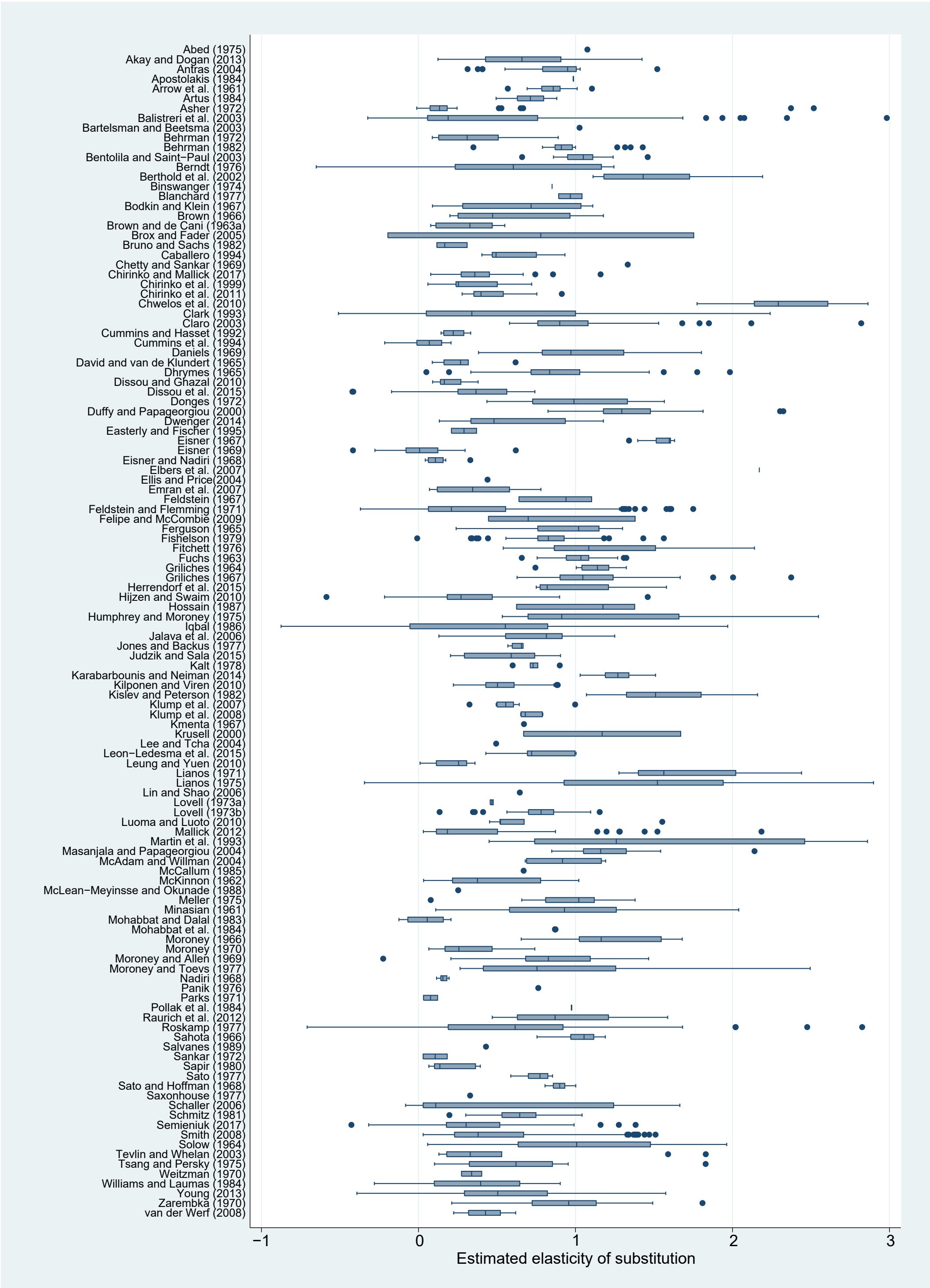
Notes: Estimates smaller than −1 and larger than 3 are excluded from the figure for ease of exposition but included in all statistical tests.

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 the appendix. Out of the 121 studies, 19 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. A simple mean of all 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 literature misleads the reader because of publication bias and misspecifications in the literature.

Figure 2.3 shows the distribution of the estimates. Curiously, the distribu-

Figure 2.4: Estimates vary both across and within studies

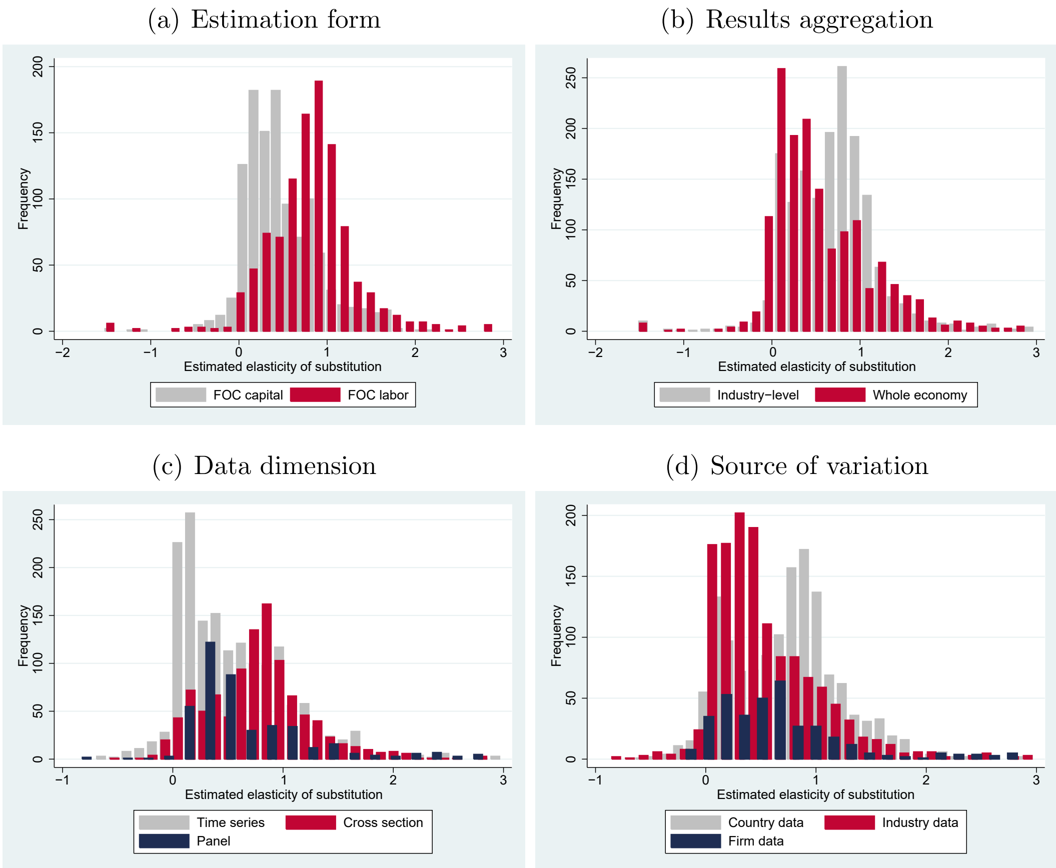


Notes: The figure shows a box plot of the estimates of the elasticity of substitution reported in individual studies. The box shows interquartile range (P25–P75) and the median highlighted. Whiskers cover (P25 − 1.5*interquartile range) to (P75 + 1.5*interquartile range). The dots are remaining (outlying)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

tion 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 2.4 we show the box plot of the estimates. Not only do elasticities vary 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 different estimates are obtained. In consequence, we had to collect more than 220,000 data points from primary

Figure 2.5: Prima facie patterns in the data



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

studies—a laborious but complex exercise. 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 2.5. 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 aggregated estimates. These patterns may explain the bimodality of the overall histogram presented in Figure 2.3. 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 2.5. Some of the differences among the estimates can also be attributable to publication bias, an issue to which we turn next.

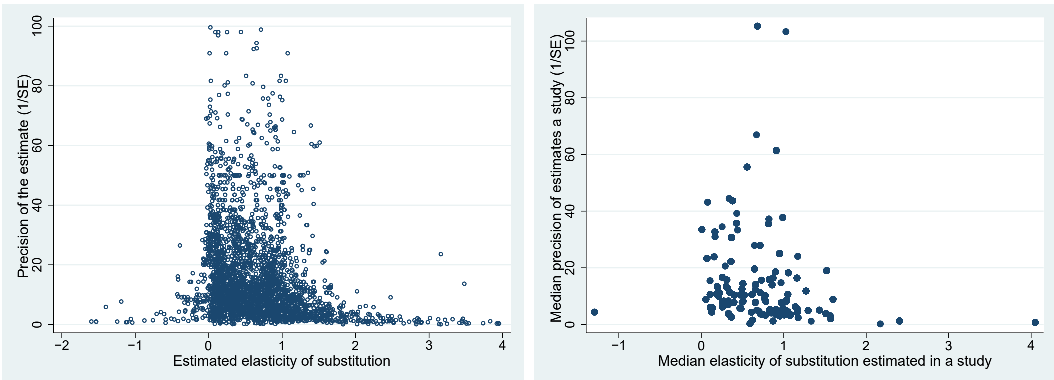
2.4 Publication Bias

Theory and intuition provides 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 inference drawn from the literature,[4] 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

[4]Other studies on publication bias in economics include, among others, Stanley (2001), Stanley (2008), Havranek & Irsova (2010), Irsova & Havranek (2010), Havranek & Irsova (2011), Havranek & Irsova (2012), Doucouliagos & Stanley (2013), Babecky & Havranek (2014), Stanley & Doucouliagos (2014), Alinaghi & Reed (2018), Doucouliagos *et al.* (2018), Gechert & Rannenberg (2018), Campos *et al.* (2019), Hampl *et al.* (2020), Hampl & Havranek (2020), Ugur *et al.* (2020), Xue *et al.* (2020), Alexander *et al.* (2021), and Elliott *et al.* (2022).

(statistically insignificant) estimates will appear. For each individual author who obtains such estimates, it makes little sense to focus on them; it will bring their study closer to the truth if they find and highlight 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 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 on average. Thus a paradox arises: publication bias can be beneficial at the micro level of individual studies, but is detrimental at the macro level of the entire 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 2.6: Negative estimates of the elasticity are underreported



Notes: 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 estimates, the right panel shows median estimates from each study. Estimates smaller than −2 and larger than 4 (together with precision values above 100 in the left panel) are excluded from the figure for ease of exposition but included in all statistical tests.

Figure 2.6 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, 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 & 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. In A.1 we use a simple Monte Carlo simulation to further explain the mechanism of publication bias and the baseline meta-analysis estimators we use.

2.4.1 Baseline Methods

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

$$\hat{\sigma}_{ij} = \sigma_0 + \gamma SE(\hat{\sigma}_{ij}) + u_{ij}, \tag{2.13}$$

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 2.1 we report the results of several specifications based on Equation 2.13. 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.

Table 2.1: Linear tests of funnel asymmetry suggest publication bias

	OLS	FE	BE	Precision	Study
SE	0.881***	0.656***	1.111***	0.755***	0.888***
Publication bias	(0.086)	(0.201)	(0.190)	(0.190)	(0.094)
	[0.49; 1.21]	–	–	[0.12; 1.40]	[0.62; 1.22]
Constant	0.492***	0.529***	0.499***	0.484***	0.544***
Mean beyond bias	(0.028)	(0.033)	(0.048)	(0.028)	(0.039)
	[0.38; 0.61]	–	–	[0.39; 0.66]	[0.44; 0.64]
Studies	121	121	121	121	121
Observations	3,186	3,186	3,186	3,186	3,186

Notes: 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 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. ***, **, 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; implementation follows Roodman (2019), and we use Rademacher weights with 9999 replications.

The first column of Table 2.1 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, precision becomes the weight, as suggested by Stanley & Doucouliagos (2017), which adjusts for the heteroskedasticity in the regression. Similar weights are also used in physics for meta-analyses of particle mass estimates (Baker & Jackson 2013). The corrected mean elasticity becomes a bit smaller, but 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.

The simple tests based on the Lombard effect and presented in Table 2.1 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

Table 2.2: 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)

Notes: Standard errors in parentheses. The method developed by Bom & Rachinger (2019) searches for a precision threshold above which publication bias is unlikely. Methods developed by Furukawa (2019) and Andrews & Kasy (2019) are described in detail in the appendix. The method developed by Ioannidis *et al.* (2017) focuses on estimates with adequate power.

threshold. This is the intuition behind the estimator due to Bom & Rachinger (2019) presented in Table 2.2. Bom & 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 the appendix.

Another nonlinear method to correct for publication bias is advocated by Andrews & 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 if the conditional publication probability is known. The underlying intuition involves jumps in publication probability at conventional p-value cutoffs. 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 the appendix. Finally, the remaining estimate in Table 2.2 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).

2.4.2 Extensions

Our results presented so far regarding publication bias can be criticized along three main lines. First, the distribution of elasticity estimates in some studies does not have to be symmetrical if the elasticity is not estimated directly but as a function of regression parameters from reduced-form estimations like (2.4). Such asymmetry in the distribution could give rise to the asymmetry of the funnel plot even in the absence of publication bias. Second, both the estimate and standard error of the elasticity can be jointly influenced by characteristics of data and methods, which would violate the exogeneity assumption and again yield an asymmetrical funnel plot even when no publication bias is present. Third, our tests of publication bias assume that researchers compare their estimates with zero. But other publication hurdles can potentially be more important: departure from the Cobb-Douglas case or other important benchmarks in the literature, such as the estimate of 1.3 by Karabarbounis & Neiman (2014) in the context of the labor share. We thank two referees of this Journal for bringing these important problems to our attention. In the remainder of this section we focus on the linear models of publication bias because they are simpler and we have shown earlier that they bring results similar to the more complex non-linear models.

First, we address the natural asymmetry in the estimates from some studies. Table 2.3 shows the results of publication bias tests when we exclude all estimates that can potentially be asymmetrically distributed. In other words, we retain only estimates for which the reported regression coefficient can be directly interpreted as the elasticity of substitution (so that no re-computation is needed, neither by us nor by the authors of the primary studies) and at the same time the coefficient features a symmetrical distribution given by the properties of the estimation technique. Doing so restricts our sample to 2,316 estimates from 67 studies, but the results remain remarkably consistent: we find strong upward publication bias and a corrected mean elasticity of about 0.5 or slightly less. Even the most conservative technique in this case, precision weighting with wild bootstrap, gives us an upper bound of the 95% confidence interval at 0.74, safely below the Cobb-Douglas case.

Second, we address the likely endogeneity of the standard error in some studies. Table 2.4 presents the results of an instrumental variable (IV) regression

Table 2.3: Direct estimates of the elasticity

	OLS	FE	BE	Precision	Study
SE	0.976^{***}	0.868^{***}	1.358^{***}	0.752^{*}	1.019^{***}
Publication bias	(0.167)	(0.317)	(0.271)	(0.396)	(0.132)
	[-0.23; 1.46]	–	–	[-0.61; 2.13]	[0.59; 1.35]
Constant	0.459^{***}	0.472^{***}	0.429^{***}	0.455^{***}	0.494^{***}
Mean beyond bias	(0.0226)	(0.0408)	(0.0575)	(0.0319)	(0.0354)
	[0.35; 0.57]	–	–	[0.31; 0.74]	[0.40; 0.60]
Studies	67	67	67	67	67
Observations	2,316	2,316	2,316	2,316	2,316

Notes: 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 elasticity of substitution and their standard errors reported in the j-th study. In this specification we only include direct estimates of the elasticity, i.e. the cases in which the regression parameter reported in a paper directly corresponds to the elasticity and no re-computation is needed. 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. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level. Whenever possible, in square brackets we also report 95% confidence intervals from wild bootstrap clustering; implementation follows Roodman (2019), and we use Rademacher weights with 9999 replications.

and a new technique called p-uniform*. IV presents 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 in the primary study: the root is correlated with the standard error by definition but is unlikely to be much correlated with the use of a particular estimation technique. Employing IV in the first column of Table 2.4 we obtain a larger estimate of publication bias and a smaller estimate of the mean elasticity corrected for publication bias, 0.3, compared to our baseline estimation presented earlier.

Table 2.4: Relaxing the exogeneity assumption

	IV	p-uniform*
Publication bias	2.186***	YES***
	(0.413)	(*0.005*)
	[1.20; 3.68]	
Mean beyond bias	0.279***	0.416**
	(0.0702)	(*0.042*)
	[0.04; 0.47]	[0.01; 0.74]
Studies	121	121
Observations	3,186	3,186

Notes: IV = the inverse of the square root of the number of observations employed by researchers is used as an instrument for the standard error. P-uniform* = a technique developed by van Aert & van Assen (2021) and based on the distribution of p-values. For IV, standard errors are clustered at both the study and country level and reported in parentheses. For p-uniform*, p-values are reported in parentheses. For both techniques, the corresponding 95% confidence intervals are reported in square brackets. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

Table 2.5: Potential sources of endogeneity

	Identif.	Data aggr.	Results aggr.	K: perpetual	Translog	Short run	All
SE	0.649***	0.803**	0.624***	0.754***	0.664***	0.473***	0.647**
(*publication bias*)	(0.219)	(0.318)	(0.146)	(0.259)	(0.212)	(0.0903)	(0.247)
Constant	0.512***	0.553***	0.569***	0.551***	0.529***	0.587***	0.613***
(*mean beyond bias*)	(0.0357)	(0.0420)	(0.0449)	(0.0337)	(0.0321)	(0.0155)	(0.0421)
SE * Identification	-0.0323						0.0874
	(0.332)						(0.263)
SE * Data aggr.		-0.299					-0.00928
		(0.334)					(0.202)
SE * Results aggr.			0.0616				-0.169
			(0.249)				(0.237)
SE * K: perpetual				-0.334			-0.285
				(0.289)			(0.285)
SE * Translog					-0.127		-0.0127
					(0.344)		(0.312)
SE * Short run						1.741*	1.707**
						(0.885)	(0.846)
Studies	121	121	121	121	121	121	121
Observations	3,186	3,186	3,186	3,186	3,186	3,186	3,186

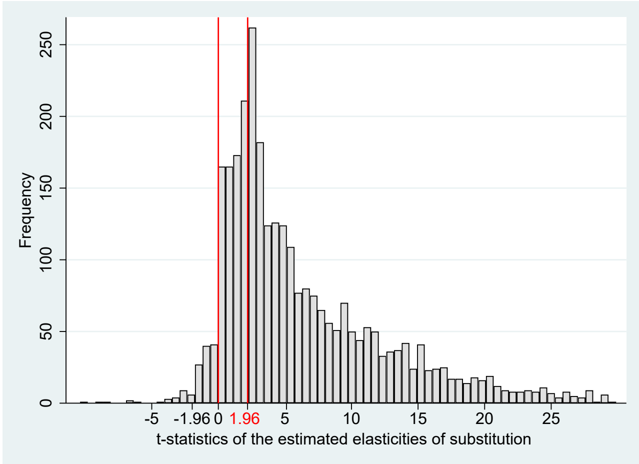
Notes: Study-level fixed effects and non-interacted variables are included but not reported. Standard errors are reported in parentheses and clustered at the study and country level. Identification = 1 if instrumental variables are used for identification. Data aggregation = 1 if state or country aggregation is used for input data. Results aggregation = 1 if the reported elasticity corresponds to an aggregate one (in contrast to elasticities corresponding to industries disaggregated at least at the 2-digit level). K: perpetual = 1 if input data for capital are measured via the perpetual inventory method. Translog = 1 if the elasticity is estimated using the translog functional form. Short run = 1 if the coefficient is taken from an explicitly short-run specification. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

The second column of Table 2.4 presents the results of p-uniform*. The technique was developed by van Aert & van Assen (2021) for standardized coefficients used in psychology, but it can also be applied to regression coefficients. At the heart of p-uniform* lies the statistical principle that p-values should be uniformly distributed *at the mean underlying effect size*: when testing the hypothesis that the estimated coefficient equals the underlying value of the effect. Publication bias affects some segments of the distribution of p-values (under-representation of large p-values, over-representation of p-values just below 0.05), but not the entire distribution. The idea of p-uniform* is to find a coefficient at which the distribution of p-values is approximately uniform; this is achieved by recomputing the reported p-values for various possible values of the underlying effect and then comparing the resulting distribution to the uniform one. In a similar vein, the technique's test for publication bias evaluates whether p-values are uniformly distributed at the precision-weighted mean reported in the literature. (The technique yields a binary result for the test of publication bias and a corresponding p-value.) Once again we obtain evidence for publication bias; the corrected mean elasticity is 0.4.

Another way to approach the endogeneity problem is to explicitly control for the most likely causes of endogeneity. We do so in Table 2.5, where we include interactions of the standard error with dummy variables for six study characteristics along with study fixed effects. We focus on the following characteristics: the use of IV, data aggregation, results aggregation, the use of the perpetual inventory method to approximate capital, the use of the translog function, and short-run estimation. For example, studies using IV techniques can be expected to deliver less precision, but at the same time systematically different results if endogeneity is an important issue in the primary literature. If a characteristic is associated with publication bias, or simply with systematically different standard errors that might give a false impression of publication bias, the interaction should prove strong. But we see no such pattern. Of the 12 coefficients for interactions estimated in Table 2.5, one is significant at the 10% level and one at the 5% level, which could easily arise by chance. Moreover, the coefficient on the non-interacted standard error remains statistically significant in all cases, and the mean beyond bias remains close to our baseline estimates. We thus fail to model the violations of exogeneity (or, alternatively, the sources of publication bias) explicitly.

The exogeneity assumption can also be relaxed by using the caliper test (Gerber & Malhotra 2008a), which moreover allows us to address the third main

Figure 2.7: The distribution of t-statistics shows jumps at 0 and 1.96



Notes: Outliers are excluded from the figure but included in all tests.

issue of our baseline approach, the focus on the zero threshold. The caliper test uses the simple idea that publication bias is the best explanation for sudden jumps in the distribution of the t-statistic. In a narrow caliper around 1.96, for example, the number of t-statistics reported above the threshold should equal the number of t-statistics below the threshold. If the former significantly outweigh the latter, we conclude publication bias likely plagues the literature. The distribution of t-statistics (Figure 2.7) does indeed show conspicuous jumps: at 0 and 1.96. The jump at 0 is so large that no statistical tests are necessary to conclude that negative estimates are discriminated against, either due to bias or a rational tendency not to report nonsensical results. In Table B.2 we test the threshold of 1.96, which is associated with statistical significance at the 5% level. In a narrow caliper of 0.05 (corresponding to t-statistics between 1.935 and 1.985), estimates above the threshold outnumber those below the threshold 30 to 9. The difference remains statistically significant with wider calipers.

In the second and third column of Table B.2 we adapt the caliper test to examine publication hurdles other than zero and 5% statistical significance with respect to zero. We focus on two values: 1.3, which is an important benchmark result by Karabarbounis & Neiman (2014), and 1, which corresponds to the Cobb-Douglas case and also the baseline noisy estimate for many regression equations like (2.4) or those that test the FOC of labor shares. Some studies do explicitly compare their estimates to these benchmarks; for the rest we recompute the t-statistics so that they correspond to this new hypothesis. We ask whether statistical (in)significance of the differences from the benchmarks

Table 2.6: Caliper tests for t-statistics corresponding to 5% significance thresholds

	Full sample $H_0 : \sigma = 0$ upper threshold	Labor share $H_0 : \sigma = 1.3$ lower threshold	Base at 1 $H_0 : \sigma = 1$ lower threshold
Caliper width = 0.05	0.277^{***} (0.067) N = 39		
Caliper width = 0.1	0.165^{***} (0.056) N = 71	-0.248 (0.251) N = 4	
Caliper width = 0.15	0.139^{***} (0.049) N = 96	-0.236 (0.197) N = 6	
Caliper width = 0.2	0.098^{***} (0.041) N = 142	-0.236 (0.197) N = 6	
Caliper width = 0.25	0.071^{**} (0.037) N = 177	-0.317^{**} (0.137) N = 9	0.322^{**} (0.156) N = 7
Caliper width = 0.3	0.088^{***} (0.033) N = 221	-0.338^{**} (0.123) N = 10	0.244^{*} (0.165) N = 8
Caliper width = 0.35	0.107^{***} (0.030) N = 258	-0.185 (0.140) N = 12	0.266^{*} (0.150) N = 9
Caliper width = 0.4	0.106^{***} (0.029) N = 292	-0.128 (0.140) N = 13	0.266^{*} (0.150) N = 9
Caliper width = 0.45	0.071^{***} (0.027) N = 326	-0.080 (0.137) N = 14	0.331^{**} (0.125) N = 10
Caliper width = 0.5	0.061^{**} (0.026) N = 353	-0.080 (0.137) N = 14	0.315^{**} (0.117) N = 12

Notes: The table reports the results of the caliper test by Gerber & Malhotra (2008a). The test compares the relative frequency of estimates above and below an important threshold for the t-statistic; with a sufficiently narrow caliper, there should be no difference. We use calipers of different sizes depending on the number of observations available. A test statistic of 0.139, for example, means that 63.9% estimates are above the threshold and 36.1% estimates are below the threshold. Standard errors are reported in parentheses and clustered at the study level. In the first column (full sample) the original reported t-statistics are evaluated. In the second column (labor share) only estimates from papers about the labor share are used, and t-statistics are recomputed to reflect the hypothesis $H_0 : \sigma = 1.3$. In the third column (base at 1) we include only reduced-form estimates for which an estimated regression parameter of zero translates to an elasticity of 1; the t-statistics of the elasticity are recomputed to reflect the hypothesis $H_0 : \sigma = 1$. N = number of estimates. The missing values for some calipers indicate no estimates available for the caliper. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level.

influences the probability of reporting the estimate. Regarding the value 1.3, we restrict our attention to estimates derived in papers on the labor share because the estimate by Karabarbounis & Neiman (2014) is relevant especially in this context (though the result would hold if all estimates were used). We see little effect of the threshold. Next, when examining the Cobb-Douglas case we include only reduced-form estimates for which a zero regression coefficient translates to an elasticity of 1. The fact that a noisy and small regression coefficient implies a unitary elasticity may affect the mechanism of publication bias, but the caliper test result would hold if we included all the estimates. Here we obtain significant results: estimates that are just consistent with the Cobb-Douglas case are reported more often than those that are significantly smaller than unity at the 5% level. Thus we find evidence of publication bias against three thresholds: positive sign, statistical significance with respect to zero at the 5% level, and consistency with the Cobb-Douglas production function.

Finally, a useful exercise is to focus on the estimates that cannot be negative by the definition of the corresponding nonlinear identification approach. In this subset of estimates any potential publication bias will stem exclusively from the preference of authors, editors, or referees for statistically significant results. Since the nonlinear estimates must be positive, there is no space for the preferential selection of a theory-consistent sign—a type of selection that can potentially be beneficial if negative estimates of the elasticity are caused by misspecifications more often than by chance. Unfortunately there are only 13 studies reporting 131 estimates that were obtained using nonlinear techniques, and such a small dataset limits the power of publication bias tests. Moreover, with nonlinear estimation (and thus an asymmetrical distribution of estimates in the absence of publication bias) the exogeneity condition for the standard error is automatically violated, which means p-uniform* is the only credible technique we can employ in this case. The technique gives us an estimate of the corrected mean elasticity at 0.45 (with the 95% confidence interval from 0.04 to 0.83) compared to the uncorrected mean of 0.71 when all studies are assigned the same weight. Therefore, while statistically insignificant at the 5% level, publication bias still exaggerates the mean reported nonlinear estimate by about 60%, compared to about 80% for the entire sample. We conclude that most of what we identify as publication bias is driven by the selection of convenient or seemingly important results, not by improving model specification.

2.5 Heterogeneity

In the previous section we have shown that 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 here. In Section 2.2 and Section 2.3 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. The variables, grouped in these categories, are discussed below and listed in the appendix together with their definitions and summary statistics.

2.5.1 Variables

Data characteristics

A central distinguishing feature of the studies concerns the source of variation. Almost half (45%) of the studies exploit variation across country or state-level, which forms our reference category. We include dummy variables equal to one if the study exploits variation across industries (43% of the estimates) or firms (12% of the estimates). Nerlove (1967) suggests that exploiting cross-country variation, where there may be systematic correlation between efficiency levels, product prices and wages, can lead to an upward bias in the estimated elasticity. Moreover, Chirinko (2008) discusses several drawbacks of cross-country variation in comparison to firm or industry-level variation, including limited variation available for identification and unaccounted heterogeneity.

We also include a dummy equal to one when the resulting estimate is reported at a very 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 & 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 employ time series data (around 53%), which we take as the reference category.

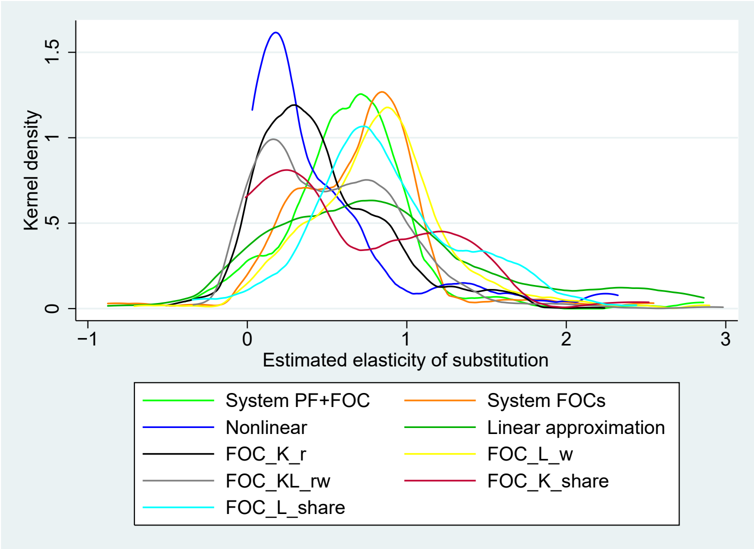
The final subset of variables covering data characteristics describes the source of data. Many estimates are based on data from the same databases—the largest number of studies employ data from the US Annual Survey of Manufactures and Census of Manufacturers. The second largest group is the KLEM database 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 still prefer not to ignore this potential source of differences in results and include the corresponding dummies as control variables.

Specification

Concerning the specification of the various studies described in Section 2.2, 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, FOC for capital based on the rental rate of capital, FOC for the capital-labor ratio based on the ratio between the wage rate and the rental rate of capital, FOC for capital share, and 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 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).

The differences in estimates derived from the various specifications are

Figure 2.8: Estimation form matters for the reported elasticities



Notes: A detailed description of the variables is available in the appendix.

clearly visible in the data (Figure 2.8). 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 & 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 essentially implies representing the production function in a consistent indexed number form. A proper choice of the point of normalization facilitates the identification of deep technical parameters. According to León-Ledesma *et al.* (2010), the superiority of the 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 a 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 the 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—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 & 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 & Moroney (1975), Bruno & Sachs (1982), and Chirinko & 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 & 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 & 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.

2.5.2 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:

$$\hat{\sigma}_{ij} = \alpha_0 + \sum_{l=1}^{49} \beta_l X_{l,ij} + \gamma SE(\hat{\sigma}_{ij}) + \mu_{ij}, \tag{2.14}$$

where $\hat{\sigma}_{ij}$ again denotes estimate i of the elasticity of substitution reported in study j, $X_{l,ij}$ represents control variables described in Subsection 2.5.1, 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.

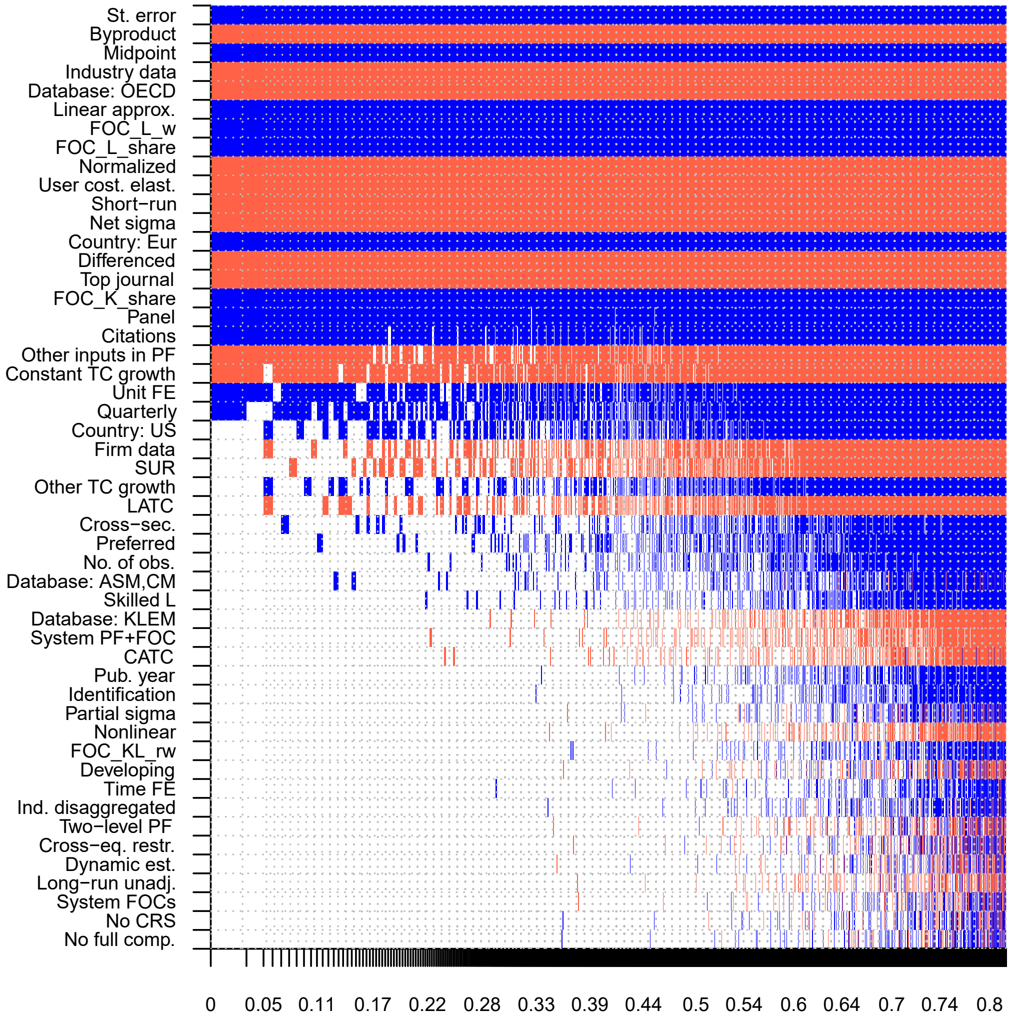
But using one regression is inadequate because of model uncertainty. With so many variables reflecting study design, including all of them would substantially attenuate the precision of our estimation. (We use 50 variables in the baseline estimation; the remaining 21 variables related to measurement of capital and labor and industry-level characteristics are included in the three subsamples presented in the appendix.) 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 2020), which arises naturally as a response to model uncertainty in the Bayesian setting.[5]

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 & 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 the appendix. BMA has been used in meta-analysis, for example, by Havranek *et al.* (2015); Zigraiova & Havranek (2016); Havranek *et al.* (2018a;b;c); Havranek & Sokolova (2020).

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

[5]If a simple OLS brought results similar to model averaging, we could simplify the analysis and just present OLS. But in our case a simple OLS regression including all variables would yield results quite different from Bayesian model averaging: 29% of the variables would lose their statistical significance (or importance in the Bayesian setting), while 17% of the variables would now be wrongly significant. The median change in the magnitude of the estimated coefficients for these variables would reach 133% in absolute value. (But note that our key results concerning publication bias and the best-guess elasticity would continue to hold.) We thus opt for the more complex but statistically more appropriate approach.

Figure 2.9: Model inclusion in Bayesian model averaging



Notes: 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 2.7. A detailed description of all variables is available in the appendix.

Table 2.7: 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		

Notes: 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 the appendix.

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 & Parmeter (2012) and resort to orthogonalization of the covariate space.

2.5.3 Results

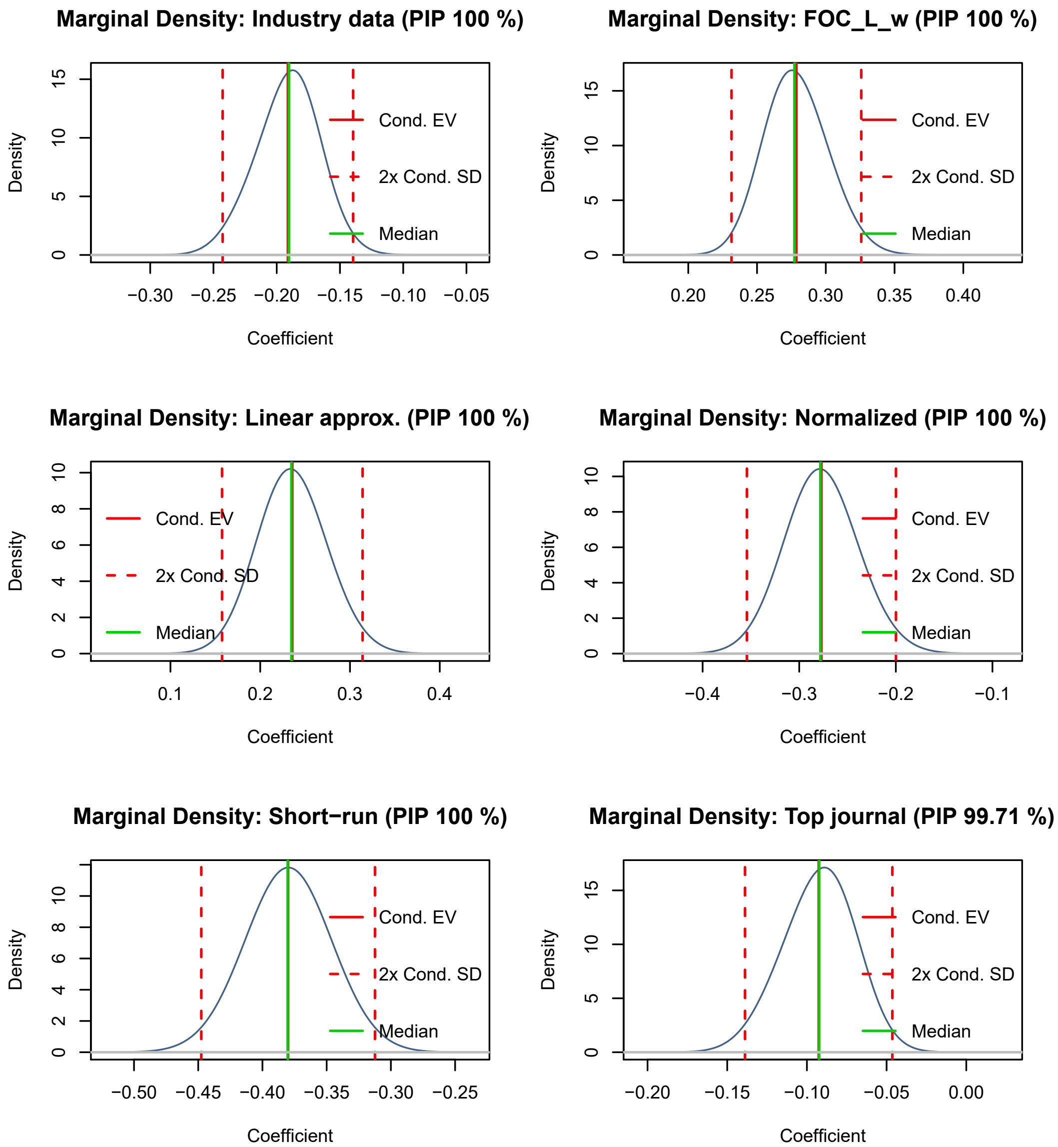
Figure 2.9 illustrates our results. The vertical axis depicts explanatory variables sorted by their posterior inclusion probabilities; the horizontal axis shows individual regression models sorted by their posterior model probabilities. The blue color indicates that the corresponding variable appears 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 heterogeneity in the estimates, as their posterior inclusion probabilities surpass 80%. Table 2.7 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 2.10 plots posterior coefficient distributions of selected variables. The results of the FMA exercise are reported in the appendix.

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 2.7). 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 2.13.

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 regression coefficient because the variable is in logs; the precise definition is available in the appendix.) The

Figure 2.10: Posterior coefficient distributions for selected variables



Notes: 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 2.7). 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.

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 exploit variation across industries tend to be significantly smaller than those using variation across countries and states, a result corroborating the prima facie pattern in the literature shown in Figure 2.5(d) in Section 2.3. This is consistent with Nerlove (1967) and Chirinko (2008), who argue that exploiting variation across countries can lead to an upward bias due to disregarded heterogeneity.

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 2.5(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 2.5(b), disappears in the BMA analysis. Elasticities computed for individual industries do not differ systematically from elasticities computed for the entire economy. 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

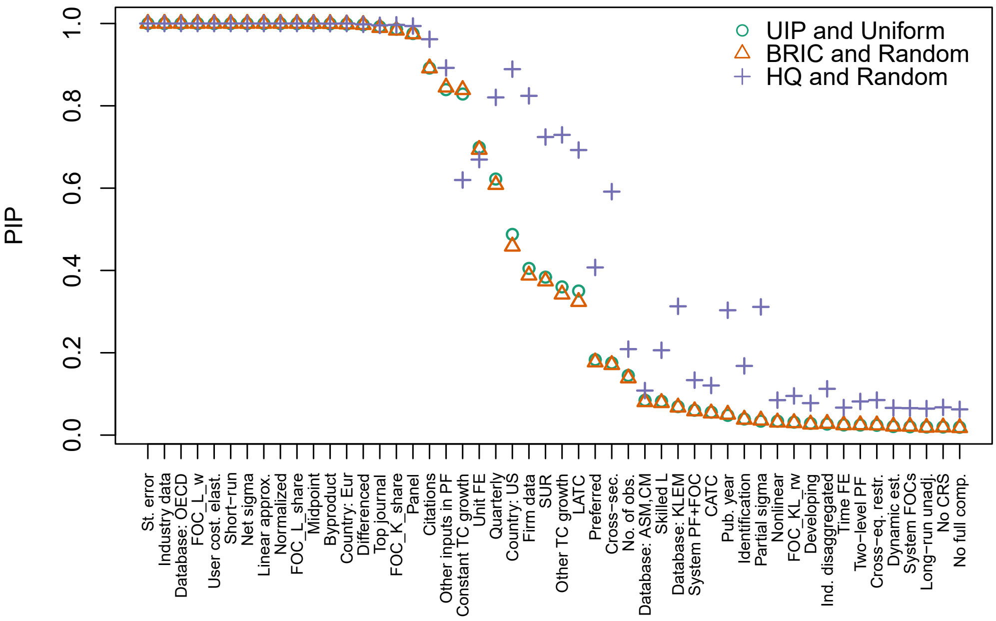
A stylized fact in the literature on capital-labor substitution has it that estimations based on the first-order condition for labor deliver larger elasticities than estimations based on the first-order condition for capital; see Figure 2.5(a) in Section 2.3. The BMA analysis corroborates this stylized 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 & 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 use of cross-country variation in 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.

Figure 2.11: Posterior inclusion probabilities across different prior settings



Notes: 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.

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 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 3.7 shows that the implied relative importance of the variables changes little when different priors are used for BMA. In the appendix 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; second, data are weighted by the inverse of the standard error. Our key results continue to hold in these specifications.

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 2.8, 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 column, 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), use of variation at the industry level, FOC for labor (ignoring FOC for capital), normalization of the production function, and the assumption of short-run or net elasticity. Changes in these variables can alter the resulting elasticity by 50% or more.

The mean implied elasticity is explored in Table 2.9. In essence, we create a synthetic study in which we use all the reported estimates but give different weights to certain 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, use of variation across industries instead of countries, and 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

Table 2.8: 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%

Notes: 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 by 0.117, more than a third of the size of the best practice estimate (one conditional on ideal data, method, and publication characteristics, as described in Table 2.9). Increasing the standard error from the sample minimum to the sample maximum is associated with an increase in the estimated elasticity by 0.461, more than one and a half of the best practice estimate. A detailed description of the variables is available in the appendix.

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 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 exploiting variation across countries, often used in the literature, and for short-run elasticity, net elasticity, and the use of a system of FOCs without a production function.

The results, shown in Table 2.9, illustrate the high degree of uncertainty that such an exercise entails: the 95% confidence intervals for all estimates

Table 2.9: 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)

Notes: 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.

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 exploiting variation across countries 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.

2.6 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 been previously 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 and might arrive at a point estimate different from 0.3. 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.* (2020) 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 case against Cobb-Douglas strengthens to the point where one has to warn against the continued use of this convenient simplification. As we show in the Introduction, a structural model built to aid monetary policy is biased from the beginning if it uses an elasticity of one for capital-labor substitution. Computational convenience should yield to the stylized fact established by half a century of meticulous research: capital and labor are gross complements.

Three caveats to the 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 certainly does not fit all situations and calibrations. While we are able to address several issues that we see as problems in the literature, in meta-analysis one can only solve methodological problems that have already been addressed by at least one previous study. The value of 0.3 is our best guess conditional on the available literature published prior to 2019, not the definitive point estimate for the elasticity. Third, we do our best to include all published studies estimating the elasticity of substitution, but still we 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.

Chapter 3

When Does Monetary Policy Sway House Prices? A Meta-Analysis[1]

Several central banks have leaned against the wind in the housing market by increasing the policy rate preemptively to prevent a bubble. Yet the empirical literature provides mixed results on the impact of short-term interest rates on house prices: the estimated semi-elasticities range from -12 to positive values. To assign a pattern to these differences, we collect 1,555 estimates from 37 individual studies that cover 45 countries and 72 years. We then relate the estimates to 39 characteristics of the financial system, business cycle, and estimation approach. Our main results are threefold. First, the mean reported estimate is exaggerated by publication bias, because insignificant results are underreported. Second, inclusion of controls correlated with policy rates (credit or money supply) decreases the estimated effects of policy rates on house prices. Third, the effects are stronger in countries with more developed mortgage markets and generally later in the cycle when the yield curve is flat and house prices enter an upward spiral.

[1]The paper was coauthored with Josef Bajzik and Tomas Havranek and published in the IMF Economic Review (2023, 71, pp. 538-573). The authors thank the editor of the journal and two anonymous referees for their helpful comments. The authors also thank Simona Malovana, Marek Rusnak, Jakub Mateju, and seminar participants at the MAER-Net Colloquium 2021, Greece; European Economics and Finance Society Annual Conference (online), 11th Biennial conference of the Czech Economic Society (online), Workshop on Macroeconomic Research 2021 (online), 3rd International Conference on European Studies (online), Czech National Bank Research Open Day and Czech National Bank Seminar for their useful comments on an earlier version of this paper. The earlier version of the paper was awarded Karel Englis Prize for the best paper in Czech economic policy by the Czech Economic Society. The work was supported by the Czech Science Foundation (project 21-09231S and project 19-26812X) and by the NPO Systemic Risk Institute (project LX22NPO5101).

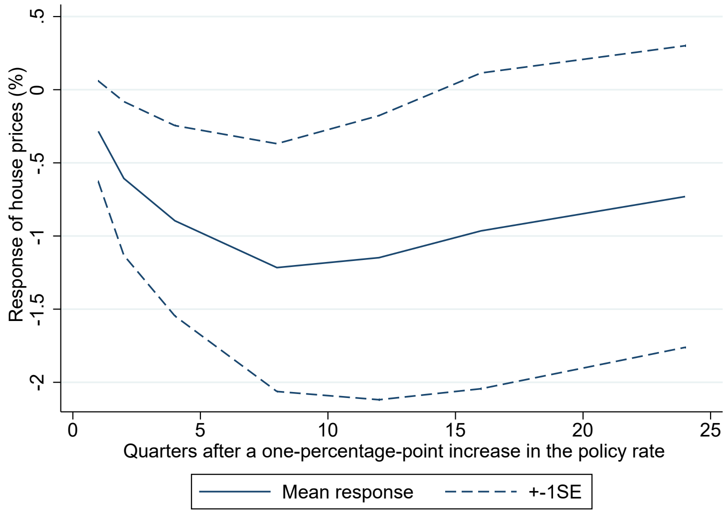
3.1 Introduction

Common wisdom has it that monetary policy is largely responsible for asset bubbles, including the rising house prices. That view sometimes translates into policy, such as in the case of the Swedish Riksbank between 2010 and 2014 or the government of New Zealand in 2021. In the most famous example of leaning against the wind, the Riksbank increased its policy rate from near zero to 2% in order to tame household indebtedness and house prices, even at substantial costs in terms of inflation and unemployment (Svensson 2014; 2017). The government of New Zealand, in turn, recently amended the mandate of the Reserve Bank of New Zealand and instructed it to consider house prices when making monetary policy decisions (Powell & Wessel 2021). The policy change in New Zealand is interesting both because its Reserve Bank has been an influential pioneer of innovations in central banking (introducing inflation targeting in 1990) and because by 2021 a large amount of research has amassed on the effects of monetary policy on house prices. This recent research, however, is rarely cited in the policy debate, which remains influenced by the arguments of Taylor (2007) in favor of the effectiveness of short-term rates in taming bubbles.[2] Perhaps one of the reasons for the relatively limited impact of the recent research is the variance in results. The literature lacks a synthesis that would assign a pattern to the different conclusions. That is what we attempt to provide in this paper.

Figure 3.1 shows the mean response of house prices to a one-percentage-point increase in the short-term monetary policy rate. The mean is extracted from 237 impulse responses reported in 37 studies. The impulse responses, computed from vector autoregressions (VARs, Sims 1980), are the main output of these studies. Hence our meta-analysis is unusual in that we collect and examine graphical results: the exact numerical results are rarely reported. For selected time horizons after the monetary policy shock we measure pixel coordinates and collect the estimated response of house prices. Meta-analyses of graphical results are rare, and a prominent recent example is the meticulous survey by Fabo *et al.* (2021) on the effects of quantitative easing. Note that Figure 3.1 shows the corresponding 68% confidence interval (one standard error on both sides of the mean), which is the norm in the VAR literature. (Few impulse responses would be statistically significant at the 5% level common

[2]For an excellent recent discussion of the pros and cons of leaning against the wind, see Benati (2021).

Figure 3.1: Mean reported response of house prices to a monetary tightening



Notes: Computed based on 1,555 estimates from 237 impulse responses reported in 37 papers. On average, the response bottoms out after two years at a 1.2% decrease in house prices following a one-percentage-point increase in the policy rate.

in most other fields of economics.) The impulse response bottoms out after two years at a 1.2% decrease in house prices following a one-percentage-point increase in the policy rate. We will call this effect, here 1.2, a semi-elasticity. It is clear that, on average, with such a small semi-elasticity central banks cannot plausibly combat double-digit inflation in house prices. The implication echoes Williams (2016), a concise narrative survey of 10 earlier papers on the topic.

But a mean figure conceals important differences in the context in which the impulse response is estimated. Perhaps in some countries and certain phases of the business cycle, leaning against the wind can help moderate the increase in house prices (and, vice versa, a loose policy may help reflate depressed housing markets). Calza *et al.* (2013) suggest that the transmission of monetary policy to house prices is stronger in countries with larger flexibility and development of mortgage markets. Similarly, Iacoviello & Minetti (2003) show that financial liberalization can be important for the strength of transmission. Assenmacher-Wesche & Gerlach (2010) examine whether transmission differs between boom and standard periods. Or perhaps the small mean response is contaminated by measurement problems, such as simple recursive identification (a problem stressed, for example, by Bjørnland & Jacobsen 2010) and omission of important variables, such as credit (Assenmacher-Wesche & Gerlach 2010). Our comparative advantage to the studies mentioned above is the richness of the meta-analysis dataset. No previous study in this literature has used data for more than 19 countries, which has made it difficult to investigate cross-country differences. Few cross-country studies examine more than a couple of business cycles. Similarly, comparisons of results with different identification of VAR models within individual studies have so far lacked statistical power. The work of the researchers who have collectively produced 237 impulse responses for various contexts allows us to examine the heterogeneity in transmission systematically.

Another problem with the mean impulse response is potential publication bias (Stanley 2001),[3] which stems from the selective reporting of results that have the intuitive sign or are statistically significant. Vector autoregressions are complex models with (at least in this literature) typically few degrees of freedom. It follows that the resulting impulse response is sometimes counterintuitive: for example, it can show that house prices do not react to policy rates, or even more puzzlingly that house prices rise following a monetary tightening. If researchers take such results as evidence that their model is misspecified, they can try to run different specifications until they obtain the desired outcome. The problem is that while the puzzling impulse responses can indeed arise because of misspecifications, they can also appear simply by chance, especially given the small datasets in the literature. Seemingly large estimated effects of monetary policy in the right direction can also be due to misspecifications or chance, but it is difficult to identify them. Zero is a clear psychological cutoff that is not mirrored by a corresponding upper threshold and thereby causes a bias towards larger effects. The resulting publication bias can only be corrected using meta-analysis techniques.

Publication bias does not imply cheating and is inevitable in observational empirical research even if all researchers are honest. (In experimental research the bias can potentially be tackled by the preregistration of experiments, see, for example, Olken 2015, but preregistration is difficult when data are publicly available, so that the researcher can inspect them before preregistration). Publication selection can even improve the results of individual studies. The

[3]For recent papers on publication bias in economics, including positive and negative evidence, see Havranek (2015), Brodeur *et al.* (2016), Bruns & Ioannidis (2016), Ioannidis *et al.* (2017), Stanley & Doucouliagos (2017), Stanley *et al.* (2017), Card *et al.* (2018), Christensen & Miguel (2018), Astakhov *et al.* (2019), DellaVigna *et al.* (2019), Blanco-Perez & Brodeur (2020), Brodeur *et al.* (2020), Ugur *et al.* (2020), Xue *et al.* (2020), Imai *et al.* (2021), Neisser (2021), Brown *et al.* (2022), Iwasaki (2022), and DellaVigna & Linos (2022). Earlier influential papers include Card & Krueger (1995), Stanley (2005; 2008), and Stanley & Doucouliagos (2010).

underlying effect of policy rate hikes on house prices will most likely be negative in most if not all contexts, so it is likely that the "wrong" sign indeed suggests to a researcher a problem with specification, sample size, or both. Thus it will improve the conclusions of an individual study when it does not focus on positive or zero responses of house prices. The idea of sign restrictions in vector autoregressions, eloquently advocated by Uhlig (2005), builds on a related principle. Unfortunately, under selective reporting the literature becomes biased as a whole since large estimates, also given by chance or misspecifications, are rarely omitted. So with individual studies we never know how much they suffer from publication bias.

For the basic identification of publication bias correction techniques we use the analogy suggested by McCloskey & Ziliak (2019), who compare publication selection to the Lombard effect in psychoacoustics: speakers involuntarily increase their vocal effort with increasing background noise. Similarly, given the example in the previous paragraph, many researchers will try harder to change the specification of their vector autoregression model if they have small samples and thus a lot of noise in estimation, a noise that often leads to insignificant initial estimates. With sufficient effort, the VAR model can be adjusted in a way that produces point estimates large enough to outweigh the large standard errors and thus delivers statistical significance. Therefore, selective reporting creates a correlation between estimates and standard errors, a correlation that otherwise should not appear in the literature. Aside from linear tests based on the Lombard effect (regressions of estimates on standard errors) we also employ recently developed nonlinear techniques by Andrews & Kasy (2019), Furukawa (2019), and van Aert & van Assen (2021). The latter technique, p-uniform*, relaxes the assumption of no correlation between estimates and standard errors in the absence of publication bias; the assumption is perhaps too strong for the VAR literature where the impulse responses are nonlinear combinations of underlying (unreported) regression coefficients. All techniques agree that the exaggeration due to publication bias is at least twofold.

In the second part of the analysis we relate the estimated impulse responses to the context in which they were obtained. To this end we collect 39 variables that reflect the characteristics of data (e.g. time coverage), specification (e.g. inclusion of long-term interest rates), estimation (e.g. nonrecursive identification), publication (e.g. the number of citations per year), and countries (e.g. the mean share of mortgages with a floating rate in the period for which the impulse response was estimated). To tackle model uncertainty in relating the estimated semi-elasticities to the 39 explanatory variables we employ Bayesian (Raftery *et al.* 1997; Eicher *et al.* 2011; Steel 2020) and frequentist (Hansen 2007; Amini & Parmeter 2012) model averaging. We address collinearity by using the dilution prior (George 2010). The finding of substantial publication bias is robust to controlling for heterogeneity. Regarding data characteristics, our results suggest that studies covering shorter time series tend to produce stronger responses of house prices to monetary shocks (that is, larger semi-elasticities in the absolute value), which is consistent with a small-sample bias. Regarding specification characteristics, we find that the omission of variables related to liquidity (credit or money supply) is associated with stronger responses of house prices to changes in the policy rate. The effect is substantial and can strengthen the reported semi-elasticity by one percentage point. In contrast, we find little evidence that estimation and publication characteristics help explain the heterogeneity observed in the literature.

The factors most useful in explaining the differences in impulse responses are variables reflecting structural heterogeneity: the characteristics of the countries and periods for which the impulse responses were produced. Three variables are especially important. First, it is the degree of development of the mortgage market (and credit markets in general). With larger credit markets in relation to GDP, the transmission of monetary policy to house prices gets stronger. Second, it is the slope of the yield curve. With flatter yield curves, the reported semi-elasticities are larger in the absolute value. Third, it is the period of a prolonged rise in house prices: when house prices have increased for several years, monetary policy becomes more potent at taming them. These country- and time-level characteristics can alter the implied impulse response by up to three percentage points. Therefore while on average house prices do not respond much to monetary policy, policy rates can help alleviate the build-up of housing bubbles in countries with developed mortgage markets during the latter part of the business cycle. Such alleviation is nevertheless costly in terms of inflation and unemployment, because even the most optimistic estimates implied by our analysis for outlying countries and time periods suggests that, after correction for publication bias, a one-percentage-point increase in the policy rate is associated with a maximum decrease in house prices of less than 3%.

3.2 The Semi-Elasticity Dataset

We collect estimates of the effect of changes in the policy rate on house prices. In general, these estimates are produced in the modern literature by two types of models: dynamic stochastic general equilibrium (DSGE) models and vector autoregression (VAR) models. The results of both can be interpreted as empirical estimates, though always conditional on theoretical considerations. DSGE models need to be calibrated (or their priors set), and of course their structure is entirely based on theory. The identification of VAR models, in turn, often has theoretical foundations as well, but in some cases only as an afterthought. Compared to DSGE, VAR models are generally more data-driven, and the corresponding estimates are thus better suited for meta-analysis methods. Moreover, DSGE estimates of the semi-elasticity are relatively rare (a prominent example being Iacoviello & Neri 2010).[4] To avoid comparing apples and oranges, we focus on VAR estimates only. A general structural VAR model has the following form:

$$A_0^i Y_t^i = a^i + \gamma^i t + A^i(L) Y_{t-1}^i + B^i(L) z_t + e_t^i, \tag{3.1}$$

where Y_t^i is a vector of endogenous variables (including policy rates and house prices) for time t and country i, a^i is a constant, $A^i(L)$ and $B^i(L)$ are distributed lag polynomials, z_t is a vector of exogenous variables, and e_t^i is an error term. The set of endogenous variables in a relevant VAR model usually includes output in addition to short-term rates and house prices. Depending on model specification, it may also include other variables, such the exchange rate, consumption, money supply, long-term interest rates, residential investment, and credit. In order to estimate (3.1), researchers rewrite it in reduced form. The principal outputs from VAR models, the reactions of the endogenous variables to structural shocks, are usually reported graphically as impulse response functions, which are easy for the reader to interpret and which cover the response over several time horizons.

To search for relevant studies we use Google Scholar because of its broad coverage and full-text capabilities. (More details on our search strategy, including the exact query, are available in Section B.1.) We calibrate our search query in order to obtain the best known studies among the first hits. We inspect the first 500 papers produced by the search. Following Fabo *et al.*

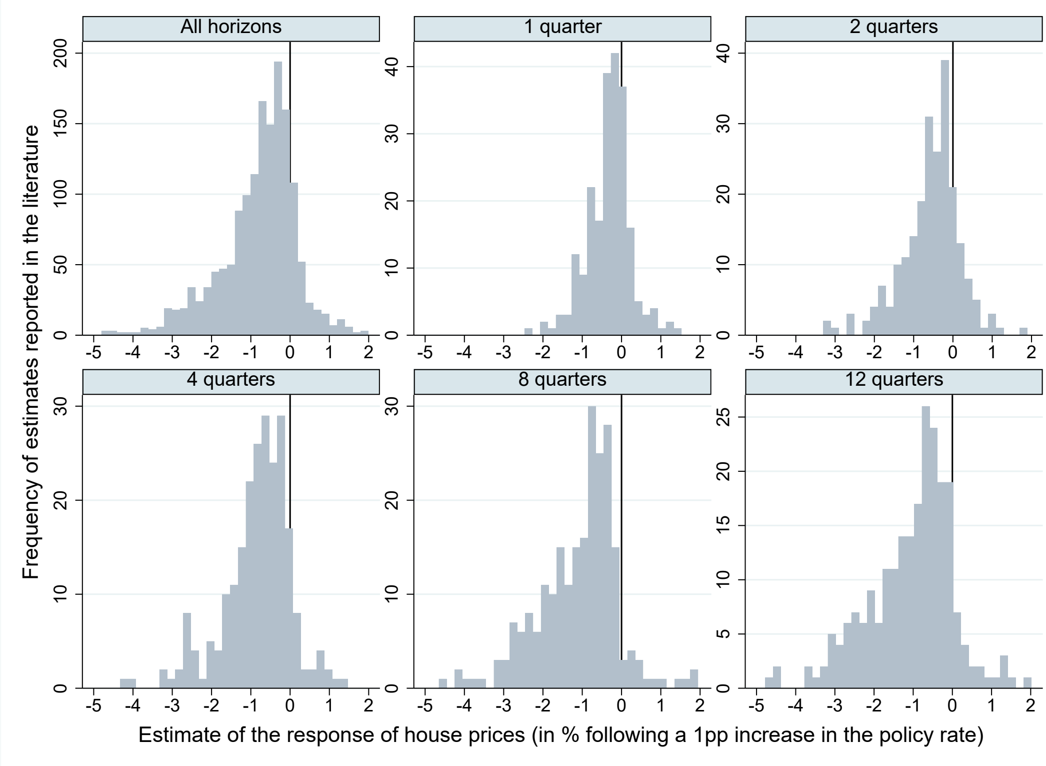
[4] There are only 5 DSGE studies (together providing only 7 impulse responses) that satisfy our inclusion criteria (e.g., report confidence intervals for their impulse responses).

(2021), we also inspect the references and citations of the studies identified by the baseline search. Each study needs to fulfill the following three criteria: First, for quantitative comparability the study must use a VAR model that includes house prices (not house price inflation); we thus cannot use a few influential studies such as Fratantoni & Schuh (2003) and Del Negro & Otrok (2007).[5] Second, monetary policy stance must be proxied by the short-term interest rate. Third, the study must report confidence intervals around the impulse response function so that we can recover the precision of the estimate, which is essential for tests of publication bias. These criteria leave a total of 37 studies, both published articles and working papers (all written in English), which collectively use unbalanced data from 45 countries between 1947 and 2018. We add the last study in April 2022. The list of included studies is available in Table B.1 in Section B.1.

From these 37 studies we collect the responses of house prices to a change in the policy rate after one, two, four, eight, twelve, sixteen, and twenty-four quarters. In each case we measure pixel coordinates using WebPlotDigitizer to recover the numerical estimate as precisely as possible. The extracted values are checked for consistency with authors' verbal assessment of their own findings. In the online appendix we provide an archive of all impulse responses together with the extracted numerical values. We gather 222 and 227 responses after one and two quarters, respectively, and label these as short-term effects. To capture mid-term effects we gather 237 estimates for both the four- and eight-quarter horizons. To capture long-term effects we collect 232 estimates for the twelve-quarter horizon, 226 estimates for the sixteen-quarter horizon, and 174 estimates for the twenty-four-quarter horizon. Because many studies do not report responses at the latter horizon, in the analysis we focus on horizons up to sixteen quarters, and in particular the mid-term effects (four and eight quarters) most relevant to monetary policy. In a few cases, the responses for the short-term effects (one and two quarters) are not reported as the corresponding impulse responses start at the four-quarter horizon. For each impulse response we standardize the effects so that they correspond to a percentage response of house prices to a one-percentage-point increase in the policy rate. We compute the standard error from the reported confidence intervals; in the few cases when

[5] Estimates from 8 studies on house price inflation can be converted to level estimates using the approach outlined in Fabo *et al.* (2021). But because we focus on publication bias, we also need standard errors for these estimates. The standard errors can be approximated only roughly using the delta method, which is why we include the corresponding studies merely as a robustness check in the appendix (Table B.4). The results do not change qualitatively.

Figure 3.2: Reported effects of monetary policy on house prices at different horizons



Notes: Outliers are omitted from the graphs for ease of exposition but included in all statistical tests.

the confidence intervals are asymmetrical, we approximate the standard errors by taking the average of both bounds.

We have already commented on the mean impulse response function, Figure 3.1, in the Introduction. A closer view of the distribution of semi-elasticities at different horizons is provided in Figure 3.2. At the one-quarter horizon, most of the estimates are close to zero, and the distribution is almost symmetrical. With an increasing horizon, the mass of the estimates moves to the left, and the distribution becomes asymmetrical. Note that very few estimates suggest a large response of house prices to changes in the policy rate. A couple of outliers are cut from the figure for ease of exposition (the largest one being -12%), but these are isolated cases. In total for all the horizons, 87% of all the semi-elasticities lie between -2% and 1%. Moreover, more than 50% of all the semi-elasticities lie between -1% and 0%.

In addition to the impulse response functions, we also collect 39 control variables that capture the specifics of each study in order to examine the heterogeneity in the estimates. Slightly fewer than two thirds of the variables included are collected from primary studies themselves, while the remaining third consist of external country-level variables included to examine structural heterogeneity and collected from the World Bank, OECD, and Eurostat. In accordance with the latest meta-analysis reporting guidelines (Havranek *et al.* 2020), the data taken from individual studies (estimates, confidence intervals, and variables reflecting estimation context) were collected by two co-authors of this paper and cross-checked to eliminate potential mistakes arising from manual collection. These variables are discussed in more detail in Section 3.4, which focuses on the heterogeneity in the literature. In the next section we focus on publication bias, which can distort the reported semi-elasticities shown in Figure 3.1 and Figure 3.2.

3.3 Publication Bias

Publication bias is the systematic difference between the distribution of results produced by researchers and the distribution of results reported by researchers (both in working papers and journal articles). Sometimes the bias or its specific forms are also called selective reporting or p-hacking, though we prefer to work with the former, more general term. Whether or not publication bias is universally harmful is still a controversial question. On the one hand, it makes little sense to build a paper on unlikely results, such as those that suggest a rise in house prices following a monetary policy tightening.[6] On the other hand, if such unlikely results are ignored, the literature as a whole gets biased upwards because it is hard to spot large estimates with the right sign and significance that are also due to chance or misspecifications. The resulting tension between the effects of publication selection at the micro and macro level is in the context of vector autoregressions nicely illustrated by the following quote due to Uhlig (2012, p. 38, emphasis added):

> At a Carnegie-Rochester conference a few years back, Ben Bernanke presented an empirical paper, in which the conclusions nicely lined up with a priori reasoning about monetary policy. Christopher Sims then asked him, whether he would have presented the results, had they turned out to be at odds instead. His half-joking reply was, that he presumably would not have been invited if that had been so. There indeed is the *danger (or is it a valuable principle?)* that a priori economic theoretical

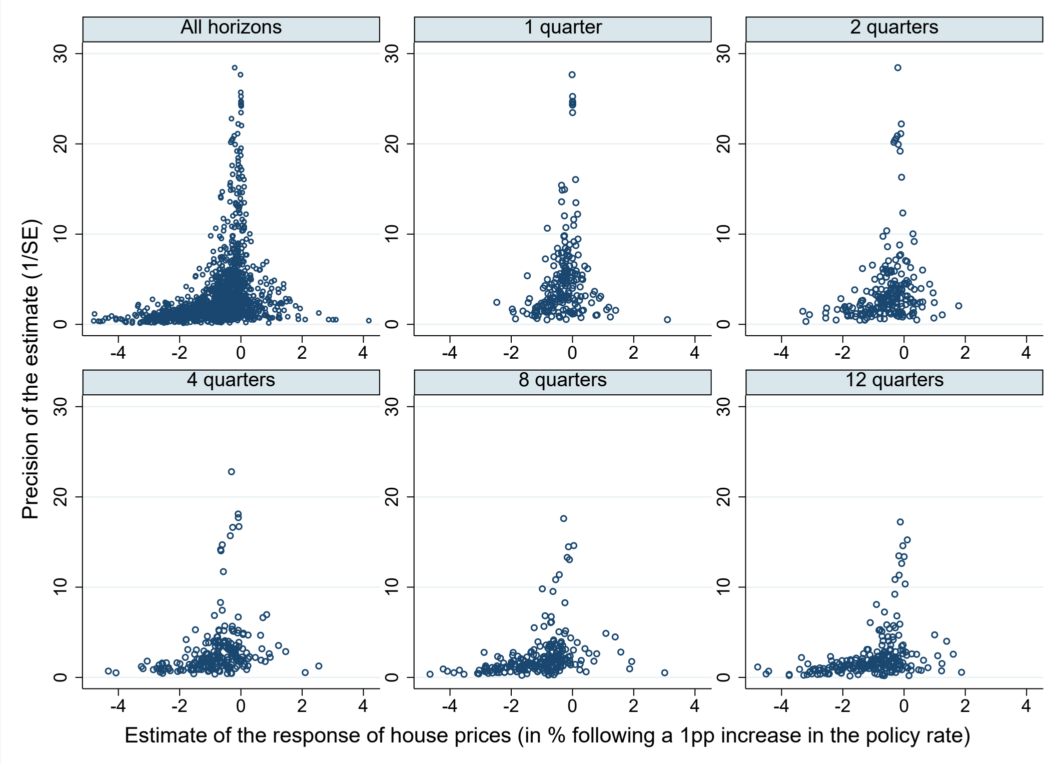
[6] Though such impulse response functions can still be explained, for example, by the theory of rational bubbles, see Galí (2014) and Galí & Gambetti (2015).

biases filter the empirical evidence that can be brought to the table in the first place.

In experimental research, publication bias can in principle be tamed by preregistration (Olken 2015; Strømland 2019), and the American Economic Association has established a registry for experimental papers explicitly to "counter publication bias" (Siegfried 2012, p. 648). Such registries are also common in medical research, where publication bias has long been recognized as a grave problem (Nosek *et al.* 2018), but we are not aware of a field in which publication bias would be extirpated by preregistration. Perhaps publication bias is allowed to survive in many fields because at the micro level of individual studies it can really represent a valuable principle, a specification check that clearly tells the researcher that something is wrong with the model or the data. It is then the task for those who evaluate the literature as a whole to correct for the macro publication bias. As we have noted in the Introduction, our basic identification procedure is based on the Lombard effect. If estimation is imprecise and data are noisy, the researcher will need to try harder to produce estimates that are fully consistent with the intuition and theory—that is, statistically significant negative responses of house prices to a monetary tightening. So we expect more precise estimates to be less biased.

The logic of the identification assumption can be described in a so-called funnel plot often used in medical research. The funnel plot is a scatter plot of estimate size (on the horizontal axis) and estimate precision (on the vertical axis). The most precise estimates will be close to the underlying mean effect, while less precise estimates will be more dispersed, together forming the shape of an inverted funnel. If the mean underlying effect is not zero, the most precise estimates will always be statistically significant and therefore reported. In the absence of publication bias all imprecise estimates will be reported with the same probability. If publication bias is present and the literature as a whole prefers significant negative responses of house prices to a monetary tightening, then given the same precision positive (and small negative) estimates will be reported with a lower probability than large negative estimates, because the latter are more likely to be statistically significant. The funnel plots reported in Figure 3.3 show signs of asymmetry consistent with publication bias. It is interesting to observe that the degree of asymmetry increases as the horizon of the impulse response increases, perhaps reflecting the fact that insignificant estimates are less acceptable at longer horizons.

Figure 3.3: Funnel plots suggest publication bias



Notes: In the absence of publication bias the scatter plots should resemble inverted funnels symmetric around the most precise estimates. Outliers are omitted from the graphs for ease of exposition but included in all statistical tests.

The asymmetry of the funnel can be tested explicitly (Card & Krueger 1995; Egger *et al.* 1997):

$$\hat{x}_{i,j} = \alpha_0 + \beta SE_{i,j} + \epsilon_{i,j}, \tag{3.2}$$

where $\hat{x}_{i,j}$ denotes the i-th estimated effect of interest rates on house prices in the j-th study, and SE is the corresponding standard error. Parameter α_0 denotes the mean effect beyond bias (that is, conditional on infinite precision and thus no publication selection), while β represents the intensity of publication bias. The simple regression has at least two problems (aside from ignoring heterogeneity, which we will address in the next section). First, it assumes a linear relationship between the standard error and the extent of publication bias. But the correlation between bias and precision can vary for different values of precision. When the estimate is very precise, small changes in precision do not alter the intensity of publication selection because they do not alter the designation of statistical significance at standard levels. When the estimate is very imprecise, small increases in precision do not achieve statistical significance and thus do not influence publication probability and selective reporting. It is for intermediate values of precision, and especially around the main threshold for statistical significance, that a relation between estimates and standard errors is more likely.

Second, (3.2) assumes that the standard error is exogenous. The assumption can be realistic in medical research where the standard error is basically given to the researcher (it is computed based on a straightforward formula of the number of observations), but in economics the computation of the standard error is a complex exercise. In any case the standard error is not given but can be influenced by the estimation approach; therefore publication bias can work through both point estimates and standard errors. A related problem is that the standard error itself is estimated, and thus (3.2) suffers from attenuation bias (Stanley 2005).

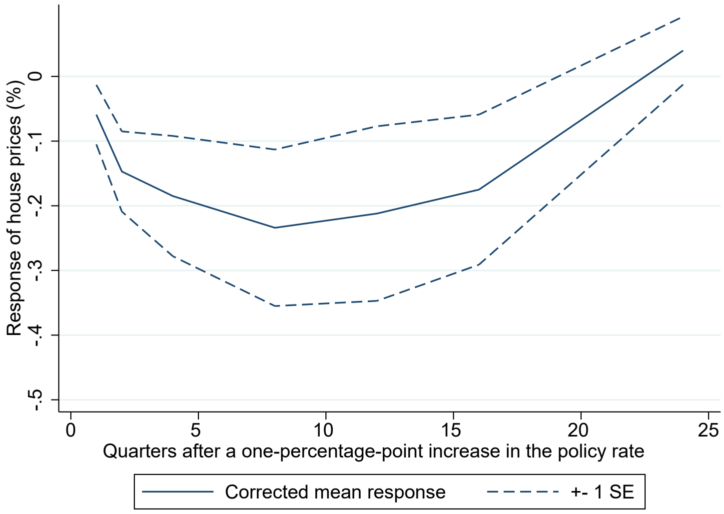
We relax the linearity assumption by employing the stem-based method by Furukawa (2019) and the selection model by Andrews & Kasy (2019). The stem-based method (alluding to the stem of the funnel plot, a portion that includes precise estimates) is a nonparametric approach that optimizes the trade-off between bias and variance. When only the most precise studies are used to compute the mean effect, little publication bias remains, but the variance of the mean estimate increases because it is inefficient to discard information. When less precise studies are included as well, the variance of the mean estimate decreases, but the mean is more contaminated by bias. Furukawa (2019) presents a straightforward way how to weigh these two problems and select the optimal number of most precise studies for the computation of the mean effect. The method rests on minimizing the mean squared error, which can be expressed as the sum of publication bias (squared) and variance; the necessary assumption is that the most precise study does not suffer from any publication bias but is still subject to sampling error. The selection model by Andrews & Kasy (2019) assumes that the probability of reporting for each estimate depends on its sign and statistical significance, with changes in probability at 0 and the main thresholds for statistical significance. The model then re-weights the estimates based on the computed reporting probabilities.

We relax the exogeneity assumption by employing the p-uniform* method by van Aert & van Assen (2021). The method does not assume anything about the relationship between estimates and standard errors but uses the statistical principle that the distribution of p-values should be uniform at the underlying mean effect size. Consequently, it recomputes p-values and searches for the mean value of the semi-elasticity that would be consistent with a uniform distribution of p-values. In addition, in the appendix we use several techniques that are robust to the exogeneity assumption but do not provide estimates of the mean semi-elasticity corrected for publication bias; instead they test for the presence of bias (Gerber & Malhotra 2008a; Elliott *et al.* 2022) or test the null hypothesis that the corrected effect is zero (Simonsohn *et al.* 2014a).

The main results are shown in Table 3.3. Panel A reports the findings of linear models (the regression of estimates on standard errors), while Panel B focuses on nonlinear models. We employ double clustering of standard errors at the level of studies and countries. Because we only have 37 studies in our dataset, we additionally report confidence intervals based on wild bootstrap. In the first part of Panel A we run the regression specified in (3.2), while in the second part we run weighted least squares with weights proportional to the inverse variance of the reported estimates. The weighted specification corrects for the heteroskedasticity inherent in (3.2). In the case of the selection model and p-uniform* in Panel B we need to specify the relevant thresholds for statistical significance. As we have noted in the Introduction, it is common in the VAR literature to use the 68% confidence interval (that is, one standard error on both sides of the mean) instead of the 95% interval common elsewhere in economics. A few VAR studies use the 90% interval, so we set our thresholds for the corresponding values of the t-statistic at 1 and 1.645. Two observations emerge from the table. First, the corrected mean effect is always smaller than the simple mean. Second, the effect at the eight-quarter horizon is usually statistically significant even after correction for publication bias and ranges from -0.40 (selection model) to -0.10 (p-uniform*). The presence of publication bias and significance of the mean effect corrected for publication bias is further supported by robustness checks presented in the appendix that use the techniques of Gerber & Malhotra (2008a), Simonsohn *et al.* (2014a), and Elliott *et al.* (2022).

The weighted least squares specification yields estimates of the mean effect close to the median of those of all the techniques considered, and we use this specification to construct the implied impulse response corrected for publication bias. The response is shown in Figure 3.4 and presents a similar shape to the one discussed earlier in relation to Figure 3.1: house prices decrease swiftly following a monetary policy tightening, the effect peaks after two years and then dissipates. The main difference is the size of the response, which is now much smaller: -0.23% after two years compared to the simple uncorrected mean estimate of -1.2%. Publication bias thus has important quantitative

Figure 3.4: Mean impulse response after correction for publication bias



Notes: Based on the weighted least squares specification reported in Panel A of Table 3.3.

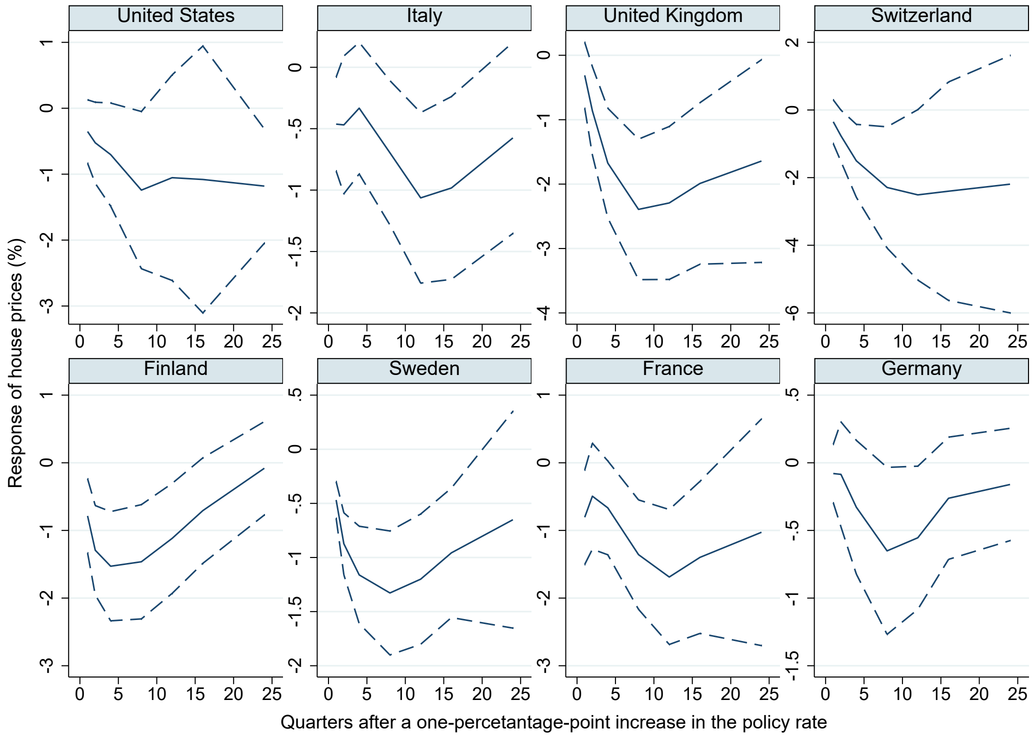
implications for the estimated effectiveness of monetary policy in taming house prices. Nevertheless, the finding of publication bias, and the mean impulse response itself, may be contaminated by the differences in the context in which the estimates are obtained. In the next section we thus turn to the heterogeneity in the estimates.

3.4 Heterogeneity

The previous literature has hinted on the differences in the transmission of monetary policy to house prices depending on the context of countries, time periods, and estimation techniques (among others, Iacoviello & Minetti 2003; Assenmacher-Wesche & Gerlach 2010; Bjørnland & Jacobsen 2010; Calza *et al.* 2013). But these studies could compare only a few countries, a few business cycles, and a few models computed using different specifications. Based on the efforts of these researchers, we build a large database of not only the reported results but also the factors that might have influenced those results. We are thus able to examine the heterogeneity in the response of house prices to policy rate shocks with much more power than the individual studies in the literature.

Consider Figure 3.5, which shows mean impulse responses reported for selected countries. While all the responses are intuitive and none shows the price puzzle, an increase in prices following a monetary policy tightening, the

Figure 3.5: Cross-country heterogeneity in transmission



Notes: The graphs show mean impulse responses reported for individual countries (the solid line) and 68% confidence intervals constructed by adding one standard error to each side of the mean (the dashed lines).

strength and speed of transmission varies greatly across countries. The maximum decrease in house prices following a one-percentage-point increase in the policy rate is −0.6% in Germany but −2.2% in the United Kingdom. In Finland house prices near their maximum response already after 2 quarters and dissipate quickly after two years, while the responses are persistent in Switzerland and the United States. The responses are quite precisely estimated for Finland and Germany, while transmission is uncertain in France, Switzerland, and the United States. In this section we try to explain these and other differences, together with evaluating the robustness of publication bias results to controlling for heterogeneity. Aside from variables that measure the characteristics of countries and the business cycle (what we call structural heterogeneity), we also control for the characteristics of data, specification, estimation, and publication. The definitions and summary statistics for all the variables are available in the appendix; the variables are also briefly summarized below. For simplicity we focus on the four-quarter horizon, which is arguably the most relevant for monetary policy if the central bank intends to defuse a housing bubble in time (the results for the eight-quarter horizon are nevertheless similar).

3.4.1 Variables

Data Characteristics. We control for the characteristics of the data used in the primary studies. First, regarding data frequency, only around 11% of the estimates come from studies that use monthly data; the rest are based on quarterly data. Second, we control for whether simple time series (78% of all observations) or panel data are used in vector autoregressions. We are also interested in whether the strength of transmission changes over time, and we thus include the mean year of the dataset used. By doing so, we control for the potential change in transmission not accounted for by variables capturing structural heterogeneity, which will be described below. We also test whether the length of the sample used in the primary studies systematically affects the estimates.

Specification characteristics. When assessing the effect of monetary policy on the overall price level, Rusnak *et al.* (2013) find that study design has a significant effect on the results. For instance, they find that including output gap as a measure of output or commodity prices besides overall prices systematically affects the results. In a similar way, we create dummies for additional endogenous variables included in VAR models estimating the transmission of monetary policy to house prices. We include a dummy equal to one if the GDP deflator is used instead of the usual consumer price index. Next, we include dummy variables that equal one if a measure of credit (usually real credit to the private sector or mortgage loans) is used (28% of cases), if the long-term interest rate is used (20% of cases), and if consumption, residential investment, the money supply, the exchange rate, and the foreign interest rate are included. We distinguish between nominal and real house prices, though nominal house prices are used in merely around 6% of the studies. We only include studies which use residential house prices, not commercial house prices, land prices, or rent prices. As far as the remaining aspects of the estimation specification are concerned, we control for the number of lags included in the VAR model. The number of lags affects the persistence of the impulse responses and can thus also affect the strength of transmission.

Estimation characteristics. Another important dimension in which estimates differ is the estimation technique. The primary studies typically use a reduced-form VAR employing ordinary least squares or maximum likelihood, and they usually rely on recursive ordering as their identification scheme (76% of all estimates). We control for the use of sign restrictions. Since sign restrictions differ across papers (the restriction may not be imposed on all variables in the same direction), we distinguish between two cases that are important for the transmission to house prices. First, we include a dummy variable equal to one if sign restrictions are imposed on the house price variable, guaranteeing the expected sign. Second, we include a dummy if sign restrictions are imposed on any other variables, but not house prices. We then control for other types of nonrecursive identification (such as long-run restrictions) and, regarding the estimation procedure, we also create a dummy variable that equals one if a Bayesian VAR is estimated (around 15% of the estimates).

Publication characteristics. While the variables introduced above can help us control for some aspects of study quality, other aspects will remain difficult to code or even observe. As additional proxies for quality, we include three publication characteristics. First, we control for the number of Google Scholar citations each study has received on average over the first three years after it appeared on Google Scholar for the first time. This way we take into account the long and variable publication lags in economics, where working papers might accumulate a significant amount of citations even prior to publication, and our chosen measure also accounts for the fact that per-year citations tend to decline as the paper gets older. We also include variables reflecting publication in a peer-reviewed journal and the RePEc discounted recursive impact factor of the outlet. We expect highly-cited studies published in peer-reviewed journals with a high impact factor to be of higher quality than other studies, ceteris paribus. A qualification is of course in order, because any potential correlation between the size of the estimates and the publication characteristics can be also due to publication bias and not necessarily due to genuine systematic effects of (unobserved) study quality on results. One must therefore be cautious with the interpretation of the results related to this group of variables.

Structural heterogeneity. We include a wide range of external variables (marked with the prefix “Country-level”), that is, variables obtained outside the primary studies to cover relevant macroeconomic, financial, demographic, and housing supply factors. For each impulse response, we compute these variables as mean values of the time span used to deliver the particular impulse response for a given country or a group of countries (in which case we weight the individual country-level values by country GDP). First, we include a measure of economic development—disposable income per capita. We also include variables capturing boom and crisis periods. Second, we include interest rate variables, which we suspect may interact with the transmission to house prices.

We control for the level of the short-term interest rate itself: transmission can be more complete at higher ("normal") monetary policy rates, while it can change at low interest rates because of excessive risk-taking by economic agents. On the other hand, very low interest rates or prolonged periods of very low interest rates may cause asymmetries in the transmission. In consequence, a prolonged period of low interest rates fueling credit and house price booms could be mirrored by a stronger reaction of house prices to monetary policy. Long-term interest rates (10-year government bond yields) are more relevant than the short-term rates for the transmission to house prices, and they are often driven by factors independent of the policy rate, such as demographics, inequality, savings glut, the relative price of capital, and amount of public investment (Rachel & Smith 2017). Due to collinearity concerns, we include the term premium (spread) instead the long-term rate per se.[7] We also include the inflation rate in the country: as shown by Rusnak *et al.* (2013), periods of high inflation are often associated with a lower credibility of the central bank and thus weaker transmission.

Third, we control for the characteristics of the lending market by including the credit-to-GDP ratio in order to account for the level of indebtedness as well as for the level of financial development. The inclusion of the mortgage-to-GDP ratio yields similar results, but because the amount of mortgages is unavailable for several countries in our dataset, we use the credit-to-GDP ratio instead to increase the number of degrees of freedom available for our analysis. We also include a variable capturing the share of mortgage loans with floating interest rates: the higher the share of floating-rate mortgages, the stronger the immediate transmission to the overall mortgage interest rate, and possibly the stronger the transmission to house prices in general. For similar reasons we also control for the average maturity of mortgage loans in the country. Fourth, regarding demographic characteristics we account for population growth in the country. If population growth is high, transmission may be weaker as house prices are driven by demographics rather than being affected by monetary policy.

Fifth, we include several characteristics of the housing sector. In order to account for house supply factors, we include the number of building permits. A low number of building permits indicates restricted housing supply and

[7] Though, as noted by Gertler & Karadi (2015), the term premium itself is likely to be influenced by short-term rate changes.

potentially hampered transmission of monetary policy.[8] We also cover the home ownership structure. We include a proxy for tourism as a demand factor rather than a housing supply one. The remaining variables capturing structural heterogeneity relate to house prices themselves. In particular, we include the standardized price-to-income ratio as a proxy for overvaluation of house prices. The price-to-income ratio, available from the OECD database, is measured as the nominal house price divided by nominal disposable income per capita and can be considered a measure of affordability. As another potential proxy to capture overvalued house prices we include a variable capturing the number of periods house price growth is above its long-term average.

3.4.2 Estimation

We intend to find out whether the variables introduced above are systematically related to the reported effects of monetary policy on house prices. The easiest way would be to regress the estimated semi-elasticities on all the variables. But because of the large number of variables (39), such an estimation would be inefficient because many of the variables will probably not belong in the best underlying model. In other words, we face substantial model uncertainty, which is coupled with collinearity. Both can be addressed by Bayesian model averaging (BMA) with a dilution prior. BMA runs many models with different combinations of the explanatory variables and then constructs a weighted average over these models with weights proportional to model fit and complexity. The dilution prior (George 2010) gives each model an additional weight proportional to the determinant of the correlation matrix, so that collinearity is penalized in the final output of Bayesian model averaging. BMA was pioneered in the social sciences by Raftery (1995) and Raftery *et al.* (1997) and recently used in meta-analysis, for example, by Havranek *et al.* (2018a;b;c), Bajzik *et al.* (2020), Cazachevici *et al.* (2020), Havranek & Sokolova (2020), Zigraiova *et al.* (2021), Gechert *et al.* (2022), and Matousek *et al.* (2022). As a robustness check, we use a frequentist alternative (frequentist model averaging, FMA), which is based on Magnus *et al.* (2010) and Amini & Parmeter (2012).

[8]The number of building permits acts as a proxy for housing supply. Other variables could serve this purpose: for example the number of dwellings. Nevertheless, the inclusion of this variable led to collinearity in our dataset. Another candidate variable would be an estimate of the sensitivity of house prices to housing supply. However, we are restricted by availability for our wide cross-country sample. Therefore, we stick to the number of building permits as a house supply proxy often used in the literature (e.g., Grimes & Aitken 2010; Paciorek 2013).

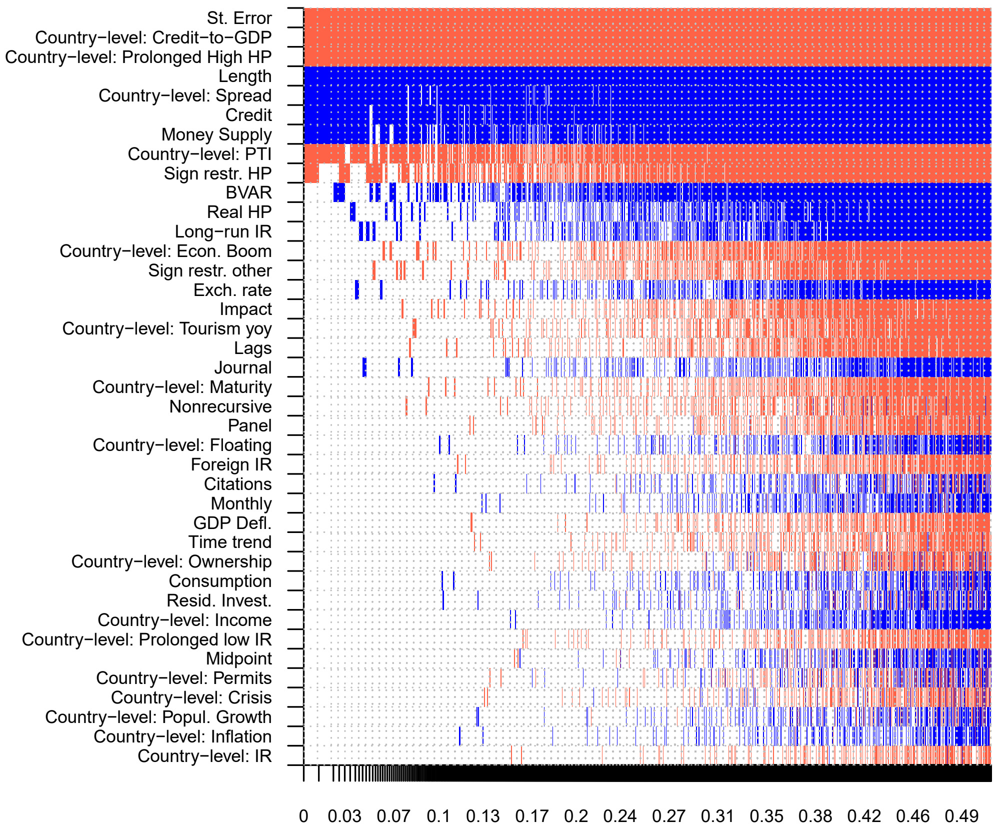
BMA can potentially run 2^{39} regressions with all the possible combinations of variables. Such a computation would take several months, and we avoid it by using the Markov Chain Monte Carlo process and its Metropolis-Hastings algorithm (Zeugner & Feldkircher 2015), which goes through the most probable models. The posterior model probability then expresses the weight of each model. The estimated coefficients for every variable are weighted by the posterior model probability through all the models. 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.

Concerning priors, in the baseline specification the unit information g-prior (UIP) recommended by Eicher *et al.* (2011) gives the prior the same weight as one observation of the data. It constitutes our benchmark setting, addressing the lack of prior knowledge regarding the parameter values. Moreover, the dilution prior addressing collinearity provides us with the benchmark model prior. Aside from the weight proportional to the determinant of the correlation matrix, all models have the same prior probability. As a robustness check of our baseline BMA results, we estimate BMA using alternative g-priors and model priors. We use a combination of the unit information g-prior and the uniform model prior and a combination of the Hannan-Quinn (HQ) g-prior and the random model prior (Fernandez *et al.* 2001; Ley & Steel 2009). As we have noted, we also use frequentist model averaging as an additional robustness check. In FMA we use Mallow's criterion for model averaging (Hansen 2007), and the covariate space is orthogonalized using the approach of Amini & Parmeter (2012).

3.4.3 Results

Figure 3.6 summarizes the results of Bayesian model averaging graphically. Columns denote individual regression models from the best ones on the left, and the variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes the cumulative posterior model probabilities; only the 10,000 best models are shown, which is why the cumulative probability does not run to 1. To ensure convergence we employ 3 million iterations and 1 million burn-ins. Blue color (darker in grayscale) means that the variable is included and the estimated sign is positive, i.e. transmission is weaker. Red color (lighter in grayscale) means that the variable is included and the estimated sign is negative, i.e. transmission is stronger. Blank cells denote exclusion

Figure 3.6: Model inclusion in Bayesian model averaging



Notes: The response variable is the estimated effect of a one-percentage-point change in the interest rate on house prices after four quarters. Columns denote individual models; the variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes the cumulative posterior model probabilities; only the 10,000 best models are shown, and they cover about 50% of posterior model probability. To ensure convergence we employ 3 million iterations and 1 million burn-ins. Blue color (darker in grayscale) = the variable is included and the estimated sign is positive, i.e. transmission is weaker. Red color (lighter in grayscale) = the variable is included and the estimated sign is negative, i.e. transmission is stronger. No color = the variable is not included in the model. The numerical results of the BMA exercise are reported in Table 3.1. A detailed description of the variables is available in the appendix.

Table 3.1: Why reported impulse responses vary

Category	Variable	PIP	Post. mean	Post. SD
Publication bias	**SE**	**1.000**	**-1.540**	**0.141**
Data characteristics	Monthly	0.031	0.011	0.086
	Panel	0.042	-0.006	0.045
	Length	**0.982**	**1.661**	**0.456**
	Midpoint	0.026	0.005	0.054
Specification characteristics	GDP Defl.	0.031	-0.005	0.052
	Foreign IR	0.033	-0.006	0.077
	Credit	**0.868**	**0.415**	**0.218**
	Consumption	0.030	0.002	0.029
	Resid. Invest.	0.029	0.003	0.043
	Money Supply	**0.683**	**0.433**	**0.342**
	Exch. rate	0.113	0.028	0.096
	Long-run IR	0.163	0.065	0.173
	Real HP	0.173	0.088	0.224
	Lags	0.073	-0.004	0.020
	Time trend	0.030	-0.003	0.032
Estimation characteristics	BVAR	0.427	0.267	0.347
	Sign restr. HP	0.491	-0.387	0.453
	Sign restr. other	0.124	-0.067	0.219
	Nonrecursive	0.052	-0.011	0.066
Publication characteristics	Citations	0.032	0.001	0.015
	Impact	0.095	-0.016	0.063
	Journal	0.068	0.014	0.065
Structural heterogeneity	Country-level: Crisis	0.024	0.000	0.004
	Country-level: IR	0.014	-0.001	0.011
	Country-level: Prolonged low IR	0.027	-0.001	0.005
	Country-level: Spread	**0.901**	**0.444**	**0.211**
	Country-level: Floating	0.039	0.000	0.001
	Country-level: Tourism	0.080	-0.001	0.006
	Country-level: Income	0.028	0.013	0.117
	Country-level: Inflation	0.020	0.001	0.010
	Country-level: Credit-to-GDP	**0.998**	**-0.011**	**0.003**
	Country-level: Popul. Growth	0.022	0.002	0.037
	Country-level: PTI	**0.648**	**-0.015**	**0.013**
	Country-level: Prolonged High HP	**0.983**	**-0.108**	**0.028**
	Country-level: Permits	0.024	0.000	0.001
	Country-level: Maturity	0.063	-0.022	0.115
	Country-level: Ownership	0.030	0.000	0.002
	Country-level: Econ. Boom	0.154	-0.007	0.019
	Constant	1.000	-1.553	NA
Observations	225			

Notes: PIP = posterior inclusion probability. SD = standard deviation. Variables with a posterior inclusion probability higher than 0.5 are shown in bold. We employ the unit information g-prior as recommended by Eicher *et al.* (2011) and the dilution prior to address collinearity (George 2010). A detailed description of the variables is available in the appendix.

of the variable. Eight variables are included in most of the best models, which means that these variables are effective in explaining the heterogeneity in the reported semi-elasticities: the standard error (a proxy for publication bias), credit to GDP (a proxy for financial development), prolonged growth in house prices and price-to-income ratio (proxies for the build-up of a housing bubble), length of the time series (a proxy for small-sample bias), the term premium (spread between short- and long-term rates, a proxy for risk-taking and position in the business cycle), and the inclusion of credit and money supply in the VAR (proxies for omitted variables, or alternatively variables introducing a masking problem because the effect of short-term rates on house prices may work through effects on liquidity). The remaining variables have posterior inclusion probabilities below 0.5, which means they are not important in explaining the differences in reported results.

The numerical results of Bayesian model averaging are reported in Table 3.1. The eight variables with posterior inclusion probabilities above 0.5 are shown in bold. The posterior means presented in the table measure the partial derivatives of the reported semi-elasticities with respect to the variables in question. Our results suggest that the finding of substantial publication bias is robust to controlling for heterogeneity. Not only that the variable proves to be important in BMA, but it also has the largest posterior inclusion probability and the estimated coefficient (posterior mean) is larger than that reported in the previous section. We conclude that our previous finding of publication bias was not driven by omitting factors associated with heterogeneity. Next, we find that studies using longer time series are likely to report evidence of weaker transmission from monetary policy decisions to house prices. The result is consistent with a small-sample bias towards more negative semi-elasticities.

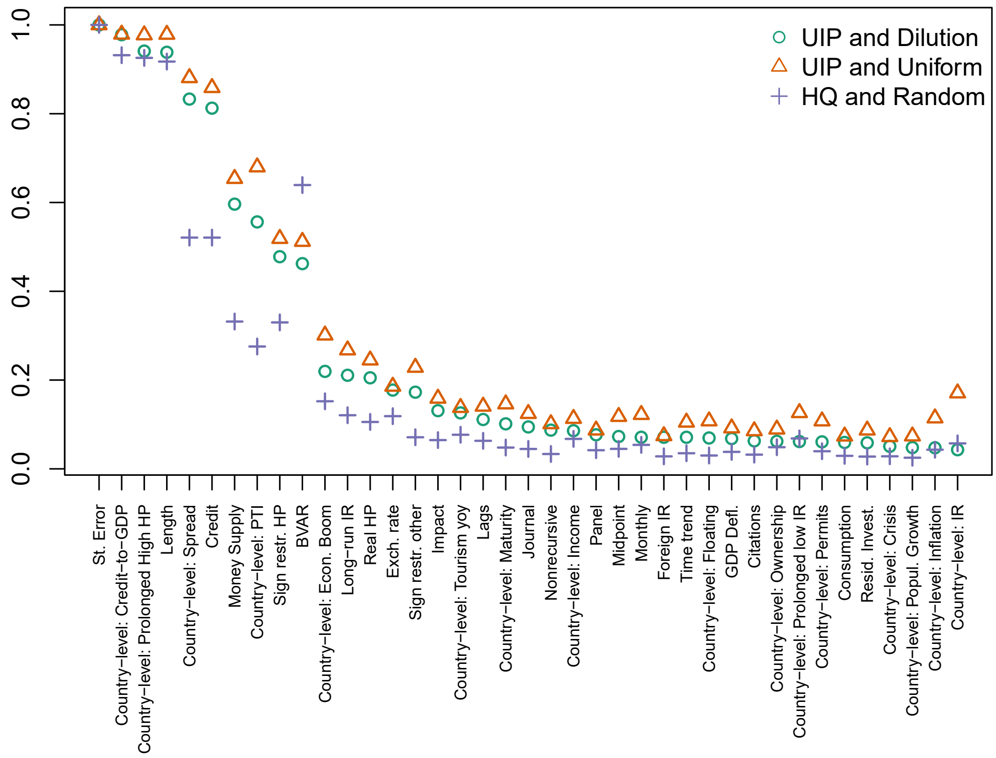
We find that specification characteristics are important for the reported estimates of the semi-elasticity. When credit or money supply are omitted from the analysis, the reported response of house prices tends to be more negative. There are two possible interpretations of the result. First, studies that exclude credit or money supply may suffer from omitted-variable bias, because they ignore the effect that liquidity itself has on house prices. Second, studies that include credit or money supply may suffer from the masking problem, because changes in the short-run rate affect the liquidity variables, which may then appear in a VAR to affect house prices on their own, masking the true causal effect of the policy rate. We also find that studies which put a (negative) sign restriction on the response of house prices tend to find, on average, more negative effects (even though the PIP here is slightly below 0.5). That finding is intuitive because with sign restrictions the price puzzle is a priori impossible. Note that sign restrictions on the response of house prices are used only by 5% of the specifications in our sample, and the results (including those on publication bias) would not be affected by excluding these restricted estimates entirely from our analysis.

Finally, our results suggest that variables reflecting the country's financial system and position in the cycle are important in explaining the reported semi-elasticities. The credit-to-GDP ratio has a PIP of almost 1 and shows a negative correlation with the reported response of house prices. Note that the result would be similar if we used the mortgage-to-GDP ratio instead. We opt for the former because data on the amount of mortgages are not available for every country and period of our dataset, so using mortgages would mean throwing away data. We interpret the finding, in line with Calza *et al.* (2013), as evidence for stronger transmission in countries with more developed mortgage (and, in general, credit) markets.

Next, we find that a flatter yield curve and an ongoing build-up of a bubble in the housing market are both associated with stronger transmission. The result is consistent with monetary policy being more effective at influencing house prices at the latter part of the business cycle, when banks and households are more prone to excess optimism and risk-taking, and adds some credence to the policy of leaning against the wind. In the next subsection, however, we show that even under the best of circumstances the strength of transmission is insufficient to substantially mitigate housing bubbles. Moreover, as documented by Schularick *et al.* (2021), interest rate hikes during a house price boom can often lead to a financial crisis. A crisis precipitates a decrease in house prices, but the overall outcome is of course not what the policy maker had in mind. Unfortunately our dataset does not allow us to fully disentangle VAR evidence on the aforementioned mechanism from soft landings. We partially address this issue by including an interaction of the *Crisis* and *Prolonged high HP* variables (Figure B.5 in the appendix). The interaction is not important in BMA, which is in line with the interpretation that financial crises are not what drives our result that monetary policy becomes more effective at taming house prices after they have increased for several years.

As we have noted, we run several robustness checks to test the robustness of our results. Figure 3.7 shows the posterior inclusion probabilities for individual variables using different sets of priors in Bayesian model averaging. The changes

Figure 3.7: Sensitivity to alternative priors



Notes: UIP = unit information prior; the prior has the same weight as one observation of data. Uniform model prior = each model has the same prior weight. Dilution model prior = the prior weight of each model is proportional to the determinant of the correlation matrix. The HQ prior asymptotically mimics the Hannan-Quinn criterion. The random model prior assign the same prior weight to each model size (e.g., models with 10 variables have the same prior probability as models with 11 variables). PIP = posterior inclusion probability.

are small and would not change our conclusions. In the appendix we show the results of frequentist model averaging, Bayesian model averaging for all semi-elasticities (not just those at the four-quarter horizon), and ordinary least squares regressions for all horizons separately. The results of FMA are broadly consistent with those of BMA, though generally yield less significance (for example, the p-values associated with the variables reflecting the inclusion of credit is 0.2). On the other hand, BMA and OLS results for all semi-elasticities imply more significance for most variables compared to our baseline BMA for semi-elasticities at the four-quarter horizon. In all cases, the finding of publication bias is statistically significant at the 1% level (in frequentist techniques) or has a posterior inclusion probability of 1 (in Bayesian techniques).

3.4.4 Implied Response

As the bottom line of our analysis we compute the impulse response implied by the entire literature but conditional on the absence of publication bias and potential misspecifications. We construct both the mean impulse response for the typical country and also responses for individual countries. In general, our results can be used to derive an implied impulse response conditional on any selected aspect of the financial system, business cycle, and estimation techniques. Technically the implied responses are computed as fitted values using the results of Bayesian model averaging and a definition of the preferred values for each variable included in BMA (or the sample mean if no preference can be made). So we plug in zero for the standard error in order to condition the implied response on the correction for publication bias. While we have noted that the linear correction for publication bias using the exogeneity assumption for the standard error is problematic in theory, we have also shown in the previous section that in the literature on monetary transmission to house prices the linear correction gives results similar to more complex methods. Since it is implausible to use the more complex methods of publication bias correction in BMA, we rely on the linear regression.

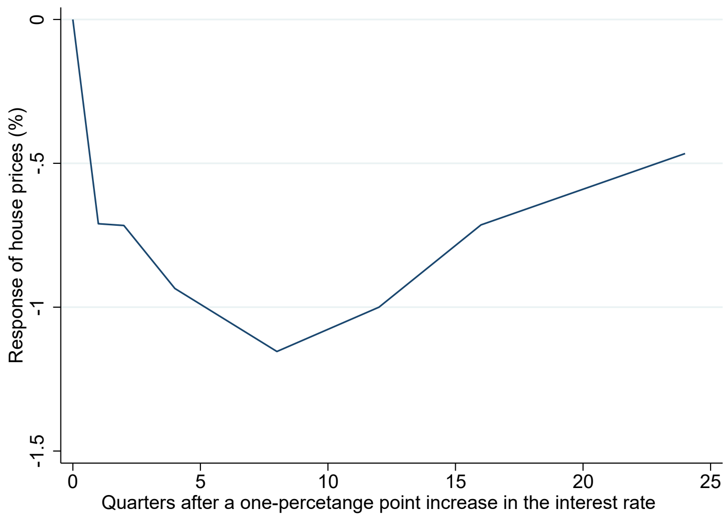
Table 3.2: Implied semi-elasticities

Horizon of the corresponding impulse response:	1Q	2Q	4Q	8Q	12Q	16Q
Agnostic on control variables (baseline)	-0.710	-0.716	-0.935	-1.154	-1.000	-0.714
Control for credit, money supply, and long-run IR	-0.016	-0.023	-0.241	-0.461	-0.306	-0.021
Excluding credit, money supply, and long-run IR	-0.920	-0.927	-1.145	-1.365	-1.210	-0.925
Finland	-0.576	-0.583	-0.801	-1.021	-0.866	-0.581
France	-1.477	-1.484	-1.702	-1.922	-1.767	-1.482
Germany	-0.497	-0.503	-0.722	-0.941	-0.787	-0.501
Italy	0.093	0.087	-0.132	-0.352	-0.197	0.088
United Kingdom	-1.344	-1.351	-1.569	-1.789	-1.634	-1.349
United States	-0.910	-0.916	-1.135	-1.354	-1.200	-0.914

Notes: The values represent the percentage response of house prices to a one-percentage-point increase in the policy rate. They correspond to mean estimates conditional on selected characteristics of the individual studies (see text for more details) and are computed based on fitted values from Bayesian model averaging (for example, by substituting "0" for the standard error, sample maximum for the number of citations, and so on). The estimates for individual countries are based on the baseline definition in the first row.

In order to put more weight on studies that use recent data we employ sample maximum for the variable capturing the mean year of data. Regarding specification characteristics, we prefer if the study uses real house prices (instead of nominal). Considering the controls for the long-term interest rate, credit, and money supply, we have no preference: as we have noted, the inclusion of these variables may create a masking problem, but their exclusion may lead to omitted-variable bias. Regarding estimation characteristics, we prefer Bayesian techniques and nonrecursive identification (structural VAR or sign restrictions). Regarding publication characteristics, we prefer highly cited studies published in peer-reviewed journals with a high impact factor: so we plug in 1 for the dummy variable reflecting journal publication and sample maxima for the number of per-year citations and the RePEc discounted recursive impact factor of the journal. We leave all other variables, including variables capturing structural heterogeneity, at sample means—of course, in the case of impulse responses constructed for individual countries we set the structural variables to the values corresponding to the individual countries.

Figure 3.8: Implied impulse response



Notes: The figure shows the mean impulse response reported in the literature and conditional on preferred aspects of data, methods, and publication. Based on the baseline exercise computed in Table 3.2.

The results are shown in Table 3.2 and Figure 3.8. While the main analysis in this section is based on the four-quarter horizon for ease of exposition, in order to compute the implied impulse response we need to run BMA analyses for each horizon separately. The corresponding analyses are not reported here, but in Table B.7 in the appendix we present the concise results of OLS estimates for each horizon. For each horizon the implied semi-elasticity is computed using the approach described in the previous two paragraphs. Note that confidence intervals or other traditional measures of statistical significance cannot be constructed, because the implied estimates are derived from Bayesian model averaging. The first row of the table shows the baseline implied response, which is also depicted graphically in Figure 3.8. The mean maximum corrected semi-elasticity is -1.2, which suggests, in practical terms, insufficiently strong transmission of monetary policy to house prices—on average at least.

The second row of Table 3.2 presents the results of the same exercise with the exception of the preferred values for specification characteristics. When one gives more weight to the omitted-variables interpretation and believes that liquidity and long-term interest rates should be controlled for in the VAR, the resulting semi-elasticity is much smaller in magnitude (-0.5 after two years). Next, we focus on the masking interpretation, according to which credit, money supply, and long-term rates should not be included in the VAR. The resulting responses of house prices are substantially larger with the semi-elasticity reaching -1.4 after eight quarters. Still the response is not large enough to be of practical importance in taming housing bubbles. It follows that different specification of the VAR model can easily change the estimated response of house prices by around one percentage point. In the remaining rows of Table 3.2 we compute impulse responses for several selected countries. Even the strongest semi-elasticity (-1.9 in France) is insufficient for plausible leaning against the wind when house prices inflation reaches double digits. The weakest semi-elasticity appears again in Italy (-0.4 after two years), which means that country-level characteristics can explain differences of up to 1.5 in the semi-elasticity. The differences explained by country and business-cycle characteristics can rise up to 3 if we select extreme values for these characteristics (not reported in the table). But even the impulse responses implied by the most extreme outliers in the values of these characteristics suggest semi-elasticities above -3.

3.5 Concluding Remarks

We collect 1,555 estimates of the reaction of house prices to a monetary policy shock at different horizons reported in 237 impulse responses from 37 studies. Our results suggest that a one-percentage-point increase in the policy rate is on average associated with a maximum decrease of 1.2% in house prices after two years. We find that transmission varies substantially across countries and time: it is stronger in countries with more developed mortgage markets and in the latter part of the business cycle. But even the most optimistic estimates for the periods and countries with characteristics conducive to more effective transmission imply semi-elasticities of less than 3 in absolute value. So while leaning against the wind may help partly mitigate housing bubbles, the policy rate is a crude instrument for such a task and one costly in terms of inflation and unemployment. Svensson (2017) compares the benefits and costs of leaning against the wind and comes to the conclusion that in most contexts costs outweigh benefits by a large margin. Targeted macroprudential policy tools in the form of binding loan-to-income or debt-service-to-income ratios appear more likely to succeed in steering house prices, although empirical evidence on their effectiveness is still relatively thin (Poghosyan 2020).

Three qualifications of our results are in order. First, in a way unusual but not unheard of in meta-analysis (Fabo *et al.* 2021), we collect data from graphical results (impulse responses and the corresponding confidence intervals). Even though we do our best to codify the numerical values as precisely as possible, a random classical measurement error inevitably arises. In a regression of the estimated semi-elasticity on the corresponding standard error, therefore, the slope coefficient is biased downward due to attenuation bias. Because in our benchmark models the slope coefficient measures the strength of publication bias, many of our estimations are likely to underestimate the effects of the bias and hence produce conservative corrections. In fact, however, the problem with measurement error is more benign in the synthesis of graphical results than in the traditional synthesis of numerical results. The reason is that numerical results are rounded. Because different studies round differently, measurement error might not be random across studies. Bruns *et al.* (2019) show that rounding can create a false impression of publication bias (for example, the clustering of t-statistics at integers such as 2).

Second, the baseline meta-analysis models that we use come from or are inspired by medical research. In medical research, it is common to assume that the standard error is given to the researcher, often directly proportional to the number of subjects. That is, the standard error is exogenous and in the absence of publication bias there should be no correlation between estimates and standard errors. But in economics the computation of the standard error forms an important part of the exercise: in the VAR literature, for example, the confidence intervals can be constructed using different bootstrapping approaches, and different estimation techniques will generally yield different intervals. It follows that publication bias can also work via unintentional manipulation of the reported precision, not only the reported point estimate as is commonly assumed in meta-analysis. One solution is to use a function of the number of observations as an instrument for the standard error, but in the VAR literature the instrument is weak. We thus employ the new p-uniform* technique (van Aert & van Assen 2021) developed in psychology, which uses the distribution of p-values and assumes nothing about the relationship between estimates and standard errors. As robustness checks we also use the techniques by Gerber & Malhotra (2008a), Simonsohn *et al.* (2014a), and Elliott *et al.* (2022) that too do not need the exogeneity assumption.

Third, in this meta-analysis we ignore the growing literature on the effects of unconventional monetary policy on house prices (see, for example, Rahal 2016; Lenza & Slacalek 2018; Rosenberg 2019). While the short-term policy rate appears to have only limited influence on house prices, other tools of monetary policy (such as quantitative easing) might have played a more prominent role recently. Indeed, our results indicate that controlling for liquidity reduces the reported effects of policy rates on house prices, which suggests that tools which primarily affect liquidity can be important. But the studies focusing on unconventional policy are quantitatively incomparable with the rest of our sample, and we believe they are best analyzed separately in a future research synthesis. The literature also lacks a thorough synthesis on the effects of macroprudential policies on house prices. As the body of relevant empirical research grows, conducting a meta-analysis will soon be possible in both realms.

Table 3.3: Linear and nonlinear tests suggest publication bias

Time after a monetary policy shock:	1 quarter	2 quarters	4 quarters	8 quarters	12 quarters	16 quarters
PANEL A: Linear models						
Regression of reported estimates on their standard errors, ordinary least squares						
Standard error (publication bias)	-0.815*	-1.117***	-1.353***	-1.160***	-0.667**	-0.375*
	(0.463)	(0.367)	(0.427)	(0.308)	(0.317)	(0.215)
	[-1.220, -.015]	[-1.987, -.351]	[-2.373, -.410]	[-2.051, -.150]	[-1.542, .009]	[-1.281, .163]
Constant (corrected mean effect)	-0.014	-0.020	-0.015	-0.233	-0.501**	-0.560***
	(0.168)	(0.185)	(0.248)	(0.219)	(0.252)	(0.201)
	[-.213, .145]	[-.417, .479]	[-.576, .739]	[-.790, .444]	[-1.072, .154]	[-.968, -.039]
Regression of reported estimates on their standard errors, weighted by inverse variance						
Standard error (publication bias)	-0.705***	-0.874***	-1.092***	-1.160***	-0.964***	-0.732***
	(0.179)	(0.147)	(0.203)	(0.204)	(0.234)	(0.199)
	[-1.228, -.393]	[-1.207, -.586]	[-1.555, -.730]	[-1.629, -.710]	[-1.534, -.497]	[-1.343, -.337]
Constant (corrected mean effect)	-0.059	-0.147**	-0.185**	-0.234*	-0.212	-0.175
	(0.046)	(0.062)	(0.093)	(0.121)	(0.135)	(0.116)
	[-.058, .014]	[-.303, .081]	[-.360, .130]	[-.496, .044]	[-.521, -.030]	[-.533, -.010]
PANEL B: Nonlinear models						
Stem-based method (Furukawa 2019)						
Corrected mean effect	-0.017	-0.192***	-0.318***	-0.324*	-0.172	-0.146**
	(0.053)	(0.074)	(0.116)	(0.185)	(0.145)	(0.071)
Selection model (Andrews & Kasy 2019)						
Corrected mean effect, break at $t = 1.645$	-0.004***	-0.155*	-0.361***	-0.404**	-0.205	-0.065
	(0.002)	(0.094)	(0.079)	(0.190)	(0.187)	(0.115)
Corrected mean effect, break at $t = 1$	-0.023	-0.008	-0.213**	-0.149	-0.182**	-0.030
	(0.027)	(0.234)	(0.130)	(0.194)	(0.110)	(0.114)
P-uniform (van Aert & van Assen 2021)*						
Corrected mean effect, break at $t = 1.645$	-0.133***	-0.121***	-0.140***	-0.134***	-0.118***	-0.095***
Corrected mean effect, break at $t = 1$	-0.072***	-0.091***	-0.103***	-0.098***	-0.093***	-0.075***
Observations	222	227	237	237	232	226

Notes: The mean uncorrected effect at the 8-quarter horizon was -1.2. Standard errors, clustered at the level of studies and countries, are depicted in round brackets; confidence intervals from wild bootstrap are in square brackets. The p-uniform* method reports p-values, which are all below 0.001 and thus not shown in the table. The selection model and p-uniform* require specifying the break corresponding to a publication selection rule. The wild bootstrap (Cameron *et al.* 2008) is implemented via the *boottest* package in Stata (Roodman *et al.* 2019). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Chapter 4

The Effect of Higher Capital Requirements on Bank Lending: The Capital Surplus Matters[1]

The existing literature has displayed mixed results in terms of the relationship between tighter bank capital regulation and lending, which may be due to poor approximation of capital requirements. We emphasise the crucial role of the excess of bank capital over the minimum capital requirement, the capital surplus, in the transmission of more stringent capital regulation. Specifically, we explore the effect of higher capital requirements on bank credit growth in the Czech Republic, drawing on a unique confidential bank-level dataset. Our results indicate that higher additional capital requirements have a negative effect on the credit supply of banks maintaining lower capital surplus. We estimate the effect on annual credit growth to be between 1.2–1.8 pp, using a wide range of model specifications and estimation techniques. Furthermore, the relationship between the capital surplus and credit growth proves to be significant also at times of stable capital requirements, i.e., the capital surplus does not serve only as an intermediate channel of higher capital requirements.

[1]The paper was coauthored with Simona Malovana and published in Empirica (2022, 49, pp. 793-832). The authors thank Magda Pečená, Lukáš Pfeifer, Katalin Varga, Václav Brož and two anonymous referee for useful comments. The paper was presented at Czech Economic Society and Slovak Economic Association Meeting 2019 and 8th International Conference on Opportunities and Challenges in Management, Economics, and Accounting, 2021. The paper was awarded Frantisek Vencovsky Prize. The work was supported by the Charles University Grant Agency (GAUK No. 9004180).

4.1 Introduction

The importance of quantifying the relationship between banks' capital, capital requirements and lending has been one of the most important research questions for almost two decades. The topic has received greater attention following the wake of the Global Financial Crisis (GFC) of 2007–2009 and then again in the light of increasing use of macroprudential policy instruments. However, the literature has not been entirely successful in consistently quantifying the relationship and it has displayed mixed results in terms of the estimated coefficients (see, for example, Malovaná *et al.* 2021).

In principle, banks can react to higher capital requirements in several ways, depending on their overall capitalisation. On the one hand, a bank can use capital in excess of the minimum capital requirement – the capital surplus – to cover higher capital requirements. Under such circumstances, the impact on the credit supply would be limited. However, even banks maintaining sufficiently high capital surplus can be expected to change their lending behaviour to some extent. Banks face internal costs of funds, or implicit costs of funds, which are set on a consolidated basis. Further, banks often set up internal capital ratio targets above the minimum required level (Berrospide & Edge 2010b; Malovaná 2017). On the other hand, if the capital surplus is not sufficiently high, a bank is expected more likely to dampen its lending activity or change the risk composition of its portfolio. Another way to satisfy higher capital requirements would be to raise equity, for example, by raising stated or issued capital, increasing interest rate margins or postponing dividend payouts.[2]

One of the crucial factors influencing the particular way a bank chooses to adjust its capital adequacy ratio is the state of the economy and the prospects for the near future (Brei & Gambacorta 2016). Under favourable economic conditions, banks may be more likely to increase their capital adequacy ratios through higher interest rate margins or by issuing equity, while in worse economic conditions, they may prefer to shift their asset structure towards a less risky composition (for example, government securities) or to reduce their total exposures (Dahl & Shrieves 1990; Jackson 1999; Heid *et al.* 2004; Brei & Gambacorta 2016).

[2] The importance of capital surplus from a theoretical point of view was discussed in a model by Goel *et al.* (2020), who study how a bank allocates capital across its business units when facing constraints. For example, if a capital constraint tightens because of, for example, stricter regulation, capital flows to the more efficient unit, i.e. the unit offering a higher marginal return on required capital, causing spillovers between banks' business units.

In this paper, we study the impact of higher capital requirements (capital buffers and Pillar 2 add-ons) on bank lending in the Czech Republic, drawing on a unique supervisory panel dataset. The detailed information on individual banks allows us to take into consideration heterogeneity among banks and to control for different effects with respect to banks' characteristics. The Czech National Bank (CNB) is a macroprudential authority responsible for setting capital requirements. Since 2014, the CNB has applied three capital buffers – a conservation buffer, a systemic risk buffer and a countercyclical capital buffer – and an additional Pillar 2 requirement.

Our results show that the effect of higher capital requirements is negative and significant across various model specifications, with the negative relationship being driven primarily by less-capitalised banks. Quantitatively, a 1 pp increase in the capital requirements depresses annual credit growth by 1.2–1.8 pp. Furthermore, we take into account banks' internal capital target and differentiate between intentionally and unintentionally formed capital surplus, showing that the change in capital requirements is transmitted almost exclusively via the intentional capital surplus.

Our paper fits into the broad field of literature on the relationship between bank capital, capital regulation and lending. A noticeable feature of this group of studies is the fact that bank capital can change due to various reasons, ranging from regulatory to economic and managerial. This aspect affects the practical significance of these studies for policymakers, who are primarily concerned with the effects of capital regulation. Unsurprisingly, many studies have focused on analysing the impact of changes in banks' *capitalisation* rather than the *capital requirements* themselves (see, for example, Bernanke *et al.* 1991; Albertazzi & Marchetti 2010; Fonseca *et al.* 2010; Jiménez *et al.* 2017). The reason is usually a lack of observable changes in capital requirements in past data or limited access to such data.[3]

Most of the pre-crisis studies only cover the links between bank lending and capital (not capital requirements) and are mostly focused on credit crunches during the early 1990s crisis period (Bernanke *et al.* 1991; Hancock & Wilcox 1993; 1994; Peek & Rosengren 1995). A pioneering work in the empirical literature examining the nexus between capital and lending is Bernanke *et al.*

[3]Some researchers focus on the overall macroprudential stance (i.e. the mix of macroprudential policies) instead of the capital requirements (see, for example Cerutti *et al.* 2017; Bruno *et al.* 2017; Gambacorta & Murcia 2017; Akinci & Olmstead-Rumsey 2018). In general, their results show that macroprudential policy tightening is associated with lower bank credit growth and house price inflation.

(1991). The authors find that insufficient capitalisation of U.S. banks limited their ability to provide loans, leading to a credit crunch in the early 1990s. As the shortage of bank capital contributed to the emergence of the crisis, the authors coin the term “capital crunch”. The capital crunch is also described by Peek & Rosengren (1995), who formulate a theoretical model stating that banks behave differently if the loss of bank capital results in binding capital requirements as compared to when the requirements are not binding. Other pre-crisis studies include, for example, Hancock & Wilcox (1993) and Hancock & Wilcox (1994), who measure the effect of loan demand and bank capital on credit growth.

The post-crisis empirical literature shows a high degree of heterogeneity in terms of the relationship between bank capital and lending.[4] These differences, however, can be seemingly well explained by the initial choice of the researcher on how to express the bank capital ratio used in the empirical exercise (see Table 4.1). In particular, Malovaná *et al.* (2021) show, in their meta-analytic study, that the relationship between capital requirements and bank lending growth is strongly negative while the effect of higher capitalisation on the extension of loans is positive. Interestingly, they also find a few signs that the relationship between bank capital and lending has changed in recent years. Specifically, a prolonged period of low interest rates is shown to weaken the positive effect, owing to the argument that, in an environment of increasingly demanding bank capital regulation and subdued bank profitability, it may be difficult for banks to maintain voluntary capital buffers and any additional capital requirements may become binding, limiting banks ability to extend additional credit to the economy.

We perceive the contribution of this paper to be twofold. First, we emphasise the crucial role of bank capital surplus in the transmission of more stringent capital regulation. Moreover, we stress the importance of distinguishing between individual banks’ regulatory capital requirements, capital adequacy ratio and capital surplus, which is an important prerequisite for reliably estimating the impact on the growth of loans. Second, we use the detailed supervisory dataset and a wide range of model specifications to provide a comprehensive picture of the transmission.

The remainder of this paper is organised as follows. Sections 4.2 presents the

[4]We discuss predominantly empirical literature; there are also a few theoretical studies building dynamic models and analysing the impact of higher capital requirements. These are, however, less relevant for this paper. We therefore do not mention them, or devote only limited attention to them.

empirical framework and describes the data. Section 4.3 reports the estimation results and section 4.4 concludes.

4.2 Econometric Framework

4.2.1 Data and Measurements

In order to examine the effects of capital requirements on bank lending, we will use confidential bank-level data for the Czech Republic. The data sample consists of 14 banks and bank groups on a consolidated basis,[5] which accounts for almost 90% of the total assets of the whole banking sector as of December 2017. Consolidated bank statements are considered, because banks usually formulate their capital planning strategies at the whole-group level. In addition, the regulatory capital requirements in Pillar 2 are expressed on a consolidated basis. With respect to time span, the sample covers 56 quarters from 2004 Q1 to 2017 Q4, giving an unbalanced panel of 630 observations in total.[6] For part of the analysis, we use a restricted sample starting in 2013 Q1. The evolution of overall capital requirements is depicted in Figure C.1 in the Appendix.

Banks in the Czech Republic maintained their capital adequacy ratios well in excess of the regulatory minimum until 2014. The aggregated capital surplus was CZK 180 billion (8.4% of risk-weighted exposures and 4.3% of total assets, see Figure 4.2(a) and 4.2(b)) at its peak in 2013 Q4. Afterwards, capital requirements stemming from capital buffers and Pillar 2 add-ons were introduced. This led to a decrease in the aggregated capital surplus to CZK 67 billion (2.8% of risk-weighted exposures and 1.1% of total assets) as of 2017 Q4. While the minimum-maximum range is fairly wide (individual banks have held their

[5]At the end of 2017, the Czech banking sector consisted of 19 banks, 5 building societies and 21 foreign bank branches. ICBC Limited and Creditas were excluded from the analysis due to their very short data history; the Czech Export Bank and the Czech-Moravian Guarantee and Development Bank were excluded because they are wholly owned by the Czech state (which provides implicit state guarantees for their liabilities) and have different business models. The foreign bank branches are excluded from the analysis, as they are not subject to domestic capital regulation. Four building societies and two mortgage banks belong to the same bank group as five other domestic banks.

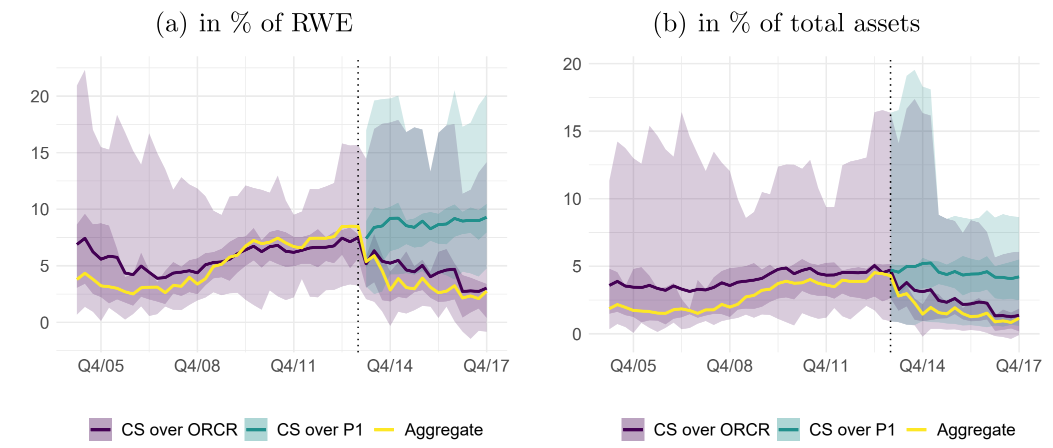
[6]Bank-level data are obtained from the CNB's internal database (FINREP and COREP reporting statements). The capital adequacy ratio was adjusted for outliers, i.e. the unreliably high values of a few small banks in the first few quarters after they entered the market. The capital adequacy ratio of one medium-sized universal bank is adjusted for a structural break in its capital caused by an unusually high dividend payout in 2015; this payout did not constitute a permanent change in the bank's dividend policy, but was a one-time tax-related issue before an IPO.

Table 4.1: The effect of 1pp increase in capital ratio on annual credit growth – literature overview

Article	No. estim.	Mean	Median	Min	Max
Capital-to-asset ratio					
Auer *et al.* (2017)	7	-2.27	-2.27	-2.27	-2.27
Berrospide & Edge (2010b)	2	3.04	3.04	2.11	3.97
Berrospide & Edge (2010a)	2	-2.27	-2.27	-2.27	-2.27
Carlson *et al.* (2011)	17	0.39	0.27	0.05	1.25
Carlson *et al.* (2013)	25	0.38	0.4	-0.04	1.26
Deli & Hasan (2017)	49	0.24	0.24	-0.56	1.08
Galac (2010)	1	3.97	3.97	3.97	3.97
Gambacorta & Marques-Ibanez (2011)	8	0.24	0.37	-0.23	0.78
Gambacorta & Shin (2018)	1	0.60	0.60	0.60	0.60
Kim & Sohn (2017)	12	-0.64	-0.17	-2.27	0.67
Labonne & Lamé (2014)	24	3.76	3.97	2.86	3.97
Malovaná & Frait (2017)	6	0.38	0.08	-2.27	3.97
Mésonnier & Stevanovic (2017)	2	1.66	1.66	-0.65	3.97
Naceur *et al.* (2018)	208	0.11	0.52	-2.27	2.93
Olszak *et al.* (2014)	84	0.09	0.25	-2.27	2.5
Roulet (2018)	48	0.00	0.06	-1.74	1.81
Watanabe (2010)	18	2.13	2.95	-2.27	3.97
Weighted	*514*	*0.89*	*0.53*	*-2.27*	*3.97*
Capital-to-risk-weighted exposures ratio					
Berrospide *et al.* (2016)	4	0.90	1.00	0.40	1.20
Brei *et al.* (2013)	44	0.39	0.70	-2.27	1.57
Carlson *et al.* (2011)	34	0.16	0.10	-0.08	1.10
Carlson *et al.* (2013)	61	0.23	0.20	-0.13	1.54
Cohen (2013)	3	-0.13	-0.28	-2.27	2.17
Cohen & Scatigna (2016)	3	1.03	-0.18	-0.7	3.97
Drehmann & Gambacorta (2012)	4	3.97	3.97	3.97	3.97
Gambacorta & Marques-Ibanez (2011)	8	0.73	0.74	0.27	1.18
Huang & Xiong (2015)	5	-0.35	-0.32	-0.45	-0.21
Kanngiesser *et al.* (2017)	5	-0.33	-0.26	-0.60	-0.22
Kim & Sohn (2017)	66	-0.19	0.07	-2.27	0.68
Kolcunová & Malovaná (2019)	52	0.53	0.21	-0.31	2.39
Košak *et al.* (2015)	96	0.42	0.42	-1.43	1.63
Malovaná & Frait (2017)	4	0.05	-0.04	-0.23	0.51
Mora & Logan (2012)	1	0.20	0.20	0.20	0.20
Naceur *et al.* (2018)	208	-0.07	0.15	-2.08	1.08
Roulet (2018)	48	-0.22	-0.18	-0.84	0.63
Wang & Sun (2013)	6	-0.35	-0.41	-1.03	0.59
Weighted	*652*	*0.44*	*0.20*	*-2.27*	*3.97*
Capital requirements					
Bridges *et al.* (2015)	40	-3.98	-2.23	-11.70	1.10
De Jonghe *et al.* (2016)	68	-1.05	-0.98	-4.45	1.10
De Jonghe *et al.* (2020)	85	-1.69	-1.08	-11.70	1.06
Kolcunová & Malovaná (2019)	22	-0.57	-0.58	-1.75	0.31
Meeks (2017)	14	-0.80	-0.58	-4.36	1.10
Weighted	*229*	*-1.79*	*-1.04*	*-11.70*	*1.10*

Note: The table summarizes a dataset of 1,400 estimates from 32 studies on the relationship between bank capital, capital requirements and lending constructed by Malovaná *et al.* (2021). Each collected elasticity was adjusted to reflect the effect of 1 pp increase in capital ratio on bank annual credit growth. Simple capital-to-asset ratio represents a ratio between bank equity and total assets. Regulatory capital ratio then refers to a ratio between regulatory capital (Common Equity Tier 1, Tier 1 and Tier 2) and risk-weighted exposures. Capital requirements are defined as a ratio between various categories of capital requirements (minimum, Pillar 2 add-ons and capital buffers) and risk-weighted exposures.

Figure 4.1: Capital surplus



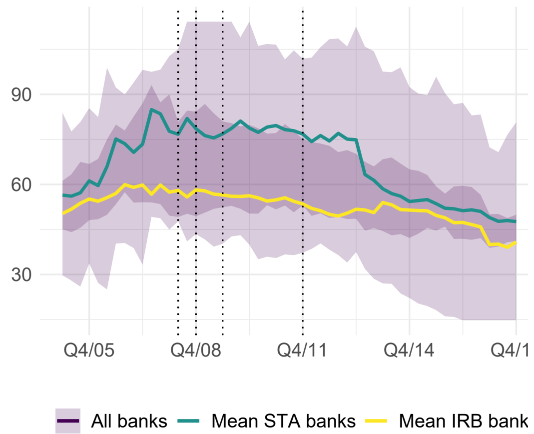
Note: The shaded areas show the min-max and 25–75% intervals; coloured lines are the averages across banks; aggregate line refers to the average over the whole banking sector. RWE stands for risk-weighted exposures.

surplus somewhere between zero and 18% over the last three years), the 25th–75th percentile range is relatively narrow at between 0.4% and 3.4% as of 2017 Q4. The average capital surplus across banks in relation to both risk-weighted exposures and total assets also decreased, reaching 3.0% and 1.3% respectively as of 2017 Q4.

The green area and the green line in Figure 4.2(a) and 4.2(b) show the hypothetical evolution of the capital surplus had no capital requirements been introduced, i.e. the capital surplus over the minimum 8% Pillar 1 capital requirement, holding all else equal. It can be seen that the higher capital requirements have taken a significant part of banks' capital surplus; if they had not been introduced, the hypothetical average capital surplus over the Pillar 1 capital minimum would have reached almost 10% by the end of 2017. However, this additional increase in the capital surplus after 2014 is due to decreasing total implicit risk weights amid a stable or slightly decreasing ratio of capital to total assets.

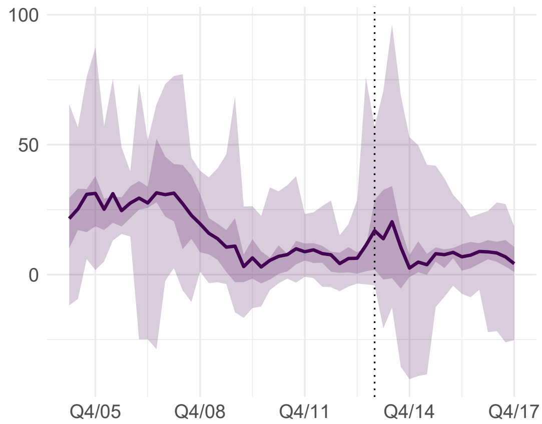
In the Czech Republic, 5 out of 14 banks on a consolidated basis use the IRB approach. Those banks have a combined market share of approximately 80%. They switched gradually to the IRB approach between 2007 and 2011 but have kept some part of their asset portfolio under the STA approach. In terms of total exposures, the transition to the IRB approach was in some cases relatively abrupt and in other cases rather gradual. No bank was using solely

Figure 4.2: Implicit risk weights



Note: The shaded areas show the min-max and 25–75% intervals; coloured lines are the averages. Mean STA and mean IRB refer to banks using solely the STA approach or the IRB approach as of 2017 Q4. Vertical lines – banks' switches to the IRB approach.

Figure 4.3: Annual credit growth



Note: The shaded areas show the min-max and 25–75% intervals; coloured line is the average.

the IRB approach as of 2017.[7] Figure 4.2 shows that the implicit risk weights of banks using solely the STA approach started to decrease slowly a few quarters later than those of banks using the IRB approach. In the case of STA banks, the decline can be explained by a change in the asset structure to less risky. The fall in the implicit risk weights of IRB banks, on the other hand, cannot be explained solely by a change in the asset structure, so migration to the IRB approach also played a role (for a more detailed discussion, see, for example, Malovaná 2018).

As for the credit growth, we use the year-on-year growth of loans to the private sector (excluding interbank loans). We exclude loans to the government and the central bank from our analysis, as they may be influenced by factors that are beyond the scope of this paper (such as the exchange rate commitment of the CNB between 2013 and 2017). Figure 4.3 shows there is significant heterogeneity among banks; credit growth has been significant in the last decade for some of them, but close to zero or even negative for others. Nevertheless, we can see that credit growth has slowed noticeably since 2014 for some banks. A drop in the growth rate is apparent in 2014, i.e. when the capital requirements were introduced. Since then, the average growth rate seems to

[7] While the STA approach takes into account the type of exposure, its external rating and the quality of collateral, the IRB approach is based on the internal ratings set by banks and takes into account the perceived risk of various asset classes in a given economic environment.

have been stable, but the dispersion has decreased significantly, i.e. the growth has continued to slow down after 2014 for some banks.

4.2.2 Main Hypotheses and Methodology

The fragmentation observed in the literature and discussed in the introduction has motivated us to empirically test the following hypotheses:

H1: Higher capital requirements have a direct negative effect on bank loan growth.

H2: Capital surplus plays an important role in the transmission. Specifically, higher capital requirements have a negative effect on capital surplus which translates to reduced credit supply.

H3: Banks overall capitalisation influences the relationship. Specifically, the negative effect of higher capital requirements on bank loan growth is stronger for less-capitalised banks for which tighter capital regulation is more likely to be binding.

Direct effect. In order to test the first hypothesis, we formulate the following baseline equation:

$$\%\Delta loans_{i,t} = \alpha\%\Delta loans_{i,t-1} + \beta ORCR_{i,t} + \gamma X_{i,t-1} + \nu_i + \epsilon_{i,t} \tag{4.1}$$

where $\%\Delta loans_{i,t}$ is the percentage year-on-year change in loans to the private sector excluding interbank loans; $ORCR_{i,t}$ are the overall regulatory capital requirements, consisting of the regulatory capital minimum, capital buffers and Pillar 2 capital add-ons; $X_{i,t-1}$ is a vector of control variables; ν_i stands for bank fixed effects; and $\epsilon_{i,t}$ is the error. We assume that the dependent variable reacts instantly to changes in the capital requirements. The justification of this assumption lies in the fact that changes in the capital requirements are usually announced in advance. Nevertheless, we also test for additional lags and leads.

The control variables in equation (4.1) comprise the usual bank-specific characteristics for credit risk (the ratio of loan loss provisions to assets) and leverage (the ratio of capital to total assets). Second, we assume that the amount of loans is affected by their price; thus, we include a proxy variable for banks' lending rate (the ratio of annualised interest income from loans to total loans). Third, we include real GDP growth, a proxy variable for the business cycle, as banks may expect higher capital requirements in response to the change in general economic conditions. The chosen set of control variables is in line with the bank-capital and bank-lending channel literature, which assumes that certain bank-specific characteristics influence banks' capital ratios, their choice of target capital ratios and their loan supply (see, for example, Malovaná 2017; Brei & Gambacorta 2016; Borio *et al.* 2017).[8] Summary statistics of all variables are provided in Table C.1 in the Appendix.

Indirect effect via capital surplus. Next, in order to test the second hypothesis, we examine in more detail the relationship between the capital requirements, the capital surplus and credit growth, employing simultaneous estimation via a system of equations. In a two-equation model, we assume that higher capital requirements affect bank credit growth indirectly via the capital surplus, defined as the excess of regulatory capital (Tier 1 capital plus Tier 2 capital) over the capital requirements in relation to risk-weighted exposures. While we assume that the capital requirements affect the surplus contemporaneously, the reaction of bank credit growth to the change in the capital surplus is delayed by one quarter:

$$CS_{i,t} = \alpha_1 CS_{i,t-1} + \beta_1 ORCR_{i,t} + \gamma_1 X_{i,t-1} + \nu_{1,i} + \epsilon_{1,i,t} \tag{4.2}$$

$$\%\Delta loans_{i,t} = \alpha_2 \%\Delta loans_{i,t-1} + \beta_2 CS_{i,t-1} + \gamma_2 X_{i,t-1} + \nu_{2,i} + \epsilon_{2,i,t} \tag{4.3}$$

where $CS_{i,t-1}$ is the capital surplus. The set of control variables $X_{i,t-1}$ is different for capital surplus equation (4.2) than the set of control variables for the loan equation.

Capital surplus can be influenced by banks' profitability, credit risk, the macroeconomic situation and the situation on the financial markets. Therefore, we use five control variables: return on assets (ROA, the ratio of net profit to total assets), the ratio of loan loss provisions to total assets, real GDP growth, PX stock index growth and the spread between the 10-year Czech government bond yield and the 3-month interbank rate (3-month Pribor). We also include the control variables capturing banks' financial asset structure and a dummy variable which takes the value of 1 if the bank uses the IRB approach for at least some part of its exposures and 0 if it uses solely the STA approach in the given

[8] In addition, we conducted a few sensitivity analyses including proxy variables for monetary policy and monetary conditions (the 3-month interbank rate, the real monetary conditions index, the estimated shadow rate and the spread described above), but we did not obtain a statistically significant relationship.

quarter.[9] We include those variables in equation (4.2) since the capital surplus depends on risk-weighted exposures (it is the denominator of the formula for the capital surplus).

There are also other hypothetical control variables that could be examined. First, if bank management could observe that the supervisory authority imposed additional capital buffers on other banks, they could predict the imposition on their bank better and react in advance, for example, by increasing equity. Including a control variable for that would likely alter the results. We believe it is not an issue in our setting, as the only individual bank capital requirements are Pillar 2 add-ons and the buffer for systemically important institutions (O-SII buffer). However, neither of these two can be observed by competing banks in advance as their setting is confidential. Further, the set of banks considered to be systemically important is reasonably stable over time, which again limits a strategic advantage for other banks. Second, an internal capital market can play an important role when assessing the significance of capital surplus in the transmission of capital requirements. Therefore, we perform the estimation on the consolidated level in the first place, making the internal capital flows irrelevant for our analysis. Given that large foreign-owned banks in our sample are relatively well capitalised, the relevance of the internal capital market is limited also from the international point of view, i.e. the banks are not likely to need a capital injection from their parent banks during the period examined.

Capitalisation. We define an interaction variable between the overall regulatory capital requirements and a dummy for less-capitalised banks to test the third hypothesis. The single-equation specification is formulated as follows:

$$\begin{aligned}\%\Delta loans_{i,t} = \alpha\%\Delta loans_{i,t-1} + \beta_1 ORCR_{i,t} * dLowCS + \\ + \beta_2 ORCR_{i,t} * (1 - dLowCS) + \gamma X_{i,t-1} + \nu_i + \epsilon_{i,t}\end{aligned} \tag{4.4}$$

where $dLowCS$ is a dummy variable which equals 1 for the five banks with the lowest total capital surplus in the period after 2014, i.e. after the introduction

[9] The transition between the STA approach and the IRB approach can be gradual; in that case, the binary dummy variable might be a reasonable approximation rather than a precise indicator. The use of this dummy is supported by the fact that banks in the Czech Republic in many cases switched abruptly to the IRB approach (in terms of total exposures on a consolidated basis) and only one bank made a gradual transition.

of capital buffers and Pillar 2 add-ons.[10]

We introduce the interaction terms with *dLowCS* also in the two-equation model:

$$
\begin{aligned}
CS_{i,t} &= \alpha_1 CS_{i,t-1} + \beta_1 ORCR_{i,t} * dLowCS \\
&\quad + \beta_2 ORCR_{i,t} * (1 - dLowCS) + \gamma_1 X_{i,t-1} + \nu_{1,i} + \epsilon_{1,i,t} \qquad (4.5) \\
\%\Delta loans_{i,t} &= \alpha_2 \%\Delta loans_{i,t-1} + \beta_3 CS_{i,t-1} * dLowCS \\
&\quad + \beta_4 CS_{i,t-1} * (1 - dLowCS) + \gamma_2 X_{i,t-1} + \nu_{2,i} + \epsilon_{2,i,t} \qquad (4.6)
\end{aligned}
$$

As discussed above, an increase in the capital requirements might be expected to have a limited effect on banks' capital adequacy ratio if banks have a high capital surplus, simply because they would use the extra capital and shrink the surplus. But if banks intentionally target a higher capital adequacy ratio than the level required by their regulator and form an intentional capital surplus – for example in order to match a planned future asset expansion or change in asset structure[11] – higher capital requirements could actually lead them to increase their capital adequacy ratio in an effort to preserve the existing surplus. Therefore, it may be important to distinguish between intentionally and unintentionally formed capital surplus.

We can expect various responses with respect to intentional and unintentional capital surplus and with respect to time. On the one hand, if a bank maintains a sufficiently large unintentional capital surplus, simply due to the long-run accumulation of high earnings, it can use it to maintain its intentional capital surplus. On the other hand, if the unintentional capital surplus is not sufficiently large, the bank may react by increasing its capital adequacy ratio via a combination of the responses listed above. Moreover, if the bank forms an intentional capital surplus in order to match a planned increase in credit supply, then higher capital requirements may slow down or even decrease lending growth via its effect on the intentional capital surplus. The bank may tend to re-build the intentional capital surplus in the long run and to restore

[10] The number of banks characterized as low capital surplus banks was chosen arbitrarily. These banks also exhibit a relatively large change in their average capital surplus in the period of changing overall regulatory capital requirements as compared to the previous period. The results were tested to the inclusion of slightly different number of banks characterized as those with low capital surplus and remained robust.

[11] A bank may also target a higher capital adequacy ratio than that required by the regulator as a consequence of its dividend policy.

the lending growth, as shown, for example, by Bridges *et al.* (2015); Berrospide & Edge (2010b); Adrian & Shin (2010).

In line with the discussion, we further differentiate between intentional and unintentional capital surplus following Malovaná (2017). The author estimates individual bank-specific capital targets for banks in the Czech Republic using a partial-adjustment model. The intentional capital surplus (ICS) is then defined as the difference between the target capital ratio and the overall regulatory capital requirements, while the unintentional capital surplus (UCS) is defined as the difference between the capital adequacy ratio and the target capital ratio.[12] A three-equation system then looks as follows:

$$ICS_{i,t} = \alpha_1 ICS_{i,t-1} + \beta_1 ORCR_{i,t} + \gamma_1 X_{i,t-1} + \nu_{1,i} + \epsilon_{1,i,t} \tag{4.7}$$

$$UCS_{i,t} = \alpha_2 UCS_{i,t-1} + \beta_2 ORCR_{i,t} + \gamma_2 X_{i,t-1} + \nu_{2,i} + \epsilon_{2,i,t} \tag{4.8}$$

$$\%\Delta loans_{i,t} = \alpha_3 \%\Delta loans_{i,t-1} + \beta_3 ICS_{i,t-1} + \beta_4 UCS_{i,t-1} \tag{4.9}$$

$$+ \gamma_3 X_{i,t-1} + \nu_{3,i} + \epsilon_{3,i,t} \tag{4.10}$$

The set of control variables in the equation for the ICS is the same as in the equation for the total capital surplus. On the other hand, the control variables in the equation for the UCS are chosen to capture its different nature. The UCS is assumed to be a result of shifts in accumulated earnings or other factors unintentionally changing the level of capital held, in particular profitability and cost ratios.

4.2.3 Identification

Examining the effect of more stringent capital regulation on bank credit growth is a complicated task that needs to be handled with care. The difficulties stem from the risk of not sufficiently addressing multiple endogeneity issues – in our case, reverse causality (or simultaneity) bias and omitted-variable bias.

Regarding the reverse causality problem, most changes in capital requirements are exogenous because they are dictated by international regulation, which sets the minimum requirements and the upper limits for capital buffers. Moreover, most macroprudential capital buffers are not bank-specific, limiting the concerns for endogeneity related to bank characteristics. Nevertheless, the countercyclical capital buffer is set in response to the position in the economic

[12] For more details on the estimation of the target capital ratio and the intentional and unintentional surplus, see Malovaná (2017).

cycle, which can create a possible concern that past credit growth can explain changes in the stringency of capital regulation. If true, this bias could somewhat inflate our estimated parameters, making them the upper bound of the true relationship. However, we have reasons to believe that reverse causality issues are limited. First, we use a set of different macrofinancial control variables, which should significantly limit the potential bias since credit shocks would impact financial and economic variables with a different lag. Second, the model is estimated at a relatively high frequency (quarterly), while changes to capital regulation attributable to macrofinancial shocks (i.e. changes in countercyclical capital buffer) are less frequent.

We address the omitted-variable bias by considering a dynamic regression specification. Since our panel data units (banks) probably differ systematically in unobserved ways that affect the outcome of interest, we also use bank fixed effects to eliminate all between-unit variation. In addition, we follow Cetorelli & Goldberg (2012), Caglayan & Xu (2016) and Gric *et al.* (2022) and consider several bank-specific characteristics to control for the supply side. We also include demand-side proxies in order to eliminate omitted variable problem as much as possible.

4.2.4 Estimation Techniques

The single-equation specifications are estimated using the standard least square dummy variable (LSDV) estimator and the bootstrap-based bias-corrected (BBBC) estimator proposed by De Vos *et al.* (2015).[13] A dynamic panel is used to control for potential persistence in the relationships. However, as shown by Nickell (1981), there is potential for endogeneity bias in dynamic panels. The Nickel bias is introduced by applying the within (demeaning) transformation in an attempt to remove unobserved heterogeneity in the panel data – subtracting the individual's mean from the relevant variable creates a correlation between the regressor and the error term. Endogeneity bias becomes especially serious in panels with a high number of individuals (large N) and a low number of time periods (low T). This bias, however, shrinks substantially with higher T. Simulations by Judson & Owen (1999) suggest that the bias is minor in panels with more than 30 observations. In our case, the short data sample consists of 14 individuals and 20 time periods, which creates potential for a minor bias.

[13]The estimator is implemented by the *xtbcfe* Stata routine. For more details on the implementation of this routine and a description of the methodology, see De Vos *et al.* (2015).

We use the BBBC estimator, which, as advocated by De Vos *et al.* (2015) and Everaert & Pozzi (2007), is suitable to deal with Nickel bias. Specifically, Everaert & Pozzi (2007) show that for panels with a short to moderate time span, the procedure provides a good alternative for existing dynamic panel data estimators.

A frequently used techniques, the difference-GMM by Arellano & Bond (1991) or the system-GMM by Arellano & Bover (1995), are asymptotically unbiased when cross-sectional dimension N goes to infinity and time dimension T is finite. However, as stressed by De Vos *et al.* (2015), Ziliak (1997) and Bun & Kiviet (2006), GMM estimators tend to have poor small sample properties due to weak instruments (the methods use instrumental variables to control for dynamic panel data bias). Further, as pointed out by Roodman (2009), when T is relatively large compared with N, many valid instruments are available, but the high number of instruments may lead to the GMM estimator being invalid even though instruments are individually valid. Thus, the GMM estimator can be more suitable in case of large N and smaller T, while for smaller N and larger T – which is exactly our case – the BBBC estimator is more suitable.

In addition to the LSDV and BBBC estimators, the two- and three-equation systems are estimated using the three-stage least squares (3SLS) procedure.[14] 3SLS can be interpreted as a combination of two-stage least squares, used to account for the endogeneity of left and right-hand side variables, and seemingly unrelated regression (SUR), used to account for correlation of errors across equations. The reason why we estimate the system of equations simultaneously stems from the potential endogeneity of the variables. For example, equation (4.2) contains different types of loans to control for the bank's asset structure as explanatory variables. In equation (4.3), credit growth depends on the capital surplus, so the capital surplus might well be assumed to be endogenous, i.e. correlated with the error term in equation (4.3). Typically, the endogenous explanatory variables are dependent variables from other equations in the system.

Suggestions whether to estimate two equations separately or jointly differ within the literature with respect to the exact specification and data used. We test for endogeneity using the Hausman procedure, as described in Wooldridge (2015): we save the residuals from the reduced form of equation (4.2) (with all

[14] 3SLS is a default option in the *reg3* STATA command. The command is meant to estimate a system of structural equations where some equations contain endogenous variables among the explanatory variables.

exogenous variables on the right-hand side) estimated as a single-equation fixed-effects regression, and test the significance of these residuals when included as another variable in equation (4.3). The residuals prove to be significant, pointing to a need for two-stage least squares. The covariance between the error terms of the two equations obtained from the variance-covariance matrix is different from zero, pointing to a need for seemingly unrelated regression. In each case, we provide sensitivity checks by estimating the system both simultaneously and equation by equation. The results are mostly similar.[15]

4.3 Empirical Results

We estimate all specifications using a shorter data sample ranging from 2013 Q1 to 2017 Q4, i.e. covering only the period of some variation in capital requirements plus four quarters before. The four additional quarters are being considered because higher capital requirements are announced at least one year before they become effective. In addition, we estimate the two- and three-equation specifications, which include the capital surplus, also on a longer data sample ranging from 2004 Q1 to 2017 Q4. This is possible because there is sufficient variation in the capital surplus throughout the period to aid identification. It can help us to better estimate the transmission of higher capital requirements through the capital surplus and also identify the relationship between the capital surplus and lending driven by factors other than tighter capital regulation. We cannot use the longer data sample for the single-equation model with overall regulatory capital requirements simply because there is not enough variation in earlier periods. For the sake of brevity, we present only selected estimation results while the rest is presented in the Appendix or available upon request.

4.3.1 Direct Effect of Higher Capital Requirements

The effect of higher capital requirements on the credit growth is negative and both statistically and economically significant, regardless of the model

[15]The Hausman test does not reject the null hypothesis of no systematic differences when comparing the OLS, 2SLS and 3SLS estimates, though, suggesting that OLS is both consistent and more efficient than 2SLS. The previous evidence of correlation of errors and the procedure suggested by Wooldridge (2015), however, yields a different outcome. We thus provide both OLS and 3SLS estimates, as well as bootstrap-based bias-corrected estimates, and compare.

specification and estimation technique. Specifically, in response to a 1 pp increase in the capital requirements, the annual credit growth falls by around 0.74 pp (Table 4.2). Given the coefficient estimate on the lagged dependent variable, the cumulative long-run effect is between −5 and −7 pp, depending on the estimation technique.[16] It takes about 5 to 6 years for the initial effect on credit growth to disappear. The cumulative effect after 1 and 2 years is around −2.4 to −3.8 pp.[17]

In terms of the coefficients on the control variables, the lending rate is significant in explaining bank credit growth with a negative sign. There is also a positive and significant effect of the capital-to-assets ratio, indicating that credit growth is higher for banks with a greater amount of regulatory capital. The intuition is the following: a higher capital-to-assets ratio provides more space for balance sheet expansion, while a higher capital requirement, holding the capital-to-assets ratio constant, reduces the capital surplus and thus reduces the space for balance sheet expansion. Moreover, changing the capital requirements while holding capital-to-assets constant is not an unreasonable condition, as we have seen that the effect of the ORCR on the capital-to-assets ratio is almost zero and not statistically significant.

Capitalisation. Even though we cover only a relatively small sample of banks located in one country, there is still noticeable heterogeneity with respect to the capital surplus held (see Figure 4.2(a)). Columns 2 and 4 of Table 4.2 reports the results of Equation (4.4). The relationship between the overall capital requirements and bank credit growth remains statistically significant only for less-capitalised banks, confirming the third hypothesis. In terms of size, the effect for less-capitalised banks is 60% stronger than the effect for all banks.

Different lags and leads. Higher capital buffers (such as the counter-cyclical capital buffer) are usually announced well in advance of them taking effect. On the other hand, Pillar 2 capital add-ons may be announced only a few months before they become effective. However, there may be a phase-in, or transitional, period during which banks are required to fulfil the higher Pillar 2 capital add-ons only partly. Banks can therefore react to the higher additional requirements in advance. They may also react with some delay after evaluating their own situation, the macroeconomic situation and the outlook

[16]The long-run effect is calculated as $\beta/(1-\alpha)$, where β is the coefficient on the overall capital requirements and α is the autocorrelation coefficient.

[17]Estimation results with additional bank-specific and macrofinancial control variables are quantitatively and qualitatively similar (see Table C.2 in the Appendix).

Table 4.2: The effect of higher capital requirements on credit growth

Estimation technique:	(1) BBBC	(2) BBBC	(3) LSDV	(4) LSDV
Credit growth (t-1)	0.852***	0.848***	0.756***	0.749***
	(0.057)	(0.060)	(0.037)	(0.047)
ORCR	-0.737**		-1.027**	
	(0.354)		(0.409)	
ORCR*dLowCS		-1.193*		-1.751***
		(0.674)		(0.576)
ORCR*(1-dLowCS)		-0.488		-0.606
		(0.327)		(0.365)
LLPA (t-1)	0.437	0.496	-0.022	0.166
	(0.575)	(0.542)	(0.270)	(0.263)
CA (t-1)	1.593***	1.449***	1.926**	1.794**
	(0.493)	(0.505)	(0.754)	(0.695)
Lending rate (t-1)	-1.269*	-1.219*	-1.521**	-1.501***
	(0.669)	(0.666)	(0.534)	(0.442)
Real GDP growth	-0.121	-0.104	-0.120	-0.084
	(0.329)	(0.347)	(0.257)	(0.295)
Observations	276	276	276	276

Note: The table presents estimation results of equations (4.1) and (4.4). The data sample covers 20 quarters from 2013 Q1 to 2017 Q4. Bank fixed effects are included. dLowCS – a dummy variable which equals 1 for the five banks with the lowest total capital surplus in the period after 2014, i.e. after the introduction of capital buffers and Pillar 2 add-ons; BBBC – bootstrap-based bias-corrected estimator; LSDV – least square dummy variable estimator with robust (clustered) standard errors. Standard errors are reported in parentheses; ***, ** and * denote the 1%, 5% and 10% significance levels.

for the near future. We, therefore, estimate the relationship between the overall capital requirements and bank credit growth with up to four lags or leads. The complete estimation results are presented in Table C.3 in the Appendix. The effect for banks with higher capital surplus turns out to be not statistically significant, similarly to the previous results. Allowing for lags or leads reveals that banks tend to react at the time when the higher capital requirements become effective, or with a slight delay. The effect tends to be weaker with more lags and turns out to be not statistically significant for leads. The immediate effect, i.e. the reaction in the same quarter, remains the strongest. A richer lag or lead structure is, therefore, not necessary and does not help to explain the variation, as it does not capture the nature of the data.[18]

[18]In addition, we introduce an interaction variable between the overall capital requirements and a dummy variable for four large banks accounting for about 75% of total consolidated banking sector assets as of 2017 Q4; the estimation results, however, remain similar for both groups, i.e. they do not yield any additional information, so we do not report them.

Table 4.3: How the effect changes with different lags and leads

No. of lags (-) or leads (+)	(1) BBBC	(2) LSDV
-4	-0.290	-0.726
	(0.504)	(0.518)
-3	-0.563	-1.097*
	(0.509)	(0.591)
-2	-0.830	-1.367**
	(0.520)	(0.631)
-1	-1.053*	-1.611**
	(0.539)	(0.710)
0	-1.193*	-1.751***
	(0.674)	(0.576)
1	-0.528	-1.071*
	(0.572)	(0.522)
2	-0.255	-0.819
	(0.596)	(0.483)
3	-0.0829	-0.536
	(0.729)	(0.722)
4	0.0697	-0.717
	(0.825)	(0.862)

Note: The table presents estimates of the coefficient on the interaction variable between ORCR and dLowCS (the dummy for banks with low capital surplus). The interaction variable enters the estimation equation with up to four lags or leads. The model also includes the interaction variable between ORCR and (1-dLowCS), which is not statistically significant in either specification. Complete results are given in Table C.3 in the Appendix. The data sample covers 20 quarters from 2013 Q1 to 2017 Q4. Bank fixed effects are included. BBBC – bootstrap-based bias-corrected estimator; LSDV – least square dummy variable estimator with robust (clustered) standard errors. Standard errors are reported in parentheses; ***, ** and * denote the 1%, 5% and 10% significance levels.

4.3.2 Indirect Effect via Capital Surplus

In this subsection, we present estimation results of the two- and three-equation models of higher capital requirements affecting bank credit growth indirectly via the capital surplus. This exercise helps us to gain more information on possible transmission channels. The stronger effect for the less-capitalised banks, identified in the previous subsection, highlights the importance of the capital surplus in the transmission of higher capital requirements.

The results obtained using the system of equations confirm those obtained using the single-equation model (see Table 4.4). Specifically, a 1 pp increase in the capital requirements depresses the total capital surplus by approximately 0.7 pp (regardless of banks' capitalisation; compare columns 1 and 3). As a result, annual credit growth decreases by around 1.5 pp (−0.67 times 2.19), this time only for less-capitalised banks. Similarly to the direct effect, the response of credit growth is not statistically significant for well-capitalised banks, which is in line with what Goel *et al.* (2020) show theoretically in their model. Furthermore, the long-run indirect effect of a 1 pp increase in the capital requirements is an approximately 6.2 pp decrease in credit growth for less-capitalised banks.[19,20]

The effect via intentional and unintentional capital surplus. Higher capital requirements tend to reduce the intentional capital surplus (ICS) and have no statistically significant effect on the unintentional capital surplus (UCS). In particular, a 1 pp increase in the capital requirements leads to a 0.8 pp decrease in the ICS (see Table C.5); this effect is similar to that estimated by Malovaná (2017). While the intentional capital surplus is formed deliberately with respect to asset structure and riskiness, the unintentional capital surplus is a result of temporary fluctuations in banks' profitability[21]; this is supported by the fact that UCS takes both positive and negative values and is much closer to zero (with a mean of 0.5, as compared to 5 for the ICS).

The results also show that the impact on bank credit growth differs for banks with relatively high and relatively low capital surplus. In particular, an

[19]We calculate the long-term impact in this system of equations assuming only first-round effects: β_{CSEq}*$\beta_{LoanEq}/(1\text{-}\alpha_{LoanEq})$.

[20]Estimation results with additional bank-specific and macrofinancial control variables are quantitatively and qualitatively similar (see Table C.4 in the Appendix).

[21]The results show that the unintentional surplus is slightly higher with higher retained earnings and a higher ratio of interest income to assets, although the effect is not significantly different from zero. However, the UCS is significantly lower for IRB banks than for non-IRB banks.

Table 4.4: How important is the capital surplus in transmission – system of two equations

Dependent variable:	(1) CS	(2) Credit growth	(3) CS	(4) Credit growth
Dependent variable (t-1)	0.516***	0.769***	0.519***	0.765***
	(0.040)	(0.0334)	(0.040)	(0.032)
ORCR (t-1)	−0.702***			
	(0.063)			
CS (t-1)		0.197		
		(0.248)		
ORCR*dLowCS			−0.668***	
			(0.084)	
ORCR*(1-dLowCS)			−0.711***	
			(0.066)	
CS (t-1)*dLowCS				2.188***
				(0.445)
CS (t-1)*(1-dLowCS)				−0.236
				(0.251)
ROA (t-1)	−0.035		−0.037	
	(0.170)		(0.172)	
LLPA (t-1)	−0.531***	0.380	−0.532***	−0.053
	(0.106)	(0.654)	(0.106)	(0.629)
Interbank loans/A (t-1)	0.002		0.010	
	(0.036)		(0.037)	
Loans to CB&CG/A (t-1)	−0.008		−0.008	
	(0.011)		(0.011)	
Loans to PS excl. IL/A (t-1)	−0.064***		−0.061***	
	(0.019)		(0.019)	
Bonds/A (t-1)	0.015		0.016	
	(0.017)		(0.017)	
Lending rate (t-1)		−0.853		−0.973*
		(0.526)		(0.505)
CA (t-1)		1.901***		1.674***
		(0.500)		(0.479)
Real GDP growth	0.100*	−0.681***	0.095*	−0.390
	(0.056)	(0.262)	(0.056)	(0.256)
PX growth	0.028***		0.029***	
	(0.011)		(0.0108)	
Spread	−1.058***		−1.077***	
	(0.212)		(0.212)	
IRB dummy	−0.891		−1.373	
	(0.556)		(1.008)	
Observations	276	276	276	276

Note: The table presents estimation results of the system of two equations (4.2)–(4.3) and (4.5)–(4.6). The data sample covers 20 quarters from 2013 Q1 to 2017 Q4. Bank fixed effects are included. dLowCS – a dummy variable which equals 1 for the five banks with the lowest total capital surplus in the period after 2014, i.e. after the introduction of capital buffers and Pillar 2 add-ons. Specifications are estimated using the three-stage least squares estimator (3SLS). Using the bootstrap-based bias-corrected estimator (BBBC) or the least square dummy variable estimator (LSDV) with robust (clustered) standard errors yields quantitatively similar results. Standard errors are reported in parentheses; ***, ** and * denote the 1%, 5% and 10% significance levels.

increase in the capital requirements of 1 pp leads to a decrease in credit growth via the ICS of −1.8 pp for banks with a low capital surplus (−0.76 times 2.39; see Table C.5); the effect is similar but slightly more negative than the effect estimated via the total capital surplus (−1.5 pp; see Table 4.4). The effect via the UCS is not statistically significant, but the link between the UCS and bank credit growth is.

4.3.3 Discussion and Sensitivity Analysis

For ease of comparison, we provide a summary of selected estimation results in Table 4.5. The effect of higher capital requirements is negative across different model specifications. The differentiation of banks based on their overall capitalisation indicates that the negative relationship primarily applies to less-capitalised banks; the impact on well-capitalised banks remains negative but ceases to be statistically significant. Quantitatively, a 1 pp increase in the capital requirements dampens annual credit growth of less-capitalised banks by about 1.2–1.8 pp in the short run and by about 5 to 7 pp in the long run.[22] These numbers are very much in line with those estimated by other studies which rely on similar bank-level data samples (Aiyar *et al.* 2014; Bridges *et al.* 2015). Similarly to us, the authors of both papers explore the effect of higher *capital requirements* rather than higher *capital adequacy ratios*; they find the effect to be between −1 and −8 pp in the short run (depending on the type of loan) and between −6 and −8 pp in the long run. Studies analysing the effect of changes in capital rather than capital requirements usually report a weaker impact on bank provision of loans.

The results using a longer sample are comparable in terms of direction and statistical significance but weaker, given that the true variation in the regulatory capital requirements takes place only since 2014 (see Tables C.6 and C.7 in the Appendix). The relationship between the capital surplus and credit growth, however, remains positive and statistically significant before 2014, as indicated by the estimation results with an additional interaction dummy controlling for the pre- and post-2014 periods (see Table 4.6). In particular, a 1 pp increase in the capital surplus leads to about a 0.6–0.7 pp increase in the credit growth of banks with lower capital surplus before 2013. This suggests

[22] The long-term relationship between higher capital requirements and the capital surplus or credit growth should be taken with caution due to the relatively short time span used in our estimation. The long-term relationship between the capital surplus and credit growth, however, is also estimated using a longer data sample, so these effects are more reliable.

Table 4.5: Selected estimation results – comparing short-term and long-term effects

Table	Specification	Estimation technique	ST effect	LT effect
	Direct effect			
4.2	All banks	BBBC	−0.737**	−4.980
4.2	Less-capitalised banks	BBBC	−1.193*	−4.889
4.2	Well-capitalised banks	BBBC	−0.488	−2.000
4.2	All banks	LSDV	−1.027**	−6.757
4.2	Less-capitalised banks	LSDV	−1.751***	−6.976
4.2	Well-capitalised banks	LSDV	−0.606	−2.414
	Indirect effect			
4.4	All banks	3SLS	−0.138	−1.450
4.4	Less-capitalised banks	3SLS	−1.462***	−6.220***
4.4	Well-capitalised banks	3SLS	0.168	−1.055

Note: The table summarizes the estimation results of 1pp increase in capital requirements on annual bank credit growth. The data sample covers 20 quarters from 2013 Q1 to 2017 Q4. Bank fixed effects are included. BBBC – bootstrap-based bias-corrected LSDV estimator with bootstrapped standard errors; LSDV – least squares dummy variable; 3SLS – three-stage least squares. ST (short-term) effect is the effect in time t for the direct specification and in time t+1 for the indirect specification. LT (long-term) effect is calculated as $\beta/(1-\alpha)$, where β is the short-term effect and α is the autocorrelation coefficient. ***, ** and * denote the 1%, 5% and 10% significance levels.

that the relationship between the capital surplus and credit growth plays an important role in banks' behaviour and does not serve only as an intermediate channel for the transmission of higher capital requirements.

4.4 Conclusions

We explore the effect of higher capital requirements on bank annual credit growth in the Czech Republic, drawing on a unique confidential supervisory panel data set. We emphasise a key role of the capital surplus in the transmission.

The differentiation of banks based on their overall capitalisation indicates that the negative relationship primarily applies to less-capitalised banks. Quantitatively, a 1 pp increase in the capital requirements depresses bank credit growth by about 1.2–1.8 pp. We find a similar effect if we first disentangle the effect of capital requirements on capital surplus – which is always negative – and then estimate the effect of capital surplus on bank credit growth, which is positive. Our results confirm the importance of the relationship between the capital surplus and credit growth. This relationship between capital surplus and credit growth is positive and statistically significant not only in the period

Table 4.6: What was the role of the capital surplus before the tightening of capital regulation

	(1)	(2)
Estimation technique:	BBBC	LSDV
Credit growth (t-1)	0.798***	0.849***
	(0.036)	(0.036)
CS (t-1)*dPostCR*dLowCS	1.633**	1.483**
	(0.585)	(0.580)
CS (t-1)*dPostCR*(1-dLowCS)	−0.073	−0.060
	(0.174)	(0.143)
CS (t-1)*(1-dPostCR)*dLowCS	0.663***	0.565*
	(0.201)	(0.303)
CS (t-1)*(1-dPostCR)*(1-dLowCS)	−0.003	−0.018
	(0.177)	(0.124)
LLPA (t-1)	−0.556	−0.428
	(0.519)	(0.391)
Real GDP growth	0.412**	0.336***
	(0.144)	(0.117)
Lending rate (t-1)	0.647	0.478
	(0.389)	(0.305)
CA (t-1)	−0.270	−0.180
	(0.277)	(0.271)
Observations	630	630

Note: The table presents estimation results of equations (4.4) enriched with dummy variable dPostCR which equals 1 for the period after 2013. The data sample covers 56 quarters from 2004 Q1 to 2017 Q4. Bank fixed effects are included. dLowCS – a dummy variable which equals 1 for the five banks with the lowest total capital surplus in the period after 2014, i.e. after the introduction of capital buffers and Pillar 2 add-ons; BBBC – bootstrap-based bias-corrected estimator; LSDV – least square dummy variable estimator with robust (clustered) standard errors. Standard errors are reported in parentheses; ***, ** and * denote the 1%, 5% and 10% significance levels.

of increasing capital requirements, but also in the period before such changes. The importance of the relationship is confirmed using different methodological approaches and time spans and can therefore be considered robust. Recognising the motives for maintaining capital surplus and its role in the transmission of higher capital requirements may have important implications for policy decision-making. Specifically, additional capital buffers may be tailored to individual banks' capital surplus to a greater extent. Furthermore, we believe that our findings may be applicable to similar countries in the region, given the size and nature of the Czech banking sector and the economy.

It is important to bear in mind that the sample period covers mostly a growing phase of the financial cycle and a build-up phase of capital requirements. Future research could focus on potential non-linearities in the estimated relationship during a less favourable phase of the financial cycle or in response to the release of capital buffers. The role of model-based capital regulation in the transmission would also be worth exploring, particularly the role of variability in risk weights under the IRB approach. Although we provide a somewhat simplified comparison of the short-term and long-term effects, it may be appropriate to re-estimate the relationship as a longer series of changes in capital requirements become available.

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Appendix A

Appendix to Chapter 2

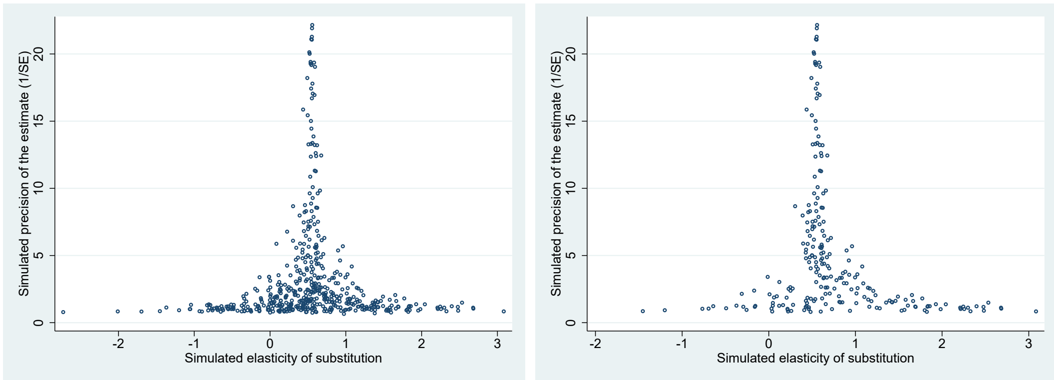
A.1 Illustrating the Effects of Publication Bias in a Monte Carlo Simulation

The impact of publication bias on a naive literature summary and on the working of benchmark meta-analysis tools can be shown in a simple Monte Carlo simulation. We illustrate what happens to the mean of the reported elasticities if some estimates are systematically underreported. To this end we employ the central estimate of the elasticity from Antras (2004), a representative and well-cited study with a point estimate of $\hat{\sigma} = 0.551$ stemming from a specification with FOC on capital, allowing for biased technological change, and relying on US macroeconomic time series data. The estimation equation reads:

$$log(Y_t/K_t) = \alpha + \sigma log(R_t/P_t^Y) + (1-\sigma)\lambda_K \cdot t + \varepsilon_t. \tag{A.1}$$

For the Monte Carlo simulation we assume this estimate to be close to the unbiased true underlying value of σ. (Our results would be qualitatively the same if we chose a different study for the simulation.) We set up our data generating process by re-estimating $\hat{\sigma}$ from 500 draws of the Antras (2004) data by adding noise to the dependent variable $log(Y_t/K_t)$ (with a sample mean of 4.16) via a random error from a Gaussian distribution with $X \sim N(0, Var)$. In order to generate the familiar funnel shape for the scatter plot of estimates and standard errors, the variance Var of the noise term X is chosen not to be constant across draws but to vary from 0.0016 to 0.8. Note that the qualitative results of the simulation are independent of this specific parametrization. The funnel would still display a range comparable to our actual dataset shown in Figure 2.6, though it would look less pretty. The funnel plot from our 500 simulated estimations (noisy versions of Antras's model) is displayed on the left-hand side of Figure A.1. It has an average $\hat{\sigma} = 0.534$ with a standard deviation equal to 0.685.

Figure A.1: Simulated funnel plots without and with publication bias



Notes: In the absence of publication bias the scatter plot should resemble an inverted funnel symmetrical around the most precise estimates. The left panel shows estimates from all 500 Monte Carlo draws obtained from the replication of the estimate in Antras (2004) (Table 5, Column I, Row 1) and by adding random noise to the dependent variable, thereby producing a symmetric funnel around Antras's estimate. The right panel shows what happens to the funnel plot if 80% of estimates that are negative or insignificantly different from zero (at a 5% level) are discarded, which results in retaining only 227 observations.

The right-hand panel of Figure A.1 shows how the funnel would change if we filtered out 80% of the simulated estimates that are either negative or insignificantly different from zero. This setup reflects a typical publication bias scenario in which significant and theory-compliant estimates are more likely to be reported. In this scenario, only 227 observations are left, and the funnel becomes asymmetric. In fact, however, it is less asymmetric that the actual funnel plot we observe in the literature (Figure 2.6), indicating that publication bias may be even more severe in practice than with the aforementioned filter. The filtered simulated dataset represents what a reviewer of the literature observes. Publication bias drives the observed average elasticity upwards from 0.534 to 0.743 and produces a correlation between point estimates and their standard errors, a correlation that was not present before (column 1 in Table A.1).

Table A.1 shows a funnel asymmetry test, a regression of estimated elasticities on the corresponding standard errors (as explained in Section 2.4) for different scenarios of bias. Column 1 refers to the unbiased symmetric funnel in Figure A.1. The test indicates no bias, and the estimated mean beyond bias is close to the true mean. If all negative estimates are dropped (column 2), the naive mean increases to 0.726. The test detects publication bias and uncovers a mean of 0.521, close to the true one. Column 3 refers to the asymmetric funnel in the right-hand panel of Figure A.1. Again the test detects publication bias and estimates the true mean fairly precisely. Columns 4 and 5 show that the working of the test does not hinge on the selection threshold of zero. If for example the Cobb-Douglas specification with $\sigma = 1$ serves as a benchmark for researchers, in the way that they discard 80% of all estimates that are significantly different from 1 at a 5% level, the mean of the reported estimates would also be biased upwards and meta-analysis tests again do a good job in detecting the bias. Even for the extreme example of column 5, where we drop 80% of estimates with $\sigma < 1.3$ and the uncorrected mean increases to 0.913, the funnel asymmetry test estimates the underlying true σ well.

Table A.1: Monte Carlo simulation of publication bias

	(1) no filter	(2) drop < 0	(3) drop 80% of < 0 or insignif $\neq 0$	(4) drop 80% of signif $\neq 1$	(5) drop 80% of < 1.3
$\bar{\sigma}$ (mean)	0.534	0.726	0.743	0.616	0.913
SE (publication bias)	-0.016	0.313***	0.499***	0.135**	0.578***
	(0.053)	(0.049)	(0.096)	(0.057)	(0.102)
Const (mean beyond bias)	0.548***	0.521***	0.515***	0.550***	0.488***
	(0.007)	(0.007)	(0.009)	(0.015)	(0.013)
Observations	500	423	227	391	151

Notes: The table shows detection of and correction for publication bias in five different scenarios. (1) Reporting all estimates. (2) Dropping all negative estimates of σ. (3) Dropping 80% of negative or insignificant (at the 5% threshold) estimates. (4) Dropping 80% of estimates that are significantly different from $\sigma = 1$ at the 5% level. (5) Dropping 80% of estimates that are smaller than $\sigma = 1.3$. The original data were obtained from Antras (2004), the specification FOC_K with trend from Table 5.1, Col I, Row 1. The Monte Carlo simulation adds noise to the dependent variable and estimates Antras's model 500 times. ***, **, and * denote statistical significance at the 1%, 5%, and 10% level. Standard errors in parentheses.

A.2 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, 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:

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

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 non-parametrically 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 an 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:

$$\tilde{Bias}^2(n) = \frac{\sum_{i=2}^{n}\sum_{j\neq i}^{n} w_i w_j \beta_i \beta_j}{\sum_{i=2}^{n}\sum_{j\neq i}^{n} w_i w_j} - 2\beta_1 \frac{\sum_{i=2}^{n} w_i \beta_i}{\sum_{i=2}^{n} w_i}, \tag{A.3}$$

$$Var(n) = \sum_{i=1}^{n} w_i, \tag{A.4}$$

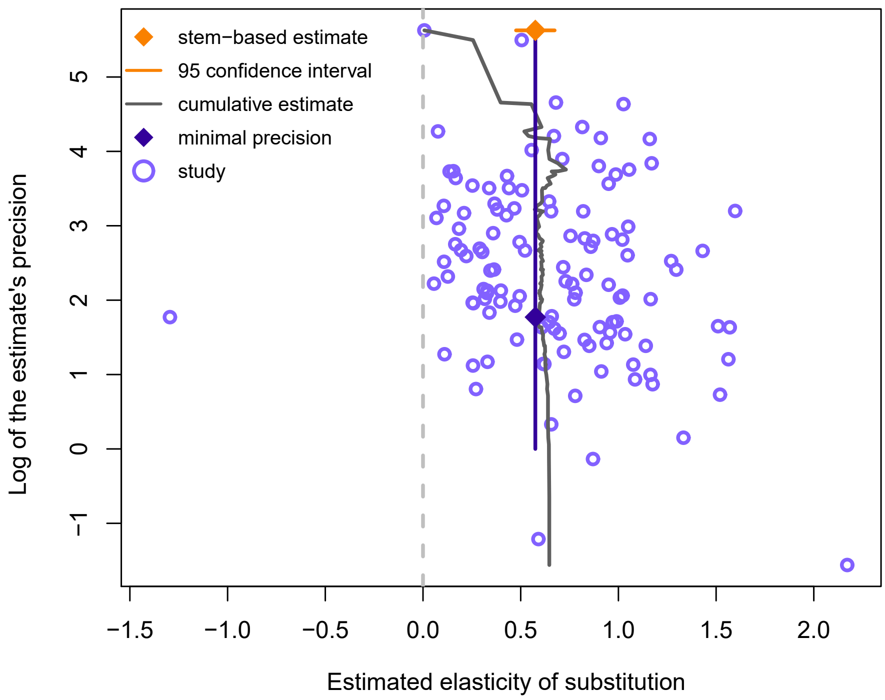
where $w_i = \frac{1}{se_i^2 + se_0^2}$. The optimal number of included studies is given by Equation A.2. The stem-based corrected estimate follows:

$$\hat{b}_{stem} = \frac{\sum_{i=1}^{n_{stem}} w_i \beta_i}{\sum_{i=1}^{n_{stem}} w_i}. \tag{A.5}$$

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

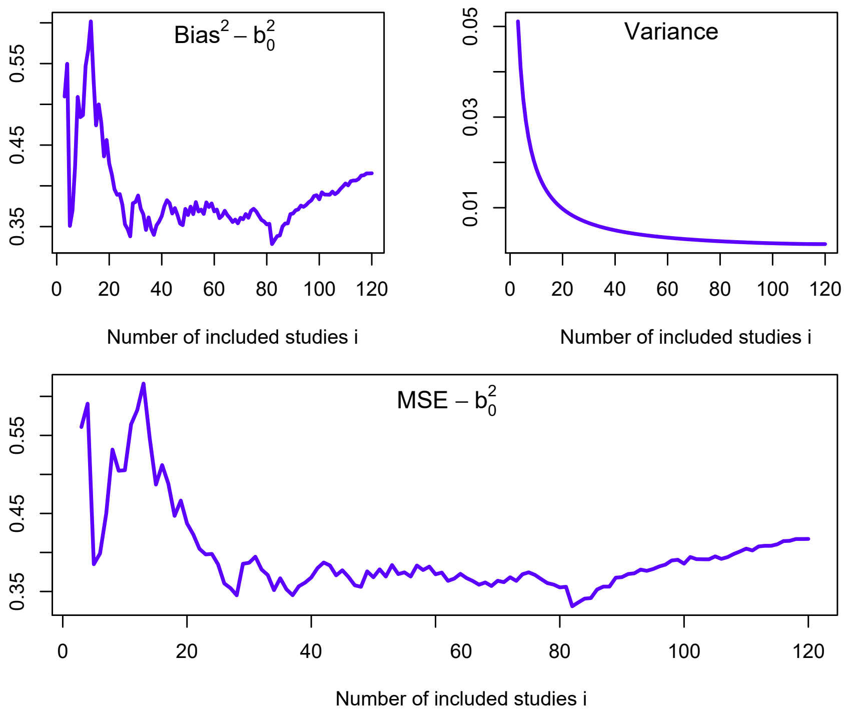
The stem-based method applied to the elasticity of substitution yields the following results: the mean underlying elasticity corrected for publication bias is 0.57 with a standard error of 0.05. Overall, 77% of the total information in the data is utilized, and the 83 most precise studies (out of 121) are included. Because the stem-based method uses study-level estimates (as preferred by Furukawa), we select median values from each study. Figure A.2 visualizes the stem-based bias correction method. Figure A.3 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 A.2: 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 A.3: The trade-off between bias and variance



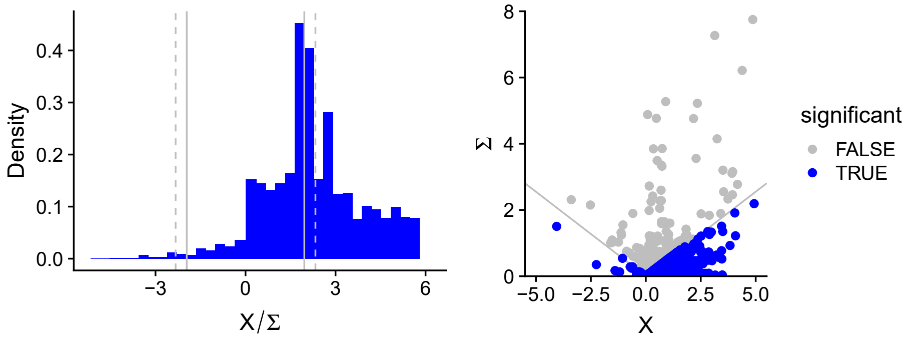
Note: The mean squared error (MSE) is the criterion for choosing the n_{stem}, the optimal number of studies to include in the stem-based estimator. The relevant components of MSE—bias and variance—are plotted.

A.3 Andrews and Kasy's Method for Addressing Selective Reporting

Andrews & 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 & Kasy (2019) identify publication probability similarly to Hedges (1992) using maximum likelihood: 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 & Kasy (2019) yields the following results. The bias-corrected estimate is 0.43 with a standard error of 0.017. We impose a cutoff 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 A.2). 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 A.4.

Figure A.4: 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-statistics 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 panels shows publication selection based on significance is not absolute, as some insignificant estimates (gray points) are reported.

Table A.2: 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

Notes: $\bar{\theta}$ 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.

A.4 Description of Variables

Table A.3: 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 variation at the industry-/sector-level is exploited in input data (reference category: cross-country-/state-level variation).	0.43	0.50
Firm data	= 1 if variation at the firm-level is exploited in input data (reference category: cross-country-/state-level variation).	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

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Table A.3: Definitions and summary statistics of explanatory variables (continued)

Variable	Description	Mean	Std. dev.
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

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Table A.3: Definitions and summary statistics of explanatory variables (continued)

Variable	Description	Mean	Std. dev.
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
Skilled L	= 1 if the production function distinguishes between skilled and unskilled labor.	0.02	0.13
Constant TC growth	= 1 if the technological change is modeled with constant growth rates (reference category: no growth of technology).	0.30	0.46
Other TC growth	= 1 if the 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

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Table A.3: Definitions and summary statistics of explanatory variables (continued)

Variable	Description	Mean	Std. dev.
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 is in an index form.	0.03	0.18
y: other	= 1 if the input data for total output is 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 the labor are measured in hours.	0.25	0.44
L: years	= 1 if the input data for the labor are measured in years.	0.07	0.25

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Table A.3: Definitions and summary statistics of explanatory variables (continued)

Variable	Description	Mean	Std. dev.
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 number of workers labor 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 the 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 total capital stock (reference category: it is 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 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 is measured via 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 category "Materials" of the GICS industry classification.	0.25	0.43

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Table A.3: Definitions and summary statistics of explanatory variables (continued)

Variable	Description	Mean	Std. dev.
Industrials	= 1 if the elasticity is estimated for the 2-digit industry in the category "Industrials" of the GICS industry classification.	0.09	0.29
Consumer	= 1 if the elasticity is estimated for the 2-digit industry in the category "Consumer goods" of the GICS industry classification.	0.14	0.34
Services	= 1 if the elasticity is estimated for the 2-digit industry in the category 'Services' of the GICS industry classification.	0.02	0.15

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

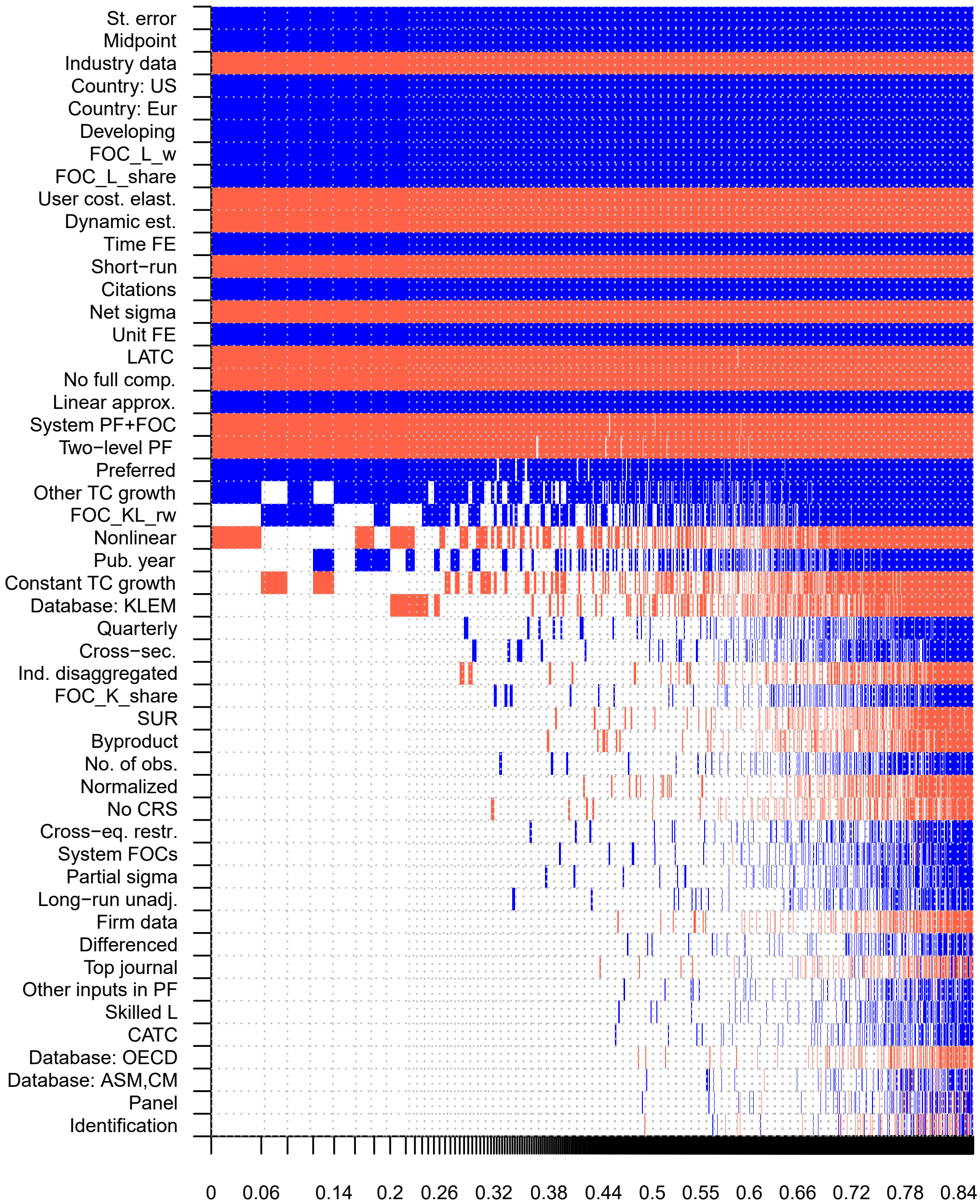
A.5 Robustness Checks

Table A.4: Results of frequentist model averaging (FMA)

	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		

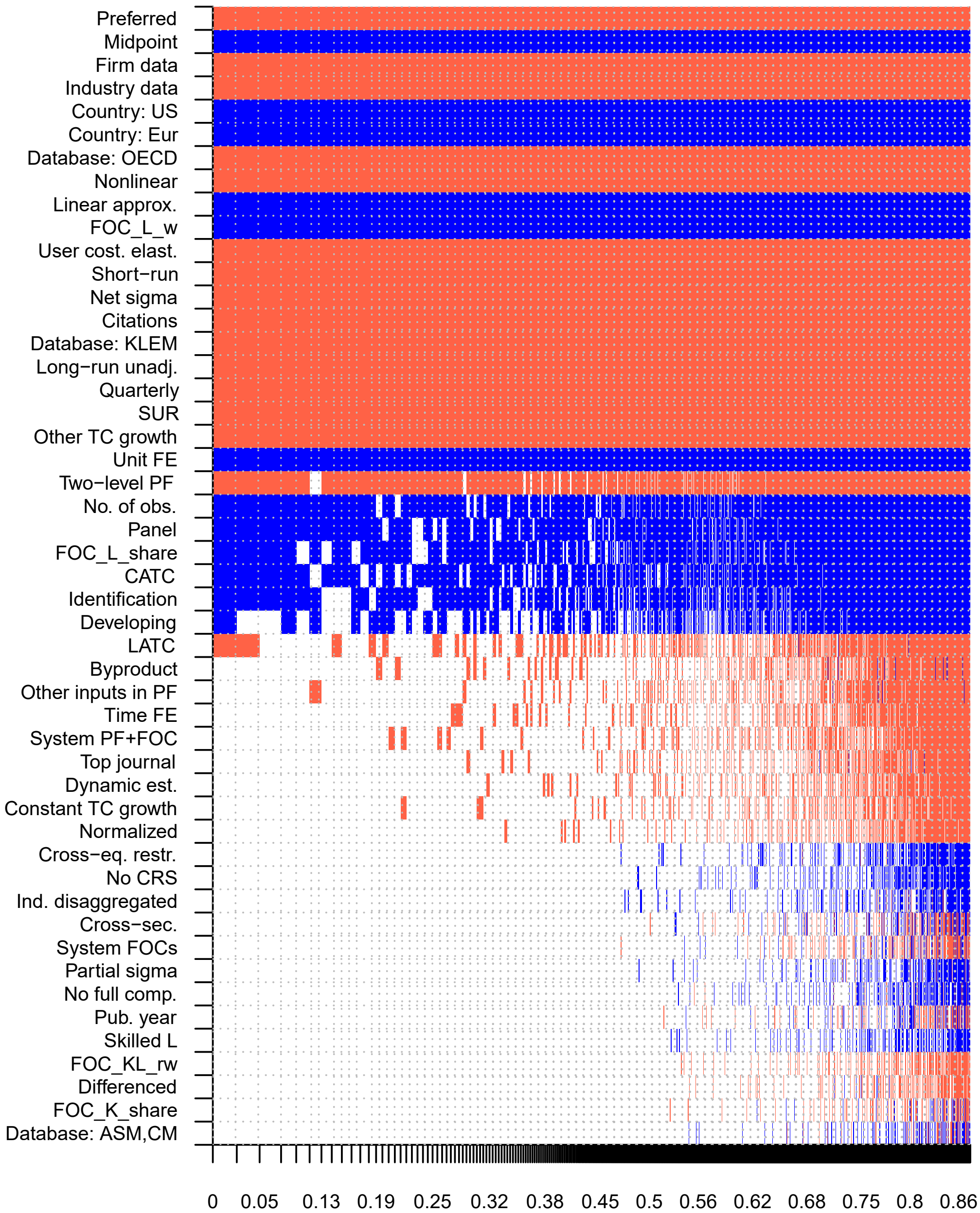
Notes: Our FMA exercise employs Mallow's weights (Hansen 2007) and the orthogonalization of the covariate space suggested by Amini & Parmeter (2012). Dark gray color denotes variables deemed important also in the BMA exercise. Light gray color denote variables important in the FMA but not BMA exercise.

Figure A.5: Model inclusion in Bayesian model averaging, weighted by the inverse of the number of estimates per study



Notes: 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 A.6: Model inclusion in Bayesian model averaging, weighted by the inverse of the standard error



Notes: 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.

A.5.1 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, 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 portion 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 & 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, 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 a difference between total value added and total wages divided by the capital stock; this approach is used in 17% of the cases, for example in Dhrymes (1965), Ferguson (1965), and Lovell (1973a). 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, and thus we do not include the corresponding variable.

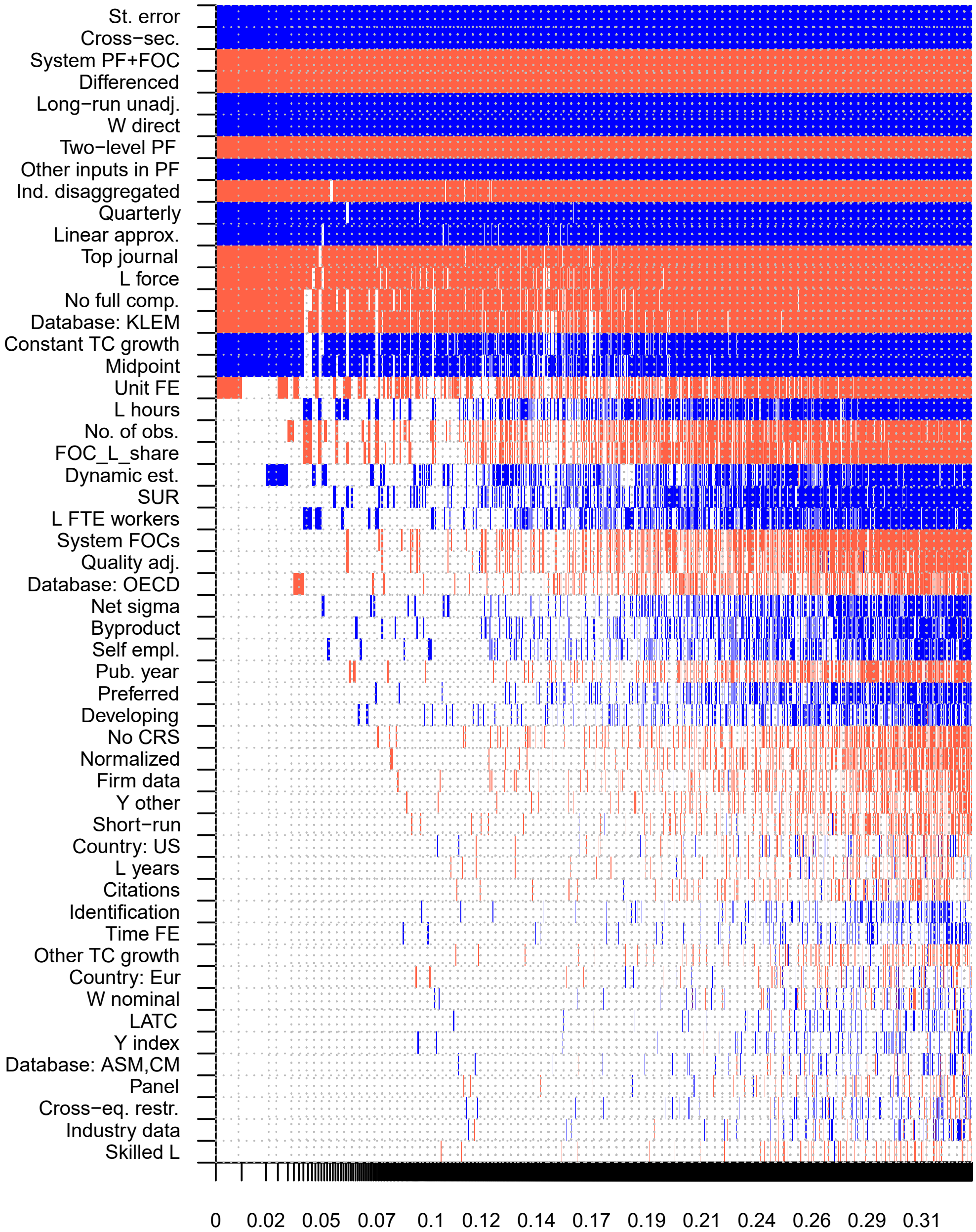
In all subsamples we control for the measurement of output: first, we include a dummy variable that equals one if output is not measured as gross product or in value added terms, but in another way—for example, as the amount of sales. Second, we include a dummy for the case when output is used in an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

How does the addition of these variables affect our results? First, we include labor-specific variables, which capture how labor and 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 A.7. 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 A.8). 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, far from the Cobb-Douglas assumption.

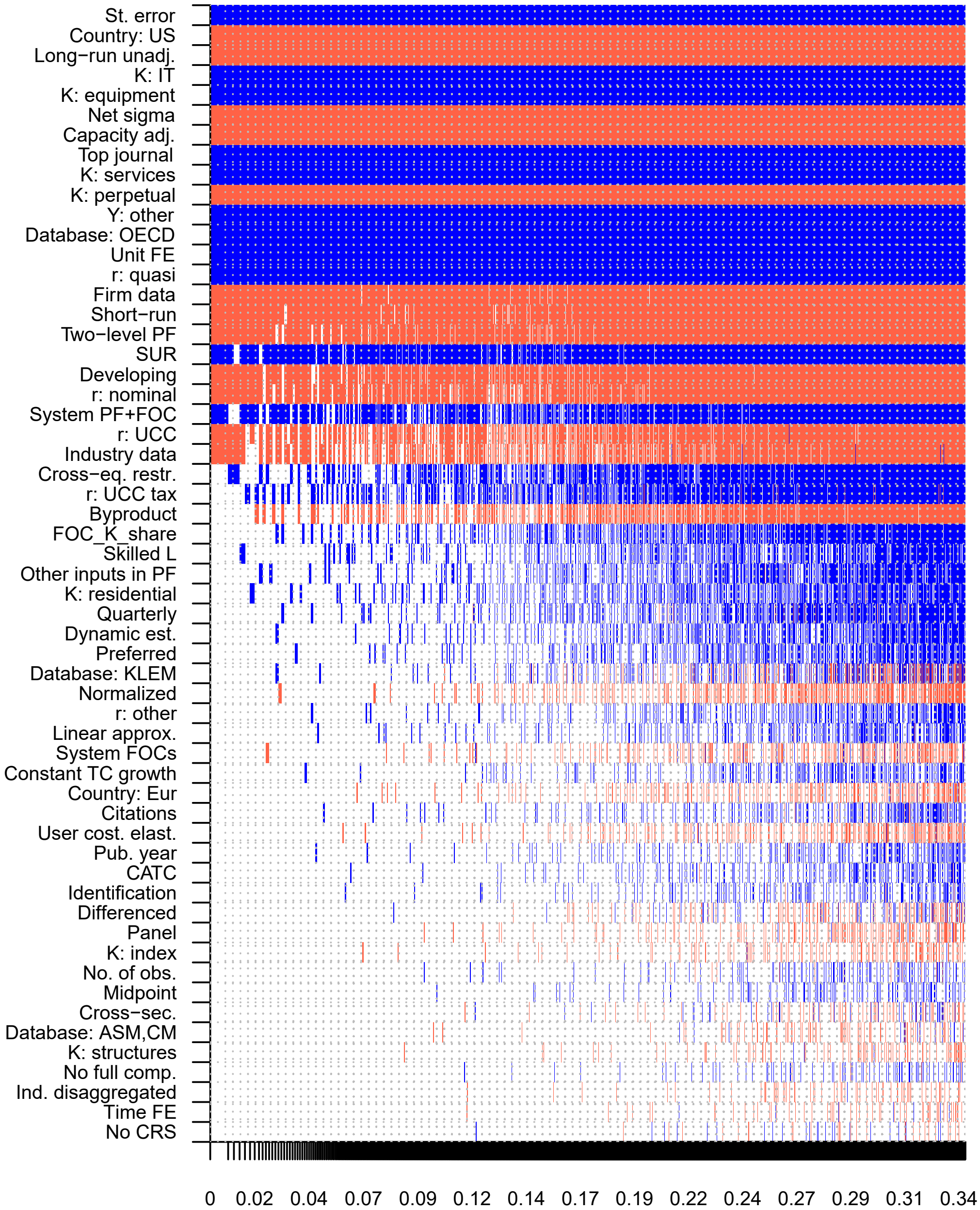
Finally, for the subsample of disaggregated elasticities we run the baseline BMA enriched with industry-relevant variables in Figure A.9. We do not find any significant determinants that would suggest that the elasticity of capital-labor substitution differs systematically across sectors or industry 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 A.7: Model inclusion in Bayesian model averaging, labor-specific variables



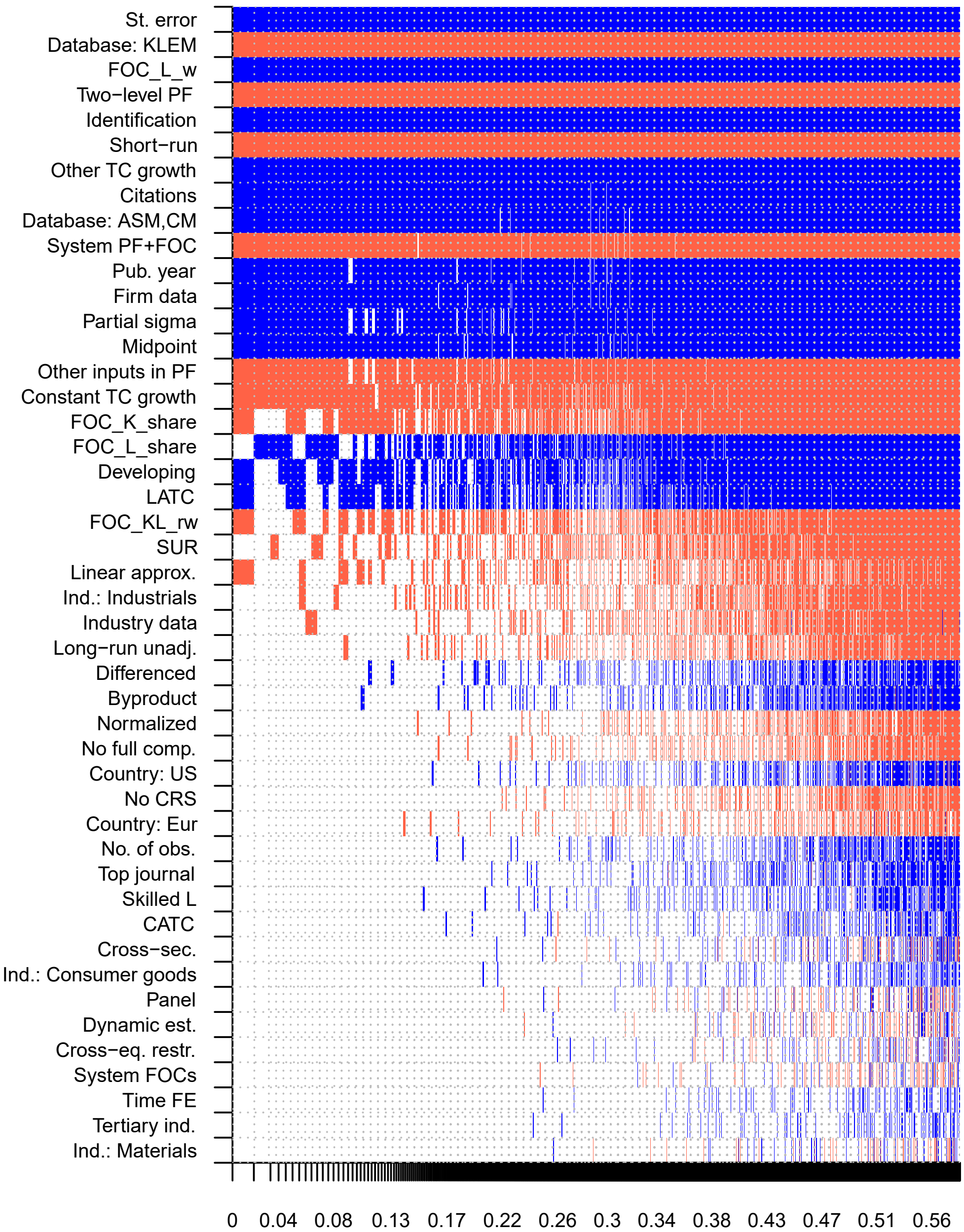
Notes: The response variable is the estimate of the elasticity of substitution. Columns denote individual models; variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes cumulative posterior model probabilities; only the 5,000 best models are shown. Blue 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 A.8: Model inclusion in Bayesian model averaging, capital-specific variables



Notes: 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 A.9: Model inclusion in Bayesian model averaging, industry-specific variables



Notes: 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.

A.6 Studies Included in the Dataset

Table A.5: Studies included in the dataset

Author	Title	Year
Abed (1975)	Labour Absorption in Industry: An Analysis with Reference to Egypt	1975
Akay & Dogan (2013)	The effect of labor supply changes on output: empirical evidence from US industries	2013
Antras (2004)	Is the U.S. Aggregate Production Function Cobb-Douglas? New Estimates of the Elasticity of Substitution	2004
Apostolakis (1984)	A Translogarithmic Cost Function Approach: Greece, 1953 - 1977	1984
Arrow *et al.* (1961)	Capital-Labor Substitution and Economic Efficiency	1961
Artus (1984)	The Disequilibrium Real Wage Rate Hypothesis: An Empirical Evaluation	1984
Asher (1972)	Industrial Efficiency and Biased Technical Change in American and British Manufacturing: The Case of Textiles in the Nineteenth Century	1972
Balistreri *et al.* (2003)	An estimation of US industry-level capital–labor substitution elasticities: support for Cobb–Douglas	2003
Bartelsman & Beetsma (2003)	Why pay more? Corporate tax avoidance through transfer pricing in OECD countries	2003
Behrman (1972)	Sectoral Elasticities of Substitution Between Capital and Labor in a Developing Economy: Times Series Analysis in the Case of Postwar Chile	1972
Behrman (1982)	Country and Sectoral Variations in Manufacturing Elasticities of Substitution between Capital and Labor	1982
Bentolila & Saint-Paul (2003)	Explaining Movements in the Labor Share	2003
Berndt (1976)	Reconciling Alternative Estimates of the Elasticity of Substitution	1976
Berthold *et al.* (2002)	"Falling Labor Share and Rising Unemployment: Long-Run Consequences of Institutional Shocks?"	2002
Binswanger (1974)	A Cost Function Approach to the Measurement of Elasticities of Factor Demand and Elasticities of Substitution	1974
Blanchard (1997)	The Medium Run	1977
Bodkin & Klein (1967)	Nonlinear Estimation of Aggregate Production Functions	1967
Brown (1966)	A Measure of the Change in Relative Exploitation of Capital and Labor	1966
Brown & De Cani (1963)	Technological Change and the Distribution of Income	1963
Brox & Fader (2005)	Infrastructure investment and Canadian manufacturing productivity	2005
Bruno & Sachs (1982)	Input Price Shocks and the Slowdown in Economic Growth: The Case of U.K. Manufacturing	1982
Caballero (1994)	Small Sample Bias and Adjustment Costs	1994
Chetty & Sankar (1969)	Bayesian Estimation of the CES Production Function	1969
Chirinko *et al.* (1999)	How responsive is business capital formation to its user cost? An exploration with micro data	1999
Chirinko *et al.* (2011)	A New Approach to Estimating Production Function Parameters: The Elusive Capital–Labor Substitution Elasticity	2011
Chirinko & Mallick (2017)	The Substitution Elasticity, Factor Shares, and the Low-Frequency Panel Model	2017
Chwelos *et al.* (2010)	Does Technological Progress Alter the Nature of Information Technology as a Production Input? New Evidence and New Results	2010
Clark & Sichel (1993)	Tax Incentives and Equipment Investment	1993
Claro (2003)	A Cross-Country Estimation of the Elasticity of Substitution between Labor and Capital in Manufacturing Industries	2003
Cummins *et al.* (1994)	A Reconsideration of Investment Behavior Using Tax Reforms as Natural Experiments	1994
Cummins & Hassett (1992)	The Effects of Taxation on Investment: New Evidence from Firm Level Panel Data	1992
Daniels (1969)	Differences in Efficiency Among Industries in Developing Countries	1969

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Table A.5: Studies included in the dataset (Continued)

Author	Title	Year
David & Van de Klundert (1965)	Biased Efficiency Growth and Capital-Labor Substitution in the U.S., 1899-1960	1965
Dhrymes (1965)	Some Extensions and Tests for the CES Class of Production Functions	1965
Dissou *et al.* (2015)	Industry-level Econometric Estimates of Energy-Capital-Labor Substitution with a Nested CES Production Function	2015
Dissou & Ghazal (2010)	Energy Substitutability in Canadian Manufacturing: Econometric Estimation with Bootstrap Confidence Intervals	2010
Donges (1972)	Returns to Scale and Factor Substitutability in the Spanish Industry	1972
Duffy & Papageorgiou (2000)	A Cross-Country Empirical Investigation of the Aggregate Production Function Specification	2000
Dwenger (2014)	User Cost Elasticity of Capital Revisited	2014
Easterly & Fischer (1995)	The Soviet Economic Decline	1995
Eisner (1967)	Capital and Labor in Production: Some Direct Estimates	1967
Eisner (1969)	Tax Policy and Investment Behavior: Comment	1969
Eisner & Nadiri (1968)	Investment Behavior and Neo-Classical Theory	1968
Elbers *et al.* (2007)	Growth and Risk: Methodology and Micro Evidence	2007
Ellis & Price (2004)	UK Business Investment and the User Cost of Capital	2004
Shahe Emran *et al.* (2007)	Economic Liberalization and Price Response of Aggregate Private Investment: Time Series Evidence from India	2007
Feldstein (1967)	Specification of the Labour Input in the Aggregate Production Function	1967
Feldstein & Flemming (1971)	Tax Policy, Corporate Saving and Investment Behaviour in Britain	1971
Felipe & McCombie (2009)	Are estimates of labour demand functions mere statistical artefacts?	2009
Ferguson (1965)	Time-Series Production Functions and Technological Progress in American Manufacturing Industry	1965
Fishelson (1979)	Elasticity of Factor Substitution in Cross-Section Production Functions	1979
Fitchett (1976)	Capital-Labor Substitution in the Manufacturing Sector of Panama	1976
Fuchs (1963)	Capital-Labor Substitution: A Note	1963
Griliches (1964)	Research Expenditures, Education, and the Aggregate Agricultural Production Function	1964
Griliches (1967)	Production Functions in Manufacturing: Some Preliminary Results	1967
Herrendorf *et al.* (2015)	Sectoral Technology and Structural Transformation	2015
Hijzen & Swaim (2010)	Offshoring, labour market institutions and the elasticity of labour demand	2010
Hossain (1987)	Allocative and Technical Efficiency: A Study of Rural Enterprises in Bangladesh	1987
Humphrey & Moroney (1975)	Substitution among Capital, Labor, and Natural Resource Products in American Manufacturing	1975
Iqbal (1986)	Substitution of Labour, Capital and Energy in the Manufacturing Sector of Pakistan	1986
Jalava *et al.* (2006)	Biased Technical Change and Capital-Labour Substitution in Finland, 1902-2003	2006
Jones & Backus (1977)	British Producer Cooperatives in the Footware Industry: An Empirical Evaluation of the Theory of Financing	1977
Judzik & Sala (2015)	The determinants of capital intensity in Japan and the US	2015
Juselius (2008)	Long-run relationships between labor and capital: Indirect evidence on the elasticity of substitution	2008
Kalt (1978)	Technological Change and Factor Substitution in the United States: 1929-1967	1978
Karabarbounis & Neiman (2014)	The global decline of the labor share	2014
Kilponen & Viren (2010)	Why do growth rates differ? Evidence from cross-country data on private sector production	2010
Kislev & Peterson (1982)	Prices, Technology, and Farm Size	1982
Klump *et al.* (2007)	Factor substitution and factor augmenting technical progress in the US: a normalized supply-side system approach	2007
Klump *et al.* (2008)	Unwrapping some euro area growth puzzles: Factor substitution, productivity and unemployment	2008

Continued on next page

Table A.5: Studies included in the dataset (Continued)

Author	Title	Year
Kmenta (1967)	On Estimation of the CES Production Function	1967
Krusell *et al.* (2000)	Capital-Skill Complementarity and Inequality: A Macroeconomic Analysis	2000
Lee & Tcha (2004)	The Color of Money: The Effects of Foreign Direct Investment on Economic Growth in Transition Economies	2004
León-Ledesma *et al.* (2015)	ProductionTechnology Estimates and Balanced Growth	2015
Leung & Yuen (2010)	Do exchange rates affect the capital–labour ratio? Panel evidence from Canadian manufacturing industries	2010
Lianos (1971)	The Relative Share of Labor in United States Agriculture, 1949-1968	1971
Lianos (1975)	Capital-Labor Substitution in a Developing Country	1975
Lin & Shao (2006)	The business value of information technology and inputs substitution: The productivity paradox revisited	2006
Lovell (1973b)	Estimation and Prediction with CES and VES Production Functions	1973
Lovell (1973a)	CES and VES Production Functions in a Cross-Section Context	1973
Luoma & Luoto (2010)	The Aggregate Production Function of the Finnish Economy in the Twentieth Century	2010
Mallick (2012)	The role of the elasticity of substitution in economic growth: A cross-country investigation	2012
Martin *et al.* (1993)	The Influence of Location on Productivity: Manufacturing Technology in Rural and Urban Areas	1993
Masanjala & Papageorgiou (2004)	The Solow model with CES technology: nonlinearities and parameter heterogeneity	2004
McAdam & Willman (2004)	Production, supply and factor shares: an application to estimating German long-run supply	2004
McCallum (1985)	Wage Gaps, Factor Shares and Real Wages	1985
McKinnon (1962)	Wages, Capital Costs, and Employment in Manufacturing: A Model Applied to 1947-58 U.S. Data	1962
McLean-Meyinsse & Okunade (1988)	Factor Demands Of Louisiana Rice Producers: An Econometric Investigation	1988
Meller (1975)	Production Functions for Industrial Establishments of Different Sizes: The Chilean Case	1975
Minasian (1961)	Elasticities of Substitution and Constant-Output Demand Curves for Labor	1961
Mohabbat & Dalai (1983)	Factur Substitution and Import Demand for South Korea: A Translog Analysis	1983
Mohabbat *et al.* (1984)	Import Demand for India: A Translog Cost Function Approach	1984
Moroney (1966)	Time-Series Elasticities of Substitution and Labor's Share in U. S. Manufacturing: The Postwar Period	1966
Moroney (1970)	Identification and Specification Analysis of Alternative Equations for Estimating the Elasticity of Substitution	1970
Moroney & Allen (1969)	Monopoly Power and the Relative Share of Labor	1969
Moroney & Toevs (1977)	Factor Costs and Factor Use: An Analysis of Labor, Capital, and Natural Resource Inputs	1977
Nadiri (1968)	The Effects of Relative Prices and Capacity on the Demand for Labour in the U.S. Manufacturing Sector	1968
Panik (1976)	Factor Learning and Biased Factor-Efficiency Growth in the United States, 1929-1966	1976
Parks (1971)	Price Responsiveness of Factor Utilization in Swedish Manufacturing, 1870-1950	1971
Pollak *et al.* (1984)	The CES-Translog: Specification and Estimation of a New Cost Function	1984
Raurich *et al.* (2012)	Factor shares, the price markup, and the elasticity of substitution between capital and labor	2012
Roskamp (1977)	Labor Productivity and the Elasticity of Factor Substitution in West German Industries 1950- 1960	1977
Sahota (1966)	The Sources of Measured Productivity Growth: United States Fertilizer Mineral Industries, 1936- 1960	1966
Salvanes (1989)	The Structure of the Norwegian Fish Farming Industry: An Empirical Analysis of Economies of Scale and Substitution Possibilities	1989

Continued on next page

Table A.5: Studies included in the dataset (Continued)

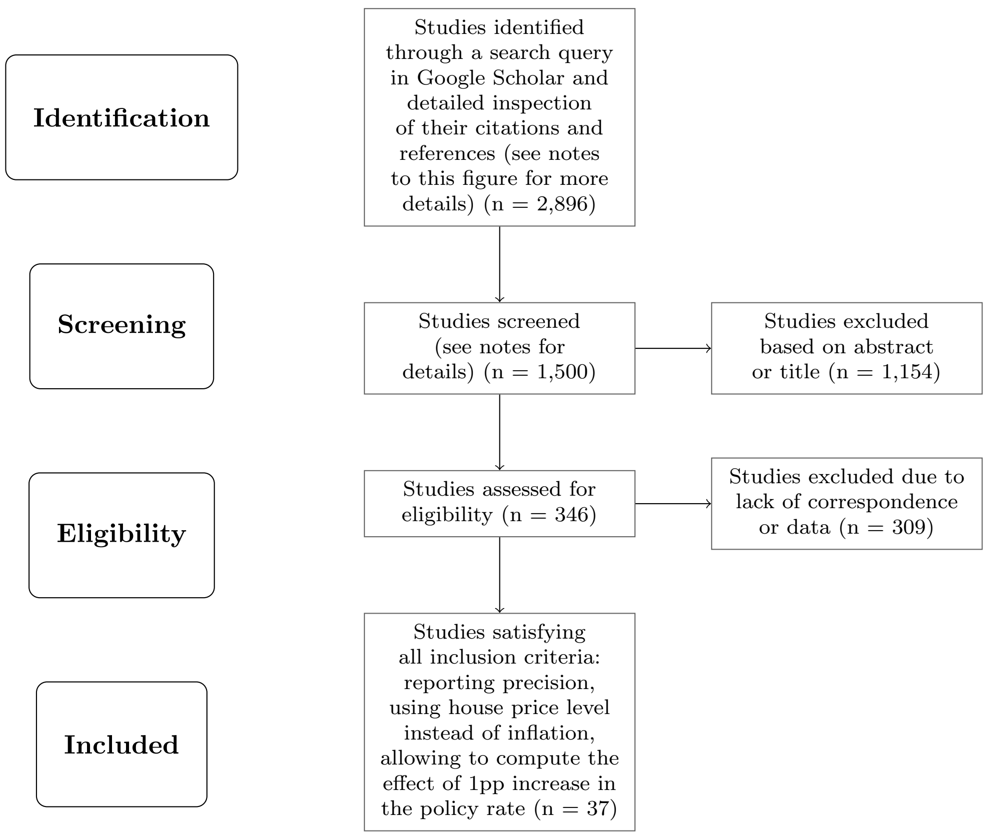
Author	Title	Year
Sankar (1972)	Investment Behavior in the U.S. Electric Utility Industry, 1949-1968	1972
Sapir (1980)	Economic Growth and Factor Substitution: What Happened to the Yugoslav Miracle?	1980
Sato (1977)	A Note on Factor Substitution and Efficiency	1977
Sato & Hoffman (1968)	Production Functions with Variable Elasticity of Factor Substitution: Some Analysis and Testing	1968
Saxonhouse (1977)	Productivity Change and Labor Absorption in Japanese Cotton Spinning 1891-1935	1977
Semieniuk (2017)	Piketty's Elasticity of Substitution: A Critique	2017
Schaller (2006)	Estimating the long-run user cost elasticity	2006
Schmitz (1981)	The Elasticity of Substitution in 19th-Century Manufacturing	1981
Smith (2008)	That elusive elasticity and the ubiquitous bias: Is panel data a panacea?	2008
Solow (1964)	Capital, Labor, and Income in Manufacturing	1964
Tevlin & Whelan (2003)	Explaining the Investment Boom of the 1990s	2003
Tsang & Persky (1975)	On the Empirical Content of CES Production Functions	1975
Van der Werf (2008)	Production functions for climate policy modeling: An empirical analysis	2008
Weitzman (1970)	Soviet Postwar Economic Growth and Capital-Labor Substitution	1970
Williams & Laumas (1984)	Economies of Scale for Various Types of Manufacturing Production Technologies in an Underdeveloped Economy	1984
Young (2013)	US Elasticities of Substitution and Factor-Augmentation at the Industry Level	2013
Zarembka (1970)	On the Empirical Relevance of the CES Production Function	1970

Appendix B

Appendix to Chapter 3

B.1 Details of Literature Search

Figure B.1: PRISMA flow diagram



Notes: Our baseline search query in Google Scholar is *("house" OR "housing" OR "real estate" OR "property" OR "residential") AND "prices" AND ("monetary policy" OR "interest rate" OR "MP" OR "IR" OR "funds rate" OR "FF rate" OR "macroprudential" OR "financial stability").* We collect the first 500 studies returned by this search and call them the baseline set. In addition we collect 500 studies most often cited by the studies in the baseline set and 500 studies that most often cite the studies in the baseline set. We are left with 1,500 studies that we screen. Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) is an evidence-based set of items for reporting in systematic reviews and meta-analyses. More details on PRISMA and reporting standard of meta-analysis in general are provided by Havranek *et al.* (2020).

Table B.1: Studies included in the dataset

Author	Title	Year
Iacoviello (2002)	House Prices and Business Cycles in Europe: a VAR Analysis	2002
Iacoviello & Minetti (2003)	Financial liberalization and the sensitivity of house prices to monetary policy: theory and evidence	2003
Giuliodori (2005)	Monetary Policy Shocks and the Role of House Prices Across European Countries	2004
Calza *et al.* (2007)	Mortgage markets, collateral constraints, and monetary policy: do institutional factors matter?	2007
Assenmacher-Wesche & Gerlach (2008a)	Financial structure and the impact of monetary policy on asset prices	2008
Assenmacher-Wesche & Gerlach (2008b)	Monetary policy, asset prices and macroeconomic conditions: a panel-VAR study	2008
Belke *et al.* (2008)	Sowing the seeds for the subprime crisis: does global liquidity matter for housing and other asset prices?	2008
Elbourne (2008)	The UK housing market and the monetary policy transmission mechanism: An SVAR approach	2008
Iacoviello & Minetti (2008)	The credit channel of monetary policy: Evidence from the housing market	2008
Jarocinski & Smets (2008)	House prices and the stance of monetary policy	2008
Vargas-Silva (2008)	Monetary policy and the US housing market: A VAR analysis imposing sign restrictions	2008
Assenmacher-Wesche & Gerlach (2009)	Financial structure and the impact of monetary policy on property prices	2009
Carstensen *et al.* (2009)	Monetary policy transmission and house prices: European cross-country evidence	2009
Assenmacher-Wesche & Gerlach (2010)	Monetary policy and financial imbalances: facts and fiction	2010
Bjørnland & Jacobsen (2010)	The role of house prices in the monetary policy transmission mechanism in small open economies	2010
Bulligan (2010)	Housing and the macroeconomy: the Italian case	2010
Demary (2010)	The interplay between output, inflation, interest rates and house prices: international evidence	2010
Aspachs-Bracons & Rabanal (2011)	The effects of housing prices and monetary policy in a currency union	2011
Musso *et al.* (2011)	Housing, consumption and monetary policy: how different are the US and the euro area?	2011
Ncube & Ndou (2011)	Monetary policy transmission, house prices and consumer spending in South Africa: An SVAR approach	2011
Sá *et al.* (2014)	Low interest rates and housing booms: the role of capital inflows, monetary policy and financial innovation	2011
Sá & Wieladek (2015)	Monetary policy, capital inflows and the housing boom	2011
Gupta *et al.* (2012a)	Monetary policy and housing sector dynamics in a large-scale Bayesian vector autoregressive model	2012
Gupta *et al.* (2012b)	Financial Market Liberalization, Monetary Policy, and Housing Sector Dynamics	2012
Wadud *et al.* (2012)	Monetary policy and the housing market in Australia	2012
Berlemann & Freese (2013)	Monetary policy and real estate prices: a disaggregated analysis for Switzerland	2013
Calza *et al.* (2013)	Housing finance and monetary policy	2013
McDonald & Stokes (2013)	Monetary policy, mortgage rates and the housing bubble	2013
Sousa (2014)	Wealth, asset portfolio, money demand and policy rule	2014
Bauer & Granziera (2017)	Monetary policy, private debt and financial stability risks	2017
Coibion *et al.* (2017)	Innocent bystanders? Monetary policy and inequality in the U.S.	2017
Wu & Bian (2018)	Housing, consumption and monetary policy: how different are the first-, second- and third-tier cities in China?	2018
Dias & Duarte (2019)	Monetary policy, housing rents, and inflation dynamics	2019
Jannsen *et al.* (2015)	Monetary policy during financial crises: Is the transmission mechanism impaired?	2019
Rosenberg (2019)	The effects of conventional and unconventional monetary policy on house prices in the Scandinavian countries	2019
Miranda-Agrippino & Rey (2020)	U.S. monetary policy and the global financial cycle	2020
Benati (2021)	Leaning against house prices: A structural VAR investigation	2021

B.2 Extensions of Publication Bias Models

B.2.1 Caliper Test

As an extension to the previously reported tests of publication bias we apply the caliper test as proposed in Gerber & Malhotra (2008a) and Gerber & Malhotra (2008b) and recently implemented by Bruns *et al.* (2019). The caliper test is based on the analysis of discontinuities in the reported t-statistics: if no selective reporting is present, there should be no discontinuities around the conventional significance thresholds. In other words the number of t-statistics reported in the literature just above the threshold ("over caliper") should not be statistically different from the number of reported t-statistics just below the threshold ("under caliper"). The test does not allow us to compute the true effect beyond bias but serves as an indicator of whether publication selection appears in the literature, thus providing us with a robustness check of the previous results. The results are presented in Table B.2. Primarily we examine the significance threshold corresponding to the 68% confidence interval: although the threshold is usually much stricter in the empirical literature featuring point estimates, in the case of VAR models and impulse response functions the 68% confidence interval is the most frequently reported (almost 70% of our estimates use it), so we suspect that publication selection could be related to this threshold. We use caliper sizes of 0.1, 0.3, and 0.5. The results show that publication selection is present at the one-quarter horizon with a wider caliper. If we test the parameter against the value of 0.4 (i.e., a 60:40 distribution around the thresholds, instead of 50:50, as reasoned in Bruns *et al.* 2019), then evidence of publication selection is stronger and common for most horizons. This is broadly in line with our previous results on publication selection.

B.2.2 Tests Based on the Distribution of p-values

p-curve

Now we look at the distribution of p-values. First, we employ the p-curve method, which is primarily intended to test the null hypothesis that the literature has no evidential value (that is, no effect of monetary policy on house prices beyond publication bias). The technique was developed by Simonsohn *et al.* (2014a) and Simonsohn *et al.* (2014b). Based on Figure B.2, we obtain evidence for evidential value, which is consistent with a right-skewed distribution, while

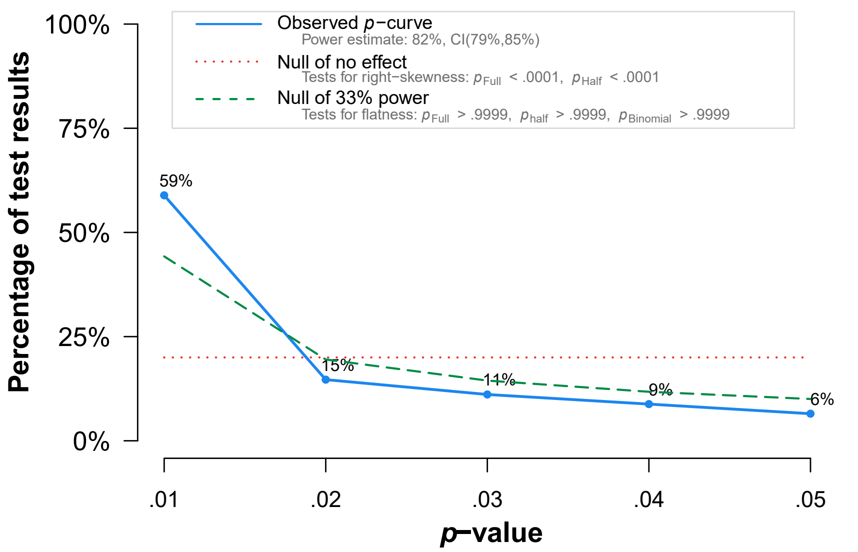
Table B.2: Results of the caliper test

Caliper size	All horizons	1 quarter	2 quarters	4 quarters	8 quarters	12 quarters	16 quarters
0.1	*0.505*	*0.688*	0.500	0.444	0.429	0.467	0.474
(95% LCI)	[0.404]	[0.432]	[0.244]	[0.0393]	[-0.0658]	[0.181]	[0.226]
0.3	*0.520*	*0.604*	0.524	*0.553*	*0.625*	0.478	0.436
(95% LCI)	[0.467]	[0.461]	[0.366]	[0.406]	[0.494]	[0.328]	[0.301]
0.5	*0.516*	**0.658**	0.474	*0.551*	*0.598*	0.500	0.425
(95% LCI)	[0.475]	[0.549]	[0.359]	[0.438]	[0.493]	[0.385]	[0.319]

Notes: The table shows the results of the caliper test for three caliper sizes 0.1, 0.3, and 0.5. The reported numbers represent the share of observations in the narrow interval that are above the significance threshold. LCI = lower bound of the confidence interval. The test parameter is the following: $C = \frac{n_{oc}}{n_{oc}+n_{uc}}$, where n_{oc} and n_{uc} stand for the number of observations with t-statistics in the interval above the threshold ("over caliper") and below the threshold ("under caliper"). For the significance threshold we use the criterion of one standard error above the estimate (commonly used in the VAR literature). The one-sided hypothesis $H_0 : C \leq 0.5$ is tested against $H_1 : C > 0.5$. 95% lower confidence intervals for the test parameters are reported in parenthesis. Significant caliper test results when testing $H_0 : C \leq 0.5$ are shown in bold; significant caliper test results when testing $H_0 : C \leq 0.4$ are shown in italics.

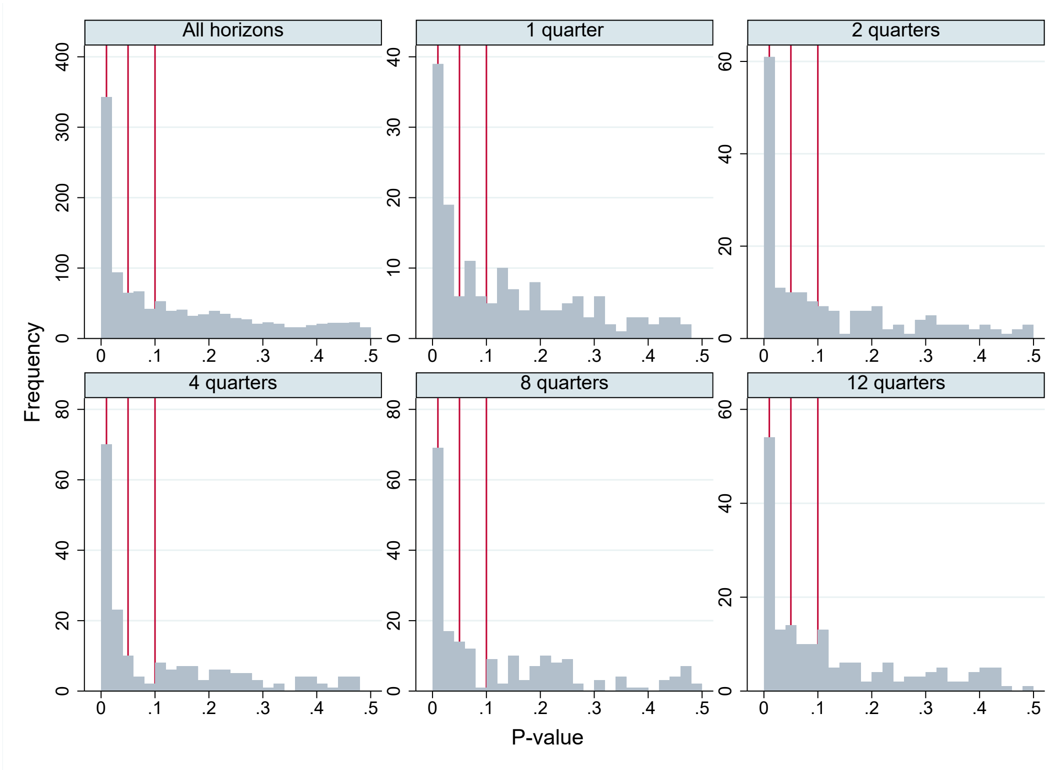
a left-skewed distribution would suggest p-hacking. In addition to contrast to the common p-curve we also plot the whole distribution of p-values (not only those significant up to the 5% significance level) in Figure B.3 to see whether there are distinct jumps at different thresholds associated with conventional statistical significance. There is no clear evidence for such jumps.

Figure B.2: p-curve results



Note: The observed p−curve includes 477 statistically significant (p < .05) results, of which 379 are p < .025. There were 1078 additional results entered but excluded from p−curve because they were p > .05.

Figure B.3: Distribution of p-values



Tests introduced by Elliott et al. (2022)

Elliott *et al.* (2022) analyze p-hacking based on the distribution of p-values and introduce novel testable restrictions. They show that the p-curve (distribution of p-values across studies) is i) non-increasing and continuous in the absence of p-hacking, ii) completely monotone, with upper bounds on p-curve. In their empirical application they use binomial, Fisher's, and density discontinuity tests, as already used before in Simonsohn *et al.* (2014a) and Cattaneo *et al.* (2020). Besides that, they also develop new, more powerful tests: a histogram-based test for non-increasingness, a histogram-based test for 2-monotonicity and bounds, and least concave majorant (LCM) test based on concavity of the CDF of p-values. The results of these tests are available in Table B.3. All of the tests have null hypothesis of no p-hacking. While with less powerful tests (binomial and Fisher) we do not reject the null of no p-hacking, we can reject it with the test for non-increasingness (CS1), 2-monotonicity (CS2B) and also density discontinuity test at horizons between 2 and 12 quarters in all cases. As in the main body of the paper, we run the tests at a threshold of t=1, instead of 1.96, as this is the most common threshold in impulse responses of VAR models.

Table B.3: Tests used by Elliott *et al.* (2022)

Test	All	1 Quarter	2 Quarters	4 Quarters	8 Quarters	12 Quarters	16 Quarters
Binomial	0.990	0.702	0.500	0.837	1.000	0.820	0.500
Fisher	1.000	1.000	1.000	1.000	1.000	1.000	1.000
Discontinuity	**0.000**	0.161	**0.000**	**0.000**	**0.001**	**0.015**	0.671
CS1	**0.069**	0.891	**0.053**	**0.000**	**0.019**	**0.000**	0.194
CS2B	**0.000**	**0.000**	**0.008**	**0.000**	**0.000**	**0.000**	**0.004**
LCM	1.000	0.987	0.999	0.996	0.691	0.997	0.994
N near t=1	178	32	21	26	35	19	23
N	1054	142	156	174	182	164	146

Notes: CS1 is the test for non-increasingness. CS2B is the test for K-monotonicity. LCM is the test based on the concavity of the CDF of p-values. Values in bold incicate rejections of the hypothesis of no p-hacking.

Table B.4: Results concerning publication bias remain similar when studies on house price inflation are included

Time after a monetary policy shock:	1 quarter	2 quarters	4 quarters	8 quarters	12 quarters	16 quarters
PANEL A: Linear models						
Regression of reported estimates on their standard errors, ordinary least squares						
Standard error (publication bias)	-0.846**	-1.082***	-1.319***	-1.152***	-0.675**	-0.386*
	(0.397)	(0.369)	(0.432)	(0.309)	(0.313)	(0.211)
	[-1.702, 0.253]	[-1.981, -0.328]	[-2.382, -0.325]	[-2.070, -0.231]	[-1.561, -0.009]	[-1.298, 0.143]
Constant (corrected mean effect)	-0.038	-0.071	-0.081	-0.293	-0.550*	-0.616**
	(0.141)	(0.185)	(0.261)	(0.246)	(0.287)	(0.246)
	[-0.392, 0.372]	[-0.464, 0.448]	[-0.663, 0.713]	[-0.939, 0.378]	[-1.186, 0.164]	[-1.142, 0.011]
Regression of reported estimates on their standard errors, weighted by inverse variance						
Standard error (publication bias)	-0.745***	-0.877***	-1.112***	-1.215***	-1.028***	-0.797***
	(0.174)	(0.137)	(0.175)	(0.160)	(0.185)	(0.168)
	[-1.157, -0.328]	[-1.206, -0.569]	[-1.551, -0.712]	[-1.657, -0.811]	[-1.536, -0.484]	[-1.331, -0.269]
Constant (corrected mean effect)	-0.079*	-0.176***	-0.212**	-0.241**	-0.215*	-0.178
	(0.047)	(0.058)	(0.087)	(0.108)	(0.123)	(0.120)
	[-0.217, 0.026]	[-0.352, 0.048]	[-0.464, 0.089]	[-0.572, 0.092]	[-0.575, 0.141]	[-0.524, 0.211]
PANEL B: Nonlinear models						
Stem-based method (Furukawa 2019)						
Corrected mean effect	-0.012	-0.122	-0.168	-0.128	-0.187	-0.146**
	(0.119)	(0.191)	(0.263)	(0.331)	(0.130)	(0.075)
Selection model (Andrews & Kasy 2019)						
Corrected mean effect, break at $t = 1.645$	-0.125***	-0.104	-0.340***	-0.412***	-0.208	-0.068
	(0.045)	(0.319)	(0.065)	(0.096)	(0.142)	(0.097)
Corrected mean effect, break at $t = 1$	0.005	-0.050	-0.187***	-0.208	-0.196	-0.070
	(0.070)	(0.130)	(0.060)	(0.163)	(0.162)	(0.131)
P-uniform (van Aert & van Assen 2021)*						
Corrected mean effect, break at $t = 1.645$	-0.164***	-0.142***	-0.159***	-0.147***	-0.124***	-0.093***
Corrected mean effect, break at $t = 1$	-0.097***	-0.110***	-0.121***	-0.110***	-0.104***	-0.080***
Observations	240	245	255	255	249	241

Notes: The results are computed using the original dataset and, additionally, estimates from 8 studies on house prices inflation recomputed using the approach of Fabo *et al.* (2021). Standard errors, clustered at the level of studies and countries, are depicted in round brackets; confidence intervals from wild bootstrap are in square brackets. The p-uniform* method reports p-values, which are all below 0.001 and thus not shown in the table. The selection model and p-uniform* require specifying the break corresponding to a publication selection rule. The wild bootstrap (Cameron *et al.* 2008) is implemented via the *boottest* package in Stata (Roodman *et al.* 2019). *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

B.3 Summary Statistics and Extensions of Heterogeneity Models

Table B.5: Description and summary statistics of regression variables

Label	Description	Mean	SD
Estimate	The reported effect of a one-percentage-point increase in the interest rate on house prices (after four quarters in %).	-0.853	1.242
Standard Error	The reported or implied standard error of the estimate.	0.780	0.894
Data characteristics			
Monthly	= 1 if the data were collected at the monthly frequency (reference category: quarterly data).	0.112	0.315
Panel	= 1 if panel data were used (ref. cat.: time series).	0.220	0.415
Length	The logarithm of the length of the data sample used in the primary study (in years).	3.123	0.289
Midpoint	The logarithm of the mean year of the data used in the study (normalized to the earliest mean year in our sample).	2.888	0.492
Specification characteristics			
GDP Defl.	= 1 if GDP deflator is included in the VAR model instead of CPI.	0.069	0.254
Foreign IR	= 1 if a foreign interest rate is included.	0.026	0.158
Credit	= 1 if credit is included.	0.278	0.448
Consumption	= 1 if consumption is included.	0.277	0.448
Res. Invest	= 1 if a measure of residential investment is included.	0.175	0.380
Money Supply	= 1 if a measure of the money supply is included.	0.204	0.403
Exch. Rate	= 1 if the exchange rate is included	0.252	0.435
Long-run IR	= 1 if the long-run interest rate (in addition to the short-run interest rate) is included.	0.197	0.398
Real HP	= 1 if real instead of nominal house prices are used.	0.940	0.237
Lags	The number of lags (in quarters) included in the model.	3.320	1.531
Time Trend	= 1 if the study uses detrended data or a time trend is added to the regression.	0.367	0.482
Estimation characteristics			

Continued on next page

Table B.5: Description and summary statistics of regression variables (Continued)

Label	Description	Mean	SD
BVAR	= 1 if a Bayesian VAR model is employed in the primary study.	0.152	0.359
Sign Restr. HP	= 1 if sign restrictions are used in the VAR model and are imposed on the house price variable (ref. cat.: Cholesky decomposition).	0.048	0.214
Sign Restr. Other	= 1 if sign restrictions are used in the VAR model but are not imposed on the house price variable (ref. cat.: Cholesky decomposition).	0.054	0.226
Nonrecursive	= 1 if another nonrecursive identification is used in the VAR model (ref. cat.: Cholesky decomposition).	0.142	0.349
Publication characteristics			
Citations	The logarithm of the mean number of annual citations of the study received during the first three years after its publication.	2.982	1.003
Impact	The recursive discounted RePEc impact factor of the outlet.	0.519	0.556
Journal	= 1 if the study is published in a peer-reviewed journal.	0.412	0.492
Structural heterogeneity			
Crisis	The number of years (out of those used in the time span of the primary study) during which a banking crisis occurred.	3.645	2.900
IR	The average three-month interest rate, OECD.	7.082	2.471
Prolonged Low IR	The number of consecutive years (out of those used in the time span of the primary study) during which the short-run interest rate was below its long-run average.	9.265	5.376
Spread	The average difference between short-term and long-term interest rates.	0.681	0.494
Floating	The share of loans with a floating interest rate.	50.698	27.273
Tourism YoY	The growth rate of the number of arrivals per capita.	3.461	4.893
Income	Average disposable income per household per capita in US dollars, OECD.	9.772	0.357
Inflation	Average consumer price inflation, OECD.	4.163	2.318
Credit-to-GDP	The credit-to-GDP ratio, BIS.	126.545	33.323
Popul. Growth	Average annual population growth, World Bank.	0.616	0.393

Continued on next page

Table B.5: Description and summary statistics of regression variables (Continued)

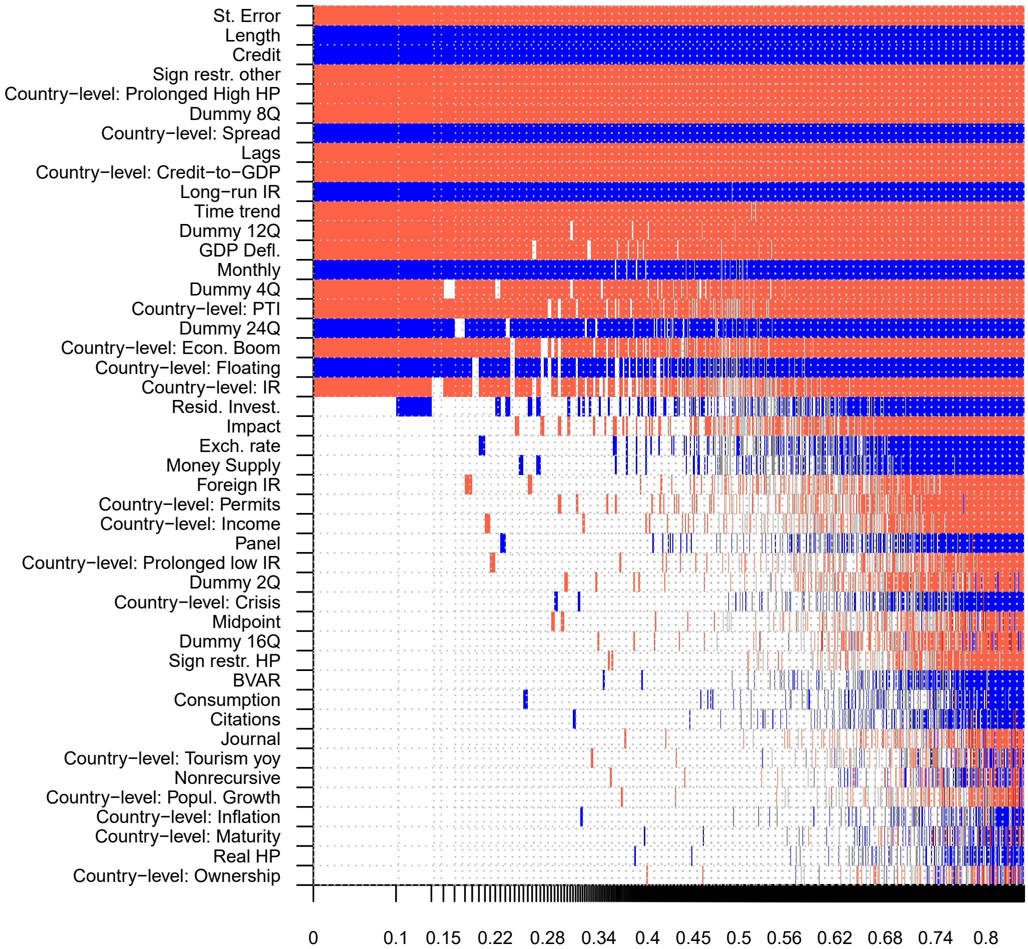
Label	Description	Mean	SD
PTI	The standardized price-to-income ratio for the housing market.	94.527	8.776
Prolonged High HP	The number of periods (out of those used in the time span of the primary study) with above-average house price growth.	12.414	4.614
Permits	The number of building permits issued in comparison to its long-run average.	101.074	21.868
Maturity	The average maturity of mortgage loans in logs.	3.088	0.224
Ownership	The share of home ownership.	61.205	8.889
Econ. Boom	The number of periods (out of those used in the time span of the primary study) with a positive output gap.	5.641	3.367

Table B.6: Results of frequentist model averaging

Category	Variable	Coef.	Std. Er.	p-value
Publication bias	**SE**	**-1.633**	**0.186**	**0.000**
Data characteristics	Monthly	0.361	0.470	0.442
	Panel	0.000	0.184	1.000
	Length	**1.493**	**0.815**	**0.067**
	Midpoint	0.110	0.327	0.736
Specification characteristics	GDP Defl.	-0.202	0.287	0.481
	Foreign IR	-0.244	0.399	0.541
	Credit	*0.264*	*0.205*	*0.197*
	Consumption	-0.128	0.247	0.606
	Resid. Invest.	0.181	0.310	0.559
	Money Supply	0.340	0.326	0.297
	Exch. rate	0.165	0.224	0.462
	Long-run IR	0.196	0.251	0.435
	Real HP	0.285	0.347	0.410
	Lags	-0.048	0.078	0.541
	Time trend	-0.205	0.212	0.334
Estimation characteristics	BVAR	0.398	0.400	0.319
	Sign restr. HP	-0.500	0.445	0.261
	Sign restr. other	-0.486	0.426	0.254
	Nonrecursive	0.000	0.278	1.000
Publication characteristics	Citations	0.026	0.112	0.817
	Impact	-0.112	0.149	0.454
	Journal	-0.024	0.198	0.903
Structural heterogeneity	Country-level: Crisis	0.029	0.044	0.504
	Country-level: IR	-0.126	0.103	0.221
	Country-level: Prolonged low IR	-0.029	0.032	0.363
	Country-level: Spread	0.224	0.218	0.304
	Country-level: Floating	0.005	0.004	0.285
	Country-level: Tourism yoy	-0.011	0.016	0.486
	Country-level: Income	0.257	0.568	0.651
	Country-level: Inflation	0.026	0.057	0.650
	Country-level: Credit-to-GDP	*0.013*	*0.008*	*0.103*
	Country-level: Popul. Growth	0.302	0.435	0.489
	Country-level: PTI	*-0.029*	*0.020*	*0.146*
	Country-level: Prolonged High HP	**-0.070**	**0.042**	**0.097**
	Country-level: Permits	0.004	0.006	0.542
	Country-level: Maturity	-0.202	0.397	0.612
	Country-level: Ownership	-0.016	0.019	0.407
	Country-level: Econ. Boom	-0.034	0.042	0.419
	Constant	-0.5212	5.2719	0.921
Observations	225			

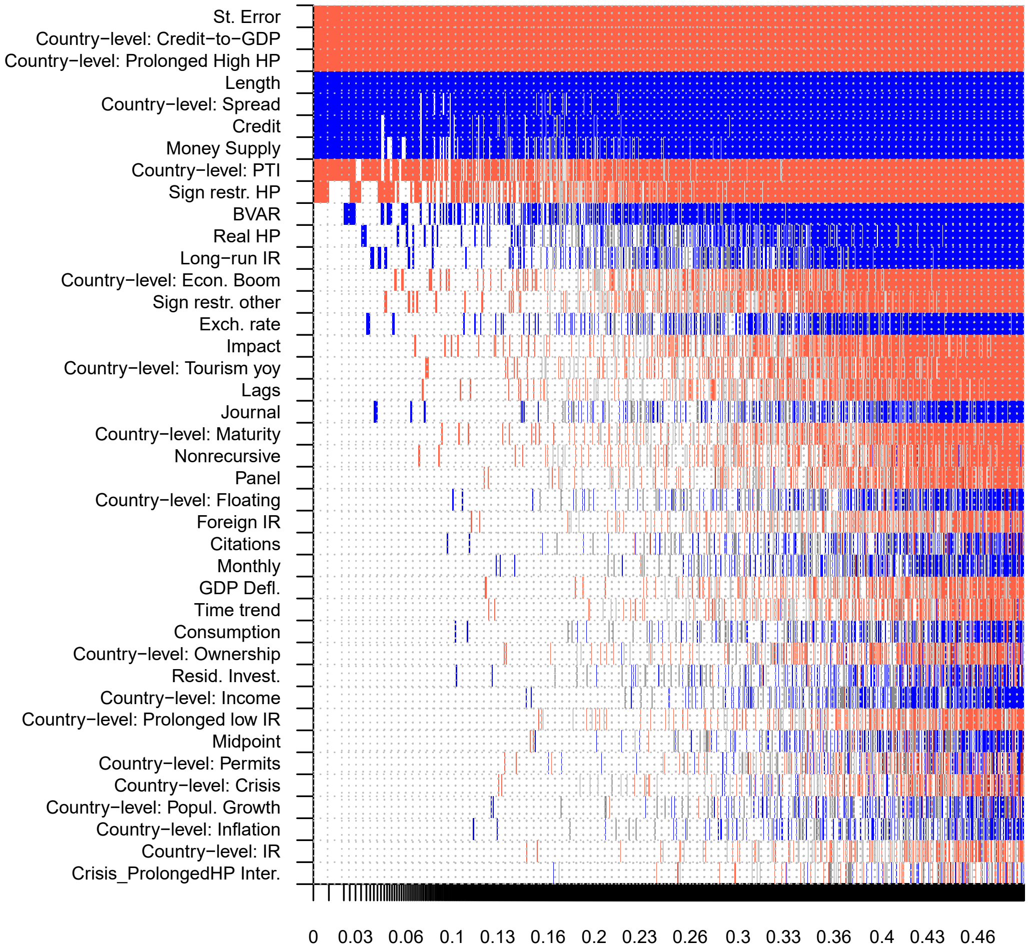
Notes: The frequentist model averaging (FMA) exercise employs Mallow's weights (Hansen 2007) and the orthogonalization of the covariate space suggested by Amini & Parmeter (2012). Variables significant at the 10% level are shown in bold; variables that were important in BMA and have a p-value lower than 0.2 are indicated in italics.

Figure B.4: Model inclusion in BMA with estimates for all horizons



Notes: Columns denote individual models; the variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes the cumulative posterior model probabilities; the 10,000 best models are shown. To ensure convergence we employ 3 million iterations and 1 million burn-ins. Blue color (darker in grayscale) = the variable is included and the estimated sign is positive, i.e., the transmission is weaker. Red color (lighter in grayscale) = the variable is included and the estimated sign is negative, i.e., the transmission is stronger. No color = the variable is not included in the model. A detailed description of all the variables is available in Table B.5.

Figure B.5: BMA with an interaction between crisis and prolonged periods of high house prices



Notes: The interaction is the variable with the smallest posterior inclusion probability (at the bottom of the figure). Columns denote individual models; the variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes the cumulative posterior model probabilities; the 10,000 best models are shown. To ensure convergence we employ 3 million iterations and 1 million burn-ins. Blue color (darker in grayscale) = the variable is included and the estimated sign is positive, i.e., the transmission is weaker. Red color (lighter in grayscale) = the variable is included and the estimated sign is negative, i.e., the transmission is stronger. No color = the variable is not included in the model. A detailed description of all the variables is available in Table B.5.

Table B.7: A robustness check using ordinary least squares

Category	Variable	1Q	2Q	4Q	8Q	12Q	16Q
Publication bias	SE	-0.764*	-1.251***	-1.681***	-1.618***	-0.953***	-0.521***
		(0.395)	(0.271)	(0.244)	(0.133)	(0.230)	(0.148)
Data characteristics	Panel	-0.263**	-0.188	-0.00259	0.0329	0.349	0.576**
		(0.109)	(0.131)	(0.158)	(0.241)	(0.293)	(0.289)
	Length	1.428**	1.437**	1.788**	2.571***	1.990***	0.918***
		(0.706)	(0.574)	(0.730)	(0.688)	(0.526)	(0.346)
Specification characteristics	GDP Deflator	0.0474	0.164	-0.328**	-0.696***	-0.831***	-0.848***
		(0.191)	(0.187)	(0.158)	(0.209)	(0.252)	(0.275)
	Credit	0.0321	0.223***	0.370***	0.597***	0.440***	0.330***
		(0.102)	(0.0418)	(0.117)	(0.161)	(0.134)	(0.123)
	Resid. invest.	0.648**	0.426*	0.205	0.113	0.286	0.442
		(0.289)	(0.257)	(0.227)	(0.315)	(0.325)	(0.322)
	Money Supply	0.0259	0.00278	0.615***	1.005***	0.660**	0.173
		(0.140)	(0.176)	(0.230)	(0.239)	(0.288)	(0.279)
	Exchange rate	-0.111	0.0978	0.214	0.254*	0.533***	0.517***
		(0.127)	(0.170)	(0.152)	(0.138)	(0.162)	(0.168)
	Long-run IR	-0.311*	-0.0493	0.266	0.455*	0.321*	0.129
		(0.177)	(0.162)	(0.214)	(0.234)	(0.168)	(0.170)
	Lags	-0.0418	-0.0480	-0.0200	-0.0982*	-0.138***	-0.186***
		(0.0378)	(0.0373)	(0.0317)	(0.0558)	(0.0457)	(0.0430)
	Time trend	-0.00914	-0.151	-0.253**	-0.637***	-0.458***	-0.334**
		(0.132)	(0.147)	(0.109)	(0.202)	(0.156)	(0.148)
Estimation characteristics	BVAR	0.0134	0.134	0.497	0.732*	0.281	0.0655
		(0.295)	(0.349)	(0.477)	(0.444)	(0.426)	(0.346)
	Sign restr. other	0.597	0.259	-0.594	-1.359***	-1.685***	-1.667***
		(0.402)	(0.338)	(0.402)	(0.455)	(0.527)	(0.515)
	Nonrecursive	0.285	0.218	-0.351	-0.682***	-0.0236	0.560***
		(0.230)	(0.272)	(0.242)	(0.253)	(0.237)	(0.177)
Publication characteristics	Citations	0.0138	-0.0458	0.0690	0.247**	0.0308	-0.176**
		(0.0911)	(0.0710)	(0.0766)	(0.114)	(0.0939)	(0.0810)
	Impact	-0.120	-0.0670	-0.147**	-0.311**	-0.613***	-0.535***
		(0.141)	(0.101)	(0.0671)	(0.126)	(0.0973)	(0.0322)
Structural heterogeneity	Country-level: IR	-0.207**	-0.214**	-0.177*	-0.187***	-0.149**	-0.186***
		(0.0813)	(0.108)	(0.103)	(0.0577)	(0.0601)	(0.0429)
	Country-level: Prolonged low IR	-0.00591	-0.0166	-0.0405	-0.0694**	-0.0234	0.00134
		(0.0230)	(0.0332)	(0.0343)	(0.0287)	(0.0267)	(0.0301)
	Country-level: Spread	-0.204	-0.117	0.291*	0.504***	0.505***	0.416***
		(0.139)	(0.198)	(0.163)	(0.110)	(0.121)	(0.150)
	Country-level: Floating	0.00880***	0.00441	0.00520	0.00803***	0.00729**	0.00870***
		(0.00205)	(0.00430)	(0.00474)	(0.00268)	(0.00285)	(0.00292)
	Country-level: Credit-to-GDP	-0.0105*	-0.0128*	-0.0177**	-0.0191***	-0.0153***	-0.0107***
		(0.00621)	(0.00737)	(0.00845)	(0.00549)	(0.00267)	(0.00314)
	Country-level: Maturity	0.743**	0.0517	-0.211	-0.215	0.381	0.737***
		(0.337)	(0.691)	(0.646)	(0.465)	(0.316)	(0.283)
	Country-level: PTI	-0.0298*	-0.0271	-0.0409***	-0.0490***	-0.0228**	-0.0192**
		(0.0170)	(0.0211)	(0.0149)	(0.00691)	(0.0104)	(0.00828)
	Country-level: Prolonged High HP	-0.0620	-0.0766**	-0.0833**	-0.103***	-0.108***	-0.0849***
		(0.0451)	(0.0333)	(0.0400)	(0.0393)	(0.0205)	(0.0251)
	Country-level: Econ. Boom	0.014	-0.020	-0.057*	-0.074**	-0.055*	-0.053
		(0.027)	(0.027)	(0.033)	(0.034)	(0.033)	(0.043)
	Observations	210	215	225	225	220	218

Notes: Standard errors, clustered at the study level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. For ease of exposition, variables which are not significant at any horizon are excluded from the table.

Appendix C

Appendix to Chapter 4

C.1 Data and Additional Estimation Results

Table C.1: Explained and explanatory variables – summary statistics

	Long sample					Short sample				
	Mean	SD	Min	Max	Median	Mean	SD	Min	Max	Median
EA	10.13	5.26	1.47	30.33	9.13	10.10	4.78	1.47	30.33	9.95
REA	3.85	4.93	-4.26	24.03	2.37	3.50	4.65	-4.26	24.03	2.70
CA	9.18	5.09	1.47	30.05	7.52	9.20	5.47	1.47	30.05	8.06
CS	5.61	4.57	-1.47	28.03	4.39	5.56	5.38	-1.47	28.03	3.81
ICS	5.38	4.20	-3.15	19.17	4.77	4.96	4.50	-3.15	19.12	3.87
UCS	0.28	2.64	-8.68	15.51	0.17	0.57	2.28	-4.94	9.22	0.27
RW	59.58	21.73	11.45	140.60	53.48	52.07	18.66	11.45	112.56	47.91
Credit growth	15.17	20.49	-21.72	87.44	9.54	10.85	20.25	-21.72	87.44	7.04
ORCR	9.49	2.49	8.00	17.01	8.00	11.46	2.74	8.00	17.01	10.50
ROA	1.07	1.15	-4.40	10.86	0.95	0.80	1.01	-4.40	3.39	0.80
LLP/A	1.87	2.00	0.00	10.75	1.32	1.67	1.84	0.08	10.75	1.23
Lending rate	5.00	2.09	-0.20	13.96	4.69	4.18	2.18	0.56	12.23	3.63
Interbank loans/A	5.13	5.50	0.00	44.85	3.64	2.68	3.27	0.00	26.59	2.04
Loans to CB&CG/A	3.72	8.25	0.00	76.99	0.62	7.24	11.54	0.00	76.99	1.87
Loans to PS excl. IL/A	56.75	17.32	12.02	89.23	55.99	56.24	17.58	12.02	86.47	56.07
Bonds/A	16.25	11.55	0.00	52.12	15.83	16.74	11.21	0.01	49.44	14.96
IRB	0.29	0.46	0.00	1.00	0.00	0.36	0.48	0.00	1.00	0.00
Real GDP growth	2.75	3.18	-5.58	7.34	2.85	2.95	2.16	-1.75	5.80	3.40
PX growth	5.19	25.01	-53.46	67.39	2.88	1.63	9.54	-15.21	19.06	-0.11
Spread	1.37	0.91	-0.04	3.68	1.46	0.77	0.68	-0.04	2.24	0.51
Observations	644	644	644	644	644	278	278	278	278	278

Note: The variables are ordered based on their order of appearance in the text. The dependent variables are located in the upper part of the summary table. Different statistics are provide for the long and short samples. The long data sample covers 56 quarters from 2004 Q1 to 2017 Q4; the short data sample covers 20 quarters from 2013 Q1 to 2017 Q4. Credit growth is winsorised at the 2nd and 98th percentiles.

Figure C.1: Bank-level regulatory capital requirements (in % of RWE)

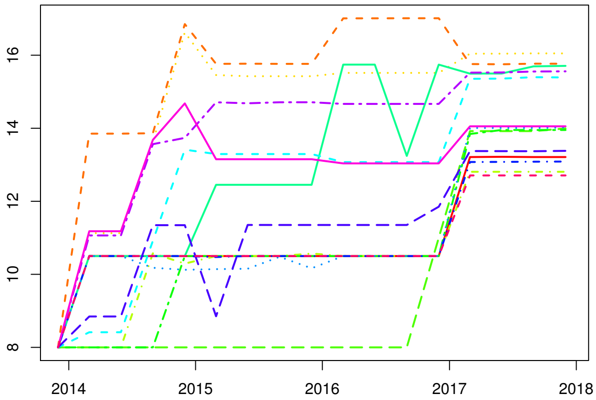


Table C.2: The effect of higher capital requirements on credit growth – additional control variables

	(1) BBBC	(2) BBBC	(3) BBBC	(4) BBBC	(5) BBBC	(6) BBBC	(7) LSDV	(8) LSDV	(9) LSDV	(10) LSDV	(11) LSDV	(12) LSDV
Credit growth (r-1)	0.848***	0.844***	0.836***	0.842***	0.835***	0.841***	0.750***	0.744***	0.732***	0.731***	0.731***	0.729***
	(0.058)	(0.061)	(0.065)	(0.065)	(0.066)	(0.066)	(0.041)	(0.050)	(0.029)	(0.033)	(0.030)	(0.034)
ORCR	-0.712*		-0.635*		-0.490		-0.959**		-0.765*		-0.569	
	(0.386)		(0.353)		(0.403)		(0.387)		(0.364)		(0.329)	
ORCR*dLowCS		-1.148*		-0.898		-0.764		-1.674***		-1.323**		-1.128*
		(0.694)		(0.718)		(0.753)		(0.506)		(0.586)		(0.541)
ORCR*(1-dLowCS)		-0.466		-0.489		-0.346		-0.556		-0.543		-0.343
		(0.371)		(0.322)		(0.369)		(0.437)		(0.337)		(0.309)
LLPA (t-1)	0.438	0.491	0.393	0.472	0.325	0.405	-0.041	0.144	0.102	0.198	0.012	0.107
	(0.581)	(0.540)	(0.503)	(0.504)	(0.499)	(0.499)	(0.266)	(0.243)	(0.242)	(0.251)	(0.284)	(0.291)
CA (t-1)	1.640***	1.507***	1.006*	1.056*	1.036*	1.087*	2.026**	1.883**	1.094	1.209	1.139	1.255
	(0.517)	(0.540)	(0.528)	(0.551)	(0.530)	(0.552)	(0.745)	(0.695)	(0.807)	(0.827)	(0.827)	(0.850)
Lending rate (t-1)	-1.259*	-1.206*	-1.893**	-1.706**	-2.066***	-1.877**	-1.473**	-1.461**	-2.304***	-2.088***	-2.492***	-2.277***
	(0.667)	(0.663)	(0.750)	(0.755)	(0.791)	(0.800)	(0.569)	(0.495)	(0.662)	(0.652)	(0.703)	(0.686)
ROA (t-1)			1.080	0.812	1.271	1.000			1.082	0.637	1.209	0.764
			(1.205)	(1.241)	(1.259)	(1.295)			(1.594)	(1.407)	(1.530)	(1.344)
LogAssets (t-1)			-6.061	-4.604	-5.549	-4.093			-9.539*	-7.190	-8.858	-6.485
			(4.025)	(4.190)	(3.975)	(4.138)			(5.247)	(5.949)	(5.464)	(6.268)
Real GDP growth	-0.149	-0.122	-0.062	-0.069	-0.046	-0.055	-0.075	-0.045	-0.020	-0.016	0.035	0.039
	(0.370)	(0.381)	(0.358)	(0.362)	(0.363)	(0.366)	(0.374)	(0.400)	(0.302)	(0.311)	(0.333)	(0.344)
Spread	0.024	0.112					0.686	0.596				
	(1.352)	(1.369)					(2.141)	(2.114)				
PX growth	0.034	0.033					0.036	0.031				
	(0.069)	(0.073)					(0.065)	(0.068)				
Nominal wage growth					-0.395	-0.390					-0.439	-0.445
					(0.414)	(0.416)					(0.348)	(0.369)
Observations	276	276	276	276	276	276	276	276	276	276	276	276

Note: The table presents estimation results of equations (4.1) and (4.4), including additional control variables. The data sample covers 20 quarters from 2013 Q1 to 2017 Q4. Bank fixed effects are included. dLowCS – a dummy variable which equals 1 for the five banks with the lowest total capital surplus in the period after 2014, i.e. after the introduction of capital buffers and Pillar 2 add-ons; BBBC – bootstrap-based bias-corrected estimator; LSDV – least square dummy variable estimator with robust (clustered) standard errors. Standard errors are reported in parentheses; ***, ** and * denote the 1%, 5% and 10% significance levels.

Table C.3: How the effect changes with different lags and leads – full regression results

Estimation technique:	(1) LSDV	(2) LSDV	(3) LSDV	(4) LSDV	(5) LSDV	(6) LSDV	(7) LSDV	(8) LSDV	(9) BBBC	(10) BBBC	(11) BBBC	(12) BBBC	(13) BBBC	(14) BBBC	(15) BBBC	(16) BBBC
Credit growth (t-1)	0.740***	0.736***	0.738***	0.747***	0.760***	0.755***	0.747***	0.734***	0.842***	0.841***	0.846***	0.854***	0.871***	0.872***	0.864***	0.864***
	(0.0528)	(0.0575)	(0.0553)	(0.0523)	(0.0437)	(0.0427)	(0.0446)	(0.0495)	(0.0438)	(0.0437)	(0.0419)	(0.0420)	(0.0444)	(0.0445)	(0.0463)	(0.0525)
ORCR (t-1)*dLowCS	-1.611**								-1.053*							
	(0.710)								(0.539)							
ORCR (t-1)*(1-dLowCS)	-0.184								-0.146							
	(0.320)								(0.422)							
ORCR (t-2)*dLowCS		-1.367**								-0.830						
		(0.631)								(0.520)						
ORCR (t-2)*(1-dLowCS)		0.0274								0.0513						
		(0.226)								(0.429)						
ORCR (t-3)*dLowCS			-1.097*								-0.563					
			(0.591)								(0.509)					
ORCR (t-3)*(1-dLowCS)			0.139								0.187					
			(0.204)								(0.430)					
ORCR (t-4)*dLowCS				-0.726								-0.290				
				(0.518)								(0.504)				
ORCR (t-4)*(1-dLowCS)				0.274								0.308				
				(0.162)								(0.435)				
ORCR (t+1)*dLowCS					-1.071*								-0.528			
					(0.522)								(0.572)			
ORCR (t+1)*(1-dLowCS)					-0.266								-0.123			
					(0.336)								(0.448)			
ORCR (t+2)*dLowCS						-0.819								-0.255		
						(0.483)								(0.596)		
ORCR (t+2)*(1-dLowCS)						-0.323								-0.141		
						(0.348)								(0.457)		
ORCR (t+3)*dLowCS							-0.536								-0.0829	
							(0.722)								(0.729)	
ORCR (t+3)*(1-dLowCS)							-0.0981								0.0426	
							(0.365)								(0.524)	
ORCR (t+4)*dLowCS								-0.717								0.0697
								(0.862)								(0.825)
ORCR (t+4)*(1-dLowCS)								0.411								0.468
								(0.416)								(0.709)
LLPA (t-1)	0.199	0.180	0.181	0.153	0.536*	0.649*	1.033*	1.330**	0.403	0.392	0.554	0.559	0.922	1.011	1.314	1.652
	(0.237)	(0.238)	(0.231)	(0.232)	(0.286)	(0.365)	(0.510)	(0.607)	(0.746)	(0.750)	(0.787)	(0.801)	(0.887)	(0.982)	(1.189)	(1.135)
Real GDP growth	-0.270	-0.452	-0.596**	-0.702**	-0.392	-0.502	-0.687*	-0.764**	-0.267	-0.420	-0.495*	-0.553*	-0.332	-0.462	-0.598*	-0.670*
	(0.293)	(0.262)	(0.258)	(0.250)	(0.254)	(0.295)	(0.340)	(0.279)	(0.378)	(0.371)	(0.299)	(0.284)	(0.361)	(0.335)	(0.356)	(0.356)
Lending rate (t-1)	-1.225**	-1.068**	-0.979*	-0.782*	-1.474**	-1.311**	-1.017	-0.853	-0.943	-0.794	-0.724	-0.572	-1.063	-0.959	-0.855	-0.529
	(0.444)	(0.407)	(0.468)	(0.427)	(0.520)	(0.593)	(0.768)	(0.714)	(0.628)	(0.637)	(0.673)	(0.651)	(0.837)	(0.852)	(0.859)	(1.017)
CA (t-1)	1.757**	1.786**	1.848**	1.866**	1.923***	2.077***	2.221***	2.171***	1.404***	1.424***	1.450***	1.461***	1.552***	1.661***	1.800***	1.732**
	(0.669)	(0.656)	(0.669)	(0.668)	(0.633)	(0.652)	(0.661)	(0.615)	(0.509)	(0.508)	(0.521)	(0.524)	(0.528)	(0.569)	(0.584)	(0.683)
Observations	276	276	276	276	262	248	234	220	276	276	276	276	262	248	234	220

Note: The table presents estimation results of equations (4.4), including additional lags and leads. The data sample covers 20 quarters from 2013 Q1 to 2017 Q4. Bank fixed effects are included. dLowCS – a dummy variable which equals 1 for the five banks with the lowest total capital surplus in the period after 2014, i.e. after the introduction of capital buffers and Pillar 2 add-ons; BBBC – bootstrap-based bias-corrected estimator; LSDV – least square dummy variable estimator with robust (clustered) standard errors. Standard errors are reported in parentheses; ***, ** and * denote the 1%, 5% and 10% significance levels.

Table C.4: How important is the capital surplus in transmission – system of two equations – additional control variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Estimation technique:	LSDV	LSDV	LSDV	LSDV	BBBC	BBBC	BBBC	BBBC	3SLS	3SLS	3SLS	3SLS
Dependent variable:	CS	Credit growth	CS	Credit growth	CS	Credit growth	CS	Credit growth	CS	Credit growth	CS	Credit growth
Dependent variable (t-1)	0.517***	0.752***	0.519***	0.753***	0.516***	0.738***	0.519***	0.735***	0.518***	0.735***	0.521***	0.731***
	(0.0397)	(0.0344)	(0.0397)	(0.0329)	(0.0396)	(0.0358)	(0.0397)	(0.0345)	(0.0397)	(0.0357)	(0.0397)	(0.0342)
ORCR (t-1)	-0.704***				-0.705***				-0.699***			
	(0.0629)				(0.0628)				(0.0629)			
CS (t-1)		0.0735				0.0675				0.00223		
		(0.256)				(0.245)				(0.246)		
ORCR (t-1)*dLowCS			-0.670***				-0.667***				-0.661***	
			(0.0842)				(0.0840)				(0.0842)	
ORCR (t-1)*(1-dLowCS)			-0.712***				-0.716***				-0.709***	
			(0.0655)				(0.0654)				(0.0655)	
CS (t-1)*dLowCS				2.026***				1.893***				1.899***
				(0.459)				(0.447)				(0.442)
CS (t-1)*(1-dLowCS)				-0.305				-0.308				-0.410
				(0.258)				(0.249)				(0.251)
ROA (t-1)	-0.0369		-0.0377		-0.0403	1.399	-0.0324	0.701	-0.0399	1.489	-0.0322	0.779
	(0.170)		(0.172)		(0.171)	(1.140)	(0.172)	(1.104)	(0.171)	(1.133)	(0.172)	(1.092)
LLPA (t-1)	-0.528***	0.120	-0.531***	-0.216	-0.531***	0.373	-0.535***	-0.0253	-0.526***	0.124	-0.529***	-0.361
	(0.106)	(0.669)	(0.106)	(0.643)	(0.106)	(0.640)	(0.106)	(0.620)	(0.106)	(0.651)	(0.106)	(0.629)
Interbank loans/A (t-1) l	0.00137		0.00910		-0.00443		0.00571		-0.00415		0.00608	
	(0.0357)		(0.0371)		(0.0356)		(0.0370)		(0.0356)		(0.0371)	
Loans to CB&CG/A (t-1)	-0.00811		-0.00784		-0.00875		-0.00852		-0.00861		-0.00830	
	(0.0107)		(0.0109)		(0.0107)		(0.0109)		(0.0107)		(0.0109)	
Loans to PS excl. IL/A (t-1)	- 0.0642***		- 0.0617***		- 0.0658***		- 0.0629***		- 0.0656***		- 0.0625***	
	(0.0185)		(0.0186)		(0.0185)		(0.0186)		(0.0185)		(0.0186)	
Bonds/A (t-1)	0.0151		0.0162		0.0123		0.0141		0.0130		0.0150	
	(0.0169)		(0.0170)		(0.0169)		(0.0170)		(0.0169)		(0.0170)	
Lending rate (t-1)		-1.032*		-1.102**		-2.074***		-1.927***		-2.453***		-2.399***
		(0.547)		(0.524)		(0.605)		(0.582)		(0.639)		(0.612)
CA (t-1)		2.158***		1.846***		1.020*		0.992*		1.196**		1.216**
		(0.512)		(0.493)		(0.548)		(0.527)		(0.556)		(0.532)
LogAssets (t-1)						-12.23***		-9.889***		-10.51***		-7.584**
						(3.215)		(3.129)		(3.323)		(3.230)
Real GDP growth	0.0941*	-0.320	0.0919	-0.150	0.0998*	-0.391	0.0952*	-0.175	0.0952*	-0.182	0.0902	0.0994
	(0.0562)	(0.346)	(0.0562)	(0.333)	(0.0560)	(0.269)	(0.0561)	(0.262)	(0.0560)	(0.291)	(0.0561)	(0.284)
PX growth	0.0283***	-0.0112	0.0287***	-0.0155	0.0277***		0.0283***		0.0281***		0.0287***	
	(0.0108)	(0.0651)	(0.0109)	(0.0623)	(0.0107)		(0.0108)		(0.0107)		(0.0108)	
Spread	-1.095***	2.240*	-1.098***	1.519	-1.051***		-1.073***		-1.057***		-1.079***	
	(0.213)	(1.228)	(0.213)	(1.183)	(0.211)		(0.212)		(0.211)		(0.212)	
Nominal wage growth										-0.631*		-0.801**
										(0.351)		(0.337)
IRV dummy	-0.894		-1.367		-0.886		-1.435		-0.896		-1.437	
	(0.556)		(1.008)		(0.555)		(1.007)		(0.556)		(1.008)	
Observations	276		276		276		276		276		276	

Note: The table presents estimation results of the system of two equations (4.2)–(4.3) and (4.5)–(4.6), including additional control variables. The data sample covers 20 quarters from 2013 Q1 to 2017 Q4. Bank fixed effects are included. dLowCS – a dummy variable which equals 1 for the five banks with the lowest total capital surplus in the period after 2014, i.e. after the introduction of capital buffers and Pillar 2 add-ons; SLS – three-stage least squares estimator; BBBC – bootstrap-based bias-corrected estimator; LSDV – least square dummy variable estimator with robust (clustered) standard errors. Standard errors are reported in parentheses; ***, ** and * denote the 1%, 5% and 10% significance levels.

Table C.5: How important is the intentional and unintentional capital surplus in transmission – system of two equations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Estimation technique:	3SLS	3SLS	3SLS	3SLS	3SLS	3SLS	BBBC	BBBC	BBBC	BBBC	BBBC	BBBC
Dependent variable:	ICS	UCS	Credit growth	ICS	UCS	Credit growth	ICS	UCS	Credit growth	ICS	UCS	Credit growth
Dependent variable (t-1)	0.313***	0.627***	0.749***	0.314***	0.630***	0.740***	0.357***	0.799***	0.870***	0.357***	0.803***	0.824***
	(0.028)	(0.047)	(0.034)	(0.028)	(0.047)	(0.033)	(0.034)	(0.073)	(0.065)	(0.034)	(0.074)	(0.057)
ORCR (t)	−0.795***	0.036					−0.756***	0.026				
	(0.036)	(0.038)					(0.037)	(0.046)				
ICS (t-1)			0.383						0.092			
			(0.290)						(0.300)			
UCS (t-1)			0.077						0.080			
			(0.370)						(0.437)			
ORCR*dLowCS				−0.757***	0.053					−0.732***	0.026	
				(0.046)	(0.059)					(0.044)	(0.065)	
ORCR*(1-dLowCS)				−0.804***	0.029					−0.764***	0.026	
				(0.037)	(0.048)					(0.039)	(0.058)	
ICS (t-1)*dLowCS						2.390***						1.682**
						(0.506)						(0.802)
ICS (t-1)*(1-dLowCS)						0.0266						−0.129
						(0.299)						(0.253)
UCS (t-1)*dLowCS						2.546***						2.082**
						(0.589)						(1.019)
UCS (t-1)*(1-dLowCS)						−0.648*						−0.614*
						(0.391)						(0.357)
ROA (t-1)	0.185*			0.194**			0.370***			0.375***		
	(0.0987)			(0.0984)			(0.106)			(0.105)		
LLPA (t-1)	−0.897***	0.096	0.418	−0.905***	0.097	−0.080	−0.858***	0.132	0.573	−0.865***	0.130	−0.013
	(0.057)	(0.119)	(0.661)	(0.057)	(0.125)	(0.629)	(0.061)	(0.197)	(0.640)	(0.062)	(0.201)	(0.505)
Interbank loans/A (t-1)	−0.038**			−0.031			−0.038**			−0.034**		
	(0.019)			(0.019)			(0.017)			(0.018)		
Loans to CB&CG/A (t-1)	0.003			0.002			0.005			0.005		
	(0.006)			(0.006)			(0.006)			(0.006)		
Loans to PS excl. IL/A (t-1)	−0.010			−0.009			−0.000			0.000		
	(0.009)			(0.009)			(0.010)			(0.010)		
Bonds/A (t-1)	−0.019**			−0.017**			−0.021**			−0.020**		
	(0.009)			(0.009)			(0.010)			(0.010)		
Lending rate (t-1)			−0.788			−0.931*			−0.680			−0.900
			(0.545)			(0.517)			(0.647)			(0.603)
CA (t-1)			1.826***			1.562***			1.381**			1.419**
			(0.526)			(0.498)			(0.639)			(0.568)
Real GDP growth	0.188***		−0.532**	0.185***		−0.186	0.191***		−0.506	0.190***		−0.262
	(0.029)		(0.268)	(0.029)		(0.261)	(0.030)		(0.350)	(0.030)		(0.286)
PX growth	−0.003			−0.002			−0.004			−0.003		
	(0.006)			(0.006)			(0.006)			(0.006)		
Spread	−0.192**			−0.205***			−0.207*			−0.210*		
	(0.111)			(0.111)			(0.117)			(0.117)		
IRB dummy	1.811***	−0.479**		1.858***	−0.476**			−0.479**				
	(0.207)	(0.223)		(0.208)	(0.224)			(0.223)				
Int. income/A (t-1)		−0.006			0.007			0.027			0.027	
		(0.165)			(0.165)			(0.208)			(0.210)	
Ret. earnings/A		0.039			0.033			−0.015			−0.015	
		(0.050)			(0.054)			(0.071)			(0.077)	
Observations	276	276	276	276	276	276	276	276	276	276	276	276

Note: The table presents estimation results of the system of three equations (4.7)–(4.9). The data sample covers 20 quarters from 2013 Q1 to 2017 Q4. Bank fixed effects are included. dLowCS – a dummy variable which equals 1 for the five banks with the lowest total capital surplus in the period after 2014, i.e. after the introduction of capital buffers and Pillar 2 add-ons; 3SLS – three-stage least squares estimator; BBBC – bootstrap-based bias-corrected estimator. Standard errors are reported in parentheses; ***, ** and * denote the 1%, 5% and 10% significance levels.

Table C.6: How important is the capital surplus in transmission – system of two equations (longer data sample)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Estimation technique:	LSDV	LSDV	LSDV	LSDV	BBBC	BBBC	BBBC	BBBC	3SLS	3SLS	3SLS	3SLS
Dependent var.:	CS	Credit growth	CS	Credit growth	CS	Credit growth	CS	Credit growth	CS	Credit growth	CS	Credit growth
Dependent var. (t-1)	0.841***	0.804***	0.835***	0.794***	0.904***	0.852***	0.896***	0.844***	0.841***	0.803***	0.835***	0.793***
	(0.0325)	(0.0369)	(0.0300)	(0.0392)	(0.0277)	(0.0370)	(0.0297)	(0.0372)	(0.0203)	(0.022)	(0.0210)	(0.023)
ORCR (t-1)	-0.181***				-0.164***				−0.182***			
	(0.0323)				(0.0463)				(0.037)			
CS (t-1)		0.237				0.195				0.231		
		(0.187)				(0.158)				(0.164)		
ORCR*dLowCS			-0.227***				-0.193***				−0.228***	
			(0.0512)				(0.0642)				(0.055)	
ORCR*(1-dLowCS)			-0.166***				-0.152***				−0.167***	
			(0.0319)				(0.0518)				(0.040)	
CS (t-1)*dLowCS				0.713**				0.583*				0.704***
				(0.264)				(0.313)				(0.236)
CS (t-1)*(1-dLowCS)				-0.0536				-0.0461				−0.056
				(0.180)				(0.135)				(0.193)
ROA (t-1)	0.0554		0.0468		0.046		0.0435		0.053		0.045	
	(0.0615)		(0.0630)		(0.107)		(0.106)		(0.076)		(0.076)	
LLPA (t-1)	-0.025	-0.261	-0.0235	-0.468	-0.0279	-0.174	-0.0131	-0.357	−0.026	−0.266	−0.024	−0.470
	(0.0419)	(0.511)	(0.0431)	(0.461)	(0.108)	(0.405)	(0.108)	(0.385)	(0.0733)	(0.477)	(0.073)	(0.480)
CA (t-1)		-0.339*		-0.353		-0.247		-0.25		−0.324		−0.342
		(0.189)		(0.291)		(0.248)		(0.243)		(0.254)		(0.252)
Lending rate (t-1)		0.693		0.519		0.522*		0.36		0.695**		0.522*
		(0.431)		(0.422)		(0.314)		(0.303)		(0.274)		(0.279)
Interbank loans/A (t-1)	-0.0137		-0.0174		-0.0119		-0.0148		−0.014		−0.018	
	(0.0288)		(0.0305)		(0.0181)		(0.0174)		(0.016)		(0.017)	
Loans to CB	0.00879		0.0105		0.00797		0.00968		0.009		0.01	
	(0.0137)		(0.0141)		(0.0122)		(0.0113)		(0.010)		(0.010)	
Loans to PS excl. IL/A (t-1)	0.00535		0.00289		0.00408		0.00248		0.005		0.003	
	(0.0102)		(0.0107)		(0.0117)		(0.0116)		(0.010)		(0.010)	
Bonds/A (t-1)	0.00522		0.00558		0.00415		0.0054		0.005		0.005	
	(0.00845)		(0.00834)		(0.0110)		(0.0115)		(0.010)		(0.010)	
Real GDP growth	-0.0362*	0.376***	-0.0363**	0.380**	-0.0337	0.310**	-0.0366	0.318***	−0.036	0.378***	−0.037	0.381***
	(0.0168)	(0.121)	(0.0167)	(0.137)	(0.0330)	(0.123)	(0.0337)	(0.118)	(0.028)	(0.129)	(0.028)	(0.128)
PX growth	0.00301		0.00287		0.00266		0.00271		0.003		0.003	
	(0.00226)		(0.00229)		(0.00311)		(0.00317)		(0.003)		(0.003)	
Spread	-0.227*		-0.230*		-0.224**		-0.224*		−0.228**		−0.231**	
	(0.117)		(0.118)		(0.110)		(0.115)		(0.0987)		(0.0987)	
IRB dummy	0.586**		0.610***		0.39		0.423		0.589**		0.612**	
	(0.206)		(0.191)		(0.315)		(0.360)		(0.263)		(0.264)	
Observations	630	630	630	630	630	630	630	630	630	630	630	630

Note: The table presents estimation results of the system of two equations (4.2)–(4.3) and (4.5)–(4.6). The data sample covers 56 quarters from 2004 Q1 to 2017 Q4. Bank fixed effects are included. dLowCS – a dummy variable which equals 1 for the five banks with the lowest total capital surplus in the period after 2014, i.e. after the introduction of capital buffers and Pillar 2 add-ons; SLS – three-stage least squares estimator; BBBC – bootstrap-based bias-corrected estimator; LSDV – least square dummy variable estimator with robust (clustered) standard errors. Standard errors are reported in parentheses; ***, ** and * denote the 1%, 5% and 10% significance levels.

Table C.7: How important is the intentional and unintentional capital surplus in transmission – system of two equations (longer data sample)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Estimation technique:	3SLS	3SLS	3SLS	3SLS	3SLS	3SLS	BBBC	BBBC	BBBC	BBBC	BBBC	BBBC
Dependent variable:	ICS	UCS	Credit growth	ICS	UCS	Credit growth	ICS	UCS	Credit growth	ICS	UCS	Credit growth
Dependent variable (t-1)	0.634***	0.808***	0.797***	0.628***	0.805***	0.769***	0.610***	0.827***	0.855***	0.605***	0.828***	0.827***
	(0.023)	(0.022)	(0.023)	(0.023)	(0.022)	(0.023)	(0.027)	(0.039)	(0.036)	(0.027)	(0.039)	(0.038)
ORCR (t)	−0.327***	0.029					−0.332***	0.055**				
	(0.030)	(0.030)					(0.030)	(0.025)				
ICS (t-1)			0.166						0.119			
			(0.193)						(0.179)			
UCS (t-1)			0.175						0.168			
			(0.208)						(0.226)			
ORCR*dLowCS				−0.371***	0.016					−0.379***	0.046	
				(0.043)	(0.044)					(0.035)	(0.038)	
ORCR*(1-dLowCS)				−0.314***	0.040					−0.317***	0.062*	
				(0.032)	(0.036)					(0.032)	(0.032)	
ICS (t-1)*dLowCS						1.642***						1.315**
						(0.388)						(0.553)
ICS (t-1)*(1-dLowCS)						−0.309						−0.247
						(0.218)						(0.159)
UCS (t-1)*dLowCS						0.443*						0.394
						(0.258)						(0.344)
UCS (t-1)*(1-dLowCS)						−0.018						0.031
						(0.260)						(0.197)
ROA (t-1)	0.334***			0.329***			0.518***			0.509***		
	(0.055)			(0.055)			(0.055)			(0.054)		
LLPA (t-1)	−0.435***	0.186**	−0.312	−0.435***	0.193**	−0.615	−0.444***	0.129	−0.150	−0.442***	0.131	−0.406
	(0.060)	(0.075)	(0.482)	(0.060)	(0.075)	(0.480)	(0.066)	(0.117)	(0.428)	(0.065)	(0.117)	(0.418)
Interbank loans/A (t-1)	−0.037***			−0.040***			−0.039***			−0.042***		
	(0.011)			(0.011)			(0.010)			(0.009)		
Loans to CB&CG/A (t-1)	0.005			0.007			0.007			0.009		
	(0.007)			(0.007)			(0.008)			(0.008)		
Loans to PS excl. IL/A (t-1)	0.000			−0.002			0.001			−0.001		
	(0.007)			(0.007)			(0.008)			(0.008)		
Bonds/A (t-1)	0.004			0.005			0.008			0.008		
	(0.007)			(0.007)			(0.007)			(0.007)		
Lending rate (t-1)			0.738***			0.463*			0.535*			0.298
			(0.276)			(0.279)			(0.313)			(0.303)
CA (t-1)			−0.292			−0.382			−0.243			−0.325
			(0.266)			(0.266)			(0.292)			(0.292)
Real GDP growth	0.001		0.395***	0.001		0.381***	0.008		0.322**	0.007		0.325***
	(0.019)		(0.132)	(0.019)		(0.131)	(0.018)		(0.130)	(0.018)		(0.121)
PX growth	0.005**			0.005**			0.006***			0.006***		
	(0.002)			(0.002)			(0.002)			(0.002)		
Spread	−0.225***			−0.231***			−0.234***			−0.241***		
	(0.070)			(0.070)			(0.073)			(0.072)		
IRB dummy	1.811***	−0.479**		1.858***	−0.476**		2.172***	−0.347*		2.197***	−0.347*	
	(0.207)	(0.223)		(0.208)	(0.224)		(0.195)	(0.178)		(0.194)	(0.180)	
Int. income/A (t-1)		−0.034			−0.029			0.147			0.146	
		(0.158)			(0.159)			(0.103)			(0.103)	
Ret. earnings/A		0.036*			0.030			0.041*			0.039	
		(0.022)			(0.023)			(0.024)			(0.025)	
Observations	630	630	630	630	630	630	630	630	630	630	630	630

Note: The table presents estimation results of the system of three equations (4.7)–(4.9). The data sample covers 56 quarters from 2004 Q1 to 2017 Q4. Bank fixed effects are included. dLowCS – a dummy variable which equals 1 for the five banks with the lowest total capital surplus in the period after 2014, i.e. after the introduction of capital buffers and Pillar 2 add-ons; 3SLS – three-stage least squares estimator; BBBC – bootstrap-based bias-corrected estimator. Standard errors are reported in parentheses; ***, ** and * denote the 1%, 5% and 10% significance levels.

Appendix D

Response to Opponents' Reports

March 15, 2024

I want to express my gratitude to all opponents for their time reading and reviewing the thesis and for their insightful questions and ideas for future research. I tried to address most of the questions for discussion during the predefense. Below, I summarize the answers to the questions from referee reports.

D.1 Response to Comments from Zuzana Fungacova, PhD

[...] Considering all the positive aspects mentioned and the high quality of these already published papers, the focus of my feedback will be on identifying opportunities for further research on these topics. [...]

Response: Thank you very much for your report. Below, I address your comments and questions for respective essays.

Essay 2: When Does Monetary Policy Sway House Prices? A Meta-Analysis

In this study the monetary policy is measured by the short-term interest rates but, as the authors also acknowledge, the range of monetary policy tools applied by central banks has changed in recent years and unconventional monetary policy tools have also been implemented and become increasingly important. In this respect it would be very interesting to study the impact of these new measures on house prices. Identifying the effects related to the specific tools might be challenging but as it is important to take their effect into account, a more comprehensive measure of monetary policy stance like e.g. shadow rate might be considered.

Response: Thank you very much for this comment. It is true that considering the effect of unconventional monetary policy on house prices would be important to capture the whole picture. We considered including studies capturing the effect of shadow rates on house prices, but at the time of writing the paper, the number of studies was very low – insufficient for a *meta-analysis. In the future, however, this will definitely present an opportunity to extend the research in this direction.*

Fabo *et al.* (2021) is frequently cited in this essay. This paper finds that central bank papers find quatitative easing to be more effective than academic papers do. Related to this, the authors of this essay might also check if similar result is found when looking at the studies investigated in this meta-analysis and checking the affiliations of the authors.

Response: Thank you very much for this suggestion. We have not collected the information on the authors' affiliation at the time of writing. In the paper by Fabo et al. *(2021), the effectiveness of quantitative easing in terms of inflation is in the direct interest of central bankers; in our case, house prices are not a direct goal of monetary policy; thus, I would argue the distortion for the house price response could be smaller than what was found in their paper. However, as they show, authors with different affiliations can have different interests in terms of the results for other variables in a VAR model, so it might impact the reported response of house prices as well. Therefore, I agree it is a very interesting idea for future research.*

Essay 3: The Effect of Higher Capital Requirements on Bank Lending: The Capital Surplus Matters

[...] The results also uncover crucial role capital surplus plays in the transmission of higher capital requirements to bank lending growth. Even if capital surplus is clearly very important in this transmission, maybe some discussion can be added on other possible channels through which higher capital requirements can be transmitted to bank lending growth.

Response: Thank you for this comment. It is true that in the paper, we mainly focus on the capital surplus as an important determinant. In the introduction of the paper, we briefly discuss various other ways how banks can react to changes in capital requirements, and we also discussed it in more detail in an extended working paper version of the article. Other possible channels through which higher capital requirements can affect bank lending include the balance sheet constraint channel, risk channel (banks can change the risk composition of their assets to less risky), and credit pricing channel (via the effect on bank lending rates). In the working paper version, we examine these potential ways of how banks can react in regressions analysing the effect of capital requirements on bank leverage, retained earnings, and implicit risk weights, before turning to laon growth in more detail. In our another paper (Ehrenbergerová et al. *2022), we do not find significant effect of capital requirements on lending rates or interest margins in the Czech Republic, irrespective of loan category, bank capitalisation or size.*

All the estimations in the paper are clearly explained and carefully executed. The authors point out possible challenges related to the estimations and address them to the extend possible. When choosing the set of control variables, it is explained that they are based on bank-capital and bank-lending channel literature. In this respect e.g. bank size or liquidity ratios are employed in this literature but I am not sure these variables were also included in this study.

Response: In the main text of the paper, we only include controls for credit risk (loan loss provisions) and leverage (capital to assets) together with bank fixed effects in the loan equation. In the regression on capital surplus, we include assets decomposition into various types of loans and bonds. In the appendix of the paper, we also included additional controls for better identification, specifically in Tables C.2 and C.4. The additional controls included a logarithm of assets to capture bank size and ROA as a proxy for bank profitability, but also an additional control for demand side (nominal wage growth) and macro-financial controls (spread and PX stock index growth). The results were qualitatively and quantitatively similar. In this response, I also include an excerpt from the results where we control not only for bank size and profitability but also for liquidity ratio, as you suggested. The first four columns in Table D.1 are original results from Table 4.4; columns (5)-(8) are newly added results where variables $Log(Assets)$, Liq. ratio, and ROA are used. The results on our main variables of interest are very similar.

The dampening effect of higher capital requirements on bank lending growth has been identified. Nevertheless, only the overall lending growth is considered. Further interesting question is if this impact concerns all types of loans or only certain loans like e.g. corporate or household loans or more risky loans. This kind of investigation could enable to get closer to identifying the real effects of this change in bank regulation.

Response: Thank you very much for this comment. Based on the data available in the bank-level supervisory dataset, we would be able to differentiate between mortgages, consumer loans, other loans for households, and loans to non-financial corporations, similarly as we later did in the paper on the effect of capital requirements on lending rates and interest margins (Ehrenbergerová et al. *2022). It would be interesting to see what part drives the effect and why, and we will consider it in future research.*
Thank you again for all the comments.

Table D.1: How important is the capital surplus in transmission – system of two equations – inclusion of additional bank-level control variables

Dependent variable:	(1) CS	(2) Credit growth	(3) CS	(4) Credit growth	(5) CS	(6) Credit growth	(7) CS	(8) Credit growth
Dependent variable (t-1)	0.516***	0.769***	0.519***	0.765***	0.515***		0.518***	
	(0.040)	(0.0334)	(0.040)	(0.032)	(0.0396)		(0.0397)	
ORCR (t-1)	-0.702***				-0.707***			
	(0.063)				(0.063)			
CS (t-1)		0.197				0.057		
		(0.248)				(0.245)		
ORCR*dLowCS			-0.668***				-0.669***	
			(0.084)				(0.084)	
ORCR*(1-dLowCS)			-0.711***				-0.718***	
			(0.066)				(0.065)	
CS (t-1)*dLowCS				2.188***				1.953***
				(0.445)				(0.447)
CS (t-1)*(1-dLowCS)				-0.236				-0.344
				(0.251)				(0.249)
ROA (t-1)	-0.035		-0.037		-0.041	1.409	-0.033	0.689
	(0.170)		(0.172)		(0.171)	(1.139)	(0.172)	(1.100)
LLPA (t-1)	-0.531***	0.380	-0.532***	-0.053	-0.532***	-0.086	-0.536***	-0.875
	(0.106)	(0.654)	(0.106)	(0.629)	(0.106)	(0.862)	(0.106)	(0.842)
Interbank loans/A (t-1)	0.002		0.010		-0.005		0.006	
	(0.036)		(0.037)		(0.036)		(0.037)	
Loans to CB&CG/A (t-1)	-0.008		-0.008		-0.009		-0.009	
	(0.011)		(0.011)		(0.011)		(0.011)	
Loans to PS excl. IL/A (t-1)	-0.064***		-0.061***		-0.066***		-0.062***	
	(0.019)		(0.019)		(0.019)		(0.019)	
Bonds/A (t-1)	0.015		0.016		0.012		0.014	
	(0.017)		(0.017)		(0.017)		(0.017)	
Lending rate (t-1)		-0.853		-0.973*		-2.008***		-1.799***
		(0.526)		(0.505)		(0.610)		(0.587)
CA (t-1)		1.901***		1.674***		0.960*		0.879*
		(0.500)		(0.479)		(0.553)		(0.531)
Real GDP growth	0.100*	-0.681***	0.095*	-0.390	0.101*	-0.427	0.097*	-0.230
	(0.056)	(0.262)	(0.056)	(0.256)	(0.0560)	(0.273)	(0.056)	(0.264)
PX growth	0.028***		0.029***		0.028***		0.028***	
	(0.011)		(0.011)		(0.011)		(0.011)	
Spread	-1.058***		-1.077***		-1.049***		-1.071***	
	(0.212)		(0.212)		(0.211)		(0.212)	
IRB dummy	-0.891		-1.373		-0.876		-1.419	
	(0.556)		(1.008)		(0.556)		(1.007)	
Log(Assets) (t-1)						-11.31***		-8.122**
						(3.419)		(3.341)
Liq. ratio (t-1)						0.195		0.353
						(0.245)		(0.238)
Observations	276	276	276	276	276	276	276	276

Note: The table presents estimation results of the system of two equations (4.2)–(4.3) and (4.5)–(4.6). The data sample covers 20 quarters from 2013 Q1 to 2017 Q4. Bank fixed effects are included. dLowCS – a dummy variable which equals 1 for the five banks with the lowest total capital surplus in the period after 2014, i.e. after the introduction of capital buffers and Pillar 2 add-ons. Specifications are estimated using the three-stage least squares estimator (3SLS). Using the bootstrap-based bias-corrected estimator (BBBC) or the least square dummy variable estimator (LSDV) with robust (clustered) standard errors yields quantitatively similar results. Standard errors are reported in parentheses; ***, ** and * denote the 1%, 5% and 10% significance levels.

D.2 Response to Comments from Martina Jasova, PhD

Given that all essays have already been published in peer-reviewed journals, I have no substantial comments or objections that would require changes to the submitted thesis. Instead, in what follows, I offer a couple of questions that could help guide the discussion in the day of the pre-defense, or could serve as references for building on this body of work in the future.

Response: Thank you very much for your report and for all the questions and potential ideas for future research. During the pre-defense, we discussed especially the first two questions, as the first essay was presented during the-predefense. Below, I briefly summarize my answers to the questions for the discussion.

Essay 1: Measuring Capital-Labor Substitution: The Importance of Method Choices and Publication Bias

How do the findings on the elasticity of substitution challenge or support existing macroeconomic models, especially regarding predictions on technological change and income inequality?

Response: Thank you for this relevant question. There are several areas in which capital-labor complementarity challenges existing macroeconomic models. First, technological change often leads to factor substitution in neoclassical models; however, with capital and labor complements, technological advancements may not necessarily lead to factor substitution, and thus, technological change may not reduce the demand for labor. The low elasticity of substitution also suggests that technological change may be skill-biased rather than factor-neutral. Technological advancements are more likely to complement high-skilled labor while substituting for low-skilled. The overall impact of technical change on economic growth and productivity may be dampened as with low substitutability, firms may be slower to adapt to new technologies. Regarding income inequality, the above-mentioned skill-biased technical change may increase wage differentials, exacerbating the gap between skilled and unskilled workers and, thus, income inequality. I also reflected on this and the following comment in the Introduction of the thesis.

Can you elaborate on the potential policy implications of your corrected estimates for the elasticity of substitution, particularly in the context of fiscal and labor market policies?

Response: Regarding fiscal policy, cuts to corporate taxes (translated via user cost of capital) are less effective in stimulating investment if the elasticity of substitution is low. Thus, corporate tax cuts would need to be stronger in order to provide the same investment stimulus as expected, or, in other words, there is less trust in the growth effects of corporate tax cuts. Related to this, Gechert & Heimberger (2022) in another meta-analysis cannot reject the hypothesis of a zero effect of corporate taxes on growth. As for the labor market, our results imply, first, the search for alternative arguments to explain the fall in the labor share that are consistent with $\sigma < 1$, like business concentration, automation, directed technical change, the deterioration of union coverage etc., instead of Piketty's explanation with rising capital intensity. Second, related to skill-biased technical change, the results may imply the call for addressing the skill gap (training programmes, education incentives, etc.) to increase productivity.

Essay 2: When Does Monetary Policy Sway House Prices? A Meta-Analysis

Considering the variances in housing markets and monetary policy effectiveness across different economies, how might the findings inform policy adjustments in countries with different economic structures?

Response: Thank you very much for this question. In the paper, we try to examine the drivers of structural heterogeneity and, thus, the effectiveness of monetary policy across different economies, in detail. We control for numerous macroeconomic, financial, demographic, and housing supply factors. To inform policy adjustment, a policymaker can potentially look at our results and adjust the assumed transmission from interest rates to house prices according to what is relevant for their country. For example, we find stronger transmission in countries like the UK and Switzerland and weaker in the US and Germany. In general, we find stronger effects in countries with high credit-to-GDP ratio (i.e., in more developed mortgage markets), during a prolonged period of house price growth (i.e., during a build-up of a house price bubble), and during times of flatter yield curve (i.e., during the latter part of the cycle).

Could you discuss the potential long-term effects of monetary policy on housing affordability and economic inequality, based on your research findings?

Response: In general, expansionary monetary policy stimulates housing demand, leading to upward pressure on prices. This can reduce housing affordability in the long term, especially when coupled with limited housing supply. As housing costs consume a larger share of household income, lower-income households may face greater challenges in affording housing, leading to widening disparities in economic inequality. Our results suggest there is significant transmission from interest rates to house prices, even after correcting for publication bias. However, the transmission is not, on average, sufficiently strong to mitigate extreme house price growth rates. Our findings support the argument that macroprudential policies and other policies like supporting housing supply might be needed to complement the effect of monetary policy to prevent house price bubbles and improve affordability challenges in future.

Essay 3: The Effect of Higher Capital Requirements on Bank Lending: The Capital Surplus Matters

How do the results contribute to the ongoing debate on the trade-off between financial stability and credit availability crises?

Response: During financial crises, maintaining credit availability is important to support recovery and prevent prolonged periods of economic stagnation. Our results suggest that the effect of higher capital requirements on credit growth is significant, but only for less capitalized banks. Thus, I would argue that capital requirements meant to sustain financial stability in the Czech Republic do not excessively cut credit and would not contribute to a credit availability crisis.

While the analysis focuses on banks on the consolidated level, the recent work of Degryse *et al.* (2023) has highlighted the importance of the intragroup transmission of stricter capital regulation. Should we expect similar results on an unconsolidated basis? To what extent would foreign ownership of Czech banks matter in understanding the additional transmission mechanisms stemming from the capital shock at the foreign parent level?

Response: Thank you for this very relevant comment. Degryse et al. *(2023)'s main results suggest that recapitalization efforts are concentrated at the subsidiary- as opposed to the headquarters-level. They also find that small, less profitable, loosely regulated subsidiaries reduce credit supply. While Czech banks are considered small in an international context, they are profitable, well-capitalized and strictly regulated. Thus, in the context of their paper, I would argue that Czech banks are less likely to reduce their credit supply in response to capital shock at the foreign parent level. In any case, a proper examination of cross-border intra-group effects of changes in capital requirements is a very relevant idea for future research.*

Thank you very much for all the questions for discussion.

D.3 Response to Comments from Prof. Dr. Jarko Fidrmuc, PhD

All the papers presented in the dissertation were published in highly respected economic journals. Therefore, it is difficult to make recommendations for further improvements. However, there are issues which could be addressed in the future research. Moreover, I would like to address some issues related to the recent economic developments, which could be discussed during the defense.

Response: Thank you very much for your referee report. I addressed the questions for discussion during the pre-defense, especially the first question on Essay 1, as that was the one presented during the pre-defense. Below, I briefly summarize my answers.

Essay 1: Measuring Capital-Labor Substitution: The Importance of Method Choices and Publication Bias

Related to the first paper, it would be interesting to see whether the information technology, digitalization and automation changed the substitution between labor and capital. Similarly, it would be interesting to see the opinion of the author on the current advances regarding the artificial intelligence.

Response: I would argue that automation can lead to an increse in the elasticity of substitution, as it provides new opportunities for substituting capital and labor and enables firms to adjust their production processes more easily. As automation replaces routine tasks performed by low-skilled labor, there is an increased emphasis on upgrading skills and adopting more advanced technologies. Automation is often associated with ongoing technological progress, leading to advancements in robotics and artificial intelligence, and these advancements may further enhance the substitutability between labor and capital by making automation more efficient and cost-effective. On the other hand, automation can also create challenges related to job displacement, income inequality, and workforce adaptation. Partly related to this, our results suggest a mild upward trend in the reported elasticities, which might be hypothetically linked to ongoing automation. In any case, this mild upward trend does not justify Cobb-Douglas specification – at this pace, the specification will become consistent with unitary elasticity in about 175 years.

Essay 2: When Does Monetary Policy Sway House Prices? A Meta-Analysis

Secondly, the housing market has been also influenced by several current developments. First, the rise of the sharing economy (Reichle *et al.* 2023) and the platforms such as Airbnb possibly contributed to nearly explosive growth of prices in several cities. By contrast, the pandemic is generally believed to have induced an opposite move from the main cities to smaller urban communities.

Response: Thank you very much for this comment. It is true that both the sharing economy and the pandemic might have influenced the transmission from monetary policy to house prices. Regarding the pandemic, we have not tried to analyse its effect, as our dataset does not sufficiently cover this period. Nevertheless, it is an interesting idea for future research. Partly related to the sharing economy and the Airbnb platform, we include a cross-country variable 'Tourism' as a factor influencing demand for housing. The variable does not seem to be a significant driver of heterogeneity in our results. Still, it can be time-specific and future research could uncover interesting associations between the sharing economy and monetary policy transmission.

Essay 3: The Effect of Higher Capital Requirements on Bank Lending: The Capital Surplus Matters

Finally, from the perspective of the third paper, it would be interesting to discuss, ex post, the Basel reform of capital requirements. In addition to the discussion at the defense, I hope that these questions will be addressed in future research and I am looking forward to see the new results in new publications.

Response: Thank you very much for this comment. In our paper, we offer an additional piece of the puzzle on the effectiveness of capital requirements of Basel III reform. Basel III reform included a Tier 1 capital ratio of 6%, CET1 ratio of 4,5%; the introduction of a capital conservation buffer, countercyclical buffer, and buffer for systematically important institutions, and also liquidity coverage ratio, net stable funding ratio; revised standardized approach for the calculation of risk-weighted assets, and changes in counterparty credit risk assessment. Regarding the evaluation of the effects of new capital requirements, Fidrmuc & Lind (2020) provide a meta-analysis of the impact of higher capital requirements on macroeconomic activity, finding a negative, albeit moderate GDP level effect in response to a change in the capital ratio. There is also a meta-analysis on the effect of bank capital (both capital ratio and capital requirements) on credit (Malovaná et al. *2023). To the best of my knowledge, there has also been a work group in BCBS on the evaluation of the reform.*

I thank the referees for all the comments, questions for discussions and suggestions for future research. I sincerely appreciate it.